Does liquidity disclosure regulation negatively affect liquidity holdings in the banking system? *

Yao Lu

The University of Chicago Booth School of Business

yao.lu@chicagobooth.edu

July 25, 2019

Abstract

I study a spillover effect of mandatory bank liquidity disclosure on banks' liquidity holdings, using the exogenous shock of the US Liquidity Coverage Ratio (LCR) disclosure requirement. I find that the disclosure reduced non-disclosing banks' liquidity holdings, especially those that learned more from the disclosure. To pin down an information channel through which liquidity disclosure affected liquidity holdings, I show that the reduction in liquidity is unlikely caused by the requirement on minimum LCR levels, that the reduction is greater when the disclosure is more important for other banks, and that the disclosure reduced banks' incentive to form relationships with the disclosing banks. Overall, my results suggest an unintended–and potentially undesirable–externality of liquidity disclosure regulation on the financial system.

*I thank Philip Berger, Jonathan Bonham, Matthias Breuer, Hans Christensen, Douglas Diamond, John Gallemore, Pingyang Gao, João Granja, Zhiguo He, Anil Kashyap, Anya Kleymenova, Christian Leuz, Miao Liu, Jinzhi Lu, Valeri Nikolaev, Thomas Rauter, Haresh Sapra, Douglas Skinner, Andrew Sutherland, Joanna Wu, and workshop participants at Chicago Booth for helpful comments and suggestions. I also thank two anonymous bankers from Goldman Sachs for providing institutional insights. All errors are my own.

1 Introduction

Lack of liquidity in the financial system was at the center of the 2007-2009 financial crisis. Runs by both creditors (i.e., deposit outflows) and borrowers (i.e., credit drawdowns) resulted in a loan-to-deposit shortfall at the aggregate level (Acharya and Mora, 2015; Ivashina and Scharfstein, 2010). Insufficient disclosure in the banking system is believed to have added to heightened uncertainty during the crisis (Bischof, Laux, and Leuz, 2018). In an effort to promote market discipline and encourage prudent liquidity reserving, global bank regulators introduced liquidity disclosure rules on top of liquidity holding requirements after the crisis. As the first of such new rules adopted in the US, the disclosure requirement (81 FR 94922) for large banks' Liquidity Coverage Ratio (LCR hereafter) information was introduced along with rules on minimum LCR levels.

Despite the importance of this topic, academic research on bank liquidity, liquidity disclosure, and related regulation has been limited (Allen and Gale, 2017; Diamond and Kashyap, 2016). Both theory and empirical work has focused on illiquidity problems during crises, while relatively few papers have studied banks' liquidity holding incentives in normal times, which could be the source of a systematic illiquidity outbreak. Even less discussed are the effects of (mandatory) liquidity disclosure on liquidity holdings. Key questions that remain unanswered include what role does liquidity disclosure play in banks' liquidity holdings decisions? How would a bank react to other banks' liquidity disclosure? Could the LCR disclosure regulation have unintended spillover effects on the financial system?

To address these questions, I study the exogenous shock of US LCR disclosure regulation and find evidence consistent with a spillover effect that reduces non-disclosing banks' liquidity holdings. Given regulators' expectation that LCR disclosure "*increases* liquidity in the market *as a whole*, thereby limiting the risk that a liquidity event will lead to asset fire sales and contagion effects in the financial sector" (81 FR 94922), my finding suggests an unintended–and potentially undesirable–externality of the LCR disclosure regulation.

Bank managers face a trade-off between holding more liquid assets (or "cash") and holding more illiquid assets (or "loans"). Holding cash incurs an opportunity cost, as cash does not generate interest income. But holding cash reduces the risk of illiquidity when a sudden liquidity shock (outflow) happens. Because of the uncertainty of future liquidity conditions in the banking system, bank managers hold precautionary liquidity to mitigate the potential loss from a liquidity shock (Gale and Yorulmazer, 2013; Acharya and Skeie, 2011). Liquidity disclosure from certain banks, especially systemically important ones, provides useful information about aggregate liquidity condition and reduces other banks' uncertainty about future liquidity. With less uncertainty about overall liquidity in the future, bank managers may be less conservative in holding liquidity. Therefore I predict that liquidity disclosure by certain banks could have a spillover effect that dampens other banks' liquidity holding incentives and that the effect comes through an information channel, where disclosure reduces bank managers' uncertainty about aggregate liquidity. This prediction is formalized in a simple model presented in Appendix A.

I empirically test this prediction by studying the heterogeneous impact of mandatory LCR disclosure on non-disclosing banks' liquidity holdings. LCR is the ratio of High Quality Liquid Assets (HQLA) to projected total net cash outflows over the next 30 calendar days. The minimum LCR rule requires large US banks to maintain an LCR of at least 80%, 90%, and 100% starting from January 1$^{\text{st}}$ of 2015, 2016, and 2017 respectively. Closely following that was the LCR disclosure requirement, which stipulated that banks with at least $700 billion assets or at least $10 trillion assets under custody disclose quarterly both quantitative information concerning their LCR calculation and a qualitative discussion about key drivers of their LCR starting from the second quarter of 2017.

This disclosure requirement could affect different non-disclosing banks differently. It is more likely to surprise a bank and improve its knowledge of liquidity in the banking system if the bank is less familiar with the disclosing banks and had known less about their liquidity before the LCR disclosure. Therefore I measure non-disclosing banks' liquidity information improvement (which decreases with their knowledge of the disclosing banks) based on the frequency of their interactions with LCR-disclosing banks in the loan syndication market, and investigate whether banks that learned more from the LCR disclosure (or with less knowledge of/interactions with the disclosing banks) reduced liquidity holdings to a greater extent after implementation of the LCR disclosure rule. I use banks' loan co-syndication as a proxy for their knowledge of each other's liquidity condition, because syndicate members have the incentive and ability to learn each other's liquidity condition, and banks tend to enter syndicates with banks they know and trust.

This setting offers several unique advantages that alleviate typical concerns in identifying a spillover effect of disclosure. First, since LCR disclosure is mandatory, identifying its spillover effect is less subject to the reflection problem (Manski, 1993), e.g., industry-wide shocks causing changes in both banks' disclosure and liquidity decisions. Second, the typical difficulty of finding firms only treated by the spillover effect and finding a control group is less of a concern in my setting. The LCR disclosure requirement only applies to seven banks (out of 210) in my sample, which gives me a large sample of non-disclosing banks that were only affected by a spillover effect. Moreover, the variation in the influence of the LCR disclosure enables comparison of banks that are more treated with those less treated, with the latter group essentially serving as the control. Third, the fact that the disclosure rule is implemented after the 100% LCR requirement helps disentangle the effect of disclosure regulation from that of fundamental regulation. Finally, the design of a generalized difference-in-differences regression with bank fixed effects, year-quarter fixed effects, and bank-level controls alleviates (though does not eliminate) concerns that the results might be driven by unrelated but concurrent institutional changes or market-wide shocks.

My empirical tests begin with a network analysis on bank co-syndication interactions. I first verify that banks in my sample interact with each other in a single and closely connected network, instead of several segregated ones. This result suggests that information about certain banks matters to all banks in the system, because they are closely connected, either directly or indirectly. I next verify that that the seven disclosing banks contribute the majority of interactions in the network. This result indicates that these banks are key players in the market, so that their disclosure provides important information to other banks. In sum, these results justify my underlying assumption that LCR disclosure provides relevant and important liquidity information to all banks in my sample.

As the first step of my regression tests, I examine the average changes in banks' liquidity holdings after the LCR disclosure rule. I find that non-disclosing banks reduced liquidity holdings significantly after the disclosure requirement, while LCR disclosing bank increased their holding but only insignificantly. I also find that banks that experienced greater improvement in liquidity information after the disclosure rule significantly reduced their liquidity holdings, while those with less improvement did not change their liquidity positions.

In my main test, I investigate the heterogeneous changes in liquidity holdings for banks differentially surprised by the LCR disclosure. I find that non-disclosing banks with greater liquidity information improvement experienced a significantly greater decline in both liquidity level and liquidity growth after the adoption of the LCR disclosure rule. This treatment effect is economically meaningful: an average non-disclosing bank experienced a 1.2 percentage points decrease in its liquidity ratio (i.e. liquid assets scaled by total assets) after LCR disclosure, which is approximately 8% of the average liquidity ratio or 10% of liquidity ratio's standard deviation. This result is consistent with the prediction that liquidity disclosure leads to lower liquidity holding incentives and that the effect comes through an information channel.

To pin down the information channel through which LCR disclosure affects bank liquidity holdings, I conduct three additional tests. First, I conduct placebo tests around the periods of minimum LCR requirement adoption and investigate whether the fact that LCR disclosing banks keep their LCR above a certain threshold is enough to reduce other banks' liquidity holdings. The fact of regulated banks maintaining a minimum LCR is much coarser a signal about aggregate liquidity than the required LCR disclosure, and it was well anticipated although not confirmed before the minimum LCR rule. Nevertheless, it contains new information that could affect other banks' liquidity holdings. Also, there are fundamental changes in liquidity holdings of the regulated banks after these dates. Both forces could continue to exist in the LCR disclosure period, and their impact could commingle with the effect of the disclosure, weakening my identification. Despite these possibilities, I fail to find a significant difference in the changes of banks' liquidity holdings level or growth after any of these dates between banks with greater liquidity information improvement and those with less. This result alleviates concerns that the LCR disclosure effect in the main results is mainly driven by the mere knowledge of disclosing banks meeting the minimum LCR level or by the effects of disclosing banks' changing liquidity fundamentals.

Second, I investigate whether non-disclosing banks' liquidity holdings responds even more to the improvement of liquidity information coming from lead arrangers in syndicates. This should be the case if the spillover effect indeed comes from information, because lead arrangers plays a key role in a syndicate, and thus their liquidity disclosure should matter more to non-disclosing banks in a syndicate. I find evidence consistent with this conjecture.

Third, I examine whether non-disclosing banks reduced their interaction with the seven disclosing banks after LCR disclosure. Disclosure regulation levels the playing field among firms with heterogeneous access to information and crowds out private information collection (e.g. Breuer, 2018; Goldstein and Yang, 2017). If LCR disclosure does provide useful information to other banks and banks form closer relationships with the disclosing banks partially because the relationships help the former learn about the latter's liquidity condition, then LCR disclosure could reduce the incentive for maintaining these relationships. Consistent with this idea, I find that non-disclosing banks on average reduced their interactions with LCR disclosing banks after the disclosure regulation and that those with closer relationships with the disclosing banks reduced these interactions more.

I conduct a series of sensitivity tests, and my main results are robust to changes in sample selection criteria, sample period, additional control variables, and an alternative measure of liquidity information improvement.

This paper contributes to three strands of literature. First, it contributes to the literature on bank liquidity and liquidity regulation by investigating the spillover effect of mandatory liquidity disclosure. Existing works on bank liquidity primarily focus on factors during a crisis that cause or exacerbate bank illiquidity, including strategic hoarding (e.g. Acharya, Gromb, and Yorulmazer, 2012; Gale and Yorulmazer, 2013), unexpectedly large liquidity shocks (e.g. Babus, 2016), counterparty risk (e.g. Afonso, Kovner, and Schoar, 2011), and undrawn loan commitments (e.g. Cornett, McNutt, Strahan, and Tehranian, 2011; Acharya and Mora, 2015) etc. Few studies examine banks' liquidity holding incentives in normal times (e.g. Acharya, Shin, and Yorulmazer, 2011) or the role played by liquidity disclosure. The papers studying the new LCR regulation focus mostly on the liquidity holding rules (e.g. Diamond and Kashyap, 2016; Du, 2017), while to the best of my knowledge, no paper studies the LCR disclosure rule.

Second, this paper contributes to the literature on bank disclosure by identifying a new potential cost of bank disclosure regulation. Bank disclosure plays an important role in the stability of the financial system, but it is unclear whether requiring more disclosure is necessarily beneficial (Acharya and Ryan, 2016; Goldstein and Sapra, 2014). Works in bank liquidity disclosure concentrate on how a specific type of mandatory liquidity disclosure, namely disclosure of banks' access to the Federal Reserve discount window (DW), could discourage potential disclosers' usage of the DW, i.e., the DW stigma (e.g. Kleymenova, 2018; Ennis, 2017). Banks respond to this disclosure requirement by holding more liquidity because of the stigma (Kleymenova, 2018). Different from these studies, I examine LCR disclosure, which provides arguably richer and more fundamental information about a bank's liquidity condition. Moreover, I study an information channel through which disclosure could induce costs to the financial system by reducing banks' uncertainty. This differs from mechanisms discussed in prior literature (Goldstein and Sapra, 2014), such as reducing risk sharing (Hirshleifer, 1978), managerial short-termism (Gigler, Kanodia, Sapra, and Venugopalan, 2014), coordination failure (Morris and Shin, 2002), and crowding out private information from the market (Bond and Goldstein, 2015).

Third, this paper contributes to the broad literature on the real effects of disclosure. Research has examined a wide range of real effects, including investment, corporate governance, health care, and corporate social responsibility (surveyed in Leuz and Wysocki, 2016; Roychowdhury, Shroff, and Verdi, 2018). Adding to this literature, I investigate a new type of real effect, i.e., banks' liquidity holdings. Most real effects research focuses on disclosure's effects on the discloser, i.e., a feedback effect (Kanodia and Sapra, 2016), and we don't have enough evidence on its spillover effect, which is critical in evaluating disclosure regulations (Leuz and Wysocki, 2016). The emerging literature in this area provides some evidence that firms learn from peers' financial statements and that this knowledge affects their investment decisions (e.g. Badertscher, Shroff, and White, 2013; Beatty, Liao, and Yu, 2013; Durnev and Mangen, 2009). Following the same logic, but looking at a different set of disclosure and outcomes, I study whether banks learn from peers' LCR disclosures and then change their liquidity holding decisions. Moreover, I am able to better identify the spillover effect, thanks to some unique features of my setting.

This study could be of interest to bank regulators, as it reveals an unintended and potentially undesirable externality of liquidity disclosure regulations. While the LCR disclosure rule aims to enhance liquidity holding of the disclosing banks, it could reduce the liquidity holding incentives of other banks, which represent the majority of banks in the banking system. Therefore it is unclear to what extent this regulation is able to "encourage sound risk-management practices" or "increase liquidity in the market as a whole", as the regulators intended, if we take its potential externalities into consideration.

The rest of the paper is organized as follows. Section 2 provides the background on LCR and the disclosure requirements in the US. Section 3 describes the empirical design. Section 4 discusses the data and sample. Section 5 reports the results. Section 6 concludes.

2 LCR regulations in the US

Liquidity Coverage Ratio (LCR) is the ratio of a bank's high-quality liquid asset (HQLA) to its projected net cash outflows over a 30-day period. LCR standard is the first of the quantitative standards for liquidity in the Basel III liquidity framework, with the purpose of enhancing banks' liquidity risk management. Regulations on minimum LCR and LCR information disclosure have been implemented in the US. Appendix B Figure B1 shows the timeline of US LCR regulation.

2.1 Minimum LCR requirements

In the US, the LCR requirement (79 FR 61440) was introduced on September 3rd, 2014. Under this rule, large banks are required to maintain a gradually increased minimum LCR. In particular, internationally active banks (those with \$250 billion or more in total consolidated assets or \$10 billion or more in on-balance-sheet foreign exposure) and any consolidated bank or savings association subsidiary of one of these banks that, at the bank level, has total consolidated assets of \$10 billion or more ("covered banks") are required to maintain an LCR of at least 80%, 90%, 100% starting from January 1st of 2015, 2016 and 2017. Bank holding companies and savings and loan holding companies without significant insurance or commercial operations that, in each case, have \$50 billion or more in total consolidated assets but are not internationally active ("modified LCR banks") must meet the 90% and 100% LCR requirements by January 1st of 2016 and 2017.

2.2 LCR disclosure requirements

The LCR disclosure requirement (81 FR 94922) was introduced on December 19th, 2016, and was implemented shortly after the 100% LCR requirement. The requirement was implemented in three stages. Starting from April 1st, 2017, "covered banks" with \$700 billion or more in total consolidated assets or \$10 trillion or more in assets under custody were required to disclose quarterly both quantitative information about their LCR calculation and a qualitative discussion of key drivers of LCR. Starting from April 1^{st}, 2018, this disclosure was required for other "covered banks". Finally, starting from October 1^{st}, 2018, "modified LCR banks" had to comply with this disclosure requirement. My sample period ends at the beginning of the second stage (i.e. the second quarter of 2018), so I only focus on the disclosure in the first stage. Seven banks (i.e., Bank of America, Bank of New York Mellon, Citigroup, Goldman Sachs, JP Morgan, State Street, and Wells Fargo) made mandatory LCR disclosures in my sample.

LCR disclosure contains new and useful liquidity information about the disclosing banks. Moreover, since the disclosing banks are major players in the banking system, their liquidity information is indicative of the aggregate market liquidity condition. As an example of this disclosure, I paste snapshots of JPMorgan's second quarter 2017 LCR disclosure in Appendix B Figure B2. Panel A is the table of quantitative information used to calculate LCR, which provides detailed breakdowns of liquid assets and cash in(out)flows and how these are weighted in calculating LCR. This disclosure illuminates the bank's liquidity position (i.e., liquid assets) and its potential liquidity needs (i.e., expected net cash outflows). While the disclosure on liquidity position might not be that "new" to the market, given similar information provided in banks' balance sheet, the detailed disclosure on expected future cash outflows is completely new, and there was no other public source of that information before this disclosure requirement. This whole new information is particularly useful for banks in predicting future liquidity needs (or the probability of a liquidity shock) in the market and making liquidity holdings decisions. Panel B is an example of a qualitative discussion (on High Quality Liquid Assets), which provides additional information beyond the quantitative disclosures. For example, the highlighted parts indicate that, although JPMorgan has access to Federal Reserve Bank discount window, the bank does not consider it as a primary source of liquidity.

3 Empirical design

I exploit three unique features of the LCR disclosure regulation to empirically test the predictions that LCR disclosure reduces other banks' liquidity holding incentives and that this effect comes from an information channel, i.e., the reduction of uncertainty about aggregate liquidity.

First, the LCR disclosure affects non-disclosing banks as a group. Since the LCR disclosure provides new and important information about disclosing banks' future liquidity condition and the disclosing banks are major players in the banking network, the disclosure improves non-disclosing banks' knowledge of aggregate liquidity.

Second, the LCR disclosure affects different non-disclosing banks differently. The impact on the non-disclosing banks varies with the liquidity knowledge these banks have about the disclosing ones. A bank that knows less about the disclosing banks' liquidity condition is more likely to be surprised. The disclosure therefore will improve its knowledge of aggregate liquidity to a greater extent.

Third, the LCR disclosure is less likely to affect non-disclosing banks' liquidity through an agency channel. The disclosure is not about the non-disclosing banks, so that their reaction to the disclosure is unlikely due to reduced information asymmetry (and agency conflict) between their insiders and outsiders. The disclosure could reduce agency costs by serving as a benchmark that disciplines non-disclosing banks' liquidity holdings. However, if that is true, it is unclear why lower agency costs should lead to lower liquidity holdings.

The first two features enable me to test not only the average change in non-disclosing banks' liquidity holdings after the LCR disclosure rule, but also the differential liquidity holdings changes for banks with different levels of liquidity information improvement. Besides, the first two and the third features suggest that this disclosure effect is more likely coming through an information channel than an agency channel.

Using these features, I can measure banks' liquidity information improvement due to LCR disclosure, which I term $LiqInfoImprove$. The variation in $LiqInfoImprove$ comes from non-disclosing banks' heterogeneous knowledge about disclosing banks, and it enables me to conduct a generalized difference-in-differences regression, comparing the changes in liquidity holdings after the LCR disclosure regulation for non-disclosing banks that are less affected versus those that are more affected. In addition, because $LiqInfoImprove$ reflects the reduction in uncertainty, this design also tests the information channel through which LCR disclosure affects liquidity holdings by reducing uncertainty.

In the following subsections, I construct a network of bank interactions and use it to measure $LiqInfoImprove$. Then I describe the details of my identification strategy and specification.

3.1 A bank interaction network

As discussed above, how much a non-disclosing bank learns about aggregate liquidity condition from the LCR disclosure, i.e., the bank's $LiqInfoImprove$, depends on its knowledge of the disclosing banks' liquidity. I construct a network of banks' loan co-syndication interactions and use the frequency of these interactions as a proxy for banks' knowledge about each other's liquidity before the LCR disclosure. Co-syndication interaction is a reasonable proxy for banks' knowledge of each other's liquidity because syndicate members have the incentive and ability to learn each other's liquidity condition, and banks tend to enter syndicates with banks they know and trust.

A bank is motivated to learn about other syndicate members' liquidity condition because, if some members fail to provide the committed amount of loan, due to liquidity problems, the bank might need to make up the difference, which could be suboptimal. While the bank does not have any legal responsibility to do so, it could nevertheless face the pressure to provide extra funding to maintain a good reputation and client relationship. For example, Ivashina and Scharfstein (2010) show that banks that co-syndicated more with Lehman Brothers experienced greater drawdowns and cut their lending more during the 2007-2009 financial crisis. Besides, if the bank does not provide extra funding and a deal fails, it will squander all its earlier investment in this deal.

A bank is also able to acquire syndicate members' liquidity information. Liquidity information collected in interbank (e.g., Federal Funds) transactions is often used by a bank's risk management department to evaluate these transactions. This information can be used again by the same risk management department when the bank co-syndicates with those same banks. A bank can also ask for information about a certain syndicate member if the former does not know the latter well.

More generally, banks tend to form syndicates with banks they "know" and "trust". Syndicate members do not explicitly examine each other's liquidity or request private information every time they start a syndicate. They self-select into groups where they are familiar with each other. Therefore it is likely that banks with closer co-syndication relationships are also banks that know each other better.[1]

Admittedly, despite the advantages of this measure, co-syndication interaction is not a perfect proxy for banks' bilateral information set. For example, co-syndication does not happen very frequently, especially for smaller banks, and counterparty liquidity is not the most important concern in these transactions, though it does matter.[2] Nevertheless, it is the best publicly available information for constructing a bank interaction network, and it arguably captures banks' knowledge of each other's liquidity condition to some extent.[3]

[1] I thank two anonymous bankers from Goldman Sachs for providing institutional details related to this discussion of loan co-syndication.

[2] Interbank (e.g., Federal Funds) transaction could be a better measure, given its higher frequency and closer connection with liquidity. Unfortunately, this data is not publicly available.

[3] Another concern with my measure of liquidity knowledge is a survival bias: a bank that is more informed about another bank's bad news might stop interacting with the latter (either in interbank market or syndicate market). If so, my measure based on bank interactions could not capture banks with superior information about other banks' weakness. In other words, my measure could be biased in the direction of capturing "informed banks with good news". This is unfortunately an inevitable bias in my measure, and it would exist even if I could use interbank transactions as my measure. However, the impact of this bias at the aggregate level is likely small. Considering that bad news is usually more transitory than good and that low interaction intensity potentially captures either 1) uninformed banks or 2) informed banks with bad news, low interaction intensity should primarily capture less informed ones on average.

3.2 Measure of liquidity information improvement

The key independent variable in my empirical tests is $LiqInfoImprove$, which measures banks' improvement in their knowledge of liquidity condition in the banking system after LCR disclosure. Using the bank network described above and the timing of the LCR disclosure shock, I construct $LiqInfoImprove_{i,t}$ for each individual bank, Bank i, on quarter t. In particular, $LiqInfoImprove_{i,t} = 0$ in and before the second quarter of 2017, when the required LCR disclosures were released for the first time. Starting from the third quarter of 2017, $LiqInfoImprove_{i,t}$ equals 1 minus the $Interactions_{i,j}$ weighted average of $Disclosure_j$, where $Interactions_{i,j}$ is the number of syndicated loans Bank i and j have issued together from the first quarter of 2010 through the second quarter of 2018[4], and $Disclosure_j$ equals 1 if Bank j is required to disclose LCR information after the second quarter of 2017 and 0 otherwise. Formally,

$$LiqInfoImprove_{i,t} = \left(1 - \frac{\sum_j Interactions_{i,j} * Disclosure_j}{\sum_j Interactions_{i,j}}\right) \cdot \mathbb{1}_{\{t \geq 3Q17\}} \tag{3.2.1}$$

Figure 1 illustrates this calculation using a hypothetical network of a bank.

This measure is intuitive. From an absolute point of view, if a bank has more interactions with banks regulated by the LCR disclosure requirement (larger $\sum_j Interactions_{i,j} * Disclosure_j$), the bank will have *broader* knowledge of aggregate liquidity condition. From a relative point of view, if a bank's interactions are mainly with LCR disclosing banks (i.e. larger $\frac{\sum_j Interactions_{i,j} * Disclosure_j}{\sum_j Interactions_{i,j}}$), the bank will have a *deeper* understanding of the liquidity condition of those banks. Moreover, banks with broader knowledge usually also have

[4]Note that it is fine to use "future" (relative to the dates of making liquidity holdings decisions) loan syndication data to construct the network, because the network is used only to measure a bank's knowledge of other banks' liquidity condition, and that knowledge could be reflected in its future loan co-syndication activities (especially considering the slow moving and low frequency nature of co-syndications). The beginning of the period (first quarter of 2010) is chosen so that the period is long enough to have sufficient observations but not so long as to include too early interactions that are not representative of those in the period of my tests (mainly around 2017). Other than this consideration, the choice of the beginning quarter is arbitrary.

deeper knowledge, which is implied by the negative correlation between $LiqInfoImprove$ and $Interactions$, as shown in Table 1 Panel C. Therefore the breadth and depth of a bank's knowledge of the liquidity condition of LCR disclosing banks tend to be complements, and thus $LiqInfoImprove_i$ can capture both. Note that the absolute value of $LiqInfoImprove$ alone is not meaningful, as it is only used to compare banks' liquidity information improvement *relative* to other banks. For example, $LiqInfoImprove_i = 0$ does not mean that Bank i knew everything about the seven disclosing banks' LCR disclosures before disclosure was required. Appendix C further discusses the intuition of this measure, using three hypothetical cases.

Admittedly, this simple definition of $LiqInfoImprove$ might not capture other factors that affect banks' liquidity information improvement after LCR disclosure. In one of the robustness tests (Table 12), I use another definition of $LiqInfoImprove$, which considers the influence of bank size on banks' interaction. The main results are robust to this alternative definition.

3.3 Identification strategy

An ideal experiment to test the spillover effect of liquidity disclosure is to randomly provide different levels of liquidity disclosure to otherwise identical non-disclosing banks and to examine whether banks seeing more disclosure on average hold less liquidity. Absent of such an experiment, empirically identifying spillover effects of disclosure using archival data faces several challenges (Leuz and Wysocki, 2016). I exploit unique features in my setting to alleviate these concerns.

First, testing spillover effects in a voluntary setting (i.e., where firms can choose whether to go public, disclose, or misreport, etc.) suffers from the reflection problem (Manski, 1993), i.e., industry-wide shocks lead to changes in both firms' disclosure choices and outcome variables. In my setting, since LCR disclosure is mandatory, its spillover effect on non-disclosing banks should not be subject to this issue.

Second, using mandatory disclosure regulations to identify spillover effects can also be challenging. Since disclosure regulations typically apply to all firms in an industry, it is hard to find firms that are mainly subject to spillover effects, instead of direct effects, and it is even harder to find a control group that is affected by neither direct nor spillover effects. This concern is alleviated by the fact that the LCR disclosure requirement only applies to seven banks (out of 210) in my sample, so that the non-disclosing banks in my tests are not affected by direct effects. Besides, I exploit the variation in the influence of LCR disclosure on different banks to differentiate banks that are more and less treated, which enables identification of the spillover effects using a generalized difference-in-differences design. Finally, since the disclosing banks are important players in the banking system, I can test the spillover effects of their disclosure with sufficient power.

Third, using a regulatory shock to identify the disclosure effect could suffer from endogeneity issues if the regulation is confounded by other concurrent regulations. This is of less a concern in my setting because the LCR disclosure rules were adopted *after* the 100% LCR requirement, which enables me to single out the effect from disclosure alone. Of course, the effects of fundamental changes could continue after the 100% LCR requirement and creep into the disclosure regulation period. This is a valid concern, especially considering the short gap between the two regulations. To deal with it, I test the impact of liquidity fundamental changes on banks' liquidity holdings, and I do not find a significant effect.

Finally, the disclosure regulation might be deliberately implemented at a time of, or simply coincide with, a general trend of other unrelated but concurrent institutional changes or market-wide shocks, which could also affect the outcomes of interest. To address this concern, I run a generalized difference-in-differences regression, controlling for bank fixed effects, year-quarter fixed effects, and bank-level key determinants of liquidity holdings. This design essentially compares the changes in liquidity holdings for banks with more improvement in liquidity information after the liquidity disclosure regulation with banks with less improvement, and the fixed effects control for time-invariant bank characteristics and market wide time-trends. The remaining concern is that banks with greater and less liquidity information improvement are fundamentally different, so that omitted time-varying bank characteristics could contribute to the observed effects. While this concern cannot be alleviated completely, I try to address it by controlling for key bank-level determinants of liquidity holdings, which partially removes the effects of time-varying bank characteristics that could contribute to the observed treatment effect. In robustness tests, I rerun the main regressions using subsamples of banks that are more similar to further eliminate this concern.

3.4 Specification

The basic idea of identifying the disclosure effects is to control the typical determinants of banks' liquidity holdings and examine whether changes in disclosure could have incremental effect. I run the following regressions on non-disclosing banks in my main tests:

$$\begin{aligned} LiquidityMeasure_{i,t+1} = {} & \beta_0 + \beta_1 LiqInfoImprove_{i,t} + \beta_2 IlliquidRatio_{i,t} \\ & + \beta_3 CoreDepositRatio_{i,t} + \beta_4 CapitalRatio_{i,t} + \beta_5 CommitmentRatio_{i,t} \\ & + \beta_6 Size_{i,t} + Bank\ FE + Year\text{-}quarter\ FE + \epsilon_{i,t} \end{aligned} \tag{3.4.1}$$

LiquidityMeasure represents two dependent variables: *LiquidityRatio* or Δ*Liquidity*.

Following Cornett, McNutt, Strahan, and Tehranian (2011), I control bank-level key drivers of liquidity holdings: illiquid assets ratio, core deposits ratio, capital ratio, unused commitments ratio, and size. *Illiquid Assets* cannot satisfy liquidity needs, and selling them (or using them as collateral) for liquidity could incur considerable costs, especially in a crisis, when liquidity is scarce. The higher the illiquid asset ratio, the higher the risk of incurring such a cost. Therefore banks have more incentives to increase liquidity when they have a higher illiquid asset ratio. *Core Deposits* are a relatively stable source of funding (compared to short-term debt) and a natural hedge to liquidity risk, because deposit insurance attracts deposit inflows in a crisis, when depositors seek a safe haven

(Gatev, Schuermann, and Strahan, 2009; Gatev and Strahan, 2006). Hence banks with a higher core deposits ratio have less incentive to increase liquidity holdings. *Capital Ratio* also affects the liquidity holding incentive. On one hand, a higher capital ratio creates a buffer for depositors, so that banks are less concerned about liquidity risk and more willing to lend (i.e., reduce liquidity). On the other hand, holding too much equity reduces banks' ability to lend. *Unused Commitments* increase banks' liquidity risk, especially in downturns, when takedown demand increases. Therefore banks with a higher level of unused commitments are more likely to increase liquidity to reduce this risk. *Size* affects the liquidity holding incentive because larger banks are perceived by the market as safer ("too big to fail"), which could reduce larger banks' incentive to hold liquidity.

4 Data and sample

I collect data of banks' loan co-syndication history from Thomson-Reuters' LPC DealScan. A bank is included in my sample if it is a syndicated loan lender that is classified as a US bank, thrift/S&L, mortgage bank, or investment bank and its executive office is in the US. Banks belonging to a common holding company are aggregated to the top bank holding company and treated as a single banking organization.

Bank holding company financial variables are acquired from FR Y-9C reports published on the websites of the Federal Reserve Bank of Chicago. Since DealScan and the FR Y-9C Reports use different bank identifiers, I manually matched banks in the two datasets by name, state, and city and dropped lender banks when I am not certain about the correct match.

The dataset consists of bank-quarter level observations of bank characteristics and loan co-syndication interactions. The sample period covers the first quarter of 2011 through the second quarter of 2018, which ends before the second phase of the LCR disclosure requirement and goes back long enough to conduct my tests (mostly using one to two years around key dates in 2015-2017). Banks in the sample have to have participated at least one syndicated loan with other banks during the first quarter of 2010 through the second quarter of 2018.

5 Results

5.1 Descriptive statistics

Table 1 reports descriptive statistics for key bank characteristics and bank interaction network variables. Panel A reports bank characteristics variables. Banks in the sample have a mean (median) liquid assets to total assets ratio of 16%(12%). Illiquid assets are 75% (79%) of total assets on average (median). Based on both mean and median measures, core deposits are around 60% of total assets, capital ratio is around 10%, and unused commitments are around 14% of the sum of unused commitments and total assets. These statistics resemble to those reported in prior literature (e.g. Cornett, McNutt, Strahan, and Tehranian, 2011; Acharya and Mora, 2015) and are slightly closer to the statistics of large banks reported by Cornett, McNutt, Strahan, and Tehranian (2011). This is understandable, as my sample selection process tends to keep larger banks.

Panel B reports descriptive statistics of bank interaction network variables. There are 210 banks in my sample. The mean (median) number of connected banks (i.e., banks with co-syndication histories) a bank has is 24 (13), and the mean (median) number of total interactions (i.e., co-syndicated loans) is 1,498 (22). The distributions of the two variables are significantly right skewed, indicating that a few very large banks participated many more syndications than the remaining banks. My key independent variable, $LiqInfoImprove$, which ranges from 0 to 1 by design, has a mean and median of 0.66 and 0.72 respectively. The fact that this ratio is closer to 1 than 0 suggests that banks on average interact more with banks other than the seven LCR disclosing banks, despite the fact that the latter are major players in the bank interactions. Figure 4 plots the histogram of the three variables.

Panel C reports the correlation matrix of network variables. $LiqInfoImprove$ is negatively correlated with the number of interactions. This means that banks that have more overall interactions with other banks (i.e., higher $Interactions$) are more likely to interact with the LCR disclosing banks (i.e., lower $LiqInfoImprove$). As discussed in Section 3.2, this feature indicates that the depth and breadth of a bank's knowledge about aggregate liquidity are complements. In other words, it is less likely to see a bank with frequent interactions with other banks but few interactions with the LCR disclosing banks. This feature ensures that my liquidity information improvement measure, which captures more about the depth of such knowledge, also captures the breadth dimension.

5.2 Network analysis

As the first step of my empirical tests, I construct the bank interaction network, described in Section 3.1, and verify two important features of it.

Figure 2 plots the network of bank co-syndication interactions for banks in the sample. Each node represents a bank, and an edge connects two nodes whenever the two banks represented by the two nodes have co-syndicated at least once. The size of each node increases with the number of different banks the bank represented by the node has co-syndicated with. Nodes are partitioned and color coded based on their modularity class (i.e., nodes in the same module have more interactions with each other than with nodes in other modules). The node layout is conducted by the ForceAtlas2 algorithm, which groups (pushes away) nodes with more (fewer) interactions.

First, I verify that the network consists of one closely connected single network, instead of several segregated networks, so that LCR disclosure matters to all banks, instead of only to those in the same networks as the disclosing banks. While the plotting algorithm tries to push away (cluster) banks that are less (more) interacted in Figure 2, all banks are closely inter-connected in a single network without clear segregation: there is not a group of nodes located apart from other nodes, and nodes with different colors are mixed together.

Second, I verify that the seven disclosing banks are major players in the bank interaction network, so that their LCR disclosures provide relevant information about aggregate liquidity condition in the banking system. Figure 3 plots banks with top 30 *interactions*, where black bars represent the LCR disclosing banks. While only seven banks disclose the required LCR information, these banks are indeed major players in the bank interaction network, contributing 52% of the total *Interactions* by all 210 banks in the sample.

In sum, verifying these two features justifies my underlying assumption that LCR disclosure provides important information about aggregate liquidity condition relevant for all banks in the banking system.

5.3 Changes in liquidity holdings after the LCR disclosure rule

I begin my analysis on the effect of the LCR disclosure rule with simple pre-post comparisons among different groups of banks. In Table 2 Panel A, I compare liquidity holdings changes after the LCR disclosure rules for LCR disclosing banks and non-disclosing banks. The liquidity holdings of LCR disclosing banks did not change significantly after the disclosure requirement, as coefficients of *Post* in columns 1 and 3 are both positive and insignificant. On the contrary, non-disclosing banks significantly reduced their liquidity holdings and liquidity growth: the coefficients of *Post* in columns 2 and 4 are both significantly negative.

In Table 2 Panel B, I compare liquidity holdings changes after the LCR disclosure rules for non-disclosing banks with top and bottom 25% improvements in liquidity information (i.e., $LiqInfoImprove$). Columns 1 and 3 imply that banks that experienced greater improvement in liquidity information after the LCR disclosure significantly reduced their liquidity holdings and liquidity growth. On the contrary, columns 2 and 4 indicate that banks with less liquidity information improvement did not change their liquidity holdings behavior.

While not causal evidence, these results are consistent with my prediction that LCR disclosure reduces non-disclosing banks' liquidity holding incentives and that the effect is stronger for banks with greater $LiqInfoImprove$. Next, I formally test this causal relationship using a generalized difference-in-differences design.

5.4 Effect of the LCR disclosure requirement on liquidity holdings

Table 3 reports the results of my main regression in model 3.4.1. In particular, I regress liquidity ratio and liquidity growth rate on liquidity information improvement, and I gradually add fixed effects and control variables from columns 1 to 5. I use a sample period of the first quarter of 2016 through the second quarter of 2018, and I drop banks with too few (less than five) or too many (more than 10,000) interactions with other banks.

Panel A reports the effect of LCR disclosure on the ratio of liquid assets to total assets. All the five coefficients of $LiqInfoImprove$ are negative, and four out of five are statistically significant. I will focus on the regression of column 5, which includes the most comprehensive controls and fixed effects. The regression coefficient of $LiqInfoImprove$ is -0.0178 and is statistically significant at 5%. This is also economically significant: an average (median) bank experiences 0.66 (0.72) increase in $LiqInfoImprove$ after LCR disclosure (Table 1 Panel B), which translates into a 1.2 (1.3) percentage point decrease in liquidity ratio (i.e., 0.66×1.78, 0.72×1.78). This is approximately 8% (10%) of the 16% (13%) mean (median) of liquidity ratio (Table 1 Panel A). While the effect is economically meaningful, it is not too large to be true: the average 1.2 percentage point drop in liquidity ratio is around 10% of the standard deviation of the liquidity ratio, 11.7%.

In Panel B, I conduct the same analysis for the growth of liquid assets. Again, all regression coefficients of $LiqInfoImprove$ are negative, and three are statistically significant. In particular, in column 5, the coefficient of interest is -0.0178 and is significant at 5% level. This means that banks with greater liquidity information improvement are faster at reducing their liquidity position.

Panels A and B combined suggest a quite significant impact of liquidity disclosure: banks with greater improvement in their knowledge about aggregate liquidity respond to LCR disclosure by cutting more liquidity and cutting at a faster pace than banks less surprised by the disclosure.

This result rules out one key alternative explanation for the overall liquidity reduction we see in Table 2 Panel A columns 2 and 4: higher liquidity holdings by the disclosing banks (indicated by columns 1 and 3) increases overall liquidity availability in the market and thus dampens other banks' liquidity holding incentives. This explanation does not predict that the effect of disclosing banks' additional liquidity holdings should be positively associated with other banks' $LiqInfoImprove$.

A key concern about the above results is that, for reasons unrelated to LCR disclosure, there might be a general trend of lowering liquidity for banks with high $LiqInfoImprove$ during the sample period, and that is what fundamentally drives the results we see in Table 3. In other words, the parallel-trends assumption for the generalized difference-in-differences design might not hold. To alleviate this concern, I map out counterfactual treatment effects over the sample period by replacing $LiqInfoImprove$ in Table 3 with a series of counterfactual $LiqInfoImprove$, where the latter is defined in the same way as the the original one, except that each of the counterfactual $LiqInfoImprove$ assumes that the LCR disclosure is available only in each of the eight quarters from the first quarter of 2016 to the second quarter of 2018, excluding the second quarter of 2017. This analysis could provide the pattern in the counter-factual treatment effects relative to the second quarter of 2017, which is the quarter immediately prior to the quarter when required LCR disclosure was first released (and observed).

Figure 5 plots the coefficients and two-tailed 90% confidence intervals of all counterfactual $LiqInfoImprove$. Panel A shows that, before the second quarter of 2017, the counterfactual treatment effects on the liquidity ratio are stable and indistinguishable from 0. The treatment effects occurred right after that quarter and are significantly negative. While statistically the parallel trend can never be verified, this result suggests that there was at least a reasonably parallel trend in the pre-regulation period. The sharp decline of the coefficients immediately after the regulation is therefore more likely due to the treatment effect than a general trend.

Panel B reports the same analysis for liquidity growth. The results are qualitatively the same as the one for the liquidity ratio. We still see a quite stable and close-to-zero pre-trend, and the coefficients immediately decline significantly below zero for two out of three quarters after LCR disclosure. The confidence intervals are wider here, potentially because liquidity growth is more volatile than liquidity level.

5.5 Effect of the minimum LCR rule on liquidity holdings

While the main results provide evidence consistent with my prediction, there could be confounding factors that contribute to the observed results. An obvious potential confounder is the minimum LCR standard adopted shortly before the LCR disclosure requirements. The regulation on minimum LCR results in two key changes in the economy. First, after the regulation is implemented, banks updated their information set that the disclosing banks now hold at least certain amount of liquidity. Second, the regulation changes the fundamentals of disclosing banks. Both the additional information and the fundamental changes could affect other banks' liquidity holding incentives, confounding the disclosure effects I try to identify.

To alleviate this concern, I created three counterfactual treatment variables, i.e., LCR_1Q15, LCR_1Q16 and LCR_1Q17, for periods starting from the first quarters of 2015, 2016, and 2017, when the minimum LCR was raised to 80%, 90% and 100% respectively. These three variables differ from the counterfactual $LiqInfoImprove$ in Figure 5 in that they assume the LCR disclosure rule is implemented during the period from the three starting quarters to the end of the regression sample periods, instead of in only one quarter.

In Table 4, I regress liquidity ratio and growth on each of the three counterfactual measures and put all of them together as controls in the main regression. Panel A shows the results for liquidity ratio. For comparison purposes, I put column 5 in Table 3 in the first column here. Columns 2-4 test the effects of LCR_1Q15, LCR_1Q16, and LCR_1Q17 individually. None of the three counterfactual treatment effects is significant. In column 5, I include all three counterfactual variables together with $LiqInfoImprove$, and only the coefficient of $LiqInfoImprove$ is significantly negative. Panel B reports similar results for liquidity growth. Again, none of the three counterfactual treatment effects is significant.

This test suggests that the mere knowledge of disclosing banks meeting a certain LCR threshold and the fundamental changes from higher LCR standards are unlikely to have contributed significantly to the LCR disclosure effects I document in the main tests.

5.6 Effect of lead arrangers' LCR disclosures

In my main tests, $LiqInfoImprove$ increases with a bank's co-syndication with LCR disclosing banks, regardless of the latter's role in the syndicate (i.e., lead arranger or participant). Since a lead arranger is at the core of issuing a syndicated loan and usually contributes the largest portion of the funding, syndicate members should care more about the lead arranger's information than that of other participants'. Therefore, if it is indeed because of the information in the LCR disclosure that the disclosure affected non-disclosing banks' liquidity holdings, the latter should be more responsive to the liquidity information coming from lead arrangers than to those from other participants in a syndicate. To test this idea, I redefine $LiqInfoImprove$ so that it increases only when a bank co-syndicates with LCR disclosing banks that are lead arrangers (i.e., does not increase if they are merely participants), and I predict that non-disclosing banks' liquidity holdings should respond even more strongly to this modified measure than to the original measure. In particular, I modify the definition in 3.2.1 as follows.

$$LiqInfoImprove\_LeadArranger_{i,t} = \left(1 - \frac{\sum_j Interactions\_LA_{i,j} * Disclosure_j}{\sum_j Interactions_{i,j}}\right) \cdot \mathbb{1}_{\{t \geq 3Q17\}} \tag{5.6.1}$$

$Interactions\_LA_{i,j}$ is the number of syndicated loans Bank i has issued with Bank j when Bank j was the lead arranger.

I then replace $LiqInfoImprove$ with $LiqInfoImprove\_LeadArranger$ and rerun the main regression 3.4.1. The results are reported in Table 5. Consistent with my prediction, the coefficients of $LiqInfoImprove\_LeadArranger$ are 37-53% more negative than those of $LiqInfoImprove$ in column 5 of Table 2, with similar statistical significance. This result further supports my argument that non-disclosing banks responded to the LCR disclosure because of the information they learned from it.

5.7 Effect of the LCR disclosure requirement on bank relationships

If LCR disclosure does provide useful information to non-disclosing banks, the disclosure could also affect the latter's decision to form relationships with the disclosing banks. Banks with close relationships with disclosing banks lost some of their information advantage (i.e., the knowledge of disclosing banks' liquidity) after LCR disclosure and therefore could have less incentive to maintain these relationships. To test this idea, I examine whether non-disclosing banks reduce their co-syndication interactions with the disclosing banks after the LCR disclosure requirement and whether those with less liquidity information improvement (i.e., those that interact more with LCR disclosing banks) cut these interactions more. I conduct tests similar to those in Table 2 and Table 4, only I replace the dependent variables with $LCR\ Interaction_{i,t}$, which is a quarterly version of the fraction on the right-hand side of Equation 3.2.1:

$$LCR\ Interaction_{i,t} = \frac{\sum_j Interactions_{i,j,t} * Disclosure_j}{\sum_j Interactions_{i,j,t}} \tag{5.7.1}$$

In Table 6, I find that non-disclosing banks significantly reduced their interactions with LCR disclosing banks on average after the disclosure requirement (column 2). The interactions among LCR disclosing banks also decreased but to a lesser extent and insignificantly (column 1). Among non-disclosing banks, those who learned more from the disclosure (i.e., top 25% $LiqInfoImprove$ banks) did not significantly change (if anything, they increased, although insignificantly) their interactions with LCR disclosing banks (column 3), while those that learned less (i.e., those who previously knew more because of closer relationships with disclosing banks) significantly reduced their interactions (column 4).

In Table 7 column 1, I conduct the generalized difference-in-differences analysis, similar to the main test, and find that banks that learned more from LCR disclosure are less likely to reduce interactions with disclosing banks than those that learned less after the disclosure regulation. Placebo tests, reported in columns 2 to 5, indicate that the differential change in interactions with disclosing banks did not take place around the periods of fundamental requirements on minimum LCR levels (despite some effects from the first stage of the minimum LCR requirements).

Overall, the evidence is consistent with my prediction that LCR disclosure reduced banks' reliance on relationships with disclosing banks to acquire information about the latter's liquidity. This analysis strengthens my earlier argument that the information provided by LCR disclosure affects banks' liquidity holdings because banks learned relevant liquidity information from the disclosures.

5.8 Robustness tests

In this section, I conduct a series of robustness tests to further sharpen my identification. One concern about the generalized difference-in-differences design is that the control and treatment groups might differ fundamentally, so that they respond differently to LCR disclosure for reasons other than differentiated liquidity information improvement. In particular, banks with more co-syndication interactions tend to have lower $LiqInfoImprove$ (Table 1 Panel C) than those with fewer interactions. Since the former could differ significantly from the latter, this difference could drive the results. To mitigate this concern, I test the robustness of my main results to the range of banks' $Interactions$ of my sample. For both panels in Table 8, I start from the full sample and gradually drop banks with too many or too few $Interactions$ from columns 1 to 4. In column 1, the coefficient of $LiqInfoImprove$ is insignificant. This is not surprising, given the noisy observations included from banks with extremely few (< 5) or many ($> 10,000$) interactions with other banks. From columns 2 to 4, although the range of $Interactions$ shrinks from 5–10,000 to 5–100, the significance and magnitude of the regression coefficients of $LiqInfoImprove$ do not change much.

Another concern is that the sample period used in my main tests could be too short, so that the observed treatment effects might only reflect mean-reversion and so they could become insignificant if the pre-period is long enough. To mitigate this concern, I gradually extend the start of my sample period from the first quarter of 2016 to the first quarter of 2011. As we can see from the results in Table 9 Panel A, the longer sample period makes the treatment effects on liquidity level even slightly stronger. In Panel B, the longer sample period only slightly reduces the magnitude of the treatment effect on liquidity growth, and the disclosure effect is still negative and significant at 5%.

Next, I investigate whether the variation of my key independent variable $LiqInfoImprove$ is driven by certain bank characteristics, so that the treatment and control groups in my regression are determined not by banks' improvement in aggregate liquidity knowledge but by variation in these bank-level variables. I first regress $LiqInfoImprove$ on a set of bank-level variables. These variables capture several major aspects of a bank's performance, such as business growth, risk-taking, asset quality, and profitability. Table 10 reports the results. Column 1 only includes independent variables that are used as controls in the main regression. Column 2 adds the two dependent variables in the main tests, liquidity ratio and liquidity growth. Column 3 includes more variables. Across the three tests, $LiqInfoImprove$ is only significantly related with illiquid ratio, bank size, and interest income. I then include the three variables in the main tests as controls. I also control for earnings before provision to control the effect of profitability. Furthermore, I include the interactions of these controls with time dummies to control for their potentially time-varying influence on liquidity holdings. The results are reported in Table 11: The additional controls do not meaningfully affect the estimated treatment effects.

Finally, I examine whether my main results are robust to a different definition of $LiqInfoImprove$. I use a simple definition (Equation 3.2.1) for this variable to capture the key concept in a way that is easy to interpret. A simple measure might, however, miss certain factors that are important to banks' liquidity information improvement. The size of banks that a bank interacts with could be one such factor. It is likely that a bank learns more about a larger bank than a smaller bank, even if it has the same number of interactions with the two. This could be because the larger bank is a more important potential lender or business partner (e.g., in loan syndication), so the bank makes more effort to collect the larger bank's information. Therefore it is sensible to weight $Interactions_{i,j}$ by the size of banks j, i.e. $Size_j$, in Equation 3.2.1. Formerly, I define:

$$LiqInfoImprove\_SizeWeighted_{i,t} = \left(1 - \frac{\sum_j Interactions_{i,j} * Size_j * Disclosure_j}{\sum_j Interactions_{i,j} * Size_j}\right) \cdot \mathbb{1}_{\{t \geq 3Q17\}} \tag{5.8.1}$$

In Table 12, I replace $LiqInfoImprove$ with $LiqInfoImprove\_SizeWeighted$ and rerun the main regressions. The regression coefficients of the newly defined independent variable are still significantly negative. Therefore my results are robust to this new definition of $LiqInfoImprove$.

6 Conclusion

With the ongoing introduction of liquidity regulations in the global banking system, understanding their unintended consequences is important. This could help regulators better anticipate the costs and benefits of these new rules. Liquidity disclosure regulations are important part of these regulatory changes because disclosure could affect both disclosing and non-disclosing banks' liquidity decisions and therefore the stability of the financial system. Despite the importance of this topic, academic research on the implications of mandatory liquidity disclosure has been scarce, partially because of the relatively short history of these regulations.

In this paper, I provide empirical evidence that mandatory liquidity disclosure could dampen banks' liquidity holding incentives. This is potentially because this disclosure provides new and relevant information about liquidity condition in the banking system, which reduces bank managers' uncertainty about the probability of future liquidity shortages, and encourages banks to reduce their precautionary liquidity holdings.

Using the mandatory LCR disclosure setting and a bank interaction network, I find that non-disclosing banks on average reduced their liquidity holdings after the implementation of the LCR disclosure rule and that those that learned more about aggregate liquidity from LCR disclosures were more likely to reduce their liquidity holdings. I also find that the requirement for a minimum LCR ratio does not have the same spillover effect, that the LCR disclosures from lead arrangers syndicates have a stronger effect, and that LCR disclosure reduces banks' incentive to form relationships with disclosing banks. My main results are robust to a series sensitivity tests.

While this paper points out an unintended spillover effect of liquidity disclosure, it does not evaluate the overall welfare implications of the rule change. Nevertheless, it sheds light on the cost-benefit assessment of this important regulatory change. The documented externality of LCR disclosure regulation could be regarded as a cost, at least in the sense that it undercuts the regulator's stated goal of increasing liquidity at the aggregate level.

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Appendix A. Model to generate the empirical prediction

I use a simple model to illustrate how banks could be incentivized to reduce liquidity holdings when better information reduces their uncertainty about future aggregate liquidity condition in the market.

I model an economy of identical banks whose managers make liquidity holdings decisions, anticipating a potential liquidity shock (e.g., unexpected large-scale deposit outflows) in the future. To bank managers, the probability of having a liquidity shock is a random variable with a uniform distribution. Bank managers are conservative, in that they always assume the chance of a liquidity shock to be the upper bound of that uniform distribution when making liquidity holdings decisions. When the liquidity shock realizes, banks with liquidity shortages can either prematurely liquidate their loans for a fixed low price or sell loans in the interbank market. Banks face a trade-off between holding more liquid assets (or "cash") or more illiquid assets (or "loans"). Holding cash incurs opportunity cost as cash does not generate interest income, but it also reduces the risk of premature loan liquidation. In equilibrium, banks' liquidity holdings increases with the upper bound of the uniform distribution of the liquidity shock probability. Liquidity disclosure provides information about the probability of a future liquidity shock to bank managers. This information reduces their uncertainty about the probability, lowers the upper bound of its distribution, and results in lower equilibrium liquidity holdings. In short, the model predicts a negative effect of liquidity disclosure on liquidity holdings in the banking system.

A.1 The basic economy

In the economy, there are a continuum of one identical banks and three dates: Days 0, 1, and 2. Each bank i has one unit of asset at Day 0, and it chooses to invest a proportion of it, α_i, in cash, and the rest, $(1-\alpha_i)$, in loans. $\alpha_i \in [0,1]$. The aggregate liquidity holdings in the banking system is $\alpha = \int_0^1 \alpha(x)dx$.

For each unit of loan holding at Day 0, there will be a certain payoff $(1 + r)$ at Day 2 and nothing in between. r is constant and is greater than 0. Cash holdings can be withdrawn at par value on either Day 1 or Day 2.

On Day 1, each bank will experience either a "liquidity shock" of t liquidity outflow (e.g., unexpected deposit withdrawal) with probability p or no liquidity shock with probability $1-p$. t is a constant, and $t < 1$, so that a bank can always put enough cash aside at Day 0 to completely meet all possible liquidity needs (i.e., t) by itself. To bank managers, p is unknown at Day 0, and it is a random variable uniformly distributed over $[\underline{p}, \overline{p}]$, where $\underline{p} \leq p \leq \overline{p}$. $\overline{p}$ ($\underline{p}$) is estimated by bank managers based on all available liquidity information at Day 0, and it reflects the worst (best) scenarios of all uncertain factors affecting p. Bank managers are conservative in the sense that they always "prepare for the worst" and make liquidity holdings decisions based on the highest possible value of p to them, i.e., $\overline{p}$.

If the liquidity needs are satisfied, the extra cash a bank has on Day 1 generates no profit or cost. If the liquidity needs of a bank are not satisfied by its own liquidity holdings, the bank can raise liquidity by either selling loans to other banks, at the price of η dollars per unit of loan or prematurely liquidate loans at the price of θ dollars per unit of loan. θ is constant and $\theta \in (0, 1)$. This ensures that liquidating a loan is worse than not investing in the loan in the first place. η is determined in the interbank market, as discussed below.

A.2 Market clearing model

At Day 1, loan and cash are traded in the the interbank market. Clearing the market requires both a price function for the loan and rationing for the cash or the loan traded, whichever is under-supplied. Pricing is not enough here because all banks are identical, and if the overall liquidity supply is smaller (greater) than liquidity demand, cash (loan) available for trade needs to be allocated evenly across all banks that needs cash (loan), i.e., through rationing.

I assume that η is determined by the interbank market in the following way. First, η increases with the *perceived* overall liquidity surplus in the market, $(1-p)\alpha/c$, and decreases with the *perceived* overall liquidity demand, $p(t-\alpha/c)$. t, p, and α are defined above. c captures market sentiment of liquidity availability: it is greater than 0, and it increases with market pessimism about liquidity availability in the economy. The market is overly optimistic when $c<1$, rational when $c=1$, and overly pessimistic when $c>1$. Therefore α/c is the *perceived* amount of overall market liquidity that could be used to meet banks' own or other banks' liquidity needs at Day 1. In short, η increases with $\lambda \equiv \frac{(1-p)\alpha/c}{p(t-\alpha/c)}$. Second, η is greater than or equal to θ, the price one can get by premature liquidation. Otherwise, banks will simply liquidate their loans, instead of selling in the interbank market. Third, η is less than or equal to $1+r$, the payoff of the loan at Day 2. This ensures a nonnegative time value of holding loans from Day 1 to Day 2. Finally, η equals 1, i.e., the loans are traded at par value at Day 1, when $\lambda=1$. This ensures that banks would be indifferent between holding cash or loans at Day 0, had they known that the perceived liquidity supply and demand would be the same at Day 1. Combining all these features, I write the price function for loan at Day 1 as:

$$\begin{aligned}\eta &= \min\{(1-\theta)\lambda+\theta, 1+r\} \\ &= \min\left\{(1-\theta)\frac{(1-p)\alpha/c}{p(t-\alpha/c)}+\theta, 1+r\right\}\end{aligned} \tag{A.2.1}$$

The four features described above are met: η increases with λ, it is bounded between θ and $1+r$, and it equals 1 when $\lambda=1$. This price function can be viewed as a modified version of "cash-in-the-market pricing", which has the form of $\min\{\frac{(1-p)\alpha}{p(t-\alpha)}, 1+r\}$ (e.g. Allen and Gale, 2005; Acharya and Yorulmazer, 2007). The modification allows loans to be liquidated outside the interbank market and thus puts a lower bound, θ, on η. It also allows market sentiment, c, to affect η.

Since all banks are identical, I assume that the under-supplied asset (either cash or loans) is allocated through rationing at Day 1. That is, a needy bank can raise $\beta = \min\{\lambda, 1\}$ portion of its total liquidity shortage by selling loans (and raise the rest $1-\beta$ by premature liquidation), while a surplus bank can use $\gamma = \min\{\frac{1}{\lambda}, 1\}$ portion of its liquidity surplus to purchase loans (and leave the remaining $1-\gamma$ unused).

A.3 The social planner's problem

The society's aggregate liquidity shortage, δ, is: $\delta = p(t-\alpha) - (1-p)\alpha = pt - \alpha$. When $\delta > 0$, the society's overall liquidity holdings is insufficient to cover all liquidity needs at Day 1, and there will be at least one bank that needs to prematurely liquidate loans. When $\delta = 0$, all banks that have liquidity shortages can raise enough liquidity through selling loans to those with extra cash at Day 1, and there will be no extra cash in the economy after the transactions. When $\delta < 0$, there will be some extra cash left in the economy after all needy banks raise sufficient liquidity through interbank transactions. Liquidating loans generates a net cost to the society, while interbank transactions do not. Also, holding extra cash does not generate a profit while holding loans does. Therefore the socially optimal choice is $\delta = 0$, where no loans need to be liquidated and no extra cash is held after Day 1.

Formally,

$$\pi_{social} = \begin{cases} (1-pt+\delta)r - (\dfrac{\delta}{\theta} - \delta + \dfrac{\delta}{\theta}r), & \text{if } \delta \geq 0 \\ (1-pt+\delta)r, & \text{if } \delta < 0 \end{cases} \tag{A.3.1}$$

The right-hand side of the above equation can be interpreted in the following way: when $\delta \geq 0$, the society's overall profit equals the interest income that could be earned from loans held at Day 0, $(1-pt+\delta)r$, minus the loss from prematurely liquidating loans at Day 1, $(\frac{\delta}{\theta} - \delta + \frac{\delta}{\theta}r)$. The loss has two parts: 1) loss of principal, $\frac{\delta}{\theta} - \delta$, which is the cost of $\frac{\delta}{\theta}$ units of loans at Day 0 minus the δ cash received from liquidating these loans; 2) loss of interest income, $\frac{\delta}{\theta}r$, as $\frac{\delta}{\theta}$ units of the originally held loans cannot to be held to Day 2. When $\delta < 0$, the extra liquidity generates no profit, and the overall profit comes only from loans held at Day 0.

Take a partial derivative of π_{social} with regard to δ, we have:

$$\frac{\partial \pi_{social}}{\partial \delta} = \begin{cases} \dfrac{(1+r)(\theta-1)}{\theta} < 0, & \text{if } \delta \geq 0 \\ r > 0, & \text{if } \delta < 0 \end{cases} \tag{A.3.2}$$

Therefore the socially optimal choice is $\delta^* = 0$, or, $\alpha^* = pt$.

A.4 The individual bank's problem

Now I study the liquidity holdings decision of an individual bank, Bank A, given the aggregate liquidity holdings α. Bank A chooses its liquidity holdings, α_A, at Day 0. To focus on the more interesting case, I assume $\alpha_A \leq t$. The expected profit of Bank A at Day 0, is:

$$\begin{aligned} E[\pi_A] = {} & (1-\alpha_A)r - p(t-\alpha_A)\beta\frac{1+r-\eta}{\eta} \\ & - p(t-\alpha_A)(1-\beta)\frac{1+r-\theta}{\theta} + (1-p)\alpha_A\gamma\frac{1+r-\eta}{\eta} \end{aligned} \tag{A.4.1}$$

The four terms in the right-hand side of the above equation represent four components of Bank A's profit. The first term is the interest income the bank could gain if all its loans are held to Day 2. The second term is the expected loss from selling $(t - \alpha_A)\beta$ unit of loans to other banks. In particular, p is the probability of Bank A having t liquidity needs at Day 1. $(t - \alpha_A)\beta$ is the liquidity shortage that can be raised by selling loans to other banks. $\frac{1+r-\eta}{\eta}$ is the loss from raising 1 unit of cash in this way.

The third term is the expected loss from prematurely liquidating $(t - \alpha_A)(1 - \beta)$ unit of loans. The last term is the expected gain from purchasing loans using extra cash.

Bank A's problem is:

$$\begin{aligned} &\max_{\alpha_A} E[\pi_A] \\ s.t.\ &\alpha_A \in [0, 1] \\ &\frac{t - \alpha_A}{\eta}\beta + \frac{t - \alpha_A}{\theta}(1 - \beta) \leq 1 - \alpha_A \end{aligned} \tag{A.4.2}$$

The second condition above requires that the amount of loans used to exchange for cash should be no more than the total loan holding.

Taking the first derivative of $E[\pi_A]$ with regard to α_A, we have:

$$\frac{\partial E[\pi_A]}{\partial \alpha_A} = -r + p[\beta\frac{1 + r - \eta}{\eta} + (1 - \beta)\frac{1 + r - \theta}{\theta}] + (1 - p)\gamma\frac{1 + r - \eta}{\eta} \tag{A.4.3}$$

Given α, the sign of the above partial derivative depends on the realization of p:

When $p < \frac{\alpha}{tc}$, we have $\eta > 1$, $\beta = 1$, and $\gamma = \frac{1}{\lambda} < 1$. Therefore:

$$\begin{aligned} \frac{\partial E[\pi_A]}{\partial \alpha_A} &= -r + p\frac{1 + r - \eta}{\eta} + (1 - p)\gamma\frac{1 + r - \eta}{\eta} \\ &< -r + p\frac{1 + r - 1}{1} + (1 - p)1\frac{1 + r - 1}{1} \\ &= 0 \end{aligned} \tag{A.4.4}$$

When $p = \frac{\alpha}{2tc}$, we have $\eta = 1$, $\beta = 1$, and $\gamma = 1$. Therefore:

$$\begin{aligned} \frac{\partial E[\pi_A]}{\partial \alpha_A} &= -r + p\frac{1 + r - 1}{1} + (1 - p)1\frac{1 + r - 1}{1} \\ &= 0 \end{aligned} \tag{A.4.5}$$

When $p > \frac{\alpha}{2tc}$, we have $\eta < 1$, $\beta = \lambda < 1$, and $\gamma = 1$. Therefore:

$$\begin{aligned}
\frac{\partial E[\pi_A]}{\partial \alpha_A} &= -r + p[\lambda \frac{1+r-\eta}{\eta} + (1-\lambda)\frac{1+r-\theta}{\theta}] + (1-p)\frac{1+r-\eta}{\eta} \\
&> -r + p[\lambda \frac{1+r-\eta}{\eta} + (1-\lambda)\frac{1+r-\eta}{\eta}] + (1-p)\frac{1+r-\eta}{\eta} \\
&= -r + p\frac{1+r-\eta}{\eta} + (1-p)\frac{1+r-\eta}{\eta} \\
&= -(1+r) + \frac{1+r}{\eta} \\
&> 0
\end{aligned} \tag{A.4.6}$$

In sum, Equation A.4.3 can be written in the following form:

$$\frac{\partial E[\pi_A]}{\partial \alpha_A} \begin{cases} < 0, & \text{if } p < \dfrac{\alpha}{tc} \\ = 0, & \text{if } p = \dfrac{\alpha}{tc} \\ > 0, & \text{if } p > \dfrac{\alpha}{tc} \end{cases} \tag{A.4.7}$$

With that, we can get the (corner) solution for Bank A's optimization problem A.4.2:

$$\alpha_A \begin{cases} = 0, & \text{if } p < \dfrac{\alpha}{tc} \\ \in [0,1], & \text{if } p = \dfrac{\alpha}{tc} \\ = 1, & \text{if } p > \dfrac{\alpha}{tc} \end{cases} \tag{A.4.8}$$

It is easy to show that the above solution meets the two conditions in Bank A's problem A.4.2. First, note that the first condition is apparently met. Therefore I investigate the second condition:

When $p < \frac{\alpha}{tc}$ and $\alpha_A = 0$, the second condition simplifies to: $\frac{t}{\eta} \leq 1$. Since $\eta > 1$ in this case, the inequality holds.

When $p = \frac{\alpha}{tc}$ and $\alpha_A \in [0, 1]$, the second condition simplifies to $t - \alpha_A \leq 1 - \alpha_A$, which apparently holds.

When $p > \frac{\alpha}{tc}$ and $\alpha_A = 1$, the second condition simplifies to $\frac{t-1}{\eta}\lambda + \frac{t-1}{\theta}(1 - \lambda) \leq 1 - 1 = 0$. Since $t < 1$ and $\lambda < 1$, this inequality again holds.

The result in A.4.8 implies that, given the aggregate liquidity holdings, α, a bank is more likely to increase liquidity holdings, α_A, when p is larger. When $p < \frac{\alpha}{tc}$ ($p > \frac{\alpha}{tc}$), an individual bank is incentivized to under(over)-reserve liquidity, deviating from aggregate liquidity holdings level α. Only when $p = \frac{\alpha}{tc}$ could an equilibrium of $\alpha_A = \alpha = ptc$ exist. If the realization of p is perfectly known by banks at Day 0, then the equilibrium of $\alpha_A = \alpha = ptc$ is always achieved. This is because an individual bank always holds more (less) than ptc when aggregate liquidity, α, is smaller (greater) than ptc, driving α towards ptc. However, since p is a random variable to bank managers at Day 0, the relative magnitude of α versus ptc is uncertain for an individual bank at Day 0. Therefore the amount of liquidity holdings by an individual bank depends on bank manager's assumption about the value of p.

Because bank managers are conservative, they always assume the largest possible realization of p. Recall that p is uniformly distributed over $[\underline{p}, \overline{p}]$. Therefore bank managers assume $p = \overline{p}$ when making liquidity holdings decisions. Accordingly, we can rewrite Bank A's liquidity holdings decision in the following way:

$$\alpha_A \begin{cases} = 0, & \text{if } \overline{p} < \dfrac{\alpha}{tc} \\ \in [0, 1], & \text{if } \overline{p} = \dfrac{\alpha}{tc} \\ = 1, & \text{if } \overline{p} > \dfrac{\alpha}{tc} \end{cases} \tag{A.4.9}$$

Note that $\overline{p}$ is a constant, and it is known by bank managers at Day 0. This is similar to the case discussed above where p is perfectly known. Following the same argument, the liquidity holdings decision of all banks will reach a unique equilibrium:

$$\alpha_A^{**} = \alpha^{**} = \overline{p}tc \tag{A.4.10}$$

Benchmarked with the socially optimal liquidity holdings, $\alpha^* = pt$, the equilibrium holding, α^{**}, can be either too high or too low, depending on the values of $\overline{p}$ and c. Banks are more likely to under-reserve liquidity if the estimated upper bound of liquidity shock is low, i.e., low $\overline{p}$, and/or if the market is optimistic about liquidity condition, i.e., low c.

A.5 The role of liquidity disclosure

Banks' disclosure of their liquidity condition provides relevant information for all banks about the probability of a future liquidity shortage, p. Such disclosure makes bank managers at least weakly more certain about the value of p. Recall from Section 2.1 that $\overline{p}$ ($\underline{p}$) reflects the worst (best) scenarios of all uncertain factors affecting p. Therefore the increased certainty translates into a reduced number of uncertain factors that affect p, leading to a weakly reduced (increased) upper (lower) bound of p. Formally, the disclosure shrinks the distribution of p to $[p_L, p_H]$, where $\{p\} \subseteq [p_L, p_H] \subseteq [\underline{p}, \overline{p}]$.

Since disclosure weakly reduces $\overline{p}$ to p_H, banks' updated liquidity holdings after liquidity disclosure will be:

$$\alpha^{***} = p_H tc \leq \overline{p}tc = \alpha^{**} \tag{A.5.1}$$

In sum, liquidity disclosure weakly reduces banks' liquidity holdings. This is because 1) such disclosure reduces the uncertainty bank managers have about the probability of a liquidity shortage, p, and lowers the upper bound of p's uniform distribution, $\overline{p}$; and 2) bank managers are conservative, and they make liquidity holdings decisions based on $\overline{p}$. Lower $\overline{p}$ gives bank managers the confidence that the "worst case scenario" is not as bad as they thought before the disclosure, which enables them to lend out additional cash.

Note that my model cannot evaluate whether the reduction in liquidity holdings is good or bad at the social level. The reduced liquidity holdings ($\alpha^{***} = p_H tc$) could be either closer or further away from the socially optimal level ($\alpha^* = pt$) than the previous holding level ($\alpha^{***} = \overline{p}tc$), depending on the values of p, p_H, $\overline{p}$, and c.

A.6 Empirical prediction

To tie the above analysis to my empirical setting, I define the improvement in banks' certainty about future liquidity shock as:

$$\Delta Certainty \equiv \overline{p} - p_H \tag{A.6.1}$$

In practice, $\overline{p}$ should vary across banks, i.e., some banks are more informed about p (and have lower $\overline{p}$) than others. Disclosure levels the playing field in the sense that p_H is more homogeneous across banks. Naturally, this results in heterogeneous $\Delta Certainty$ across banks. Combining Equations A.5.1 and A.6.1, the relationship between certainty improvement, $\Delta Certainty$, and change in liquidity holdings, $\Delta\alpha$, is:

$$\Delta\alpha = \alpha^{***} - \alpha^{**} = -tc\Delta Certainty \tag{A.6.2}$$

This is my main empirical prediction. In other words, banks with greater improvement in their certainty about future liquidity shock are more likely to reduce liquidity holdings.

Appendix B. LCR regulations

Figure B1. Timeline of LCR regulations

This graph shows the timeline of LCR regulations in the US, including the disclosure regulation, and the sample periods used in the main empirical tests. Information used in the graph comes from Federal Register documents 79 FR 61440 and 81 FR 94922.

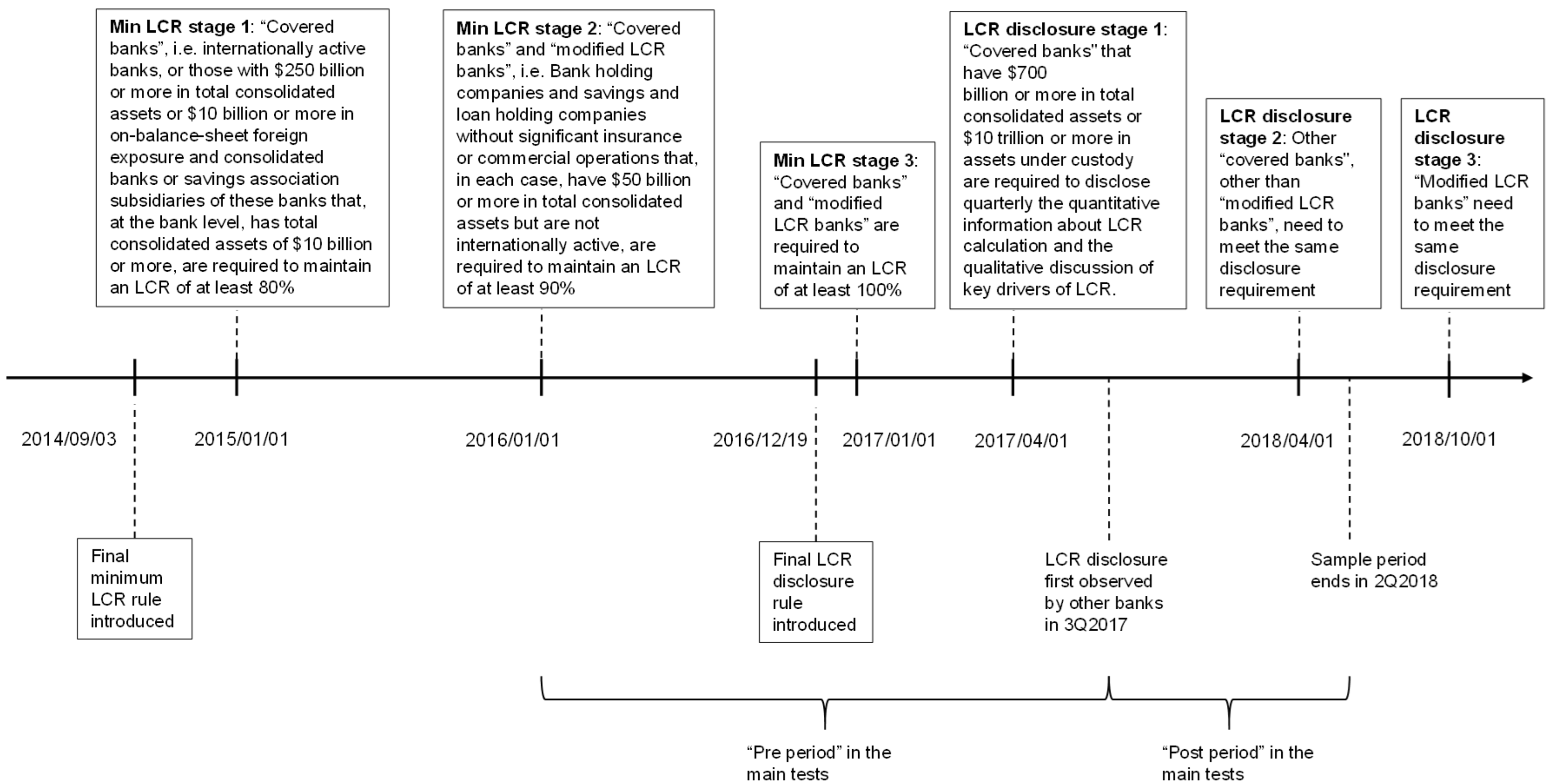


Figure B2. Examples of LCR disclosure

These two figures are examples of quantitative information and qualitative discussions included in mandatory LCR disclosures. The figures come from snapshots of JPMorgan's 2Q2017 LCR disclosure.

Panel A: Example of quantitative information

	Three months ended June 30, 2017 (in millions)	Average Unweighted Amount[a]	Average Weighted Amount[b]
	HIGH-QUALITY LIQUID ASSETS		
1	**Total eligible high-quality liquid assets (HQLA), of which:**[c]	$ 544,328	$ 540,785
2	Eligible level 1 liquid assets	520,713	520,713
3	Eligible level 2A liquid assets	23,611	20,070
4	Eligible level 2B liquid assets	4	2
	CASH OUTFLOW AMOUNTS		
5	**Deposit outflow from retail customers and counterparties, of which:**	$ 699,361	$ 43,959
6	Stable retail deposit outflow	424,595	12,738
7	Other retail funding outflow	251,035	26,430
8	Brokered deposit outflow	23,731	4,791
9	**Unsecured wholesale funding outflow, of which:**	**679,517**	**251,049**
10	Operational deposit outflow	472,594	117,870
11	Non-operational funding outflow	198,634	124,890
12	Unsecured debt outflow	8,289	8,289
13	**Secured wholesale funding and asset exchange outflow**[d]	**577,735**	**154,404**
14	**Additional outflow requirements, of which:**	**532,055**	**134,815**
15	Outflow related to derivative exposures and other collateral requirements	146,852	41,916
16	Outflow related to credit and liquidity facilities including unconsolidated structured transactions and mortgage commitments	385,203	92,899
17	**Other contractual funding obligation outflow**	**3,509**	**3,509**
18	**Other contingent funding obligations outflow**[e]	**283,122**	**10,033**
19	**TOTAL CASH OUTFLOW**	**$ 2,775,299**	**$ 597,769**
	CASH INFLOW AMOUNTS		
20	**Secured lending and asset exchange cash inflow**[d]	**$ 569,502**	**$ 122,803**
21	**Retail cash inflow**	**30,421**	**8,435**
22	**Unsecured wholesale cash inflow**[f]	**22,077**	**14,657**
23	**Other cash inflows, of which:**	**20,136**	**19,904**
24	Net derivative cash inflow	5,209	5,209
25	Securities cash inflow	1,908	1,908
26	Broker-dealer segregated account inflow	12,787	12,787
27	Other cash inflow	232	–
28	**TOTAL CASH INFLOW**	**$ 642,136**	**$ 165,799**
			Average Weighted Amount[b]
29	**HQLA AMOUNT**[c]		**$ 540,785**
30	**TOTAL NET CASH OUTFLOW AMOUNT EXCLUDING THE MATURITY MISMATCH ADD-ON**		**$ 431,970**
31	**MATURITY MISMATCH ADD-ON**		**37,294**
32	**TOTAL NET CASH OUTFLOW AMOUNT**		**$ 469,264**
33	**LIQUIDITY COVERAGE RATIO (%)**[c]		**115%**

(a) Represents the average notional amount of (1) eligible HQLA before applying regulatory-prescribed haircuts; and (2) balances subject to outflows and inflows over a prospective 30-day period before applying regulatory-prescribed outflow and inflow rates.
(b) Represents the average weighted amount after applying regulatory prescribed (1) HQLA haircuts; and (2) cash outflow and inflow rates, respectively.
(c) Excludes excess HQLA at JPMorgan Chase Bank N.A. and Chase Bank USA, N.A that are not transferable to non-bank affiliates.
(d) Outflows on line 13 predominantly relate to securities loaned or sold under repurchase agreements and collateralized deposits; these amounts are largely offset by inflows reported on line 20 from securities borrowed or purchased under resale agreements and margin loans. These amounts include outflows and inflows associated with certain prime brokerage activities.
(e) Predominantly reflects repurchases of the Firm's issued debt securities that mature more than 30 calendar days after the calculation date.
(f) Predominantly reflects repayments of wholesale loans.

Panel B: Example of qualitative discussions

High Quality Liquid Assets

As mentioned above, HQLA is the amount of liquid assets that qualify for inclusion in the LCR. HQLA primarily consist of unencumbered cash and certain high quality liquid securities as defined in the LCR rule.

For the three months ended June 30, 2017, the Firm's average HQLA was $540.8 billion. This amount excludes the amount of HQLA at JPMorgan Chase Bank, N.A. and Chase Bank USA, N.A in excess of each entity's standalone 100% minimum LCR requirements that is not transferable to non-bank affiliates within the Firm.

The following table presents the Firm's average HQLA included in the LCR broken out by HQLA-eligible cash and securities for the three months ended June 30, 2017.

Average weighted amount (in millions)		Three months ended June 30, 2017
HQLA		
Eligible cash[(a)]	$	377,868
Eligible level 1 securities[(b)(c)]		142,845
Total eligible Level 1 assets		**520,713**
Eligible level 2a securities[(c)(d)]		20,070
Eligible level 2b securities[(c)(e)]		2
Total HQLA	**$**	**540,785**

(a) Represents cash on deposit at central banks, primarily Federal Reserve Banks.
(b) Predominantly U.S. Treasuries, U.S government-guaranteed agency mortgage-backed securities ("MBS"), and sovereign bonds.
(c) HQLA eligible securities may be reported in securities borrowed or purchased under resale agreements, trading assets, or securities on the Firm's Consolidated balance sheets.
(d) Primarily U.S. government-sponsored enterprise agency MBS net of applicable haircuts under the LCR rules. Excludes excess level 2a securities held by JPMorgan Chase Bank, N.A.
(e) Substantially all the Firm's eligible corporate bonds and equity securities are considered excess HQLA held by JPMorgan Chase Bank, N.A., and as a result are excluded from the Firm's reported HQLA.

As of June 30, 2017, in addition to HQLA reported above, the Firm had approximately $233 billion of unencumbered marketable securities, such as equity securities and fixed income debt securities, available to raise liquidity, if required. This includes HQLA-eligible securities included as part of the excess liquidity at JPMorgan Chase Bank, N.A. that is not transferable to non-bank affiliates. The Firm also maintains borrowing capacity at various Federal Home Loan Banks ("FHLBs"), the Federal Reserve Bank discount window and various other central banks as a result of collateral pledged by the Firm to such banks. Although available, the Firm does not view the borrowing capacity at the Federal Reserve Bank discount window and the various other central banks as a primary source of liquidity. For additional information, see Liquidity Risk Management on pages 67-71 of the 2Q17 Form 10-Q.

Appendix C. Intuition of *LiqInfoImprove*

To better understand the measure $LiqInfoImprove$, let's consider three hypothetical cases. In Case 1, Bank A has 5 interactions in total and only 1 of which is with a covered bank, Bank C, while Bank B has 10 interactions in total and also only 1 of which is with a covered bank, Bank C. Since Bank C accounts for 20% (10%) of Bank A's (Bank B's) total interactions, it is sensible to believe that Bank A cares more and knows better about Bank C than Bank B does. Therefore the required disclosure from Bank C conveys more information to Bank B than to Bank A.

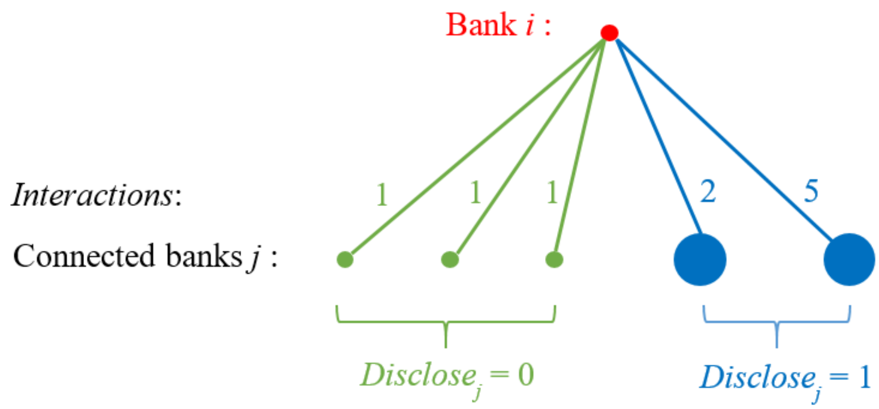
In Case 2, Bank A has 5 interactions in total, only 1 of which is with Bank C, while Bank B has 5 interactions in total, only 2 of which are with Bank C (or 1 with Bank C and 1 with another covered bank, Bank D). Since covered bank(s) account(s) for 20% (40%) of Bank A's (Bank B's) total interactions, Bank B should know more about covered banks in general than Bank A does, and thus the incremental disclosure from all covered banks provides more information to Bank A than Bank B.

In Case 3, Bank A has 5 interactions in total, only 1 of which is with Bank C, while Bank B has 10 interactions in total, only 2 of which are with Bank C (or 1 with Bank C and 1 with Bank D). While Bank B has more absolute connections with covered bank(s) than Bank A does, the percentage of interaction with covered bank(s) is the same for the two banks. Therefore their knowledge about covered banks' liquidity position should be similar before the required LCR disclosures, and thus the disclosures enhance their knowledge of aggregate liquidity to a similar extent.

Figures

Figure 1. Demonstration of *LiqInfoImprove* Calculation

This figure demonstrates the calculation of $LiqInfoImprove_i$, the measure of liquidity information improvement for Bank i, due to the implementation of LCR disclosure requirement. Since the required disclosures first appear in the quarterly reports of 2Q2017, Bank i experienced liquidity information improvement, i.e., $LiqInfoImprove_i$ became non-zero, starting from 3Q2017. $LiqInfoImprove_i$ equals 1 minus the $Interactions_{i,j}$ weighted average of $Disclose_j$, where $Interactions_{i,j}$ is the number of syndicated loans Banks i and j have issued together during 1Q2010-2Q2018, and $Disclose_j$ equals 1 if Bank j is required to disclose LCR information after 2Q2017.



$$LiqInfoImprove_{\,i,t=3Q17} = 1 - \frac{\sum_j Interactions_{i,j} * Disclose_j}{\sum_j Interaction_{i,j}}$$

$$= 1 - \frac{1*0+1*0+1*0+2*1+5*1}{1+1+1+2+5}$$

$$= 0.3$$

Figure 2. Network of bank interactions

This figure plots the network of bank co-syndication interactions for banks in the sample. Each node represents a bank, and a line connects two nodes whenever the two banks represented by the two nodes have a connection, i.e., the two banks have issued at least one syndicated loan during 1Q2010 – 2Q2018. The size of each node increases with the number of connections the bank represented by the node has with other banks. Nodes are partitioned and color coded based on their modularity class (i.e., nodes in the same class, or module, have more connections with each other than connections with nodes in other modules). The node layout is conducted by ForceAtlas2 algorithm, which clusters nodes with more connections and separates nodes with fewer connections. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan.

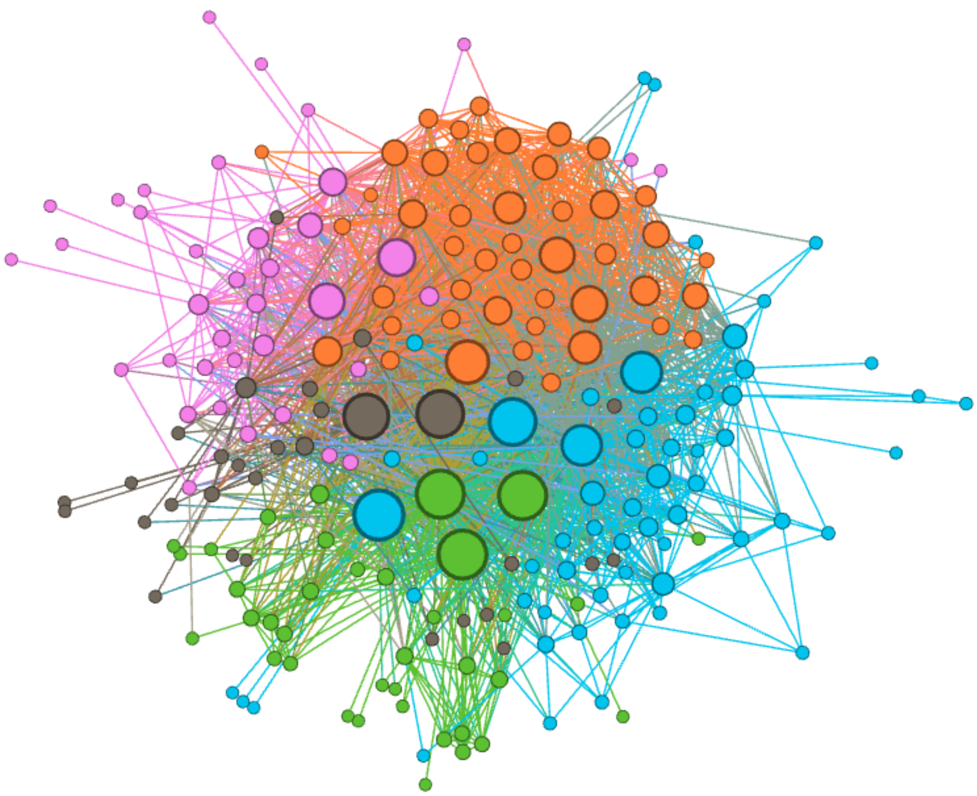


Figure 3. Banks with top 30 *Interactions*

This figure plots banks with top 30 *Interactions*. *Interactions* is the total number of syndicated loans a bank have issued with other banks during 1Q2010 – 2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan.

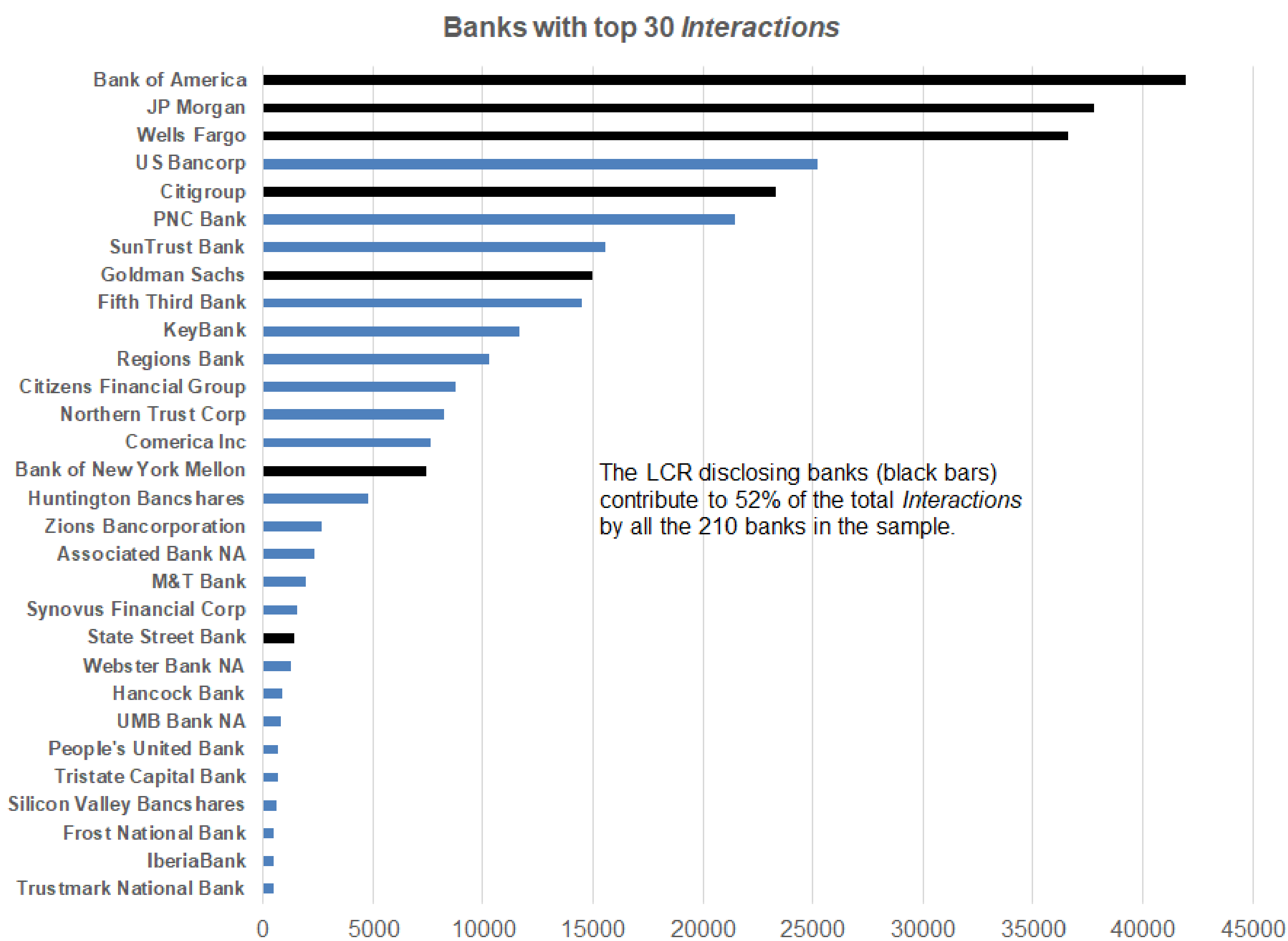
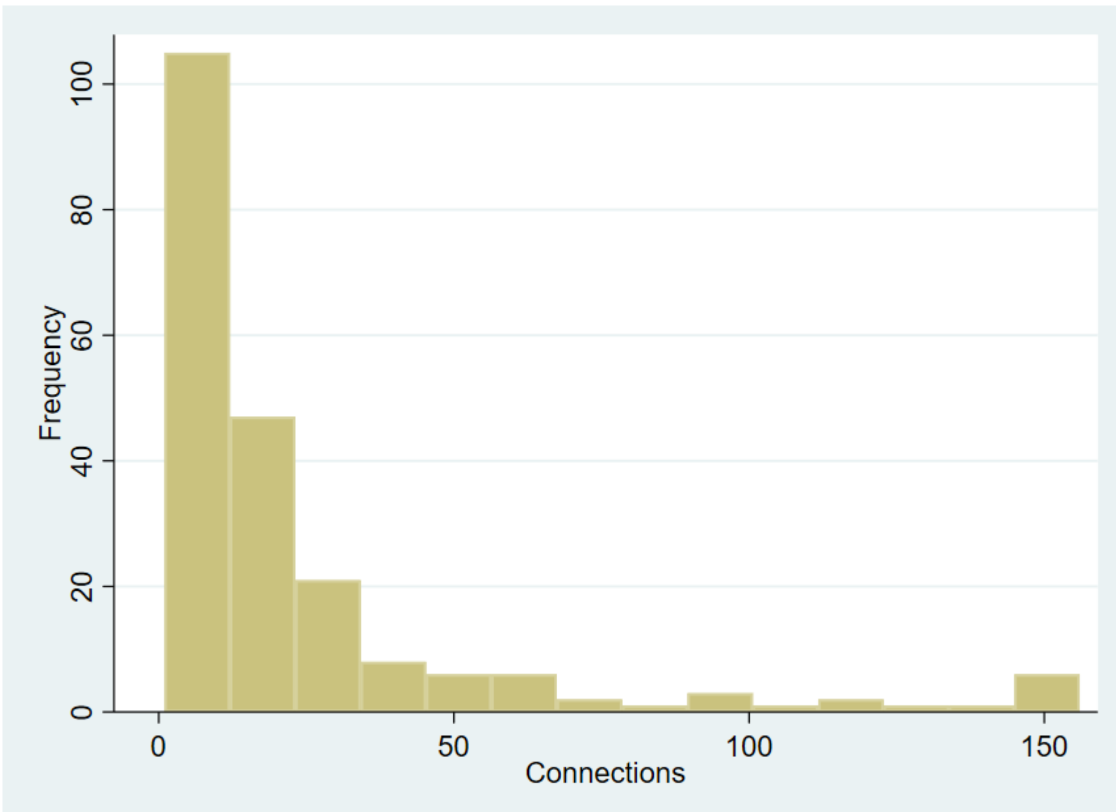
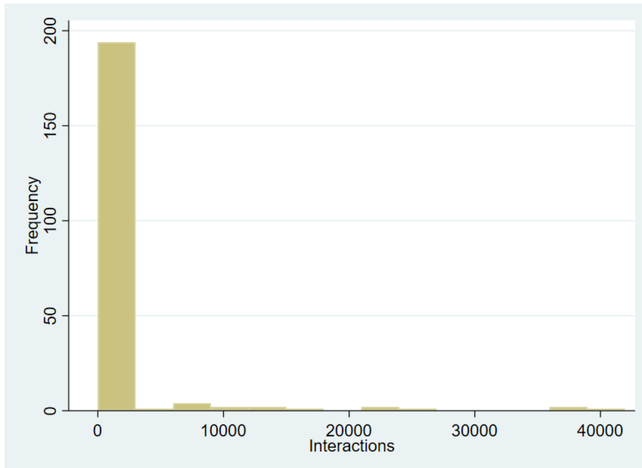


Figure 4. Distributions of bank interaction network variables
Connections is the total number of other banks a bank have issued syndicated loans with during 1Q2010 – 2Q2018. *Interaction* is the total number of syndicated loans a bank have issued with other banks during 1Q2010 – 2Q2018. $LiqInfoImprove$ is the degree of liquidity information improvement after the LCR disclosure requirement is implemented (i.e., Equation 3.2.1 when $t \geq 3Q17$). Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan.

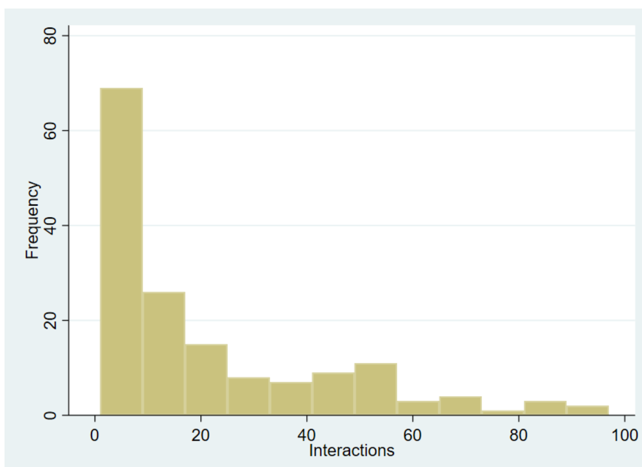
Panel A: Histogram of *Connections*



Panel B: Histogram of *Interactions*



Full sample



Interaction range: 1-100

Panel C: Histogram of $LiqInfoImprove$

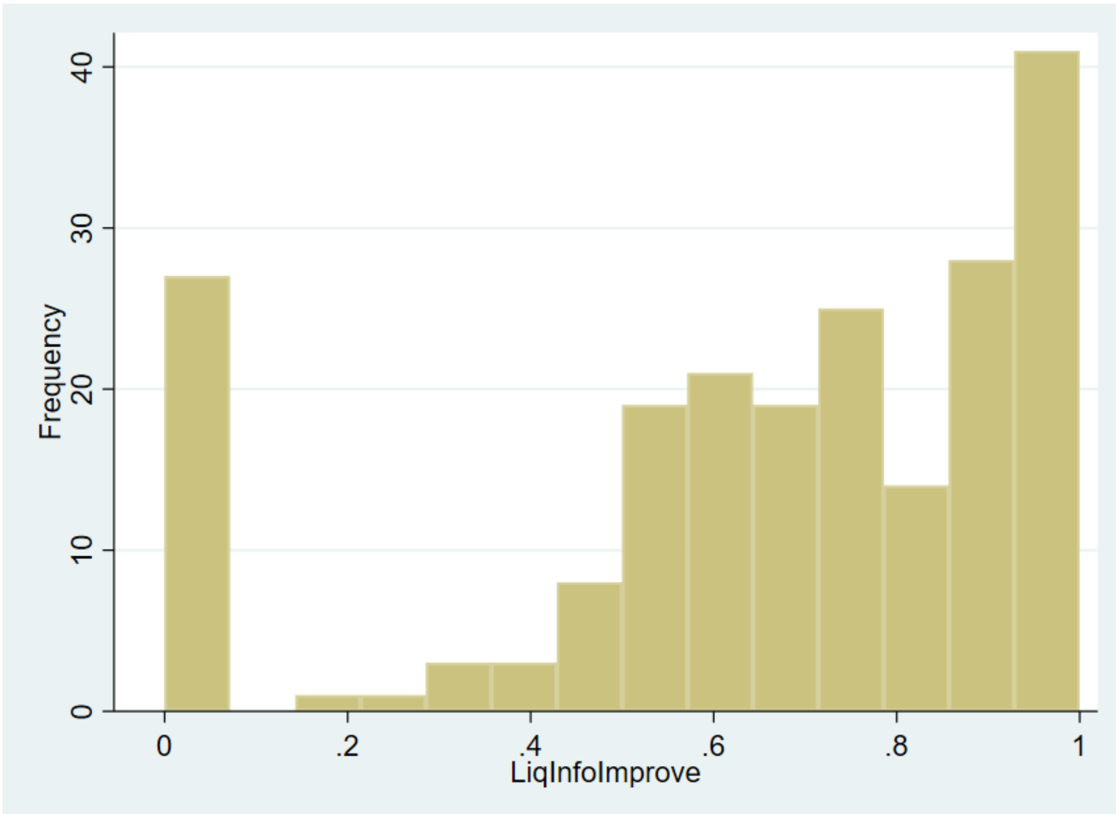
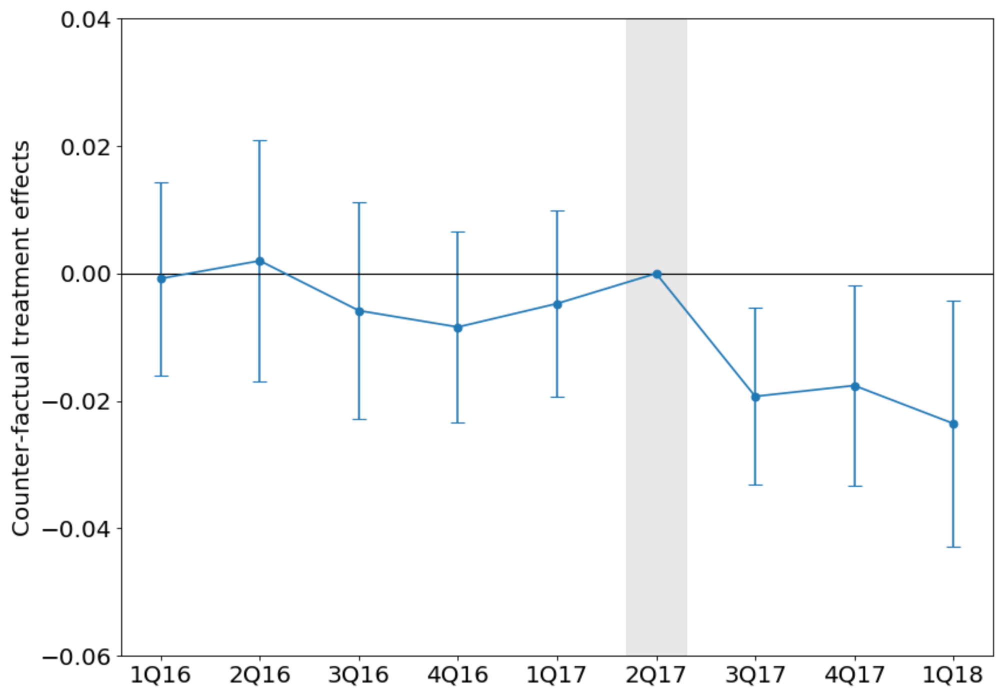


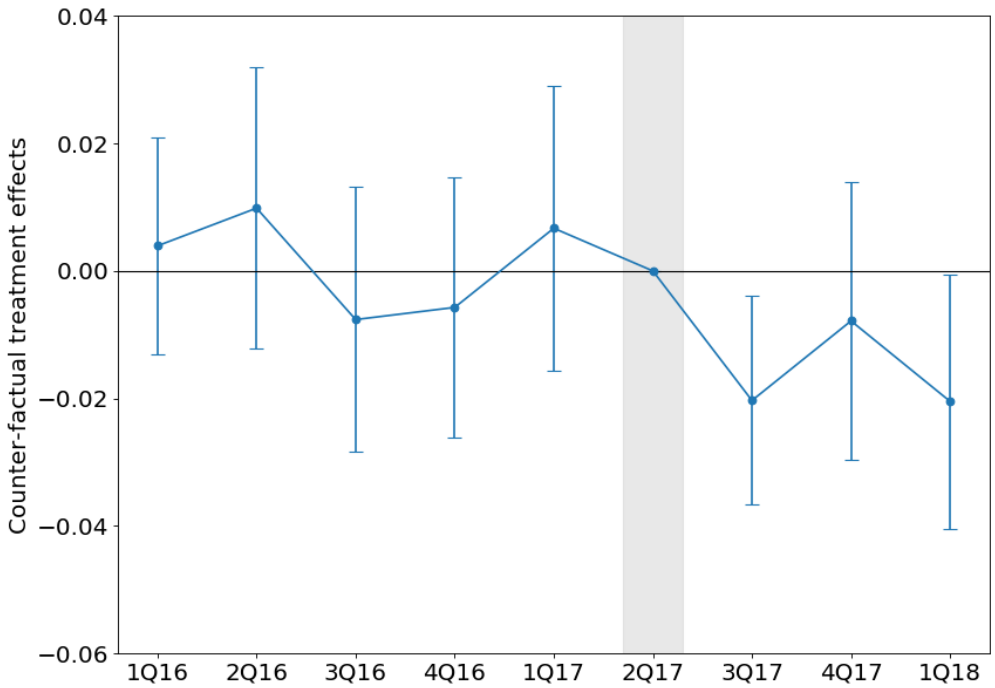
Figure 5. Pattern of the counter-factual treatment effects

This figure displays OLS regression coefficients and two-tailed 90% confidence intervals (based on standard errors clustered at the bank level) of a series of eight "*counter-factual LiqInfoImprove*", which replaces the single $LiqInfoImprove$ in model 3.4.1. The *counter-factual LiqInfoImprove* is the same as the original $LiqInfoImprove$ except that each of the eight *counter-factual LiqInfoImprove* assumes that the LCR disclosure is available only in each of the eight quarters from 1Q2016 to 2Q2018, excluding 2Q2017. This analysis shows the pattern in the *counter-factual treatment* effects relative to the quarter (2Q2017) immediately prior to the quarter when required LCR disclosure was first observed.

Panel A: Counter-factual treatment effects on liquidity ratio



Panel B: Counter-factual treatment effects on liquidity growth



Tables

Table 1. Descriptive statistics

Panel A: Bank characteristics

This table reports descriptive statistics for bank characteristics in the sample. *LiquidityRatio* is the ratio of liquid assets to total assets. $\Delta Liquidity$ is the change in liquid assets scaled by total assets at the beginning of the quarter. *IlliquidRatio* is the ratio of illiquid assets to total assets. *CoreDepositRatio* is the ratio of core deposits to total assets. *CapitalRatio* is the ratio of total equity to total assets. *CommitmentRatio* is the ratio of unused commitments to the sum of unused commitments and total assets. *Size* is the natural log of total assets. $\Delta Loan$ is the percentage change in total loans outstanding. NPL is non-performing loans scaled by lagged total loans. LLP is loan loss provision scaled by lagged total loans. ALL is the allowance of loan losses scaled by lagged total loans. NCO is net charge-offs scaled by lagged total loans. *Interest* is total interest income scaled by lagged total loans. EBP is earnings before provisions (net income plus loan loss provision) scaled by lagged total assets. The sample covers quarterly US bank holding companies for the sample period of 1Q2011-2Q2018. To be included, banks have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago.

	count	mean	sd	min	p25	p50	p75	max
LiquidityRatio	5772	0.160	0.117	0.010	0.077	0.124	0.205	0.687
ΔLiquidity	5556	0.001	0.028	-0.240	-0.010	0.000	0.011	0.280
IlliquidRatio	5772	0.747	0.132	0.000	0.707	0.788	0.832	0.934
CoreDepositRatio	5772	0.580	0.147	0.000	0.506	0.601	0.679	0.896
CapitalRatio	5772	0.109	0.048	-1.191	0.088	0.104	0.122	0.709
CommitmentRatio	5771	0.140	0.057	0.000	0.102	0.137	0.176	0.357
Size	5772	15.508	1.765	8.864	14.201	15.144	16.315	21.683
ΔLoan	5556	0.016	0.049	-0.772	0.001	0.010	0.021	1.078
NPL	5555	0.019	0.027	0.000	0.006	0.011	0.020	0.343
LLP	5551	0.001	0.003	-0.051	0.000	0.001	0.001	0.084
ALL	5556	0.015	0.009	0.000	0.010	0.013	0.018	0.130
NCO	5551	0.001	0.003	-0.037	0.000	0.000	0.001	0.081
Interest	5551	0.015	0.009	-0.140	0.012	0.014	0.016	0.186
EBP	5551	0.003	0.003	-0.039	0.002	0.003	0.003	0.059

Panel B: Bank interaction network

This table reports the descriptive statistics for bank interaction network variables. *Connections* is the total number of other banks a bank have issued syndicated loans with during 1Q2010 – 2Q2018. *Interactions* is the total number of syndicated loans a bank have issued with other banks during 1Q2010 – 2Q2018. *LiqInfoImprove* is the degree of liquidity information improvement after the LCR disclosure requirement is implemented (i.e., Equation 3.2.1 when $t \geq 3Q17$). Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan.

	(1)							
	count	mean	sd	min	p25	p50	p75	max
Connections	210	23.714	32.533	1.000	4.000	12.500	25.000	156.000
Interactions	210	1498.176	5753.142	1.000	6.000	21.500	97.000	41937.000
LiqInfoImprove	210	0.655	0.310	0.000	0.532	0.720	0.900	1.000

Panel C: Correlation of bank interaction network variables

This table reports the correlation matrix for bank interaction network variables at 2Q2018. See Panel B for variable definitions. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

	Connections	Interactions	LiqInfoImprove
Connections	1		
Interactions	0.814***	1	
LiqInfoImprove	-0.121	-0.147*	1

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2. Changes in liquidity holdings after LCR disclosure regulation

This table reports the estimates from the OLS regressions of banks' liquidity ratio and liquidity growth on the indicator variable *Post*. *Post* equals 1 starting from 3Q2017 and 0 otherwise. Columns 1 and 3 (Columns 2 and 4) in Panel A report results on LCR disclosing (non-disclosing) banks. Columns 1 and 3 (Columns 2 and 4) in Panel B report results on non-disclosing banks with top (bottom) 25% $LiqInfoImprove$ level. See Table 1 for other variable definitions. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2016-2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. t-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

Panel A: LCR disclosing vs. all banks

VARIABLES	(1) Liquidity Ratio_t (Disclosing)	(2) Liquidity Ratio_t (Non-disclosing)	(3) $\Delta\text{Liquidity}_t$ (Disclosing)	(4) $\Delta\text{Liquidity}_t$ (Non-disclosing)
Post_{t-1}	0.00720	-0.00307**	0.00887	-0.00371**
	(1.436)	(-2.332)	(0.952)	(-2.578)
$\text{IlliquidRatio}_{t-1}$	-0.644***	-0.422***	0.359	0.533***
	(-3.794)	(-7.357)	(1.136)	(8.725)
$\text{CoreDepositRatio}_{t-1}$	0.0286	-0.0344	0.0840	-0.126***
	(0.350)	(-0.899)	(0.585)	(-2.959)
$\text{CapitalRatio}_{t-1}$	-1.583**	-0.00101	-0.800	0.256*
	(-2.981)	(-0.00871)	(-1.124)	(1.753)
$\text{CommitmentRatio}_{t-1}$	-0.577	0.0549	0.0370	0.0667
	(-1.620)	(0.947)	(0.0569)	(1.267)
Size_{t-1}	-0.276***	-0.0262***	-0.452**	-0.0353***
	(-3.912)	(-3.920)	(-2.853)	(-3.841)
Observations	63	1,586	63	1,586
R-squared	0.991	0.975	0.356	0.296
Bank Fixed Effects	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18
Interactions Range	Full	Full	Full	Full

Panel B: Top vs. bottom 25% *LiqInfoImprove* banks

VARIABLES	(1) Liquidity Ratio_t (Top 25% LiqInfoImprove)	(2) Liquidity Ratio_t (Bottom 25% LiqInfoImprove)	(3) $\Delta \mathrm{Liquidity}_t$ (Top 25% LiqInfoImprove)	(4) $\Delta \mathrm{Liquidity}_t$ (Bottom 25% LiqInfoImprove)
Post_{t-1}	-0.00544**	-0.00140	-0.00896***	0.000255
	(-2.241)	(-0.609)	(-3.103)	(0.119)
$\mathrm{IlliquidRatio}_{t-1}$	-0.365***	-0.649***	0.669***	0.347***
	(-5.422)	(-6.576)	(8.110)	(4.504)
$\mathrm{CoreDepositRatio}_{t-1}$	-0.0782	0.00349	-0.158**	-0.123
	(-1.083)	(0.0302)	(-2.060)	(-1.138)
$\mathrm{CapitalRatio}_{t-1}$	-0.186	-0.00895	0.211	-0.0201
	(-1.230)	(-0.0315)	(1.261)	(-0.0794)
$\mathrm{CommitmentRatio}_{t-1}$	0.130**	0.0992	0.155**	0.108
	(2.333)	(0.701)	(2.505)	(1.121)
Size_{t-1}	-0.0233**	-0.0187	-0.0224	-0.0392**
	(-2.074)	(-1.313)	(-1.282)	(-2.517)
Observations	392	356	392	356
R-squared	0.963	0.987	0.354	0.243
Bank Fixed Effects	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18
Interactions Range	Full	Full	Full	Full

Table 3. The effect of LCR disclosure regulation on liquidity holdings

This table reports the estimates from the OLS regressions of non-disclosing banks' liquidity ratio and liquidity growth on the degree of their liquidity information improvement, with changes in controls from columns 1 to 5. See Table 1 for variable definitions. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2016-2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. *t*-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

Panel A: Effect on liquidity ratio

VARIABLES	(1) Liquidity Ratio_t	(2) Liquidity Ratio_t	(3) Liquidity Ratio_t	(4) Liquidity Ratio_t	(5) Liquidity Ratio_t
$\text{LiqInfoImprove}_{t-1}$	-0.0123*	-0.0127***	-0.0122	-0.0221*	-0.0178**
	(-1.673)	(-5.489)	(-0.221)	(-1.841)	(-2.055)
$\text{IlliquidRatio}_{t-1}$					-0.449***
					(-6.326)
$\text{CoreDepositRatio}_{t-1}$					0.0156
					(0.356)
$\text{CommitmentRatio}_{t-1}$					0.0599
					(0.946)
Size_{t-1}					-0.0236***
					(-2.979)
$\text{CapitalRatio}_{t-1}$					0.0607
					(0.423)
Observations	1,245	1,245	1,245	1,245	1,245
R-squared	0.002	0.970	0.003	0.971	0.977
Bank Fixed Effects	No	YES	No	YES	YES
Year-quarter Fixed Effects	No	No	YES	YES	YES
Clustering Level	NA	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18
Interactions Range	5-10000	5-10000	5-10000	5-10000	5-10000

Panel B: Effect on liquidity change

VARIABLES	(1) $\Delta\text{Liquidity}_t$	(2) $\Delta\text{Liquidity}_t$	(3) $\Delta\text{Liquidity}_t$	(4) $\Delta\text{Liquidity}_t$	(5) $\Delta\text{Liquidity}_t$
$\text{LiqInfoImprove}_{t-1}$	-0.00212	-0.00183	-0.0103**	-0.00990*	-0.0178**
	(-1.306)	(-1.553)	(-2.220)	(-1.874)	(-2.324)
$\text{IlliquidRatio}_{t-1}$					0.516***
					(6.909)
$\text{CoreDepositRatio}_{t-1}$					-0.0671
					(-1.328)
$\text{CommitmentRatio}_{t-1}$					0.0989
					(1.634)
Size_{t-1}					-0.0238**
					(-2.454)
$\text{CapitalRatio}_{t-1}$					0.404**
					(2.501)
Observations	1,245	1,245	1,245	1,245	1,245
R-squared	0.001	0.076	0.037	0.111	0.310
Bank Fixed Effects	No	YES	No	YES	YES
Year-quarter Fixed Effects	No	No	YES	YES	YES
Clustering Level	NA	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18
Interactions Range	5-10000	5-10000	5-10000	5-10000	5-10000

Table 4. The effects of LCR disclosure regulation vs. minimum LCR regulation
This table reports the estimates from the OLS regressions of non-disclosing banks' liquidity ratio and liquidity growth on the degree of their liquidity information improvement and on the increases of LCR requirements. LCR_n are the counter-factual treatment variables, which are constructed in the same way as $LiqInfoImprove$, except for assuming that the LCR disclosures were available starting from quarter n, where $n = 1Q15$, $1Q16$, *or* $1Q17$. See Table 1 for the definitions of other variables. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2014-2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. t-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

Panel A: Effects on liquidity ratio

VARIABLES	(1) Liquidity Ratio_t	(2) Liquidity Ratio_t	(3) Liquidity Ratio_t	(4) Liquidity Ratio_t	(5) Liquidity Ratio_t
$\text{LiqInfoImprove}_{t-1}$	-0.0178**				-0.0175***
	(-2.055)				(-2.611)
LCR_1Q15_{t-1}		-0.0106			-0.00632
		(-1.220)			(-0.839)
LCR_1Q16_{t-1}			-0.0101		-0.00814
			(-1.464)		(-1.391)
LCR_1Q17_{t-1}				-0.00731	0.00406
				(-0.924)	(0.613)
$\text{IlliquidRatio}_{t-1}$	-0.449***	-0.429***	-0.464***	-0.391***	-0.622***
	(-6.326)	(-7.919)	(-6.561)	(-5.848)	(-13.45)
$\text{CoreDepositRatio}_{t-1}$	0.0156	0.00587	0.0429	0.0152	0.0617*
	(0.356)	(0.138)	(1.126)	(0.348)	(1.940)
$\text{CommitmentRatio}_{t-1}$	0.0599	-0.0594	-0.0118	0.0364	0.0169
	(0.946)	(-0.723)	(-0.187)	(0.520)	(0.319)
Size_{t-1}	-0.0236***	-0.0221	-0.0255**	-0.0278***	-0.0126
	(-2.979)	(-1.422)	(-1.995)	(-3.257)	(-1.584)
$\text{CapitalRatio}_{t-1}$	0.0607	-0.433***	0.149	0.120	-0.364***
	(0.423)	(-4.304)	(1.440)	(0.859)	(-2.838)
Observations	1,244	1,126	1,089	1,105	2,369
R-squared	0.977	0.972	0.977	0.979	0.965
Bank Fixed Effects	YES	YES	YES	YES	YES
Year-quarter Fixed Effects	YES	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q14-4Q15	1Q15-4Q16	1Q16-4Q17	1Q14-2Q18
Interactions Range	5-10000	5-10000	5-10000	5-10000	5-10000

Panel B: Effects on liquidity change

VARIABLES	(1) ΔLiquidity$_{t}$	(2) ΔLiquidity$_{t}$	(3) ΔLiquidity$_{t}$	(4) ΔLiquidity$_{t}$	(5) ΔLiquidity$_{t}$
LiqInfoImprove$_{t-1}$	-0.0178**				-0.0194**
	(-2.324)				(-2.392)
LCR_1Q15$_{t-1}$		-0.0144			-0.00922
		(-1.639)			(-1.307)
LCR_1Q16$_{t-1}$			-0.00477		-0.00206
			(-0.604)		(-0.342)
LCR_1Q17$_{t-1}$				-0.00472	0.0102
				(-0.476)	(1.104)
IlliquidRatio$_{t-1}$	0.516***	0.440***	0.523***	0.592***	0.257***
	(6.909)	(7.012)	(6.676)	(7.299)	(6.448)
CoreDepositRatio$_{t-1}$	-0.0671	-0.0908*	-0.0398	-0.0749	-0.0518**
	(-1.328)	(-1.873)	(-0.916)	(-1.365)	(-2.063)
CommitmentRatio$_{t-1}$	0.0989	-0.0551	-0.0141	0.0756	0.0400
	(1.634)	(-0.549)	(-0.182)	(1.007)	(0.788)
Size$_{t-1}$	-0.0238**	-0.0652***	-0.0340**	-0.0277**	-0.0213***
	(-2.454)	(-3.350)	(-2.490)	(-2.326)	(-2.992)
CapitalRatio$_{t-1}$	0.404**	0.115	0.527***	0.447**	0.182***
	(2.501)	(0.758)	(3.641)	(2.344)	(3.050)
Observations	1,244	1,080	1,041	1,057	2,270
R-squared	0.310	0.286	0.339	0.339	0.184
Bank Fixed Effects	YES	YES	YES	YES	YES
Year-quarter Fixed Effects	YES	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q14-4Q15	1Q15-4Q16	1Q16-4Q17	1Q14-2Q18
Interactions Range	5-10000	5-10000	5-10000	5-10000	5-10000

Table 5. The effect of lead arrangers' LCR disclosure

This table reports estimates from the OLS regressions of non-disclosing banks' liquidity ratio and liquidity growth on the degree of their liquidity information improvement coming from lead arrangers' LCR disclosure. $LiqInfoImprove\_LeadArranger$ is defined in Equation 5.6.1. See Table 1 for definitions of other variables. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2016-2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. t-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

VARIABLES	(1) Liquidity Ratio_t	(2) $\Delta\mathrm{Liquidity}_t$
$\mathrm{LiqInfoImprove\_LeadArranger}_{t-1}$	-0.0272**	-0.0244**
	(-2.120)	(-2.149)
$\mathrm{IlliquidRatio}_{t-1}$	-0.449***	0.516***
	(-6.303)	(6.891)
$\mathrm{CoreDepositRatio}_{t-1}$	0.0145	-0.0690
	(0.329)	(-1.367)
$\mathrm{CapitalRatio}_{t-1}$	0.0608	0.403**
	(0.422)	(2.489)
$\mathrm{CommitmentRatio}_{t-1}$	0.0602	0.0970
	(0.967)	(1.608)
Size_{t-1}	-0.0241***	-0.0244**
	(-3.020)	(-2.486)
Observations	1,244	1,244
R-squared	0.977	0.310
Bank Fixed Effects	YES	YES
Year-quarter Fixed Effects	YES	YES
Clustering Level	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18
Interactions Range	5-10000	5-10000

Table 6. Changes in co-synidcation relationship after LCR disclosure regulation

This table reports the estimates from the OLS regressions of banks' co-synidcation interactions with the LCR disclosing banks on the indicator variable *Post*. *Post* equals 1 starting from 3Q2017 and 0 otherwise. *LCR Interaction* is defined in Equation 5.7.1. Column 1 (Column 2) reports results on LCR disclosing (non-disclosing) banks. Column 3 (Column 4) reports results on non-disclosing banks with top (bottom) 25% *LiqInfoImprove* level. See Table 1 for other variable definitions. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2016-2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. t-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

VARIABLES	(1) LCR Interaction_t (Disclosing)	(2) LCR Interaction_t (Non-disclosing)	(3) LCR Interaction_t (Top 25% LiqInfoImprove)	(4) LCR Interaction_t (Bottom 25% LiqInfoImprove)
Post_{t-1}	-0.0250	-0.0329***	0.00295	-0.0579**
	(-0.464)	(-3.549)	(1.098)	(-2.423)
$\text{IlliquidRatio}_{t-1}$	1.861*	0.122	0.0102	0.930
	(2.077)	(0.538)	(0.132)	(1.495)
$\text{CoreDepositRatio}_{t-1}$	-1.061	-0.132	-0.00430	-0.258
	(-1.222)	(-0.832)	(-0.0642)	(-0.424)
$\text{CapitalRatio}_{t-1}$	-6.955	0.255	0.143	-0.313
	(-0.991)	(0.535)	(0.576)	(-0.324)
$\text{CommitmentRatio}_{t-1}$	1.072	-0.0118	0.0211	-0.718
	(0.482)	(-0.0404)	(0.242)	(-0.681)
Size_{t-1}	-0.895	-0.0363	-0.0144	-0.0879
	(-1.807)	(-1.001)	(-0.689)	(-0.815)
Observations	63	1,587	393	356
R-squared	0.329	0.573	0.166	0.575
Bank Fixed Effects	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18
Interactions Range	Full	Full	Full	Full

Table 7. The effect of LCR disclosure regulation on co-syndication relationship
This table reports the estimates from the OLS regressions of non-disclosing banks' co-synidcation interactions with the LCR disclosing banks on the degree of their liquidity information improvement and on the increases of LCR requirements. *LCR Interaction* is defined in Equation 5.7.1. *LCR_n* are the counter-factual treatment variables, which are constructed in the same way as *LiqInfoImprove*, except for assuming that the LCR disclosures were available starting from quarter n, where $n = 1Q15,\ 1Q16,\ or\ 1Q17$. See Table 1 for the definitions of other variables. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2014-2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. t-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

VARIABLES	(1) LCR Interaction_t	(2) LCR Interaction_t	(3) LCR Interaction_t	(4) LCR Interaction_t	(5) LCR Interaction_t
$\text{LiqInfoImprove}_{t-1}$	0.179***				0.192**
	(2.920)				(2.514)
LCR_1Q15_{t-1}		0.112*			0.118*
		(1.891)			(1.967)
LCR_1Q16_{t-1}			0.0203		0.0246
			(0.351)		(0.406)
LCR_1Q17_{t-1}				0.00586	-0.0374
				(0.118)	(-0.613)
$\text{IlliquidRatio}_{t-1}$	0.176	-0.0301	0.452	0.142	0.230*
	(0.618)	(-0.164)	(1.039)	(0.447)	(1.761)
$\text{CoreDepositRatio}_{t-1}$	-0.306	0.150	0.0850	-0.300	-0.0877
	(-1.492)	(0.702)	(0.369)	(-1.352)	(-0.747)
$\text{CommitmentRatio}_{t-1}$	-0.180	0.563	0.173	0.0927	0.127
	(-0.490)	(1.296)	(0.412)	(0.241)	(0.534)
Size_{t-1}	-0.0246	-0.0275	0.0875	-0.0570	0.00241
	(-0.539)	(-0.396)	(0.853)	(-1.027)	(0.0743)
$\text{CapitalRatio}_{t-1}$	0.312	0.158	-0.652	0.00866	0.263
	(0.492)	(0.368)	(-0.772)	(0.0112)	(0.941)
Observations	1,244	1,141	1,089	1,105	2,384
R-squared	0.514	0.525	0.536	0.538	0.483
Bank Fixed Effects	YES	YES	YES	YES	YES
Year-quarter Fixed Effects	YES	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q14-4Q15	1Q15-4Q16	1Q16-4Q17	1Q14-2Q18
Interactions Range	5-10000	5-10000	5-10000	5-10000	5-10000

Table 8. Robustness to changing range of total interactions for sample banks

This table reports the estimates from the OLS regressions of non-disclosing banks' liquidity ratio and liquidity growth on the degree of their liquidity information improvement, with changes in the range of *Interactions* within which banks are included in the sample. See Table 1 for variable definitions. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2016-2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. *t*-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

Panel A: Effect on liquidity ratio

VARIABLES	(1) Liquidity $\mathrm{Ratio_t}$	(2) Liquidity $\mathrm{Ratio_t}$	(3) Liquidity $\mathrm{Ratio_t}$	(4) Liquidity $\mathrm{Ratio_t}$
$\mathrm{LiqInfoImprove}_{t-1}$	-0.00498	-0.0178**	-0.0181*	-0.0158*
	(-0.861)	(-2.055)	(-1.933)	(-1.847)
$\mathrm{IlliquidRatio}_{t-1}$	-0.430***	-0.449***	-0.452***	-0.379***
	(-7.200)	(-6.326)	(-6.146)	(-5.087)
$\mathrm{CoreDepositRatio}_{t-1}$	-0.0258	0.0156	0.0222	0.00796
	(-0.638)	(0.356)	(0.491)	(0.170)
$\mathrm{CommitmentRatio}_{t-1}$	0.0642	0.0599	0.0740	0.0455
	(1.140)	(0.946)	(1.121)	(0.641)
Size_{t-1}	-0.0253***	-0.0236***	-0.0229***	-0.0208***
	(-3.490)	(-2.979)	(-2.846)	(-2.650)
$\mathrm{CapitalRatio}_{t-1}$	0.0186	0.0607	0.0551	0.189
	(0.161)	(0.423)	(0.377)	(1.268)
Observations	1,586	1,244	1,163	893
R-squared	0.976	0.977	0.973	0.972
Bank Fixed Effects	YES	YES	YES	YES
Year-quarter Fixed Effects	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18
Interactions Range	Full	5-10000	5-1000	5-100

Panel B: Effect on liquidity ratio

VARIABLES	(1) $\Delta\text{Liquidity}_t$	(2) $\Delta\text{Liquidity}_t$	(3) $\Delta\text{Liquidity}_t$	(4) $\Delta\text{Liquidity}_t$
$\text{LiqInfoImprove}_{t-1}$	-0.00730	-0.0178**	-0.0187**	-0.0212***
	(-1.229)	(-2.324)	(-2.295)	(-2.832)
$\text{IlliquidRatio}_{t-1}$	0.531***	0.516***	0.509***	0.597***
	(8.498)	(6.909)	(6.566)	(6.879)
$\text{CoreDepositRatio}_{t-1}$	-0.108**	-0.0671	-0.0653	-0.0480
	(-2.329)	(-1.328)	(-1.229)	(-0.861)
$\text{CommitmentRatio}_{t-1}$	0.0796	0.0989	0.102	0.0408
	(1.526)	(1.634)	(1.596)	(0.611)
Size_{t-1}	-0.0313***	-0.0238**	-0.0235**	-0.0214**
	(-3.170)	(-2.454)	(-2.397)	(-1.989)
$\text{CapitalRatio}_{t-1}$	0.279*	0.404**	0.390**	0.535***
	(1.922)	(2.501)	(2.372)	(3.149)
Observations	1,586	1,244	1,163	893
R-squared	0.319	0.310	0.307	0.347
Bank Fixed Effects	YES	YES	YES	YES
Year-quarter Fixed Effects	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18	1Q16-2Q18
Interactions Range	Full	5-10000	5-1000	5-100

Table 9. Robustness to changing sample period

This table reports the estimates from the OLS regressions of non-disclosing banks' liquidity ratio and liquidity growth on the degree of their liquidity information improvement, with changes in the sample period. See Table 1 for variable definitions. The sample covers quarterly US bank holding companies for the sample period of 1Q2011-2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. *t*-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

Panel A: Effect on liquidity ratio

	(1)	(2)	(3)	(4)
VARIABLES	Liquidity Ratio_t	Liquidity Ratio_t	Liquidity Ratio_t	Liquidity Ratio_t
$\text{LiqInfoImprove}_{t-1}$	-0.0178**	-0.0187**	-0.0200**	-0.0281***
	(-2.055)	(-2.096)	(-2.300)	(-2.647)
$\text{IlliquidRatio}_{t-1}$	-0.449***	-0.591***	-0.623***	-0.668***
	(-6.326)	(-11.35)	(-13.40)	(-13.27)
$\text{CoreDepositRatio}_{t-1}$	0.0156	0.0391	0.0604*	0.0799**
	(0.356)	(1.182)	(1.897)	(2.453)
$\text{CommitmentRatio}_{t-1}$	0.0599	0.0306	0.00862	-0.00564
	(0.946)	(0.610)	(0.164)	(-0.0933)
Size_{t-1}	-0.0236***	-0.0257***	-0.0129	-0.00503
	(-2.979)	(-4.116)	(-1.629)	(-0.597)
$\text{CapitalRatio}_{t-1}$	0.0607	0.0665	-0.364***	-0.425***
	(0.423)	(0.622)	(-2.848)	(-3.772)
Observations	1,244	1,788	2,369	3,908
R-squared	0.977	0.972	0.965	0.949
Bank Fixed Effects	YES	YES	YES	YES
Year-quarter Fixed Effects	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q15-2Q18	1Q14-2Q18	1Q11-2Q18
Interactions Range	5-10000	5-10000	5-10000	5-10000

Panel B: Effect on liquidity change

VARIABLES	(1) ΔLiquidity$_t$	(2) ΔLiquidity$_t$	(3) ΔLiquidity$_t$	(4) ΔLiquidity$_t$
LiqInfoImprove$_{t-1}$	-0.0178**	-0.0180**	-0.0156**	-0.0135**
	(-2.324)	(-2.359)	(-2.268)	(-2.156)
IlliquidRatio$_{t-1}$	0.516***	0.377***	0.257***	0.155***
	(6.909)	(7.520)	(6.610)	(6.997)
CoreDepositRatio$_{t-1}$	-0.0671	-0.0554*	-0.0546**	-0.0616***
	(-1.328)	(-1.688)	(-2.266)	(-3.078)
CommitmentRatio$_{t-1}$	0.0989	0.0703	0.0408	0.0121
	(1.634)	(1.363)	(0.844)	(0.343)
Size$_{t-1}$	-0.0238**	-0.0188**	-0.0220***	-0.0151***
	(-2.454)	(-2.373)	(-3.129)	(-3.619)
CapitalRatio$_{t-1}$	0.404**	0.365***	0.183***	0.0710*
	(2.501)	(3.165)	(3.168)	(1.951)
Observations	1,244	1,788	2,369	3,908
R-squared	0.310	0.245	0.185	0.120
Bank Fixed Effects	YES	YES	YES	YES
Year-quarter Fixed Effects	YES	YES	YES	YES
Clustering Level	Bank	Bank	Bank	Bank
Sample Period	1Q16-2Q18	1Q15-2Q18	1Q14-2Q18	1Q11-2Q18
Interactions Range	5-10000	5-10000	5-10000	5-10000

Table 10. Potential bank characteristics that drive *LiqInfoImprove*

This table reports the estimates from the OLS regressions of *LiqInfoImprove* on a series of bank characteristics. See Table 1 for variable definitions. The sample used in this test covers quarterly US bank holding companies in the period of 3Q2017-2Q2018, when *LiqInfoImprove* became non-zero. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. *t*-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

	(1)	(2)	(3)
VARIABLES	LiqInfoImprove	LiqInfoImprove	LiqInfoImprove
IlliquidRatio	0.192***	0.114	0.217
	(2.792)	(0.735)	(1.362)
CoreDepositRatio	-0.00814	-0.000964	-0.0300
	(-0.144)	(-0.0166)	(-0.508)
CapitalRatio	-0.194	-0.261	-0.162
	(-1.532)	(-1.534)	(-0.889)
CommitmentRatio	-0.0426	-0.0431	-0.0782
	(-0.307)	(-0.310)	(-0.551)
Size	-0.0621***	-0.0629***	-0.0619***
	(-11.76)	(-11.50)	(-10.90)
LiquidityRatio		-0.0901	-0.00176
		(-0.563)	(-0.0105)
ΔLiquidity		-0.0584	0.0636
		(-0.178)	(0.189)
ΔLoan			0.0683
			(0.458)
NPL			-0.654
			(-1.057)
LLP			-4.985
			(-0.445)
ALL			-1.735
			(-1.242)
NCO			-7.782
			(-0.725)
Interest			3.820**
			(2.190)
EBP			-0.908
			(-0.311)
Observations	562	561	561
R-squared	0.228	0.228	0.245
Sample Period	3Q17-2Q18	3Q17-2Q18	3Q17-2Q18
Interactions Range	5-10000	5-10000	5-10000

Table 11. Robustness to additional control variables

This table reports the estimates from the OLS regressions of non-disclosing banks' liquidity ratio and liquidity growth on the degree of their liquidity information improvement, with additional control variables. See Table 1 for variable definitions. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2016-2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. *t*-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

VARIABLES	(1) Liquidity Ratio_t	(2) $\Delta\text{Liquidity}_t$
$\text{LiqInfoImprove}_{t-1}$	-0.0165**	-0.0189**
	(-1.978)	(-2.549)
$\text{IlliquidRatio}_{t-1}$	-0.455***	0.519***
	(-6.463)	(7.091)
$\text{CoreDepositRatio}_{t-1}$	0.0123	-0.0651
	(0.259)	(-1.189)
$\text{CapitalRatio}_{t-1}$	0.0862	0.424**
	(0.589)	(2.505)
$\text{CommitmentRatio}_{t-1}$	0.0565	0.0976
	(0.960)	(1.634)
Size_{t-1}	-0.0296***	-0.0269**
	(-3.437)	(-2.518)
Interest_{t-1}	0.160	0.153
	(0.834)	(0.716)
EBP_{t-1}	-0.739	-0.516
	(-0.852)	(-0.506)
Observations	1,240	1,240
R-squared	0.978	0.340
Control Size*Time	YES	YES
Control IntRate*Time	YES	YES
Control EBP*Time	YES	YES
Control Illiquidity*Time	YES	YES
Bank Fixed Effects	YES	YES
Year-quarter Fixed Effects	YES	YES
Clustering Level	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18
Interactions Range	5-10000	5-10000

Table 12. Robustness to redefining *LiqInfoImprove* using size-weighted *Interactions*

This table reports estimates from the OLS regressions of non-disclosing banks' liquidity ratio and liquidity growth on the degree of their size-weighted liquidity information improvement, $LiqInfoImprove\_SizeWeighted$, which is defined in Equation 5.8.1. See Table 1 for definitions of other variables. The sample used in this test covers quarterly US bank holding companies in the period of 1Q2016-2Q2018. Banks in the sample have to have participated at least one syndicated loan issuance with other banks during 1Q2010 – 2Q2018. Syndicated loan data is obtained from Thomson-Reuters' LPC DealScan. Banks' quarterly fundamentals are obtained from FR Y-9C reports from the websites of Federal Reserve Bank of Chicago. t-statistics are in parentheses. ***, **, * indicate statistical significance at less than 1, 5, and 10%, respectively.

	(1)	(2)
VARIABLES	Liquidity $\mathrm{Ratio}_{\mathrm{t}}$	$\Delta\mathrm{Liquidity}_{\mathrm{t}}$
$\mathrm{LiqInfoImprove\_SizeWeighted}_{t-1}$	-0.0156*	-0.0156**
	(-1.950)	(-2.183)
$\mathrm{IlliquidRatio}_{t-1}$	-0.451***	0.515***
	(-6.336)	(6.885)
$\mathrm{CoreDepositRatio}_{t-1}$	0.0146	-0.0681
	(0.333)	(-1.350)
$\mathrm{CapitalRatio}_{t-1}$	0.0603	0.403**
	(0.420)	(2.496)
$\mathrm{CommitmentRatio}_{t-1}$	0.0585	0.0974
	(0.921)	(1.606)
Size_{t-1}	-0.0238***	-0.0240**
	(-3.022)	(-2.478)
Observations	1,244	1,244
R-squared	0.977	0.309
Bank Fixed Effects	YES	YES
Year-quarter Fixed Effects	YES	YES
Clustering Level	Bank	Bank
Sample Period	1Q16-2Q18	1Q16-2Q18
Interactions Range	5-10000	5-10000