

Regulation and Market Liquidity

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Abstract

We examine the effects of post-crisis financial regulation, encompassing the Dodd-Frank Act and Basel III, on market liquidity of the U.S. fixed income market. We estimate structural breaks in a large panel of liquidity measures of corporate and Treasury bonds. Our methodology does not require a priori knowledge of the timing of breaks, can capture not only sudden jumps but also breaks in slow-moving trends, and displays excellent power properties. Against the popular claim that post-crisis regulation hurt liquidity, we find no evidence of liquidity deterioration during periods of regulatory intervention. Instead, breaks towards higher liquidity are often detected.

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The aftermath of the 2008-09 financial crisis has witnessed one of the most active periods of regulatory intervention in U.S. financial history since the New Deal (Barr, 2012). A centerpiece of this sweeping reaction to the near collapse of the financial system, the Dodd-Frank Wall Street Reform and Consumer Protection Act (Dodd-Frank), was signed into law in July 2010. With Dodd-Frank, hundreds of regulatory rulemaking requirements have been subsequently met, affecting virtually every dimension of modern financial activity, from derivatives trading to housing finance to capital requirements for depository institutions. In the backdrop of this intervention, a lack of rigorous assessment of the complex costs and benefits of the new rules has been highlighted (Cochrane, 2014). While Law scholars have been active in the regulatory debate at the qualitative level, quantitative work in Economics and Finance has been occasional and surprisingly sparse.

Pertinently to this debate, this paper investigates the crucial claim that U.S. post-crisis financial regulatory over-reach might have adversely affected the provision of market liquidity of a vast class of financial assets, structurally decreasing liquidity levels and increasing liquidity risk in fixed-income markets across the board.

Such claim is linked, but not uniquely, to a specific set of provisions embedded within recent legislation, the so-called Volcker Rule, statutorily delineated in Section 619 Title VI of the 2010 Dodd-Frank Act and finalized by multiple regulatory agencies in January 2014. According to this provision, any banking entity is prohibited from engaging in proprietary trading or from acquiring or retaining an ownership interest in, sponsoring or having certain relationships with a hedge fund or private equity fund, subject to certain exemptions. Although this is in no way the only dimension of Dodd-Frank along which serious welfare losses or liquidity shortages could have been potentially triggered, it emerged as one of the most hotly debated, with roughly 17,000 public comments filed during the process of federal regulatory rulemaking (Bertrand, Bombardini and Trebbi, 2015). Specifically, some commentators¹ have highlighted how by placing undue artificial

¹For instance regulators write in the final version of the Volcker Rule (p.5578 Federal Register / Vol. 79, No. 21 / Friday, January 31, 2014 / Rules and Regulations) "As discussed above, several commenters stated that the proposed rule would impact a banking entity's ability to engage in market making related activity. Many of these commenters represented that, as a result, the proposed exemption would likely result in reduced liquidity[...]" and the Federal Register explicitly mentions on the matter of reduced liquidity comments received from "AllianceBernstein; Rep. Bachus et al. (Dec. 2011); EMTA; NASP; Wellington; Japanese Bankers Ass'n.; Sen. Hagan; Prof. Duffie; Investure; Standish Mellon; IR&M; MetLife; Lord Abbett; Commissioner Barnier; Quebec; IIF; Sumitomo Trust; Liberty Global; NYSE Euronext; CIEBA; EFAMA; SIFMA et al. (Prop. Trading) (Feb. 2012); Credit Suisse (Seidel); JPMC; Morgan Stanley; Barclays; Goldman (Prop. Trading); BoA; Citigroup (Feb. 2012); STANY; ICE; BlackRock; SIFMA (Asset Mgmt.) (Feb. 2012); BDA (Feb. 2012); Putnam; Fixed Income Forum/Credit Roundtable; Western Asset Mgmt.; ACLI (Feb. 2012); IAA; CME Group; Wells Fargo (Prop. Trading); Abbott Labs et al. (Feb.14, 2012); Abbott Labs et al. (Feb. 21, 2012); T. Rowe Price; Australian Bankers Ass'n. (Feb. 2012); FEI; AFMA; Sen. Carper et al.; PUC Texas; ERCOT; IHS; Columbia Mgmt.; SSgA (Feb. 2012); PNC et al.; Eaton Vance; Fidelity; ICI (Feb. 2012); British Bankers' Ass'n.; Comm. on Capital Markets Regulation; Union Asset; Sen. Casey; Oliver Wyman (Dec. 2011); Oliver Wyman (Feb. 2012) (providing estimated impacts on asset valuation, borrowing costs, and transaction costs in the corporate bond market based on hypothetical liquidity reduction scenarios); Thakor Study. The Agencies respond to comments regarding the potential market impact of the rule in Part IV.A.3.b.3., infra."

limits on securities inventory and retained risk and directly affecting inter-dealer trading, the Volcker Rule could have severely limited market liquidity². When recently the Congressional debate shifted on the merits of regulatory relief, one of the provisions considered for rolling back within Dodd-Frank included the prohibition of proprietary trading on the part of insured banking entities and their affiliates below certain thresholds³.

A balanced view of the potential adverse welfare consequences of such provision is summarized in Duffie (2012): *“The Agencies’ proposed implementation of the Volcker Rule would reduce the quality and capacity of market making services that banks provide to U.S. investors. Investors and issuers of securities would find it more costly to borrow, raise capital, invest, hedge risks, and obtain liquidity for their existing positions. Eventually, non-bank providers of market-making services would fill some or all of the lost market making capacity, but with an unpredictable and potentially adverse impact on the safety and soundness of the financial system. These near-term and long-run impacts should be considered carefully in the Agencies’ cost-benefit analysis of their final proposed rule. Regulatory capital and liquidity requirements for market making are a more cost effective method of treating the associated systemic risks.”* Duffie (2012) further remarks on the needs for an appropriate assessment of the cost and benefits of the rule, an assessment that the empirical analysis we perform systematically complements. Thakor (2012) raises similar issues.

Another focal point of post-crisis regulatory reform has been the Basel III framework, which was produced in 2010 by the Basel Committee on Banking Supervision at the Bank for International Settlements. The Basel III final rule adopted by the U.S. federal banking regulators also implements some provisions from the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 (Dodd-Frank Act; P.L. 111-203), which also addressed capital reserve requirements for banks (Getter, 2014). Basel III demands higher capital and liquidity buffers for banks, and imposes leverage restrictions on systemically important financial institutions. Despite the fact that higher levels of bank capital may reduce the probability of another financial crisis, critics claim that these regulations might have unduly constrained banks’ ability to deploy capital to market-making, and forced banks to charge clients more to use their balance sheet when they facilitate

Available at <http://www.gpo.gov/fdsys/pkg/FR-2014-01-31/pdf/2013-31511.pdf>

²For example, on May 20, 2015 The Wall Street Journal in an article titled "Why Liquidity-Starved Markets Fear the Worst" reports "[...] a large part of the explanation lies in changes to regulation aimed at addressing weaknesses exposed by the financial crisis. Banks must now hold vastly more capital, particularly against their trading books. The ring-fencing of proprietary trading in the U.S. and retail banking in the U.K. has also squeezed liquidity." Similar reasoning is implied by Alan Greenspan on the Financial Times on August 17, 2015, who writes "Lawmakers and regulators, given elevated capital buffers, need to be far less concerned about the quality of the banks’ loan and securities portfolios since any losses would be absorbed by shareholders, not taxpayers. This would enable the Dodd-Frank Act on financial regulation of 2010 to be shelved, ending its potential to distort the markets — a potential seen in the recent decline in market liquidity and flexibility."

³See S.1484 - Financial Regulatory Improvement Act of 2015, Title I: Regulatory Relief and Protection of Consumer Access To Credit. The bill is sponsored by Senate - Banking, Housing, and Urban Affairs Chairman Richard Shelby (R-AL).

trades or provide financing⁴.

This paper formally assesses the effect of the U.S. post-crisis regulatory intervention, encompassing the Dodd-Frank Act and Basel III, on market liquidity of a large portion of the U.S. fixed-income market.

Our biggest empirical challenge is the unknown timing of regulatory impact due to the protracted process of rulemaking and the associated anticipatory responses and lagging reactions of market participants. For example, the Volcker Rule took almost four years to finalize, with the deadline being postponed several times. During the four years of rulemaking, different banks wound down their proprietary trading desks at different times⁵. Conventional micro-econometric methods which compare liquidity before and after a treatment date are difficult to apply in this setting because it is unclear when regulation should have effects. The result of these methods could be sensitive to the assumption of the date around which the comparison is conducted⁶.

To address this challenge, we employ recent econometric approaches based on large factor models (Stock and Watson, 2011; Chen, Dolado and Gonzalo, 2014) to identify structural breaks in both levels and dynamic latent factors for a large set of liquidity proxies in fixed-income markets. Our empirical approach is attractive on several dimensions. First, our tests do not require a priori knowledge of the exact timing of the breaks. Second, we can capture not only sudden breaks in levels, but also breaks in slow-moving trends. Finally, the tests display excellent power properties and appear robust to confounding factors in a battery of Monte Carlo simulations.

We explore the market for U.S. corporate bonds, a heterogeneous asset class directly affected by the Volcker Rule and Basel III capital regulation. Exploiting the segmented nature of corporate bond market, we construct a large panel of liquidity measures by bond issue size, credit rating, and lead underwriter's identity. Given that original underwriters typically tend to make markets on the specific securities underwritten, this allows us to potentially identify bank-specific liquidity breaks and more nuanced disaggregated dynamics. We also study U.S. Treasuries, an asset class which is exempted from the Volcker Rule, but is still affected by the stringent capital regulation of Basel III. Several commentators have ascribed recent episodes of trading disruption (e.g. the flash crash of October 15, 2014) to liquidity depletion.

Against the popular claim that post-crisis regulation systematically hurt liquidity, we find no evidence of liquidity deterioration during periods of regulatory interventions. While our methodologies do not allow to exactly quantify the causal effect of specific regulatory provisions on market liquidity, our portfolio of tests rebuts the hypothesis of a permanent reduction in liquidity. The em-

⁴See "Global Macro Research: A Look at Liquidity", Goldman Sachs, August, 2015.

⁵The section "A Brief History of the Volcker Rule" in online appendix provides a detailed account of the rulemaking process of the Volcker Rule. Figure 1 in the online appendix provides a full timeline of post-crisis regulatory events.

⁶For example, if liquidity deterioration occurred before the regulation is implemented, a test comparing the liquidity around the date of implementation may find no liquidity reduction.

empirical evidence robustly shows either no breaks or statistically significant breaks toward higher liquidity in the aftermath of Dodd-Frank and Basel III. Under the shared assumption that Dodd-Frank and Basel III represented in fact massively consequential policy interventions, large negative effects on liquidity have to be rejected. This is a substantial contribution in sharpening the debate on post-crisis financial regulatory intervention in the U.S. and Europe. We also present concordant evidence from microeconomic approaches based on difference-in-differences of matched bonds samples that support these findings. Our work both qualifies frequent informal discussion on the lack of evidence of large deterioration in market liquidity provision, a view shared by a growing group of market participants and policy makers⁷, and is relevant to the rigorous assessment of the welfare consequences of the Dodd-Frank Act and Basel III in terms of hindering the market making capacity of large financial institutions, one of the main welfare costs observers have ascribed to the recent regulatory surge.

This paper employs four different estimation strategies. First, we employ standard multiple breakpoint testing (Bai and Perron, 1998, 2003) on the level of liquidity as a first-pass examination on the potential dates around which liquidity depletion may manifest. We find no evidence of liquidity depletion during the period of regulatory intervention (July 2010-December 2014), a period encompassing regulatory events such as the passage of the Dodd-Frank Act and Basel III, the proposal and finalization of the Volcker Rule, and related shutdowns of proprietary trading desk by different banks. On the contrary, statistically significant breaks toward higher liquidity are often detected during this period.

Our second and third methodologies apply recent econometric approaches based on large factor models (Stock and Watson, 2011) to capture breaks in latent factor structures in the large panel of disaggregated liquidity measures. Specifically, our second methodology focuses on single breakpoint testing for large dynamic factor models (Chen, Dolado and Gonzalo, 2014), while our third methodology extends to more a realistic multiple breakpoint case, transposing the intuition of Chen, Dolado and Gonzalo (2014) to Bai and Perron (2003) type tests. These methodologies

⁷For example, the semi-annual Monetary Policy Report of the Federal Reserve in July 2015 writes: "*Despite these increased market discussions, a variety of metrics of liquidity in the nominal Treasury market do not indicate notable deteriorations*", and "*similar to the Treasury market, a range of conventional liquidity metrics in corporate bond markets also generally do not point to a significant deterioration of market liquidity in recent years*". See also Dudley (2015) and the New York Fed's Liberty Street Economics blog series, in particular "Has U.S. Corporate Bond Market Liquidity Deteriorated?" by Adrian et al., Liberty Street Economics, October 05, 2015.

This view is also echoed by some market participants. A Wall Street Journal commentary titled "Overlooking the Other Sources of Liquidity" writes that "*fortunately for investors, recent reforms and regulatory pressures have dramatically increased the number of participants who can make prices and provide liquidity across many fixed-income markets. Markets that have opened to competition now enjoy better pricing, efficiency and resiliency*". The global head of credit at Morgan Stanley, Steve Zamsky, said that "in our day-to-day, moment-to-moment business today, marketplace works just fine". The chief investment officer of Oppenheimer Funds, Krishna Memani, the president of Bianco Research, Jim Bianco, and the president of Better Markets, Dennis Kelleher, also voiced scepticism on the "overheated" worries on bond market liquidity.

allow flexible forms of structural breaks (including breaks in trends, in serial correlation, or in factor loadings), and help us to answer the deeper question whether market liquidity would be higher or lower in absence of regulatory intervention. In simulations we show that our methodologies can successfully identify the onset of a gradual liquidity deterioration, even when masked by confounding factors, and accurately estimate the counterfactual path of liquidity using observed data.

We apply these methodologies to a large panel of disaggregate liquidity measures for corporate bonds. Our tests robustly capture breaks in latent liquidity dynamics at the start and at the end of the 2008-09 crisis (and indeed these tests can be employed to precisely time the beginning and end of the liquidity crisis). This reassures us on the tests having sufficient power within this specific empirical application. However, we find no systematic statistical evidence of structural breaks leading to lower liquidity during the period of regulatory intervention (July 2010-December 2014).

As opposed to time-series approaches delineated above, our fourth estimation strategy relies on a standard microeconomic approach in estimating liquidity deterioration around salient regulatory events, namely difference-in-differences matching (Heckman, Ichimura, Todd, 1997; Heckman, Ichimura, Smith, and Todd, 1998; Smith and Todd, 2005). In this part of the analysis we focus on the finalization of the Volcker Rule alone. We construct a dataset of bonds matched by issue size and credit rating, split between treatment and control based on whether the original underwriter is covered or not by Volcker Rule provisions. Matching allows for balancing between covered and non-covered bonds, assuaging concerns of attenuation due to heterogeneity across the two groups of securities.

Consistently across all four estimation strategies, this paper finds no systematic evidence of deterioration in liquidity levels or structural breaks in dynamic latent factors of the U.S. fixed-income market during periods of heightened regulatory interventions. This is in stark contrast to the popular claim that post-crisis would cause severe depletion in market liquidity. Instead, consistent with the view shared by an increasing group of policy makers and market participants, we find breaks toward higher liquidity during these periods, possibly due to entry of non-banking participants and increase in competition between market makers. We also document some changes in the market structure, notably the diminishing dealer inventory and the shift from principal-based trading towards agency-based trading. These evolutions in market structure started before the regulatory intervention, and do not appear to be associated with deterioration in commonly used liquidity measures. To the best of our knowledge, this is one of the very first studies to statistically assess liquidity depletion related to regulatory activity post-2008.

Our work is related to several strands of literature in both economics and finance. The first strand of literature studies the determinants and measurement of market liquidity. A recent comprehensive survey on this literature can be found in Vayanos and Wang (2012). Theoretical works

such as Grossman and Stiglitz (1980), Kyle (1985), Roll (1984), Grossman and Miller (1988), Amihud and Mendelson (1986), Gromb and Vayanos (2002), Duffie, Garleanu, and Pedersen (2005), and Brunnermeier and Pedersen (2009) relate illiquidity to underlying market imperfections such as participation costs, transaction costs, asymmetric information, imperfect competition, funding constraints, and search frictions. Many empirical works have since studied various measures of market liquidity across different asset classes, such as price impact (Amihud measure), price reversal (Roll measure), and bid-ask spreads. It has been shown that these liquidity measures are related to market frictions as suggested by theory, and can explain asset returns in both cross section and time series (see Amihud, Mendelson and Pedersen (2006) for a recent survey). Recent studies of fixed-income market liquidity can be found in Edwards, Harris, and Piwowar (2007), Bao, Pan and Wang (2011), Feldhütter (2011), Dick-Nielsen, Feldhütter, and Lando (2012), Krishnamurthy (2002), and Hu, Pan and Wang (2012).

A second strand of connected literature studies statistical tests of structural changes⁸. These methodologies have been widely used in the macroeconomic literature to study structural changes in inflation-output relations, labor productivity, and monetary policy regimes⁹. Our paper contributes to this literature by employing a test of multiple breaks with unknown dates in dynamic factor models, transposing the intuition of Chen, Dolado, and Gonzalo (2014) to Bai and Perron (1998) type tests. We show that this type of tests is particularly useful when the timing of regulatory impact is unclear.

A third and important strand of literature pertains to the cost-benefit analysis of financial regulation. By every stretch of imagination, this literature remains considerably underdeveloped relative to potential welfare benefits of rigorous and data-driven regulatory intervention. Such limitations have been lamented not only by financial economists such as Cochrane (2014), but have been central motivation of judicial intervention¹⁰. Cochrane (2014) discusses at length the complexity of deriving meaningful assessments of regulatory counterfactuals in financial and banking regulation, question also discussed in Posner and Weyl (2013). Relative to the pessimistic assessment in Coates and John (2014) of the infeasibility of meaningful cost-benefit analysis in financial and banking regulation¹¹, our paper offers a more optimistic counterpoint, at least in terms of ex-post

⁸Important theoretical contributions include Andrews (1983), Andrews and Ploberger (1994), Bai and Perron (1998), Stock and Watson (2002, 2011), and Chen, Dolado, and Gonzalo (2014).

⁹See McConnell and Perez-Quiros (2000), Hansen (2001), and Stock and Watson (2011) for examples of applications.

¹⁰Coates and John (2014) referring to *Business Roundtable et al. v. SEC*, 647 F. 3d 1144 (D.C. Cir. 2011), report that "*One panel of the U.S. Court of Appeals for the District of Columbia Circuit, composed entirely of Republican-appointed judges, has held that existing law requires the SEC to quantify the costs and benefits of its proposed rules*".

¹¹Specifically speaking about the Volcker Rule, Coates and John (2014, p.73): "*Could the agencies go beyond conceptual CBA and conduct a reliable, precise, quantified CBA/FR? The short answer is no. There is simply no historical data on which anyone could base a reliable estimate of the benefits of preventing banks from engaging in proprietary trading or investing in hedge and private equity funds.*"

quantitative assessment¹² along the specific dimension of market liquidity depletion. Related our study, Bessembinder et al. (2016) find that trade execution costs of corporate bonds have decreased over time, a finding consistent with ours. However, they interpret the decline in inventory and the shift of dealers' business model as a sign of liquidity deterioration induced by post-crisis regulation, while we find that the shift started before regulatory intervention, and does not seem to be associated with deterioration in other commonly used liquidity measures. In other OTC markets, Loon and Zhang (2016) provide evidence that Dodd-Frank improves the liquidity in the CDS market through several reforms such as public dissemination of transactions and central counterparty (CCP) clearing.

A fourth literature touched by this paper revolves around the post-financial crisis policy responses. McCarthy, Poole, and Rosenthal (2013) debate political distortions in post-crisis responses, an issue also explored in Frieden (2015) and Mian, Sufi, and Trebbi (2014). More explicitly, Mian, Sufi and Trebbi (2010) focus on the legislative response to the financial crisis pre-dating the Dodd-Frank Act, while Kaiser (2013) offers an interesting and detailed discussion of the congressional evolution of the Dodd-Frank Act itself. Finally, the regulatory rulemaking of Dodd-Frank is fully explored from a systematic empirical perspective by Bertrand, Bombardini, and Trebbi (2015).

The remainder of this paper is organized as follows. In Section 1 we discuss the main empirical measures, the variables construction, and provide a descriptive analysis of our samples. In Section 2 we discuss our econometric model and single breakpoint/multiple breakpoint testing in dynamic factor models. Our main empirical results on U.S. corporate bonds are reported in Section 3 and on Treasuries in Section 4. Section 5 concludes.

1 Data

1.1 U.S. Corporate Bonds Sample Description

The first main data set used for this paper is the Financial Industry Regulatory Authority's (FINRA) TRACE. This data currently provides transaction-level information of approximately 99% of all secondary corporate bond market transactions. Our sample period is from April 1, 2005 to December 31, 2014, covering the 2008-09 financial crisis and post-crisis regulatory interventions. We filter out erroneous trades following Dick-Nielsen, Feldhütter, and Lando (2012).

We merge the cleaned TRACE transactions to bond characteristics provided by Mergent Fixed Income Data. This data provides bond-level information such as issue date, issuance size, coupon rate, maturity date, credit ratings, underwriter identity and roles. Following Dick-Nielsen, Feldhütter, and Lando (2012), we limit the sample to fixed-rate bonds that are not callable, convertible,

¹²See also Cochrane (2014)'s discussion of retrospective analysis of financial regulation.

putable, or have sinking fund provisions. We drop bonds issued more than 10 years ago, since these old bonds present very few transactions. Since our goal is to provide the most comprehensive coverage of U.S. corporate bond market, we keep bonds with semi-annual coupons because they are the most common bonds in the U.S.. The raw TRACE data contains 34,422 bonds. After applying the above filters, our final sample contains 18,632 semi-annual coupon bonds¹³. Using the underwriting information from Mergent, we link each bond to its lead underwriters.

We first construct the nine measures for each corporate bond in our sample. Then we calculate the equal weighted average by bond rating group (investment-grade v.s high-yield) and issue size (above \$1 billion v.s. below \$1 billion) for each underwriter, which we refer as disaggregate series¹⁴. Since smaller underwriters only underwrite a limited number of bonds, this makes the underwriter-level measure of liquidity quite noisy. Therefore, we keep the top 4 biggest underwriters, Bank of America (Merrill Lynch), JPMorgan Chase, Morgan Stanley and Goldman Sachs, and combine the rest into a residual “Others” group. We also construct aggregate liquidity measures for the whole corporate bond market.

1.2 Corporate Bonds Liquidity Measures: Construction

Market liquidity is the degree to which investors can execute a given trade size within a given period of time without moving the price against the trade. We use the following nine liquidity measures which are commonly used in the literature to capture different aspects of liquidity (the easiness to trade, the pecuniary cost of trading, etc.). Previous literature has shown that these liquidity measures generally affect asset prices, indicating that investors indeed care about them¹⁵. All measures below are decreasing in the level of liquidity¹⁶.

1. *Amihud measure.* Amihud (2002) constructs an illiquidity measure based on the theoretical model of Kyle (1985). We use a slightly modified version of this measure following Dick-Nielsen, Feldhütter, and Lando (2012). The Amihud proxy measures the price impact of a trade per unit traded. For a given bond, define $r_{j,i,t}$ as the return and $Q_{j,i,t}$ as the trade size (in million \$) of the j -th trade on day i in month t . The daily Amihud measure is the average of the absolute returns

¹³This is different from Dick-Nielsen, Feldhütter, and Lando (2012), who keep the no-coupon bullet bonds. They cover 2,224 bullet bonds and turn to focus on more liquid segment of the market.

¹⁴We also experimented with value-weighted averages with similar results to the ones reported below.

¹⁵Dick-Nielsen, Feldhütter, and Lando (2012) show that higher value of Amihud measure, Roll measure, IRC, Amihud variability, and IRC variability are associated with significantly higher credit spreads of corporate bonds. However, the evidence of turnover and zero-trading days is mixed.

¹⁶Some measures (e.g. Amihud) require a minimum number of trades to compute. We keep all the observations even if some liquidity measures are missing in certain days because we want to have a comprehensive coverage of the entire bond universe. To be sure, measures such as zero-trading days and turnover can be computed for all bonds.

divided by the corresponding trade size within day i :

$$Amihud_{i,t} = \frac{1}{N_{i,t}} \sum_{j=1}^{N_{i,t}} \frac{|r_{j,i,t}|}{Q_{j,i,t}} \quad (1)$$

where $N_{i,t} + 1$ is the number of trades recorded on day i . We exclude retail trades (i.e. trades below \$100,000 in volume), as they are unlikely to have price impact. At least two trades are required on a given day to calculate the measure, and we define a monthly Amihud measure by taking the median of the daily measures within month t .

2. *Imputed round-trip cost (IRC)*. Feldhütter (2012) shows that if a bond that does not trade for days suddenly has two or three trades with the same volume within a short period of time (one day in our definition), then such trades are likely part of a pre-matched arrangement in which a dealer has matched a buyer and a seller. These trades are defined as a set of imputed round-trip trades. The difference between highest and lowest price in a set of imputed round-trip trades is the bid-ask spread collected by the dealer, which is a measure of liquidity of the bond. We follow this approach. Specifically, for a given bond, on each day i we identify sets of imputed round-trip trades indexed by k . A set of imputed round-trip trades involves two or more transactions with the same trading volume. Define $P_{k,i,t}^{\max}$ (resp. $P_{k,i,t}^{\min}$) as the maximum (resp. minimum) price among all the transactions in the k -th set of round-trip trades for that bond on day i in month t . The imputed round-trip cost of k -th set of round-trip trade is defined as:

$$IRC_{k,i,t} = \frac{P_{k,i,t}^{\max} - P_{k,i,t}^{\min}}{P_{k,i,t}^{\min}}. \quad (2)$$

We define a monthly IRC measure by taking the mean of the IRC of each set of imputed round-trip trades within month t , weighted by the number of transactions involved in each set of imputed round-trip trades.

3. *Roll measure*. The intuition of the Roll measure is as follows: the transaction price tends to bounce between the bid and ask price, which causes consecutive trade returns to be negatively correlated. Under certain assumptions as shown in Roll (1984), the Roll measure equals to the bid-ask spread. The Roll measure is defined as two times the square root of the negative covariance between two consecutive daily returns $r_{i,t}, r_{i-1,t}$ in month t . If the covariance is positive, the covariance is replaced with zero.

$$Roll_t = 2\sqrt{-Cov(r_{i,t}, r_{i-1,t})} \quad (3)$$

4. *Non-block trades*. A trade is defined as non-block trade if the trading volume is less than \$5 million for investment-grade bonds, and \$1 million for high-yield bonds. The frequency of non-

block trades is defined as the ratio between the number of non-block trades and the total number of trades in month t .

5. *Size (negative log)*. Lower liquidity is usually associated with smaller size of trade. We first take the negative logarithm of the par value for each trade, then compute the monthly median for each security.

6. *Turnover (negative)*. The annualized turnover for month t is defined as the annualized trading volume divided by the amount outstanding. In what follows we take the negative of turnover as proxy of illiquidity, for consistency with the other measures.

7. *Zero trading days*. We define this measure as the ratio between days with zero trade and the number of trading days in month t .

8. *Variability of Amihud* and 9. *Variability of IRC*. Investors not only care about the current level of liquidity, but also the risk of future liquidity. Therefore, we create the standard deviations of the daily Amihud measure and imputed round-trip costs in a month as measures of liquidity risk.

1.3 U.S. Treasuries Sample Description

We use the CRSP Treasury database to construct our liquidity measures for the U.S. Treasury market. The daily data file is used to construct the Roll measure, and the monthly data file is used to construct the on-the-run premium.

We restrict our analysis to the same period as our corporate bond sample, April 1, 2005 to December 31, 2014. Our sample consists of Treasury bills, notes, and bonds that are noncallable, nonflowering, and with no special tax treatment. We also drop observations with obvious pricing errors such as negative prices. Treasury securities with remaining maturity less than 30 days are also dropped because of potential liquidity problems. After applying the filters, our final sample contains 1,124 bonds. In addition to bond prices, we obtain the total Treasury trading volume from Securities Industry and Financial Markets Association (SIFMA), and the total public debt outstanding from Bloomberg.

The liquidity measures for U.S. Treasuries are the following:

1. *Yield curve fitting noise*. Hu, Pan, and Wang (2013) proposes a market-wide liquidity measure by exploiting the connection between the amount of arbitrage capital in the market and observed “noise” in U.S. Treasury bonds—the shortage of arbitrage capital allows yields to deviate more freely from the curve, resulting in more noise in prices. They construct the noise measure by first fitting Treasury daily prices into a smooth yield curve, and then calculate the mean squared errors¹⁷.

2. *On-the-run premium*. On-the-run Treasury bond (latest issue) usually enjoys a price premium over old bonds with similar maturity. We follow Gurkaynak et al. (2007) to construct the

¹⁷We obtain the measure from the authors’ website at http://www.mit.edu/~junpan/Noise_Measure.xlsx

liquidity premium as the difference between the yield of this synthetic off-the-run bond and the on-the-run bond.

3. *Roll measure* and 4. *Turnover (negative)*. Roll measure and Turnover (negative) measure are constructed similarly as in the case of corporate bonds.

1.4 Summary Statistics and Descriptives

Table 1 reports the summary statistics of the aggregate-level liquidity measures of the U.S. corporate bonds for the period April 2005 to December 2014. For a typical bond, there is no single trade on 74% of business days. The annualized turnover rate is only 29%¹⁸. In comparison, stocks in NYSE have a turnover ratio of 92% in December 2014¹⁹. Among all the trades, only 4% are block trades, and the median trade size is \$35,000.

To get a quantitative assessment of the illiquidity, one can compare various trading cost measures to credit spreads, the compensation for investors to bear the credit and liquidity risk of corporate bonds. The average credit spread of a U.S. corporate bond over a Treasury bond is 2.20% over our sample period. In comparison, the mean Amihud measure, which is based on the impact of \$1 million dollar trade, is 1.29%, as reported in Table 1. This amounts to half of the average credit spread earned in a year. The average IRC, which measures the cost charged by dealers in a round-trip trade, is 0.70%. This equals to a third of the average credit spread. The average Roll measure is 1.59%, which implies a bid-ask spread as large as three-fourth of the average credit spread.

Additionally, investors face high uncertainty in trading cost when executing their trades, as shown by a high time series variability of the Amihud and IRC measure. In synthesis, Table 1 shows that the U.S. corporate bond market is typically not particularly liquid. In this respect, the a priori concerns of public commentators of the effects of regulatory intervention on market liquidity were well placed.

In Table 2 we report the monthly linear correlations for each pair of liquidity proxies, to show consistency across our nine different measures of liquidity. Correlations are typically positive and sizeable, with partial exceptions of the Turnover (negative) measure²⁰.

2 Econometric Model

Our goal is to formally test for structural breaks in the market liquidity of fixed-income assets in the aftermath of the financial crisis. If post-crisis financial regulation indeed generates adverse

¹⁸The average of turnover across bonds is much lower than the aggregate turnover of the market (total trading volume divided by total bond outstanding). This is because most of the total trading volume comes from a small group of large size bonds.

¹⁹See <http://www.nyxdata.com/nysedata/asp/factbook/> for the historical trading volume of NYSE stocks.

²⁰In online appendix Table 1, we provide summary statistics of the 180 disaggregate series. In online appendix Figure 2, we plot time series of nine liquidity measures for each underwriter.

impacts on market liquidity, we should be able to detect structural breaks towards lower liquidity in the period of regulatory intervention (July 2010-December 2014). We present here the econometric setup that we are going to employ.

As anticipated in Section 1 we take both an aggregate-level and a disaggregate-level perspective in our analysis. Let us define the matrix Y of L aggregate liquidity measures observed for T periods. Y is of dimension $(T \times L)$. With the term "aggregate" liquidity measure we mean a measure of liquidity (such as those listed in Subsection (1.2)) that aggregates all securities in a market irrespective of identity of the underwriter, issue size, or credit rating. Although intuitive, this approach may mask heterogeneity in the dynamics of different types of securities. Therefore, to identify specific structural breaks that might arise only within particular classes of securities or only for bonds where markets are made by specific underwriters/banks, we will refer to disaggregate liquidity measures as the matrix X of $N > L$ liquidity measures observed for T periods. X is of dimension $(T \times N)$ where each column measures liquidity grouping bonds at the level of

$$(\textit{identity of the underwriter} \times \textit{issue size} \times \textit{credit rating}) \quad (4)$$

As a matter of accounting, recall that for our case we have $L = 9$ measures. With 4 major underwriters plus 1 for the residual Others, 2 types of issue sizes (small or large), 2 types of credit rating (high yield and investment grade), we have $N = 180$. Our sample covers $T = 117$ months.

2.1 Multiple Breakpoint Tests for Liquidity Levels

Our first methodology studies the question of whether regulatory intervention has produced structural breaks in the *level* of liquidity, in either Y or X . We employ tests for multiple breakpoint estimation (Bai and Perron, 1998, 2003). The underlying assumption of these tests is that the level of liquidity fluctuates around a stable mean in absence of structural changes. If regulation shifts the long-run mean towards a different level, these tests will detect the dates when the changes occur. Although highly stylized, this analysis offers a first-pass examination of the potential dates around which liquidity depletion may have happened. More flexible models allowing for more general types of breaks will be presented below.

2.2 Single Breakpoint Testing for Dynamic Factor Models

Our second and third methodologies employ a more innovative approach based on dynamic factor models (Stock and Watson, 2011; Chen, Dolado and Gonzalo, 2014) to capture breaks in the latent factor structure. This approach allows flexible forms of structural breaks, such as breaks in trends, in serial correlation, or in factor loadings. These methodologies are more recent and deserve a more complete discussion. We now introduce the basic notation, econometric setup,

and follow the exposition in Chen, Dolado and Gonzalo (2014), to which we refer for a detailed discussion of the proofs and the Monte Carlo evidence of power and size of the tests.

Consider a set of N observed liquidity measures constructed as in Section 1 and observed for $t = 1, \dots, T$ periods, say, at monthly frequency. The matrix of observed disaggregate variables²¹ X of dimension $(T \times N)$ is expressed as function of r unobserved factors F of dimension $(T \times r)$, a matrix Λ of factor loadings of dimension $(N \times r)$, and a matrix of idiosyncratic errors ε of dimension $(T \times N)$. As typical in the literature, we have in period t :

$$X_t = \Lambda F_t' + \varepsilon_t. \quad (5)$$

This formulation accommodates flexibly several possible latent structures: r static factors; or \tilde{r} dynamic factors and $p = r/\tilde{r} - 1$ lags; or an arbitrary combination of static and dynamic factors and lags (Stock and Watson, 2011).

Due to their flexibility in accommodating general dynamics across correlated time series, large factor models have enjoyed substantial success in the macroeconomics and finance literature. Stock and Watson (2002) show that the latent factors are consistently estimable by principal component analysis (PCA), an approach we follow here. PCA allows to estimate the r factors of X :

$$\hat{F}_t \equiv \left[\hat{F}_{1t}, \hat{F}_{2t}, \dots, \hat{F}_{rt} \right] \quad (6)$$

by focusing on the first r largest eigenvalues of the matrix XX' in the case $T \leq N$ (or of the matrix $X'X$ in the case $T > N$) and selecting the (appropriately orthogonalized and normalized) corresponding eigenvectors. Following Chen, Dolado and Gonzalo (2014) we also define $\hat{F}_{-1t} \equiv \left[\hat{F}_{2t}, \dots, \hat{F}_{rt} \right]$.

The number of factors r has to be estimated, as the true number of factors is unknown. Let us indicate with \hat{r} such estimated value over the full sample.

To this goal we employ ten different estimators, some with better finite sample properties than others, with the aim of providing an exhaustive range of \hat{r} 's. Eight of the estimators we employ follow the popular information criteria (IC) proposed by Bai and Ng (2002), including their preferred IC_{p1} , IC_{p2} , PC_{p1} , and PC_{p2} . IC estimators, however, can occasionally display in finite samples a somewhat undesirable dependency on a specific parameter necessary to the estimation: the maximum number of admissible factors in the model (typically indicated as $kmax$). This may lead to overestimation of the true number of factors (Ahn and Horenstein, 2014). It is also the reason we additionally employ the recent ER (eigenvalue ratio) and GR (growth ratio) estimators of Ahn and Horenstein (2014), which do not share this drawback and, by focusing

²¹For the dynamic factor model analysis let us indicate with an abuse of notation X as the matrix of first differenced and normalized liquidity measures, as indicated by Stock and Watson (2011).

on the ratio of subsequent eigenvalues (or the ratio of their logs), also hinge on the straightforward intuition of principal component analysis screeplots (i.e. a popular graphical representation of the progressive explanatory power of each principal component ranked by size of its eigenvalue). We consider all number of factors between the minimum and the maximum of the estimated $\{IC_{p1}, IC_{p2}, IC_{p3}, PC_{p1}, PC_{p2}, PC_{p3}, AIC_3, BIC_3, ER, GR\}$, allowing for at least $\hat{r} = 2$ unobserved factors (a necessary condition for the statistical tests below).

We now proceed in introducing structural breaks in (5) and focus initially on the methodology for testing a single breakpoint, leaving multiple breakpoints to Section 2.3. It is relevant first to specify whether one is interested in breaks in the factor loadings Λ or in the factors F . Let us begin by representing a single structural break in all factor loadings at date τ :

$$X_t = \Lambda F_t' + \varepsilon_t \quad t = 1, \dots, \tau \quad (7)$$

$$X_t = \Gamma F_t' + \varepsilon_t \quad t = \tau + 1, \dots, T \quad (8)$$

where Γ is the post-break matrix of factor loadings of dimension $(N \times r)$. An important insight of Chen, Dolado and Gonzalo (2014) is that (7)-(8) can be represented as

$$X_t = \Lambda F_t' + \Delta G_t' + \varepsilon_t \quad (9)$$

where $\Delta = \Gamma - \Lambda$ measures the change in the loadings and

$$G_t = 0 \quad t = 1, \dots, \tau \quad (10)$$

$$G_t = F_t \quad t = \tau + 1, \dots, T.$$

The notation so far has focused on a single structural breakpoint for all r factors. At a given breakpoint, Chen, Dolado and Gonzalo (2014) distinguish between two types of breaks: small and large. Consider k_2 small breaks, of the type discussed by Stock and Watson (2002, 2009). These are defined as local-to-zero instabilities in the factor loadings that asymptotically average out without affecting estimation and inference under PCA. These are not the type of breaks we are interested in. In the context of large policy shifts, one is most likely interested in big structural breaks, indicated as $k_1 = r - k_2$. The formal definition is given in Chen, Dolado and Gonzalo (2014), but more importantly it is proven that under k_1 big breaks in (9), \hat{F}_t estimated by PCA delivers inconsistent estimates of the space of the original factors F_t . Instead, defining G_t^1 the partition of G_t corresponding to the large breaks only, the full sample PCA delivers consistent estimates of the space of the new factors $[F_t \ G_t^1]$. Specifically, over the full sample the number of factors tends to be overestimated by k_1 . Chen, Dolado and Gonzalo (2014) prove that a factor model with r unobserved factors and with $0 < k_1 \leq r$ big structural breaks in the factor loadings

at time τ admits a representation with (asymptotically) $r + k_1$ factors. Particularly, given an IC estimator in Bai and Ng (2002) \hat{r} and under general assumptions, it is shown (Proposition 2, p.34):

$$\lim_{N, T \rightarrow \infty} \mathbb{P} [\hat{r} = r + k_1] = 1. \quad (11)$$

An important remark at this point is to notice that if the break date τ were known, one could recover a consistent estimate of r by simply splitting the sample in a “before-breakpoint” and “after-breakpoint” subsamples and performing PCA and Bai and Ng (2002) or Ahn and Horenstein (2014) in either subsample. In either case,

$$\begin{aligned} \lim_{N, T \rightarrow \infty} \mathbb{P} [\hat{r}_{before} = r] &= 1 \\ \lim_{N, T \rightarrow \infty} \mathbb{P} [\hat{r}_{after} = r] &= 1. \end{aligned} \quad (12)$$

both \hat{r}_{before} and \hat{r}_{after} typically lower than the full sample estimate \hat{r} .

For the sake of generality, we take the exact breakpoint date τ as unknown. Although we explicitly consider the exact date of the finalization of the Volcker Rule in the difference-in differences matching below, the possibility of anticipatory behavior or of delayed response for a policy intervention so sizeable and publicly debated would caution against a ‘known breakpoint’ approach. Hence, we do not impose such restriction here.

Chen, Dolado and Gonzalo (2014) present a test for the null $H_0 : k_1 = 0$ versus the alternative of at least one big break $H_1 : k_1 > 0$ based on detecting breaks in \hat{F}_t estimated over the full sample by PCA. The implementation is straightforward. Define $\hat{\beta}$ the estimated $(\hat{r} - 1) \times 1$ coefficient vector obtained by regressing \hat{F}_{1t} on \hat{F}_{-1t} and \hat{S} its corresponding Newey-West HAC covariance matrix²². One can test for structural breaks in β by focusing for the case of unknown breakpoint $\tau = T\pi$ with $\pi \in \Pi \equiv (\pi_0, 1 - \pi_0)$ and $0 < \pi_0 < 1$ based on Andrews (1993) Sup-Wald statistic or Sup-LM statistic. Specifically, for given τ , and hence $\pi = \tau/T$, define $\hat{\beta}_1(\pi)$ the estimated $(\hat{r} - 1) \times 1$ coefficient vector obtained by regressing \hat{F}_{1t} on \hat{F}_{-1t} for $t = 1, \dots, \tau$ and $\hat{\beta}_2(\pi)$ the estimated $(\hat{r} - 1) \times 1$ coefficient vector obtained by regressing \hat{F}_{1t} on \hat{F}_{-1t} for $t = \tau + 1, \dots, T$ the Sup-Wald statistic is:

$$\mathcal{L}^*(\Pi) = \sup_{\pi \in \Pi} T\pi(1 - \pi) \left(\hat{\beta}_1(\pi) - \hat{\beta}_2(\pi) \right)' \hat{S}^{-1} \left(\hat{\beta}_1(\pi) - \hat{\beta}_2(\pi) \right) \quad (13)$$

²²Newey and West (1987). \hat{S} is estimated over the full sample.

and the Sup-LM statistic is:

$$\mathcal{L}(\Pi) = \sup_{\pi \in \Pi} \frac{1}{\pi(1-\pi)} \left(\frac{1}{\sqrt{T}} \sum_{t=1}^{T\pi} \hat{F}_{-1t} \hat{F}_{1t} \right)' \hat{S}^{-1} \left(\frac{1}{\sqrt{T}} \sum_{t=1}^{T\pi} \hat{F}_{-1t} \hat{F}_{1t} \right) \quad (14)$$

In the analysis we will maintain a conservative $\pi_0 = 0.3$ which in our case is not overly restrictive as it allows a search for structural breaks between January 2008 and January 2012 covering the full financial crisis, the full legislative debate on Dodd-Frank and large part of the regulatory rule-making period for the Volcker Rule. We employ the critical values for the (13) and (14) statistics reported in Andrews (1993).

To conclude this subsection, let us consider the matter of detecting a structural break in the factors themselves as opposed to a break in the factor loadings at τ . There are at least two different formulations for a break in the factors one should consider. First, the formulation discussed in Chen, Dolado and Gonzalo (2014) considers maintaining unvaried the loadings Λ , but changing the variance-covariance matrix of the r original factors:

$$\mathbb{E}[F_t F_t'] = \Sigma \quad t = 1, \dots, \tau \quad (15)$$

$$\mathbb{E}[F_t F_t'] = \Xi \quad t = \tau + 1, \dots, T \quad (16)$$

where Σ is the factor covariance before the break and Ξ after the break and both are $(r \times r)$. Given that the approach above focused on testing breaks in the \hat{F}_t PCA factors estimated over the full sample, it may not appear surprising that the Sup tests above (based on regressing \hat{F}_{1t} on \hat{F}_{-1t}) will be naturally able to pick up breaks of the type (15)-(16). In fact, the same regression approach described above will reject the null of big breaks in presence of changes in factors.

It is possible however to discriminate between breaks in loadings and breaks in factors by noticing that in the case of breaks in factors:

$$\lim_{N, T \rightarrow \infty} \mathbb{P}[\hat{r} = r] = \lim_{N, T \rightarrow \infty} \mathbb{P}[\hat{r}_{before} = r] = \lim_{N, T \rightarrow \infty} \mathbb{P}[\hat{r}_{after} = r] = 1. \quad (17)$$

This implies that in the case of breaks in the factors typically \hat{r} estimated over the whole sample will be identical as when estimated on subsamples either before or after the breakpoint. In the case of breaks in the loadings, instead, \hat{r} estimated over the full sample will be higher than when estimated on subsamples either before or after the breakpoint, as evident from the result in (11).

A second formulation for a break is more drastic and entails a break in the number of factors r in (5), that is the addition or subtraction of specific factors in the model at date τ . Section 2.4 offers an application of this methodology to this formulation and shows how it can be incorporated in this setting.

2.3 Multiple Breakpoint Testing for Dynamic Factor Models

Let us now focus on multiple structural breaks M in factor loadings at unknown dates $\tau_1, \tau_2, \dots, \tau_M$. This structure partitions the sample period of length T in $M + 1$ intervals:

$$\begin{aligned} X_t &= \Lambda F'_t + \varepsilon_t \quad t = 1, \dots, \tau_1 \\ X_t &= \Gamma^1 F'_t + \varepsilon_t \quad t = \tau_1 + 1, \dots, \tau_2 \\ &\dots \\ X_t &= \Gamma^M F'_t + \varepsilon_t \quad t = \tau_M + 1, \dots, T \end{aligned} \tag{18}$$

where Γ^m with $m = 1, \dots, M$ are the post first break matrices of factor loadings of dimension $(N \times r)$. In the context of multiple breakpoints, standard estimators in the literature include the ones proposed by Bai and Perron (1998, 2003), which we employ in combination to the regression approach delineated in Section 2.2. Considering the regression of \hat{F}_{1t} on \hat{F}_{-1t} with the goal of detecting not one, but multiple breakpoints, we implement the recommended approach of Bai and Perron (1998, 2003).

Consider for the interval $t = \tau_m + 1, \dots, \tau_{m+1}$ the regression of \hat{F}_{1t} on \hat{F}_{-1t} in this subsample and call the estimated coefficient $\hat{\beta}_m$. Notice that, like $\hat{\beta}_1(\pi)$ and $\hat{\beta}_2(\pi)$ in Section 2.2, $\hat{\beta}_m$ depends on the breakpoint parameters, $\pi_m = \tau_m/T$ and $\pi_{m+1} = \tau_{m+1}/T$. Given M , let us also define $\hat{\beta} = (\hat{\beta}'_1, \hat{\beta}'_2, \dots, \hat{\beta}'_{M+1})'$. Bai and Perron (1998) first consider the Sup-F type test of the null hypothesis of no structural break ($M = 0$) against the alternative hypothesis that there is a known number of breaks $M = k$:

$$\begin{aligned} &\sup_{(\pi_1, \dots, \pi_k)} F_T(\pi_1, \dots, \pi_k; r-1) \\ &= \frac{1}{T} \left(\frac{T - (k+1)(r-1)}{k(r-1)} \right) \hat{\beta}' R' (R \hat{S} R')^{-1} R \hat{\beta} \end{aligned} \tag{19}$$

where R is the matrix such that $(R \hat{\beta})' = (\hat{\beta}'_1 - \hat{\beta}'_2, \dots, \hat{\beta}'_k - \hat{\beta}'_{k+1})$ and \hat{S} is now an estimated HAC variance covariance matrix of $\hat{\beta}$ ²³.

As the number of breaks is unknown, a second type of test is more useful: Bai and Perron (1998) consider a test of the null hypothesis of no structural break ($M = 0$) against the alternative hypothesis that there is an unknown number of breaks $M = m$ with m ranging between 1 and \bar{m} , which is given²⁴. The test is referred to as the double maximum test and two different statistics are

²³In the tests we perform we apply a short trimming of 10%. The Bai and Perron requires a minimal admissible distance expressed as fraction of T among any pair of breakpoints τ_m and τ_{m+1} and we set it to 10% of the sample length, in order to allow for relatively close multiple breaks. In all the test we also allow the distribution of ε_t to vary across different intervals.

²⁴In the tests we perform we allow for a maximum of $\bar{m} = 5$ total breakpoints (which, as shown below, will prove

employed:

$$UD \max F_T(\bar{m}; r - 1) = \max_{1 \leq m \leq \bar{m}} \sup_{(\pi_1, \dots, \pi_k)} F_T(\pi_1, \dots, \pi_k; r - 1) \quad (20)$$

which is unweighted with respect of each break number, and

$$WD \max F_T(\bar{m}; r - 1, a_1, \dots, a_{\bar{m}}) = \max_{1 \leq m \leq \bar{m}} a_m \sup_{(\pi_1, \dots, \pi_k)} F_T(\pi_1, \dots, \pi_k; r - 1) \quad (21)$$

which is a weighted version, where weights are defined such that the marginal p-values are equal across values of m ²⁵.

The final test proposed by Bai and Perron is a sequential test. One proceeds by testing ℓ breaks against $\ell + 1$ breaks. The test is commonly labelled $\sup F_T(\ell + 1|\ell)$ and intuitively is built as follows. Consider the $\ell + 1$ intervals generated by the ℓ break points under the null hypothesis. Within each interval a separate test of the type $\sup_{(\pi_1)} F_T(\pi_1; r - 1)$ is run, i.e. a test of the null hypothesis of no break versus the alternative hypothesis of 1 break. The test rejects the null hypothesis in favor of $\ell + 1$ breaks if, relatively to the sum of squared residuals obtained under the ℓ breaks model obtained by regressing \hat{F}_{1t} on \hat{F}_{-1t} and aggregated across all intervals, there is one additional break that produces a sum of squared residuals sufficiently smaller under the $\ell + 1$ breaks model.

Bai and Perron (2003) recommend to first obtain both the UD max and WD max to test whether at least one break is detected in the entire sample, as these tests are more prompt in rejecting the null hypothesis in presence of multiple but contiguous breaks (e.g. which would be the case for instance if there were a break at the beginning of the crisis and one at its end). If at least one break is detected, then the sequential approach should be employed. Specifically one should select $M = m$ such that $\sup F_T(\ell + 1|\ell)$ are insignificant for $\ell \geq m$. We follow this approach here.

2.4 Breaks in Trends and a Simulation Example

We first provide a simple example to illustrate the flexibility of the dynamic factor model to capture breaks in trends, which are a realistic type of structural break in our setting. Suppose the illiquidity measure, l_t , is jointly driven by supply of liquidity, s_t , and demand for liquidity, d_t . Suppose that post-crisis regulations lead to an upward trend with a constant drift γ in illiquidity

to be sufficiently high and is also the value suggested in Bai and Perron, 2003).

²⁵Specifically $a_1 = 1$ and $a_m = c(r - 1, \alpha, 1)/c(r - 1, \alpha, m)$, for $m > 1$, where α is the significance level of the test and $c(r - 1, \alpha, m)$ is the asymptotic critical value of the corresponding Sup-F test for m breaks, which is reported by Bai and Perron (1998, 2003).

from $\tau + 1$:

$$l_t = -\alpha s_t + \beta d_t + e_t \quad t = 1, \dots, \tau \quad (22)$$

$$l_t = -\alpha s_t + \beta d_t + \gamma (t - \tau) + e_t \quad t = \tau + 1, \dots, T \quad (23)$$

Taking the first difference of the above equation system gives:

$$x_t = -\alpha f_{1t} + \beta f_{2t} + 0 f_{3t} + \varepsilon_t \quad t = 1, \dots, \tau \quad (24)$$

$$x_t = -\alpha f_{1t} + \beta f_{2t} + \gamma f_{3t} + \varepsilon_t \quad t = \tau + 1, \dots, T \quad (25)$$

Where $x_t = l_t - l_{t-1}$ is the innovation in illiquidity, $f_{1t} = s_t - s_{t-1}$ is the supply factor, $f_{2t} = d_t - d_{t-1}$ is the demand factor, $f_{3t} = 1$ is the regulation factor, and $\varepsilon_t = e_t - e_{t-1}$ is the differenced measurement errors. It is immediately obvious that the break in trend can be reformulated as a break in the loading on the regulation factor, which can be consistently estimated by our methodology, as shown in Section (2.2).

We simulate a panel of 180 liquidity measures to illustrate the power of our tests²⁶. A detailed discussion of simulation and the Monte Carlo evidence of power and size of the tests can be found in Chen, Dolado and Gonzalo (2014). Figure 1 plots the simulated liquidity index, defined as the average of 180 standardized simulated liquidity measures. The blue solid line is the path with the structural break, and the green dotted line plots the counterfactual scenario where regulation has no effects by design. The star sign indicates the date when the structural break happens. The difference between the two paths is the regulation-induced liquidity gap. We can see that the magnitude of liquidity deterioration is very small at the beginning compared with the normal fluctuations of liquidity, and builds up very slowly. We conduct our structural break tests described in Section (2.2) and (2.3). The estimated break date is marked by the vertical dashed line. Despite of the small magnitude, both tests successful identify the date of the structural break.

We also use the dynamic factor model to estimate the counterfactual path of liquidity assuming there is no structural break. We first use the observed data before the break to estimate the loadings. Specifically, we regress each of the 180 liquidity measures on the estimated factors. Then we predict the counterfactual path of liquidity assuming the factor loadings in the post-break period are the same as the pre-break period. The red dash line shows the estimated path. Our estimation accurately traces out the true counterfactual path. Such accuracy is obtained because the large

²⁶To mimic our empirical application, we simulate 180 liquidity measures driven by two latent factors: a supply factor and a demand factor. The two factors follow AR(1) process with autocorrelation of 0.5, and cross-correlation of 0.5. The loading parameters on the two latent factors are drawn from N(0,1). A structural break occurs in July 2010 where 180 liquidity measures start to load on a new regulation factor, which follows AR(1) process with autocorrelation of 0.5 and an upward drift of 0.1. The loading parameters on the regulation factor follows N(0,0.2). The cross-correlation between regulation and supply and demand factor is also 0.5.

cross-section dimension ($N = 180$) of our liquidity measures compensate the relatively short time span for loading estimation (62 months).

3 Results for Market Liquidity of U.S. Corporate Bonds

For U.S. corporate bonds we present four different estimation strategies. We will begin by applying multiple breakpoint tests in levels to measures of market liquidity provisions. Subsequently, we will focus on a dynamic factor model and presents results of both single and multiple breakpoints in factor loadings, with the understanding that also further testing for factor breaks is available. Finally, we will focus on difference-in-differences matching results.

3.1 Multiple Breakpoint Tests for Liquidity Levels

We begin by studying break in levels of our main nine liquidity measures (or properly seven measures of liquidity levels and two measures of liquidity risk) employing the Bai and Perron (1998, 2003) estimation approach for multiple unknown breakpoints in the undifferenced and unstandardized time series. This simple test serves as a visual examination on the potential dates around which liquidity depletion may have occurred.

At the onset we do not separate bonds by underwriter, issue size, and credit rating. Rather we aggregate all bonds and plot their time series in Figure 2²⁷. The estimated means for each sub-period (red dashed line) are also reported, where the break dates (a shift in the red dashed line) are estimated by the Bai and Perron (1998-2003) approach and are breaks significant at the 5% confidence level²⁸.

Concerning the dating of the structural breaks, the estimators should pick up at least the drastic reduction in liquidity produced by the near collapse of the U.S. financial system in September 2008 and the subsequent break towards more normal market liquidity levels at the end of 2009. Any detection of subsequent structural breaks towards lower levels of liquidity over the periods 2010-2014 needs instead to be carefully examined, as potential telltale indication of liquidity depletion concurrent with (and possibly caused by) regulatory intervention. The 2010-14 period covers important regulatory events such as the approval of Dodd-Frank, shutdowns of proprietary trading desks by major banks, Basel III, and the approval of the interim and the finalized Volcker Rule.

The double maximum tests indicate the presence of at least one structural break at 5% confidence level in all nine proxies²⁹. The sequential sup $F_T(\ell + 1|\ell)$ indicates three breakpoints for

²⁷In the online appendix Figure 3, we create an aggregate liquidity index using the average z-score of 9 liquidity measures. This approach helps to average out some noises from a particular liquidity measure but may lose some detailed information. We apply the same analysis on this liquidity index, and reach similar conclusion if we examine each measure separately.

²⁸The estimated break dates are reported in online appendix Table 2.

²⁹In online appendix Table 3 and 4 we report the relevant statistics for the double maximum tests and the sup $F_T(\ell + 1|\ell)$ tests.

the IRC, IRC (standard deviation), Roll measure, and Non-block trades; one for the Amihud, Amihud (standard deviation), and Turnover (negative), and four for Size (negative) and Zero trading. As clarified by Figure 2, the Bai-Perron approach indicates clearly breaks in liquidity around the financial crisis. None of the structural breaks towards lower liquidity happen during the period of regulatory intervention. Instead, breaks towards higher liquidity are detected for seven out of nine liquidity measures.

We further compare the estimated mean liquidity in the subperiods before and after the crisis. With the possible exceptions of turnover and non-block trades, most of the liquidity measures indicate higher liquidity levels at the end of the sample period comparing to the pre-crisis level: the price impact of large transactions goes down (Amihud), bid-asked spreads tighten (IRC), the price reversal goes down (Roll), the median trade size stays stable, and trading becomes more frequent (Zero trading). Turnover and block trade are somewhat lower than the pre-crisis level, but the breaks occurred before or during the crisis, well before regulations came into place. In fact, using the aggregate bond turnover statistics from SIFMA we find that corporate bond turnover has been on a downward trend for more than ten years, and actually flattens out during the post-crisis period³⁰, suggesting factors other than post-crisis regulation may be the driving force. For example, an increasing share of corporate bond trading may have moved to bond ETFs which is not captured by TRACE data³¹. The reduction in the share of block trades may be driven by market structure transition from over-the-counter market to electronic trading platforms where transactions are conducted predominantly as non-block trades (Hendershott and Madhavan, 2015). Even the share of block trade seems to be lower, the median trade size is similar to the pre-crisis level.

While this is *prima facie* evidence against drastic reductions in liquidity following regulatory intervention, it is still possible that at the level of specific types of corporate bonds structural breaks may arise. In Figure 3 we present a graph tracing for each month the fraction of the 180 disaggregated market liquidity variables that are described to have a statistically significant (at 5% confidence level) break in that month and in what direction (i.e. towards lower liquidity -in blue- or higher liquidity -in red). The bulk of the structural breaks toward lower liquidity happens in July and August 2008, right before Lehman Brothers' failure. As it appears clear in Figure 3, if anything, around subsequent periods of regulatory intervention the disaggregate liquidity measures pointed systematically toward higher liquidity, not lower.

To understand the source of the disaggregate-level structural breaks, Figure 4 shows the decomposition of break dates by underwriting bank³². We can see that the bankruptcy of Lehman

³⁰The result is reported in online appendix Figure 4.

³¹In online appendix Figure 4, we adjust the turnover by adding the trading volume of corporate bond ETFs. The trading volume from ETFs accounts a non-trivial share of decline in turnover, especially for high yield bonds.

³²In online appendix Figure 5 and 6, we show the decomposition by types of bonds and measures of liquidity. The results are consistent.

Brothers in September 2008 caused liquidity reductions for all underwriters. In comparison, the later recoveries are more heterogenous: bonds underwritten by JP Morgan and Goldman Sachs experienced earlier recovery in liquidity than bonds of other underwriters. This is consistent with anecdotal evidence that these two banks had relatively stronger balance sheets throughout the crisis.

The most important observation from this graph, however, is from the later period when banks start to shutdown their proprietary trading desks after the passage of the Dodd-Frank Act. Were proprietary trading indispensable for market making, one would expect to see bank-specific liquidity reductions line up with an announced trading desk shutdown by the same bank. This is hardly the case: no large bank specific liquidity reduction is observed after 2010 (all the bank-specific frequencies of liquidity reduction are below 5% after 2010)³³. On the contrary, many banks experienced liquidity increases around July 2012, in the midst of regulatory interventions. There appears to be no clear evidence that the shutting down of proprietary trading desks was associated with an adverse impact on market liquidity.

3.2 Single Breakpoint Tests for the Dynamic Factor Model

This subsection shifts the attention to a dynamic factor model with the goal of assessing whether the underlying structure of correlation and of latent dynamics of liquidity across different bond types displays salient breaks during the period of crisis and post-crisis regulatory intervention. Comparing to the breakpoint tests for liquidity levels in the previous subsection, this approach allows more realistic modeling of the liquidity processes and captures more flexible forms of breaks, including breaks in trends, in serial correlation, and in factor loadings.

We discuss here the application of Chen, Dolado and Gonzalo (2014) using the 2005-14 monthly sample and our full matrix X of $N = 180$ differenced and standardized time series. A first preliminary step requires to estimate the number of factors over the full sample $T = 117$. According to our discussion in Section 2.2 this approach will not deliver a consistent estimate of the number of true factors in (5), but rather the sum of the true factors r and the number of big breaks in these factor loadings k_1 . In online appendix Table 5 we report the full set of estimates based on Bai and Ng (2002) and Ahn and Horenstein (2014). Here we impose a $kmax = 10$ and notice that the estimates for $\{IC_{p1}, IC_{p2}, IC_{p3}, PC_{p1}, PC_{p2}, PC_{p3}, AIC_3, BIC_3, ER, GR\}$ range from 3 to 10. Although this range is not particularly tight, this is of little effect for the interpretation of our main findings in Figure 5.

Figure 5 reports the Sup-Wald and the Sup-LM test statistics of the full interval over which the unknown breakpoint is allowed to belong given a conservative $\pi_0 = 0.3$. Such sample restric-

³³A gradual shutdown of the trading desk would not be a problem for our test, since the estimated break points will show up sometime after the announcement date. However, we see none of this lagged liquidity reduction.

tion is due to power loss concerns for the Sup tests (Andrews, 1993). Our interval of search of breakpoints covers the period between January 2008 and January 2012. Figure 5 also reports the Andrews (1993) critical values above which the structural break is significant at the 10% and 5% confidence. We perform the analysis for any possible number of factors in the range estimated in online appendix Table 5.

As evident from Figure 5, the Sup tests systematically pick breaks in factor loadings (at 5% confidence) when we allow a number of estimated factors above 4. Typically the Sup statistic indicates the breakpoint as occurring during the 2008-2009 recession or shortly after. This is informative because again such dating does not correspond to regulatory events of prominence, but rather corresponds to the financial crisis itself. In essence what the Chen, Dolado and Gonzalo (2014) methodology allows us to exclude is that a structural break in the underlying factor structure of the disaggregate liquidity occurred around dates of post-crisis regulatory activity³⁴.

So far the methodology in this subsection has focused on a single breakpoint, a restriction that, given the multitude of potential shocks affecting the U.S. financial system during our period of analysis, one should find unwarranted. We relax this restriction in the following subsection.

3.3 Multiple Breakpoint Tests for the Dynamic Factor Model

This subsection employs the Bai and Perron (1998, 2003) approach within the dynamic factor model, transposing the logic of Chen, Dolado and Gonzalo (2014) to the multiple breakpoint setting.

Figure 6 reports the results. Each panel represents a different factor models ranging from $\hat{\ell} = 2, \dots, 10$ estimated factors, employing the Bai and Perron (1998, 2003) preferred approach to the first $\hat{\ell}$ PCA estimated factors of the matrix X of differenced and standardize disaggregate liquidity measures. The blue solid line represents the liquidity index, defined as the average of 180 standardized liquidity measures. The dashed vertical lines indicate estimated dates of breaks in dynamic factor model³⁵. The double maximum tests indicates the presence of at least a structural break at the 5% confidence level in all nine dynamic factor models. The sequential sup $F_T(\ell + 1|\ell)$ indicates at most two breakpoints for the models with $\hat{\ell} = 2, 3, 4, 5$, all essentially coincident with the start and end of the recession and the financial crisis. As in the previous section, such dating occurs well before regulatory events of prominence (the passage of the Dodd-Frank Act in July 2010, the announcement of the final rules of Basel III in July 2013, or the announcement of the finalized Volcker Rule in January 2014), but rather appears to correspond to dynamics within the confines of the financial crisis itself.

³⁴In online appendix Table 6, we report the number of factors before and after the break.

³⁵In online appendix Table 7, 8, 9, and 10 we report the estimated break dates, double maximum test statistics, the sup $F_T(\ell + 1|\ell)$ test statistics, and the number of factors in each subperiod.

With $\hat{r} = 6, 7, 8, 9, 10$, more breakpoints in the factor loadings appear. Notably, there are breaks in late 2010 and 2011, which fall into the regulatory intervention period. To examine whether these breaks indicate deterioration or improvement in liquidity, we estimate the counterfactual path of liquidity assuming no structural breaks occur from the last estimated breakpoint onwards. Specifically, we first estimate the factor loadings using the data in the interval immediately before the structural break. Then we predict the counterfactual path of the average liquidity after the break assuming the factor loadings take the same value as before. For the models which do not detect breaks during the regulatory intervention period ($\hat{r} = 2, 3, 4, 5$), we use the break closest to the regulatory intervention to conduct the counterfactual analysis. We conduct this exercise for each of the 180 liquidity measures, and take the average to create a liquidity index.

The red dashed line in Figure 6 is the estimated counterfactual path of liquidity in absence of the last structural break. Comparing the observed and counterfactual path, we can tell that whether liquidity would be lower or higher in absence of the breaks. Consistently across all the nine specifications, these structural breaks during or right before the regulatory intervention period lead to slightly higher liquidity (lower illiquidity as the figure shows) comparing the counterfactual path. This is consistent with Figure 3 which shows the level of liquidity breaks towards higher liquidity, not lower around this time period. One likely explanation could be the ability of our model to pick up an increasing role for electronic trading and for open-end mutual funds³⁶.

3.4 Difference-in-Differences Matching for Liquidity Levels

We now present a more standard estimation strategy based on a difference-in-differences exercise augmented by matching of corporate bonds based on pre-treatment covariates (Heckman, Ichimura, Smith, and Todd, 1998; Smith and Todd, 2005). Here, for reason that will become clear in the construction of the test, we will focus only on the finalization of the Volcker Rule in January 2014 as our treatment date. Given the limitation in our “post” sample of just 12 months available, we will take a symmetric 12-month window around January 2014³⁷.

We proceed as follows. First, we manually classify the top 40 underwriters into two groups –one covered by Volcker Rule and the other not covered based on the revised finalized version of Volcker Rule³⁸. Then we identify the set of bonds which have at least one underwriter not covered by the Volcker Rule, that is a non-banking entity for which proprietary trading is not restricted.

³⁶See also Dudley (2015). In online appendix Table 11, we show that some of the latent factors are indeed significantly correlated with innovations in bond mutual fund flows.

³⁷Relative to the analysis above, the approach of this subsection is more restrictive, as it focuses on a single regulatory dimension and relies on a difference-in-differences type of identification, but it is also an approach much more familiar to applied econometricians. In addition, by relying on different identifying assumptions, complements nicely the macroeconomic estimation strategy above.

³⁸See the the following document from Federal Register for details of the final rule: <http://www.gpo.gov/fdsys/pkg/FR-2014-01-31/pdf/2013-31476.pdf>

This set of bonds is a useful benchmark as at least one of the underwriters who typically make market on that bond is virtually unconstrained by the main regulatory restriction in the rule, and hence virtually free to provide liquidity services in case banking entities were so impaired. For each of these 3, 106 non-Volcker Rule bonds that are outstanding between January 2013 and December 2014, we find a match among all the Volcker Rule bonds issues in the same month, matures in the same month, has the same credit rating (investment grade/high yield), and has a relative size difference less than 50% of the average size of the pair³⁹.

Table 3 reports the results for a difference-in-differences model for each of our nine liquidity proxies where the treatment is administered to the Volcker Rule bonds after January 2014 and each regression controls for the reciprocal of issue age, the reciprocal of issue age squared, bond fixed effects, and month fixed effects. Standard errors are two-way clustered at the bond and month level. In seven out of nine measures the treatment does not predict reductions in liquidity with a confidence level of 5%. Only for IRC and IRC (standard deviation) we find a statistically significant effect. This is not particularly worrying since more than 80% observations have missing IRC and IRC (standard deviation) measure in the matched sample⁴⁰. Overall, there seems to be no robust evidence of liquidity depletion as consequence of the Volcker Rule.

The regression evidence is also supported by the graphical representation. In Figure 7 we show the time series of the Volcker Rule bonds and non-Volcker Rule bonds around the time when the revised finalized version of the Rule was approved (the vertical line, January 2014). Both time series are normalized to take value of 0 at December 2013. Were evidence of liquidity depletion present in the data, one would expect to see systematically higher levels of the blue line after the treatment, a sign of reduced liquidity or heightened liquidity risk. This is hardly the case both in reporting unconditional time series as in Figure 7 or time series where bond and month fixed effects are conditioned out (not reported to save space).

3.5 Comments on the Change of Market Structure

With systematic evidence supporting the absence of structural deterioration in corporate bond liquidity, we will now conclude this section by going back to two of the most often cited evidences for liquidity depletion: the decline in dealer corporate bond inventories and the increase in agency trading.

Figure 8 shows the amount of corporate bonds held by dealer banks as the percentage of total corporate bond outstanding. We apply the Bai and Perron (1998, 2003) approach to estimate break

³⁹There are fewer non-Volcker Rule bonds so we start our matching with them. If more than one bond satisfies the above criteria, we keep the one with smallest relative size difference. Since the Volcker Rule bonds are significantly larger than non-Volcker Rule bonds, many observations are dropped due to the last criterion on relative size. We ended up with a matched sample of 350 pairs of bonds.

⁴⁰This is because the non-Volcker Rule bonds are usually very small and thinly traded. Therefore, liquidity measures which require a certain number of transactions of specific types are very noisy.

points to this series, and three lessons can be learned from this test.

First, the estimation shows, as is obvious in observing the time series of the raw data, that the major reductions in dealer inventories occurred at the onset of the financial crisis (September 2008), far ahead of the post-crisis financial regulation. Therefore, at a minimum, there are other important factors driving the reductions of the inventories unrelated to the post-crisis financial regulation. One potential factor is the deleveraging of broker-dealers forced by rehypothecation lenders (Mitchell and Pulvino, 2012).

Second, the abnormally high level of bond holdings in 2007 seems the result of a pre-crisis run-up of risk-taking, as shown by a series of breaks towards greater holding amounts between 2002 and 2007. In this light, the dramatic reduction during the crisis appears actually more a “getting back to normal”. In this sense, using the pre-crisis level as a baseline to calculate the change of inventory is somewhat misleading.

Third, there are two minor breaks, one in August 2011 and the other in March 2013, that fall into the period of regulatory intervention. However, as our tests on market liquidity have systematically shown, no structural reductions in market liquidity occurred during this period. This seems to suggest that some of the holdings may be held by the proprietary trading desks for risk-taking purposes, exactly the kind of activities that the Volcker Rule restrains. This possibility has also been raised by informal discussions (Brainard, 2015)⁴¹. The fact that dealer banks rapidly reduced their bond holding during 2008-09 crisis suggests that they demanded rather than supplied liquidity at the time when liquidity was most needed⁴². Another possibility is that this data series from the Federal Reserve overstates pre-crisis inventories because it improperly includes non-agency MBS. Indeed, one of the post-crisis breaks corresponds to the date of survey revision⁴³.

Another commonly cited evidence of liquidity depletion is the shift from principal-based transactions to agency-based transactions. Principal and agency transactions are two main types of trade that dealers may conduct. Principal trading occurs when a dealer uses its own inventory to fill the order for the client. The purpose behind principal trading is for the dealer to create extra profits (over and above the commission charged) for its own portfolios through price appreciations and bid-ask spreads. Traditionally, large banks have mainly focused on principal trading. Agency trading instead involves a dealer searching for the security demand by a client from other clients or dealers. It is an empirical question whether regulation has caused the shift to agency trading, and it is also unclear that such shift of business model would lead to liquidity deterioration.

⁴¹In a speech by Federal Reserve Governor Lael Brainard at Salzburg Global Forum on July 1, 2015, he also mentioned that "since not all broker-dealer inventories are used for market-making activities, the extent to which lower inventories are affecting liquidity is unclear."

⁴²We thank Albert Kyle for suggesting this point.

⁴³See "Revised survey of primary dealers sheds new light on inventories," *The Credit Line*, April 18, 2013.

Figure 9 plots the fraction of agency transactions over time⁴⁴. We apply the Bai and Perron (1998, 2003) approach to estimate break points in the level of this series. Coincidentally with the decline in bond inventory, we find that there is a secular increasing trend of agency-based transactions, and the bulk of the increase occurred before regulatory interventions. The timing casts doubts on the claim that the post-crisis regulation causes this change. Moreover, comparing to the time series of our liquidity measures in Figure 2, the increase in agency-based transactions does not line up with periods of liquidity reductions, suggesting the two are not necessarily equivalent⁴⁵.

A more interesting question is what explains the structural breaks towards higher liquidity levels during the regulatory intervention period. We suggest that post-crisis regulation, by encouraging competition in market-making, could be a contributing factor. The idea is that big banks used to enjoy a big funding advantage over non-bank entities in corporate bond market-making business due to explicit (e.g. deposit insurance) and implicit (e.g. too-big-too-fail status) subsidies from the government. The funding advantage of big banks generated an entry barrier for non-bank entities to compete in this capital intensive business. If post-crisis regulations by and large reduced the funding advantage of big banks, this might have led to a level playing field for non-bank entities to compete. As a result, more players can now enter the market, and increased competition should induce a downward pressure on the price of intermediacy.

There is evidence consistent with this explanation. The average number of competing market-makers trading a bond has increased by 40% from the period of July 2007-April 2009 to the period of May 2009-May 2014 (Bessembinder et al. 2016). Competition between trading venues has also intensified: bond ETFs and electronic trading platforms such as MarketAxess provide investors with cost-effective ways to trade corporate bonds outside the OTC market dominated by big banks. Some market commentators also express a similar view. For example, on July 26, 2015 The Wall Street Journal in an article titled "Overlooking the Other Sources of Liquidity" reports "*Missing from much of this debate, however, is recognition of the radical transformation that has taken place in many fixed-income markets as barriers to entry have fallen and new liquidity providers have stepped forward.*"⁴⁶ With more non-bank entities entering the market-making business, overall liquidity supply may increase, and sources of liquidity supply may become more diversified. In this sense, post-crisis regulation might have actually made market liquidity more resilient. This perspective is often missing in the post-crisis policy debate and definitely requires further investi-

⁴⁴TRACE does not disseminate the agency trade indicator. We create a proxy which equals to 1 if two or more transactions of the same bond with the same volume and at the same price happen at the same time. See Dick-Nielsen (2009) for a detailed discussion for measuring agency trades with the TRACE database.

⁴⁵Even if agency transactions may not directly impact liquidity, a legitimate concern is that it may bias the measurement of liquidity. To address this concern, we drop all agency trades and repeat our tests exclusively on principal-based transactions. We still find no systematic evidence of liquidity deterioration. The results are available upon request.

⁴⁶See also an industry discussion panel titled "Are There Structural Issues in the U.S. Bond Market?" organized by the Brookings Institute for discussion on regulation and competition in market-making business.

gation beyond the scope of this paper.

4 Results for Market Liquidity of U.S. Treasuries

This section extends our analysis to the U.S. Treasuries market. Much of the interest and the discussion pertinent to this market's liquidity can be ascribed to the salience of events like the flash crash of October 15th, 2014 when the yield of the U.S. 10-year note dropped by 34 basis points from 2.2% to 1.86% in the eight minutes between 9:33 and 9:45AM Eastern Time.

In Table 4 we report the summary statistics for this asset class, including Noise, On-the-run premium, Roll measure (all expressed in basis points) and Turnover (negative) over the April 2005-December 2014 sample⁴⁷, again calculated at the monthly frequency. The correlations among these proxies are intuitively positive, with the exception of Turnover (negative), as reported in Table 5. The reason for this counterintuitive negative correlation is given by the construction of the measure for the Treasuries. As the denominator in the Turnover variable is the total stock of public debt outstanding, the explosion of U.S. sovereign debt as consequence of the automatic stabilizers and the 2009 Fiscal Stimulus appear to severely affect the quality of this measure post 2009, an issue that will become clearer below.

We employ the Bai and Perron (1998, 2003) approach to estimate breakpoints in the level of the four liquidity time series: Noise, On-the-run premium, Roll measure and Turnover (negative)⁴⁸. The corresponding double maximum tests indicates the presence of at least one structural break at the 5% confidence level in all four proxies, with the exception of the UD max for the Noise variable. However, for the same variable WD max reject the null that there is no break. The sequential sup $F_T(\ell + 1|\ell)$ indicates three breakpoints for the Noise and Roll measures, one for the On-the-run premium and four for the Turnover (negative). Figure 10 reports an informative visualization of when the breakpoints happen over time and in which direction the series breaks. For both the Noise and Roll measures this approach clearly captures the sudden deterioration of market liquidity around the 2008-09 financial crisis and a return to normality mid-2009. The Roll measures seems to suggest further liquidity amelioration in December 2011 (in fact close to the release of the first Proposed Volcker Rule published in November 2011). The On-the-run premium exhibits qualitatively very similar dynamics, as evident from the North-East panel in Figure 10, but our approach fails to pick up a structural break at the start of the crisis. The only proxy that seems to systematically break in terms of lower liquidity levels for Treasuries is Turnover (negative) in October 2008. However, looking at the components of this measure, this result appears mainly

⁴⁷In the online appendix Figure 7, we extend our analysis to an earlier sample period from 1995 to 2005, which covers the collapse of LTCM, a liquidity crisis much smaller in scale comparing to the 2008-09 financial crisis.

⁴⁸Given the small number of time series available for the analysis of liquidity of Treasuries we do not employ dynamic factor model approaches in this Section. Online appendix Table 12, 13 and 14 report the estimated break dates, the double maximum test statistics and sup $F_T(\ell + 1|\ell)$ test statistics respectively.

driven by two factors: 1. Treasury issuance dramatically increased after 2008. 2. The Federal Reserve balance sheet structurally increased, holding a very large portfolio of public debt due to the Quantitative Easing. Since the Fed typically is not actively trading, the turnover should intuitively drop.

5 Conclusions

This paper complements, both methodologically and substantively, a rigorous retrospective analysis of post-crisis regulatory intervention in domestic financial markets. Such analysis has been surprisingly bare in terms of systematic empirical evidence and it appears to be a necessary exercise in informing future legislative and rulemaking activities aimed at improving financial markets stability (Cochrane, 2014).

We specifically focus on the aftermath of the 2008-09 U.S. financial crisis and on the role played by the Dodd-Frank Act of 2010 and Basel III as potential triggers of liquidity shortages driven by retrenchment of financial institutions adversely affected by overreaching regulation. Several market participants have claimed this assessment to be crucial in the context of an informed cost-benefit analysis of regulatory intervention and rulemaking.

We initially focus on a large set of liquidity proxies with emphasis on the U.S. corporate bond market (an asset class likely to be adversely affected by regulatory tightening through disruption of ordinary market-making activities) and with particular attention paid to different underwriters, credit ratings, and issue sizes.

Our analysis is based on multiple estimation strategies, including standard breakpoint tests in levels, tests for structural breaks in dynamic factor models and difference-in-differences matching analysis. Reassuringly, the data display no statistical evidence of substantial deterioration in market liquidity after 2010. The tests presented are powerful enough to pick structural breaks in the data -they clearly pinpoint the crisis itself as a liquidity breakpoint- yet they consistently show no significant liquidity deterioration in the period of regulatory intervention covering the approval of the Dodd-Frank Act and Basel III, shutdowns of proprietary trading desks by major banks, or the proposal and finalization of the Volcker Rule. If anything, we detect evidence of liquidity improvement during periods of regulatory interventions, possibly due to the entry of non-banking participants. Evidence from the U.S. Treasuries market, by and large, confirms the absence of liquidity deterioration.

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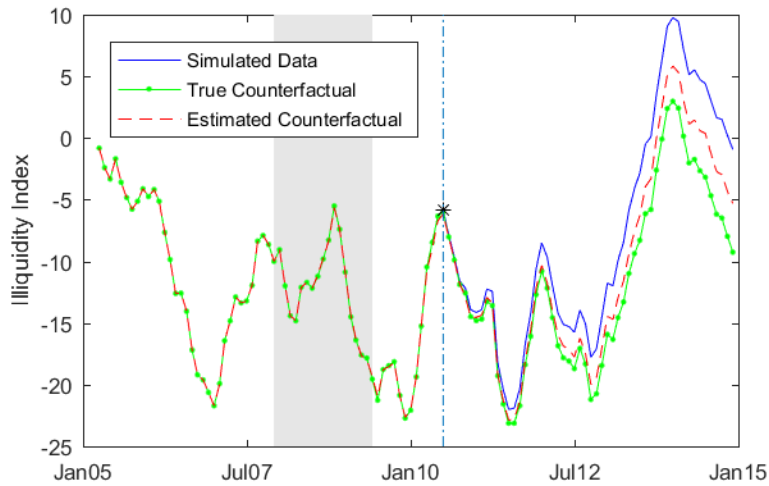


Figure 1. Simulated Liquidity Index

Notes: This graph shows the average of 180 simulated liquidity measures over time. The blue solid line represents the average of 180 liquidity measures if regulation leads to a gradual deterioration in market liquidity, while the green dotted line represents counterfactual scenario where regulation has no effects. The dashed vertical line indicates the date of true and estimated structural break in the latent factor structure. The red dashed line is the estimated counterfactual path. The break date is estimated by Chen, Dolado, and Gonzalo (2014) and Bai and Perron (2003) approach with 5 percent significance level. The sample period is from April 2005 to December 2014. The data frequency is monthly. The grey area indicates recession.

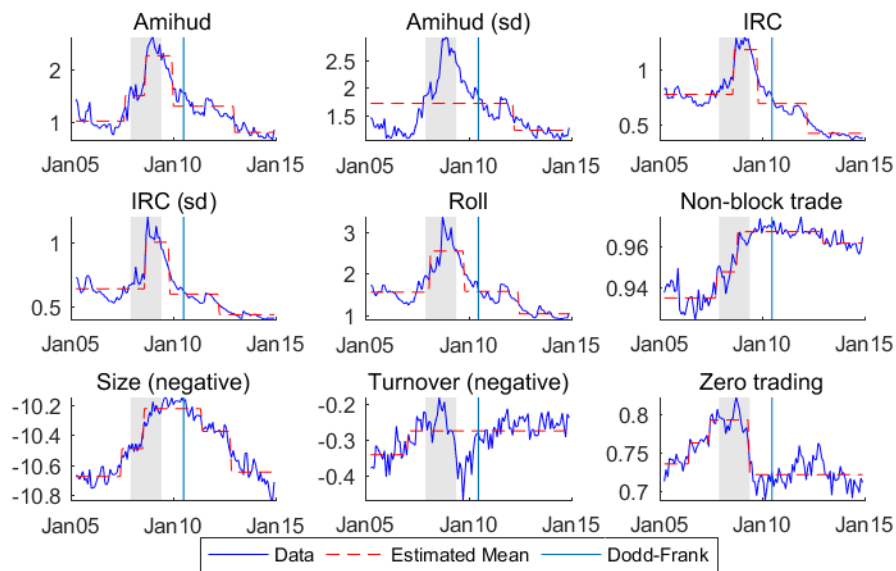


Figure 2. Time Series of Liquidity of U.S. Corporate Bonds (Aggregate-level)

Notes: This graph shows the time series of 9 aggregate-level liquidity measures of U.S. corporate bond market (blue line), and the estimated mean for each sub-period (red dashed line). The break dates (dates with a shift in the level of the red dashed line) are estimated by the Bai and Perron (1998-2003) approach with 5 percent significance level. The sample period is from April 2005 to December 2014. The data frequency is monthly. The grey area indicates recession.

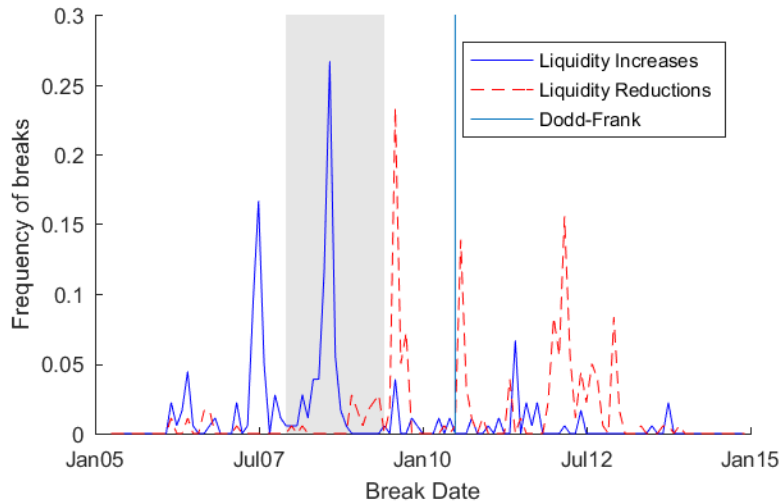


Figure 3. Breaks in the Means of Liquidity (Disaggregate-level)

Notes: This graph shows the frequency of break in the levels of 180 disaggregate-level liquidity measures for the U.S. corporate bond market over time. The x-axis shows the dates and the y-axis shows the corresponding fraction of the 180 liquidity measures which have a break at each date. The break dates are estimated using the Bai and Perron (1998-2003) approach with 5 percent significance level. The solid vertical line indicates the passage of Dodd-Frank Act (July, 2010). The sample period is from April 2005 to December 2014. The data frequency is monthly. The grey area indicates recession.

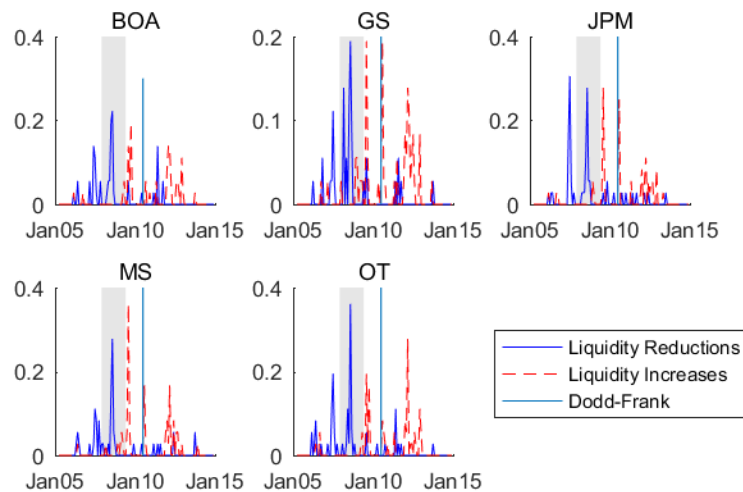


Figure 4. Breaks in the Means of Liquidity by Underwriter (Disaggregate-level)

Notes: This graph shows the decomposition of break dates by underwriter. The x-axis shows the dates and the y-axis shows the corresponding fraction of the 36 ($=9 \times 2 \times 2$) liquidity measures of each underwriter which have a break at each date. The break dates are estimated using the Bai and Perron (1998-2003) approach with 5 percent significance level. The solid vertical line indicates the passage of Dodd-Frank Act (July, 2010). The sample period is from April 2005 to December 2014. The data frequency is monthly.

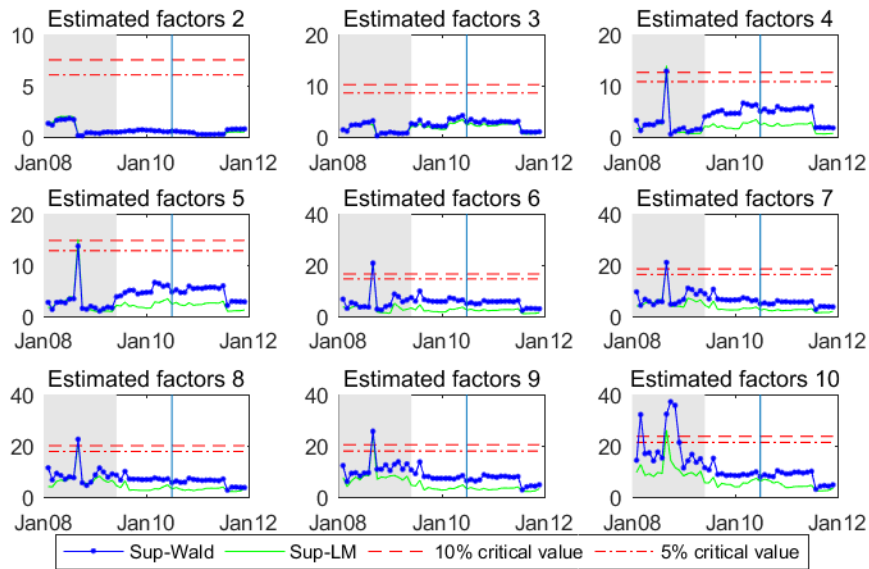


Figure 5. Test Statistics of a Single Break in the Dynamic Factor Structure

Notes: This graph shows the test statistics of a single break in factor structure of 180 disaggregate-level liquidity measures employing the Chen, Dolado, and Gonzalo (2014) approach. The sample period is from April 2005 to December 2014. The solid vertical line indicates the passage of Dodd-Frank Act (July, 2010). The data frequency is monthly. The grey area indicates recession.

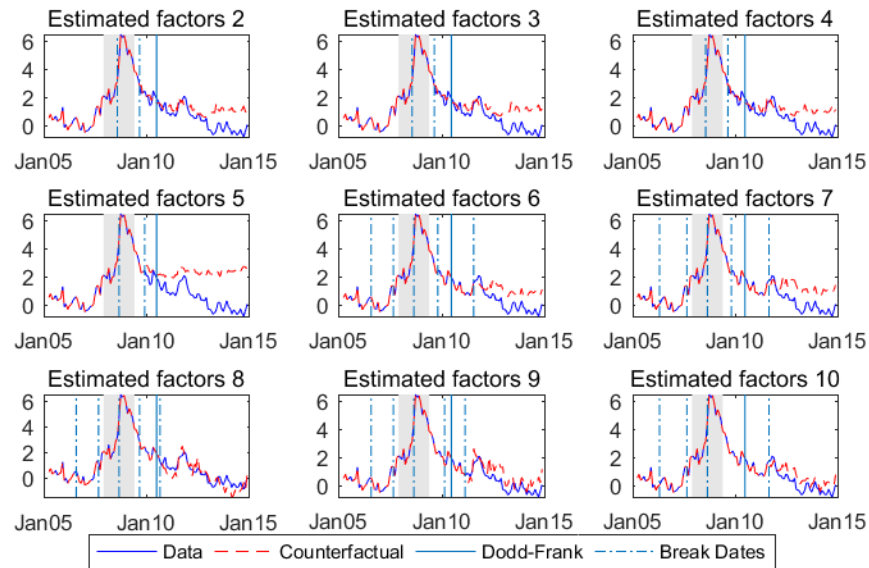


Figure 6. Liquidity Index of the U.S. Corporate Bond Market

Notes: This graph shows the average of 180 standardized liquidity measures of U.S. corporate bond market (blue solid line) and the estimated counterfactual path (red dashed line). The dashed vertical line indicates the dates of estimated structural breaks in the latent factor structure. The solid vertical line indicates the passage of Dodd-Frank Act (July, 2010). The break dates are estimated by Chen, Dolado, and Gonzalo (2014) and Bai and Perron (2003) approach with 5 percent significance level. The sample period is from April 2005 to December 2014. The data frequency is monthly. The grey area indicates recession.

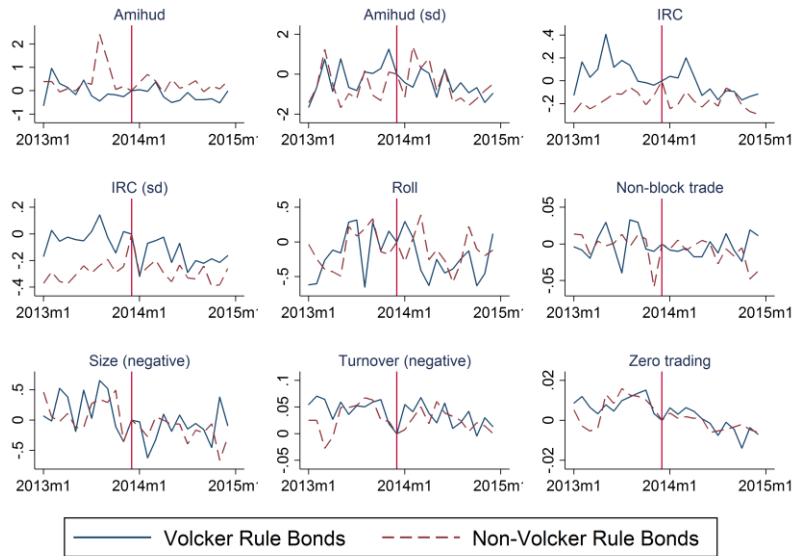


Figure 7. Liquidity of Volcker Rule and Non-Volcker Rule Bonds (Matched Sample)

Notes: This graph shows the time series of liquidity of Volcker Rule bonds and non-Volcker Rule bonds around the time when revised finalized version of the Volcker Rule was approved (January 2014). A non-Volcker Rule bond is defined as a bond which at least one of the underwriters is not subject to the Volcker Rule. A Volcker Rule bond is defined as a bond which all of the underwriters are subject to the Volcker Rule. Both time series are normalized to 0 in December 2013. The red vertical line indicates the date when the revised finalized version of the Volcker Rule was approved (2014m1). The sample period is from January 2013 to December 2014. The data frequency is monthly.

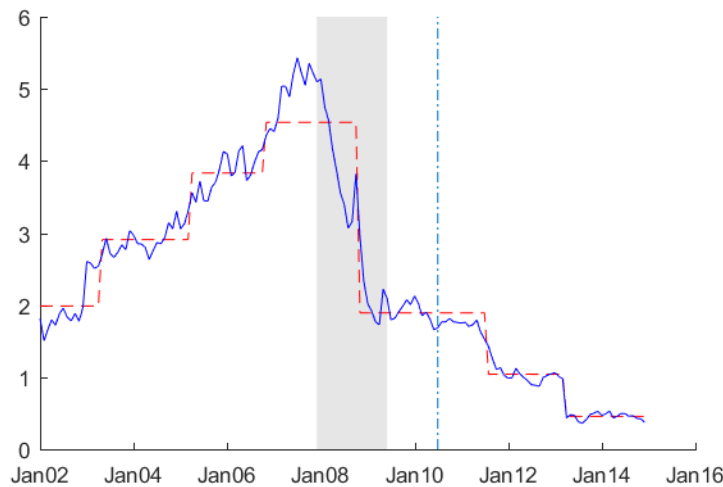


Figure 8. Primary Dealer Corporate Bond Holding

Notes: This graph shows the time series of the U.S. primary dealer corporate bond holding as the percentage of total corporate bond outstanding (blue line) and the estimated mean for each sub-period (red dashed line). The solid vertical line indicates the passage of Dodd-Frank Act (July, 2010). The break dates (dates with a shift in the level of the red dashed line) are estimated by the Bai and Perron (1998-2003) approach with 5 percent significance level. The sample period is from January 2002 to December 2014. The data frequency is monthly. The grey area indicates recession.

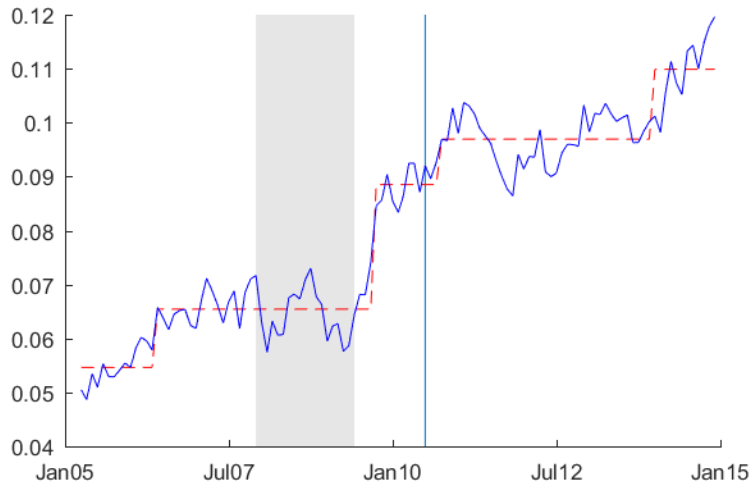


Figure 9. Fraction of Agency Transactions

Notes: This graph shows the fraction of agency transactions (blue line), and the estimated mean for each sub-period (red dashed line) over time. The break dates (dates with a shift in the level of the red dashed line) are estimated by the Bai and Perron (1998-2003) approach with 5 percent significance level. The solid vertical line indicates the passage of Dodd-Frank Act (July, 2010). The sample period is from April 2005 to December 2014. The data frequency is monthly. The grey area indicates recession.

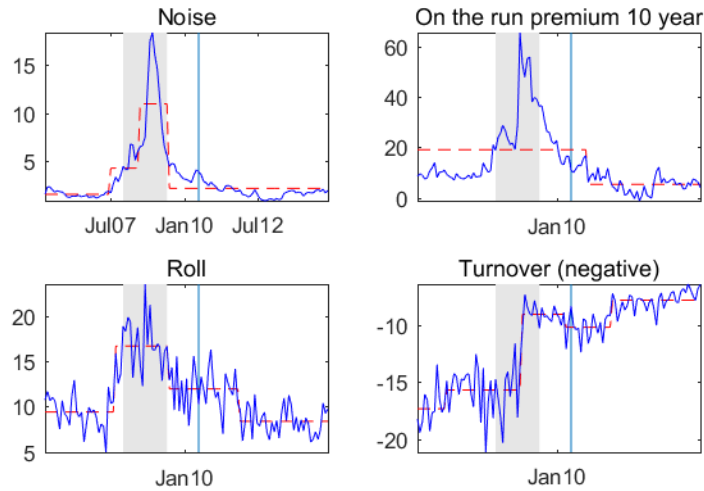


Figure 10. Time Series of Liquidity of the U.S. Treasury Bonds

Notes: This graph shows the time series of liquidity measures of U.S. Treasury market (blue line), and the estimated mean for each sub-period (red dashed line). The break dates (dates with a shift in the level of the red dashed line) are estimated by the Bai and Perron (1998-2003) approach with 5 percent significance level. The solid vertical line indicates the passage of Dodd-Frank Act (July, 2010). The sample period is from April 2005 to December 2014. The data frequency is monthly. The grey area indicates recession.

Table 1: Summary Statistics of the U.S. Corporate Bond Liquidity (Aggregate-level)

Measures	N	mean	sd	p10	p25	p50	p75	p90
Amihud	117	1.29	0.48	0.79	0.94	1.17	1.47	2.12
Amihud (sd)	117	1.57	0.48	1.10	1.19	1.43	1.83	2.35
IRC	117	0.70	0.24	0.40	0.49	0.70	0.82	1.08
IRC (sd)	117	0.61	0.18	0.42	0.47	0.58	0.67	0.88
Roll	117	1.59	0.54	0.96	1.23	1.52	1.81	2.41
Non-block trade	117	0.96	0.01	0.93	0.94	0.96	0.97	0.97
Size (negative)	117	-10.48	0.20	-10.71	-10.66	-10.51	-10.28	-10.18
Turnover (negative)	117	-0.29	0.05	-0.36	-0.32	-0.28	-0.25	-0.23
Zero-trading	117	0.74	0.03	0.71	0.72	0.74	0.76	0.79

Notes: This table shows the summary statistics of 9 aggregate-level liquidity measures for the U.S. corporate bond market. The sample period is from April 2005 to December 2014. The data frequency is monthly. The unit of Amihud, Amihud (sd), IRC, IRC (sd), and Roll is percentage point. The unit of Non-block trade, Turnover (negative) and Zero-trading is 1.

Table 2: Correlation Table of the U.S. Corporate Bond Liquidity (Aggregate Level)

	Amihud	Amihud (sd)	IRC	IRC (sd)	Roll	Non-block trade	Size (negative)	Turnover (negative)
Amihud (sd)	0.98							
IRC	0.88	0.84						
IRC (sd)	0.91	0.88	0.98					
Roll	0.93	0.93	0.96	0.97				
Non-block trade	0.29	0.33	-0.15	-0.06	0.01			
Size (negative)	0.75	0.76	0.51	0.51	0.58	0.65		
Turnover (negative)	-0.07	0.05	-0.28	-0.20	-0.10	0.27	0.00	
Zero-trading	0.40	0.43	0.54	0.52	0.56	-0.43	0.03	0.37

Notes: This table shows the correlations among 9 aggregate-level liquidity measures for the U.S. corporate bond market. The sample period is from April 2005 to December 2014. The data frequency is monthly.

Table 3. Difference-in-Difference Regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Amihud	Amihud (sd)	IRC	IRC (sd)	Roll	Non-block trade	Size (negative)	Turnover (negative)	Zero- trading
Volcker*Post	-0.231 [0.466]	-0.115 [0.359]	0.106* [0.0573]	0.113** [0.0474]	0.0177 [0.122]	0.00168 [0.00265]	0.0145 [0.0859]	-0.0160 [0.0118]	-0.00202 [0.00320]
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bond F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	542	340	1807	1260	1992	2006	2006	11060	11060
Adjusted R-squared	0.458	0.398	0.238	0.325	0.245	0.312	0.524	0.353	0.879

Notes: This table shows the difference-in-difference regression of Volcker Rule bonds and non-Volcker Rule bonds around the time when revised finalized version of the Volcker Rule is approved (January 2014). A non-Volcker Rule bond is defined as a bond which at least one of the underwriters is not subject to the Volcker Rule. Each of the non-Volcker Rule bonds in our sample is matched to a Volcker Rule bond which issues in the same month, matures in the same month, has the same rating group (investment-grade/high-yield), and has a relative size difference less than 50 percent of the average size of the pair. The sample period is from January 2013 to December 2014. The data frequency is monthly. Control variables include the reciprocal of issue age, and the reciprocal of issue age squared. The standard errors are two-way clustered at the bond and month level. ***,**,* indicates 1 percent, 5 percent, and 10 percent significance level respectively.

Table 4. Summary Statistics of the U.S. Treasury Liquidity

Measure	N	mean	sd	p10	p25	p50	p75	p90
Noise	117	3.14	3.24	1.20	1.48	1.93	3.33	6.51
On the run premium	117	13.48	12.62	3.33	6.23	8.94	16.39	28.73
Roll	117	13.37	4.09	8.62	10.35	12.73	15.83	19.23
Turnover	117	-11.48	3.93	-17.64	-14.76	-9.79	-8.11	-7.39

Notes: This table shows the summary statistics of liquidity measures for the U.S. Treasury market. The sample period is from April 2005 to December 2014. The data frequency is monthly. The unit of Noise, On the run premium and Roll measure is basis point. The unit of Turnover (negative) is 1.

Table 5. Correlation Table of the U.S. Treasury Liquidity

	Noise	On the run premium	Roll
On the run premium	0.90		
Roll	0.62	0.72	
Turnover	0.03	-0.08	-0.37

Notes: This table shows the correlations between liquidity measures for the U.S. Treasury market. The sample period is from April 2005 to December 2014. The data frequency is monthly.