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Repo Rate Spillovers: Evidence from a Natural Experiment

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Why These Findings Are Important

Securities dealers rely on the repurchase agreement (repo) market for short-term borrowing that is secured by a variety of types of security. The author examines a Federal Reserve policy shift that increased interest rates on repos secured by U.S. Treasuries to examine that impact of changes in one repo rate on the broader financial markets. When Treasury repo rates rose, the increase in funding costs differed across dealers and so did the impact on non-Treasury repo borrowing and on trading liquidity provided by dealers in non-Treasury assets.

Key Findings

1. **Higher Treasury repo rates functioned as a funding cost shock for dealers. Dealers paying lower repo interest rates experienced larger relative funding cost increases.**

2. **Repo borrowing contracted when repo rates rose, and more at dealers with larger increases in funding costs.**

How the Author Reached These Findings

In 2021, the Federal Reserve unexpectedly raised the offered rate for Treasury collateralized repos by 5 basis points with no other policy changes. The author uses this rate rise as an experimental design to see how changes in rates in one collateral class spill over into other collateral classes. The analysis utilizes confidential data from the U.S. tri-party repo market and transaction level data from non-Treasury securities markets. Dealer-level tests are performed using both of these datasets that are designed to see how dealers change their activity after the policy change.

REPO RATE SPILLOVERS: EVIDENCE FROM A NATURAL EXPERIMENT

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ABSTRACT. This paper explores how rises in funding costs in one asset class can spill over into other classes via the repurchase agreement market. More specifically, I inspect how a one-time policy induced exogenous increase in Treasury collateralized repo rates affected non-Treasury collateralized repo borrowing by dealers. Dealers were heterogeneously exposed to the rate rise due to their pre-period portfolio composition. More exposed dealers saw a 5% relative increase in the cost of borrowing in the repo market against non-Treasury collateral. This increase in funding costs came completely from an increase in repo rates, but dealers partially managed increased funding costs by lowering their total amount of repo borrowing. Rate rises are best explained by the asset class being used to collateralize the repo. However, the quantity declines are best explained by the dealer's total increased funding costs rather than increased funding costs for individual classes, suggesting that dealers transmitted the rate change to less directly affected asset classes following the policy shock. Affected dealers decreased their non-Treasury secondary market activity immediately following the policy shock, and this resulted in declines in profitability and increased bid-ask spreads. These results show that shocks originating in one specific repo collateral class can propagate to others through the dealer's balance sheet.

Keywords: Repurchase Agreement; Tri-Party Repo; Collateral; Rate Transmission

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1. Introduction

The U.S. repurchase agreement market (repo market) is one of the most important short-term funding markets in the world. As of August 2024, it has over $6 trillion of outstanding contracts, most of which is overnight and much involving one of the major U.S. primary dealers.[1] Primary dealers use this market to fund their activity in a variety of different asset classes, including Treasuries, Agency MBS, and corporate bonds (Hu, Pan and Wang, 2021; Paddrik, Ramirez and McCormick, 2021). While prior research has shown that repo funding cost fluctuations affect its collateral asset class (Macchiavelli and Zhou, 2022), it is less understood how rate fluctuations in one collateral class can affect rates and quantities in other classes.

This paper examines the interplay between the repo rates offered on different collateral classes and how rate changes in one collateral class can propagate to other classes. This study is motivated by previous periods of instability in the repo markets. Often, the repo rates on different asset classes move together, which is consistent with the rate propagation mechanism above. However, the markets for the collateral assets are typically also reacting to fundamental price shocks, so it is difficult to differentiate between rate volatility originating from the repo market and volatility that is driven by an economic shock affecting the secondary market for these assets simultaneously.[2] In this research, I use an exogenous change to a Treasury repo policy rate in order to see how this policy rate propagated into other asset classes. Since this change originated within the repo market itself, this allows me to disentangle secondary market price movements from the rate dynamics in the repo market.

To give a brief overview of the main results, I first show that a rise in Treasury repo rates does spill over to repo collateralized by other asset classes, but it impacts dealers heterogeneously based on their portfolio composition before the rate rise. More "exposed" dealers face higher relative funding cost increases, due to higher relative rates they pay on their repo. They manage increased funding costs by lowering their total quantity of repo, which shows that dealers are price-elastic with their borrowing demand. I show that the quantity declines were transmitted to all collateral on the dealer's balance sheet, not just collateral classes most directly impacted by the rise in Treasury rates. The ultimate

[1]Numbers are obtained from the New York Federal Reserve's Primary Dealer Statistics, which provides all outstanding repo agreements on primary dealer balance sheets on a weekly basis.

[2]A good example of this is in March 2020, when the repo rates on Treasuries and corporate bonds moved simultaneously. However, the secondary markets for these assets also were impacted by market instability caused by the Covid-19 pandemic.

consequence of lower non-Treasury repo volumes is lower non-Treasury secondary market trading volume, higher bid-ask spreads, and lower profitability at more exposed dealers. These results show the connection between the Treasury repo market and markets for other assets and suggest that heterogeneous exposure to policies targeting the repo market can have real consequences for other securities markets.

When showing these results, I will focus exclusively on dealers' non-Treasury repo activity in the tri-party segment of the U.S. repo market. The tri-party segment is the primary place for dealers' repo funding for non-Treasury collateral, and it is costly for dealers to obtain non-Treasury collateralized funding in other venues (Hu, Pan and Wang, 2021; Macchiavelli and Zhou, 2022)[3]. The tri-party market segment is also the primary repo segment where the Federal Reserve intervenes to assist with monetary policy implementation.

Historically, the Federal Reserve has set monetary policy rates by controlling the supply of bank reserves being traded in the Federal Funds Market. However, in 2008 the Federal Reserve shifted to setting its policy rates by putting ceilings and floors on rates in overnight funding markets. The floor is maintained by the Overnight Reverse Repo (ON RRP) Facility. The ON RRP facility is a place where money market funds can lend to the Federal Reserve at a fixed rate against Treasury collateral. Since cash ultimately enters the repo market through money market funds, this puts a floor on the borrowing costs of dealers and thus other rates in the broader repo market. Typically, large changes in the ON RRP rate merely reflect one-for-one changes to the Fed Funds target rate, but the Federal Reserve can make technical adjustments to this rate while keeping other policy rates fixed.

The Federal Reserve made one such adjustment on June 17, 2021, when they raised the ON RRP rate from 0 to 5 basis points. This immediately led to a rise in dealer funding costs on their repo activity. Since Treasury collateral is the safest form of collateral, its rate is a floor on the rates for all other forms of collateral in the repo market. Thus, dealers had to start paying more for both their Treasury and non-Treasury repo activity. Crucially for this study, dealers' Treasury repo portfolios were uniformly impacted by this policy. However, non-Treasury repo is often conducted well above ON RRP rate, and thus non-Treasury repo rates may be more insulated from technical adjustments.

[3] A full explanation of all the repo segments and their roles is beyond the scope of this paper. Figure 2 breaks down the different repo market segments and briefly describes them. Non-Treasury repo can only be done in three of the four repo segments and is essentially exclusive to the NCCBR and tri-party markets. Figure 1 shows primary dealer non-Treasury repo activity by type of activity and market segment. This figure shows that the tri-party segment is the main source of dealer non-Treasury repo funding.

To fix ideas, I compare the impact of the technical adjustment on dealers who were paying above average pre-adjustment rates on their non-Treasury repo activity to dealers who were paying below average rates. The former increased their rates after the adjustment by much less than the latter, and dealers paying below average rates lowered their non-Treasury repo activity noticeably after the adjustment took place (see figure 3). This suggests that the exogenous change in Treasury repo rates spilled over into non-Treasury asset classes by decreasing the relative quantity of repo activity performed by more impacted dealers.

I argue in this paper that the technical adjustment effectively amounted to a positive funding shock for dealers, and that certain dealers were more exposed to this policy than others. In order to do this, I create a dealer specific measure of pre-adjustment exposure to the higher Treasury repo rate being set by the technical adjustment. For each dealer, I look at their daily repo transactions over a 30 day period prior to the adjustment. For each transaction, I calculate the added dollar funding cost if the transaction had to be adjusted to *directly compete* with the new rate being offered on Treasury collateral. This added dollar funding cost keeps the quantity and haircut of the transaction fixed and calculates the added dollar cost if the repo rate rose to at least 5 basis points (b.p.), the new floor on rates set by the Treasury repo rate. Since Treasury collateral is the safest form of collateral, dealers should not charge less than the Treasury repo rate on non-Treasury collateral. However, dealers may have more flexibility with collateral classes with rates above the new rate being offered by Treasury collateral, and so for transactions trading above 5 b.p. the added funding costs is assumed to be 0.[4]

For each day, I create a measure called *gap*, which aggregates the transaction level added funding costs to see what the dealer would have had to pay on the aggregate in the new Treasury repo rate environment. This gap measure creates a cross-sectional measure of a dealer's direct exposure to the technical adjustment, where higher gap dealers would have had to raise their repo funding costs dramatically in order to keep the same quantity of borrowing. I show that there is significant heterogeneity in dealers' daily *gap*s before the adjustment, but that virtually all *gap*s immediately reverted to 0 afterwards. I take the average daily *gap* over the 30 days prior to the adjustment, which I refer to as the *FundingGap*.

[4]In reality, the rates rose even on repos trading above the 5 b.p. threshold, as seen in figure 3. However, dealers that were paying rates above 5 b.p. had to raise their rates afterwards by significantly less (also in figure 3). In this paper, I will remain agnostic to the direct mechanism for this and take it as given, but various mechanisms could include market power or flexibility in contracting terms away from the floor on rates created by Treasury repo.

This paper's core test will be a Difference-in-Differences test using *FundingGap* as a continuous treatment, which compares dealers based on how directly exposed they were to the policy change. The intuition for these tests is that dealers who are paying rates near 0, and thus have high *FundingGaps*, must raise rates on their entire repo portfolio a full 5 basis points in response to the policy change, while dealers who are conducting repo at rates significantly above zero may be able to insulate themselves from the higher funding costs. Put differently, *FundingGap* measures the cost to the dealer of directly competing with the higher Treasury repo rates.

I start by showing that dealers with higher *FundingGap* did, in fact, face relative higher funding costs after the adjustment by the Federal Reserve. I do this by running a difference-in-differences regression with the dealers' portfolio funding costs on a given day as the dependent variable, and the *FundingGap* measure interacted with a post-policy indicator as independent variables. Coefficients in this regression are positive, large, statistically significant at the 5% level, and robust to a variety of different fixed effect specifications. I next examine pre-trends by looking at the daily treatment effect in a 90 day window around the adjustment date. Results confirm that the relative increase in funding costs happened immediately upon June 17, with no significant pre-trends, and was sustained after the adjustment.

Next I decompose portfolio funding costs into constituent elements, namely repo rate, repo haircut, and total collateral quantity. Unsurprisingly, the increased funding costs come completely through high *FundingGap* dealers having to pay relatively higher repo rates. Repo haircuts were little changed for affected and non-affected dealers. Affected dealers, however, did lower their total quantity of repo in response to the exogenous increase in Treasury repo rates. Parallel trends tests confirm that both the rate increase and quantity declines happened almost immediately upon June 17, with no significant pre-trends or delay in the policy effects. I also show that affected dealers did not increase their Treasury repo activity to supplement their non-Treasury repo, suggesting that the quantity declines are a result of a total quantity decrease rather than a shift to Treasury repo.

I use non-parametric tests to quantify these results. Higher *FundingGap* dealers faced roughly 15% higher funding costs relative to dealers that faced no *FundingGap* at all. This was driven by a 3 b.p. relative increase in the rates they were paying, which amounted to an approximately 120% relative increase in their repo borrowing rates. Simultaneously, high *FundingGap* dealers borrowed approximately 10% less using non-Treasury repo as a result of the funding costs increase, although the number gets close to 25% for the highest

FundingGap dealers. This would imply that a 10% increase in borrowing rates leads to between 0.83% and 2% less borrowing in the repo market.

Next, I look at what caused the decline in borrowing. I examine two different hypotheses: first, that dealers treat collateral classes independently and thus the decline in borrowing is driven by higher borrowing rates on individual collateral classes, and second, that the dealer is more concerned with its entire total funding cost and decreases its total borrowing to reflect total cost increases. In order to compare these two possibilities, I break down the data to the dealer-collateral class level. Since the tri-party segment is general collateral[5], and thus contracting terms are determined at the class level, this allows me to separate the collateral borrowing costs from the balance sheet costs.

I run a difference-in-differences specification that splits dealers based on their additional cost of borrowing against each collateral class vs. their additional balance sheet cost of a dollar more of borrowing overall, each calculated as a function of their pre-adjustment portfolios. I find that the relative funding costs and rate increases are explained predominately by the increased cost of borrowing against different types of collateral. However, the quantity declines are explained by the dealer's total repo funding exposure. These results tell an intuitive story. Dealers must pay more on certain collateral classes in order to remain competitive with the new policy rate. Dealers then lowered their total quantity of repo due to increased borrowing costs. However, they lowered their repo borrowing across all collateral classes based on increased total funding costs, rather than increased funding costs in the classes directly impacted. This result suggests that dealers are relatively price elastic with their borrowing demand, but that this price elasticity is a function of the aggregate rates they are paying rather than the collateral they are using.

Next, I look beyond the repo market to the secondary market for non-Treasury securities. I hand match dealer *FundingGap* measures with transaction level Regulatory TRACE data to see how increased funding costs impact dealer behavior in the secondary market. I show that affected dealers lowered their total trading volume by relatively more than non-affected dealers, both in interdealer and client markets. They also experienced increased bid ask spreads and declining profits after the technical adjustment. These results are all consistent with previous literature that links secondary market liquidity to intermediary funding costs (Brunnermeier and Pedersen, 2008; Chung et al., 2017; Macchiavelli and Zhou, 2022).

[5]In the repo market, rates are either determined based on the individual asset being used as collateral or based on a broader asset class that this individual belongs to. The former is referred to a *specific collateral repo* and the lateral a *general collateral repo*.

This paper contributes to several different strands of literature. It is most directly related to the literature on the repo market. Previous research has highlighted the importance of the tri-party market segment as an ultimate source of funding for dealers (Huber, 2023; Krishnamurthy, Nagel and Orlov, 2014; Munyan, 2017). Much of this funding is short term, which can make the market fragile and subject to runs (Begalle et al., 2016; Gorton, Metrick and Ross, 2020; Martin, Skeie and Von Thadden, 2010). Most studies tend to focus on the Treasury portion of this segment, since it is the largest, but recent studies have looked more closely at other collateral classes, such as corporate bonds and equities (Hu, Pan and Wang, 2021), and previous studies on the 2008 financial crisis suggest that ABS and corporate bonds may have been more important as a marginal funding source than Treasuries (Copeland, Martin and Walker, 2014; Krishnamurthy, Nagel and Orlov, 2014). I study similar channels during a relatively stable period in the overall market and show how interconnected these different collateral classes can be. My results are novel in that I am the first to document how rate shocks can transmit between collateral classes, and to quantify these effects. While my results specifically apply to how exogenous changes to the U.S. Treasury repo rate affects funding costs in other asset classes, the mechanism I study is relevant for any situation where one collateral class is considered superior to another and thus creates a rate floor for the second asset.

This paper also contributes to the literature on bank funding costs. Traditionally, banks have primarily depended on consumer deposits to fund their activity, but recently they have begun to rely more on wholesale funding, which includes repo funding (Choi and Choi, 2021; Craig and Dinger, 2013). This added dependence on wholesale funding comes with its own unique financial stability risks (Carpinelli and Crosignani, 2021; Huang and Ratnovski, 2011; Perignon, Thesmar and Vuillemey, 2018). One unique aspect of wholesale funding markets, and particularly repo markets, is that there is no analogue to deposit insurance. Instead of insuring individual institutions, regulators seek to backstop the system by providing sources of liquidity that can be used in times of crisis. The consequence of this different market structure is that banks typically over collateralize their borrowing, and they often do this using the same securities they are making markets in (Duffie, 1996; Infante, 2019; Infante and Vardoulakis, 2020). I contribute to this literature by showing that this market structure allows for a unique form of rate transmission in the repo market, namely across collateral classes. My results show that changes in the repo rate in a specific collateral class can transmit to the secondary market for other collateral classes through dealer balance sheets in the repo market, which is an aspect of bank funding that is particularly salient in the wholesale funding market.

I also contribute to the literature on dealer funding costs and secondary market liquidity. Historically, determinants of market liquidity have been studied in market microstructure(Amihud, 2002; Glosten and Milgrom, 1985; Kyle, 1985) and determinants of funding costs have been examined in the banking and macroeconomics literature (Bernanke and Gertler, 1989; Diamond and Dybvig, 1983; Kiyotaki and Moore, 1997). More recent literature has integrated these two literatures, and has shown that market liquidity and funding costs are codeterminate (Bruche and Kuong, 2021; Brunnermeier and Pedersen, 2008; Macchiavelli and Zhou, 2022; Ma, Xiao and Zeng, 2022). My results are largely consistent with the hypotheses and results from this literature, as I show that funding constraints matter for measures of secondary market liquidity such as bid-ask spreads and total trading volume.

I proceed as follows: Section 2 discusses institutional details on the tri-party repo market and the policy experiment I use; Section 3 describes the methodology used; Section 4 describes the data; Section 5 presents the main results of the paper; and Section 6 concludes.

2. Institutional Background

2.1. **The Tri-party Repo Market and the ON RRP Window.** The U.S. repo market is split into four segments, defined jointly based on whether a repo is centrally cleared or the collateral is managed by a third party custodian bank. Certain aspects of the U.S. repo market have change in recent years, but this paper is concerned with the market as it existed in 2021.[6] For this reason, all descriptions and discussions will assume the market structure as in 2021. A full description of all four segments is given in figure 2.

The two centrally cleared segments, at this time, primarily existed as interdealer markets, where dealers swap cash and collateral to meet the needs of their clients in the two non-centrally cleared markets. Dealers primarily use the non-centrally cleared tri-party market segment(called simply tri-party) to borrow cash from money market funds, and this market operates similarly to a deposit market. The non-centrally cleared bilateral market segment is where dealers lend cash and securities to hedge funds, and serves as a source of profit for dealers, much like a small business lending market[7].

[6]For example, the Fixed Income Clearing Corporation introduced Sponsored GC since the main period studies in this paper, and this could impact lending into the tri-party repo market. However, it does not impact any of the analyses in this study, so will not be expanded upon

[7]More detail on each segment can be found in a series of briefs on the OFR's website (https://www.financialresearch.gov/briefs/). These briefs include in depth descriptions of the tri-party market segment (Paddrik, Ramirez and McCormick, 2021), the centrally cleared segments (Kahn and Olson, 2021), and the non-centrally cleared segment (Hempel et al., 2023).

The subject of this paper is the tri-party market segment. Dealers use this market for overnight and term funding, and borrow against both Treasury and non-Treasury collateral. This segment is a *general collateral* segment. This means that dealers and money market funds agree to terms based on collateral classes, rather than specific pieces of collateral[8]. The collateral classes include Treasuries, Agency RMBS, ABS, investment non-investment grade corporate debt, equities, and other categories. The tri-party segment is fairly self contained as a source of funding against non-Treasury collateral (Macchiavelli and Zhou, 2022), as the bilateral market has comparably less borrowing activity by dealers (Hempel et al., 2023).[9]

Since this segment serves as the most important source of overnight funding for dealers, it is the subject of much scrutiny and intervention by the Federal Reserve. This is partially by convenience, since this market is more transparent to regulators than the bilateral segments[10], but it is also because this market helps determine rates in other short term funding markets. Much Federal Reserve activity is related to short term rates in the tri-party segment, and the Federal Reserve considers it part of its normal monetary policy operations. One of its main tools is the *ON RRP window*.

Since the 2008 financial crisis, bank reserves have become ample, and so the Federal Reserve has primarily controlled interest rates by allowing outside options to market participants that simultaneously put both ceilings and floors on rates that market participants are willing to accept. The primarily tool for putting ceiling on rates is the IOER, and for the floor it is the ON RRP rate.

Crucially for this study, dealers do not lend into the ON RRP window. Instead, it is used by money market funds and dealers must compete with it. Money market funds cash lend to the Federal Reserve against Treasury collateral, so the window also does not directly interact with non-Treasury collateral either. Thus, any changes in ON RRP policy only impact dealer's non-Treasury repo portfolio through competitive forces rather than directly impacting their investment strategy.

[8]Specifically, they agree to rates based on collateral classes. Haircuts can be based on more granular categories, but they are also pre-arranged in a collateral schedule and are typically not decided on a repo-by-repo basis

[9]The bilateral market does contain a significant amount of dealer cash borrowing in the strict legal sense. However, much of this activity is, in reality, clients sourcing securities from dealers, so it is really a form of lending to clients rather than dealers borrowing to fund their balance sheet.

[10]Prior to 2014, the Federal Reserve Bank of New York facilitated a series of reforms in this market that increased visibility and data availability. See https://www.newyorkfed.org/medialibrary/media/tripartyrepo/pdf/report_120215.pdf

2.2. **The June 17, 2021 Technical Adjustment.** This paper is concerned with how repo rate changes in one collateral class can impact dealer repo decisions in other collateral classes. In order to explore this, there needs to be a plausibly exogenous shock to rates in one collateral class that was unrelated to rates in other classes. I am going to exploit an isolated change in the rate that the Federal Reserve offers at the ON RRP window to money market funds as this source of exogenous variation.

As mentioned above, the ON RRP serves as a floor on short term rates, and IOER a ceiling. ON RRP rates in the first half of 2021 stayed at the lower Fed Funds target rate, and IOER 10 basis points above that. Over this period, repo rates generally decline in all asset classes and even began going negative in the bilateral markets (Hempel and Kahn, 2021). The Federal Reserve became concerned that rates may be getting too low in short term funding markets and decided to adjust the ON RRP/IOER window upwards. During the June 15-16, 2021, FOMC meeting the Federal Reserve authorized the open markets trading desk at the New York Federal Reserve to raise the rate they offer at the ON RRP window by 5 b.p., up from zero (referred to as a technical adjustment). This change was not announced until June 17 and was unaccompanied by any other changes in other monetary policy rates, with the exception of the IOER which remained 10 basis points above the ON RRP offer rate. June 17 also served as the implementation date, so market participants were not made aware of the change prior to the implementation date.

Dealers that were paying rates on their repo that were below this 5 b.p. threshold had to raise their rates to compete with it, and didn't have many other options. However, dealers with higher rates seemed to have been able to manage their repo rates slightly more in response. In figure 3, I perform a basic split, where I group dealers into two groups based on their non-Treasury repo activity prior to June 17. Panel A shows that dealers which were paying lower rates prior to the adjustment had to raise their repo rates by approximately 5 b.p. in response to the policy change. However, higher rate dealers seemingly had some ability to manage the rate they paid after the adjustment, and the change ended up having a lower cost impact on this group of dealers.

Panel B shows the total quantity of non-Treasury repo by these two groups in 2021, relative to their June 16 level. These quantities were relatively stable, and do not show any significant pattern changes leading up to the adjustment. However, following the adjustment the dealers that were paying lower rates decreased their total repo activity by a noticeable amount relative to the higher rate dealers, and this decline sustains itself throughout much of the remainder of the year. While not conclusive, these two graphs suggest that the lower rate dealers chose to pull back on their non-Treasury repo activity due to facing higher funding costs, and that the higher rate dealers were more insulated from this funding cost increase.

3. Methodology

In this paper, I argue that the results in figure 3 are driven by a relative increase in funding costs among dealers that were more directly impacted by the technical adjustment in the ON RRP rate. I will proceed in four steps: a description of the main heterogeneity measure used, description of dealer level tests, dealer× collateral class level tests, and tests involving dealer non-Treasury secondary market activity.

3.1. **FundingGap Measure.**

The technical adjustment that occurred on June 17 serves as a temporal discontinuity, but in order to properly measure the impact of the rate change on dealer repo decisions I need a cross-sectional measure to instrument for dealer exposure to the higher rates. This instrument will be created by looking at the total increase in costs that dealers had to make up in response to the adjustment in order to make their portfolios competitive with the new rate. More precisely, for each transaction that occurs during the month prior to the adjustment I calculate:

$$gap_{i,s,t} = max(0, 0.05 - r_{i,s,t}) * Q_{i,s,t} * (1 - h_{i,s,t}) \tag{1}$$

where r is the interest rate on the repo, Q the total quantity, h is the haircut, s refers to a specific transaction, and i is a dealer. This measures the added dollar cost of performing the same transaction needed to raise the transaction above the 5 b.p. threshold created by the new ON RRP rate. For each day, I calculate the total *Gap* for all transactions performed on that day[11]:

$$Gap_{i,t} = (\sum_{s \in S_{i,t}} gap_{i,s,t})/total\_volume_{i,t}, \tag{2}$$

[11] I define $total\_volume_{i,t}$ as the total dollar quantity of repo by dealer i on day t. This normalization allows for comparison across dealers, since large dealers do significantly more volume than small dealers.

where $S_{i,t}$ is the set of all of dealer i's transactions occurring on that day. The denominator in *Gap* is just the total repo volume transacted by dealer i on that day, which normalizes the measure, so it is comparable across dealers.

My instrument, hereafter called $FundingGap$, will be based dealer i's average $Gap_{i,t}$ over the 30 days prior to the technical adjustment. Empirically, this measures the added portfolio cost to the dealer to make their pre-adjustment activity competitive with the technical adjustment. To put it differently, if a dealer is paying a pre-adjustment rate significantly above 5 b.p., they may have flexibility to adjust their repo without raising their portfolio costs. Some potential mechanisms for this include market power (Huber, 2023), relationship lending (Han, Nikolaou and Tase, 2022; Paddrik, Ramirez and McCormick, 2021), or adjustment of other elements of the repo such as tenor or optionality. However, rates on repos occurring below 5 b.p. have to be raised in order to remain competitive. Thus, $FundingGap$ is a measure of the direct exposure of the dealer to the funding shock.

This study depends on $FundingGap$ capturing an exogenous shock to non-Treasury repo rates. This means that it must capture both the unexpected increase in funding costs, as well as cross-sectional heterogeneity that allows for comparisons across dealers. Figure 5 illustrates both of these. Panel A shows the *Gap* measure for 90 days around the technical adjustment. On June 17 dealer *Gap* went from $40bn to 0, which suggests that dealers did face an immediate and sustained increase in their funding costs as a result of the technical adjustment. The second figure gives the cross-sectional distribution of *Gap*s 1 week before vs. 1 week after the adjustment. Dealer *Gap*s displayed large variation before the adjustment, but immediately, all dealers jumped to 0 funding costs very quickly afterwards.[12]

3.2. **Dealer Level Tests.** The first set of tests involve verifying that higher $FundingGap$ dealers did, in fact, face relatively higher funding costs after the adjustment occurred. In order to test this, I calculate the dealer's total repo funding costs with:

$$FundingCost_{i,t} = \sum_{s \in S_t} r_{s,t} * Q_{s,t} * (1 - h_{s,t}) \tag{3}$$

[12]Jumping to 0 is partially an artifact of the measures construction. The important thing to note about figure 5 is how discontinuous the line is, which suggests that dealers were not aware of the policy prior to it being implemented.

where variables are defined similarly to above. Then I run the difference-in-differences regression

$$FundingCost_{i,t} = \alpha + \beta FundingGap_i \times post_t + \lambda post_t + \sigma FundingGap_i + \epsilon \quad (4)$$

where $post_t$ is an indicator equal to 1 after June 16. The β coefficient captures any relative funding cost changes for dealers who had a higher *FundingGap* prior to the adjustment. The main analysis will include versions of (4) with time and dealer fixed effects to test for the robustness of β. If the $FundingGap_i$ variable captures dealer exposure to the adjustment, then $\beta > 0$. While this predicted increase in funding costs is unambiguous, how exactly it gets distributed to the components of Funding Cost is ambiguous.

The identification assumption in (4) is that the *FundingGap* measure captures how dealer funding costs changed due to the change in Treasury repo rates but is uncorrelated with other drivers in dealer funding costs during this period. Since the Federal Reserve only interacts with the Treasury Repo Market, it is unlikely that the policy decision was driven by relative considerations between different parts of the non-Treasury market. Additionally, the policy decision was taken in isolation and was not associated with other policy interventions. Tests discussed below will look at the treatment effect in a tight window around the adjustment, which will help to rule out effects of any subsequent policy changes. One potential identification issue in (4) is that the dealer's *FundingGap* will be correlated with the dealer's pre-adjustment investment strategy. Further, since different collateral classes would have been impacted differently any results could be a result of a collateral level outcome rather than a dealer level outcome. The next section will address this concern.

Funding cost has three components: the rate, the quantity, and the haircut. The rate that high *FundingGap* dealers are paying will mechanically increase in response to the adjustment. However, the other variables could increase or decrease, depending on the price elasticity of these variables. In order to test this, the following three regressions will be run:

$$r_{i,t} = \beta_r FundingGap_i \times post_t + \lambda_r post_t + \sigma_r FundingGap_i + \epsilon \quad (5)$$

$$Q_{i,t} = \beta_Q FundingGap_i \times post_t + \lambda_Q post_t + \sigma_Q FundingGap_i + \epsilon \quad (6)$$

$$h_{i,t} = \beta_h FundingGap_i \times post_t + \lambda_h post_t + \sigma_h FundingGap_i + \epsilon. \quad (7)$$

These three equations measure the dealer-portfolio level dynamics after the technical adjustment. This allows for quantifying the impact of the adjustment on dealer non-Treasury repo activity, but it masks some richness, which we can capture by breaking the data down further to the collateral-dealer-time level.

As is typical in difference-in-difference settings, the experimental design is dependent on the shock being exogenous. The primary way to test for this is to validate the parallel trends assumption. I will validate this assumption in all dealer-level tests by running the regression:

$$X_{i,t} = \beta_t FundingGap_i \times \eta_t + \gamma_i + \eta_t + \epsilon \tag{8}$$

where in this regression $X_{i,t}$ refers to one of the outcome variables in regressions (4)-(7) and η_t and γ are time and dealer fixed effects, respectively. The regression coefficients β_t will capture the treatment effect on each day. The parallel trends assumption states that $\beta_t = 0$ before the rate hike occurs, which would imply no relative difference between high and low $FundingGap$ dealers prior to the technical adjustment.

3.3. **Dealer-Collateral Tests.** Any dealer level effects could be caused by higher cost of funding against specific collateral classes or could be caused by higher dealer balance sheet costs overall. I will break down the data to the dealer-collateral-day level to differentiate these two channels. In this analysis, I create two measures called $CollateralGap$ and $DealerGap$. $DealerGap$ is the same as $FundingGap$, and measures the total dealer exposure to the higher funding costs.

$CollateralGap$ is defined at the dealer$\times$ collateral class level. It measures how a dealer is exposed to the higher funding cost in a specific collateral class. It is defined analogously to equation (2). It is useful to show the difference between the $DealerGap$ and $CollateralGap$ variables via an example. Assume dealer A does \$10 of Agency MBS collateralized repo at a rate of 0% and \$10 of corporate bond collateralized repo at a rate of 0.04%, and both sets of repos are done at a zero haircut. In this case dealer A would have two $CollateralGap$ measures, equal to $max(0, 0.05-0)*10 = 0.5$ for Agency MBS and $max(0, 0.05-0.04)*10 = 0.1$ for corporate bonds. They will have one $DealerGap$ variable, equal to $0.5 + 0.1 = 0.6$.

Once these variables are created, the following regression is run:

$$X_{i,j,t} = \beta_d DealerGap_i \times post_t + \beta_c CollateralGap_{i,j} \times post_t + \gamma_i + \eta_t + \iota_j + \epsilon \tag{9}$$

where $X_{i,j,t}$ is either the rate, quantity, or haircut that the dealer i has in collateral class j on day t and γ, η, and ι are dealer, time, and collateral class fixed effects, respectively. The intuition is that that β_c will capture the direct impact of higher funding costs in specific collateral classes for the dealer, while β_d will capture the added costs from higher funding costs over the entire dealer balance sheet. Using the above example, $CollateralGap$ is higher for Agency MBS but low for corporate bonds. Conversely, once the $CollateralGap$ is controlled for, $DealerGap$ is relatively higher for corporate bonds and lower for Agency MBS.

As with the dealer level tests, the parallel trends assumption must be validated for all dealer-collateral tests. To test this, I will run an analogous regression to (8):

$$X_{i,j,t} = \beta_d^t DealerGap_i \times \eta_t + \beta_c^t CollateralGap_{i,j} \times \eta_t + \gamma_i + \eta_t + \iota_j + \epsilon, \tag{10}$$

where ι_j is a collateral class fixed effect. In this regression, β_d^t will capture the dynamic effect coming from the total cost impact on the dealer's balance sheet, and β_c^t from the direct impact on the dealer's pre-adjustment collateral positions.

3.4. **Secondary Market Tests.** The final analysis will involve sample dealers' secondary market activity. This approach will use the dealer gap measure as a measure of exposure and use a similar approach to above. I will test how dealers change their trading volume around the adjustment, as well as how their profitability and bid ask spreads are impacted. Of course, these three variables are jointly determined so this could be seen as different versions of the same outcome variable, namely the dealer's secondary market activity.

The test will occur at the dealer×day frequency. The primary trading volume tests will be

$$X_{i,n,t} = \beta DealerGap_{i,t} \times post_t + \gamma_i + \eta_t + \epsilon \tag{11}$$

where $X_{i,n,t}$ is equal to total trading activity of dealer i for a given trading direction n(i.e. buy/sell) at date t. If changes in funding costs affect other dealer activities, then this would imply $\beta < 0$ for trading activity. Whether this decline will be uniform across buy and sell orders is ambiguous, but tests described below will show that any differences are not significant. Regression (11) will be run for both client and interdealer markets separately.

Additional tests will run similar regressions to (11), but with profitability (i.e. trading profit and loss) as the dependent variable. Profitability is measured at the dealer×time level, so the dependent variable in (11) will be given by the profitability for dealer i over time period t for their entire portfolio and not treat buy and sell orders differently. The right hand side of (11) will remain unchanged.

I also analyze bid ask spreads for affected dealers. This analysis will be run only over cusips that the dealer has both bought and sold within the same week and will be at the cusip×week×dealer level. These tests will use the regression:

$$bidask_{i,j,t} = \beta DealerGap_{i,t} \times post_t + FE + \epsilon \tag{12}$$

A large literature predicts that higher funding costs should lead to bigger bid ask spreads (Brunnermeier and Pedersen, 2008; Gromb and Vayanos, 2002; Huh and Infante, 2021), which would predict that $\beta > 0$. In all secondary market tests, the collateral gap measures will be excluded. This is due to it being difficult to map secondary market cusips cleanly to general collateral repo categories.

Parallel trends tests will be conducted for secondary market transactions. The regressions will look identical to (8), but with secondary market outcomes as the dependent variable.

4. Data

There will be three datasets used in this study: transaction level tri-party repo data provided by the Federal Reserve Bank of New York (FRBNY), account level settlement data provided by the Bank of New York (BONY), and secondary market transaction level data from FINRA TRACE.

4.1. **Tri-party Repo Data.**

The primary outcome variables in this study will be rates, haircuts, and total collateral used in repo agreements on a given day. Rates will come from the FRBNY transaction level data, and haircuts and total collateral used will come from the BONY account settlement data. These two datasets are merged by merging on account id and file date, which allows for connecting a cusip that has been used for settlement to an actual transaction being performed on that account. All inter-affiliate repo transactions are excluded from this study.

Since the subject of this study is non-Treasury repo, all repos collateralized by Treasuries are excluded from this study. This is done in two ways. First, all repo transactions labeled as either Treasury Excluding Strips or Treasury Strips in the FRBNY data are excluded. Second, any transaction settled using a Treasury CUSIP in the BONY data is also excluded. In the tri-party repo market, it is possible, but rare, for a non-Treasury repo transaction to be covered by a Treasury repo if the dealer does not have enough of the non-Treasury Repo to cover the transaction.[13] Given the first filter based on the FRBNY data, the second filter is not crucial for this study.

The merged tri-party data is aggregated in two ways: up to the dealer×day level and the dealer×day×collateral class level. All rates and haircuts are weighted averages, weighted by the notional amount of the repo. Data is aggregated to the file dates in the files given to the OFR. The collateral classes that are used are the ones provided in the FRBNY data. This is beneficial because the FRBNY collateral classes are broad and are also the collateral classes being used when specifying rates. A drawback, as mentioned above, is that it is possible that the collateral being exchanged does not match the collateral specified in the Federal Reserve's data. Once Treasuries are fully excluded, however, this issue becomes marginal.

4.2. **Secondary Market Trading Data.** Secondary Market Trading Data comes from Regulatory FINRA TRACE data containing secondary market activity by dealers. This data contains information on trades involving Treasuries, agency MBS, corporate bonds, Equities, and a variety of other transactions. The OFR's data naturally excludes Treasuries, and I also exclude data on mortgage TBAs since they are not used in tri-party Repo Agreements. This dataset contains dealer identities. I match these dealer identities with their corresponding highest subsidiary level in the repo data, where they are matched by hand. Any duplicate transactions or interaffiliate transactions are eliminated following Choi, Huh and Seunghun Shin (2024).

The main variables used are total dealer trading volume, dealer profitability, and bid-ask spreads. Dealer trading volume are aggregated to the week×dealer×trade direction level and profitability are aggregated to the week×dealer level. Profitability is calculated by subtracting the total dollar value of securities bought from the total dollar value of securities sold for that dealer in that week. Bid-ask spreads are calculated at the cusip×dealer×week level and is calculated by taking the weighted average difference between the bid prices and

[13]The labels in the FRBNY data correspond to dropdown menus that are on the screen when transactions are being entered into BONY's platform. However, they do not refer to any tangible category as it relates to CUSIP settlement, so there is a possibility that the collateral category being entered on BONY's system is completely distinct from the collateral actually being exchanged. In practice, this is not a major issue.

the ask prices, weighted by the face value of the trade. Tests involving bid ask spreads only include cusips that the dealer both bought and sold in a given week.

4.3. **Quantity Variable Calculations.** Several outcome variables are quantities and so are best dealt with in terms of percent changes. Typically, this is done by transforming the variable with natural logs, but since quantities can go to zero, especially in the post period, I take a different approach. This applies when the outcome variable is either funding costs (from the repo data), collateral quantity (repo data), trading profitability (TRACE data), or total trading activity (TRACE data). For any tests involving these variables, I normalize each variable by that dealer's average pre-adjustment volume during the month prior to the Federal Reserve's technical adjustment. For collateral level tests, I do the normalization at the dealer×collateral level. Coefficients in tests with any of these outcome variables should be interpreted in percent terms.

4.4. **Summary Statistics.** The final data used includes all tri-party repo and non-Treasury secondary market data from 2021. Summary statistics for the entire sample are given in table 1a. Panel A is based on the repo file aggregated to the dealer× day level, Panel B to the collateral×dealer×day level, and Panel C gives the summary statistics from the TRACE data. Column 2 of each panel describes the nature of the variable, i.e. whether or not it is a static treatment variable, a quantity variable transformed as explained in the previous subsection, or in some other format.

Any tests involving variables that have been transformed to be a fraction of its pre-adjustment value will use a balanced panel. Thus days where dealers had no repo activity are given a 0 value, but still included in the panel. Tests for other dependent variables may not have a balanced panel, since they are usually transaction level variables (e.g. rate or haircut) so 0's don't make as much sense. The third column in table 1a gives the sample size for each variable and shows that there are not many instances of these missing variables and that these choices are not essential ones.

5. Main Results

The main results appear in five subsections, beginning with the differential effects of the technical adjustment on funding rates on more affected dealers. Next, funding costs are broken into constituent components (rate, haircut, and collateral quantity) to see how the funding shock was distributed to each component. The third subsection quantifies these relationships. The fourth subsection presents the collateral level repo results, and the final subsection presents the secondary market results.

5.1. **Funding Costs Treatment Effects.** The regression results for (1) are presented in table 2. The first column presents the results with no fixed effects. First, note that the coefficient on the Post variable implies that all dealers paid approximately 8% higher funding costs in the post period. Second, dealers with a higher *FundingGap* tend to have lower funding costs, which is natural given that they are paying a lower rate on their repo.

The main coefficient of interest is the one on $post \times FundingGap$. This has a coefficient of 0.023, and is significant at the 5% level. This translates to a relative increase in funding costs from going to the 25 to 75th percentile of 0.023*2.2*100=4.6% above pre-adjustment levels. Focusing only on the sample of dealers with a positive *FundingGap*, this would translate to a total increase in funding costs of 8% above a dealer with a 0 *FundingGap*. This implies that the 75th percentile positive *FundingGap* dealer would experience an increased daily funding cost of $1.29bn above a dealer with a zero *FundingGap*.

Columns (2) and (3) include day fixed effects and day and dealer fixed effects, respectively, in order to control for any time trends or dealer specific effects. The coefficient on the interaction term remains virtually unchanged between the different specifications.

The parallel trends assumption is tested by running regression (8), and the coefficients are given in figure 5. The increased funding costs immediately follow the technical adjustment. The increase reaches its peak on the day immediately following the adjustment but persists a full 90 days after the adjustment. The lack of pre-trends is notable and suggests that this adjustment was unexpected and was the immediate source of higher funding costs for affected repo dealers.

5.2. **Decomposing Funding Costs Outcomes.** Next I decompose total funding costs into rate, collateral quantity, and haircut (regressions (5)-(7)). The results are given in table 3. Panel A runs (5). Consistent with figure 3, dealers with a larger *FundingGap* saw a relatively higher increase in rate. This would imply that the technical adjustment was more salient for these dealers. The coefficient on *FundingGap* is negative, meaning that more affected dealers pay lower rates on their repo before the adjustment, which is to be expected.

Panel B runs regression (6). Higher *FundingGap* dealers decreased their total quantity of repo after the adjustment. This result is large (coefficient of -0.039) and statistically significant at the 1% level. The next section discusses quantification, but this does suggest a relatively high price elasticity of funding demand for dealers. It is noteworthy that higher *FundingGap* dealers did not intermediate a statistically significantly different amount of repo prior to the technical adjustment.

Panel C runs regression (7). In the tri-party repo market haircuts are relatively fixed due to the way contracts are written, so we would expect less ability for dealers to quickly change their behavior in response to the adjustment (Hu, Pan and Wang, 2021). Verifying this intuition the coefficients in panel C are small in magnitude and only statistically significant at the 10% level. Affected dealers do face lower haircuts overall, which is due to them typically borrowing against higher quality collateral.

Columns (2) and (3) of Panels (A)-(C) add day and dealer fixed effects, and the results on the interaction variable of interest remain virtually unchanged between all specifications. The coefficient in the third column of panel (C) becomes smaller and not statistically significant, confirming that haircuts were largely unaffected by the technical adjustment.

One potential explanation for the results in table 3 is that affected dealers might have adjusted their non-Treasury repo portfolio by shifting into Treasuries. While absolute costs of performing Treasury repo will also have increased the *relative* costs compared to non-Treasury repo may have declined. Table 4 tests this possibility by looking at dealer's Treasury repo portfolio but using the same *FundingGap* measure as in table 2. Panels (A) and (B) show that higher *FundingGap* dealers did not see any statistically significant difference in either the rate or total quantity of repo after the adjustment. Haircuts have negative coefficients that are significant at the 10% level, but the coefficients are small in magnitude. Overall, there is little evidence that high *FundingGap* dealers were differentially impacted in the Treasury market or that they adjusted their Treasury portfolio after the technical adjustment.

Panel A of figure 10 shows how rate and quantity change in a 90 day window around the adjustment. Similar to Total Funding Costs, rates and quantity see an almost immediate and substantive change relative to their pre-adjustment levels. There is also no strong indication of pre-trends in either rates or quantities. I perform the same analysis for Treasury repo, and Panel B shows that higher *FundingGap* dealers do not have any statistically significantly different behavior before or after the technical adjustment.

The results in this section show that the technical adjustment was more salient for high *FundingGap* dealers, likely because they were not able to adjust the rates they are charging as flexibly as dealers that were less directly impacted by the adjustment. These dealers, however, were able to manage the funding costs they were paying by lowering the total quantity of repo they performed. This suggests that changes in repo rates on one kind of collateral, in this case Treasuries, does spill over to other forms of collateral and can affect dealer funding decisions.

5.3. **Quantifying Previous Results.** The previous section ends with a qualitative explanation of the results, but deriving the quantitative relationship between outcome variables is difficult from the regression coefficients in table 3 due to *FundingGap* being a continuous treatment variable. In order to speak to quantification, Figure 9 splits the sample into seven quantiles based on the *FundingGap* measure and runs a simple regression of the outcome variable at the top of the graph on a post indicator equal to one after the technical adjustment.[14]

The first two quantiles all contain dealers with a *FundingGap* of 0, so we would expect them to exhibit little differences in all specifications. This is confirmed in all specifications, quantile 1 and 2 do not show a statistically significant difference in either funding costs, repo rate, quantity, or haircut on their non-Treasury repo after the adjustment.

The dealers in the top two quantiles experienced an approximately 15% increase in their funding costs above baseline in the bottom two quantiles. As in the previous section, this was driven by a 3 b.p. increase in rate. The highest quantiles decreased their total funding costs by decreasing their total repo quantity by approximately 10% relative to the lowest two quantiles. This 3 b.p. increase in rates represented a 120% increase in rates for quantile 6 and 7 relative to quantile 1 and 2. All of these numbers together would imply that a 10% increase in the rate a dealer is paying on their entire portfolio would lead to a 0.8% decline in the quantity of collateral being used for repo borrowing.

5.4. **Dealer×Collateral Results.** The results from the previous section could be driven by the direct cost of intermediating specific general collateral classes or through broader costs to the dealer of maintaining its entire balance sheet. Put differently, the agency MBS is more substitutable with Treasuries, so we may expect the change in Treasury repo rates to transmit to agency MBS relatively more than other kinds of collateral. The previously documented decline in quantity of repo could happen because every dealer who intermediates on agency MBS collateral does less agency MBS collateralized repo, or because dealers who

[14]Seven quantiles was chosen in order to comply with disclosure requirements, but the results remain unchanged if more quantiles are chosen

do a lot of agency MBS collateralized repo lower their entire non-Treasury repo borrowing across all collateral classes.

In order to test this, I run regression (9), which separates the dealer's *FundingGap* in specific general collateral classes from it's broader *FundingGap* exposure. The results are presented in table 5. Panel A has Funding Costs as the main outcome variable and is analogous to the analysis in table 2, but at the dealer×collateral level. The first column runs the regression with *CollateralGap* as the continuous treatment variable, and the second with *DealerGap*. Unsurprisingly, both coefficients are positive and statistically significant, which means they both capture some dimension of the additional funding costs to these dealers.

Column (3) runs a full specification with both the *CollateralGap* and *DealerGap* variables. Once both variables are controlling for each other, the coefficient of the Collateral Gap variable becomes the only one that remains positive and statistically significant. Indeed, the coefficient on the *DealerGap* interaction variable goes from being positive and statistically significant to negative, although no longer statistically significant at conventional levels. This suggests that once the funding cost of borrowing against specific collateral classes is controlled for, dealers that faced higher general balance sheet costs may be able to manage the remainder of their repo portfolio to lower their general funding costs. Column (4) runs a specification with dealer, collateral class, and day fixed effects, and the results from column (3) remain unchanged.

Panel B and C test this hypothesis by decomposing funding costs as in previous tests. Panel B runs the regression with rate as the dependent variable. Columns (1) and (2) run the regressions with the *CollateralGap* and *DealerGap* interactions separately, and both are statistically significant. Columns (3) and (4) run the full specification with both treatment variables. Once the variables are controlling for each other, the coefficient on the collateral gap variable remains approximately equal to the coefficient in column (1), while the coefficient on the dealer *DealerGap* variable becomes smaller in magnitude and is no longer statistically significant.

Panel C has the repo quantity as the dependent variable, with each column running an analogous specification to panels A and B. In columns (1) and (2) the coefficients are both negative and statistically significant, although the coefficient on collateral gap is smaller in magnitude and only significant at the 10% level. In the full specification in columns (3) and (4), however, only the coefficient on *DealerGap* remains large in magnitude and statistically significant. The results in Panel B and C are in stark contrast to each other and confirm that the higher funding costs previously documented in table 2 come purely through the increased cost of borrowing against specific collateral classes, while the decline in total repo quantity comes from dealers with biggest increase in balance sheet costs more generally.

Figure 10 shows the results from table 5 in event time by running regression (10). Panel A shows that coefficients from both the *CollaterGap* and *DealerGap* variables both increased immediately, suggesting that the adjustment was very salient for all dealers roughly equally. However, the coefficient on *CollateralGap* is initially higher than the coefficient on *DealerGap*, and the *CollateralGap* coefficients remain statistically significantly different from 0 for 90 days after the adjustment, while the *DealerGap* variable coefficients decline back to 0 quickly. The decline in repo quantity seen in Panel C of table 5 was also immediate and sustained for the entire 90 day post-adjustment period, but the decline is completely coming from the *DealerGap* treatment variable.

The results from this section have a relatively straightforward interpretation. The increased funding costs to dealers come through higher rates that stem from the direct competition that certain collateral classes have with Treasuries. Dealers were able to manage the rates they paid on collateral classes where they were generally paying above the 5 b.p. technical adjustment so that they were insulated from the change. However, dealers still faced generally higher funding costs and pulled back their repo quantity to lower these funding costs. More affected dealers lowered their quantity of repo for all collateral classes, rather than only focusing on collateral classes where they faced the biggest increase in rates. This means that the dealer's balance sheets propagated the adjustment to collateral classes not directly impacted, which suggests that balance sheets are important for transmitting repo rate changes to the broader financial market.

One factor that could be driving these results would be shifts in the term structure of the rates on the balance sheet of affected dealers. If affected dealers could lower their portfolio costs by lowering their term, then we would expect this to change their total transaction volume without changing their actual borrowing quantity. This seems unlikely to be driving the results, since it would predict an increase, rather than decline, in transaction volume.

Formal tests with repo term as dependent variable are run in table 6 and figure 11. There is a statistically significant decline in term after the adjustment, and the coefficient on *DealerGap* is the only coefficient that remains significant in the full specification. Quantitatively, the decline equates to a decline in the average portfolio term for affected dealers of roughly half a day.

It does seem that dealers are lowering the term of their portfolio in response to the adjustment, but in the opposite direction from the concern above. If anything, this would bias against the broader decline in transaction volume documented earlier. Figure 11 further confirms that this decline is unlikely to be driving prior results, as it shows that the decline in term was gradual, while the dynamics in figure 8 are immediate. Interestingly, this does point to dealers managing their term in order to manage their funding costs, but it does not contradict the results previously presented.

5.5. **Secondary Market Activity After the Adjustment.** Next I look at how affected dealers adjust their secondary market activity after the technical adjustment. First I run regression 11 with total trading volume as the dependent variable. If the increased funding costs limit the ability of dealers to fund their other activities, this would imply a decline in total secondary market trading volume.[15] This is tested in table 7, with Panel A running the regression for the client market and Panel B for the interdealer market. These regressions are run at the dealer× day× trade direction level.

The first three columns of both panels run the regression with progressively more fixed effects. The coefficient in column (1) implies that more impacted dealers experienced a decline in total trading volume in both the client and interdealer markets. In this data, Dealer Gap approximately doubles when moving from the 25th to 75th percentile. Thus, the coefficients in column (1) imply a 11% decline in client markets (8% in interdealer markets) for 75th percentile dealers relative to the 25th percentile. This decline is significant at the 1% level and remains large as additional fixed effects are added. Column (3) runs the same specification, but adds day and dealer fixed effects to control for time trends and dealer specific factors. The coefficients in this column imply a 6% decline in client and interdealer trading when moving from the 25th to the 75th percentile. Ceofficients in Panel A and B remain significant at least at the 5% level in all specification, regardless of the level of fixed effects.

Column (4) tests whether or not the decline in buy and sell trading volume is asymmetric. The coefficient on the triple interaction term shows the additional buying the dealer is performing relative to selling during post the technical adjustment. Both the coefficients in

[15]Collateral is to a certain extent fungible in this market, so it is possible that dealers could fund their activity using securities other than the ones that were directly impacted. However, I have already shown that the affected dealers did not shift into funding themselves by doing Treasury collateralized repo. Since this paper uses the remainder of the repo market, it still represents a funding shock even if they are not directly collateralizing their repo with the purchased collateral

Panel A and Panel B are positive, but they are small relative to the baseline coefficient in row 1, and neither are statistically significant at conventional levels.

Figure 12 gives the results from table 7 in event time. A regression is run where the Dealer Gap variables are interacted with week dummies, and all coefficients are normalized based on 2 weeks before the technical adjustment. Figure 12 plots these coefficients. Following the adjustment, trading volume declines and remained depressed for 2 months afterwards, until it recovered to approximately the same relative level as before. Figure 12 implies that it took approximately 90 days for affected dealer's quantity of repo to recover, implying that trading volumes recovered faster than repo activity.

Table 8 gives the results with bid ask spread and profitability as the dependent variables, as described in section 3.3. The first three columns run regression (12), with varying levels of fixed effects. The coefficient on the interaction term remains positive and statistically significant at the 1% level across all levels. The coefficient remains stable across different levels of fixed effects, which lowers concerns over omitted variable bias (Altonji, Elder and Taber, 2005; Oster, 2019). The coefficient of 0.013 implies that the bid ask spread increased by 0.026 when moving from the 25th to 75th percentile of dealer exposure to the technical adjustment. This is approximately equal to an increase of 6% in the median dealer's bid ask spread.

Next, I run regression (11) to test how affected dealer's profitability was impacted by the higher funding costs. The results are presented in columns (4)-(6) in table 8, with increasing level of fixed effects as above. The results are weaker for profitability than bid ask spread, but the coefficients do suggest a decline in profitability. Columns (4) and (5) suggest that trading profitability for the 75th percentile dealer declines 36% more than the 25th percentile dealer, but the coefficients are not statistically significant. The coefficient in column (6) controls for both dealer and day fixed effects, and is smaller in magnitude, implying a 10% drop in trading profitability after the adjustment. This coefficient is marginally significant at the 10% level.

The results in this section generally agree with theoretical and empirical studies that connect funding conditions to market liquidity (Andersen, Duffie and Song, 2019; Brunnermeier and Pedersen, 2009; Macchiavelli and Zhou, 2022). Dealers faced higher funding costs in the repo market, as documented above, and this led to lower activity, and ultimately liquidity, in the secondary market for the same assets. What is unique about this setting is that the funding shock originates from inside the repo market in one asset class and then propagated out to the secondary market for other asset classes. As far as I am aware I am the first to give direct evidence for this spillover channel.

6. Conclusion

In this research I inspect how changes in funding costs in one asset class can spill over into other asset classes, and ultimately into those unrelated asset classes' secondary market. I utilize a one-time technical adjustment by the Federal Reserve that caused a rise in Treasury collateralized repo rates to see how this rate rise transmitted to the non-Treasury repo portfolio of repo dealers. I show that the rates of non-Treasury repo did have to increase to remain competitive against Treasury repo. Subsequent higher funding costs led to dealers pulling back on their total quantity of non-Treasury repo, suggesting a high degree of price elasticity of borrowing demand for dealers.

I show that the decrease in non-Treasury repo was primarily driven by higher total balance sheet costs, rather than through increased cost of borrowing against specific asset classes. Finally, I show that this decline in non-Treasury repo activity led to a decline in total trading activity, an increase in bid-ask spreads, and a decrease in trading profitability in the secondary market for these assets. Due to the exogenous and unexpected nature of the rise in Treasury repo rates, I am able to show that these spillover effects are quantitatively large and relatively sustained.

Repo for different asset classes are often treated as distinct markets themselves, but the entire market is somewhat integrated through dealer balance sheets. As far as I am aware, I am the first to give direct, causal evidence for how shifts in the borrowing rates for one asset class can impact other asset classes through dealer balance sheets. This speaks to the importance of treating the repo market as unified through the balance sheets of dealers, and supports policy designed to increase balance sheet health through the lens of stabilizing short term funding markets.

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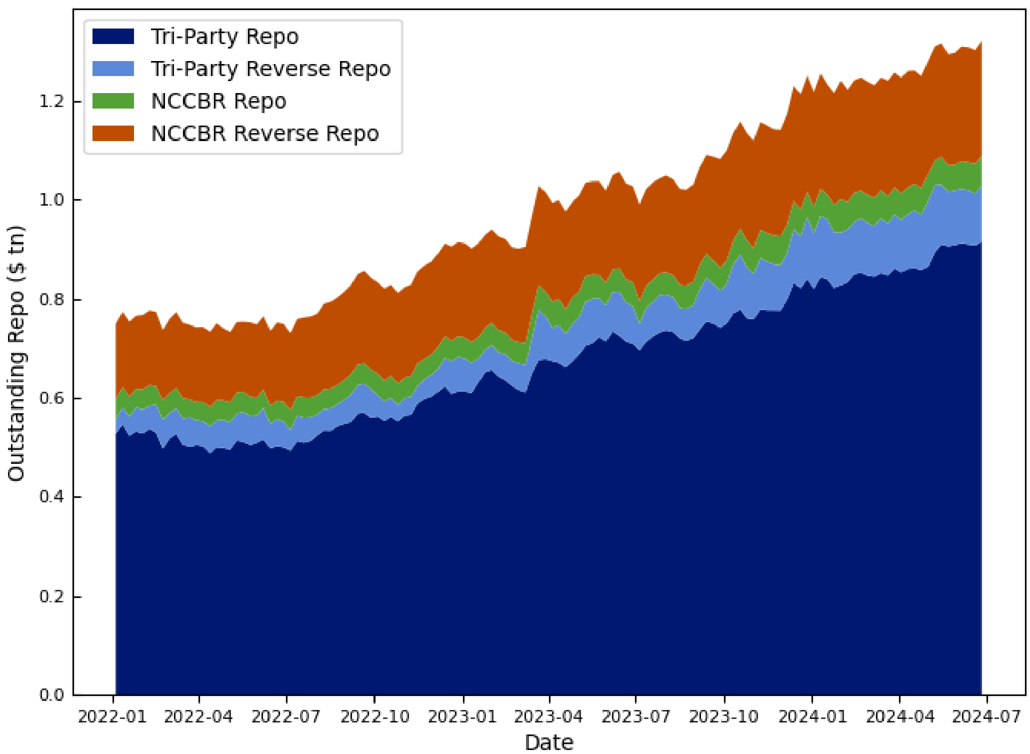
Figure 1. Primary Dealer non-Treasury Repo Volume by Position and Market
Repo refers to dealer cash borrowing against non-Treasury collateral, and Reverse repo cash lending. NCCBR refers to the non-centrally cleared bilateral market segment, and tri-party the tri-party market segment.



Source: NY Federal Reserve Primary Dealer Statistics

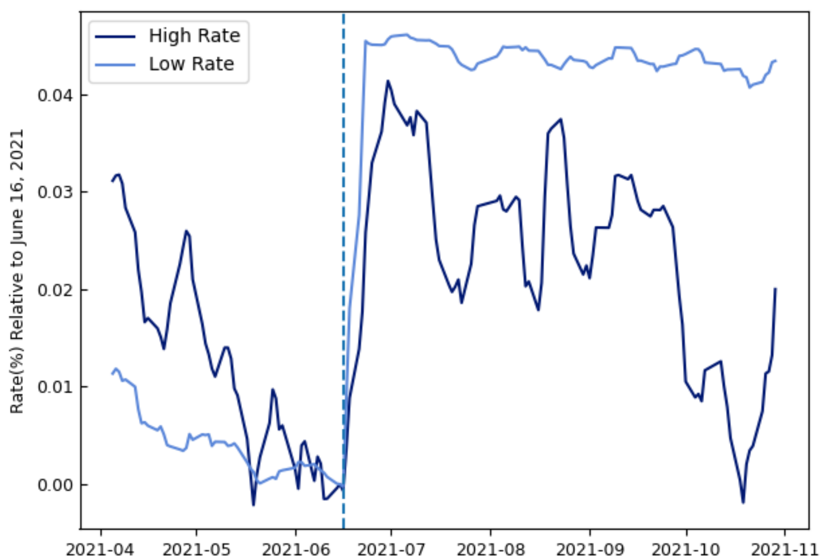
FIGURE 2. The Four Main Segments of the U.S. Repo Market

		Settlement	
		Tri-party	**Bilateral**
Clearing	**Centrally**	Fixed Income Clearing Corporation (FICC) GCF Repo (GCF) · Centrally cleared by FICC · Settled on BONYs Tri-party platform · General collateral repo only	FICC DVP Service (DVP) · Centrally cleared by FICC · No central custodian · Specific collateral repo possible
	Non-Centrally	Bank of New York Mellon (BONY) Tri-party · No central counterparty · Settled on BONYs tri-party platform · General collateral repo only	Non-Centrally Cleared Bilateral Repo (NCCBR) · No central counterparty · No central custodian · Specific collateral repo possible

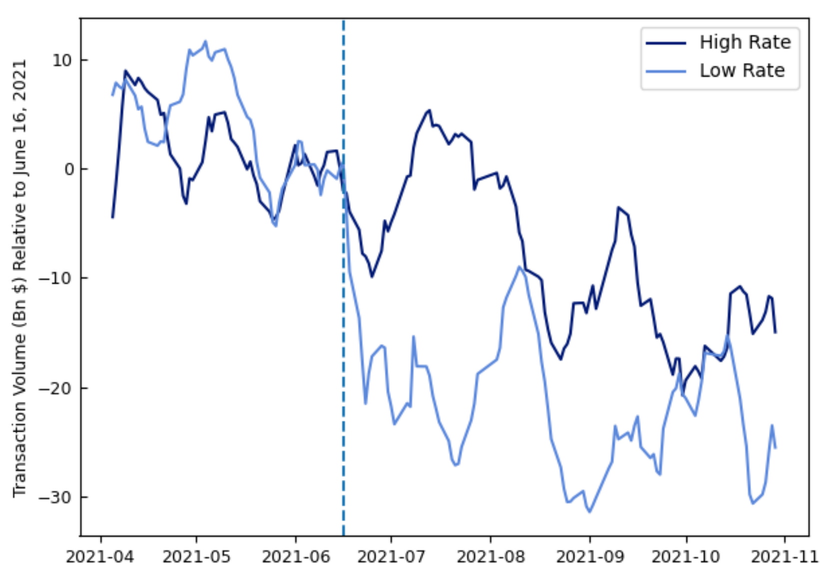
Source: Authors' creation, Author's Analysis

Figure 3. **Intermediation Dynamics around June 16, 2021**
On June 16, 2021 the Federal Reserve raised the rate they offered on Treasury collateralized repo to money market funds by 5 basis points. These graphs show how dealers adjusted their rates and quantity of non-Treasury collateralized repo transactions after the technical adjustment. Each graph splits the sample in half based on whether the dealer paid above or below median rates on their non-Treasury repo from May 15 till June 16. The top graph gives the median rate by day within each group, and the bottom graph gives the aggregate daily quantity of non-Treasury collateral used by each group.

(a) Repo Rates



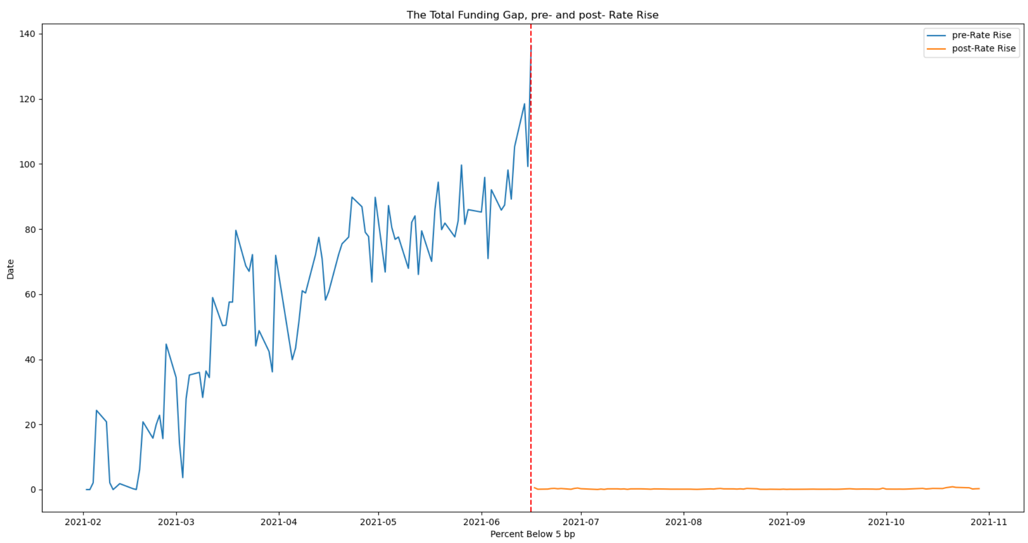
(b) Repo Quantities



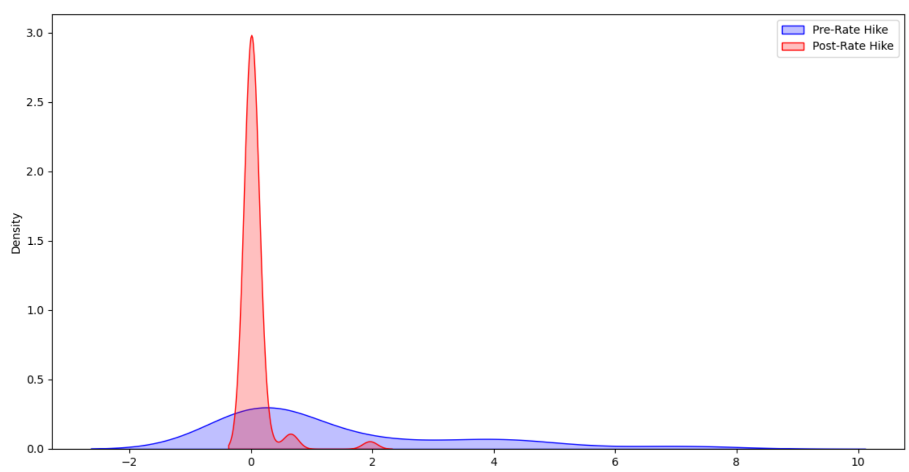
Source: FRBNY Tri-party Repo Data, Author's Analysis

Figure 5. **Dealer *Gap* around the Technical Adjustment**
The top graph gives the time series average of the total *FundingGap* by all dealers in the sample before and after the technical adjustment on June 16, 2021. The total *FundingGap* by a dealer on day t is defined as $\sum_{s \in S_t} max(0, 0.05 - r_{s,t}) * Q_{s,t} * (1 - h_{s,t})$, where S is all repo transactions by the dealer on a given day, r is the rate, Q is total collateral used, and h is the haircut. The bottom graph gives the density of the *FundingGap* during the pre- and post-adjustment periods.

(a) Panel A: Daily Gap before and after the Technical Adjustment

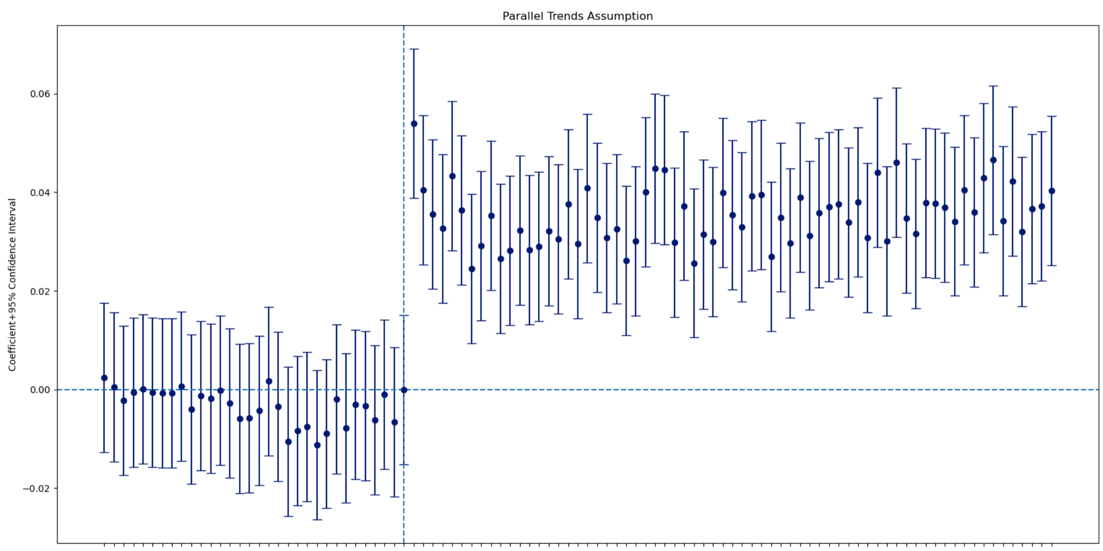


(b) Panel B: Cross-sectional Distribution of Gaps, June 9th vs. June 22nd



Source: FRBNY Tri-party Repo Data, Author's Analysis

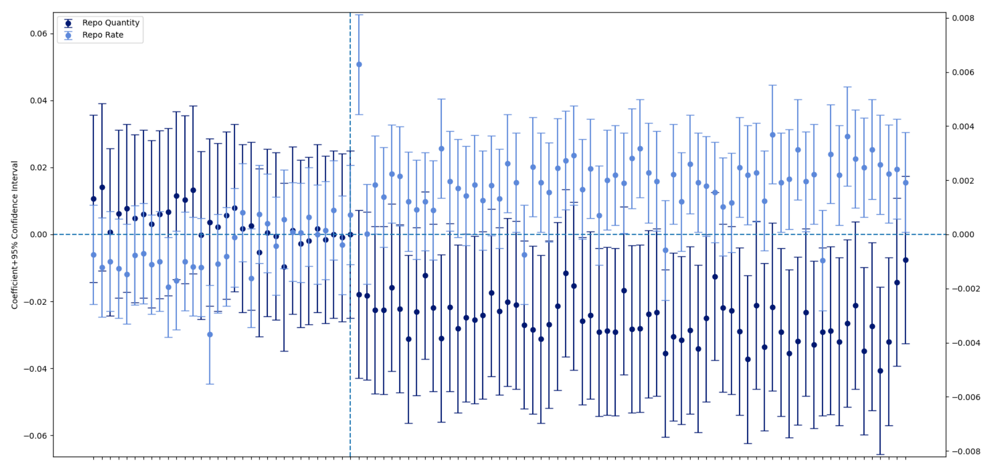
Figure 7. **Dealer Funding Costs around the Technical Adjustment**
I graph the coefficients + 95% confidence intervals from running regression (8) with funding costs as the dependent variable over a 90 day window around the technical adjustment date. The dependent variable is total cost to the dealer of funding their entire non-Treasury repo portfolio on that day. This variable is normalized by the average total *FundingGap* during the month prior to the adjustment. Standard errors are double clustered at the dealer and date level.



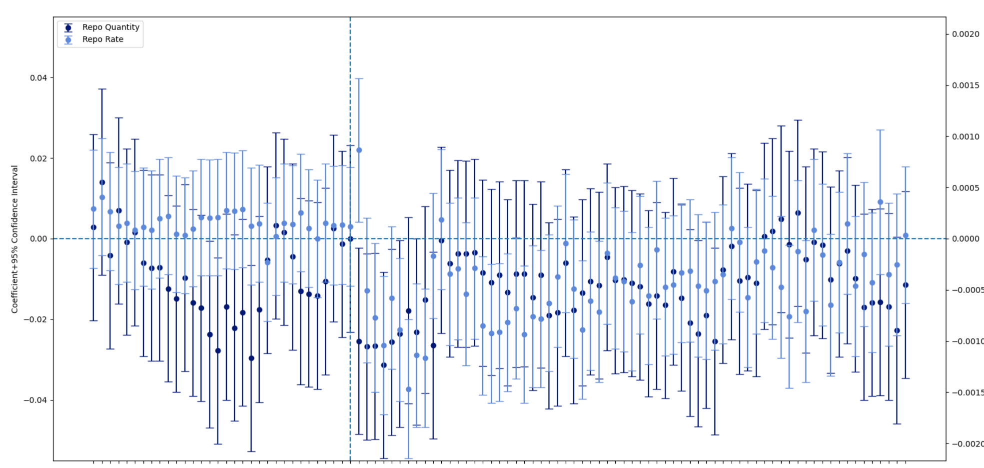
Source: FRBNY Tri-party Repo Data, Author's Analysis

Figure 8. **Decomposing Funding Costs around the Technical Adjustment**
I graph the coefficients + 95% confidence intervals from running regression (8) with rate and quantity as the dependent variable over a 90 day window around the technical adjustment date. The left axis corresponds to the magnitude of coefficients in the repo rate regression, and the right to repo quantity. Panel A gives the regression coefficients for the non-Treasury portion of the dealer's repo portfolio, and Panel B for the Treasury portion. Treatment variables are defined the same in both graphs. Standard errors are double clustered at the dealer and date level, and given by bands around the point estimates.

Panel A: Non-Treasury Collateral

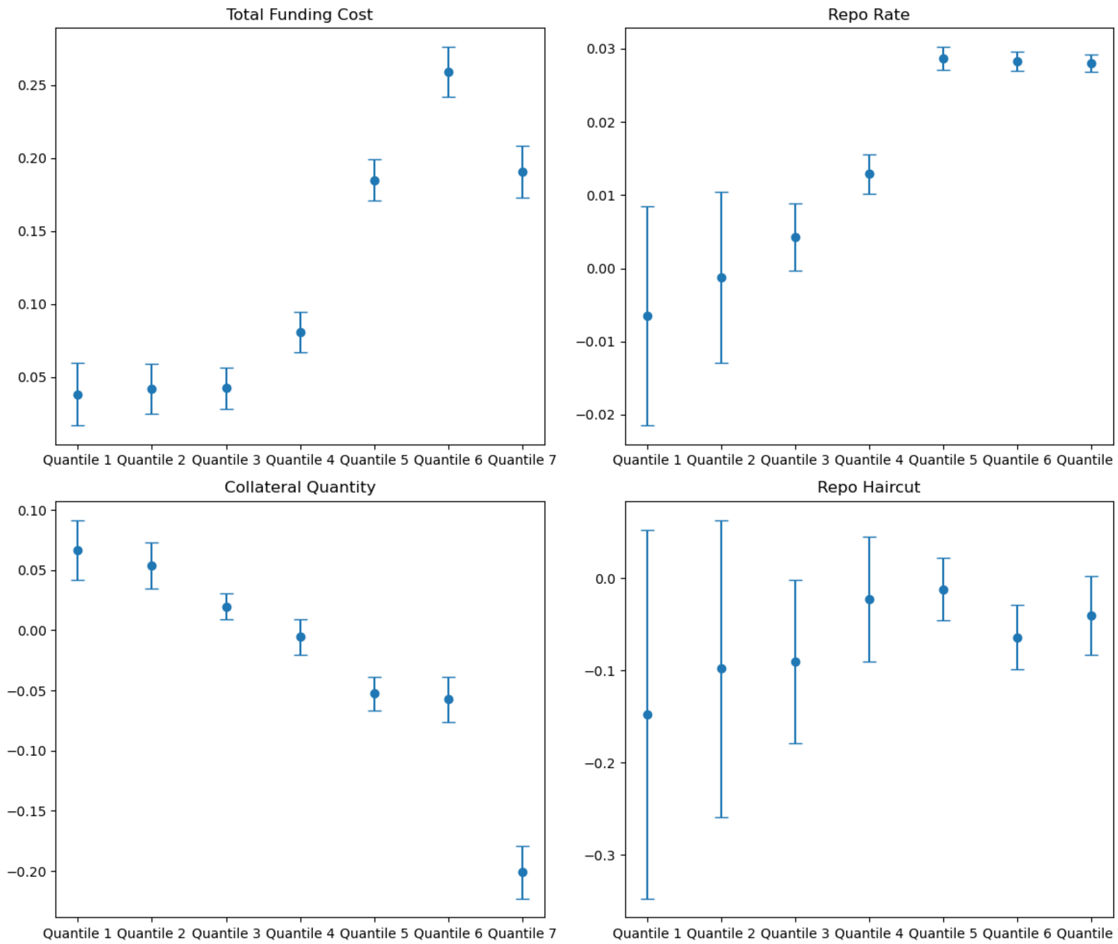


Panel B: Treasury Collateral



Source: FRBNY Tri-party Repo Data, Author's Analysis

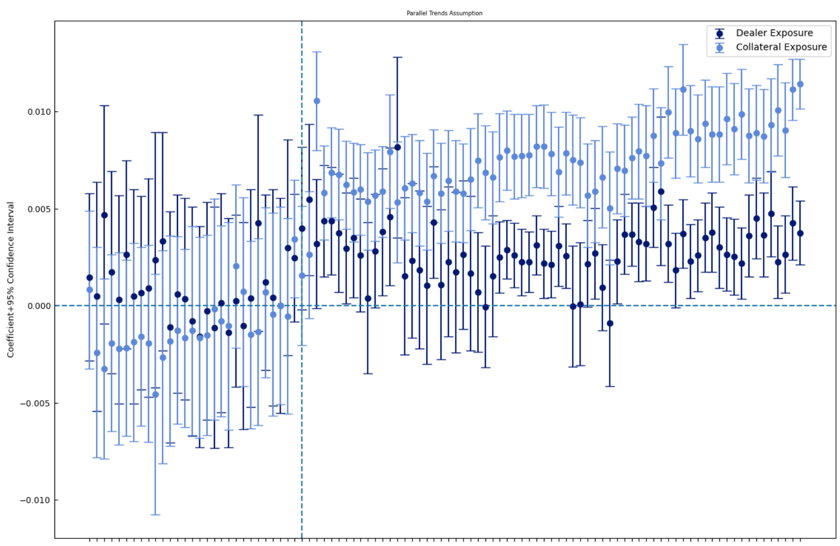
Figure 9. **Quantifying Treatment: Affected vs. Non-Affected Dealers**
Dealers are split into seven quantiles based on their pre adjustment FundingGap. Using each quantile sample, a basic difference regression is run, which regresses an outcome variable on a post indicator equal to 1 after June 16. The outcome variables are given at the top of each graph below, and the coefficient plus 95% confidence bands are given from running this difference regression within each quantile group.



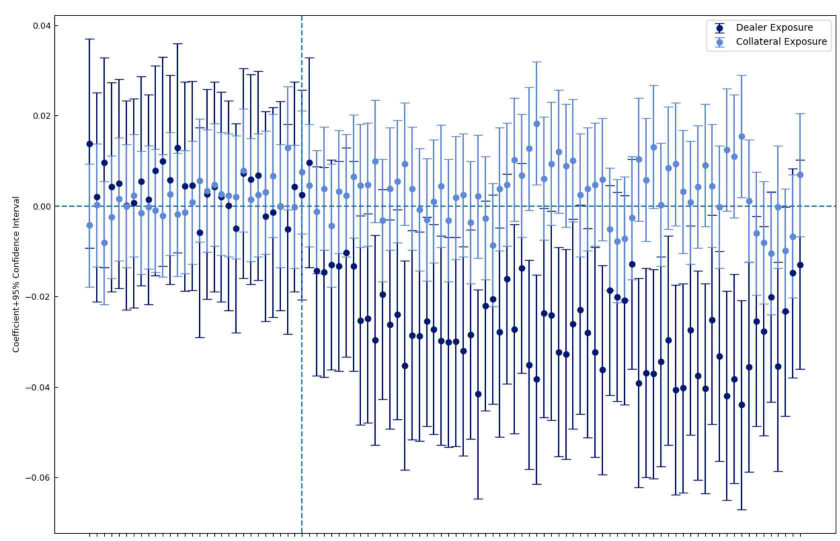
Source: FRBNY Tri-party Repo Data, Author's Analysis

FIGURE 10. **Dynamic Treatment Effect: Collateral vs. Dealer Exposure**
Analysis is run on the dealer×day×collateral class dataset discussed in section 3.3. For each dealer×collateral class pair, a treatment variable is constructed by on the *FundingGap* for that dealer in that specific asset class. This treatment variable is defined identically to the dealer's portfolio level treatment variables used in prior tables and figures, but broken down at the collateral class level. Regression (10) is run, which includes both the collateral level treatment effect discussed here and the dealer level treatment variable used in prior analyses. Panel A gives the paired coefficients when the dependent variable is the rate, and Panel B when it is quantity. 95% confidence intervals are given, and standard errors are clustered at the day×dealer level.

Panel A: Dependent Variable: Rates

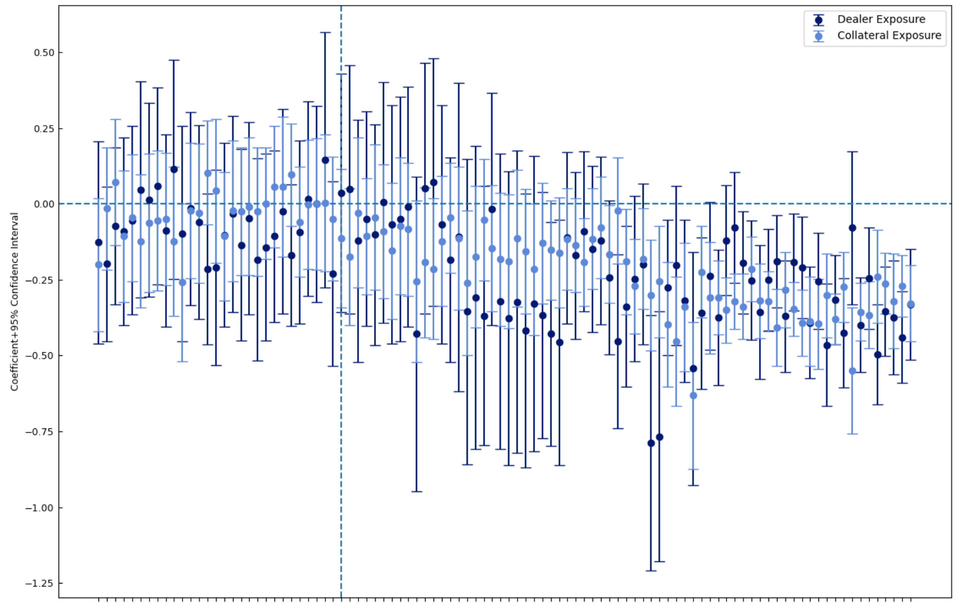


Panel B: Dependent Variable: Quantities



Source: FRBNY Tri-party Repo Data, Author's Analysis

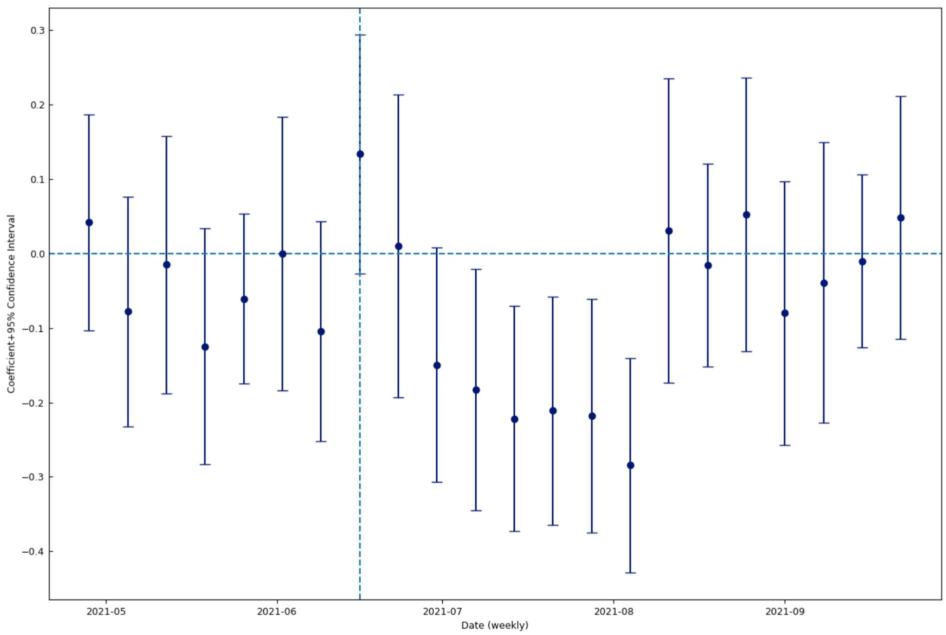
FIGURE 11. **Repo Maturity Structure around Technical Adjustment**
Identical analysis to the one in figure 10 is run, but with the weighted average days to maturity of the dealer's repo portfolio as the outcome variable.



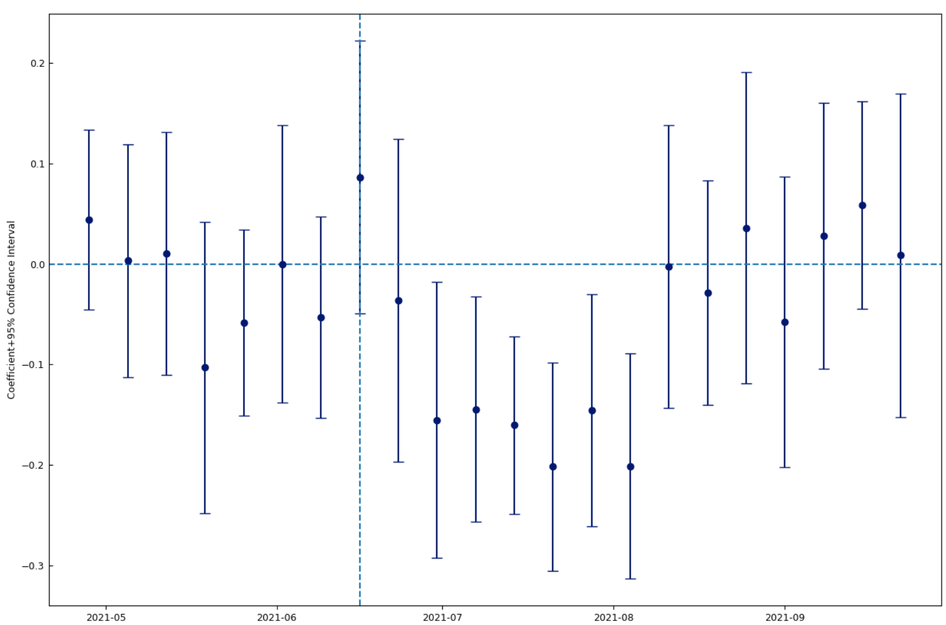
Source: FRBNY Tri-party Repo Data, Author's Analysis

FIGURE 12. **Secondary Market Trading around Technical Adjustment**
Regression (8) is run, with total weekly trading by dealers as an outcome variable. The weekly coefficients around the technical adjustment are given, including 95% confidence intervals. Panel A is based on the client sample and Panel B the interdealer sample.

Panel A: Client Market



Panel B: Interdealer Market



Source: FRBNY Tri-party Repo Data, FINRA TRACE, Author's Analysis

Table 1. Summary Statistics

(a) Summary statistics are presented for all variables from the three major datasets used in the empirical analysis. Panel A contains data from repo transaction data that has been aggregated to the dealer and day level. Panel B uses the same transaction data, but aggregated to the dealer, collateral, and day level. Panel C comes from regulatory TRACE data, and how each variable is aggregated and used is described in the main text.

Panel A: Repo, Dealer× Day level (source: NY Fed and BONY)

Variable	Type	N	Mean	St. Dev.	25th %	50th %	75th %
FundingGap	Static Treatment Variable	11,163	1.363	1.953	0.02	0.31	2.16
Total Funding Costs	Fraction of Pre-adjustment	11,163	1.182	1.062	0.80	1.02	1.38
Rate	Percent	10,685	0.142	0.160	0.05	0.08	0.21
Collateral Quantity	Fraction of Pre-adjustment	11,163	1.034	0.618	0.80	1.02	1.38
Haircut	Percent	10,685	3.873	2.467	2.21	3	5

Panel B: Repo, Collateral×Dealer× Day level (source: NY Fed and BONY)

Variable	Type	N	Mean	St. Dev.	25th %	50th %	75th %
Dealer-Gap	Static Treatment Variable	38,613	0.903	1.415	0.11	0.31	0.95
Collateral-Gap	Static Treatment Variable	38,613	1.446	2.411	0	0	3.45
Total Funding Costs	Fraction of Pre-adjustment	38,613	0.541	0.351	0.25	0.54	0.81
Rate	Percent	37,011	0.230	0.258	0.05	0.18	0.35
Collateral Quantity	Fraction of Pre-adjustment	38,613	0.650	0.322	0.42	0.69	0.91
Term	Days	37,011	19.193	29.686	1	1.56	34

Panel C: Secondary Market Trading Data (source: Regulatory TRACE)

Variable	Type	N	Mean	St. Dev.	25th %	50th %	75th %
FundingGap	Static Treatment Variable	-	0.96	1.27	0.11	0.41	2.04
Trading Volume (Client)	Log of Dealer's Trading Volume	28,137	18.01	0.000	15.55	18.59	20.43
Trading Volume (Interdealer)	Log of Dealer's Trading Volume	30,696	18.13	0.000	15.61	18.50	20.63
Profitability	Fraction of Pre-adjustment	16,730	0.98	0.77	0.48	0.86	1.33
Spread	Dollars	1,239,518	0.15	0.46	0.00	0.03	0.22

Source: FRBNY Tri-party Repo Data, FINRA TRACE, Author's Analysis

Table 2. Treatment Effect on Dealer's Total Funding Costs
This table runs a difference in difference regression around the date of the ON RRP rate increase. Post is an indicator for after the rate increase takes effect. FundingGap measures the post-technical adjustment funding cost, assuming the pre-adjustment portfolio remains the same, as defined in the main text. Standard errors are double clustered by dealer and time, and fixed effects are given at the bottom of the table. In the top panel the dependent variable is rates, and in the bottom panel, it is the log of the total repo lending against non-Treasury collateral.

	(1)	(2)	(3)
VARIABLES	Total Funding Costs	Total Funding Costs	Total Funding Costs
post×FundingGap	0.023**	0.023**	0.023**
	(0.011)	(0.011)	(0.010)
post	0.087***		
	(0.026)		
FundingGap	-0.074***	-0.075***	
	(0.012)	(0.012)	
Day FE	N	Y	Y
Dealer FE	N	N	Y
Observations	11,163	11,163	11,163
Within R-squared	0.062	0.061	0.047

*** p<0.01, ** p<0.05, * p<0.1

Source: FRBNY Tri-party Repo Data, Author's Analysis

Table 3. Treatment Effect on Dealers: Pricing and Volume Effects
This table runs a difference in difference regression around the date of the ON RRP rate increase. Post is an indicator for after the rate increase takes effect. FundingGap measures the post-technical adjustment funding cost, assuming the pre-adjustment portfolio remains the same, as defined in the main text. Standard errors are double clustered by dealer and time, and fixed effects are given at the bottom of the table. In the top panel the dependent variable is rates, and in the bottom panel it is the log of the total repo lending against non-Treasury collateral.

Panel A

	(1)	(2)	(3)
VARIABLES	Rate	Rate	Rate
post×FundingGap	0.005***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)
post	0.007		
	(0.005)		
FundingGap	-0.038***	-0.038***	
	(0.007)	(0.007)	
Day FE	N	Y	Y
Dealer FE	N	N	Y
Observations	11,163	11,163	11,163
Within R-squared	0.195	0.195	0.006

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Panel B

	(1)	(2)	(3)
VARIABLES	Collateral Quantity	Collateral Quantity	Collateral Quantity
post×FundingGap	-0.039***	-0.039***	-0.039***
	(0.011)	(0.011)	(0.012)
post	0.033		
	(0.034)		
FundingGap	-0.010	-0.010	
	(0.016)	(0.016)	
Day FE	N	Y	Y
Dealer FE	N	N	Y
Observations	11,163	11,163	11,163
Within R-squared	0.062	0.061	0.047

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Panel A			
	(1)	(2)	(3)
VARIABLES	Haircut	Haircut	Haircut
post×FundingGap	0.009*	0.009*	0.008
	(0.005)	(0.005)	(0.018)
post	-0.074*		
	(0.043)		
FundingGap	-0.575***	-0.550***	
	(0.115)	(0.110)	
Day FE	N	Y	Y
Dealer FE	N	N	Y
Observations	11,163	11,163	11,163
Within R-squared	0.210	0.210	0.002

*** p<0.01, ** p<0.05, * p<0.1

Source: FRBNY Tri-party Repo Data, Author's Analysis

Table 4. Treatment Effect on Dealers: Treasury Collateral
This table runs a difference in difference regression around the date of the ON RRP rate increase. Post is an indicator for after the rate increase takes effect. FundingGap measures the post-technical adjustment funding cost, assuming the pre-adjustment portfolio remains the same, as defined in the main text. Standard errors are double clustered by dealer and time, and fixed effects are given at the bottom of the table. In the top panel the dependent variable is rates, and in the bottom panel it is the log of the total repo lending against non-Treasury collateral.

Panel A			
	(1)	(2)	(3)
VARIABLES	Collateral Quantity	Collateral Quantity	Collateral Quantity
post×FundingGap	-0.003	-0.003	-0.003
	(0.013)	(0.011)	(0.011)
post	0.009		
	(0.031)		
FundingGap	0.021*	0.021*	
	(0.012)	(0.013)	
Day FE	N	Y	Y
Dealer FE	N	N	Y
Observations	8,967	8,967	8,967
Within R-squared	0.019	0.019	0.000

*** p<0.01, ** p<0.05, * p<0.1

Panel B			
	(1)	(2)	(3)
VARIABLES	Rate	Rate	Rate
post×FundingGap	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
post	0.025***		
	(0.005)		
FundingGap	-0.006***	-0.006**	
	(0.002)	(0.003)	
Day FE	N	Y	Y
Dealer FE	N	N	Y
Observations	8,967	8,967	8,967
Within R-squared	0.108	0.52	0.000

*** p<0.01, ** p<0.05, * p<0.1

Panel C			
	(1)	(2)	(3)
VARIABLES	Haircut	Haircut	Haircut
post×FundingGap	-0.006*	-0.006*	-0.006*
	(0.003)	(0.003)	(0.003)
post	0.018		
	(0.012)		
	0.014*	0.003*	
	(0.008)	(0.002)	
Day FE	N	Y	Y
Dealer FE	N	N	Y
Observations	11,163	11,163	11,163
Within R-squared	0.210	0.210	0.002

*** p<0.01, ** p<0.05, * p<0.1

Source: FRBNY Tri-party Repo Data, Author's Analysis

Table 5. Treatment Effect at the DealerXCollateral Level
The baseline specification is run at the dealerXcollateral level. The dependent variables are listed at the top of the table. DealerGap is defined identically to FundingGap in table 1 and 2, and is defined at the dealer level. CollateralGap is calculated similar to FundingGap, but calculated at the dealer×collateral level. Standard errors are double clustered by dealer and time, and fixed effects are given at the bottom of the table.

Panel A

	(1)	(2)	(3)	(4)
VARIABLES	Total Funding Costs	Total Funding Costs	Total Funding Costs	Total Funding Costs
post×CollateralGap	0.050***	-	0.061***	0.056***
	(0.007)	-	(0.007)	(0.007)
post×DealerGap	-	0.037**	-0.019	-0.019
	-	(0.011)	(0.012)	(0.012)
Dealer FE	N	N	N	Y
Collateral FE	N	N	N	Y
Day FE	N	N	N	Y
Observations	47,397	47,397	47,397	47,397
Within R-squared	0.063	0.043	0.083	0.044

*** p<0.01, ** p<0.05, * p<0.1

Panel B

	(1)	(2)	(3)	(4)
VARIABLES	Rate	Rate	Rate	Rate
post×CollateralGap	0.012***	-	0.011***	0.011***
	(0.004)	-	(0.003)	(0.003)
post×DealerGap	-	0.014**	0.003	0.004
	-	(0.006)	(0.004)	(0.003)
Dealer FE	N	N	N	Y
Collateral FE	N	N	N	Y
Day FE	N	N	N	Y
Observations	47,763	47,763	47,763	47,763
Within R-squared	0.171	0.079	0.202	0.060

*** p<0.01, ** p<0.05, * p<0.1

Panel C				
	(1)	(2)	(3)	(4)
VARIABLES	Collateral Quantity	Collateral Quantity	Collateral Quantity	Collateral Quantity
post×CollateralGap	-0.013*	-	-0.001	-0.001
	(0.007)	-	(0.009)	(0.009)
post×DealerGap	-	-0.031***	-0.030**	-0.029**
	-	(0.009)	(0.014)	(0.014)
Dealer FE	N	N	N	Y
Collateral FE	N	N	N	Y
Day FE	N	N	N	Y
Observations	47,763	47,763	47,763	47,763
R-squared	0.008	0.011	0.014	0.005

*** p<0.01, ** p<0.05, * p<0.1

Source: FRBNY Tri-party Repo Data, Author's Analysis

TABLE 6. Treatment Effect at the DealerXCollateral Level
The baseline specification is run at the dealerXcollateral level. The dependent variables are listed at the top of the table. DealerGap is defined identically to FundingGap in table 1 and 2, and is defined at the dealer level. CollateralGap is calculated similar to FundingGap, but calculated at the dealer×collateral level. Standard errors are double clustered by dealer and time, and fixed effects are given at the bottom of the table.

	(1)	(2)	(3)	(4)
VARIABLES	Term	Term	Term	Term
post×CollateralGap	-0.23***	-	-0.14	-0.08
	(0.08)	-	(0.09)	(0.08)
post×DealerGap	-	-0.39***	-0.22**	-0.39***
	-	(0.05)	(0.09)	(0.10)
Dealer FE	N	N	N	Y
Collateral FE	N	N	N	Y
Day FE	N	N	N	Y
Observations	47,397	47,397	47,397	47,397
Within R-squared	0.063	0.043	0.083	0.044

*** p<0.01, ** p<0.05, * p<0.1

Source: FRBNY Tri-party Repo Data, Author's Analysis

TABLE 7. Secondary Market Trading after Technical Adjustment
Difference in difference regressions are run following regression (11) with total trading volume as the dependent variable. DealerGap is defined identically to FundingGap in the main text, and matched to the transaction data by hand. Panel A is run over transactions occurring with non-dealers (i.e. clients) and panel B is interdealer trading. Each columns includes different sets of fixed effects, given at the bottom of the table. The first three columns of each panel run the same specification, with different fixed effects. The fourth column runs the same specification as column (3), but with an indicator for whether the trade is a buy or sell. Standard errors are clustered at the same level as the fixed effects.

Panel A

VARIABLES	(1) Trading Volume	(2) Trading Volume	(3) Trading Volume	(4) Trading Volume
post×DealerGap	−0.057***	−0.061***	−0.027**	−0.030**
	(0.020)	(0.020)	(0.011)	(0.014)
post×DealerGap× Buy Order				0.006
				(0.015)
Buy FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	Yes	Yes
Dealer FE	No	No	Yes	Yes
Observations	27,245	27,245	27,245	27,245
Within R^2	0.005	0.003	0.000	0.008

*** p<0.01, ** p<0.05, * p<0.1

Panel B

VARIABLES	(1) Trading Volume	(2) Trading Volume	(3) Trading Volume	(4) Trading Volume
post×DealerGap	−0.041***	−0.044***	−0.032***	−0.040**
	(0.016)	(0.017)	(0.011)	(0.014)
post×DealerGap× Buy Order				0.014
				(0.014)
Buy FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	Yes	Yes
Dealer FE	No	No	Yes	Yes
Observations	29,749	29,749	29,749	29,749
Within R^2	0.008	0.007	0.000	0.17

*** p<0.01, ** p<0.05, * p<0.1

Source: FRBNY Tri-party Repo Data, FINRA TRACE, Author's Analysis

Table 8. Spreads and Profitability after the Adjustment
Difference in difference regressions are run following regression (12) and (11) with profitability as the dependent variable. The first three columns present the results for bid asp spreads, and the second three for profitability. Each set of columns includes different sets of fixed effects, given at the bottom of the table. Standard errors are given below coefficient estimates and are clustered at the same level as the fixed effects given below.

VARIABLES	(1) Spread	(2) Spread	(3) Spread	(4) Profit	(5) Profit	(6) Profit
post×DealerGap	0.013***	0.013***	0.014***	−0.037***	−0.030**	−0.030**
	(0.004)	(0.004)	(0.005)	(0.01)	(0.01)	(0.01)
post	−0.026***			−0.12***	−0.12***	
	(0.003)			(0.025)		
DealerGap	0.002	0.002		0.06**		
	(0.025)	(0.025)		(0.03)		
Observations	1,221,201	1,221,201	1,221,201	12,481	12,481	12,481
R^2	0.001	0.002	0.247	0.017	0.028	0.855

*** p<0.01, ** p<0.05, * p<0.1

Source: FRBNY Tri-party Repo Data, FINRA TRACE, Author's Analysis