The Liquidity Impacts of Volcker Rule Deregulation on Corporate Bonds

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Abstract

I find via a difference-in-differences analysis on corporate bond transactions that the deregulation of the Volcker Rule had no statistically significant effect on the liquidity of these bonds. These findings expand upon an existing body of literature which examines the liquidity impacts of the Volcker Rule's proprietary trading restrictions on large commercial banks which were enacted in response to financial instability in the Great Recession. This paper's statistical approach leverages a broad collection of transaction data between June 2019 and September 2022, and measures the convertibility of bonds using the Amihud metric. However, this paper notes that additional statistical methods are necessary to more fully understand the liquidity impacts of Volcker Rule deregulation both with respect to empirical strategy and liquidity measurement[1].

[1] I am incredibly grateful for the assistance of Professor Jón Steinsson for the design and completion of this work. The data collection, cleaning, construction, and visualization in this paper would not have been possible without the insightful assistance of my research assistant and data engineer, Jackson Keane. I would further like to acknowledge the librarian of the University of California's Economics Department, James Church, for his assistance with data collection by helping me gain access to the Wharton Research Data Service. I take full responsibility for all shortcomings in this work.

Introduction

My research is motivated by the inherent tension between financial regulation and the availability of credit in the financial system. As a core component of the Dodd-Frank Act passed in response to the financial crisis in 2008 and 2009, the establishment of the Volcker Rule offered researchers a unique lens into the dynamics of the regulation-liquidity tradeoff. Regulation of the corporate bond market in the last twenty five years has ebbed and flowed (Wu, 2020). After Congress repealed the Glass Steagall Act in 1999, prohibitions on lending from commercial banks to investment banks were extricated from the law. Commercial banks were able to take riskier assets onto their balance sheets which implicitly increased the risk to consumer deposits (Yu, 2003). In the aftermath of the Great Recession, however, the trajectory of regulatory scrutiny reversed course. After a series of the largest commercial banks were rescued by a combination of leveraged buyouts, emergency loans and balance sheet relief from the Federal Reserve and the US Treasury Department, calls for an overhaul of statutory risk-mitigation requirements achieved a bipartisan chorus (Hryckiewicz, 2014)**.**

Congress answered this call with the passage of the Dodd-Frank Act. This watershed legislation included key provisions, including higher capital requirements for banks, new "stress testing" for banks, and novel limitations on the investments and proprietary trades with which large depository institutions were allowed to engage. This research builds on a body of literature that examines the liquidity impacts of that final component. Influenced by the policy prescriptions of former Chairman of the Federal Reserve, Paul Volcker, a core component of the Dodd-Frank Act that carried his namesake prohibited all commercial banks with consolidated assets greater than ten billion USD from engaging in proprietary trading (Thakor, 2012). After a series of intensive public comment, five federal agencies including the Federal Reserve implemented the final rule in 2014 and included a trading exception for these banks' market making responsibilities. Investments in corporate bonds and venture funds outside the scope of market making became legally prohibited within these covered firms (Thakor, 2012).

At the same time the Volcker Rule was being finalized up until its initial implementation in 2014, a new phenomenon emerged in financial markets. Commercial banks began holding reserves in excess of their reserve requirements well into the recovery from the financial crisis (Allahrakha, 2019). This phenomenon was without historical precedent: commercial lending entities had held only enough reserves to meet their requirements for nearly as long as the Federal Reserve Bank in St. Louis has collected data on the statistic (Federal Reserve Bank of St. Louis). While the Federal Reserve did begin offering its depositors a positive interest rate on reserves, the literature has speculated that suboptimal levels of reserves have been used for lending in the real economy despite the availability of good credit (Walter, 2009). The imposition of the Volcker Rule, therefore, posits a potential reconciliation to the paradox of excess reserves being held at the Federal Reserve in a market of good creditors to which funds have not been lent. Lending from commercial banks into the real economy is one of the Federal Reserve's primary channels between monetary policy and consumption and investment decisions made by firms and consumers. The imposition of a ban on proprietary trading amongst corporate banks was concurrent with a buildup of more than 2.7 trillion USD in excess reserves at peak before the COVID-19 recession which, by definition, were never lent into the real economy (Federal Reserve Bank of St. Louis).

The Volcker Rule, therefore, creates at least two research questions that carry important implications for the Federal Reserve's monetary policy mechanisms. First, does the Volcker Rule negatively impact credit conditions by deteriorating the liquidity of corporate bonds? Second, can the Volcker Rule explain inaccessibility in credit markets after 2014? Evidence that the liquidity of corporate bonds improved after the Volcker Rule was softened in 2020 would be suggestive of 1) confirmation that regulatory relaxation improved liquidity and credit access in the market for corporate debt and 2) a potential justification for the disequilibrium in the market for loanable funds. Since 2014, the literature has capitalized on the coverage requirement of the Volcker Rule to analyze corporate bond liquidity, liquidity acting as a proxy for credit access (Trebbi, 2015). Because the rule only applies to covered firms, bonds underwritten by firms that are not covered by the rule can be compared to bonds underwritten by covered firms. In their work *Regulation and Market Liquidity,* Trebbi, et al. (2015) performs this analysis to assess potential changes in corporate bond liquidity caused by the Volcker Rule by comparing differences in the liquidity of corporate bonds issued by covered and uncovered firms both before and after the rule was imposed in 2014. While the literature has extensively analyzed the liquidity impacts of the Volcker Rule's imposition, the liquidity effects of recent deregulation have been sparsely explored. This work, therefore, attempts to build on the literature's estimation of the Volcker Rule's liquidity impacts by studying changes in corporate bond liquidity in June 2020 when the rule was first softened using the methods deployed by Trebbi, et al. (2015).

The Federal Open Market Committee brought the federal funds rate to its effective lower bound during the COVID-19 pandemic, and coupled with the unparalleled concentration of excess reserves held by commercial lenders, monetary policy was again confronted by similar constraints central bankers faced in the Great Recession (Yilmazkuday, 2021). In June 2020, policy makers agreed to soften components of the Volcker Rule designed to prevent direct lending from commercial banks and other investment firms (known collectively as "covered funds") to hedge funds and private equity funds which are viewed as comparatively riskier than holding deposits in a Federal Reserve account for instance (Board of Governors, 2020). By relaxing the lending exclusions and other restrictions on finance between covered depository institutions and these riskier funds, it becomes reasonable to examine if substantive changes in the liquidity of corporate debt occurred as a result of this deregulation.

Figure 1 describes the difference-in-difference analysis performed by Trebbi, et al (2015). They reviewed all corporate bond transactions between the first quarters of 2013 and 2015 that were reported to the Financial Industry Regulatory Authority. For each corporate bond in the sample they constructed a monthly measure of illiquidity, using the Amihud metric. This metric measures the price impact of a trade per unit traded, and is commonly used as a baseline for understanding temporal shifts in liquidity for a variety of financial assets. By summing the bond's returns and dividing by the total transaction volume in a month and then by the total number of monthly transactions, this measure captures the impact of a single trade on the relative return of a bond. Higher values imply lower liquidity levels because larger price impacts are suggestive of greater volatility (both below and above the bond's initial offering price). Finally, they matched each corporate bond not covered by the Volcker Rule to a covered bond that shared the same issuance data, credit rating, and a similar issuance size because these qualities directly impact bond liquidity. The vertical line in the figure represents the original finalization of the Volcker Rule in 2014, and as the figure makes visually clear, no statistically significant changes in liquidity between covered and uncovered bonds were observed after the regulation was initially implemented. My work will apply this same empirical approach used by Trebbi, et al. to examine whether a statistically significant shift in bond-level liquidity occurred in the 18 months following the regulatory softening of the Volcker Rule in June 2020.

Relevance to Contemporary Banking

As rising interest rates exert downward pressure on equity prices, bonds naturally become a comparatively more attractive asset. But the banking sector in the first quarter of 2023 was unique. The same set of deregulations that softened the Volcker Rule in 2020 included provisions that lower capital requirements and diminish the stringency of stress tests at smaller regional banks with lower levels of trading activity (Board of Governors, 2020). In the beginning of 2023 the Federal Reserve was pressured to relax its monetary tightening in response to a series of regional bank failures that put pressure on the solvency of larger European banks, and on the equity values of American commercial banks (Rennison, 2023).

While not the only reason, the smaller, regional bank failures occurred in part because these institutions took on a calamitous amount of interest rate risk by holding low par value, long-term treasury bonds on their balance sheets without adequate planning for a strong intervention from the Federal Reserve (Rennison, 2023). In other words, this breaking point in the financial system presents a unique threat in current financial markets, where the ability to convert between assets, namely bonds, will serve as an important mechanism to counteract the buildup of risk. In the context of the collapse of Silicon Valley Bank and Signature Bank, the systemic factors which influence the liquidity of corporate bonds can inform risk prevention strategies. And when depositor flight at regional banks threatens to create larger banking panics, liquidity conditions will influence the ability of these banks to post collateral on emergency loans and make sudden, dramatic changes to their balance sheets. The convertibility of bonds to cash or more liquid assets should, therefore, play a role in the cost-benefit analysis of banking regulation.

Literature Review

The literature examining the liquidity impact of the initial implementation of the Volcker Rule in 2014 is robust but lacks a uniform consensus. In his work *The Economic Consequences of The Volcker Rule,* Thakor relies on standard economic financial theory alongside earlier studies of the consequences of the Volcker rule in a single compendium. Thakor finds that the Volcker rule will have a negative effect on liquidity provision and market making for securities, and increase borrowing costs for lendees because commercial banks are not just brokers that connect lenders and borrowers, but “qualitative asset transformers” that can absorb supply and demand imbalances. Thakor does not use original data sets, but instead relies on the well-established empirical works of Adrian and Shin (2007); Brunnermeier and Pedersen (2009); and Comerton-Forde, Hendershott, Jones, Moulton, and Seasholes (2010) to show that the absence of a market maker has persistently positive price impacts on trades. The empirical literature studying the precise liquidity impacts of the Volcker Rule are partially agreeable to Thakor’s conclusion.

In their work *The Effects of the Volcker Rule on Corporate Bond Trading: Evidence from the Underwriting Exemption,* Allahrakha, et al. find that the Volcker rule had no effect on the net riskiness of covered dealers’ trades, and increased the cost of trades by 20 to 45 basis points after correcting for contemporaneous market conditions influencing the risk premium and exogenous factors impacting the liquidity of corporate bonds. As a result of the Volcker Rule, substantial supplies of liquidity for corporate bonds were cut out of markets, which as Allahrakha, et al. show, diminished the market share of covered dealers in the corporate bond market to non-covered dealers. Allahrakha, et al. make use of the Trade Reporting and Compliance Engine (TRACE) to assess the mean risk premium, volatility, and maturity of corporate bonds, the Mergent's Fixed Income Securities Database (FISD) to assess whether or not a bonds is new or "seasoned" (ie. more than 40 days past issuance), and Federal Reserve's National Information Center (NIC) to hand-match dealers identified from TRACE data to their parent institutions, and therefore, whether that dealer represents a Volcker-covered firm. Their empirical method is unique: in order to tease out the effect of the Volcker Rule on 1) the volatility of corporate bonds trades by covered dealers and 2) the markup due to diminished liquidity, they use a difference in differences approach to assess the impact of "switching on and off the Volcker Rule *within* an individual covered dealer as well as *within* a particular bond".

In contrast, however, Trebbi, et al. in their work *Regulation and Market Liquidity* find that the imposition of the Volcker Rule had no tangible effect on the liquidity of corporate debt. Using a standard difference-in-differences approach aforementioned and a multiple break-point testing scheme, they show that no discernible change in the liquidity of corporate bonds that were being traded by firms that escaped the purview of the Volcker Rule took place after 2014. The contradiction between Trebbi, et al. and Allahrakha's work, though, introduces a larger ambiguity that is ubiquitous throughout the literature examining bond liquidity: there is no standard or uniform measure of liquidity. While Allahrakha finds that there was a discernible increase in the premium of corporate bonds after the implementation of the Volcker Rule, Trebbi, et al. conclude that returns relative to the volume and frequency of trading were stable before and after the implementation. Differences in the basic conclusion on the liquidity impact of the Volcker Rule, therefore, should be interpreted in reference to a preferred measure of liquidity depending on the nature of the inquiry. But irrespective of the qualitative differences between

Trebbi, et al. and Allahrakha, the literature is generally conclusive that Volcker Rule had a deleterious effect on the profitability of covered and uncovered firms.

Adedipe in his work titled *The Impact of the Volcker Rule on Systematically Important Financial Institutions: An Event Study* finds that the Volcker Rule was not empirically effective at reducing equity returns at what they define as “Systematically Important Financial Institutions” or SIFI alone, but concluded that the rule after its imposition in 2014 also had negative effects on the abnormal equity firms of proprietary trading firms. Using data collected from Yahoo! Finance, Adedipe documents the stock prices of SIFIs, proprietary firms, and what he refers to as entities in the “control group” that operate similarly to SIFIs but are not subject to the Volcker rule. He uses the standard method of pinpointing “value-relevant information” by classifying an event as first-order if it appears on the front page of a relevant media outlet, and then uses a dummy regression to derive abnormal returns. In general, his result–that the regulation failed to isolate the intended effect on reducing abnormal equity growth to SIFIs rather than the larger financial system–builds on the aforementioned literature’s analysis of the SIFIs role in market making, and how the reduction in liquidity propagated by the Volcker rule could have downstream pricing implications for securities.

Data Construction

My sample includes all corporate bond transactions reported to the Financial Industry Regulatory Authority (FINRA) between June 2019 and September 2022. FINRA’s Trade, Reporting, and Compliance Engine (TRACE) houses a dataset labeled “Beta” which provides transaction-level information including return, trade volume, credit rating, issuance date and size, transaction date, and a bond's CUSIP ID for each transaction in the sample. In keeping with Trebbi, et al. we review only semiannual bonds that are not callable, convertible, putable, or have sinking fund provisions. We select semiannual bonds because they are the most commonly traded bonds which maximizes our sample size. After the original dataset is filtered on these characteristics, we are left with 494,247 unique transactions.

We merge the cleaned TRACE data with bond-level characteristics from the Fixed Income Securities Database (FISD) which houses information on bond underwriters. We link each bond in the sample to its respective lead underwriters. The FISD database, however, does not update a bond underwriter's legal information if the underwriting institution changes their name or becomes subsumed by a larger firm. Therefore, after associating each bond with its respective underwriter, we standardize the list of underwriters so that no underwriter is either duplicated in the sample or distinguished from its parent company. Finally, we filter the merged data for the bonds associated with the top fifty underwriters by the total volume of bonds each underwriter in the sample helped bring to market. This is in keeping with Trebbi, et al. and filters out less than twenty percent of the original merged data. By identifying the fifty largest underwriters we can manually classify each bond as covered or uncovered by the Volcker Rule.

Using the finalized version of the Volcker Rule or Section 13 of the Bank Holding Company Act, we manually classify each of the fifty remaining underwriters as covered by the Volcker Rule if they have consolidated total assets greater than ten billion and do not qualify for Volcker Rule exclusions, including those for foreign funds and broker-dealers. Finally, each bond in the data set is classified as covered by the Volcker Rule if at least one of its underwriters is barred from proprietary trading by the Volcker Rule, and classified as uncovered if none of its underwriters are barred from proprietary trading. Logically, if a bond was underwritten by a firm that was not covered by the Volcker Rule, then it faces fewer statutory trading constraints. Leveraging this categorical difference between uncovered and covered bonds is the touchstone of this analysis because we are interested in the extent to which the trading constraints placed on bonds that are holistically underwritten by covered firms are associated with larger liquidity premiums controlling for all other factors that distinguish the two types of bonds.

Amihud Metric

For each remaining bond in the sample we construct a monthly measure of illiquidity using a formula set forward by Amihud to ascertain the price impact of corporate bond transactions per unit traded. For the jth trade of a bond in the ith month, the monthly Amihud metric sums the absolute returns of a bond issue in month i divided by the total quantity of the bond traded in month i, and divides the sum of returns per unit traded by the number of trades in month i.

$$Amihud_i = \frac{1}{N_j} \sum_{j=1}^{N_j} \frac{|r_{j,i}|}{Q_{j,i}}$$

If the return on a bond is effectively zero, then the transaction had no impact on the market price of the bond because the dealer experienced no loss or gain. An asset with a constant, predictable price is intuitively more liquid than an asset possessing greater volatility in price because investors can be less certain the asset will be convertible to cash at the same price it was purchased. Therefore, summing the bond returns and dividing by the total trading quantity (in millions of dollars) in a given month yields a measure of illiquidity for the full transaction volume of a given bond on a monthly basis (Amihud, 2002). Larger returns suggest a bond was less liquid, and larger trading volumes decrease the price impact of single transactions because they become more diluted, so larger trading volumes are associated with higher levels of liquidity. In addition, higher trading volumes indicate that larger amounts of the asset are available for exchange which is suggestive of higher liquidity. Finally, to ascertain the price impact of the average monthly transaction of a given bond, we divide the aggregated returns by the number of monthly transactions. The final measure is recorded for each bond on a monthly basis between June 2019 and September 2022.

Difference-in-Differences Regression Model

I now deploy a traditional statistical estimation strategy using a modified difference-in-differences approach to ascertain discernible changes in the liquidity of corporate bonds in the period after the Volcker Rule was softened in June 2020. In keeping with Trebbi, et al. we construct two time series for the liquidity of corporate bonds that are both covered and exempt from the Volcker Rule. Each time series consists of a single or aggregated monthly value of liquidity depending on the number of uncovered corporate bonds for which a monthly Amihud metric of illiquidity exists. We proceed as follows. After manually classifying the top fifty underwriters, we identify the bonds for which at least one underwriter is not covered by the Volcker Rule and label this bond as “uncovered”. In order to isolate the differences in the liquidity of covered and uncovered bonds we do not simply compare monthly measures of liquidity for the two separate classes of bonds. We match each uncovered bond in our sample with a covered bond that shares the same issuance month, credit rating (high yield vs. investment grade), and a relatively similar issuance size such that the relative difference is less than 50% of the average of the two issuances. Issuance date, credit rating, and issuance size are all covariates independently influencing liquidity, so we seek to pair bonds that share these qualities so that any liquidity differences are due solely to regulatory factors (Trebbi, 2015). For bonds that receive multiple matches, we keep the bond with the smallest relative size difference. We aggregate the pairs of covered and uncovered bonds by month and calculate the average Amihud illiquidity metric for each group of covered and uncovered bonds for each month in the sample for visual coherence in Figure 3, but the regression is applied to the full final dataset. Finally, we normalize the average monthly metrics to zero in May 2020 so that differences in the pre- and post-softening periods are relative to liquidity values immediately preceding the regulatory relaxation. This final procedure ensures that underlying differences in liquidity between covered and uncovered bonds that may be due to differences in an underwriter's impact on a bond's liquidity are not part of the regression estimation. We finish with 923 pairs of bonds.

With the final two time series constructed, we deploy a standard difference-in-differences regression model that controls for a second order polynomial in a bond's issue age, time-fixed effects, and bond-fixed effects according to the following specification:

$$metric_{i,t} = \alpha + (issue\ age\ ) + (issue\ age\ )^2 + covered_i +$$

$$post\ softening_t + (covered \times post\ softening)_{i,t} + \epsilon_{i,t}$$

The second-order polynomial in issue age accounts for the non-linear relationship between a bond's issue age and the Amihud illiquidity metric due to natural fluctuations in the business cycle across time. The time-fixed effects account for macroeconomic fluctuations creating disparate liquidity conditions in the pre- and post-regulatory relaxation, and the bond-fixed effects account for differences between the liquidity of each bond category based on whether the bond is covered by the Volcker Rule. Finally, the coefficient '$(covered \ \times post\ softening)$' estimates the change in the difference between the liquidity of covered and uncovered bonds both before (one year) and after (two years) the period of regulatory softening.

Discussion

Table 1 reports the results of our difference-in-differences model. These results demonstrate no statistically significant departure from the difference between covered and uncovered bonds before the regulatory relaxation at the five-percent level. Had the regulatory relaxation in June 2020 caused a unique increase in the liquidity of bonds previously covered by the Volcker Rule, our results would have shown a systematic relative decline in the Amihud illiquidity metric for covered bonds. Without evidence of liquidity enhancement in the group of bonds covered by the Volcker Rule, I cannot reject the null hypothesis that relaxing the Volcker Rule had no statistically significant effect on the liquidity of corporate bonds.

Had there been a statistically significant effect on the liquidity of corporate bonds, Figure 2 would show a comparatively larger gap between the blue line (tracking the illiquidity of covered bonds) and the red line (tracking the illiquidity of uncovered bonds) after the horizontal line denoting the onset of deregulation. Specifically, the blue line should be categorically lower after this point in time. In Figure 3 the visual discrepancy between the blue and red data would have been more pronounced had the difference-in-differences technique revealed a statistically significant effect on the liquidity of corporate bonds. These visualizations demonstrate that there was no discernible effect on the liquidity of corporate bonds.

However, while this difference-in-differences model is a standard tool familiar to applied econometricians, it is limited in two critical ways. First, the model inherently assumes that changes in the liquidity difference between Volcker Rule and non-Volcker Rule bonds should be tested at one breakpoint. Specifically, the model assumes that if deregulation had an impact on the liquidity of corporate bonds, that this effect is observable in one period and immediately after the implementation of the regulation. As the literature has demonstrated, however, the effects of regulation and predictable shifts in financial markets on general financial conditions can become apparent before those shifts occur if market participants anticipate them (Winkler, 2000). Further, due to the schedule at which regulation is implemented, ie. the speed at which new compliance dates are set after the initial implementation of regulation etc., the full effects of regulation can be delayed. This prompts, therefore, a more nuanced analysis where multiple "breakpoints" are tested for shifts in liquidity conditions rather than only testing the date of implementation (Trebbi, 2015). Finally, because our analysis demands an intensive matching procedure, we eliminate a large portion of our original data to conform to the constraints of our matching protocol. It is essential to eliminate the effect of covariates on bond-level liquidity in order to isolate the effect of deregulation on changes in liquidity, but a more robust sample size would certainly strengthen our analysis.

A difference-in-differences analysis also requires a discussion of the "parallel trends" assumption. Because the starting date for this analysis is chosen somewhat arbitrarily (for the purpose of data symmetry), results that show a statistically significant effect on the treated group rely on an assumption that the "larger" or "smaller" gap created by the treatment was not created by a cause applied to both groups before the first observation date. In other words, if two variables are naturally growing apart from one another over a long period of time the difference-in-differences analysis could hypothetically demonstrate a statistically significant treatment effect that does not support causal inference if a subsection of the data is selected for observation. Without verifying that the “change” in the difference between treatment and control groups was not set in motion by variables affecting one or both groups before the first observation, no analysis of this kind can definitively assert causality. In the case of this study, verifying that the liquidity of covered and uncovered bonds exhibited parallel trends before the first observation date is not necessary because this analysis has demonstrated no statistically significant difference between these groups of bonds. If this analysis is replicated in the future, however, statistical techniques that analyze the movement of these time series before the first observation date will be necessary to confirm the inference that a treatment date had a statistically significant effect on the liquidity of corporate bonds.

Conclusion

This study examined the impact of the softening of the Volcker Rule on the liquidity of corporate bonds. In June 2020 five federal agencies eliminated restrictions the Volcker Rule placed on depository institutions with consolidated assets greater than ten billion USD. Covered firms regained the ability to invest in venture funds and expand their proprietary portfolios. The literature’s estimation of the Volcker Rule’s impact on corporate bond liquidity premia is irresolute. Evidence that the rule enhanced the liquidity premium of corporate bonds is in conflict with evidence that standard measures of liquidity showed no systematic change after the implementation of the rule. The literature has also speculated that the loanable funds market has remained in disequilibrium after the Great Recession as evidenced by the excessive buildup of deposits at the Federal Reserve. Researchers have speculated that enhanced regulation could explain this imbalance between supply and demand if analytical methods could demonstrate a causal link between trading restrictions and elevated debt premia.

This research contributes to both of these empirical questions. We examine whether the 2020 deregulation improved the liquidity of corporate bonds. We clean the "Beta" dataset retrieved from TRACE which provides a detailed description of all corporate bond transactions reported to FINRA between June 2019 and September 2022. The dataset includes each bond trade's return, volume, credit rating, issuance size and date along with other relevant metrics. We clean this dataset in keeping with Trebbi, et al. and merge it with underwriter information retrieved from FISD. This merged data links each bond in our sample to its lead underwriters. We select the top fifty underwriters with respect to trade volume and manually classify each underwriter as covered or uncovered by the rule pursuant to proprietary trading restrictions and other exclusions contained in the Volcker Rule. We classify a bond as uncovered if at least one of its underwriters is exempt from the Volcker Rule, and keep each bond in the sample that is uncovered. Finally, we match each uncovered bond in the sample with a covered bond that shares the same credit rating, issuance date, and relative issuance size.

For each bond in the final sample we construct a measure of illiquidity using the Amihud metric. This metric measures the price impact of a trade per unit traded, where higher values suggest lower levels of liquidity. Using a standard difference-in-differences estimation strategy we show that Volcker Rule deregulation had no statistically significant effect on the liquidity of corporate bonds. Our analysis contributes to the discussion of the Volcker's Rule's impact on bond-level liquidity and on the cause of a potential disequilibrium in the loanable funds market. Should further research continue to explore the liquidity effects of June 2020 Volcker Rule deregulation, analytical methods that deploy multiple breakpoint testing similar to those in the work of Trebbi, et al. would be advisable to account for the potential staggered impact that deregulation has on measures of bond liquidity. In addition, the Volcker Rule was eliminated in its entirety, so our work is not symmetric in analytical power to the work of Trebbi and other authors aforementioned. Should a full repeal or a reinstatement of the provisions that were rolled back in June 2020 take place, further research should apply the analytical methods in this paper to those treatment dates.

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Appendix

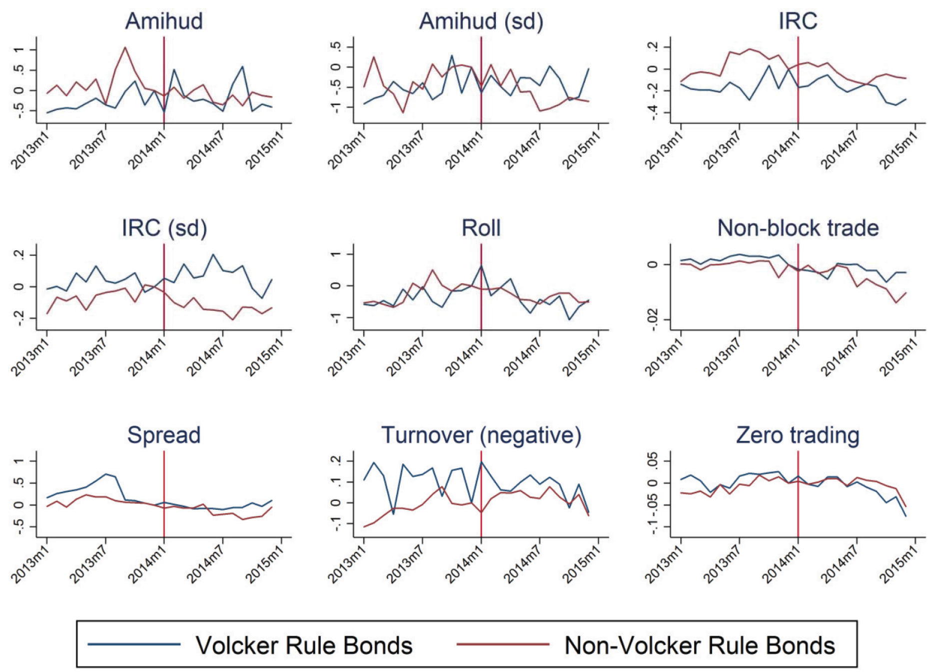

Figure 1 (Source: Trebbi, et al., 2015)

	Dependent variable:
	AMIHUD
One_OD	-0.056^{***}
	(0.014)
One_ODSqrd	0.000^{***}
	(0.000)
cov	1.644
	(1.018)
Before_After	-0.369
	(0.873)
cov:Before_After	-1.361
	(1.188)
Constant	$564{,}790.100^{***}$
	(145,160.500)
Observations	38,281
R^2	0.001
Adjusted R^2	0.001
Residual Std. Error	51.289 (df = 38275)
F Statistic	8.234^{***} (df = 5; 38275)
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 1

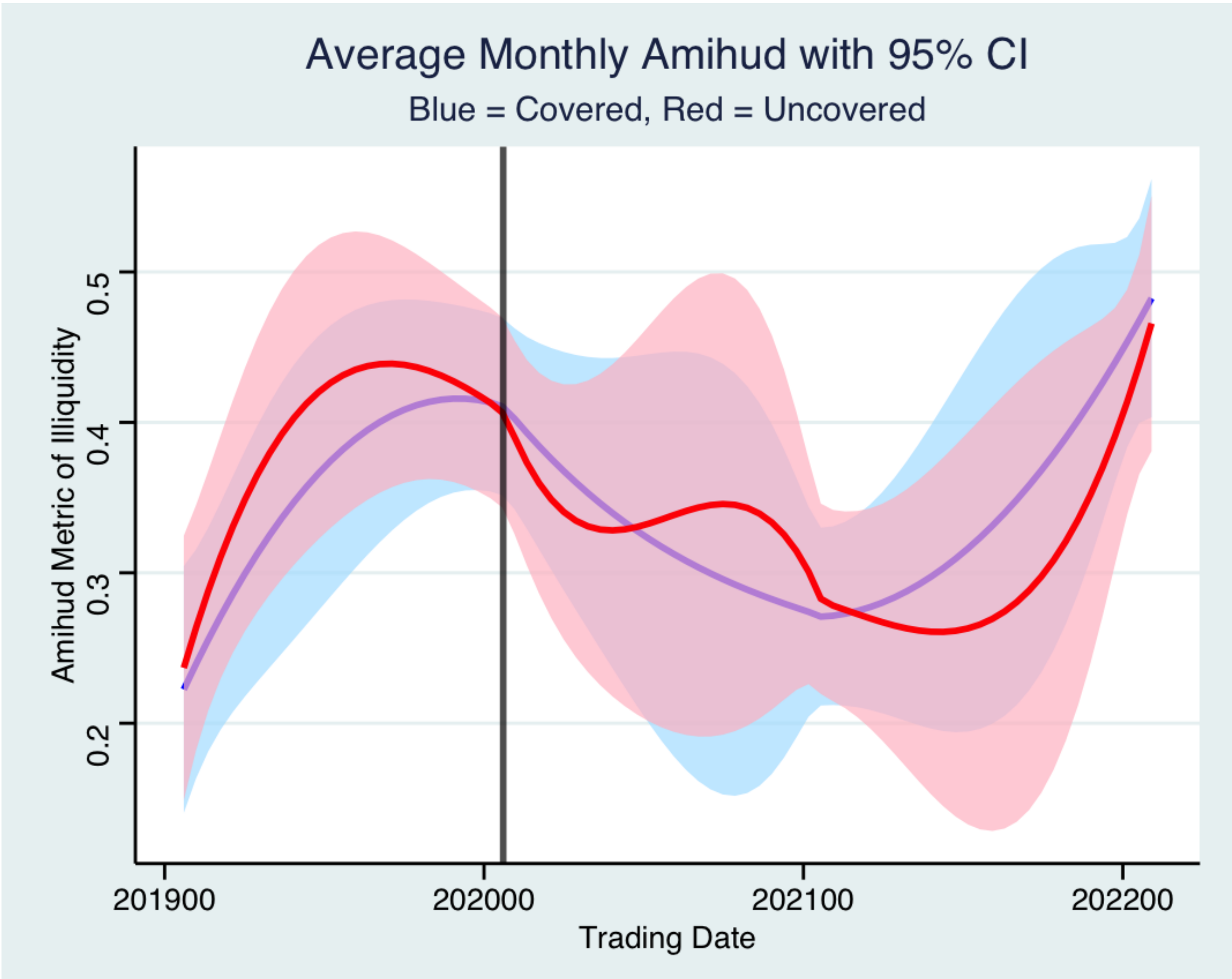
Average Monthly Amihud with 95% CI
Blue = Covered, Red = Uncovered
Amihud Metric of Illiquidity
0.2
0.3
0.4
0.5
201900
202000
202100
202200
Trading Date

Figure 2

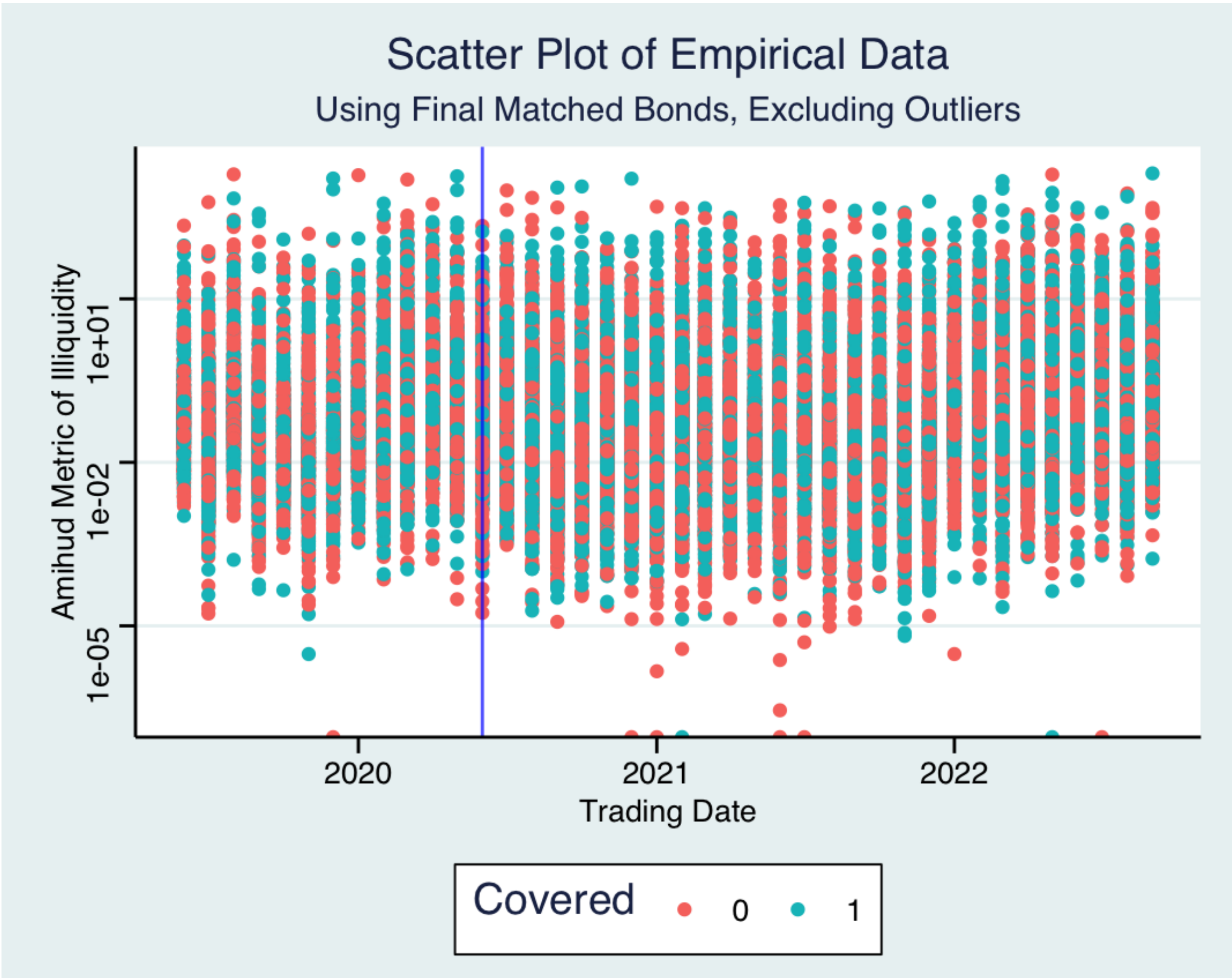
Scatter Plot of Empirical Data
Using Final Matched Bonds, Excluding Outliers
Amihud Metric of Illiquidity
1e+01
1e-02
1e-05
2020
2021
2022
Trading Date
Covered
0
1

Figure 3