Bank of Finland Research Discussion Papers
2 • 2021

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Bank of Finland
Research

Bank of Finland Research Discussion Papers
Editor-in-Chief Esa Jokivuolle

Bank of Finland Research Discussion Paper 2/2021
12 February 2021

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ISBN 978-952-323-366-9, online
ISSN 1456-6184, online

Bank of Finland
Research Unit

PO Box 160
FIN-00101 Helsinki

Phone: +358 9 1831

Email: research@bof.fi
Website: www.suomenpankki.fi/en/research/research-unit/

Investor monitoring, money-likeness and stability of money market funds*

Maija Järvenpää and Aleksi Paavola†

First draft: April 27, 2020
This version: February 3, 2021

Abstract

An asset is money-like if investors have no incentives to acquire costly private information on the underlying collateral. However, privately provided money-like assets—like prime money market fund (MMF) shares—are prone to runs if investors suddenly start to question the value of the collateral. Therefore, for risky assets, lack of money-likeness is a necessary condition for lack of run incentives. But is it a sufficient one? This paper studies the effect of the U.S. money market fund reform of 2014–2016 on investor monitoring, money-likeness and stability of institutional prime MMFs. Using the number of distinct IP addresses accessing MMFs' regulatory reports as a proxy for investor monitoring, we find that the reform increased monitoring and thus decreased money-likeness of institutional prime funds. However, we also show that after the reform, institutional prime funds that are more likely to impose the newly introduced redemption restrictions are more monitored, suggesting that investors may monitor in order to avoid being hit by the restrictions. Overall, our results indicate that increased monitoring, or decreased money-likeness, has not made institutional prime MMFs run-free, and it may have actually created a new source of fragility for MMFs.

Keywords: Money market funds, money markets, money market fund reform, money-likeness, information sensitivity, monitoring

JEL Codes: G01, G23, G28

*We thank our discussants Sven Klingler and Geert van Moer for valuable comments and Marko Terviö, Sami Torstila, and Juuso Välimäki for helpful comments and guidance. We also thank participants at the Nordic Finance Network PhD Workshop 2020 and Helsinki GSE IO PhD Student Workshop Spring 2020 for their feedback and comments. Finally, we are grateful for Michael Ungeheuer and Fabian Brunner for providing us with the log file data, including the list of "robot" IP addresses at IP address-day level. Any remaining errors are ours.

†Järvenpää: Aalto University School of Business, Department of Finance, maija.jarvenpaa@aalto.fi. Paavola: Bank of Finland, Aalto University School of Business and Helsinki GSE, aleksi.paavola@bof.fi. Järvenpää acknowledges financial support from OP Group Research Foundation. Paavola acknowledges financial support from Emil Aaltonen Foundation, OP Group Research Foundation, Suomen Arvopaperimarkkinoiden Edistämissäätiö, and Yrjö Jahnsson Foundation. The views expressed in this paper are those of the authors and do not necessarily reflect the views of the Bank of Finland.

1 Introduction

For an asset to be used as money, it must be information insensitive (Gorton and Pennacchi, 1990; Dang et al., 2017). In other words, agents expect to be able to sell the asset at par value whenever needed and thus have have no incentives to acquire costly private information on the underlying collateral. This collective ignorance ensures that the asset is immune to adverse selection, which further facilitates efficient trade (e.g. Holmström, 2015). As a result, money-like assets should not be monitored much by investors and conversely, active monitoring of the asset suggests that it is not money-like.

The built-in downside of any privately provided money-like asset is that the asset can lose its money-likeness if investors suddenly start to question the value of the underlying collateral. In practice, a sudden increase in incentives to acquire information can cause a market freeze or a bank-run-like panic. (Dang et al., 2015) In other words, in order to be immune to runs, the risky asset cannot be money-like, and hence reducing run risk of the asset requires limiting its money-likeness. However, whether decreasing money-likeness is *sufficient* for reducing runnability is another question.

Motivated by these theoretical ideas, in this paper, we study the effect of the U.S. money market fund (MMF) reform of 2014–2016 on investor monitoring and thus money-likeness. The purpose of the reform is to reduce probability and severity of runs on prime MMFs, especially those targeted to institutional investors. As MMFs have traditionally been regarded as (risky) money-like assets, the reform aims at decreasing their run risk by limiting the characteristics that have made MMF shares money-like in the past. More specifically, the reform alters the payoff offered to investors, for example by allowing the funds to impose redemption restrictions during market stress. To evaluate the reform, we present two questions. First, did the reform decrease money-likeness of institutional prime MMFs as intended? Second, if it did, why did the funds still experience an investor run in March 2020?

Our contributions are two-fold. First, by using granular data on investors' monitoring activities, we provide broad empirical evidence that the reform decreased money-likeness of institutional prime funds. Second, we show that after the reform, investors may monitor and consequently run from institutional prime funds in order to avoid being hit by the new redemption restrictions. Such potential pre-emptive run motives suggest that despite the decreased money-likeness, the reform may not have improved the stability of institutional prime funds as much as intended. In fact, with the redemption restrictions, the reform may have created new type of incentives to acquire information, and subsequently run from institutional prime funds.

Money market funds are open-ended investment funds that invest in short-term debt with low credit risk. MMFs can be divided into two broad categories based on their investment objective.

Prime funds invest primarily in debt issued by financial and non-financial corporations, while *government* funds invest in debt instruments issued by the U.S. government and government agencies. Traditionally, in the United States, MMF shares have been redeemable on demand at a fixed price of $1 per share (fixed net asset value, or NAV) and hence used by individuals and companies as liquid, money-like investments receiving very little investor attention.

As privately provided money-like assets, U.S. MMFs, and especially institutional prime funds—the riskiest type of MMFs targeted to institutional investors—have been subject to runs as more sophisticated and better informed investors have rushed to redeem their shares after the default of Lehman Brothers in fall 2008 (Kacperczyk and Schnabl, 2013; Schmidt et al., 2016) and during the eurozone crisis in 2011 (Chernenko and Sunderam, 2014; Gallagher et al., 2020). As a response to these experiences, the Securities and Exhange Comission (SEC) introduced a comprehensive regulatory reform, announced in July 2014 and implemented in October 2016. The primary objective of the reform is to decrease probability and severity of future runs on institutional prime MMFs. Indeed, in the SEC's press release in July 2014 SEC Chair Mary Jo White argues that "[The reforms] fundamentally change the way that money market funds operate. They will reduce the risk of runs in money market funds and provide important new tools that will help further protect investors and the financial system".[1]

Two elements of the reform have received most public attention. First, the reform enables prime funds to restrict investor redemptions by allowing them to set redemption gates and liquidity fees. Such restrictions can be set when the share of liquid assets in the fund drops sufficiently low. Second, the reform requires prime funds targeted to institutional investors to quote their net asset value per share (NAV) based on market prices i.e. to adopt a floating instead of a fixed NAV. In other words, after the reform, payoff of institutional MMF shares is no longer stable, and receiving the payoff whenever needed is no longer guaranteed during market stress. Effectively, the reform makes the payoff profile of institutional prime MMF shares less similar to that of run-prone (uninsured) bank deposits and more similar to that of traditional investment funds. Both changes should reduce investors' incentives to try to redeem before others as gains from being the first-mover are lower than with fixed and immediately redeemable (yet risky and uninsured) payoff. In other words, the reform aims at making institutional prime funds potentially less run-prone by reducing their money-likeness.[2]

By definition, decreased money-likeness must be accompanied with increased incentives to engage in monitoring. In the first part of this paper, by using granular data on investors' monitoring activities, we show that, in line with its objectives, the reform indeed increased monitoring and

[1]For a summary of the objectives and key elements of the MMF reform, see the SEC's press release on the reform.

[2]In addition to the contractual changes, the reform includes several new disclosure requirements for all MMFs. For example, the reform removes the 60-day publication delay of form N-MFP, making N-MFP data more timely and thus, more valuable. Funds are also required to disclose some key metrics in their websites on a daily basis.

thus decreased money-likeness of institutional prime MMFs.

In our analysis, we proxy investors' demand for MMF portfolio information by using downloads of funds' regulatory reports in the SEC's EDGAR system. These files contain a record of each instance when someone downloads any regulatory filing submitted to EDGAR. Specifically, we measure downloads of form N-MFP, a detailed and standardized portfolio report that each fund must submit monthly. We separate algorithmic information gathering and human views, and calculate the number of distinct IP addresses that access each fund's regulatory reports monthly. This variable proxies the number of fund investors that decide to acquire information on riskiness of the fund in a given month. Because information on fund portfolios is available also on fund websites and from private data vendors, our measure provides a lower bound proxy for investor information acquisition.

Using a differences-in-differences specification, we estimate the differential increase in investors' information acquisition efforts towards institutional prime MMFs relative to retail prime and government MMFs before and after the SEC reform. Importantly, the reform took place in otherwise relatively stable conditions in financial markets. Thus, it can be seen as an exogenous shock to investors demand for MMF portfolio information: the developments within the typically very stable MMF industry are unlikely to be significantly affected by anything but the reform, such as unusual market developments. To capture changes in the steady state level of investor monitoring, we focus on funds that remain active throughout the reform period with the same fund type.

We find a significant and persistent increase in investor monitoring of *all* fund types around the reform implementation. Before the reform, the level of monitoring not only for the less risky government funds but also for riskier institutional and retail prime funds is very low; on average 3.5 unique non-robot and 41 robot IP addresses download N-MFP reports per fund and per month. However, investor monitoring picks up significantly for all fund types around the reform implementation. The post-reform monthly averages are 7.9 non-robot and 200 robot investors per fund, increasing by 125% and 388%, respectively.

The reform-induced increase in investor monitoring is particularly strong for institutional prime funds, especially when taking into consideration the concurrent reallocation of investor funds from prime to government MMFs. Despite the relative decrease in the average size of institutional prime funds, monitoring increases more in institutional prime than in retail prime (2.33 non-robot or 7.72 robot IPs) or government funds (0.85 non-robot or 9.36 robot IPs). To take into account the changes in the number of investors in each fund, we normalize the level of monitoring by dividing absolute measures by fund size (total net assets or its natural logarithm) and confirm that also such monitoring *intensities* increase significantly in institutional prime funds compared to other funds. This applies to both non-robot and algorithmic monitoring.

In contrast, we do not find robust evidence of increased monitoring of retail prime funds compared to government funds. While monitoring by robots increases more in retail prime funds than in government funds, we do not find a similar increase in non-robot monitoring. However, we note that even though there is no clear increase in non-robot monitoring of retail prime funds through form N-MFPs, retail investors may be more accustomed to use other data sources, such as fund web pages. If this is the case, monitoring through other channels may well have increased.

We perform various robustness checks to confirm our results. Because the reform may modify the investor base of funds, the increase in monitoring may be driven by changes in investor characteristics and fund risk taking rather than the elements of the reform itself. To tackle this concern, we control for changes in fund risk taking and investor sophistication. Also, we study monitoring *within* institutional prime funds and use feeder funds as an alternative control group for institutional prime funds. Feeder funds invest all their assets in so called master funds and hence, their N-MFP reports do not contain relevant information on portfolio risks of the (master) fund. We find that monthly monitoring of non-feeder funds increases by around two non-robot (or 17–19 robot) IPs more than that of feeder funds after the reform. In addition, our results are robust to alternative choices of the sample period. We also confirm that results are not driven by pre-existing trends in monitoring differences between fund types.

All in all, these results suggest that the reform increased monitoring of all MMFs, and that the effect is somewhat stronger for institutional prime MMFs. However, compared to the overall increase in monitoring, the magnitude of the differential effect on monitoring of institutional prime funds is relatively modest. This implies that rather than redemption restrictions and floating NAV, i.e. the contractual changes that are specifically designed to reduce run risk in institutional prime funds, improved availability and quality of information on MMFs may be the main driver of decreased money-likeness.

Finally, we study potential unintended consequences of the increased monitoring. Specifically, we ask if after the reform, investors monitor in order to avoid the new redemption restrictions. If so, institutional prime funds could be subject to so-called pre-emptive runs, in which better informed investors run because they anticipate a redemption gate or a liquidity fee and consequently, expect other investors to run too as suggested by Cipriani et al. (2014). In this case, decreased money-likeness would not be sufficient to prevent runs as the increased monitoring is not only a stabilizing but also a destabilizing mechanism.

Indeed, recent evidence from market turmoil related to Covid-19 in March 2020 suggests that despite decreased money-likeness institutional prime MMFs are still subject to runs, and that gates and fees may be a new source of vulnerability for MMFs. In particular, between March 11 and March 25, investors withdrew approximately USD 97 billion from institutional prime funds. The run stopped on March 18, when the Federal Reserve introduced the Money Market Fund Liquidity

Facility (MMLF), a backstop for MMFs. Li et al. (2020) and Cipriani and La Spada (2020b) show that withdrawals during the market stress were higher in funds that were more likely to impose gates and fees, suggesting that the threat of redemption restrictions may have amplified, rather than prevented, the run.

By studying investors' monitoring behavior, we provide further evidence of potential pre-emptive run motives due to the new redemption restrictions. In these analyses, similarly to Li et al. (2020) and Cipriani and La Spada (2020b), we exploit the share of weekly liquid assets (WLA), a regulatory metric introduced by the MMF reform. If the fund's WLA drops below 30%, it has an option to set redemption restrictions, namely redemption gates or liquidity fees. In particular, we conjecture that if any pre-emptive run motives exist, funds with lower levels of WLA (that are more likely to set restrictions) should be more monitored after the reform. In addition, we hypothesize that during the period of heavy outflows in March 2020, investors run when they anticipate a gate or a fee and expect other investors to run. In other words, outflows should be higher in illiquid and thus more monitored funds with already more run-prone investors.

We find that after the implementation of the reform, monitoring of institutional prime funds is targeted towards more illiquid funds. In particular, after the reform, a 10 percentage point decrease in WLA is associated with $0.6 - 0.7$ downloads more by unique non-robot IP addresses per month. The increase is non-negligible as the average level of non-robot investors that monitor institutional prime funds each month is around 10 after the reform. Furthermore, we show that similar to the run on institutional prime funds during the eurozone crisis, in March 2020, more sophisticated investors were more prone to run from funds that were subject to more intense monitoring. However, while in the eurozone crisis the more sophisticated investors run to avoid credit risk exposure to European issuers (Gallagher et al., 2020), in March 2020 their redemptions were significantly amplified by fund illiquidity. More specifically, we find that during the run in March 2020, 10 percentage point decrease in WLA is associated with an increase of 0.7 percentage points in average daily outflows from the share classes with more sophisticated investors. Or alternatively, sophisticated share classes residing in illiquid funds (below 40% WLA in the previous day) experience 1.58 percentage points higher daily outflows compared to similar share classes in liquid funds.

Since March 2020, potential pre-emptive run incentives created by the reform have received an increasing amount of attention by media (e.g. Financial Times, 2020), policymakers (e.g. Quarles, 2020) and researchers. Cipriani et al. (2014) present the theoretical motivation for such runs. In addition, Li et al. (2020) and Cipriani and La Spada (2020b) show that outflows during the Covid-19 crisis were amplified by the possibility of gates and fees. In contrast, we show that the threat of gates and fees drives not only outflows during the run but also monitoring, suggesting that the increased post-reform monitoring of investors may have a destabilizing—rather than a stabilizing—effect. We are not aware of any other paper that would study the relationship between investor information acquisition and the probability of imposing gates and fees.

In addition, to our knowledge, this is the first paper to use investor monitoring data to estimate the effect of the U.S. MMF reform on the money-likeness of the institutional prime MMF shares. We are aware of only one other paper that discusses investor monitoring in the context of the MMF reform. Specifically, Baghai et al. (2020) show graphically that the TNA-weighted quarterly downloads of MMF prospectuses and N-MFP reports increased more for institutional than retail non-government funds after the reform. We confirm their finding that monitoring of institutional prime funds increases more than that of retail prime funds and provide several important extensions. First, while Baghai et al. (2020) use CRSP mutual fund data to identify which funds and fund types are monitored, we build our sample of MMFs by using regulatory filings (form N-MFPs). Our approach provides a perfect one-to-one mapping between SEC EDGAR log files and our fund-level data and allows us to study the relationship between a rich set of different fund-level characteristics and monitoring. Furthermore, in addition to retail prime funds, we use government funds as a control group. Including government funds in our analysis is important because 1) they are the only MMF type not affected by the set of new regulations and 2) the differential effect between institutional and retail prime funds is likely affected by their different investor bases with different information acquisition costs and information sources. Finally, by using feeder funds as an alternative control group, we confirm that increased monitoring is related to increased value of information rather than lower information acquisition costs of institutional prime fund investors.

Furthermore, this paper is closely related to the research on the effects of the U.S. MMF reform of 2014–2016. Cipriani and La Spada (2020a) show that prime MMF investors require a yield premium after the reform and link this with decreased money-likeness of prime MMF shares, and show that the elasticity of substitution between prime and government MMFs decreases due to the reform. Baghai et al. (2020) argue that the reform makes prime funds less money-like and hence, fund flows become more sensitive to performance and consequently, prime funds increase their risk-taking. We complement these papers by showing that the mechanism suggested by them, decreased money-likeness, indeed was in play during the MMF reform.

The theoretical premise behind our work is the information-based view of money, starting from Gorton and Pennacchi (1990) and recently developed by e.g. Holmström (2015), Dang et al. (2015) and Dang et al. (2017). Our results provide support to these theories in the MMF setting. In addition to our work, empirical literature testing information-based theories of money includes, for example, Cipriani and La Spada (2020a) and Benmelech and Bergman (2018).[3] These authors study money-likeness by focusing on yield spreads and liquidity, while our approach is to quantify the intensity of information acquisition directly.[4]

[3] Dang et al. (2019) provide a broad review on the empirical literature related to information-based theories of money.

[4] In finance literature, our method of measuring investors' information acquisition through EDGAR log files has been used by several other authors; see e.g. Loughran and McDonald (2014), Lee et al. (2015), Bauguess et al. (2018), and Gallagher et al. (2020).

Furthermore, our paper contributes to the large body of empirical research on money market funds. Traditionally, the literature on MMFs has focused on prime fund risk-taking incentives and investor behavior during crises, such as the one in September 2008 (Kacperczyk and Schnabl, 2013; Schmidt et al., 2016; La Spada, 2018) and the eurozone crisis of 2011 (Chernenko and Sunderam, 2014; Gallagher et al., 2020). In this strand of literature, the closest papers to ours are Schmidt et al. (2016) and Gallagher et al. (2020) who both study the role of investor sophistication in prime MMFs and find that more sophisticated investors are more prone to run either due to their lower information acquisition costs or higher strategic complementarities in withdrawal decisions (incentives to mimic each others actions).[5] Especially Gallagher et al. (2020) show that funds with more sophisticated investors experience higher withdrawals related to eurozone credit risk as well as are subject to more investor monitoring during the euro crisis. In contrast, we provide evidence that the higher information acquisition and perhaps run incentives are not related only to crisis episodes with heightened credit and liquidity risk but also to the reform that alters the contract offered to investors.

The remainder of this paper is organized as follows. Section 2 describes the main features of the U.S. MMF industry and the reform of 2014–2016. Section 3 describes our data and key variables of interest, and Sections 4–6 present the main empirical results. Section 7 concludes.

2 Background

2.1 Money market funds, the reform of 2014–2016 and the run in March 2020

Money market funds (or money market mutual funds) are open-ended investment funds that invest in short-term debt securities with limited credit risk. The two main types of MMFs are *government* and *prime* funds. Government funds invest in Treasury and Agency securities and repos backed by those securities. In contrast, prime funds may invest also in highly-rated, short-term debt securities issued by financial and non-financial companies such as commercial paper and certificates of deposits.[6] With total net assets of more than USD 4,000 billion as of February 2020, the money market fund industry is an important provider of short-term credit and liquidity for corporations, and especially for financial institutions.

Traditionally, MMF shares in the U.S. have been widely used by companies and individuals as liquid, money-like investment. To a large extent, MMF shares look similar to bank deposits as they are redeemable on demand and, until the MMF reform of 2014–2016, at par value. Concerning the latter characteristic, MMFs are regulated under Rule 2a-7 of the Investment Company Act of 1940, which has allowed funds to use amortized cost accounting and so called penny rounding method.

[5] The significance of the latter channel is somewhat unclear because sophisticated investors are also more likely to be larger and thus more able to coordinate their actions during stress periods (Chen et al., 2010).

[6] In addition, there are municipality MMFs that invest in debt issued by states. We do not include municipality funds in our analysis as they are a relatively small and special subset of the U.S. MMF industry.

Consequently, the fund can quote its NAV at a fixed price of one dollar per share as long as its mark-to-market NAV does not differ from the fixed one more than 0.5%. If the portfolio value of a MMF drops below 99.5 cents on the dollar, the fund can no longer use the fixed NAV convention. This event is known as "breaking the buck".

Like traditional banks, also MMFs are prone to investor runs due to the maturity and especially liquidity mismatch of their assets and liabilities. In particular, investors have incentives to run in bad times as first-movers can avoid losses that are borne by shareholders that stay in the fund. This mechanism is amplified by the fixed NAV convention and the consequent risk of breaking the buck.

The most notable run event in the U.S. MMF industry took place in September 2008 when the Reserve Primary Fund broke the buck due to its (relatively small, less than 1.5%) exposure to Lehman Brothers debt securities. Investors quickly questioned the value of its portfolio and the fund lost two thirds of its liabilities in 24 hours, forcing it to suspend operations and initiate liquidation. Simultaneously, the panic spread to other prime MMFs, especially the ones targeted for institutional investors. The run only ended when the Treasury announced a temporary guarantee program for MMF liabilities and the Federal Reserve offered MMFs a liquidity backstop through the Asset-Backed Commercial Paper Money Market Mutual Fund Liquidity Facility (for details, see Duygan-Bump et al., 2013). Also, institutional prime MMFs experienced a somewhat milder run during the eurozone crisis as investors withdrew their investment from funds with higher eurozone exposures (Chernenko and Sunderam, 2014; Gallagher et al., 2020).

The experiences of past runs led to a series of regulatory reforms in the U.S. MMF industry, of which the most comprehensive one was announced in July 2014. Elements of the reform that have received the most public attention are the introduction of redemption restrictions and floating NAV. In particular, the reform gives all prime funds an option to introduce redemption gates and liquidity fees when the share of weekly liquid assets (WLA[7]) of the fund drops below 30%. Funds may prohibit redemptions temporarily for at most 10 days, and the liquidity fee can be at most 2%. Additionally, prime funds targeted to institutional investors are required to quote their NAV based on market prices (floating NAV). These two elements of the reform became legally binding in October 2016.

These contractual changes aim at reducing run incentives of prime and especially institutional prime fund investors. First, floating NAV should reduce first-mover advantage related to stable share value and the risk of breaking the buck. Second, gates and fees provide funds with an opportunity to stop the run when it occurs. More specifically, the possibility of redemption restrictions itself may discourage investors from running in the first place as the remaining investors do not

[7]Rule 2a-7 defines WLA as Treasury securities, Agency securities maturing within the next 60 days, and other securities maturing during the next 5 business days.

have to bear the negative externality imposed by the redeeming investors (Diamond and Dybvig, 1983).

While the objective of the reform, decreasing run risk, is widely accepted, the means have received criticism. For example, Hanson et al. (2015) notes that due to the low secondary market liquidity of MMF portfolio holdings, floating NAV may not remove the first-mover problem associated to fixed NAV. In addition, Cipriani et al. (2014) warn that the mere possibility of gates and fees may expose prime funds to pre-emptive runs, in which better informed investors run in order to avoid gates or fees. As a conclusion, the two contractual changes of the reform may not be sufficient to significantly remove run incentives of institutional prime fund investors.

In line with this view, the market turmoil related to Covid-19 in March 2020 suggests that institutional prime MMFs are still subject to runs. As shown in Figure 1, institutional prime funds lost approximately 30% of their assets between March 11 and March 25. At the same time, assets of retail prime funds remained relatively stable and assets of government funds increased significantly, approximately 21%.

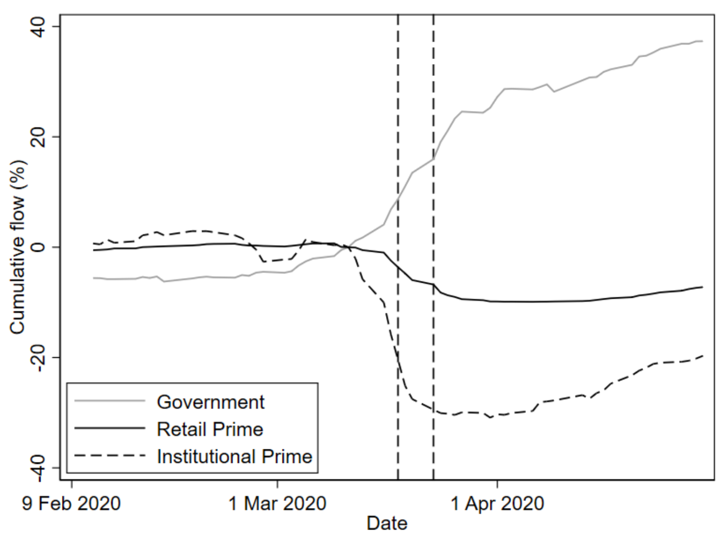


Figure 1: Run on prime MMFs in March 2020. The graph plots the cumulative change in total net assets for different types of MMFs compared to 11 March, 2020, when the run on prime MMFs started. Vertical lines denote announcement (18 March, evening) and implementation (March 23, morning) of the Federal Reserve's Money Market Mutual Fund Liquidity Facility (MMLF). Sample is all prime and government MMFs, and data covers dates from 4 February, 2020, to 30 April, 2020.

Importantly, Li et al. (2020) and Cipriani and La Spada (2020b) show that the fear of gates or fees likely amplified the run in March 2020. In particular, funds sold their most liquid assets in order meet the increasing number of redemption requests. As a consequence, the level of weekly liquid assets in many funds decreased, bringing them closer to the 30% WLA threshold that allows the fund to impose redemption gates and liquidity fees. And most importantly, the increased threat of gates and fees further increased incentives of investors to redeem their shares, making the run even worse than it would have been otherwise. The MMF market stabilized only after the Federal Reserve introduced the MMLF that enabled MMFs to sell also the more illiquid assets.

2.2 MMF shares as money-like assets

An asset is said to be information insensitive when agents have no incentives to acquire information about asset quality as they expect to be able to sell the asset at par whenever needed. Costly information production on payoffs of such assets is not profitable in normal times, and therefore they are immune to adverse selection (see e.g. Gorton and Pennacchi, 1990; Dang et al., 2017). The main benefit of information insensitive assets is that collective ignorance can facilitate efficient trade (Holmström, 2015), and thus such assets are often money or *money-like*. In other words, money-likeness and a low value of information production go hand in hand.

MMFs are a typical example of privately supplied money-like assets: they hold portfolios of low-risk debt, guarantee fixed share price, and allow immediate redeemability of shares. In particular, MMF shares are debt backed by debt, which Dang et al. (2015) show to be the most information insensitive type of contract. Given the stable and immediately redeemable payoff, not many investors should find it worthwhile to acquire information on fund risks or *monitor* the funds. Indeed, Gallagher et al. (2020) show that while investors selectively acquired information on prime MMFs' exposures to European banks during the eurozone crisis in 2011, they hardly monitor MMFs' portfolio risks in normal times.

Unfortunately, money-likeness does not come without a cost. A privately produced (risky) money-like asset can loose its money-likeness when investors' incentives to acquire private information on the underlying collateral suddenly increase. The result is a collapse in trade or in practice, a market freeze or an investor run. (Dang et al., 2015) In the case of MMFs, shares can serve as a medium of exchange if investors expect them to have stable value irrespective of the arrival of new information. But in crises, some investors begin to collect information and use it against others by withdrawing their funds before others. (Dang et al., 2019) Thus, while money-likeness and the lack of information production provide a platform for efficient trade for MMFs, they are also a potential source of instability as investors may suddenly question the value of the asset.

In line with this theoretical prediction, investor information acquisition plays an important role in runs on institutional prime MMFs. Gallagher et al. (2020) show that during the eurozone crisis in 2011, sophisticated investors started to selectively acquire information on MMFs' risk exposures to Europe. As a result, the more sophisticated investors ran from funds with high exposures to European credit risk, and fund managers counteracted this move by withdrawing funding from European issuers. In a similar vein, Schmidt et al. (2016) show that fund flows responded heterogeneously to the the default of Lehman Brothers because the more sophisticated investors withdrew funds more aggressively from institutional prime MMFs. To explain their empirical findings, the authors develop a model in which the degree of strategic complementarities in redemption decisions is increasing in the share of informed investors in the fund. Thus, larger and more sophisticated institutional investors are more likely to both 1) acquire information about fund risks and 2) respond more aggressively to fund portfolio risks.[8] All in all, empirical evidence on the run behavior of institutional prime MMF investors indicates that the runs can be interpreted as events in which funds suddenly became subject to more intense monitoring and consequently lost their money-likeness.

One interpretation of the the U.S. MMF reform of 2014–2016 is that it decreased the money-likeness of prime MMF shares by making them more information sensitive (see e.g. Dang et al. (2019), Cipriani and La Spada (2020b), Baghai et al. (2020)). Abandoning the fixed NAV convention means that institutional prime MMF shares may at any time drop in value. Furthermore, floating NAV implies that institutional prime MMF shares become more equity-like and no longer have a debt-on-debt structure, which Dang et al. (2015) shows to be the most information insensitive type of contract. Simultaneously, the threat of redemption gates and fees means that prime funds prohibit or restrict investor withdrawals if market conditions turn sour. To avoid investing when NAV is about to drop or gates/fees are to be imposed, investors may have incentives to acquire information on MMF portfolios and be aware of their underlying risks. Consequently, the reform potentially increases the value of information production, and thus decreases money-likeness of prime MMFs—especially institutional prime MMFs.

On one hand, the anticipated upside of the decreased money-likeness is that suddenly emerging incentives to start acquiring information on underlying risks should no more trigger a run in institutional prime MMFs. On the other hand, given criticism received by the reform and the run in March 2020, one may question whether the reform has decreased money-likeness of institutional prime funds enough or whether the decreased money-likeness is even a sufficient condition to shield the funds from runs.

In this paper, we empirically assess if the reform indeed decreased money-likeness of institutional prime MMF shares as intended. Because we cannot measure the value of information acquisition for investors, we study their monitoring activities before and after the MMF reform. Investors should monitor funds only when the payoff from acquiring information exceeds its cost. Thus, assuming unchanged information acquisition costs, an increase in monitoring after the reform implies that investors value fund information more and therefore MMF shares have become less money-like. In addition, in the latter part of this paper, we ask why do investors monitor institutional prime funds

[8] As shown by Hellwig and Veldkamp (2009), if redemptions decisions are strategic complements (as with MMFs at least with fixed payoff), investors have incentives to acquire information to "know what others know". Thus, the mechanisms likely reinforce each other in the build-ups to runs: more information gathering leads to stronger strategic complementarities, again increasing incentives to acquire information.

after the reform, and whether the monitoring itself may have contributed to the run of March 2020 by creating a possibility to run pre-emptively.

3 Data

3.1 Data sources

We collect data from three sources: monitoring data from SEC EDGAR log files, monthly fund-level data from monthly regulatory reports of MMFs submitted to the SEC's EDGAR database, and daily data on share class level flows during the Covid-19 induced market panic in March 2020 from iMoneyNet.

In most of our analyses, we look at drivers of and changes in investor monitoring. Therefore, our key variable of interest is the share of investors that monitor each fund. To measure this, we utilize the SEC's EDGAR log files. These files contain a record of each instance when someone accesses any electronic regulatory filing submitted to the SEC's EDGAR system. The EDGAR log files are a widely used data source for measuring investor attention and information acquisition (e.g. Loughran and McDonald, 2014; Lee et al., 2015; Bauguess et al., 2018; Gallagher et al., 2020). To distinguish human downloads from those of computer algorithms, we utilize the "robot" filter proposed by Ryans (2017). To fully understand the extent of both algorithm-based and manual information acquisition, we use both non-robot and robot downloads as complementary measures in our analysis.

We focus on the downloads of form N-MFP filings. Form N-MFP is a regulatory report that all U.S. MMFs are required to submit monthly.[9] Unlike less frequently updated filings such as prospectuses that contain more general fund information, monthly N-MFP filings are likely to be used for ongoing monitoring of fund risk and performance. In form N-MFP, funds disclose a large set of standardized information on their activities, such as fund type, size, performance, redemptions and subscriptions, and most importantly, a detailed, security-level list of their portfolio holdings.

We merge the monitoring data with fund-level information from N-MFP filings themselves. In particular, we collect monthly data on fund type, size, performance and portfolio composition. We merge this data with the log file data set by SEC Series ID number and reported fund type (prime/government) in form N-MFP. Furthermore, we classify each fund in each month either as institutional or retail based on their reported retail fund status. In the final monitoring data set, our sample period is from January 2015 to June 2017. We use data until June 2017 as the SEC EDGAR log files are available only until that point. The frequency of the data is monthly. The sample includes all U.S. prime and government money market funds.

[9] The content of form N-MFP was updated (and the name relabeled as N-MFP1) in April 2016 and further in October 2016 (N-MFP2). We refer to all of these versions of N-MFP as "form N-MFP".

Our data collection and sample construction process is further described in Appendix A.

3.2 Measuring monitoring

To measure investor monitoring, we calculate three complementary monitoring variables for each fund: $Monitoring$, $Monitoring/TNA$ and $Monitoring/\log(TNA)$. Specifically, $Monitoring$ is the number of distinct IP addresses that access N-MFP filings of the fund monthly. $Monitoring/TNA$ is $Monitoring$ divided by fund size, measured by the total net assets (in USDbn) of the fund. Similarly, $Monitoring/\log(TNA)$ is $Monitoring$ divided by the log of fund size (in USDmn). We calculate all variables by using both non-robot and robot IP addresses. We use the number of distinct IP addresses as opposed to the number of views as we think that this measure is better suitable for measuring the share of investors that monitor each fund. In other words, we want to count the number of viewers, not views. This distinction is particularly relevant, when the same IP address accesses multiple filings of the same fund in the same month.

$Monitoring$ measures the absolute level of monitoring, while the size-adjusted $Monitoring/TNA$ and $Monitoring/\log(TNA)$ aim at capturing monitoring intensity. Concerning the latter two variables, we would ideally like to adjust the number of IP addresses by the number of investors in the fund. Unfortunately, this information is unavailable in form N-MFP, and we need to opt for alternative proxies. In essence, $Monitoring/TNA$ measures the number of investors that download fund information per one billion of total net assets. The drawback of this measure is that an additional one billion of TNA is unlikely to be associated with similar increases in the number of investors for differently-sized funds. Larger funds are likely to cater investors with higher average investments, and hence the relationship between the number of investors and fund TNA is unlikely to be linear. Instead, dividing by $\log(TNA)$ assumes a concave relationship between the number of investors and fund portfolio TNA. We see this is a conservative estimate: for example, if a fund's TNA doubles from USD 100 million or USD 5 billion, $Monitoring/log(TNA)$ stays constant if $Monitoring$ increases by 15% or 8%, respectively.

While in theory the TNA-adjusted variables are better-suited for measuring share of monitoring investors, the main drawback is that the smallest funds often exhibit extremely high values for $Monitoring/TNA$. To some degree, the problem is alleviated by using $Monitoring/\log(TNA)$ for which funds exhibit more similar values regardless of fund size. To mitigate the concern of extreme values driving our results, we winsorize both TNA-adjusted monitoring variables at 2nd and 98th percentiles.[10]

Our source for monitoring data, SEC EDGAR log files, has three main limitations. First, while N-MFP filings represent the most comprehensive publicly available data set on MMFs, most of the same data can be obtained from other sources as well. These include private data vendors, such as

[10] Winsorizing reduces the magnitude of our key results.

iMoneyNet and Crane Data, and fund websites. In other words, downloads of N-MFP filings from the SEC's EDGAR represent a relatively small subset of demand for MMF information. Therefore, our monitoring variables should be seen as lower bound proxies of investor information acquisition.

Second, different types of investors likely use different information sources and this should be taken into account when interpreting our results. For example, downloading and analyzing regulatory reports or writing algorithms to collect information requires both financial sophistication and analyst capacity. For this reason, our monitoring measure is likely to capture monitoring by more sophisticated, institutional investors—and often as robot views. Similarly, as most retail investors likely rely on fund websites, their demand for information might be underestimated by our measures.

Finally, in addition to the contractual changes, the reform includes several new disclosure requirements for all MMF types, which may affect our results. These new disclosure requirements became binding in April 2016, six months before to the implementation of floating NAV and redemption restrictions for prime MMFs. Importantly, considering our monitoring variable, the reform removes the 60-day publication delay of form N-MFP, making N-MFP data more timely and thus, more valuable. It is possible that we observe increased investor monitoring just because the focus of investors' information acquisition efforts shifts from other data sources to form N-MFPs. This concern is alleviated by the fact that the reform includes several new web site disclosure requirements as well, making also the fund website data more valuable. In particular, each fund is required to disclose its NAV, net redemptions, and key liquidity metrics on a daily basis. However, we cannot rule out the possibility that the increase in investor monitoring is driven by the improved availability of information in general (rather than the contractual changes, namely gates and fees and floating NAV).

4 The effect of the reform on money-likeness of institutional prime MMFs: investor monitoring across fund types

In this section, we study the effect of the MMF reform on investor monitoring across different MMF types. The rationale for this analysis is that if the reform decreases money-likeness and thus increases the value of information in MMFs, the effect is strongest in funds that 1) are most affected by the reform, and 2) have investors with lowest information acquisition costs (per dollar invested). Institutional prime funds fulfill both of these criteria. Therefore, we expect that *the reform increases investor monitoring more in institutional prime funds than in retail prime or government funds.*

A potential source of bias in our results is the large number of fund closures and conversions around the reform implementation. In particular, 60% of prime funds that are active in November

2015 close, or even more likely, convert into government funds, by the end of November 2016. To ensure that we measure *changes* in monitoring, rather than just the effect of conversions from prime to government funds, whenever we use government funds as a control group, we focus on the balanced panel of funds that are active from January 2015 to June 2017 with the same fund type (prime or government) (*active* sample). This sample includes 62 prime funds and 115 government funds.

4.1 Descriptive evidence

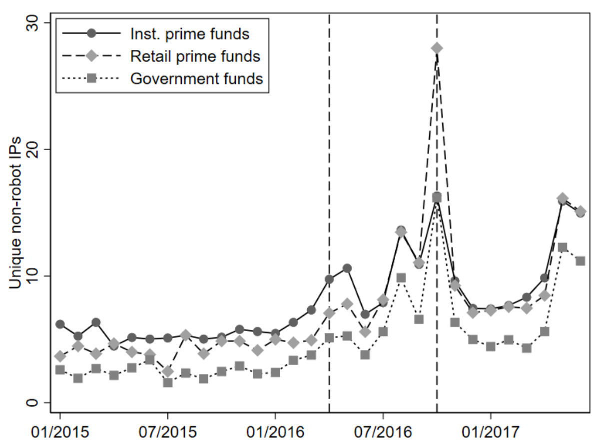
Starting with descriptive evidence, Figure 2 plots average values of $Monitoring$ for government, institutional prime, and retail prime MMFs. Until the beginning of 2016, both prime and government funds are subject to very little investor monitoring as the average number of distinct non-robot investors gathering information on MMFs hovers around five, four, and two per fund for institutional prime, retail prime, and government funds respectively. The same figures for downloads by distinct robot IPs are larger: around 45 for institutional and retail prime funds, and around five less for government funds. The low level of monitoring prior to the implementation of the reform is consistent with Gallagher et al. (2020), who show that investors hardly monitored MMFs in 2011–2012 outside the worst months of the euro crisis.

From April 2016 onwards, report downloads start to pick up for all fund types. As the actual implementation date of floating NAV and redemption restrictions is in October 2016, investors seem to anticipate the upcoming regulatory changes. The anticipation behavior makes sense especially for algorithmic information gathering; investors could prefer to set up and test their systems before the reform is fully implemented. An alternative explanation for the upward sloping trend in monitoring from April 2016 onwards is that investors may respond, albeit somewhat slowly, to the new disclosure requirements.

We are interested in persistent changes in (steady-state) monitoring rather than any exceptional ad-hoc information acquisition taking place in the run-up to the reform due to known or anticipated establishments, closures, and conversions of funds. While such information gathering is natural as investors study portfolios of new funds and changes in existing funds' assets, this monitoring is not related to changes in the MMF contract *per se*. Thus, in our empirical tests, we drop April–October 2016 from the sample to exclude monitoring that is driven by anticipation of the reform rather than the reform itself. In our robustness tests, we discuss how including data from April–October 2016 and all funds active at any point in time during January 2015–June 2017 (*all funds* sample) affects our results.

Information gathering remains at elevated levels after the implementation of the reform.[11] This suggests that the increase in monitoring is not solely driven by increased public attention to MMFs around the implementation date. The increase in the steady state level of downloads is striking especially for robot IPs. All in all, the graph indicates that monitoring of all types of MMFs increases significantly after the reform.

The increase in monitoring efforts towards not only prime but also government funds is interesting. Those funds do not lose the contractual features that preserve money-likeness, and remain investing in (nearly) risk-free securities. They could therefore be seen as information insensitive assets. However, by increasing quality and timeliness of information and general interest towards MMFs, the reform may have boosted demand also for portfolio information of government funds. Especially in the case of algorithmic monitoring, the marginal cost of downloading portfolio data for an additional fund is negligible, which may encourage some investors to collect information on all fund types.



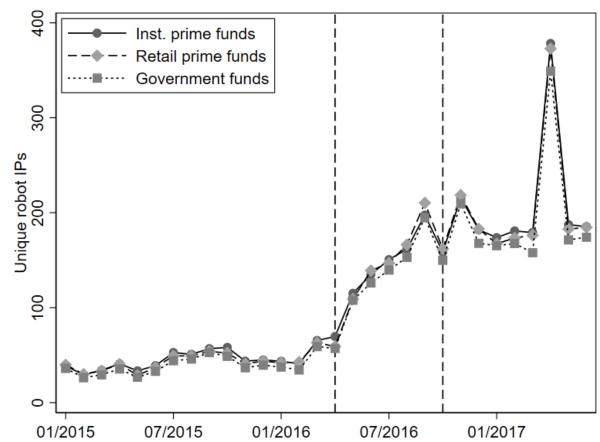


Figure 2: Fund monitoring: distinct IP addresses. The left (right) graph plots the monthly average number of distinct non-robot (robot) IP addresses viewing fund information by fund type. The vertical lines are April 2016 (increased information disclosure requirements for funds introduced) and October 2016 (floating NAV and redemption gates introduced). The sample consists of funds that remain active with same fund type during January 2015–June 2017.

Based on visual inspection of Figure 2 alone, it is not possible to infer if the effect of the reform on investor monitoring is strongest for institutional prime funds as suggested by the hypothesis that floating NAV and redemptions restrictions decrease money-likeness. However, as discussed in Section 2, concurrently with the acceleration of overall monitoring, the major reallocation of

[11]The number of distinct robot IP addresses accessing fund reports increases heavily in April 2017 for all fund types, concentrating especially on days between 12th and 21st of April. As there was no major market events during that time and the spike is present only for robot IP addresses (but not for robot page views, or non-robot views/IPs), we attribute the increase to technical factors. Luckily, in our econometric analysis, the increase is absorbed by month fixed effects as it is common to all fund types.

investor funds from prime to government MMFs takes place. Importantly, the average size of prime funds—and thus likely the number of investors in them—shrinks, while the opposite happens for government funds. Still, like shown in Figure 2, the absolute level of investor information acquisition in prime funds seems to increase at least as strongly as in government funds. Indeed, Figure 3 shows how the average level of fund size adjusted monitoring, $Monitoring/TNA$, grows significantly more in institutional prime than government and retail prime MMFs around the implementation of the reform, and the gap persists until the end of our data set. This anecdotal evidence supports the hypothesis that the reform increased monitoring and hence decreased money-likeness of institutional prime MMFs. Next, we proceed to test this hypothesis more formally.

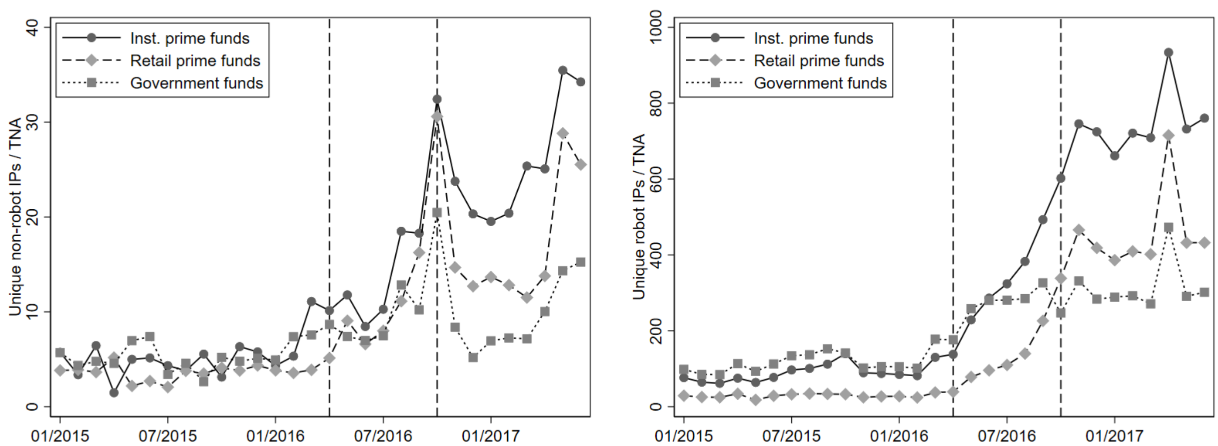


Figure 3: Fund monitoring intensity: distinct IP addresses. The left (right) graph plots the monthly average of number of distinct non-robot (robot) IP addresses viewing fund information divided by fund TNA (in USDbn) by fund type. The vertical lines are April 2016 (increased information disclosure requirements for funds introduced) and October 2016 (floating NAV and redemption gates introduced). The sample consists of funds that remain active with same fund type during January 2015–June 2017.

4.2 Regression results

To formally estimate the effect of the reform on investors' monitoring efforts towards different fund types, in the spirit of Cipriani and La Spada (2020a), we run the following differences-in-differences specification for active prime and government fund between January 2015 and June 2017 at the fund level:

$$y_{i,t} = \mu_{ij} + \delta_t + \beta_1 \times Prime_i \times Post_t + \beta_2 \times Prime_i \times Inst_{i,t} \times Post_t + \epsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is the level of monitoring for fund i in month t measured by either $Monitoring$, $Monitoring/TNA$ or $Monitoring/\log(TNA)$. Dummy $Post_t$ takes value of one after October 2016; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. To control for unobserved time-invariant differences in monitoring between funds with different clienteles, we include fixed effects for fund i and retail status j, μ_{ij}. Similarly, δ_t are month fixed effects that capture any (e.g. technology-driven) industry-level shocks to information acquisition.[12] We run the regression for the balanced panel of *active* MMFs between January 2015 and June 2017, excluding the reform implementation months April–October 2016.

Our interest is on β-parameters: β_1 measures the reform-induced change in monitoring in retail prime relative to government funds, and β_2 measures the additional effect for institutional funds. Thus, $\beta_1 + \beta_2$ gives the total effect of the reform on monitoring for institutional prime funds relative to government funds. If the contractual changes of the reform, namely floating NAV and redemption restrictions, increase monitoring and decrease money-likeness, we would expect estimates of these parameters should be positive and significant.

Table 1 presents descriptive statistics of our data separately for active prime and government funds. As shown already in the previous section, the pre-reform levels of monthly downloads of fund reports are overall quite similar across different monitoring variables and fund types. However, while the average monitoring intensity of prime funds doubles or triples after the reform, increase in the government fund monitoring is milder. On average, government funds increase and prime funds decrease in size in terms of total net assets after the reform. Unsurprisingly, government funds hold much more safe securities and liquid assets than prime funds.

A key for our identification strategy is that investors' demand for information in institutional prime versus retail prime and government funds in 2016–2017 is not driven by other factors than the elements of the MMF reform. In other words, we assume that monitoring differentials between institutional prime, retail prime and government would have remained constant without the reform. We argue that such assumption is warranted as the reform took place in otherwise relatively stable and tranquil conditions in financial markets, implying that monitoring decisions of MMF investors are unlikely to be affected by e.g. any unusual market developments in the U.S. MMF industry. In Section 4.3, we discuss the plausibility of the parallel trends assumption in our setting.

The results of regression (1) are in Table 2 for non-robot IP addresses. The dependent variable is $Monitoring$ in column (1), $Monitoring/TNA$ in column (2), and $Monitoring/\log(TNA)$ in column (3). Column (1) shows that due to the reform, monitoring of institutional prime funds increases by 2.33 and 0.85 non-robot IP addresses per month more than that of retail prime or government funds, respectively. In addition to the absolute level of monitoring, as shown in columns (2) and (3), also the TNA-adjusted monitoring variables increase more in institutional prime than in retail prime (second row) or government funds (third row). For example, the reform-induced

[12] Following Cipriani and La Spada (2020a) and La Spada (2018), we use standard errors from Driscoll and Kraay (1998) with 3-month lags that allow for heteroskedasticity, autocorrelation, and cross-sectional (or "spatial") correlation for up to three months. We calculate significance values using the fixed-b asymptotics of Vogelsang (2012) that should provide better approximations than using the usual normal or chi-squared distributions, in particular for joint hypothesis tests. This is a conservative choice; our main results are statistically less significant with the fixed-b approximation.

increase in non-robot monitoring per one billion of fund total net assets is 15.66 distinct IP addresses higher in institutional prime funds relative to government funds. All in all, estimated coefficients for β_2 and $\beta_1 + \beta_2$ are positive and statistically significant irrespective of monitoring variable, implying that institutional prime funds experience larger increase in investor monitoring than retail prime or government funds.

While institutional prime funds experience a moderate and statistically significant increase in monitoring irrespective of the dependent variable, monitoring of retail prime funds relative to government funds increases by only one measure: $Monitoring/TNA$ (8.00; the first row of Table 2). In our robustness checks in Appendix B.3, we show that this coefficient is driven by small (less than USD 1 billion in TNA) funds. Therefore, our interpretation of the results is that there is no significant change in the non-robot investor monitoring of retail prime funds compared to government funds. However, we emphasize that retail investors may be more accustomed to use other data sources, such as fund web pages. If this is the case, even though there is no increase in non-robot monitoring of retail prime funds through downloads of form N-MFPs, fund monitoring through other channels may still have increased.

Turning to the results for algorithmic information gathering in Table 3, as for non-robots, robot monitoring increases more substantially by all measures in institutional prime funds than other fund types (second and third row). However, also retail prime funds experience an increase in algorithmic monitoring compared to government funds, although the coefficient of $Monitoring$ is not significant (first row). A potential explanation for increased prime fund monitoring may be that some institutional investors who monitor funds in an automatized way do it for all prime funds, whether institutional or retail.

In general, the observation that algorithmic monitoring reacts to the reform differently across fund types is somewhat puzzling. Once the data collection algorithm has been set up, the marginal cost of monitoring an additional fund should be very low. Still, interestingly, the increase in automatized monitoring is most substantial in prime and especially in institutional prime funds.

In conclusion, both robot and non-robot monitoring and consequently the value of investor information acquisition increases most in the funds that are most affected by the reform—institutional prime MMFs—suggesting that the reform decreased the money-likeness of their shares. The same, however, does not apply to retail prime funds that experience an increase only in robot monitoring. In addition, the magnitude of the differential effect on monitoring of institutional prime funds is rather small compared to the overall average increase in monitoring. This suggests that rather than floating NAV and redemption restrictions, the new disclosure requirements imposed by the reform may be the main driver of decreased money-likeness of MMFs.

4.3 Robustness: Parallel trends in monitoring

As we employ a diff-in-diff specification, our central assumption is that the monitoring differences between institutional prime, retail prime and government funds satisfy the parallel trends assumption. That is, the increase in monitoring is not driven simply by trend growth in monitoring of institutional prime funds. Based on a simple graphical inspection of Figures 2–3, the development of our monitoring measures for different fund types seems almost identical before April 2016. Not only trends but also levels of monitoring are very close to each other, albeit exhibiting slight month-to-month volatility. To test the parallel trends assumption formally, we run the following regression:

$$y_{i,t} = \mu_{ij} + \delta_t + \sum_{q \neq Q2-2015} \alpha_{1,q} \times Prime_i \times Q_t^q + \sum_{q \neq Q2-2015} \alpha_{2,q} \times Prime_i \times Inst_{i,t} \times Q_t^q + \epsilon_{i,t}, \tag{2}$$

where Q_t^q are dummies equal to one if month t belongs to quarter q, and other variables are as in regression (1). α-coefficients measure the development of monitoring difference between prime and government funds relative to the omitted baseline quarter (Q2/2015).[13] Specifically, $\alpha_{1,q}$ estimates the difference between retail prime and government funds at quarter q, while $\alpha_{2,q}$ measures the additional difference for institutional funds. If α's are increasing or decreasing already before the second quarter of 2016, there are non-parallel pre-reform trends in information acquisition in prime and government funds. In contrast, if α's are stable, so are monitoring differences between fund types.

Results of regression (2) are in Figure 4 for different dependent variables $y_{i,t}$. Left- and right-hand side graphs use non-robot and robot IP measures, respectively; top row presents results for absolute monitoring measures, middle row for $Monitoring/TNA$ measures, and bottom row for $Monitoring/\log(TNA)$ measures. In general, the are no clear pre-reform trends in α's for either retail (α_1) or institutional funds ($\alpha_1 + \alpha_2$).[14]

Our main worry is that the baseline results that suggest increased monitoring of (institutional) prime funds (Tables 2–3) are driven by existing upward trend in prime fund monitoring. In the context of Figure 4, this corresponds to an increasing pre-reform trend in α-coefficients. Visually, such concern seems noteworthy especially for robot monitoring (top-right graph).

To address concerns of non-parallel rends, we verify our results by allowing for linear monitoring trends for institutional prime, retail prime, and government MMFs. To accurately identify pre-

[13] To better capture changes in underlying monitoring trends across fund types, we opt for quarterly (instead of monthly) dummies due to the low level and high relative month-to-month volatility of our monitoring measures.

[14] Even though there are no clear trends in pre-reform α-coefficients, some of them are statistically significantly different from zero. This shows that monitoring differences are somewhat volatile (despite lacking clear trends). The volatility seems to be present both before and after the reform, and is stronger for absolute monitoring measures (top row).

reform trends, we opt for a longer sample and use data since January 2011.[15] Specifically, we modify regression (1) by adding linear time trends for prime and institutional prime funds and estimate regression:

$$\begin{aligned} y_{i,t} = &\mu_{ij} + \delta_t + \gamma_1 \times Prime_i \times t + \gamma_2 \times Prime_i \times Inst_{i,t} \times t \\ &+ \beta_1 \times Prime_i \times Post_t + \beta_2 \times Prime_i \times Inst_{i,t} \times Post_t + \epsilon_{i,t}, \end{aligned} \tag{3}$$

where t denotes the number of months since January 2011. Other variables are as in regression (1). Results of regression (3) are in Tables 4 and 5 for non-robot and robot IP addresses, respectively. They are qualitatively similar to our baseline results in Tables 2–3. The addition of linear trends slightly decreases the estimated increase in absolute and log(TNA)-adjusted monitoring (coefficients in columns (1) and (3) decrease), particularly for robot IPs. Still, the monitoring increase for institutional prime funds remains positive and statistically significant (although the third row of the column (1) in Table 3 only just loses statistical significance). In any case, the result of increased institutional prime MMF monitoring is not driven by linear pre-reform trends.

In Appendix B, as additional robustness tests, we perform our diff-in-diff analysis with alternative samples and potentially confounding variables.

[15] Using a longer sample means that the data within which $Monitoring/TNA$ and $Monitoring/\log(TNA)$ are winsorized changes slightly compared to our baseline specification (where the sample starts in 2015). Our main results in Tables 2–3 are similar irrespective of the winsorizing sample.

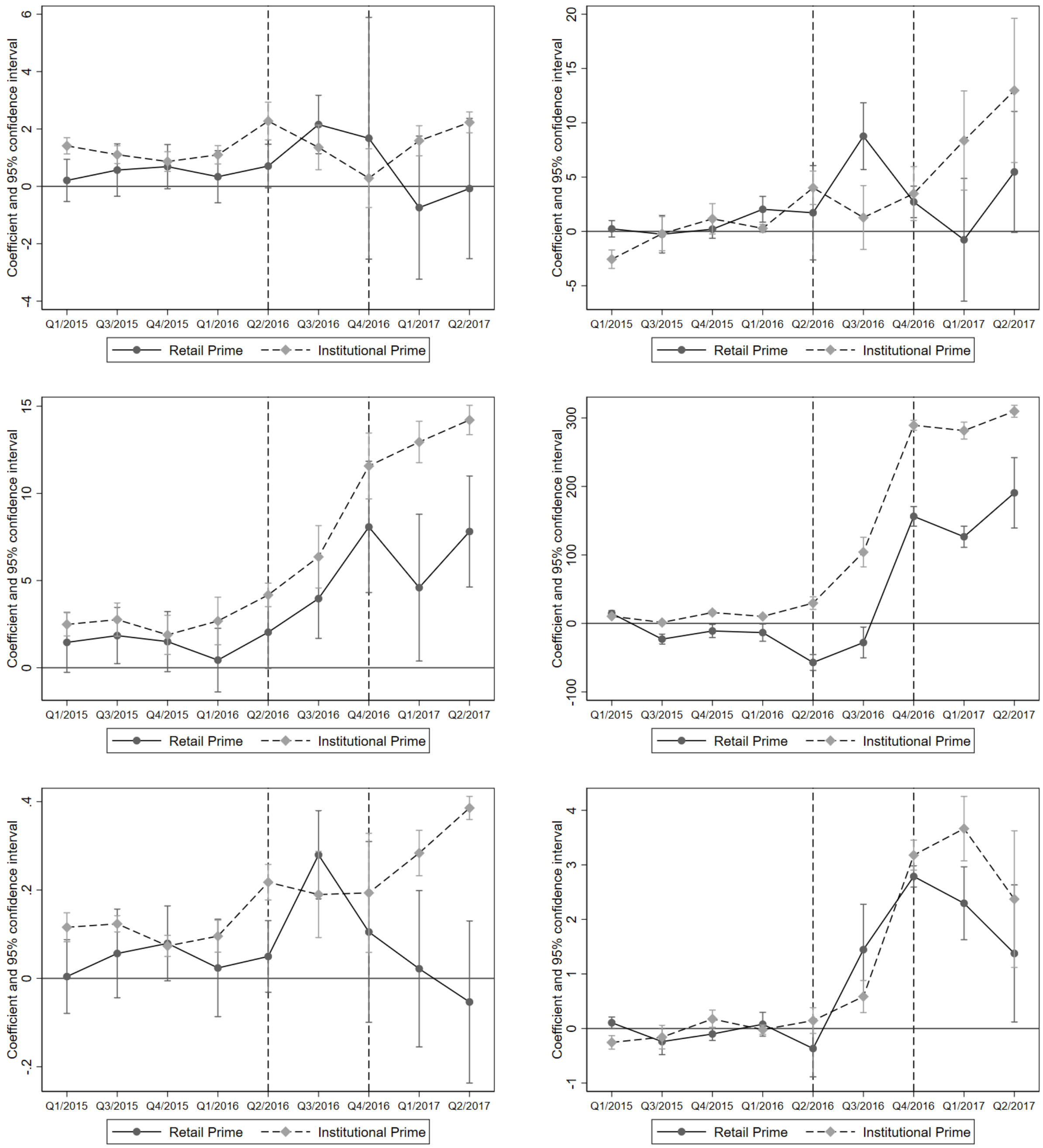


Figure 4: Pre-trends. Graphs plot α-coefficients of regression (2) for different dependent variables $y_{i,t}$. Left (right) column graphs: non-robot (robot) unique IP addresses. Upper row: absolute measures. Middle row: absolute measures divided by fund TNA (in USDbn). Bottom row: absolute measures divided by the log of fund TNA (in USDmn). *Retail Prime* (solid line) reports coefficients $\alpha_{1,q}$ and *Institutional Prime* (dashed line) reports coefficients $\alpha_{1,q} + \alpha_{2,q}$ for each quarter q. The baseline (omitted) quarter Q2/2015 is missing from the x-axis. Data is monthly from January 2015 to June 2017 and includes only *active* funds that existed continuously throughout the sample with the same fund type. The vertical lines are Q2/2016 (April 2016; increased information disclosure requirements for funds introduced) and Q4/2016 (October 2016; floating NAV and redemption gates introduced). All regression include month and fund-retail status fixed effects. Standard errors are from Driscoll and Kraay (1998) with 3-month lags and capped bars report 95% confidence intervals.

5 Increased value of portfolio information: monitoring within institutional prime funds

So far, we have documented that funds most affected by the reform experience higher increase in monitoring after the reform, implying that the reform increases value of information. However, another potential explanation for our findings is that the reform itself does not have an effect on (gross) value of information, but instead we may observe a differential effect on monitoring across fund types simply because after the reform investors in institutional funds become on average more sophisticated (i.e. have lower information acquisition costs) relative to investors in other fund types. This concern is relevant as the floating NAV requirement for institutional prime MMFs effectively separates institutional and retail investors into distinct funds (whereas prior to the reform an institutional fund may have had some retail share classes and vice versa). To rule out lower information acquisition costs as the sole driver of increased monitoring, we study monitoring *within* institutional prime funds i.e. within funds with broadly similar investor bases.

More specifically, we utilize feeder funds, instead of retail prime and government funds, as a control group. Feeder funds invest all their funds in a master fund, and hence, they are just intermediaries between the investors and the master fund. In all other analyses, we drop feeder funds as including both feeder and master funds would result in double counting. The reason to include them as a control group here is that N-MFP reports of feeder funds do not contain any relevant information on the riskiness of the (master) fund portfolio as the only asset of the feeder fund is its investment in the master fund. If the reform increases value of information production on fund portfolio risks, we should observe a differential response in monitoring between institutional prime non-feeder and feeder funds, as only the reports of the former provide information to assess portfolio risk. Hence, we test next whether *the reform increases investor monitoring more in non-feeder than in feeder institutional prime funds.*

Starting again with descriptive evidence, Figure 5 shows the number of distinct non-robot and robot IP addresses that download N-MFP reports for institutional prime non-feeder and feeder funds monthly. The sample is the unbalanced panel of institutional prime funds that are active in any month between January 2015 and June 2017. The sample includes in total 221 funds of which 30 are feeder funds. The results are similar if we include only funds that remain active throughout the sample period but such sample includes only five feeder funds. The pattern in Figure 5 is very similar to that of prime vs. government funds. Both feeder and non-feeder funds experience a surge in both non-robot and robot investor gathering after the implementation of the reform, and by mere visual inspection we cannot infer if the increase is stronger for non-feeder funds.

To formally measure the differential development of monitoring between feeder and non-feeder funds due to the MMF reform, we run the following differences-in-differences specification for the unbalanced panel of all institutional prime funds active in any month between January 2015 and

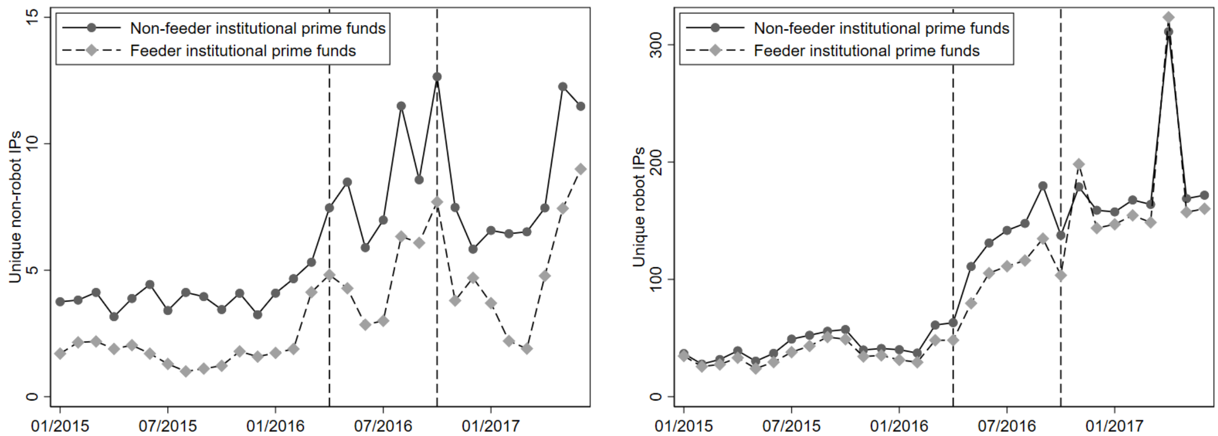


Figure 5: Fund monitoring: feeder vs. non-feeder funds. The left (right) graph plots the monthly average of number of distinct non-robot (robot) IP addresses viewing fund information for non-feeder (solid line) and feeder prime funds (dashed line). The vertical lines are April 2016 (increased information disclosure requirements for funds introduced) and October 2016 (floating NAV and redemption gates introduced). Sample is the unbalanced panel of all institutional prime funds that exist in any month between January 2015–June 2017.

June 2017:

$$y_{i,t} = \mu_i + \delta_t + \theta \times NonFeeder_i \times Post_t + \gamma \times Controls_{i,t-1} + \epsilon_{i,t}, \tag{4}$$

where $NonFeeder_i$ is a dummy equal to one for non-feeder funds and zero otherwise. Other variables are as regression (1). The interest is on the estimate for θ that we expect to be positive if monitoring increases more in non-feeder than feeder funds.

The results of regression (4) are in Table 6. Indeed, the reform increases monitoring more in non-feeder than feeder funds by around two non-robot investors or 17–19 robot investors.

6 Monitoring and pre-emptive runs

Results of the previous sections provide broad evidence that the reform decreased money-likeness of institutional prime funds. As money-like assets are inherently runnable, decreased money-likeness is a necessary condition for decreased run incentives. Hence, in this respect, our findings are in line with the key objective of the reform. Yet, as discussed in Section 2, the market turmoil related to Covid-19 in March 2020 shows that institutional prime MMFs are still subject to runs. So clearly, either the reform has not decreased money-likeness *enough* or decreased money-likeness is not a *sufficient* condition to prevent runs on institutional prime MMFs. In other words, either the reform has the intended effect but the magnitude of the effect is not significant enough, or alternatively, the reform does not have the overall effect it was intended to have.

Currently, the narrative of the March 2020 run is in line with the latter explanation—the reform may not work as intended. More specifically, and as described in Section 2, Li et al. (2020) and Cipriani and La Spada (2020b) show that instead of preventing the run, possibility of gates and fees may have encouraged investors to run pre-emptively.

In this section, we ask whether post-reform monitoring exhibits such pre-emptive run motives as well. The motivation to study monitoring in this context comes from theory. In particular, Cipriani et al. (2014) show with a simple three-period bank run model that the mere possibility of gates and fees creates a risk of a pre-emptive run. In such a run, not all but *informed investors* (those that learn the stochasticity of the final payoff and the payoff itself earlier than others) run after receiving a sufficiently poor signal of expected fund performance as they anticipate that the fund may impose a gate or a fee if sufficiently many investors withdraw. In other words, the pre-emptive run is not a sunspot run but a strategic response by those investors that have decided to put effort on information acquisition in order to exit the fund prior to potential redemption restrictions.

Consequently, we conjecture that after the reform, institutional prime fund investors monitor, and subsequently run in order to avoid gates and fees. Throughout the section, we utilize the fact that gates and fees can be set when the level of WLA in the fund's portfolio drops below 30%, and consequently, more illiquid funds are more likely to set redemption restrictions. We make two main predictions. First, we expect that funds that are more likely to impose gates and fees—more illiquid funds—are more monitored after the reform. Second, we expect that like in a theoretical pre-emptive run, in March 2020, investors run when 1) they expect gates or fees to be imposed based on their monitoring activities *and* 2) they expect other investors to run as well.

We start with descriptive evidence on the positive relationship between fund monitoring and illiquidity after the reform. In Figure 6, we take all institutional prime funds and divide fund-month level observations into quintiles based on portfolio illiquidity, measured by *Illiquid share*, defined as 100 minus the share of WLA in the fund's portfolio. Next, we calculate the average number of distinct IP addresses accessing N-MFP reports of a fund in the next month by quintile. We perform this analysis for two time periods, from January 2015 to March 2016 (pre-reform) and from November 2016 to June 2017 (post-reform). In line with our expectation, higher levels of monitoring are associated with higher illiquidity quintiles during post-reform period. In contrast, during the pre-reform period, quintiles do not differ in terms of monitoring.

Visual inspection does not show a similar relationship for robot monitoring and illiquidity. As shown in Figure 7, irrespective of time period, illiquidity quintiles do not seem to differ in terms of monitoring. In other words, unlike monitoring by humans only, robot monitoring is not concentrated on more illiquid funds. One explanation for this finding could be that algorithmic information gathering in general is not targeted towards specific funds as the main advantage of such information acquisition method is the ability to extend information collection from specific reports on a

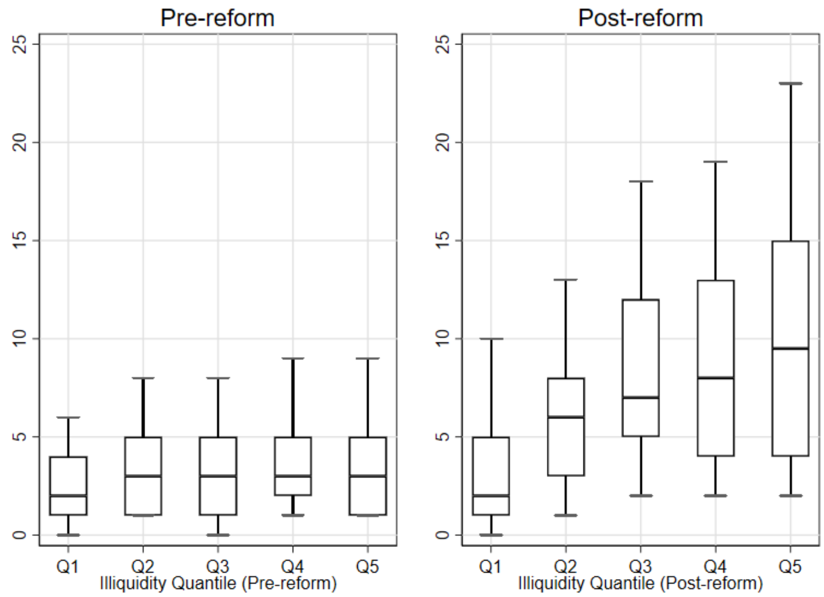


Figure 6: Non-robot monitoring and fund illiquidity. The graphs plot pre-reform (left) and post-reform (right) relationships between monthly average non-robot IP addresses that download fund information by quintiles of one-month lagged fund portfolio illiquidity. Observations have been divided into quintiles based on *Illiquid share*: the lowest 20% of observations are in Q1, the next 20% in Q2, and so on. *Illiquid share* is 100 minus the share of weekly liquid assets in the fund's portfolio in percentages. The graphs show 90th percentile (upper line cap), 75th percentile (box upper border), median (middle black line inside the box), 25th percentile (box lower border), and 10th percentile (lower line cap) of the respective quintile. Sample is the unbalanced panel of all institutional prime funds between January 2015 and June 2017. The left graph uses only pre-reform (until March 2016) data, and the right graph uses only post-reform (since November 2017) data.

larger set of funds. In contrast, non-robot monitoring may capture a more active decision to acquire information.

Next, we test more formally if fund illiquidity drives monitoring after the reform. More specifically, we run the following regression for all institutional prime funds active in any month between January 2015 and June 2017:

$$y_{i,t} = \mu_i + \delta_t + \beta_1 \times IlliquidShare_{i,t-1} + \beta_2 \times IlliquidShare_{i,t-1} \times Post_t + \gamma \times X_{i,t-1} + \epsilon_{i,t-1}, \tag{5}$$

where $IlliquidShare_{i,t-1}$ is $100 - WLA$ of the fund i in month $t-1$. Control variables include expense ratio (proxy for investor sophistication), fund size and holding risk (net share of risky assets). Our coefficient of interest is β_2, which measures the additional increase in reform-induced monitoring for one percentage point decrease in WLA. We are also interested in the magnitude of the sum $\beta_1 + \beta_2$ as it measures the sensitivity of monitoring to fund illiquidity *after* the reform.

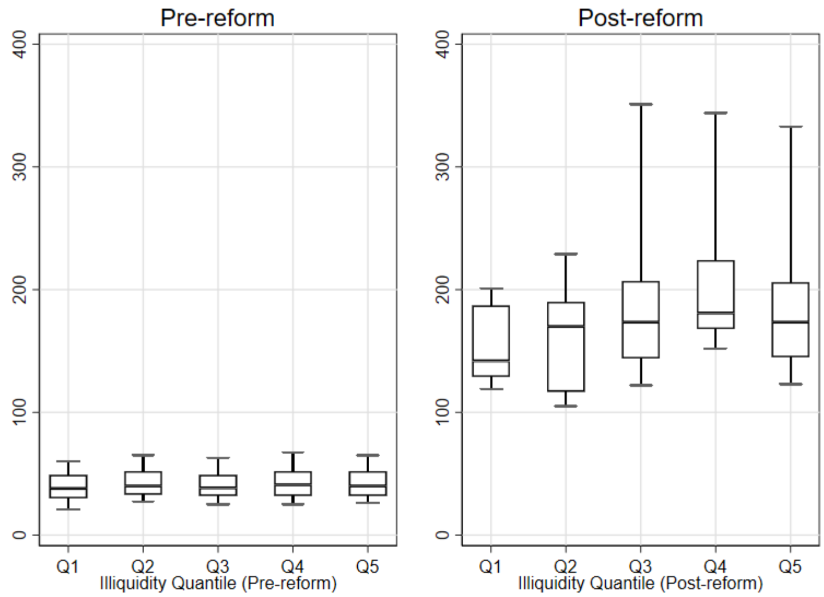


Figure 7: Robot monitoring and fund illiquidity. The graphs plot pre-reform (left) and post-reform (right) relationships between monthly average robot IP addresses that download fund information by quintiles of one-month lagged fund portfolio illiquidity. Observations have been divided into quintiles based on *Illiquid share*: the lowest 20% of observations are in Q1, the next 20% in Q2, and so on. *Illiquid share* is 100 minus the share of weekly liquid assets in the fund's portfolio in percentages. The graphs show 90th percentile (upper line cap), 75th percentile (box upper border), median (middle black line inside the box), 25th percentile (box lower border), and 10th percentile (lower line cap) of the respective quintile. Sample is the unbalanced panel of all institutional prime funds between January 2015 and June 2017. The left graph uses only pre-reform (until March 2016) data, and the right graph uses only post-reform (since November 2017) data.

Table 7 shows the results. Starting with non-robot monitoring in columns (1)–(2), as expected, prior to the introduction of gates and fees, fund illiquidity does not drive monitoring (β_1 in the first row small and insignificant). In contrast, the reform increases monitoring more in illiquid funds (β_2 in the second row positive and significant) and after the reform, fund illiquidity is positively associated with monitoring (sum $\beta_1 + \beta_2$ in the sixth row positive and significant). Specifically, after the reform, an increase of 10 percentage points in *Illiquid share* of the fund is associated with $0.6 - 0.7$ more non-robot investors monitoring the fund during the next month. As the average level of non-robot investors monitoring institutional prime funds per month is around 10 after the reform implementation, the magnitude of the effect is non-negligible.

Surprisingly, we also find a (barely) statistically significant relationship between algorithmic monitoring and fund illiquidity (columns (3)–(4), sixth row). However, given the larger average magnitude of monthly robot monitoring at around 150–200 IPs per fund, the size of the effect ($2.5-3.5$ IPs more for each 10 percentage points of portfolio illiquidity) is economically insignificant. Furthermore, in unreported tests, we run regression (5) separately for pre- and post-reform samples, and the association between post-reform robot monitoring and fund illiquidity disappears. In contrast, the non-robot result is unaffected.

We confirm that after the reform, investor monitoring is not sensitive to only portfolio illiquidity in general but redemption restrictions imposed by the WLA threshold matter. Table A9 in the Appendix replicates Table 7 but measures portfolio illiquidity by *Daily illiquid share*, equal to 100 minus daily liquid assets, instead of *Illiquid share*. Lagged daily portfolio illiquidity is positively associated with higher non-robot investor monitoring already before the MMF reform, and importantly, the relationship does not change after the reform. In particular, monitoring becomes more responsive to portfolio illiquidity after the reform only to the extent that the illiquidity is related to WLA and hence the redemption restrictions.[16]

Overall, these results indicate that after the reform, rather than the overall monitoring by both robots and humans, a deliberate decision to monitor a certain institutional prime fund is positively associated with illiquidity of the fund's assets—and consequently the probability of liquidity gates and fees. Such relationship is in line with the hypothesis that investors monitor in order to exit prior to redemption restrictions.

If investors acquire information in order to avoid gates and fees, do they also act based on that information? In other words, do monitoring and subsequent anticipation of gates and fees have an amplifying effect on runs on institutional prime funds as in the theoretical model of pre-emptive runs? To answer this question, we analyze outflows from institutional prime funds during the Covid-19 induced market stress in March 2020. Here, we utilize the earlier research on MMFs showing that the more sophisticated have in previous runs been the primary source of outflows (Schmidt et al., 2016; Gallagher et al., 2020). In particular, we ask if the run in March 2020 was (again) initiated by these more sophisticated investors and if the outflows of these investors were higher in more illiquid and thus more monitored funds.

Contrary to other parts of this paper, where we utilize SEC log file data and monthly regulatory reporting, in this analysis we use data from iMoneyNet because unlike form N-MFP, iMoneyNet provides daily data on fund flows and characteristics. Data on flows at a daily, rather than monthly level is essential here as, like shown in Figure 1, the run on institutional prime funds lasted only approximately a week and a half. Because iMoneyNet provides data on share classes with daily frequency and expense ratios—our proxy for investor sophistication—vary across share classes (rather than funds), we run the following analyses at share class level. In our share class-level analyses, we exclude share classes with assets less than 10 million USD in assets as their flows tend to be very volatile in percentage terms, or alternatively, some of them are inactive in the sense that their

[16]In unreported results, we also control for *Daily illiquid share* in regression (5). Compared to results in Table 7, the estimated coefficients for β_2 and $\beta_1 + \beta_2$ decrease but remain statistically significant. In this specification, our concern in the possible multicollinearity between *Illiquid share* and *Daily illiquid share*.

assets remain unchanged for weeks or months. The sample includes in total 76 share classes in 33 funds.

In Figure 8, we divide all institutional prime share classes into four bins based on their level of investor sophistication and illiquidity of the underlying portfolio. As more sophisticated investors are likely to have on average lower expense ratios due to their larger investment amounts, we use (low) expense ratio as a proxy for investor sophistication similarly to, for example, Schmidt et al. (2016) and Witmer (2019). $HiSoph_{i,t-1}$ is a dummy that equals 1 when the expense ratio of the share class i is below 20 basis points in day $t-1$. *Illiquid* is a dummy that equals 1 when WLA of the fund (of the share class) is less than 40% and 0 otherwise. We divide share classes into bins each day based on *HiSoph* and *Illiquid* of the previous day and then calculate the percentage change in aggregate assets of each bin during March 11 and March 25. The graph shows that 1) within illiquid (liquid) funds, more sophisticated share classes experience higher cumulative outflows; 2) within more (less) sophisticated share classes, outflows are higher for more illiquid fund portfolios, and most importantly; 3) since March 18 but before the start of the MMLF on March 23, outflows of more sophisticated investors seem to be more sensitive to illiquidity of the fund. In Figure 9, we repeat the same analysis with average share class level daily flows (instead of cumulative flows). All in all, graphical evidence suggests that outflows from institutional prime share classes in March 2020 are driven by the more sophisticated investors like in earlier runs on MMFs, and that these outflows are amplified by fund illiquidity.

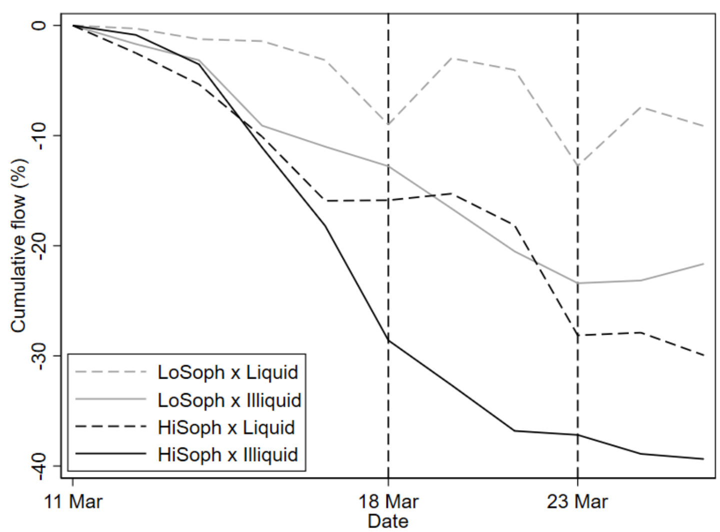


Figure 8: Cumulative flows: the role of illiquidity and investor sophistication. The graph plots the cumulative change in share class assets compared to 11 March, 2020, for different bins of share classes. A share class is defined as *Illiquid* if the fund where it resides has WLA below 40%; a share class is defined as *HiSoph* if its expense ratio is below 20 basis points. Bins are rebalanced daily. Vertical lines denote announcement (March 18, evening) and implementation (March 23, morning) of the Federal Reserve's Money Market Mutual Fund Liquidity Facility (MMLF). Sample is all institutional prime MMF share classes with over 10 million USD in assets, and data covers dates from 11 March, 2020, to 25 March, 2020.

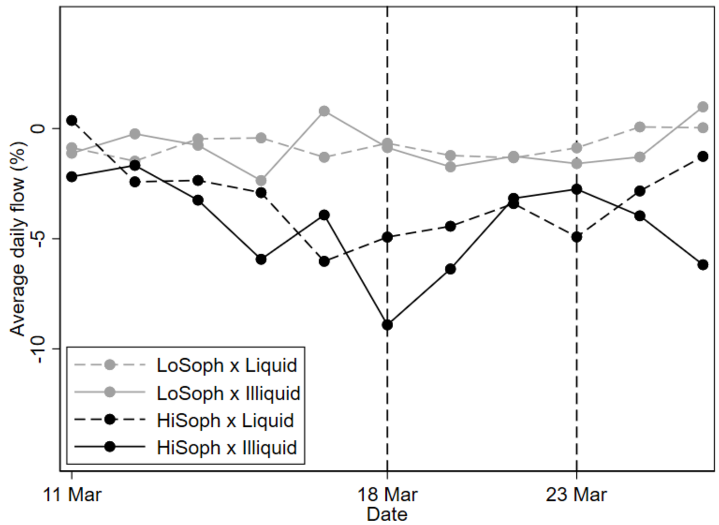


Figure 9: Daily percentage flows: the role of illiquidity and investor sophistication. The graph plots the average daily change in share class assets compared to the previous day for different bins of share classes. Bins are rebalanced daily with most recent WLA figures. A share class is defined as *Illiquid* if the fund where it resides has WLA below 40%; a share class is defined as *HiSoph* if its expense ratio is below 20 basis points. Bins are rebalanced daily. Vertical lines denote announcement (March 18, evening) and implementation (March 23, morning) of the Federal Reserve's Money Market Mutual Fund Liquidity Facility (MMLF). Sample is all institutional prime MMF share classes with over 10 million USD in assets, and data covers dates from 11 March, 2020, to 25 March, 2020.

As a more formal test of the role of fund illiquidity and investor sophistication during the run on institutional prime MMFs in March, we run the following regressions at share class-day level for all institutional prime MMF share classes by using data from iMoneyNet:

$$\begin{aligned} Flow_{c,t} &= \beta_1 HiSoph_{c,t-1} + \beta_2 HiSoph_{c,t-1} \times IlliquidShare_{i,t-1} \\ &+ \beta_3\, LoSoph_{c,t-1} \times IlliquidShare_{i,t-1} + \omega \times X_{c,t-1} + \epsilon_{i,t}, \end{aligned} \tag{6}$$

and

$$\begin{aligned} Flow_{c,t} &= \beta_1 HiSoph_{c,t-1} + \beta_2 HiSoph_{c,t-1} \times Illiquid_{i,t-1} \\ &+ \beta_3\, LoSoph_{c,t-1} \times Illiquid_{i,t-1} + \omega \times X_{c,t-1} + \epsilon_{i,t}, \end{aligned} \tag{7}$$

where we use different measures of fund illiquidity. In both equations, $Flow_{c,t}$ is the percentage change in assets of share class c on day t[17], and $HiSoph_{c,t-1}$ (at share class level) are defined as earlier. $LoSoph_{c,t-1}$ is 1 when $HiSoph_{c,t-1}$ is 1 and 0 otherwise.[18] Control variables $X_{c,t-1}$ include

[17] We acknowledge that in addition to investor flows, the percentage change in share class assets includes also the revaluation of existing portfolio assets. Thus, as a robustness check, we run our regressions using the investor flow reported by funds in iMoneyNet. Our results are both qualitatively and quantitatively unchanged. However, several funds do not report flows to iMoneyNet, and hence we lose more than one third of observations by using this data. This leads to a loss of some statistical significance especially in Table 9.

[18] Our sample includes only two instances in which the sophistication status of a share class changes over time. This happens in the beginning of March when two funds modify their expense ratio by a few basis points around the

one-day lagged share class and fund assets, and holding risk. In regression (6), $IlliquidShare_{i,t-1}$ (at fund level) is a continuous measure of illiquidity of fund i at day $t-1$, defined as 100-WLA (in %). Instead, in regression (7), $Illiquid_{i,t-1}$ is a dummy equal to one if WLA of fund i at day $t-1$ is below 40%, meaning the fund is relatively close to the 30% threshold that allows it to impose redemptions restrictions. We run the regressions for two time periods, namely for the pre-crisis period, which is one month prior to the run, and for the crisis period, which we define to be from March 11 (the start of heavy outflows) to March 20 (the last day before the start of MMLF).

Given earlier studies on the role of investor sophistication in MMF runs, we expect β_1 to be negative and significant in regressions (6)–(7) if we do not include the interaction terms for investor sophistication and fund illiquidity (including the interaction terms in the regression changes the interpretation of β_1). However, our main coefficient of interest in both regressions is β_3. In regression (6), β_3 measures the additional outflow for one percentage point increase in $IlliquidShare$ (decrease in WLA) in the more sophisticated share classes; in regression (6), it measures the additional outflow from a sophisticated share class if the fund is also illiquid (has WLA below 40%). If the threat of gates and fees amplifies the run of the more sophisticated investors, β_3 should be negative and significant in both regressions.

Table 8 and Table 9 show the results of regressions (6) and (7), respectively. Regardless of the specification, in the pre-crisis period neither investor sophistication nor fund illiquidity drive share class flows. In contrast, like Figure 9 suggests, during the run episode, more sophisticated investor base and fund illiquidity are both associated with higher daily outflows (column (3) of Tables 8–9), although statistical significance varies across specifications. Importantly, column (4) of both tables shows that especially the more sophisticated investors run from more illiquid funds. Specifically, a decrease of 10 percentage points in the previous-day WLA is associated with 0.7 percentage point increase in daily outflow (Table 8). Similarly, sophisticated share classes residing in illiquid funds (below 40% WLA in the previous day) experience 1.58% higher outflows compared to similar share classes in liquid funds (Table 9). In contrast, outflows of the less sophisticated share classes are not sensitive to fund illiquidity neither before nor during the run (third row in columns (2) and (4) of Tables –9). These findings suggest that fund illiquidity *amplifies* (rather than triggers) a run in funds with already more run-prone investors.

In conclusion, the more illiquid funds are more monitored and also more exposed to runs by the more sophisticated investors. In other words, investors may monitor and later run in order to avoid being hit by redemption gates or liquidity fees. This suggests that investor monitoring has not only a stabilizing effect through decreased money-likeness but also a potentially destabilizing effect, enabling investors to run before redemption restrictions and making those restrictions less effective.

20bp threshold.

7 Conclusion

In this paper, we study the effect of the U.S. MMF reform of 2014–2016 on money-likeness and stability of MMFs by using granular data on investor monitoring. While all fund types are more monitored after the reform, we show that institutional prime funds—the funds most affected by the reform—experience a steeper increase in investor monitoring than other types of funds. The additional increase in monitoring of institutional prime funds implies that the reform increases value of information for investors of those funds. As low incentives to produce information is a necessary condition for money-likeness, our results suggest that institutional prime funds are now less money-like than prior to the reform. This finding is consistent with the objectives of the reform as well as with the earlier research on the effects of the reform.

Furthermore, we show that after the implementation of the reform, investor monitoring of institutional prime funds is targeted towards funds that are more likely to impose redemption restrictions. This suggests that investors may monitor funds in order to redeem their shares before redemption restrictions are set. Consequently, the first-mover advantage in investor redemptions may not have disappeared from MMFs as a result of the reform. If earlier investors monitored and ran to avoid investing in a fund that is likely to break the buck, now their objective may be to avoid gates and fees. From this perspective, it is not clear if the reform and subsequent increased monitoring has made institutional prime MMFs more stable than before.

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A Data collection and cleaning from SEC EDGAR

Data construction of our monitoring data set consists of three parts. First, we collect a list of all N-MFP filings submitted to the SEC's EDGAR system, including accession numbers (unique report level identifier), report dates, filing dates, and HTML and XML addresses. Second, by using the list of filings, we collect data from the N-MFP filings themselves (hereafter, N-MFP data). Finally, from SEC EDGAR log files, we choose each observation where the accession number matches any accession number in our list of N-MFP filings.

A.1 N-MFP data

N-MFP reports provide us with three separate data sets, namely fund, share class and portfolio security level data sets. Fund level data set includes the following key variables: accession number, report date, filing date, series ID, fund type, retail fund flag (from April 2016 onwards), feeder fund flag, TNA, and 7-day annualized gross yield. The main variables in the share class level data set are accession number, class ID, class-level TNA, minimum initial investment, and 7-day annualized net yield of the share class. Finally, portfolio security level data set includes accession number, issuer name, CUSIP, other unique identifier, asset class, maturity date, and value of the security holding.

We use the accession numbers to link variables from different data sets to each other. In our main data set, observations are at fund-fund type-month level. Whenever we aggregate share class level data to the fund level, we use class TNAs as weights. Similarly, when aggregating portfolio security level data to the fund level, values of the security holdings are used as weights.

After collecting the data, we perform some preliminary cleaning. In particular, to avoid double counting, in all of our analyses except in that n Section 5, we drop feeder funds that invest all their funds into so called master funds. In addition, if the reported TNA is negative, we set it to 0. We also drop share class TNAs if they do not sum up to fund TNA and there are more than one share class in the fund (if there is only one share class, we set share class TNA to fund TNA). Also, we drop filings that have later been amended. Furthermore, we exclude observations in which fund TNA is less than USD 1 million as these funds are unlikely to be active.

Next, we create several new variables. First, we use the reported fund type (Item A.10) to define three distinct fund types, namely government, prime and municipal and assign each reported fund type into one of the three categories. In our analyses, we consider only prime and government funds. While the reform applies to municipality funds as well, we exclude them from our analyses as they are a relatively small subset of the US MMF industry with very different risk profile (only state debt and high WLA levels) than prime funds—our primary fund type of interest.

In order to study effects of the reform, we have to be able to divide prime funds into institutional and retail funds. Prior studies typically divide funds based on the primary share class level retail-institutional designation. However, we need to be able to identify those funds that are affected by the reform and hence, we pursue the following procedure. From April 2016 onwards, funds start to report in form N-MFP whether they are a retail fund as defined in the regulation (Item A.10.a.).[19] We define a fund either as retail or institutional based on the value reported in this field. Prior to April 2016, we use the value reported in April 2016. This approach has two major limitations. First, before April 2016, our classification is retrospective and hence does not take into account potential changes in institutional-retail status of the fund. Second, if the fund exits from the sample prior to April 2016, its institutional-retail status cannot be defined. Due to the latter limitation, we lost observations for 28 out of 508 prime or government funds active in any month during January 2015–June 2017.

From share class level data set, we calculate share class TNA weighted net yield of the fund. Using this variable, we calculate expense ratio (fees) of the fund as the difference between gross and net yield. From portfolio security level data set, we calculate three key variables, namely the share of weekly liquid assets (WLA), the share of daily liquid assets (DLA) and holding risk (or net share of risky assets). We define WLA and DLA as in Rule 2a-7. In particular, a security is defined as a WLA if it is a Treasury security, an Agency security with remaining maturity less than 60 days, or any other security with remaining maturity less than five business days. In addition, a security is part of DLA if it is a Treasury Security or any other security with remaining maturity less than one business day. We define holding risk as the difference between the share of the riskiest asset class and the share of government (Treasury and Agency) debt or securities backed by government debt. From available asset classes in form N-MFP, we choose certificates of deposits as the riskiest asset class as it is closest to bank obligations, the risky asset class used in most of the empirical papers on MMFs that utilize iMoneyNet data (Kacperczyk and Schnabl, 2013; La Spada, 2018; Baghai et al., 2020; Cipriani and La Spada, 2020a). In unreported robustness tests, we also consider a broader measure of holding risk such that risky asset classes are certificates of deposits, commercial paper and asset-backed commercial paper. All our results remain qualitatively and quantitatively similar with this alternative measure.

A.2 SEC EDGAR log files

Our SEC EDGAR log file data set contains a record of each instance when someone looks at any N-MFP form submitted to SEC EDGAR. For each page visit, we observe the time stamp, IP address, and the accession number of the filing. The SEC EDGAR logs are available until the end of June 2017.

[19] According to Rule 2a-7, the fund is a retail fund if it has processes that aim at limiting the beneficial shareholders of the fund no natural persons.

As is standard in literature, we do not consider so called index page views as the index page only shows the list of available documents for each filing. Furthermore, we utilize the "robot" filter proposed by Ryans (2017) to distinguish human downloads from those of computer algorithms. Specifically, an IP address is categorized each day as *human* when all of the following conditions are met: the same IP address does not download 1) more than 25 filings per minute, 2) filings by more than three CIKs per minute, and 3) more than 500 filings per day. The robot filter is applied to the whole log file data set, not only to the downloads of N-MFP filings.

Using the accession number, we obtain the corresponding fund series ID and fund type from the N-MFP data set (before dropping amended filings) for each log file observation. Then, we aggregate the log file data and calculate the number of distinct IP addresses that download N-MFP filings of each fund ID-fund type pair per month. As a complementary measure, we calculate the number of downloads per month at fund ID-type level. We calculate both variables for non-robot IP addresses and robot IP addresses. The central assumptions here are that investors monitor portfolios, and portfolio composition is determined by fund type. For example, if a fund has converted from prime to government fund (but kept the same fund ID), we do not consider downloads of old prime fund N-MFP reports as monitoring of the new government fund as these two have fundamentally different portfolios. Finally, we merge the four monitoring variables with the N-MFP data set by series ID, fund type and month.

B Additional robustness tests

B.1 Robustness: different samples

Our baseline results use the balanced panel of *active* funds. Tables A1 and A2 report results for the unbalanced panel of all funds active in any month during January 2015–June 2017. Results are similar, albeit milder than with the active sample. However, these results are driven by data on closed and especially newborn funds after the reform, and hence this sample is not as well-suited to answer our research question as the baseline sample.[20]

Moreover, our baseline sample excludes the reform anticipation period months from April to October 2016. As discussed, we choose this sample to avoid measuring any exceptional one-off monitoring activities around the implementation date that are likely related to the reform rather than persistent fund monitoring. For comparison, we also show results that include the anticipation period April–October 2016 in the sample and run the following regression:

$$\begin{aligned} y_{i,t} = \mu_{ij} + \delta_t + \beta_1 \times Prime_i \times Post_{4-2016} + \beta_2 \times Prime_i \times Inst_{i,t} \times Post_{4-2016} \\ + \beta_3 \times Prime_i \times Post_{10-2016} + \beta_4 \times Prime_i \times Inst_{i,t} \times Post_{10-2016} + \epsilon_{i,t}, \end{aligned} \tag{8}$$

[20]This is particularly true for robot IPs that are probably more affected by fund closures and establishments due to lags in setting up algorithmic monitoring.

where dummies $Post_{4-2016}$ and $Post_{10-2016}$ take value of one after April 2016 and October 2016, respectively. In this specification, $\beta_1+\beta_2$ measure the reform-induced change in monitoring in retail prime relative to government funds, and $\beta_3+\beta_4$ measure the additional effect for institutional funds. The sum $\beta_1+\beta_2+\beta_3+\beta_4$ gives the total effect for institutional prime funds relative to government funds. The results are in Tables A3 and A4. Again, they are qualitatively similar to our baseline results—especially estimates of robot monitoring are practically unchanged across samples (Tables 3 and A4). However, as suggested already by Figure 2, the level of non-robot monitoring of retail prime but also government funds is particularly strong during the reform implementation period (notably in October 2016). This weakens our baseline result of increased non-robot monitoring of institutional prime funds.[21] However, this likely reflects one-off investor information gathering triggered by the implementation of the MMF reform, which we deliberately want to exclude from our analysis.

B.2 Robustness: Changes in investor base and fund risk-taking

As discussed in Section 2, institutional prime funds experience significant outflows around the reform implementation. Consequently, their investor base likely changes. This poses a challenge for our identification strategy as instead of the elements of the reform itself, the altered investor base may drive changes in monitoring. To tackle this concern, we include a set of lagged control variables in our analysis.

First, it is possible that after the reform, investors of institutional prime funds are on average less risk averse than before, increasing risk taking incentives of funds and consequently monitoring incentives of investors. As a proxy for fund risk taking we include *Holding risk* (the share of certificates of deposit minus the share of government assets in the fund's portfolio) in the set of controls. Second, prime funds decrease and government funds increase in size after the reform. As larger funds likely have broader investor base and therefore may be subject to more monitoring (in absolute terms), we control for fund size (log of TNA in USDmn). Finally, the remaining investors in institutional prime funds may be on average more sophisticated than prior to the reform, and sophisticated investors are more likely to monitor funds. Therefore, we include the expense ratio paid by investors (in basis points) as a proxy for investor sophistication to our set of controls.[22]

Tables A5 and A6 present the results with controls included. The coefficients of interest are qualitatively and quantitatively similar to our baseline specification without control variables.[23]

[21] Specifically, estimates in the second row of Table 3 are lower than those in the sixth row of Table A4 (institutional versus retail prime fund monitoring). A milder decrease in coefficients can be seen between the third row of Table 3 and the seventh row of Table A4 (institutional prime versus government fund monitoring).

[22] In the literature, expense ratio is a typical proxy for investor sophistication (see e.g. Schmidt et al., 2016). On average, more sophisticated investors reside in funds with higher investment minimums, which usually go hand-in-hand with lower expense ratios. As expense ratios determined are at the share class level, we aggregate the data to fund level by using share class TNA-weighted averages.

[23] We note that if potential changes in investor sophistication, fund risk-taking, or fund size are themselves caused by the reform, they are bad controls and should not be included in the analysis at all. To mitigate this concern, all

B.3 Robustness: fund size

As discussed earlier, the TNA-adjusted measures of monitoring tend to be higher for smaller funds. This poses the question whether the increase of monitoring measures is driven by these small funds. To test whether this is the case, we run an additional robustness test. Tables A7–A8 report results of regression (1) for a *large* funds sample—those *active* funds whose TNA is above USD 1 billion in both November 2015 and October 2016.

Starting with non-robot monitoring (Table A7), the main observation is that the magnitude of the estimated coefficients for $Monitoring/TNA$ are much lower than in the baseline results but the key coefficients (β_2 and $\beta_1+\beta_2$) remain positive and highly significant. In line with our baseline results, institutional prime fund monitoring still increases compared to other fund types (the coefficients in rows 2–3 are positive and significant). However, there is no change in the monitoring differential between retail prime and government funds (the first row coefficients are not significantly different from zero). Compared to the baseline results, the estimated coefficients in columns (1) and (3) remain practically unchanged. All in all, our interpretation is that these results confirm the broad-based increase in human investor monitoring of institutional prime funds but show that the change in the monitoring differential between retail prime and government funds is not significant.

For algorithmic monitoring, the results for *large* funds (Table A8) are qualitatively similar to our baseline results. The main differences are in magnitudes: most coefficients for the TNA-adjusted variables are smaller (columns (2)–(3)) but still positive and highly significant. In contrast, coefficients of $Monitoring$ in column (1) are higher. This is not surprising because the potential number of investors accessing fund reports is likely to grow with fund size. Overall, the interpretation is in line with our baseline results: relative to government funds, the reform boosts algorithmic information gathering in retail prime funds and, to an even greater extent, in institutional prime funds.

control variables are lagged by one month.

Table 1: Descriptive statistics. Descriptive statistics for prime and government funds. Data is monthly from January 2015 to June 2017 and consists *active* funds that exist continuously throughout the sample with the same fund type. *Non-robot IPs* (*Robot IPs*) is the number of distinct non-robot (robot) IP addresses that download N-MFP filings of the fund each month. *Non-robot IPs/TNA* (*Robot IPs/TNA*) is the same number divided by total net assets (TNA) of the fund (in USDbn). *Non-robot IPs/log(TNA)* (*Robot IPs/(TNA)*) is the same number divided by the log of TNA of the fund (in USDmn). *Size* is TNA of the fund (in USDmn). *Expense ratio* is the difference between the fund's 7-day annualized gross yield and share class TNA weighted mean net yield in basis points. *Holding risk* is the share of certificates of deposit minus the share of government assets in the fund's portfolio in percentages. *Weekly/daily lliquid share* is 100 minus the share of weekly/daily liquid assets in the fund's portfolio in percentages. Definitions of weekly and daily assets are from Rule 2a-7. In particular, daily liquid assets include Treasury debt and assets that mature within one business day. Weekly liquid assets include daily liquid assets, government agency debt that matures within 60 days and other assets that mature within the next five business days. *Gross yield* (*Net yield*) is the fund's 7-day annualized gross yield (share class TNA weighted mean net yield) in basis points. *Pre Mean* is the mean prior to April 2016; *Post Mean* is the mean after October 2016.

Prime funds	Mean	SD	P10	Median	P90	Pre Mean	Post Mean
Non-robot IPs	7.94	7.82	2.00	6.00	16.00	5.27	9.99
Robot IPs	111.31	83.33	33.00	73.00	205.00	44.65	209.40
Non-robot IPs/TNA	11.59	29.55	0.12	1.29	26.95	4.76	21.53
Robot IPs/TNA	271.21	755.00	1.23	19.32	554.57	74.94	617.38
Non-robot IPs/log(TNA)	0.96	0.78	0.22	0.72	2.16	0.60	1.31
Robot IPs/log(TNA)	14.98	12.23	3.63	9.98	33.14	5.62	29.12
Size	14,770	23,543	172	4,321	42,564	18,217	8,652
Expense ratio	24.00	19.29	0.00	19.91	52.00	19.17	30.07
Holding risk	5.62	25.02	-28.06	11.67	32.38	7.45	6.73
Weekly illiquid share	47.03	15.04	25.91	51.16	61.66	51.97	43.84
Daily illiquid share	67.06	14.60	49.59	69.62	81.92	70.36	65.89
Gross yield	52.57	34.70	17.00	48.00	106.50	29.19	93.19
Net yield	28.34	30.91	0.00	16.56	78.00	10.02	63.12
Government funds	**Mean**	**SD**	**P10**	**Median**	**P90**	**Pre Mean**	**Post Mean**
Non-robot IPs	4.83	5.76	1.00	3.00	11.00	2.56	6.76
Robot IPs	102.80	78.80	29.00	63.00	193.00	39.17	195.61
Non-robot IPs/TNA	7.60	23.28	0.05	0.65	14.65	5.29	9.32
Robot IPs/TNA	204.66	601.54	1.98	21.11	388.89	116.50	316.87
Non-robot IPs/log(TNA)	0.60	0.63	0.10	0.40	1.35	0.33	0.83
Robot IPs/log(TNA)	13.45	10.64	3.45	9.69	27.70	5.45	24.72
Size	11,228	17,856	187	3,563	27,313	8,423	15,754
Expense ratio	20.04	15.87	4.00	18.19	40.00	12.13	31.05
Holding risk	-98.89	4.73	-100.00	-100.00	-98.14	-98.73	-98.97
Weekly illiquid share	9.44	11.17	0.00	3.20	26.35	10.58	7.34
Daily illiquid share	32.49	31.16	0.00	27.20	75.30	35.42	28.49
Gross yield	33.22	25.57	6.00	32.00	75.00	15.38	64.09
Net yield	13.18	19.44	0.00	2.02	42.00	3.25	33.05

Table 2: Non-robot IPs. The table reports results of regression (1), with the dependent variable $y_{i,t}$ being either unique non-robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or by log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is funds that have existed continuously from January 2015 to June 2017 with the same fund type (*Active*). Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	-1.48	8.00***	-0.06
	(0.82)	(1.38)	(0.10)
$Prime_i \times Inst_{i,t} \times Post_t$	2.33*	7.66**	0.35**
	(1.06)	(2.57)	(0.13)
$\beta_1 + \beta_2$	0.85**	15.66***	0.29***
Observations	4071	4071	4071
R^2	0.66	0.64	0.66
Within R^2	0.37	0.12	0.46

Table 3: Robot IPs. The table reports results of regression (1), with the dependent variable $y_{i,t}$ being either unique robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billion (2) or by log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is funds that have existed continuously from January 2015 to June 2017 with the same fund type (*Active*). Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	1.42	227.84***	2.68***
	(1.80)	(7.92)	(0.17)
$Prime_i \times Inst_{i,t} \times Post_t$	8.04***	211.93***	1.88***
	(1.57)	(10.72)	(0.25)
$\beta_1 + \beta_2$	9.47***	439.77***	4.55***
Observations	4071	4071	4071
R^2	0.98	0.68	0.93
Within R^2	0.97	0.19	0.91

Table 4: Non-robot IPs: linear trends. The table replicates results of Table 2 but allows for different linear monitoring trends for prime and institutional prime funds and uses a longer data sample. Specifically, the table reports results of regression (3), with the dependent variable $y_{i,t}$ being either unique non-robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billion (2) or by log of fund TNA in USD million (3). Data is monthly from January 2011 to June 2017, April–October 2016 excluded. The sample is funds that have existed continuously from January 2015 to June 2017 with the same fund type (*Active*). Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	-1.78*	4.25***	-0.11
	(0.87)	(1.15)	(0.09)
$Prime_i \times Inst_{i,t} \times Post_t$	2.31**	6.44***	0.30**
	(1.10)	(1.56)	(0.12)
$\beta_1 + \beta_2$	0.53	10.69***	0.18***
Observations	12339	12339	12339
R^2	0.61	0.56	0.59
Within R^2	0.26	0.08	0.36

Table 5: Robot IPs: linear trends. The table replicates results of Table 3 but allows for different linear monitoring trends for prime and institutional prime funds and uses a longer data sample. Specifically, the table reports results of regression (3), with the dependent variable $y_{i,t}$ being either unique robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or by log of fund TNA in USD million (3). Data is monthly from January 2011 to June 2017, April–October 2016 excluded. The sample is funds that have existed continuously from January 2015 to June 2017 with the same fund type (*Active*). Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	-1.01	184.08***	2.02***
	(2.47)	(8.17)	(0.32)
$Prime_i \times Inst_{i,t} \times Post_t$	9.60***	92.23***	0.98***
	(2.53)	(17.69)	(0.32)
$\beta_1 + \beta_2$	8.59***	276.31***	3.01***
Observations	12339	12339	12339
R^2	0.96	0.66	0.91
Within R^2	0.96	0.16	0.90

Table 6: Monitoring in non-feeder vs. feeder funds. The table reports results of regression (4). The dependent variable $y_{i,t}$ is either unique non-robot (columns (1)–(2)) or robot IPs (columns (3)–(4)) that download N-MFP report monthly. Even columns include control variables. Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is the unbalanced panel of all institutional prime funds active in any month between January 2015 to June 2017. Dummy $Post_t$ takes value of one from April 2016 onwards; dummy $NonFeeder_i$ is equal to one for non-feeder funds and zero otherwise. Controls: $Size_{i,t-1}$ is the log of fund TNA (in USDmn); $Holding\,risk_{i,t-1}$ is the share of certificates of deposit minus the share of government assets in fund portfolio in percentages; $Expense\,ratio_{i,t-1}$ is the difference between the fund's 7-day annualized gross yield and share class TNA weighted mean net yield in basis points. All regression include month and fund fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	Non-robot IPs		Robot IPs	
	(1)	(2)	(3)	(4)
$NonFeeder_i \times Post_t$	1.94***	1.95***	16.76***	19.24***
	(0.50)	(0.41)	(4.32)	(3.21)
$Size_{i,t-1}$		0.01		-3.03
		(0.24)		(1.54)
$Holding\,risk_{i,t-1}$		0.01		0.04
		(0.01)		(0.03)
$Expense\,ratio_{i,t-1}$		-0.03		-0.31**
		(0.02)		(0.12)
Observations	3496	3445	3496	3445
R^2	0.61	0.61	0.93	0.94
Within R^2	0.17	0.17	0.91	0.92

Table 7: Investor monitoring and portfolio illiquidity. Results of regression (5). The dependent variable $y_{i,t}$ is either unique non-robot (columns (1)–(2)) or robot IPs (columns (3)–(4)) that download N-MFP report monthly. Even columns include control variables. Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is the unbalanced panel of all institutional prime funds active in any month between January 2015 to June 2017. $Illiquid\,share_{i,t-1}$ is 100 minus the share of weekly liquid assets in the fund's portfolio in percentages. Dummy $Post_t$ takes value of one from April 2016 onwards Controls: $Size_{i,t-1}$ is the log of fund TNA (in USDmn); $Holding risk_{i,t-1}$ is the share of certificates of deposit minus the share of government assets in fund portfolio in percentages; $Expense\,ratio_{i,t-1}$ is the difference between the fund's 7-day annualized gross yield and share class TNA weighted mean net yield in basis points. All regression include month and fund fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	Non-robot IPs		Robot IPs	
	(1)	(2)	(3)	(4)
$IlliquidShare_{i,t-1}$	-0.01	-0.02	-0.18**	-0.16*
	(0.01)	(0.01)	(0.07)	(0.07)
$IlliquidShare_{i,t-1} \times Post_t$	0.08**	0.08**	0.53***	0.40***
	(0.03)	(0.03)	(0.15)	(0.11)
$Size_{i,t-1}$		0.19		-2.43
		(0.27)		(1.35)
$Holding\,risk_{i,t-1}$		0.01		0.06
		(0.01)		(0.03)
$Expense\,ratio_{1,t-1}$		-0.02		-0.27*
		(0.02)		(0.12)
$\beta_1 + \beta_2$	0.07**	0.06**	0.35*	0.24
Observations	2988	2986	2988	2986
R^2	0.62	0.62	0.93	0.94
Within R^2	0.18	0.18	0.91	0.91

Table 8: The Covid-19 run on institutional prime MMFs: share class characteristics. Table reports results of regression (6). The first two columns use data from 18 Feb–10 Mar (pre-run period); columns (3)–(4) use data from 11 Mar–20 Mar (run episode). The dependent variable is $Flow_{c,t}$, the percentage change in assets of share class c on day t. $IlliquidShare_{i,t-1}$ is $100 - WLA$ of fund i in day $t-1$. $HiSoph_{c,t-1}$ is dummy equal to one if share class c has expense ratio below 20bp on day $t-1$; $LoSoph_{c,t-1}$ is one when $HiSoph_{c,t-1}$ is one and zero otherwise. Controls include one-day lagged *Holding risk* (the share of certificates of deposit minus the share of government assets in fund portfolio in percentages), share class size (the log of share class TNA in USD mn), and fund size (the log of fund TNA in USD mn). Sample is all institutional prime MMF share classes with over 10 million USD in assets. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-day lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	Pre-run: Feb 18–Mar 10		Run: Mar 11–20	
	(1)	(2)	(3)	(4)
$HiSoph_{i,t-1}$	0.72	0.19	-1.88	1.87
	(0.61)	(0.41)	(0.69)	(0.97)
$IlliquidShare_{i,t-1}$	0.00		-0.03**	
	(0.01)		(0.01)	
$LoSoph_{i,t-1} \times IlliquidShare_{i,t-1}$		-0.00		-0.00
		(0.01)		(0.01)
$HiSoph_{i,t-1} \times IlliquidShare_{i,t-1}$		0.01		-0.07*
		(0.01)		(0.02)
Controls	Yes	Yes	Yes	Yes
Observations	1182	1182	591	591
R^2	0.00	0.01	0.07	0.08

Table 9: The Covid-19 run on institutional prime MMFs: share class characteristics. Table reports results of regression (7). The first two columns use data from 18 Feb–10 Mar (pre-run period); columns (3)–(4) use data from 11 Mar–20 Mar (run episode). The dependent variable is $Flow_{c,t}$, the percentage change in assets of share class c on day t. $Illiquid_{i,t-1}$ is a dummy equal to one if WLA of fund i in day $t-1$ is below 40%. $HiSoph_{c,t-1}$ is dummy equal to one if share class c has expense ratio below 20bp on day $t-1$; $LoSoph_{c,t-1}$ is one when $HiSoph_{c,t-1}$ is one and zero otherwise. Controls include one-day lagged *Holding risk* (the share of certificates of deposit minus the share of government assets in fund portfolio in percentages), share class size (the log of share class TNA in USD mn), and fund size (the log of fund TNA in USD mn). Sample is all institutional prime MMF share classes with over 10 million USD in assets. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-day lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	Pre-run: Feb 18–Mar 10		Run: Mar 11–20	
	(1)	(2)	(3)	(4)
$HiSoph_{i,t-1}$	0.72	0.82	-2.01*	-1.25
	(0.61)	(0.60)	(0.70)	(0.58)
$Illiquid_{i,t-1}$	-0.19		-0.71	
	(0.18)		(0.31)	
$LoSoph_{i,t-1} \times Illiquid_{i,t-1}$		-0.04		0.27
		(0.29)		(0.16)
$HiSoph_{i,t-1} \times Illiquid_{i,t-1}$		-0.31		-1.56**
		(0.26)		(0.42)
Controls	Yes	Yes	Yes	Yes
Observations	1182	1182	591	591
R^2	0.01	0.01	0.07	0.08

Table A1: Non-robot IPs: all funds. The table replicates results of Table 2 but includes all (instead of only *active*) funds. The table reports results of regression (1), with the dependent variable $y_{i,t}$ being either unique non-robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or by log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is funds that have existed at some point in time from January 2015 to June 2017. Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	-1.62	7.32***	-0.09
	(0.88)	(1.45)	(0.10)
$Prime_i \times Inst_{i,t} \times Post_t$	1.82*	2.02	0.26**
	(0.76)	(3.07)	(0.09)
$\beta_1 + \beta_2$	0.20	9.34**	0.17***
Observations	7535	7535	7535
R^2	0.58	0.61	0.58
Within R^2	0.28	0.07	0.36

Table A2: Robot IPs: all funds. The table replicates results of Table 3 but includes all (instead of only *active*) funds. The table reports results of regression (1), with the dependent variable $y_{i,t}$ being either unique robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or by log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is funds that have existed at some point in time from January 2015 to June 2017. Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	2.31	216.22***	2.63***
	(2.18)	(10.54)	(0.21)
$Prime_i \times Inst_{i,t} \times Post_t$	0.23	532.43***	3.11***
	(3.66)	(76.06)	(0.42)
$\beta_1 + \beta_2$	2.54	748.65***	5.74***
Observations	7535	7535	7535
R^2	0.93	0.80	0.91
Within R^2	0.91	0.25	0.87

Table A3: Non-robot IPs: anticipation period included. The table replicates results of Table 2 but uses all data from all months, including the reform anticipation period of April–October 2016. Specifically, the table reports results of regression (8), with the dependent variable $y_{i,t}$ being either unique non-robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017. The sample is funds that have existed continuously from January 2015 to June 2017 with the same fund type (*Active*). Dummies $Post_{04-2016}$ and $Post_{10-2016}$ take value of one from April 2016 and October 2016 onwards, respectively; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_{2016-04}$	1.07**	2.10**	0.15*
	(0.46)	(0.90)	(0.07)
$Prime_i \times Inst_{i,t} \times Post_{2016-04}$	-0.15	2.04***	-0.01
	(0.63)	(0.57)	(0.07)
$Prime_i \times Post_{2016-10}$	-1.14	6.69***	-0.11
	(1.44)	(1.44)	(0.13)
$Prime_i \times Inst_{i,t} \times Post_{2016-10}$	0.68	4.41	0.20
	(1.92)	(2.69)	(0.19)
$\beta_1 + \beta_3$	-0.07	8.80***	0.05
$\beta_2 + \beta_4$	0.53	6.45**	0.19
$\beta_1 + \beta_2 + \beta_3 + \beta_4$	0.46	15.25***	0.23***
Observations	5310	5310	5310
R^2	0.63	0.64	0.62
Within R^2	0.42	0.12	0.47

Table A4: Robot IPs: anticipation period included. The table replicates results of Table 3 but uses all data from all months, including the reform anticipation period of April–October 2016. Specifically, the table reports results of regression (8), with the dependent variable $y_{i,t}$ being either unique robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017. The sample is funds that have existed continuously from January 2015 to June 2017 with the same fund type (*Active*). Dummies $Post_{04-2016}$ and $Post_{10-2016}$ take value of one from April 2016 and October 2016 onwards, respectively; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_{2016-04}$	4.80	-65.47***	0.47
	(2.82)	(13.09)	(0.65)
$Prime_i \times Inst_{i,t} \times Post_{2016-04}$	-1.89	133.87***	0.06
	(3.91)	(30.49)	(0.44)
$Prime_i \times Post_{2016-10}$	-2.77	297.31***	2.31***
	(2.92)	(10.98)	(0.60)
$Prime_i \times Inst_{i,t} \times Post_{2016-10}$	8.35*	58.90	1.72***
	(3.65)	(38.26)	(0.44)
$\beta_1 + \beta_3$	2.03	231.84***	2.78***
$\beta_2 + \beta_4$	6.46**	192.77***	1.78***
$\beta_1 + \beta_2 + \beta_3 + \beta_4$	8.49**	424.61***	4.56***
Observations	5310	5310	5310
R^2	0.96	0.71	0.91
Within R^2	0.96	0.17	0.89

Table A5: Non-robot IPs: controls included. The table replicates results of Table 2 but includes control variables. Specifically, the table reports results of regression (1), with the dependent variable $y_{i,t}$ being either unique non-robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or by log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is funds that have existed continuously from January 2015 to June 2017 with the same fund type (*Active*). Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. Controls: $Size_{i,t-1}$ is the log of fund TNA (in USDmn); $Holdingrisk_{i,t-1}$ is the share of certificates of deposit minus the share of government assets in fund portfolio in percentages; $Expense\,ratio_{i,t-1}$ is the difference between the fund's 7-day annualized gross yield and share class TNA weighted mean net yield in basis points. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	-1.06	8.14***	-0.06
	(0.88)	(0.96)	(0.10)
$Prime_i \times Inst_{i,t} \times Post_t$	2.27*	9.10*	0.35**
	(1.01)	(3.82)	(0.14)
$Holding\,risk_{i,t-1}$	0.02	-0.38***	-0.00
	(0.01)	(0.09)	(0.00)
$Expense\,ratio_{i,t-1}$	-0.02***	0.19**	0.00
	(0.01)	(0.06)	(0.00)
$Size_{i,t-1}$	0.34***		
	(0.06)		
$\beta_1 + \beta_2$	1.22***	17.24***	0.29***
Observations	4069	4069	4069
R^2	0.66	0.66	0.66
Within R^2	0.38	0.17	0.47

Table A6: Robot IPs: controls included. The table replicates results of Table 3 but includes control variables. Specifically, the table reports results of regression (1), with the dependent variable $y_{i,t}$ being either unique robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or by log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is funds that have existed continuously from January 2015 to June 2017 with the same fund type (*Active*). Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. Controls: $Size_{i,t-1}$ is the log of fund TNA (in USDmn); $Holding\,risk_{i,t-1}$ is the share of certificates of deposit minus the share of government assets in fund portfolio in percentages; $Expense\,ratio_{i,t-1}$ is the difference between the fund's 7-day annualized gross yield and share class TNA weighted mean net yield in basis points. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	1.19	230.89***	2.69***
	(1.91)	(25.56)	(0.30)
$Prime_i \times Inst_{i,t} \times Post_t$	7.31***	267.76***	2.69***
	(1.47)	(38.99)	(0.41)
$Holding\,risk_{i,t-1}$	0.08*	-10.23***	-0.07***
	(0.03)	(2.29)	(0.01)
$Expense\,ratio_{i,t-1}$	-0.06	6.29***	0.07***
	(0.03)	(1.20)	(0.01)
$Size_{i,t-1}$	-0.18		
	(0.32)		
$\beta_1 + \beta_2$	8.50**	498.66***	5.39***
Observations	4069	4069	4069
R^2	0.98	0.71	0.93
Within R^2	0.97	0.24	0.92

Table A7: Non-robot IPs: large funds. The table replicates results of Table 2 but uses only *large* funds sample. Specifically, the table reports results of regression (1), with the dependent variable $y_{i,t}$ being either unique non-robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or by log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is funds that exist with the same fund tpye throughout the sample and have TNA above USD 1 billion in both November 2015 and October 2016 (*large*). Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	-0.95	0.29	-0.06
	(0.83)	(0.26)	(0.09)
$Prime_i \times Inst_{i,t} \times Post_t$	2.34	3.35***	0.33*
	(1.34)	(0.83)	(0.15)
$\beta_1 + \beta_2$	1.39*	3.64***	0.27***
Observations	2622	2622	2622
R^2	0.67	0.40	0.68
Within R^2	0.37	0.18	0.48

Table A8: Robot IPs: large funds. The table replicates results of Table 3 but uses only *large* funds sample. Specifically, the table reports results of regression (1), with the dependent variable $y_{i,t}$ being either unique robot IPs that download N-MFP report monthly (1), or the same number divided by either fund TNA in USD billions (2) or log of fund TNA in USD million (3). Data is monthly from January 2015 to June 2017. The sample is funds that exist with the same fund tpye throughout the sample and have TNA above USD 1 billion in both November 2015 and October 2016 (*large*). Dummy $Post_t$ takes value of one from April 2016 onwards; dummies $Prime_i$ and $Inst_{i,t}$ are equal to one for prime and institutional funds, respectively. All regression include month and fund-retail status fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	(1) Monitoring	(2) Monitoring/TNA	(3) Monitoring/log(TNA)
$Prime_i \times Post_t$	3.81	4.79***	1.06***
	(2.40)	(0.60)	(0.25)
$Prime_i \times Inst_{i,t} \times Post_t$	8.44***	60.94***	2.59***
	(2.10)	(7.75)	(0.21)
$\beta_1 + \beta_2$	12.26***	65.73***	3.65***
Observations	2622	2622	2622
R^2	0.98	0.45	0.96
Within R^2	0.98	0.24	0.96

Table A9: Investor monitoring and daily portfolio illiquidity. The table replicates results of Table 7 but replaces $Illiquid\ share_{i,t-1}$ with $DailyIlliquid\ share_{i,t-1}$. Specifically, the table reports results of regression (5). The dependent variable $y_{i,t}$ is either unique non-robot (columns (1)–(2)) or robot IPs (columns (3)–(4)) that download N-MFP report monthly. Even columns include control variables. Data is monthly from January 2015 to June 2017, April–October 2016 excluded. The sample is the unbalanced panel of all institutional prime funds active in any month between January 2015 to June 2017. $DailyIlliquid\ share_{i,t-1}$ is 100 minus the share of daily liquid assets in the fund's portfolio in percentages. Dummy $Post_t$ takes value of one from April 2016 onwards Controls: $Size_{i,t-1}$ is the log of fund TNA (in USDmn); $Holding\ risk_{i,t-1}$ is the share of certificates of deposit minus the share of government assets in fund portfolio in percentages; $Expense\ ratio_{i,t-1}$ is the difference between the fund's 7-day annualized gross yield and share class TNA weighted mean net yield in basis points. All regression include month and fund fixed effects. Standard errors (in parentheses) are from Driscoll and Kraay (1998) with 3-month lags and significance levels are derived using fixed-b asymptotics of Vogelsang (2012). * 10%, ** 5%, *** 1%

	Non-robot IPs		Robot IPs	
	(1)	(2)	(3)	(4)
$DailyIlliquidShare_{i,t-1}$	0.05***	0.04***	0.39**	0.30***
	(0.01)	(0.01)	(0.12)	(0.08)
$DailyIlliquidShare_{i,t-1} \times Post_t$	-0.00	-0.00	-0.16	-0.06
	(0.01)	(0.01)	(0.09)	(0.07)
$Size_{i,t-1}$		0.12		-2.60
		(0.28)		(1.50)
$Holding\ risk_{i,t-1}$		0.00		-0.00
		(0.01)		(0.03)
$Expense\ ratio_{1,t-1}$		-0.03		-0.34**
		(0.02)		(0.12)
$\beta_1 + \beta_2$	0.04*	0.04*	0.23*	0.24***
Observations	2988	2986	2988	2986
R^2	0.62	0.62	0.93	0.94
Within R^2	0.17	0.17	0.91	0.91

Bank of Finland Research Discussion Papers 2021

ISSN 1456-6184, online

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ISBN 978-952-323-366-9, online