Credit Risk, Liquidity, and Lies†

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Abstract

We examine the relative effects of credit risk and liquidity in the interbank market using bank-level panel data on Libor submissions and CDS spreads, allowing for the possibility that Libor-submitting firms may strategically misreport their funding costs. We find that interbank spreads were very sensitive to credit risk at the peak of the crisis. However, liquidity premia constitute the bulk of those spreads on average, and Federal Reserve interventions coincide with improvements in liquidity at short maturities. Accounting for misreporting, which is large at times, is important for obtaining these results.

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1 Introduction

Bank funding markets came under extraordinary pressure during the financial crisis that began in 2007, threatening both the stability of important financial institutions and the functioning of the financial system as a whole. Because of the critical role of these markets in financial stability and monetary transmission, central banks around the world responded with a number of emergency measures. These included both efforts to inject funds directly into short-term markets, intended to relieve *liquidity* strains, and efforts to ensure the solvency of the banking system, intended to reduce perceptions of *credit risk*. In this paper, we empirically investigate the extent to which these two factors drove interbank pressures during the crisis in order to shed light on the functioning of this market and, by extension, the potential efficacy of policy interventions.

We add to the literature investigating the relative importance of credit risk and liquidity by exploiting cross-bank and term-structure dimensions of the data. The richness of our large panel dataset greatly increases the power and flexibility of our identification relative to pure time-series approaches, such as Taylor and Williams (2009). In particular, it allows us to examine differences in liquidity conditions across maturities and changes over time in the degree to which credit risk passes through to interbank spreads. Our main finding is that, although interbank spreads were very sensitive to counterparty credit risk at the height of the crisis, liquidity premia constitute the bulk of those spreads on average. Furthermore, Federal Reserve interventions designed to bolster funding markets coincided with improvements in liquidity at short maturities. These results help to reconcile papers like Afonso et al. (2011) and Angelini et al. (2011), which ascribe a large importance to credit risk during the crisis, with papers like Acharya and Merrouche (2013), McAndrews et al. (2017), and Schwarz (2018), which emphasize liquidity hoarding and the effectiveness of central bank liquidity programs. We find that both credit risk and liquidity have been important in different contexts.

We measure bank funding costs using individual banks' submissions to the London Interbank Offer Rate (Libor) panel across a spectrum of maturities over the period 2007 - 2013, and we measure the same banks' credit risk using credit default swaps (CDS) written on their debt. A key innovation of our paper is to match these two data sources as closely as possible to each other while taking care to address the various sources of measurement error that arise at this level of granularity. For instance, the five-year CDS spread is commonly used in the literature to measure interbank credit risk because of that contract's superior liquidity, but five-year credit risk may differ substantially from the short-term credit risk that is relevant for interbank contracts. We use all available CDS quotes on each day to estimate the entire term structure of credit risk, allowing us to match the maturity of each bank's reported funding costs exactly while still incorporating as much information as possible. A separate measurement issue concerns the well publicized attempts of panel banks to manipulate the Libor data during this time. Previous studies in this liquidity vs. credit risk literature that have used Libor as a measure of funding costs have largely ignored this complication, potentially biasing their results. We construct a model that accounts for misreporting incentives, and build it into our estimation to control for and measure these effects.

To identify the time-varying liquidity and credit-risk components of interbank spreads across maturities, we apply state-space methods to our panel data. We calculate how large and volatile each of the two components was and show how they changed with market conditions, including Federal Reserve interventions. Over our sample, the estimated liquidity premium constitutes the majority of the funding spread (on average) at most maturities. It tends to be larger at longer maturities than at short maturities, consistent with Gorton et al. (2014), who argue that lenders in money markets shifted from long to short maturities during the crisis. During the time that Federal Reserve liquidity facilities were in force, our measures of liquidity premia dropped significantly for the maturities at which those programs were targeted. For example, when the Term Auction Facility (TAF), which primarily extended 28-day loans, expanded rapidly in 2008, our one-month liquidity premium plunged by over 100 basis points, but liquidity premia at longer maturities did not decline. We do find that credit risk has been important at times; for example, it accounted for most of the spike in interbank spreads around the failure of Lehman Brothers in September 2008 and most of the decline in spreads after the Federal Reserve's bank "stress tests" in May 2009. But for much of the subsequent sample the sensitivity of funding costs to CDS spreads was close to zero. Consequently, on average this component makes up only about a fifth of the aggregate interbank spread.

Although the aggregate Libor rate is perhaps the most widely used interest-rate benchmark in the world, the underlying micro data have been relatively unexploited by researchers. Instead, in previous bank-level studies, the most common method for measuring rates paid by banks to each other is to infer them from funds-transfers in payments-systems records, as in Furfine (1999). However, recent research suggests that the payments-based procedure may suffer from significant measurement error in some situations (see Armantier and Copeland, 2012 and also Kovner and Skeie, 2013). In addition, that approach cannot be applied when no transactions occur, as was frequently the case in term funding markets during the period we study. A key benefit of the Libor data is that, in principle, they provide the only source of information about banks' shadow borrowing costs when no borrowing is actually taking place.

As noted above, however, these data do come with complications. As is now well known, at least some of the Libor-reporting banks frequently lied about their borrowing rates either for reputational reasons or in an attempt to influence the direction of the market for financial gain.[1] Because the incentives to misreport can be related to credit risk, accounting for misreporting is necessary to obtain unbiased estimates of the credit and liquidity factors in the model. To do this, we build on recent work regarding how banks choose to report in the Libor survey (Youle, 2014, Snider and Youle, 2012, Chen, 2013, Bonaldi, 2017, Gandhi et al., 2018). We find that the effect of misreporting varies across time, maturities, and banks, but we estimate that it was most pronounced—biasing the aggregate rate downward by as much as 35 basis points—during the height of the crisis. Given our use of bank-level data, controlling for these misreporting effects turns out to be important. A version of the model that sets the misreporting terms to zero produces results that indicate credit risk plays little to no role interbank spreads, in conflict with the findings of Afonso et al. (2011) and others. Furthermore, we find that this alternative model is also strongly rejected by the data in favor of our baseline model that does give a role to misreporting.

Our paper contributes to the growing literature on the behavior of the interbank market during the crisis and the policy responses to its breakdown. In one of the first and best known of these studies, Taylor and Williams (2009) argue that wide interbank spreads mostly reflected

[1] See Hou and Skeie (2014) and Duffie and Stein (2015) for overviews.

counterparty credit risk among borrowing institutions, not a lack of liquidity in the market, and that consequently efforts by central banks to boost market liquidity were essentially useless. While a number of subsequent papers support this general conclusion, a separate set of studies has taken the opposite position, arguing that funding stress has primarily been a liquidity problem, not a credit-risk problem.[2] Our results show that both credit risk and liquidity played important roles at different times and different maturities, and we obtain these findings in the context of arguably better measures of both credit risk and borrowing costs. Our finding that sensitivity to credit risk varies significantly over time complements Afonso et al. (2011) by confirming (with much different data) that attention to credit risk in the interbank market increased in the immediate aftermath of the Lehman Brothers default. However, as noted above, we also find that this attention eventually returned to negligible levels.[3] Finally, our results may help to assess theoretical models of the interbank market in which credit risk and liquidity play a role, such as Eisenschmidt and Tapking (2009), Heider et al. (2015), and Acharya and Skeie (2011).

Three previous papers have used the cross-sectional aspects of the Libor data to study bank funding costs. Filipović and Trolle (2013) and Christensen et al. (2014) exploit variation in Libor in the maturity dimension, but they impose no-arbitrage cross-equation restrictions that we relax. (Given the dysfunction in funding markets during much of the sample, the no-arbitrage assumption seems strong.) Gefang et al. (2011) use Libor variation across both banks and maturities, but they do not match individual CDS and Libor spreads as we do. Rather, they model credit risk as a single unobserved factor driving both Libor and CDS, with different loadings at each bank. Thus, their identification comes mostly from the time-series component of the data and does not fully exploit the within-bank correlations. Moreover, all three papers employ single factors for credit and liquidity that affect all maturities proportionally. Our approach allows for different factors at each maturity, which turns out to be particularly important for the liquidity results.

[2] Afonso et al. (2011), Angelini et al. (2011), and Smith (2012), like Taylor and Williams, emphasize the prominence of credit risk in these markets, and Brunetti et al. (2011) provide additional evidence that central bank interventions were ineffective. In contrast, Gefang et al. (2011) and Schwarz (2018) estimate a large liquidity premium in interbank spreads; Acharya and Merrouche (2013) document liquidity hoarding by large settlement banks; and Wu (2011), Rai (2013), Christensen et al. (2014), and McAndrews et al. (2017) all find that central bank liquidity facilities significantly reduced bank funding rates.

[3] Angelini et al. (2011) also present evidence that lending banks may have paid more attention to credit risk at some times during the crisis than at others.

2 Matching Libor and CDS Data

U.S. Dollar Libor is calculated based on a survey of a panel of banks in North America, Europe and Japan that—during the period we examine—was conducted daily by the British Bankers Association.[4] Broadly speaking, the panel consists of the largest global banks that are active in dollar funding markets. The survey question is: "At what rate could you borrow funds, were you to do so by asking for and then accepting inter-bank offers in a reasonable market size just prior to 11 am?" Every day, this question is answered by a respondent at each of the panel banks for ten maturities (up to one year).[5] During our sample, the individual bank-level responses were made public at the same time that the reference rate was published.

The primary data in this paper consist of the bank-level Libor submissions and matched CDS spreads on a subset of the dollar-Libor-panel banks. We follow standard practice and subtract OIS rates, matched by maturity, from each bank's Libor quote on each day to remove the short-rate expectations and term-premium components associated with the risk-free rate.[6] We label the difference between Libor quotes and OIS rates as "LOIS" spreads.

2.1 Using CDS as a Proxy for Credit Risk

As in prior literature that investigates the credit and liquidity components of short-term funding costs, we use CDS spreads to proxy for credit risk. Some of the earlier papers in this literature (e.g. Taylor and Williams, 2009) use 5-year CDS spreads alongside short-term LOIS rates. The idea that 5-year CDS proxy well for credit risk comes from the more active primary and secondary market for this tenor. For example, Culp et al. (2016) show that single-name CDS with one to five years remaining to maturity accounted for the largest proportion of CDS outstanding over our sample period. However, while the approach of using five-year CDS draws information from the segment of the CDS market with the greatest liquidity, it also accepts a mismatch in maturity

[4]Responsibility for the administration of LIBOR was handed over to Intercontinental Exchange Benchmark Administration Ltd on January 31, 2014. For more information, see `https://www.theice.com/iba/libor`.

[5]The composition of the survey panel varies across currencies and changes over time. The aggregate value that is published as "the" Libor reference rate for each currency at each maturity on each day is calculated as the trimmed mean of the survey responses, where the trimming excludes the 25% highest and 25% lowest submissions rounded to the nearest integer number of respondents.

[6]We obtain dollar-denominated CDS quotes for the senior debt of the banks from Markit and the Libor and OIS data from Bloomberg.

between funding rates and CDS spreads that may introduce measurement error relative to true short-term credit risk. Other papers do match the maturities of CDS and interbank rates more closely, but this may come at the cost of potential measurement error in individual short-term credit risk, as short-term CDS contracts are more thinly traded.[7] Although some evidence suggests that the relative liquidity of short-term CDS may have improved over time, their low trading volumes imply that short-maturity CDS contracts, in isolation, may provide a noisier signal of short-term credit risk than five-year contracts do.[8]

Our approach is to fit a Nelson and Siegel (1987) curve to the full term structure of each bank's CDS. These curves utilize all of the available CDS data, including quotes from the most-liquid portion of the market (the 5-year maturity). Our CDS data provider, Markit, requires multiple, fresh quotes in order to post a CDS spread for any given firm-maturity combination on a specific day. Bearing this in mind, we build the term structure under the following constraints:

1. The Nelson-Siegel curve is fitted weighing the CDS data by inverse maturity. That is, the penalty for poor fit at the 30-year tenor is $1/60^{th}$ the size of the penalty at 6 months.
2. We only build CDS curves for bank-day combinations for which we have *both* 6- and 12-month CDS quotes for that bank.[9]
3. As bad fit of the Nelson-Siegel curve may be indicative of a day in which CDS is a particularly poor proxy of actual credit risk, we eliminate any observations where the CDS curve fitting error were large, choosing a cut-off of 25% of the CDS spread. Such days may be due to poor CDS liquidity (see e.g. Hu et al., 2013; Musto et al., 2018), or represent other conditions that could drive a wedge between CDS spread and credit risk, and their removal helps assure that our results are not driven by observations not well approximated by our methods.[10]

Given that we use the information from all tenors to construct our daily term structure curves and exclude any days for which 6- or 12-month CDS quotes are unavailable (days which were left

[7] Eisenschmidt and Tapking (2009), Filipović and Trolle (2013), Sul (2015), McAndrews et al. (2017), and Schwarz (2018) all make use of 6-month and/or 1-year CDS spreads to measure the credit risk of the banks in their studies alongside short-term funding rates.

[8] Culp et al. (2016) show that, over the period of our sample, CDS with 1-year-or-less remaining maturity become a more significant portion of the single-name outstanding. See Exhibit 7 and the related discussion in that paper.

[9] This eliminates about 12% of the sample.

[10] This resulted in the elimination of roughly 4% of the underlying data.

blank by Markit due to unavailable or stale quotes), our CDS curve should mitigate concerns about illiquidity-induced measurement error to the extent possible. Therefore, in our baseline model, we take exact maturity-matched reads off of these curves as our measures of short-term credit risk. We then offer a variety of tests to verify robustness to this approach.[11]

It is important to note a feature of the Markit CDS data, which is used in most studies in this literature, including ours. These data are quotes obtained from dealers, who are the primary CDS market-makers, and likely reflect a mix of transactions- and model-based information. To the extent that dealers report model-based—as opposed to transaction-based—quotes (in the absence of trading), there exists the potential to introduce noise into our proxy of credit risk that could affect our results. The size of this potential noise is a function of two things: the relative quantities of transactions-based quotes versus model-based quotes, and the presence of any systematic divergence between these quote "categories" over our sample. We do not know the magnitude of the effect this may have on the underlying results in this literature, including our paper, but to the extent that the data in model- and transaction-based quotes diverged systematically and/or persistently, our results on the contribution of credit risk to interbank spreads could be slightly misstated, with the direction and magnitude of misspecification depending on the properties of the divergence.[12] To the extent that the noise induced by model-based quotes was instead more "two-sided" and idiosyncratic, thereby more a function of CDS market liquidity, the impact should be detectable by examining how CDS market liquidity affects our results. We conduct robustness tests for this in Section 5.3.2.

[11] We thank the referee for raising issues which helped to lead to the filtering procedures outlined here. It is important to clarify that we fit a separate curve to the data available for each bank on each day and do not rely upon historical correlations in any way when constructing these curves. That is, every bank gets a completely new CDS curve on every day, based on the quotes that are reported only on that day. Usually there are 11 quotes on each day for each bank, covering maturities of 6 months to 30 years. See the online appendix for more detail.

[12] One potential example of a systematic distortion: dealers could post model-based quotes that are generally more conservative compared to executed transactions. If model-based quotes represent the offer side of the bid-ask spread which is prone to widening quickly in response to market volatility, for example, these model-based quotes may also move up faster at times when the conservative dealer sees the risk as having changed (i.e., the bid-offer spread widens when CDS spreads rise). If model-based quotes were a dominant fraction of the total data, and they consistently behave as we speculate here, they could potentially mute our perceived sensitivity of interbank lending markets to credit risk, or influence our measures of its time-variation. This type of measurement error also demonstrates the importance of basing the credit risk component off of information using as much of the maturity spectrum as possible, allowing us to improve the likelihood that we are using some transaction-based information for each bank-day observation. Finally, we note again that this issue is one confronted by all users of CDS data at a single-name level.

Finally, while the procedure to filter CDS data ensures that the credit risk proxy used in the analysis is as free as we can make it of the effects of CDS liquidity, it removes credit risk data completely for those bank-day observations. To the extent that there is correlation between CDS market liquidity and the broader credit risk and liquidity discussion which is the focus of the paper, this removal could bias our results. We are able to provide something of a test for filter-generated bias by running the analysis with and without the filter in step 3 above. We find that the filter does not fundamentally alter our results.

We start the sample in August 2007. Prior to that date, there was virtually no interesting variation in either LOIS or CDS spreads. Moreover, the CDS data become sparse as we go back further in time. We stop the sample in June 2013 because of the significant changes to the Libor procedures (i.e. the elimination of same-day reporting of the underlying submissions) instituted at that time. Over the sample period, 21 different banks participated in the panel, which contained 16 to 20 banks at any given time. We have Libor quotes for every bank that was in the panel on each day during this period. The CDS data, on the other hand, are sometimes missing because of the rules imposed by Markit regarding quote quality discussed above. The state space methods used in our estimation allow us to estimate parameters regardless of occasional missing CDS data.[13]

2.2 Empirical Features of Libor and CDS Spreads

To illustrate some key features of these data, Figure 1 plots the 1-week and 12-month LOIS spreads and (Nelson-Siegelized) CDS quotes, respectively. In each case, the solid lines show the cross-sectional average on each day, and the dashed lines show the range across banks. Although we do not use them in our subsequent analysis, we show the data prior to August 2007 to illustrate the magnitude of the structural break that occurs at the beginning of our sample.

The LOIS and CDS data have some clear commonalities. Relative to the post-2007 sample, both series are much closer to zero with little cross-sectional variation prior to August 2007. After that time, the average levels of both series rise significantly, and the dispersion widens. We see increases in both series after the default of Lehman Brothers in September 2008, as well as smaller increases around the times of tensions in Europe in mid-2010 and late 2011. These comovements

[13] The online appendix contains a more detailed discussion of the data coverage and sources.

might suggest that at least part of the reason for the movement in LOIS has to do with the same counterparty credit-risk factors that are driving CDS spreads.

However, there are also some important differences between the two series. Most significantly, Figure 1a shows that the term spread for LOIS widens substantially in the post-2007 period—the difference between the 1-week and 12-month series rises to over 150 basis points in 2009—whereas Figure 1b shows that the term spread in CDS remains close to zero, at least on average. Thus, at least at this superficial level, it does not seem that credit risk can fully account for the widening of both short- and long-term LOIS spreads. In addition, there are distinct differences in the timing and relative magnitudes of movements in the two series. For example, the jump in LOIS spreads in August 2007 is quite abrupt, but in CDS spreads it is more gradual; after the collapse of Lehman, both spreads widen, but the LOIS spread peaks relatively quickly, in October 2008, while the average CDS spread peaks in mid-2009. During the European crisis in 2011, CDS spreads widen to about the same levels that they had reached in the aftermath of Lehman, but LOIS spreads widen by a much smaller amount than they had in 2008. Finally, while the cross-sectional variation of the LOIS spreads increases somewhat after August 2007, the increase in the dispersion of the CDS spreads is far more dramatic. For example, the coefficient of variation for the 12-month CDS spread averages 0.5 after that date, while that for the corresponding LOIS spread averages just 0.1.

Despite the co-movement between the aggregate LOIS and CDS time series, the cross-sectional correlation between LOIS and CDS spreads is weak at best. Daily cross-sectional correlations between LOIS and CDS spreads average less than 0.15 at all maturities over our sample.[14] This pattern is puzzling because interbank lenders to a given bank and the writers of CDS contracts on that bank face losses in the same states of the world. In the United States, for example, the National Depositor Preference Act of 1993 treats both bonds and interbank loans as "general or senior liabilities," which stand together in the same place in line in the event of insolvency (after depositors but before subordinated debtholders) and receive the same *pro rata* recovery rate.[15] This strict correspondence may be muddled by variations in failure-resolution statutes across countries and in corporate structure across institutions. Nevertheless, a bank's default on one claim is nearly

[14]Michaud and Upper (2008) also note the weak cross-sectional correlation between CDS and Libor quotes.
[15]See Federal Deposit Insurance Act Section 11(d).

always associated with defaults on other claims, particularly those of similar maturity, so we would still generally expect the correlation between matched LOIS and CDS spreads to be very high. Furthermore, it seems particularly odd that the spreads should not be correlated in the cross section given the strong correlation that the aggregate series display over time. On its face, that observation would seem to suggest that lending banks demand a premium to compensate them for the aggregate default probability of the banking sector but not for the default probabilities of the particular banks they are lending to.

It is tempting to appeal to information asymmetries to explain these discrepancies, as in Heider et al. (2015). But this cannot be the whole story. Interbank lenders generally have access to contemporaneous CDS quotes when negotiating lending terms, and, during our sample period, CDS dealers also had access to contemporaneous bank-level Libor submissions. Indeed, many of the lending banks in the interbank market are the banks that are also making markets in CDS, and the same information sets should thus be priced into both instruments. For example, the Libor panel contains 11 of the G-14 dealer banks, which were collectively responsible for about 80% of all CDS activity during our sample period.[16] An alternative possibility, considered below, is that at least part of the discrepancy between LOIS and CDS spreads might be due to banks not reporting their true funding costs in the Libor survey.

3 Modeling Credit Risk, Liquidity, and Misreporting

In this section we develop a model of Libor determination to motivate and interpret our empirical tests. We first discuss our generalization of the standard empirical interbank-modeling framework to a panel context and our interpretation of the reduced-form parameters in that framework. We then extend the model to incorporate misreporting incentives.

3.1 "Fundamental" Determinants of Interbank Costs

We begin by extending the basic time-series framework that has been used in many previous studies of interbank funding costs, following Taylor and Williams (2009), to incorporate cross-sectional heterogeneity in the bank and maturity dimensions. Specifically, let L_{imt} be the spread of the interbank rate paid by bank i in time t for a loan of maturity m, relative to the risk-free rate

[16] See Chen et al. (2011).

at the same maturity. Let C_{imt} be a measure of the counterparty credit risk for the same bank, at the same time, at the same maturity (i.e., the default risk on that bank's borrowings at horizon m). In our case, L_{imt} will be measured as the bank-level Libor-OIS spread, and C_{imt} as the CDS spread. We posit that each bank's interbank borrowing rate is determined as follows:

$$L_{imt} = \lambda_{mt} + \phi_{imt} C_{imt} \tag{1}$$

where λ_{mt} is a maturity-specific liquidity premium, and ϕ_{imt} is a measure of interbank sensitivity to credit risk. λ_{mt} and ϕ_{imt} will be the central objects of interest in our estimation. By definition, λ_{mt} does not vary across banks—it is a market-wide liquidity premium that reflects the scarcity of funds at a given point in time. In principle, ϕ_{imt} could vary across both banks and maturities, but we argue below that it is *a priori* reasonable to restrict it to vary only in the time dimension. That is still a generalization of previous specifications, which have typically assumed this parameter to be constant over time.

Following the previous empirical literature, the key identifying assumption of equation (1) is that the liquidity premium λ_{mt} is not bank-specific. This reflects the observation that, although individual banks' liquidity needs may differ, the price that they pay for liquidity in equilibrium should primarily depend upon the aggregate demand for and supply of reserves. To be more precise, consider a stylized environment with a set of I risk-neutral banks, each of which at time-t chooses to hold a quantity of reserves x_{it} and each of which can borrow or lend without constraint at maturity m in the interbank market. Assume that the interbank market is competitive and denote each bank's gross borrowing rate R_{imt}. Assume that the aggregate supply of reserves is fixed by the central bank and that reserves pay the instantaneous gross risk-free rate R^f_{0t}. The m-period risk-free rate is

$$R^f_{mt} = \frac{1}{m} \int_0^m E_t[R^f_{0t+s}] ds. \tag{2}$$

Banks have some incentive to hold reserves because each bank faces a random cash outflow during the period over which its interbank loans are outstanding. Let this outflow be distributed with a density f_{imt} that possibly varies across time, maturities, and banks. If the cash outflow exceeds the reserves that the bank has chosen to hold in any period, the bank pays a (possibly bank-specific) cost k_{it}. This cost could reflect the cost of asset fire-sales, penalties associated with borrowing from the central bank, or, in the extreme, bankruptcy.[17] Under these assumptions, the expected return on an m-maturity interbank loan made by bank i is $R^f_{mt} + k_{it}f_{imt}(x_{it})$, where the term $k_{it}f_{imt}(x_{it})$ is the expected marginal cost of a cash shortfall. Since banks are risk-neutral and the market is competitive, expected returns must be equal across all banks. Thus, the equilibrium allocation of reserves solves $k_{it}f_{imt}(x_{it}) = k_{jt}f_{jmt}(x_{jt})$ for all i, j. The corresponding equilibrium spread is our liquidity premium, λ_{mt}. It reflects the balance of aggregate reserve supply with the demand that results from precautionary hoarding to protect against liquidity outflows. It will vary over time as banks' perceptions of the probability of such outflows changes.

Now suppose that banks default on their obligations with some exogenous probability. Let the probability that a loan at maturity m to bank i defaults be ρ_{imt}, with the lending bank losing a fraction δ^L_{imt} of the loan and interest in the event of such a default, and further assume that any borrowing bank defaults on its bonds in the same states of the world that it defaults on its interbank loans, although possibly with a different conditional loss rate δ^C_{imt}. Thus, m-maturity CDS holders on bank i expect to pay out at rate $\rho_{imt}\delta^C_{imt}$.

Given risk neutrality, the equilibrium interest rate on an interbank loan to bank i is equal to the expected return on that loan, adjusted for its expected losses:

$$R_{imt} = \frac{R^f_{mt} + k_{it}f_{imt}(x_{it})}{1 - \pi_{imt}\delta^L_{imt}}. \quad (3)$$

Let C_{imt} be the time-t spread on a CDS contract that insures the bonds of bank i over the subsequent m periods. Since bonds default in the same state of the world as interbank loans, we have $\pi_{imt}\delta^L_{imt} = \phi_{imt}C_{imt}$, where $\phi_{imt} = \frac{\delta^L_{imt}}{\delta^C_{imt}}$ is the relative expected conditional loss rates on the two instruments. Substituting ϕ_{imt} and λ_{mt} into equation (3) and defining $L_{imt} = \log R_{imt} - \log R^f_{mt}$ produces equation (1), up to an approximation error due to Jensen's inequality.

While our empirical exercises begin directly with equation (1) and are not dependent upon the

[17] Although it is quite stripped down, the funding-cost portion of this model shares the intuition of other models of the interbank market, such as Allen et al. (2009) and Eisenschmidt and Tapking (2009), in which banks choose their reserve holdings to cover expected liquidity needs. Of course, as studied by Cocco et al. (2009), Acharya et al. (2012), and others, departures from perfect competition could also be important in this market.

specific structural model just described, that model does motivate certain parameter restrictions and help to interpret our results. First, the assumption that the "liquidity premium" λ_{mt} does not vary across banks is a key identifying restriction of the empirical tests, and the structural model clarifies why that assumption is reasonable: this parameter reflects the marginal opportunity cost of interbank lending, which does not depend on borrower characteristics and must be identical across lenders in equilibrium. Second, the structural model implies that the coefficient ϕ_{imt} should only vary across banks and maturities to the extent that the *relative* conditional loss rates between CDS holders and interbank lenders differ in those dimensions. Since there is no particular reason to suspect such variation, we will assume this coefficient to be constant across i and m (that is, we set $\phi_{imt} = \phi_t$ for all values of i and m), a restriction that also greatly improves the identification of the model.[18]

The flexibility of our framework, in which λ_{mt} and ϕ_{imt} are allowed to vary over states of the world, also makes the model amenable to alternative structural interpretations. For example, fluctuations in the value of a bank's relationships, as emphasized by Acharya and Merrouche (2013), could be another reason for changes in its credit-risk sensitivity. Similarly, if the value of interbank relationships is correlated with funding-market liquidity, it will show up in estimates of λ_{mt}. This possibility is consistent with our interpretation of λ_{mt} in the sense that a deterioration in the value of relationships that is systematic across banks (but uncorrelated with credit risk) could be fairly characterized as a drop in liquidity. Thus, we think of changing relationships across banks as one possible source of the fluctuations we document in liquidity and credit-risk sensitivity.

Finally, the above structural framework produces at least one important testable hypothesis. Namely, central bank liquidity programs that provide funds to banks at or below the market rate must cause the liquidity premium to fall if they are large enough. (For any k_{it} and f_{imt}, $\lim_{x\to\infty} k_i f_i(x) = 0$.) Intuitively, such programs reduce the demand for loans in the interbank market, putting downward pressure on the rate. At the same time, they unambiguously reduce probabilities of cash shortfalls.

[18] If lenders are not risk neutral, fluctuations in their risk aversion will also likely be reflected in our estimates of ϕ_t. Time-variation in this sensitivity could also be consistent with the model of Heider et al. (2015), in which lenders' incentives to monitor depend in part on the level of credit risk. Again, however, there is no obvious reason that this should cause differences in pricing across borrowing banks.

3.2 Reporting incentives in Libor

In the spring of 2008, the *Wall Street Journal* published a series of articles calling into question the veracity of Libor quotes.[19] The *Journal* articles pointed to the weakening relationship between Libor and CDS quotes specifically as *prima facie* evidence of misreporting. The authors went on to point out several specific banks for which Libor quotes seemed particularly out of line with CDS spreads and speculated that, "one possible explanation for the gap is that banks understated their borrowing rates... At times of market turmoil, banks face a dilemma. If any bank submits a much higher rate than its peers, it risks looking like it's in financial trouble."

In addition to reputational concerns, banks may have direct financial incentives to misreport. Many of the banks on the Libor panel make markets or hold positions in Libor-linked financial products, such as interest rate derivatives and syndicated loans. If large enough, such positions could net the banks, their clients, or their traders substantial sums of money in short periods of time, even from a few-basis-point move in Libor. The incidence of position-driven misreporting is attested to by the numerous exchanges between Libor respondents and traders, which have come to light through recent legal investigations, wherein Libor submitters repeatedly comply with requests from traders at their banks for particular configurations of Libor quotes.[20]

Subsequent to the *Wall Street Journal* story, most of the banks on the Libor panel have been investigated, with ten having settled allegations of malfeasance with U.S. and U.K authorities as of this writing, and numerous individual bankers face or have pled guilty to criminal charges. Meanwhile, U.K. regulators have undertaken a set of reforms of the reporting process intended to discourage future misreporting (Wheatly, 2012), and a number of academic studies have attempted to uncover evidence of the misreporting *ex post* in the data (Abrantes-Metz et al., 2012; Gandhi et al., 2018; Kuo et al., 2012; Snider and Youle, 2012; Poskitt and Dassanayake, 2015). A smaller literature has also emerged focusing on the game played by banks in setting rates and the consequences for the shape of the resulting rate distribution (Snider and Youle, 2012; Chen, 2013;

[19] Mollenkamp, C., "Bankers Cast Doubt on Key Rate Amid Crisis." *Wall Street Journal*, 16 April 2008; Mollenkamp, C. and Whitehouse, M., "Study Casts Doubt on Key Rate," *Wall Street Journal*, 29 May 2008.

[20] See, for example, Vaughan, L. and Finch, G., "Secret Libor Transcripts Expose Trader Rate-Manipulation" *Bloomberg*, 13 Dec. 2012, and the many instances cited in Arvedlund (2014). Gandhi et al. (2018) estimate that the panel banks collectively gained $33 billion from position-driven manipulation over the 2001-2010 period.

Youle, 2014; Bonaldi, 2017). These models generally view the decision to misreport as reflecting a potential benefit (either reputational or position-driven) traded off against a potential cost of deviating from the truth.

To incorporate into our model the possibility that banks may strategically mis-state their funding costs in the Libor survey, we borrow from the theoretical literature on misreporting. Specifically, we distinguish between each borrowing bank i's "true" funding cost at each maturity L_{imt} and the cost that it reports for Libor purposes $\widehat{L}_{imt}$.[21] The bank's reported value for Libor purposes may reflect several considerations, including that the bank may have a portfolio-specific reason to distort Libor in one direction or the other. Except at the trimming quantiles, the marginal effect of any bank's quote on aggregate Libor is constant (either zero or $2/I$), so we assume that the benefits from distortion are approximately linear. We also consider that banks may receive reputational or signaling benefits from reporting a low value for Libor. Again, we assume that these benefits are linear in $\widehat{L}_{imt}$ at each point in time. Thus, in any period, both incentives to misreport are captured by an expression of the form

$$\text{misreporting benefits}_{imt} = \gamma_{0,imt} + \gamma_{1,imt}\widehat{L}_{imt}. \tag{4}$$

In theory, the marginal benefit of misreporting $\gamma_{1,imt}$ can take either sign. We assume that each bank may face a quadratic cost of reporting a value far away from the truth. This cost may reflect potential regulatory or legal penalties, or simply the psychological and moral costs of lying. We define "far away" in terms of the cross-sectional dispersion of Libor quotes at each point in time. This reflects the idea that more heterogeneity in the market is associated with greater financial-market uncertainty and lowers the probability of a lie being detected. Thus, we have:

$$\text{cost of lying}_i = \frac{\gamma_{2t}}{2} \frac{\left(\widehat{L}_{imt} - L_{imt}\right)^2}{\text{std}_{mt}\left[\widehat{L}_{imt}\right]} \tag{5}$$

where std_{mt} denotes the time-t standard deviation across banks at maturity m. For simplicity, we assume that the parameter γ_{2t} is the same across banks and maturities, since there is no obvious

[21] For ease of exposition, we suppose that each bank reports its spread over the time-t, m-maturity risk-free rate, but this makes no difference since the risk-free rates are assumed to be common knowledge.

reason to expect that the penalties for fraudulent behavior should depend on the identity of the perpetrator or the specific contract that was lied about.

Banks may have misreported not just for financial gain but simply because they did not want to appear different from their peers. Indeed, investigations into the Libor scandal suggest that misreporting out of fear of differentiating oneself from the competition was a strong motivating factor. Arvedlund (2014) reports numerous examples that illustrate this point. In transcripts and other documents released by government agencies, traders and Libor submitters speak of "fit[ting] in with the rest of the crowd," not "draw[ing] unwanted attention to [them]selves," and "not want[ing] to be an outlier in the LIBOR fixings, just like everybody else."[22] An additional reason that banks may have reported Libor numbers similar to their peers is that they may have engaged in outright collusion in an attempt to influence aggregate Libor rates in order to boost their portfolio returns. This type of activity has also come to light in post-crisis legal investigations. For example, the 2012 settlement between the Commodity Futures Trading Commission and UBS noted that that bank "colluded with at least four other panel banks to make false submissions" in yen-related Libor contracts.[23] Such behavior is also not well captured by a model in which banks only trade off their own immediate profits against the costs of lying. Rather, in order for the cartel to be successful, the colluding banks must effectively face a cost of deviating too far from each other.

In light of these observations, we expand the misreporting model to allow for the possibility that banks may worry that reporting a value much different from other banks (in either direction) may bring unwanted scrutiny by markets or regulators or that they may be engaged in implicit or explicit collusion with other banks that would subject them to some cost if their submission deviates too much from the rest of the cartel. We capture these incentives with a second cost

[22] In one particularly telling instance, two UBS employees engaged in the following exchange via text message:

> TRADER: [A senior manager] wants us to get in line with the competition by Friday...
>
> TRADER-SUBMITTER: ... if you are too low you get written about for being too low ... if you are too high you get written about for being too high...
>
> TRADER: middle of the pack there is no issue...

[23] See http://www.cftc.gov/PressRoom/PressReleases/pr6472-12

function:

$$\text{cost of being an outlier}_{imt} = \frac{\gamma_{3t}}{2} \frac{\left(\widehat{L}_{imt} - \overline{\widehat{L}_{mt}}\right)^2}{\text{std}_{mt}\left[\widehat{L}_{imt}\right]} \tag{6}$$

where γ_{3t} is a cost parameter and $\overline{\widehat{L}}_{mt}$ is the cross-sectional mean of reported Libor spreads. We assume that the moments $\overline{\widehat{L}}_{mt}$ and $std_{mt}[\widehat{L}]$ are known to all banks when they do their optimization. Since traders usually have a good sense of the conditions in the day's market before submitting their quotes, this seems a reasonable approximation.[24] We note that this formulation will effectively make banks' misreporting incentives a function of their credit risk. In particular, it gives banks with high credit risk an incentive to under-report Libor in order to remain close to the other banks.

3.3 Banks' Choice Problem

In each period, each borrowing bank chooses $\widehat{L}_{imt}$ at each maturity to maximize benefits less costs in each period. Assuming that the true funding costs are determined from equation (1), Appendix A shows that this maximization produces bank-level Libor submissions that can be written in reduced form as a linear function of the liquidity premium, bank-level CDS spreads, and the cross-sectional mean and standard deviation of all banks' CDS. In particular:

$$\widehat{L}_{imt} = \lambda_{mt} + \phi_t C_{imt} + \beta_{1imt}\sigma^C_{mt} + \beta_{2t}\left(C_{imt} - \overline{C}_{mt}\right) \tag{7}$$

where $\overline{C}_{mt}$ and σ^C_{mt} are the cross-sectional mean and standard deviation of CDS spreads at each maturity, $\beta_{2t} = -\phi_t\gamma_{3t}/(\gamma_{2t}+\gamma_{3t})$, and β_{1imt} is a complicated function of the structural parameters, including γ_{1imt}. Equation (7) will be the target of our estimation.

If banks always told the truth, $\widehat{L}_{imt}$ would simply be equal to $\lambda_{mt} + \phi_t C_{imt}$. Thus, the last two terms in equation (7) reflect the bank-level misreporting bias. Note, however, that since the last term of equation (7) always averages to zero, the *aggregate* bias in Libor is simply equal to $\bar{\beta}_{1mt}\sigma^C_{mt}$, where $\bar{\beta}_{1mt}$ is the cross-sectional average of the β_{1imt} terms. The result that the size of the bias should increase with the cross-sectional dispersion of true funding costs also appears in

[24] For example, in its May 29, 2008 article, the *Wall Street Journal* noted, "When posting rates to the BBA [British Banker's Association], the 16 panel banks don't operate in a vacuum. In the hours before banks report their rates, their traders can phone brokers at firms such as Tullett Prebon PLC, ICAP PLC and Compagnie Financière Tradition to get estimates of where brokers perceive the loan market to be."

Chen (2013), although it arises through a slightly different mechanism.[25]

Finally, we note that the applicability of our model does not depend on the amount of lending that actually takes place in equilibrium. Anecdotally, interbank volumes were very low, perhaps zero, at longer maturities during much of our sample. This is a possible outcome of our model, one in which the equilibrium rate is too high for any bank to find it worthwhile to borrow. In this case, the equilibrium rate is still a well defined object, albeit one that cannot be observed from market prices. One advantage of the Libor data is that, assuming misreporting can be corrected for, they provide a source of information *even when* volumes are zero. Of course, one might wonder how informed the Libor reporters really are about their borrowing costs when they are not actually doing any borrowing. But this lack of information does not cause any particular problems for our model either. Indeed, one can reinterpret the model as one in which a bank is unsure of its own true funding cost and (in addition to possible strategic misreporting behavior) makes an informed guess about its value by looking at the distribution of other banks' submissions, which it weights by the term γ_{3t}. Consequently, we expect our model to produce unbiased and meaningful estimates of λ_{mt} and ϕ_t even when there are no underlying interbank transactions.

4 Estimation

We estimate the model by treating equation (7) as a measurement equation and the reduced-form parameters as unobserved state variables. We impose a final set of cross-equation restrictions to further reduce the dimension of the system by assuming that β_{1imt} is the same across all maturities for each bank (that is, we assume $\beta_{1imt} = \beta_{1it}$ for all values of m). Given our other assumptions, maturity variation in this parameter could only come from variation in γ_{1imt}. Thus, this restriction implies that marginal misreporting benefits at a given bank are the same across all maturities and any point in time. Specifically, we estimate

$$\widehat{L}_{imt} = \lambda_{mt} + \phi_t C_{imt} + \beta_{1,it}\sigma^C_{mt} + \beta_{2,t}\left(C_{imt} - \overline{C}_{mt}\right) + \epsilon_{imt} \tag{8}$$

[25]Since our estimation will allow us to infer λ_{mt} and ϕ_{imt}, equation (7) allows us estimate each bank's "true" Libor values. Of course, if we can back out "true" Libor rates as econometricians, we should recognize the possibility that market participants can also do it. Our model is essentially unchanged if we permit all banks to have full knowledge of all of the L_{imt} in real time. Of course, in this case, it would not make sense to consider reputational benefits of misreporting, but equation (4) still holds as an approximation to possible trading gains.

where ϵ_{imt} is a normally distributed iid error with variance matrix $\mathbf{R}$, which we assume to be diagonal.[26] Since these equations are linear in the observables C_{imt}, σ^C_{mt}, and $\overline{C}_{mt}$, we can use the Kalman filter to infer the values of the time-varying parameters λ_{mt}, ϕ_t, β_{1it}, and β_{2t} (for all i and m). Estimation is joint over 17 banks and 5 maturities, and we thus have 85 measurement equations and 24 state variables.

The fixed parameters of the model are estimated by Gibbs sampling, following Kim and Nelson (1999), much in the style of the state-space modeling used in the time-varying vector autoregression literature (e.g. Cogley and Sargent, 2002). Following that literature, we collect our state variables in the vector θ_t, and we approximate their evolution with independent random walks:

$$\theta_t = \theta_{t-1} + \nu_t \tag{9}$$

where $\nu_t \sim N(0, \mathbf{Q})$ and $\mathbf{Q}$ is a diagonal matrix. As noted earlier, some observations are missing for some banks, either because we lack CDS data or because banks entered or departed the Libor panel. Missing observations are handled through the Kalman-filter-based imputation procedure demonstrated in Aruoba et al. (2009). In our results presented below, we estimate the model on weekly averages of the daily data. Appendix C provides additional detail on the estimation procedure.

5 Results

5.1 State Variable Estimates

Figure 2 shows our smoothed estimates of the liquidity premia λ_{mt} across maturities by plotting the median of the posterior distributions, along with 5th and 95th percentiles. These premia are fairly tightly estimated. They are generally increasing in maturity. This result is perhaps not surprising given Figures 1a and 1b, which showed the steep term structure of LOIS spreads that was not matched by the term structure of the CDS data. It is also consistent with anecdotes and evidence that liquidity was particularly strained and trading volumes particularly

[26] The assumption of uncorrelated measurement errors is common in the state-space and term-structure literatures as a necessary way to reduce the number of parameters that need to be estimated. If we attempted to estimate the full $\mathbf{R}$, rather than a diagonal version, it would require us to estimate an additional 3,570 terms, which would be weakly identified at best.

low in longer maturities during the crisis (e.g., Gorton et al., 2014). The one-week and one-month liquidity premia drop precipitously, and indeed take negative values, in mid-October, 2008. This observation, to which we return later, suggests that Federal Reserve interventions in funding markets around this time significantly ameliorated liquidity strains at short maturities. On the other hand, even by the end of the sample, the 6- and 12-month liquidity premia had not fully retraced their upward moves during the crisis.

Figure 3 shows the results for the credit risk component of the estimated model. Specifically, panel 3a shows our estimate of the sensitivity to credit risk, ϕ_t. This series displays significant variation over time. Indeed, our estimate of the weekly standard deviation of changes in ϕ is 0.11.[27] Specifically, credit risk sensitivity spikes during a relatively brief episode immediately following the Lehman bankruptcy when we estimate ϕ_t to jump to a value of about 2.5.[28] We discuss these results further in Section 6.

Panel 3b combines our point estimate of ϕ_t with the CDS data to produce the aggregate credit-risk components $\phi_t \overline{C}_{mt}$. Unlike liquidity, credit risk exhibits virtually no differences across maturities. Although credit-risk contributes substantially to LOIS spreads during certain episodes, the path of ϕ_t causes it to spend a significant amount of time close to zero. Indeed, outside of the period from late 2008 to mid-2009, average interbank credit spreads rarely rise above 50 basis points at any maturity.

Figure 4 shows the average misreporting bias ($\overline{\beta}_{1,t}\sigma_t^C$) which varies considerably over time. On average, it is slightly negative, consistent with previous empirical work. In particular, we find that the bias (based on the medians of our posterior distributions) averages around -5 basis points at all maturities. It attains its largest magnitudes of -25 to -30 basis points in early 2009 (about the same time that bank CDS spreads reach their peaks). These magnitudes are similar to those estimated by Youle (2014) and Kuo et al. (2012). The finding that the bias was greater during the crisis period is consistent with opportunistic misreporting. However, we also estimate that misreporting

[27] This is obtained from the corresponding element of **Q**.

[28] Afonso et al. (2011) use entirely different measures of bank funding costs and credit risk—and a broader sample of banks—to examine the response of the interbank market immediately after Lehman. Their results are consistent with ours in the sense that they conclude that sensitivity to credit risk increased during that period, although they argue that most of this sensitivity took the form of quantity rationing rather than differences in spreads.

biases were considerably smaller and not statistically significant in the period of stress beginning in mid-2011, even though the levels and dispersion of CDS spreads over that period were similar to the 2008-2009 period. This suggests, perhaps, that banks made more of an effort to report correctly in an environment of enhanced regulatory attention to this problem, a finding consistent with the results of Gandhi et al. (2018).

Recall that our estimate of the reduced-form parameter β_{2t} allows us to infer the ratio of the perceived cost of lying to the perceived cost of deviating from other banks (γ_{2t}/γ_{3t}). The estimate of this ratio averages 0.04 over the sample, never exceeds 1, and is rarely statistically significant (see Figure D.1 in the online appendix). This result implies that the perceived cost of differing from other banks was always much greater than the perceived cost of lying *per se*. This finding supports our extension of the misreporting model to include a penalty for deviating from the group and helps to explain the relatively tight cross-sectional variance of LOIS spreads compared to that of CDS spreads through both the numerator and the denominator of the ratio. All else equal, a value of γ_{2t}/γ_{3t} close to zero would imply that banks do not vary their reported Libor quotes commensurately with their credit risk, but that they would all want to report similar values.

5.2 Decomposition of LOIS spreads

Table Ia summarizes the relative contributions of the liquidity, credit-risk, and misreporting components, based on our estimates of λ_{mt}, ϕ_t, and $\bar{\beta}_{1t}$, to overall LOIS spreads. The top panel reports our median point estimates of the three components averaged over time. The bottom panel reports the average ratios of each component to the "true" LOIS spread.[29] The liquidity component dominates the true LOIS spread at all maturities greater than one week. At horizons greater than 1 month, it accounts for roughly 80% of the level of the spread on average. At the one-month maturity, the liquidity premium constitutes a larger fraction of the average spread than the credit-risk premium does (54% versus 47%) though its average value is slightly smaller (13.5 basis points versus 17.7 basis points). This tension, which also exists to a lesser extent at other maturities, reflects the fact that the average credit-risk component is pulled upward by a relatively brief episode in 2008 and 2009; for most of the sample and at most maturities it is lower than the

[29] "Truth" is defined as the estimate we obtain by subtracting the estimated misreporting bias from the reported quote.

liquidity-risk component. Meanwhile, the average misreporting component is modestly negative, as noted above. It is similar in magnitude, on average, across maturities, although as a fraction of the overall LOIS spread it is less important at longer maturities.

Table Ib decomposes the time-series variance of the (true) LOIS spread into the variance of the liquidity and credit-risk components and the covariance between them. The variance of the credit-risk component is further decomposed, to a first-order approximation, into the variation due to credit risk itself (as measured by CDS spreads) and the variation due to changes in credit-risk sensitivity (ϕ_t).[30] Again, this decomposition is performed using the medians of the distributions of our estimated state variables.

Although liquidity seems to be the largest component of the longer-run *levels* of spreads, it is fluctuations in the credit-risk component that drive the *movements* over time on average. In addition, movements in credit risk itself (as captured by CDS spreads) account for less than half of this variation. Most is due to movements in credit-risk *sensitivity*, represented by the time-varying parameter ϕ_t. As noted above, fluctuations in ϕ_t played a large role in driving LOIS spreads during the crisis. This variation would be missed in specifications that assume a constant coefficient on CDS spreads. Aside from the one-month horizon, the covariance terms contribute significantly to the variance of LOIS spreads. Most of this covariance derives from correlation between λ_{mt} and $\overline{C}_{mt}$ (rather than λ_{mt} and ϕ_t). In particular, the negative covariance at shortest maturities primarily results from the drop in short-term liquidity premia in late 2008, at the same time that aggregate CDS spreads were rising.

5.3 Validation exercises

5.3.1 Regression validation of liquidity estimate

Our measure of liquidity in the interbank market follows an often-implemented approach of measuring the liquidity component of LOIS spreads as a "residual." That is, in our setup, the common component of what remains in the LOIS data after accounting for credit and misreporting is labeled liquidity. An alternative approach—taken, for example, by Schwarz (2018)—is to use direct

[30]The latter decomposition is not exact because ϕ_t and $\overline{C_t}$ are multiplied together, and it is accomplished by holding each of those components constant at its sample mean while allowing the other component to vary.

proxies for liquidity itself in the estimation.[31] We validate our model-based estimates of liquidity using a similar rationale by comparing them to other liquidity measures external to our model. We also view this validation as strengthening the case for the correct identification of the other model components. That is, the fact that our estimated liquidity series correlates well with external measures of liquidity also lends credence to our estimates of credit risk and misreporting.

We validate our liquidity component by examining how well it is explained by two short-term liquidity variables that are completely unrelated to any form of credit risk. First, we use a time series of the total available lending capacity from the TAF. This measure is based on the total volume for collateralized loans made available by the Federal Reserve during our sample. This total capacity number is constructed by aggregating the total sizes of each of the auctions whose funding would have been available at a given point in time. This number reflects total funding made available by the Federal Reserve and does not reflect the any individual bank's choices about how much funding to attempt to obtain. Since these auctions were for loans which were primarily made at 28-day maturities (with none exceeding 84 days), they should have had a beneficial effect on liquidity at the one-week and one-month horizon, though not necessarily at the longer horizons in our sample. The second liquidity variable we use is the spread between rates on recently issued Treasury bills and the off-the-run Treasury coupon curve. This spread measure is similar to the Schwarz (2018) liquidity proxy and has also been used in a different context by Greenwood et al. (2015) as an indicator of excess demand for liquidity at short horizons. An advantage of this series is that we can match it exactly to the maturities of the Libor data.[32]

Table II shows the results of these regressions. Because the errors in the model displayed very high serial correlation, resulting in Durbin-Watson statistics in the range of 0.1, we report results using the Cochrane and Orcutt (1949) procedure.[33] In nearly all cases, the coefficients

[31] In Schwarz's paper, a liquidity metric for the German bund market serves as a proxy for liquidity in the interbank market, under the assumption that liquidity conditions in the two markets are likely to be highly correlated.

[32] We note, however, that data on bill rates do not exist at the 1-year horizon for part of our sample or at the 1-week horizon at all.

[33] Using OLS with Newey-West correction produces consistent standard errors in the presence of serial correlation, but it does not overcome the inefficiency of the OLS coefficient estimate in such a situation. Moreover, it does not correct for the spurious correlation that biases R^2 upward. Cochrane-Orcutt is a feasible generalized least-squares regression that explicitly models the autocorrelation of the error term and is asymptotically efficient in the presence of (first-order) serial correlation. For comparison, the OLS regression results with Newey-West autocorrelation-consistent standard errors are reported in the online appendix.

are statistically significant with the expected signs. In particular, periods in which the Treasury bill/coupon spread is larger are also periods in which our measure of the liquidity premium is higher at the corresponding maturity. Increases in the total capacity of the TAF are associated with improvements in our liquidity premium at short maturities but deterioration at long maturities. This is consistent with activity migrating from long-term lending to the one-week and one-month sectors of the market, which were being supported by the Fed.

5.3.2 Checking the effects of CDS liquidity

A concern raised about models such as these which use CDS spreads to proxy for credit risk is that—particularly when the credit-risk sensitivity parameter is time-varying—the model may confuse deviations between the empirical proxy for credit risk (CDS spreads) and actual credit risk with changes in the sensitivity to credit risk (ϕ_t). While our approach to the credit-risk proxy attempts to make the best use of *all* available CDS information, here we pause to investigate concerns that the link between CDS and credit risk may be weakened by poor liquidity in the CDS market.

If fluctuations in CDS liquidity were an important driver of our results, then the estimates of liquidity and credit-risk sensitivity factors produced by our model would be significantly correlated with CDS liquidity. To check for this possibility, we run regressions of our estimates of liquidity (λ_t) and credit-risk sensitivity (ϕ_t) on two measures of CDS liquidity: Nelson-Siegel curve fitting errors for the shortest observable maturities and a sample of CDS bid/ask spreads. As discussed in section 2.1 above, fitting errors for the CDS term structure curves represent a measure of CDS liquidity which we have used already as a preliminary screen by dropping any observation for which the fitting error was greater than 25% of the overall CDS spread. Here, we include in each regression the mean of the cross-sectional distribution of the absolute value of these curve-fitting errors. The CDS bid/ask spreads are constructed by Markit for each contract at each maturity on each day that they receive sufficient quotes. Unfortunately, Markit only began collecting these data in 2010, and we only have observations for the three US banks in the Libor panel. While this limitation is significant, one might expect CDS liquidity to be highly cross-sectionally correlated across banks. We construct indices of bank-CDS liquidity at the six- and twelve-month horizons by averaging the three bid/ask spreads that we observe at each of those maturities on each day.[34]

Table III reports the results.[35] The coefficients shown are in a sense upper bounds on the contamination of our results, because we expect measures of CDS liquidity to be correlated with actual conditions in bank funding markets. Nonetheless, in most cases, the external liquidity measures have little to no significant correlation with the model estimates. The adjusted R^2's (which exclude the effect of serial correlation) are all fairly close to zero. Even in the cases where the coefficients are statistically significant, their economic magnitudes are low. In short, although it is true that liquidity in CDS may create some concern that CDS spreads reflect more than pure credit risk, we find no evidence that the resulting noise in the data is driving our results.

5.4 Does Accounting for Misreporting Matter?

Even though our estimate of the aggregate misreporting bias is modest on average, including the misreporting terms in our model has a large influence on both the fit and the qualitative results. To see this, we re-estimated the model under the assumption that γ_{1it} and γ_{3t} are always zero (i.e., there is no benefit to misreporting and no cost of deviating from other banks). These assumptions are sufficient to make the β_{1it} and β_{2t} terms in equation (7) equal to zero. Consequently, the model that we now estimate contains only the liquidity and credit-risk terms.

Figure 5a plots the posterior trace of $\mathbf{R}$ for this restricted model versus the baseline model estimated above. This is a summary measure of how closely either model is able to match the LOIS quotes.[36] Including the misreporting terms reduces the estimate of the trace by more than half, indicating that these terms are important for fitting the data.

Apart from the significantly better econometric fit of including misreporting in the model, Figure 5b shows the economic intuition behind including the misreporting mechanism by looking at the estimate of credit risk sensitivity during a period of roughly one year during the peak of the financial crisis. The panel on the left shows estimates of ϕ_t under both the baseline model and the model without misreporting terms. Without accounting for misreporting, the model is best fit by

[34] The online appendix contains further details and a table of summary statistics on these two liquidity proxies.

[35] Again, we use Cochrane-Orcutt regression because of the highly serially correlated errors, with autocorrelation-consistent OLS results reported in the online appendix.

[36] Our prior distribution in both cases is essentially flat over the region depicted in Figure 5a.

credit risk sensitivity that always hovers much closer to zero. In addition, as seen by focusing exclusively on the model without misreporting in the panel on the right, we see that without accounting for misreporting the sensitivity to credit risk actually becomes slightly negative—indicating a risk-*loving* attitude among interbank lenders—at the peak of the crisis. Such a finding would also be at odds with previous research, including Afonso et al. (2011), which finds heightened credit risk sensitivity at exactly this time. Due to the credit risk sensitivity being driven to zero in the absence of misreporting, this model puts essentially all of the level and variation in the LOIS spread in the liquidity term, rather than giving credit risk an important role. This can be seen in Tables IVa and IVb, which reproduce Tables Ia and Ib for the model that excludes misreporting.

There are at least two statistical reasons that accounting for misreporting matters. First, while the effects of misreporting on the aggregate level of Libor are modest, the effects on individual banks' submissions can be large. The flexibility introduced by the misreporting terms helps us to fit the cross-sectional data much better, since (as noted in Section 2) the differences between banks can generally not be explained by credit risk alone. It therefore sharpens our estimates of λ and ϕ. Second, the time-series correlation between the levels and dispersion of CDS quotes (C_{it} and σ_t^C) is quite high—on the order of 80% across maturities in our data. Thus, leaving out the β_1 term in equation (8) introduces omitted-variable bias. While in theory that bias could go in either direction, in practice it pushes our estimates of ϕ_t toward zero with the λ_t terms picking up most of the slack.

5.5 Is time-variation in credit risk sensitivity important?

The baseline model articulated in equation (8) allows ϕ to vary over time. That is, in the baseline setup, the sensitivity of interbank spreads to bank credit risk can be different in the crisis than it is in the recovery. As a robustness check, we estimated a version of the model in which ϕ was fixed. This was done through the inclusion of an additional Gibbs step to the estimation. That is, we extracted ϕ from the θ_t vector, creating θ_t^* (and Q^*), which continues to evolve according to equation (9), and drawing a fixed ϕ in a separate step.

The resulting posterior distribution for the fixed ϕ peaks at a value slightly above the time-series average of the median path of the time-varying ϕ_t for the baseline model. Figure 6 shows that the posterior distribution also has a hint of bimodality below the peak, which we view as owing to the potential for different sensitivities to credit risk at different points in time in the sample. We build the Bayes Factor to do formal model comparison and find a log-Bayes factor of 120, indicating that the data prefer the baseline specification under the strictest selection criteria.[37] This aligns with evidence from Afonso et al. (2011) suggesting that attention to credit risk was higher during the crisis than at other times, and that earlier work decomposing credit risk and liquidity which assumes a constant level of sensitivity to credit risk may be misspecified.[38]

6 Event Studies

We use the results of our model to examine the drivers of funding pressures in different episodes during and after the financial crisis. Among other things, this exercise provides suggestive evidence on the efficacy of various policy measures taken during that time. Specifically, Table V reports the decomposition of changes in average LOIS spreads around five events that garnered wide attention by showing changes in the three fundamental components of those spreads (λ_{mt}, $\overline{C}_{mt}$, and ϕ_t) in the weeks surrounding each event.

The first event ("Beginning of crisis") includes the suspension of redemption of funds by BNP Paribas that marked the start of the financial crisis and that corresponded to the sudden jump in LOIS spreads that was evident in Figure 1a. The table shows that, during the month of August 2007, LOIS spreads at all but the shortest maturities exhibited increases that were in the 99th percentile. Our decomposition shows that these increases were due to a deterioration in liquidity, but also to the rise in ϕ. Similarly, the second column shows the breakdown for the period around the introduction of the TAF in December 2007. In the weeks following the TAF announcement, LOIS spreads fell across the board. We attribute a sizable portion of the fall at the one- and three-month maturities to improved liquidity. Again, movement in ϕ is also estimated to have played a role, though the total shift in credit risk was not large. The Fed expanded the TAF and introduced

[37] For reference, Kass and Raftery (1995) suggest a log Bayes Factor of 5 as the the most-stringent threshold for evidence against a model. We construct the log-Bayes Factor by building the log-marginal data densities via the harmonic mean method.

[38] These results and replications of Tables Ia and Ib under a fixed-ϕ regime are included in the online appendix.

a host of new liquidity facilities targeted at relatively short maturities in October 2008.[39] As shown in the third column, LOIS spreads narrowed significantly around this time, and, in the first two maturities likely strongly influenced by the TAF, our model attributes most of this narrowing to an improvement in liquidity (seen by comparing the sizes of the λ contribution to the size of the contribution from the Total Credit Risk effect). These results compliment Wu (2011), who also found liquidity improvements in response to the TAF. Interestingly, in both the December 2007 and October 2008 episodes, liquidity improvements at the short end were not accompanied by such improvements at the long end. Indeed, if anything, longer-term liquidity deteriorated during these periods, particularly for the 12-month maturity. This result, which echoes our regression findings in Table II, may reflect a substitution by banks into the maturities where the facilities were targeted and away from longer-term funding, which was not generally supported by these programs.

To see more plainly how short-term liquidity spreads changed during the period when the TAF was operational, Figure 7 plots our estimates of $\lambda_{1wk,t}$ and $\lambda_{1mn,t}$ against the level of total capacity for TAF loans by the Federal Reserve. The decreases in liquidity premia during the two TAF-related windows considered in the table are evident, but, as suggested by the regression results reported in Table II, the correspondence between the series extends over the entire life of the facility. Again, no such relationship exists with the longer-maturity liquidity spreads (not shown).

As seen by the relatively muted movements in the $\overline{C}$ panel of the table for the first three events, CDS spreads did not move substantially in any of the episodes discussed so far. The final two columns of the table consider events in which they did. First, in May of 2009, the Federal Reserve announced the results of the Supervisory Capital Assessment Program (also known as the bank "stress tests"), and bank CDS spreads narrowed significantly in response. Simultaneously, LOIS spreads narrowed, particularly at longer maturities, and we estimate that this occurrence was entirely due to the improvement in credit risk; we do not see any improvement in liquidity around this time. Similarly, we do not find a significant deterioration in liquidity in response to the turbulence in European sovereign debt markets that arose in August 2011, even though bank CDS spreads widened dramatically. Moreover, the CDS widening did not pass through to a significant

[39] The new facilities included the Commercial Paper Funding Facility, the Money Market Investor Funding Facility, and new or increased liquidity swap lines with numerous foreign central banks.

degree into LOIS spreads because our estimate of ϕ was near zero around this time.

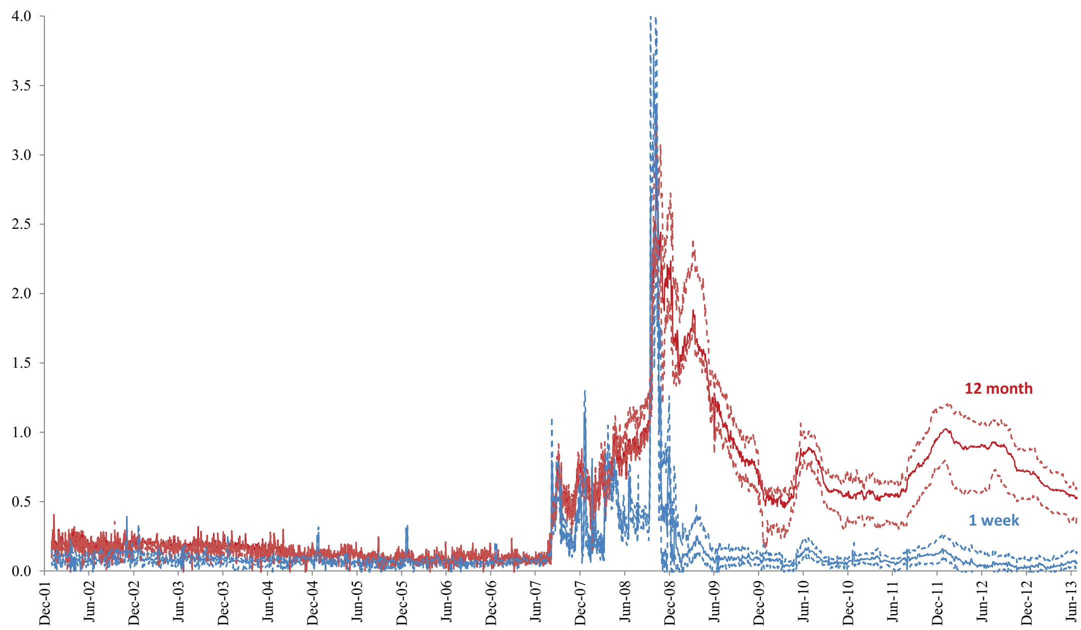
7 Conclusion

This paper develops a model that combines the fundamental determinants of interbank spreads given by Libor data with the possible costs and benefits of strategic misreporting by Libor-submitting firms. It also maximizes usage of the information about banks' short-term credit risk by utilizing the full CDS information set. By explicitly modeling misreporting as we re-examine the credit risk and liquidity debate about short-term funding spreads, we merge two growing strands of the literature and place this important decomposition for policymakers onto firmer footing.

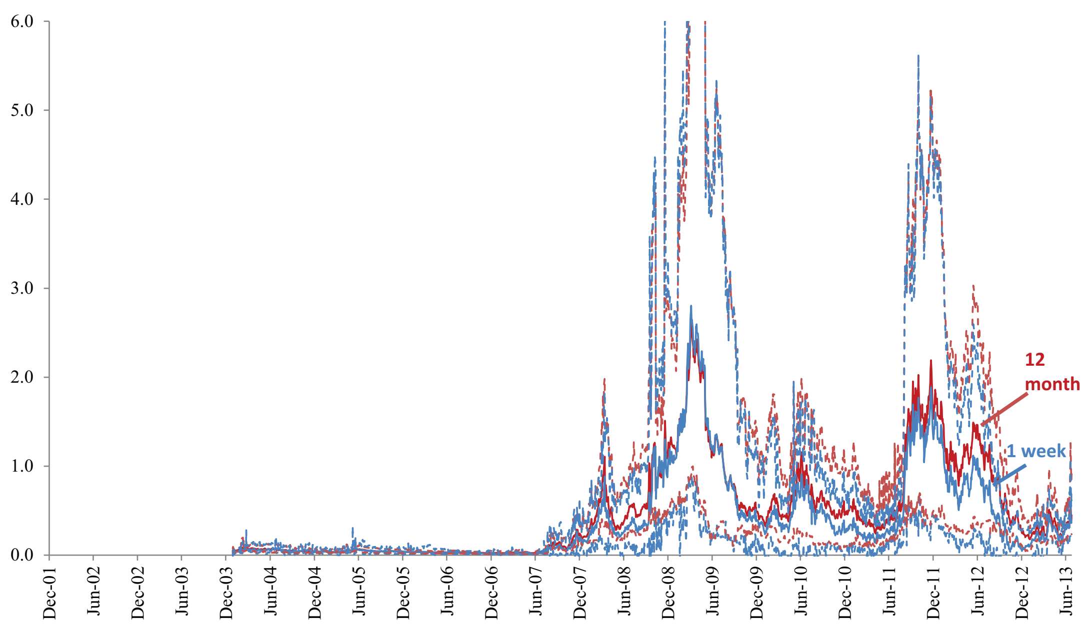
We conclude that, during the period examined, liquidity was the largest component of bank funding costs, especially at longer maturities. Furthermore, we find that, at shorter maturities, liquidity improved significantly following Federal Reserve interventions in short-term funding markets, in contrast to some previous studies, such as Taylor and Williams (2009), that suggest the Fed's actions had limited effects. Thus, it appears that policymakers have had ample scope to affect bank borrowing costs and funding-market pressures through liquidity interventions. However, we also find that most of the *variation* in LOIS spreads during the crisis was due to the credit-risk component and that much of the fluctuations in that component stem from movements in the interbank rate's sensitivity to credit risk, rather than from changes in the level of credit risk *per se*. This suggests policymakers' ex-ante regulatory actions to prevent credit risk and ex-post actions (such as the SCAP examinations of 2009) to clarify the magnitude of actual credit risk may also help calm funding markets. In particular, we find very high sensitivity to credit risk around the failure of Lehman Brothers, but not subsequently.

We find the misreporting bias to be modest on average, but our results indicate that accounting for it is of first-order importance when disentangling the credit and liquidity factors in models which examine bank-level data. Additionally, it appears important to account for the possibility that banks perceive a cost of being outliers in the distribution of Libor submissions, rather than just a cost of not telling the truth.

Figure 1: Dispersion of CDS and Libor-OIS Submissions



(a) Dispersion of Libor-OIS Submissions. The solid lines show the cross-sectional average on each day and the dashed lines show the range across banks.



(b) Dispersion of Nelson-Siegelized CDS Quotes. The solid lines show the cross-sectional average on each day and the dashed lines show the range across banks.

Figure 2: Estimated path of liquidity premia ($\lambda_m t$), by maturity, with 5th and 95th percentiles (shown in percentage points).

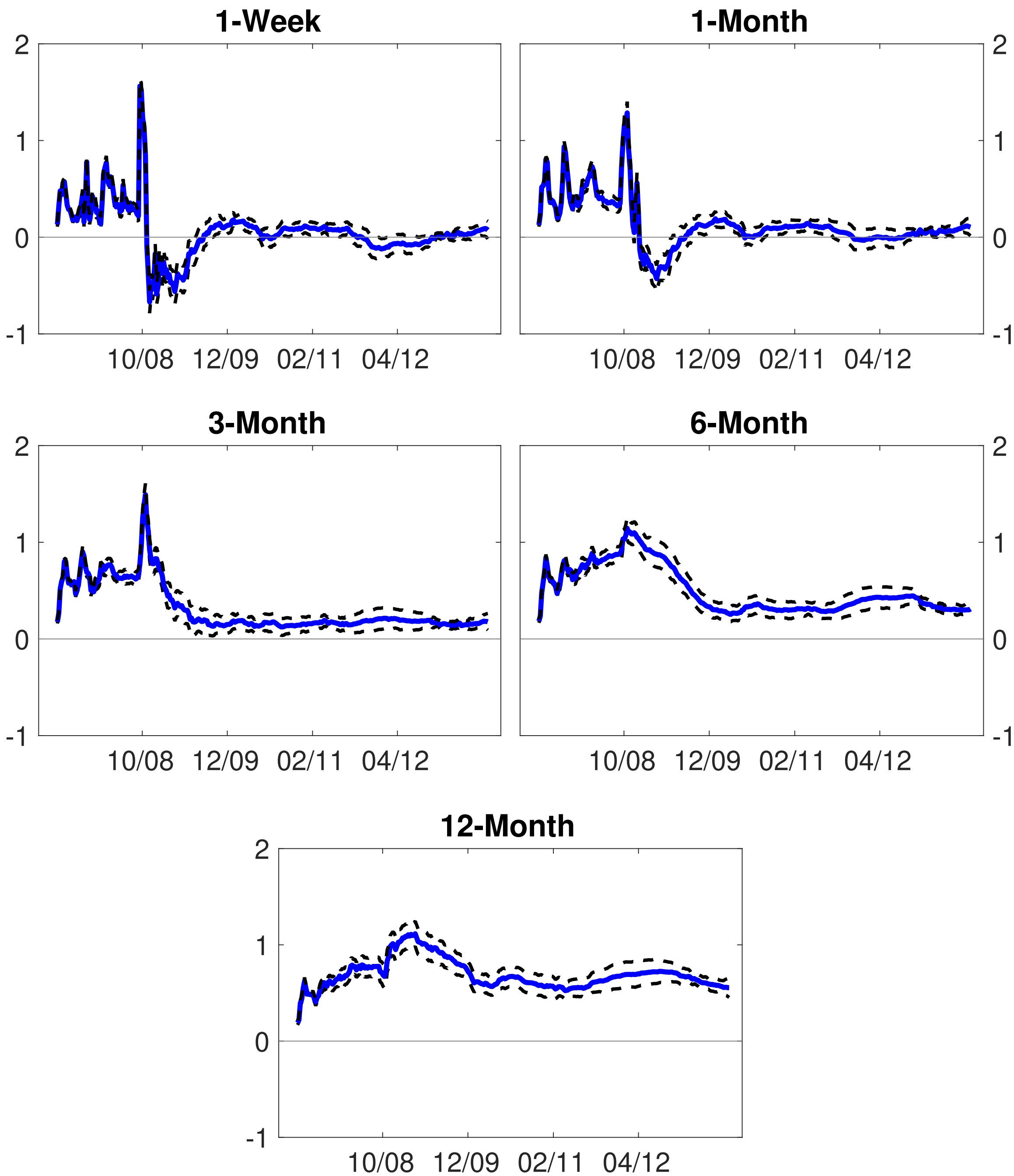
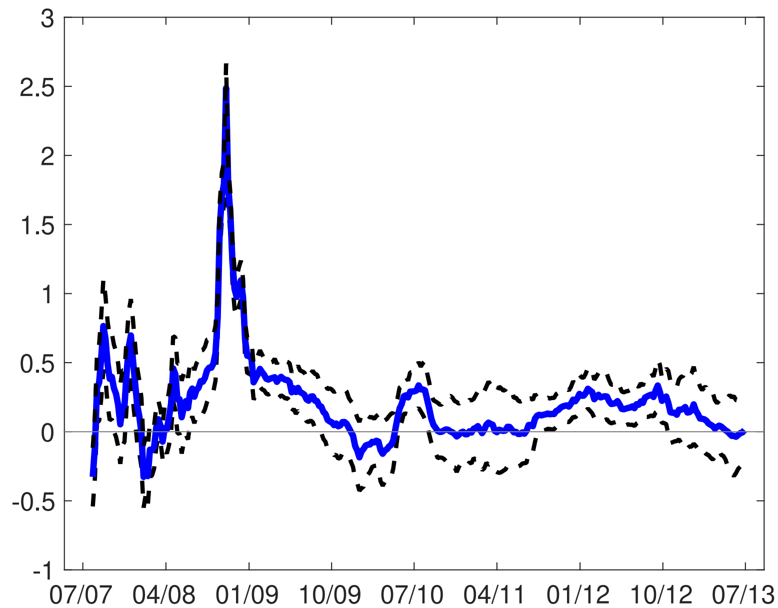
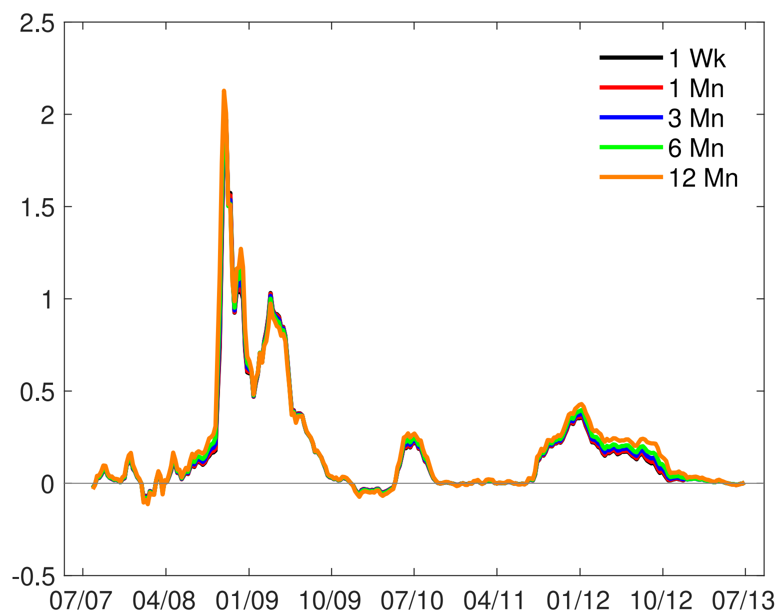


Figure 3: Credit Risk



(a) Estimated path of credit-risk sensitivity (ϕ_t), with 5th and 95th percentiles (shown in percentage points).



(b) Aggregate Credit-Risk Component, $\phi_t \bar{C}_{mt}$ (shown in percentage points).

Figure 4: Estimated path of the average misreporting bias ($\overline{\beta}_{1,t}\sigma_t^C$), by maturity, with 5th and 95th percentiles (shown in percentage points).

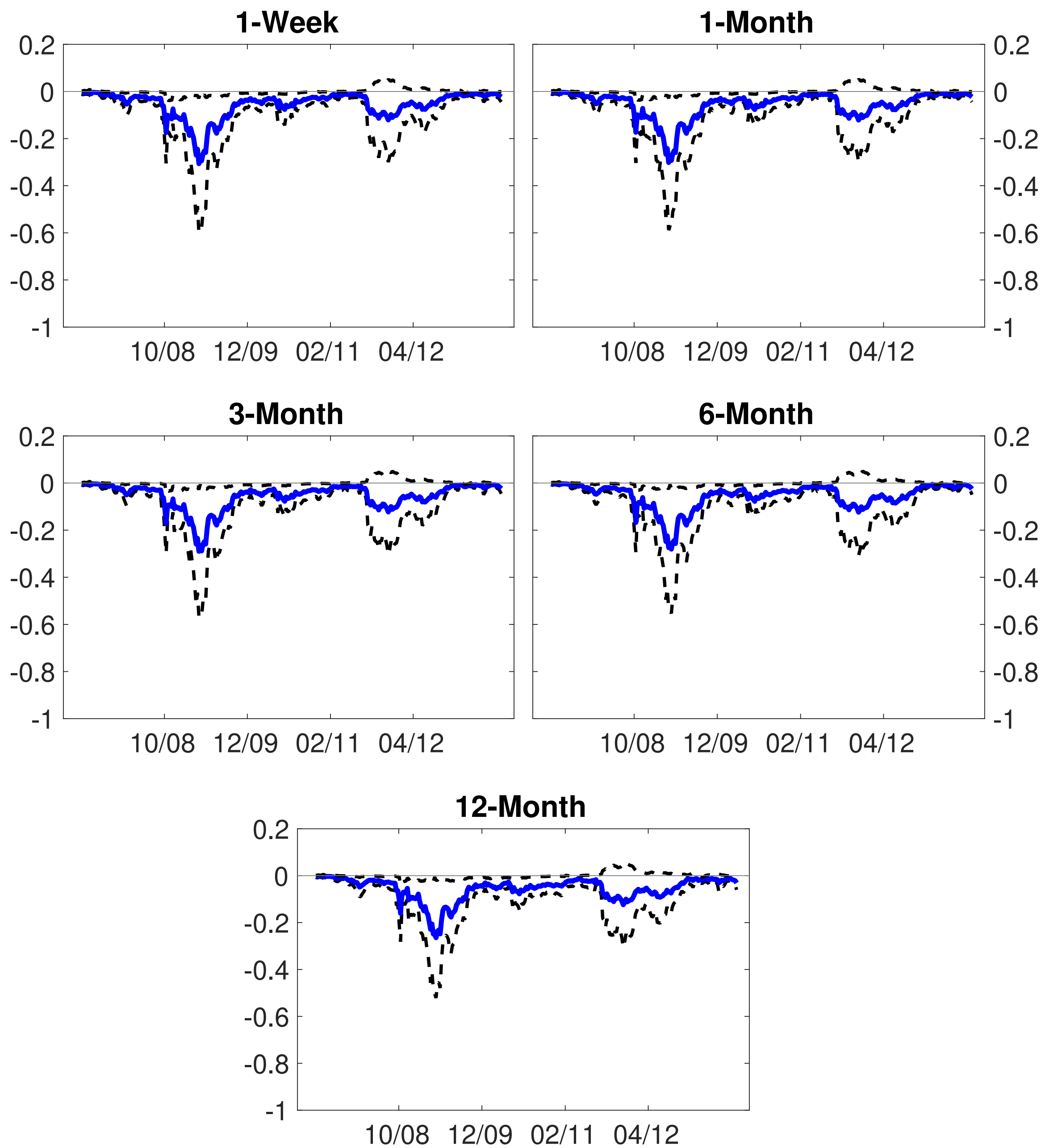
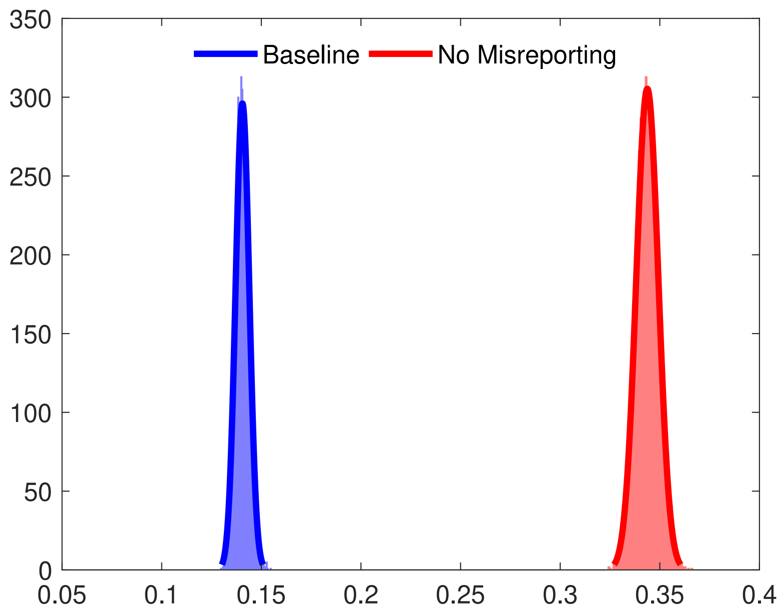
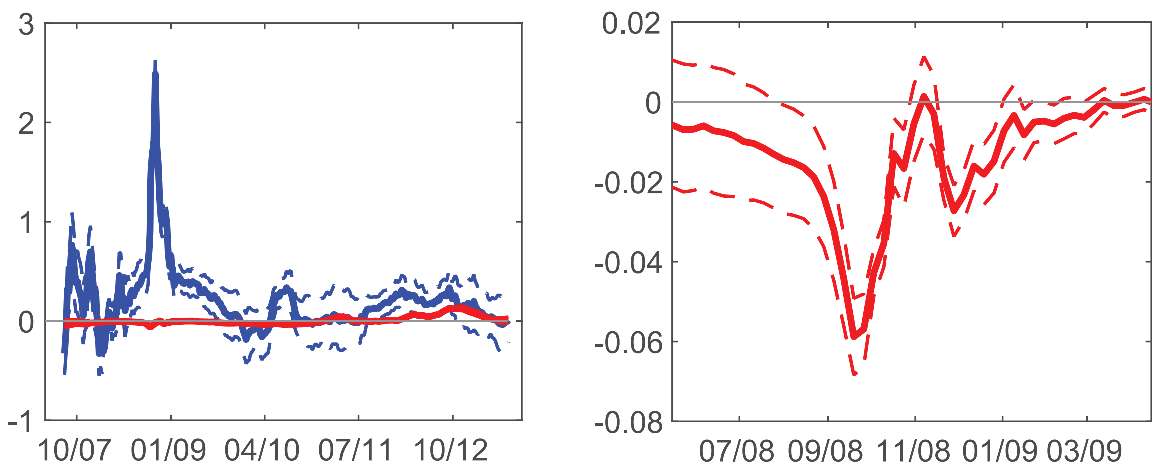


Figure 5: Model without Misreporting



(a) Posterior distributions of the trace of **R** under the baseline model (blue) and the model with misreporting terms set to zero (red).



(b) Estimated path of credit-risk sensitivity (ϕ_t), with 5th and 95th percentiles (shown in percentage points). On the left, ϕ_t is shown for both the baseline model (blue) and the model with misreporting terms set to zero (red) for a period of roughly a year around the peak of the crisis. On the right, we show only the model with the misreporting terms set to zero—for a period of roughly a year around the peak of the crisis—in order to show the dip below zero at the appropriate scale.

Figure 6: Posterior distribution of ϕ in the specification where ϕ is not time-varying.

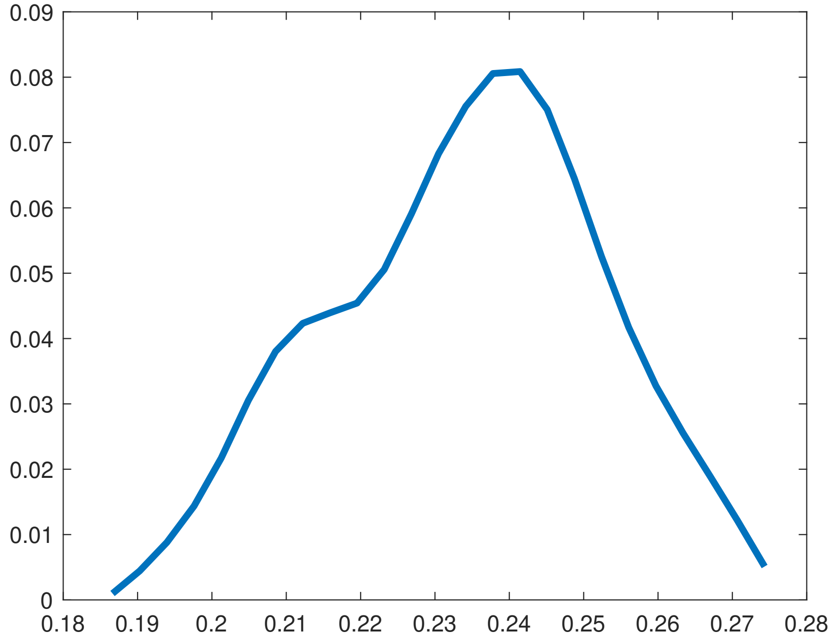


Figure 7: The estimated path of the short-maturity liquidity premia (λ_t) and the level of total capacity for TAF loans at the Federal Reserve.

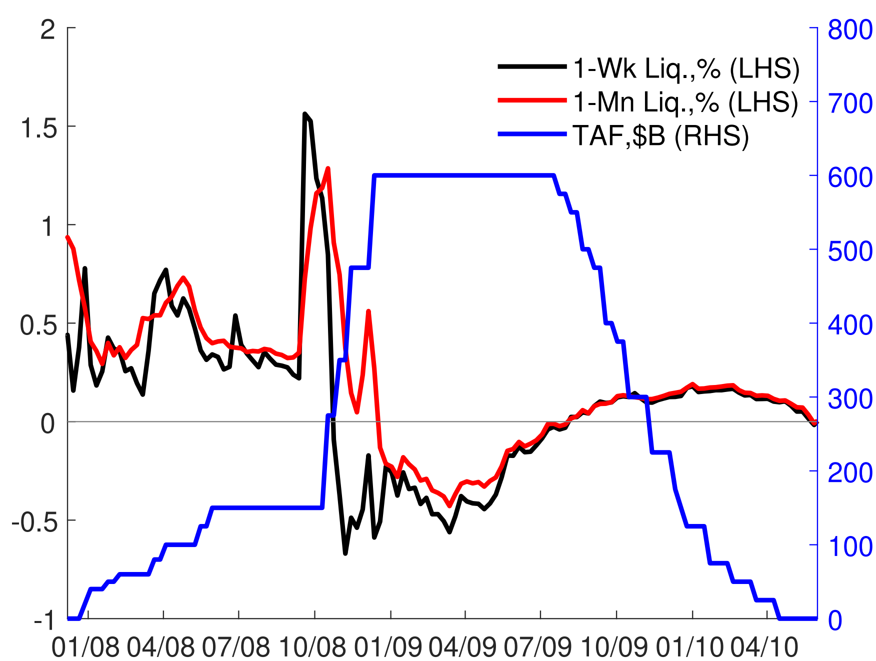


Table I: Model Results

(a) Relative Contributions of Model Components

	1wk	1 mn	3 mn	6 mn	12 mn
Average value					
Liquidity	0.069	0.135	0.313	0.499	0.691
Credit Risk	0.175	0.177	0.181	0.188	0.201
Misreporting	-0.054	-0.054	-0.054	-0.054	-0.055
Average fraction of "true" LOIS spread					
Liquidity	36%	54%	76%	83%	85%
Credit Risk	63%	47%	24%	17%	15%
Misreporting	-30%	-23%	-14%	-8%	-6%

NOTE: The top panel shows the average level of each of the indicated components of the average LOIS spread at each maturity, reported in percentage points. The second panel shows the unweighted average value of each component when normalized by the contemporaneous value of the bias-corrected LOIS spread. The contributions are calculated using the medians of the posterior distributions of the estimates.

(b) Variance Decomposition

	1 wk	1 mn	3 mn	6 mn	12 mn
Liquidity	0.074	0.063	0.064	0.058	0.024
Credit Risk (ϕ and $\overline{C}$)	0.096	0.096	0.098	0.1	0.107
ϕ only	0.038	0.039	0.042	0.046	0.057
$\overline{C}$ only	0.012	0.012	0.012	0.012	0.012
Liq/Credit risk Cov	-0.047	0.006	0.088	0.107	0.073
Liquidity	60%	38%	26%	22%	12%
Credit Risk (ϕ and $\overline{C}$)	78%	58%	39%	38%	53%
ϕ only	31%	23%	17%	17%	28%
$\overline{C}$ only	10%	7%	5%	5%	6%
Liq/Credit risk Cov	-38%	4%	35%	40%	36%

NOTE: The table shows the contribution of each of the indicated components to the overall time-series variance of the average LOIS spread at each maturity. Contributions are calculated using the medians of the posterior distributions of the estimates. Units in the top panel are percentage points squared. The contributions of liquidity, credit risk, and their covariance sum identically to the total variation in LOIS. The contributions of the credit-risk components, ϕ and $\overline{C}$, are based on linear approximations. The bottom panel expresses the variance decomposition as a percentage of the total variance of each LOIS spread.

Table II: External Validation of λ

	1wk			1mn			3mn		
Intercept	0	0.02**	0.01	0	0.01**	0.01**	0.01**	0.01**	0.01**
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
T-bill spread [a]	0.28***		0.28***	0.16***		0.16***	0.12***		0.12***
	(0.05)		(0.05)	(0.03)		(0.03)	(0.03)		(0.03)
TAF Total Capacity		-0.66***	-0.69***		-0.76***	-0.73***		-0.06	-0.1
		(0.24)	(0.23)		(0.22)	(0.21)		(0.15)	(0.15)
Error AR(1)	0.9	0.88	0.89	0.95	0.96	0.95	0.98	0.98	0.98
Adj. R^2 [b]	0.36	0.57	0.66	0.34	0.82	0.83	0.25	0.07	0.32
N. Obs.	308	308	308	308	308	308	308	308	308

	6mn			12mn [c]		
Intercept	0.01***	0.01***	0.01***	0.00***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
T-bill spread	0.11***		0.11***	-0.15***		-0.17***
	(0.04)		(0.04)	(0.03)		(0.03)
TAF Total Capacity		0.21**	0.20***		0.29***	0.31***
		(0.09)	(0.09)		(0.05)	(0.05)
Error AR(1)	0.99	0.99	0.99	0.99	0.98	0.98
Adj. R^2 [b]	0.09	0.69	0.7	0.06	0.94	0.95
N. Obs.	308	308	308	264	264	264

NOTE: Regression performed using Cochrane and Orcutt (1949) procedure to account for serial correlation in the errors. Asterisks indicate significance at the 10(*), 5(**) and 1(***) percent levels, respectively. *a*: In the regression for the 1-week horizon, we use the 1-month Treasury note-bill spread, which is the shortest maturity available, all other maturities are matched exactly. *b*: Adjusted R^2 for Cochrane-Orcutt procedure excludes contribution of lagged error term. *c*: 12-month data only available since 2008.

Table III: Checking for Contamination from CDS Illiquidity

	λ^{6M}		λ^{12M}		ϕ	
	Full sample	Since 2010	Full sample	Since 2010	Full sample	Since 2010
Intercept	0.01***	0.01***	0.02***	0.01***	0.01**	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
6m Avg. fit err.	−0.01	0.02			0.39	0.23*
	(0.12)	(0.03)			(0.48)	(0.14)
6m b/a		−0.15				−0.76
		(1.31)				(15.5)
12m Avg. fit err.			−0.08	−0.11 *	−0.52	−0.15
			(0.13)	(0.06)	(0.65)	(0.28)
12m b/a				1.41		3.18
				(2.62)		(27.90)
Error AR(1)	0.99	0.99	0.97	0.99	0.95	0.96
Adj. $R^{2\ a}$	0.00	0.01	0.00	0.17	0.01	0.12
N obs	308	167	308	167	308	167

NOTE: Regression performed using Cochrane and Orcutt (1949) procedure to account for serial correlation in the errors. The labels "6m Avg. fit err." and "12m Avg. fit err." correspond to the mean absolute fitting error of the Nelson-Siegel curve used to fit the CDS data; they are the mean from the cross-sectional distribution (of sample banks) at the 6- and 12-month maturity, respectively. Asterisks indicate significance at the 10(*), 5(**) and 1(***) percent levels, respectively. *a*: Adjusted R^2 for Cochrane-Orcutt procedure excludes contribution of lagged error term.

Table IV: Model Results – Without Controlling for Misreporting

(a) Relative Contributions of Model Components

	1wk	1 mn	3 mn	6 mn	12 mn
Average value					
Liquidity	0.196	0.245	0.439	0.646	0.849
Credit Risk	0.002	0.002	0.002	0.003	0.005
Misreporting	0	0	0	0	0
Average fraction of "true" LOIS spread					
Liquidity	96%	89%	100%	103%	101%
Credit Risk	0%	0%	0%	0%	0%
Misreporting	0%	0%	0%	0%	0%

(b) Variance Decomposition

	1 wk	1 mn	3 mn	6 mn	12 mn
Liquidity	0.123	0.153	0.227	0.236	0.165
Credit Risk	0	0	0	0	0.001
ϕ only	0.001	0.001	0.001	0.001	0.001
$\bar{C}$ only	0	0	0	0	0
Liq/Credit risk Cov	-0.008	0.001	-0.002	-0.004	0.006
Liquidity	107%	99%	101%	101%	96%
Credit Risk	0%	0%	0%	0%	0%
ϕ only	1%	0%	0%	0%	1%
$\bar{C}$ only	0%	0%	0%	0%	0%
Liq/Credit risk Cov	-7%	1%	-1%	-2%	4%

NOTE: Using the model that excludes the misreporting terms, the table shows the contribution of each of the indicated components to the overall time-series variance of the average LOIS spread at each maturity. Units in the top panel are percentage points squared. The contributions of liquidity, credit risk, and their covariance sum identically to the total variation in LOIS. The contributions of the credit-risk components, ϕ and $\overline{C}$, are based on linear approximations. Units in the top panel are percentage points squared. The bottom panel expresses the variance decomposition as a percentage of the total variance of each LOIS spread.

Table V: Decomposition of Model Results Across Various Time Periods

		Beg. of Crisis	TAF	Fed Facilities	SCAP	European Crisis
Start		7/30/07	12/3/07	9/29/08	4/27/09	7/25/11
End		8/31/07	1/4/08	11/7/08	5/22/09	9/9/11
LOIS	1w	0.5	−0.27	−2.01 ***	−0.05	0.01
	1m	0.53**	−0.65 ***	−1.00 ***	−0.12	0.04
	3m	0.54***	−0.35 *	−0.63 ***	−0.31 *	0.11
	6m	0.59***	−0.22	−0.36 *	−0.33 **	0.13
	12m	0.45***	−0.11	−0.28 *	−0.35 ***	0.17
λ	1w	0.44*	−0.13	−1.90 ***	0.25	−0.08
	1m	0.46***	−0.51 ***	−0.78 ***	0.16	−0.05
	3m	0.47***	−0.20 **	−0.33 ***	−0.04	0.01
	6m	0.51***	−0.07	0.06	−0.06	0.02
	12m	0.37***	0.05	0.30***	−0.05	0.05
Total Credit Risk	1w	0.06	−0.11	−0.17 **	−0.37 ***	0.12
	1m	0.06	−0.06	−0.21	−0.37 ***	0.12
	3m	0.06	−0.04	−0.31 *	−0.37 ***	0.13
	6m	0.07	−0.01	−0.44 ***	−0.37 ***	0.13
	12m	0.09	−0.01	−0.64 ***	−0.36 ***	0.14
$\bar{C}$	1w	0.01	−0.03	0.16	−0.73 ***	0.96***
	1m	0.01	−0.03	0.14	−0.74 ***	0.97***
	3m	0.02	−0.02	0.07	−0.75 ***	0.99***
	6m	0.02	−0.02	−0.01	−0.76 ***	1.01***
	12m	0.02	−0.01	−0.13	−0.75 ***	1.01***
ϕ		0.53***	−0.65 ***	−0.47 **	−0.09	0.09

Notes: The table shows cumulative changes in the LOIS spread and CDS spread data and in the medians of the posterior distributions of our state-variable estimates during certain episodes of interest. Asterisks indicate values that are in the top or bottom 5, 2.5, and 0.5 percentiles, based on the distributions observed during our sample period. LOIS, liquidity, and CDS spreads are reported in percentage points.

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A Derivation of Measurement Equations

Each bank's first-order condition is:

$$\gamma_{1imt} - \gamma_{2t}\left(\frac{\widehat{L}_{imt} - L_{imt}}{\text{std}_{mt}\left[\widehat{L}_{imt}\right]}\right) - \gamma_{3t}\left(\frac{\widehat{L}_{imt} - \overline{\widehat{L}}_{mt}}{\text{std}_{mt}\left[\widehat{L}_{imt}\right]}\right) = 0. \tag{A.1}$$

Solving for $\widehat{L}_{imt}$ and substituting equation (1) (imposing $\phi_{imt} = \phi_t$ for all i and m), gives

$$\widehat{L}_{imt} = \frac{\gamma_{1imt}\text{std}_{mt}\left[\widehat{L}_{imt}\right] + \gamma_{2t}\left(\lambda_{mt} + \phi_t C_{imt}\right) + \gamma_{3t}\overline{\widehat{L}}_{mt}}{\gamma_{2t} + \gamma_{3t}}. \tag{A.2}$$

Averaging across banks and rearranging delivers

$$\overline{\widehat{L}}_{mt} = \lambda_{mt} + \phi_t \overline{C} + \frac{\overline{\gamma}_{1t}}{\gamma_{2t}}\text{std}_{mt}\left[\widehat{L}_{imt}\right] \tag{A.3}$$

where $\overline{\gamma}_{1t}$ is the cross-sectional mean of γ_{1i}. Substituting back into equation (A.2),

$$\widehat{L}_{imt} = \lambda_{mt} + \phi_t\frac{\gamma_{2t}C_{imt} + \gamma_{3t}\overline{C}_{mt}}{\gamma_{2t} + \gamma_{3t}} + \left(\frac{\gamma_{1imt}}{\gamma_{2t} + \gamma_{3t}} + \frac{\gamma_{3t}\overline{\gamma}_{1mt}}{\gamma_{2t}(\gamma_{2t} + \gamma_{3t})}\right)\text{std}_{mt}\left[\widehat{L}_{imt}\right]. \tag{A.4}$$

The cross-sectional variance is therefore

$$\begin{aligned}\text{var}_{mt}\left[\widehat{L}_{imt}\right] &= \left(\frac{\phi_t\gamma_{2t}}{\gamma_{2t} + \gamma_{3t}}\right)^2(\sigma^C_{mt})^2 + \frac{\text{var}_{mt}\left[\widehat{L}_{imt}\right]\text{var}_{mt}\left[\gamma_{1imt}\right]}{(\gamma_{2t} + \gamma_{3t})^2} \\ &= \frac{(\phi_t\gamma_{2t})^2}{(\gamma_{2t} + \gamma_{3t})^2 - \text{var}_{mt}\left[\gamma_{1imt}\right]}(\sigma^C_{mt})^2. \end{aligned} \tag{A.5}$$

Thus, we can write equation (7), by defining

$$\beta_{1imt} = \left(\frac{\gamma_{1imt}}{\gamma_{2t} + \gamma_{3t}} + \frac{\gamma_{3t}\overline{\gamma}_{1mt}}{\gamma_{2t}(\gamma_{2t} + \gamma_{3t})}\right)\frac{\phi_t\gamma_{2t}}{\sqrt{(\gamma_{2t} + \gamma_{3t})^2 - \text{var}_{mt}\left[\gamma_{1imt}\right]}} \tag{A.6}$$

and

$$\beta_{2t} = -\frac{\phi_t\gamma_{3t}}{\gamma_{2t} + \gamma_{3t}}. \tag{A.7}$$

Online Appendix for:

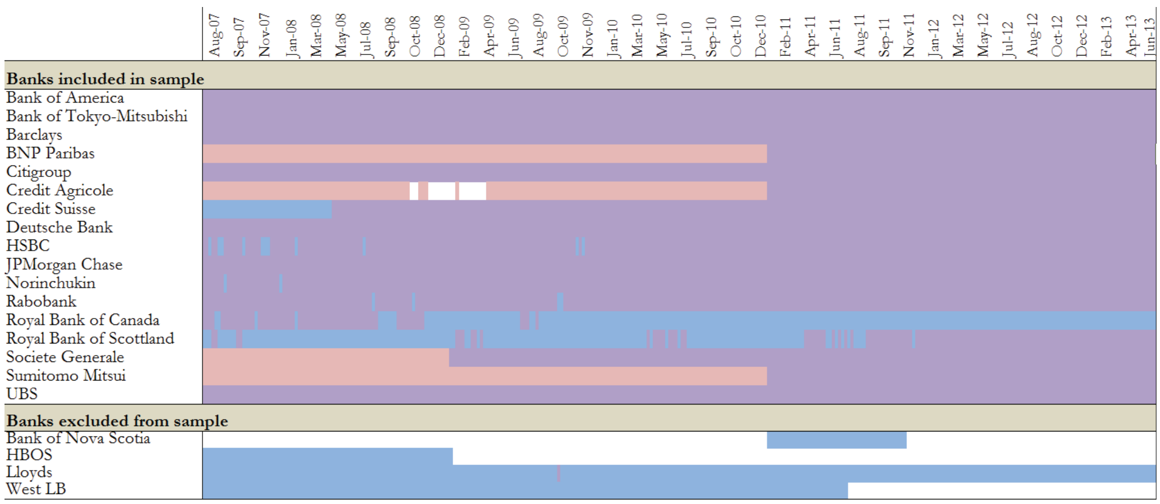
Credit Risk, Liquidity, and Lies

by: Kurt F. Lewis and Thomas B. King

A Data and CDS Curves

Figure A.1 shows week-by-week breakdown of the data used in the model in the weeks for which each bank was a member of the USD Libor panel (in blue) and therefore has Libor-submission data available; the weeks for which short-maturty CDS data are available for each bank (in red); and the weeks for which both the CDS and Libor data are available (in purple). Blank cells indicate that a bank was neither in the Libor panel nor had CDS data available that week.

Figure A.1: Data summary



NOTE: The weeks for which each bank was a member of the USD Libor panel are colored blue and therefore has Libor-submission data available. The weeks for which short-maturty CDS data are available for each bank are colored red, and the weeks for which both the CDS and Libor data are shown as purple. Blank cells indicate that a bank was neither in the Libor panel nor had CDS data available that week.

We obtain daily bank-level Libor submissions from Bloomberg for every USD Libor panel bank at maturities of 1 week, 1 month, 3 months, 6 months, and 1 year. These submissions are available on all weekdays, excluding U.K. holidays. As noted in the text, we subtract maturity-matched USD OIS rates, also obtained from Bloomberg, from each Libor submission on each day. We then take weekly averages of each bank's LOIS spread at each maturity. Figure A.1 shows the composition of the Libor panel during the sample period used in the estimation, July 30, 2007, through June 28, 2013. (Figure 1a in the text shows some of the daily data dating back to when they first become available.) The blue and purple regions indicate the weeks during which each bank was in the panel.

We obtain daily bank-level CDS quotes from Markit for each of the Libor panel banks at maturities of 6 months and 1, 2, 3, 4, 5, 7, 10, 15, 20, and 30 years, when those quotes are available. Markit surveys over 30 dealers for end-of-day indicative quotes and reports an aggregate of each underlying CDS name at each maturity. They exclude contributor quotes that they judge to be stale or that are outliers in the cross section, and they do not report a composite CDS spread for a given maturity on a given day if they do not have at least two underlying quotes that meet these standards. The data are available on all weekdays, including U.S. and U.K. holidays (although there tends to be very little movement on days such as Christmas). We drop all bank-day observations

Table A.1: Average Nelson-Siegel CDS fitting errors

	6-month	12-month
Ave. abs. fitting error	1.8 bp	2.9 bp
As % of ave. level	2.7%	3.9%

for which Markit does not report both 6- and 12-month spreads and all bank-day observations for which it does not report spreads for at least five different maturities in total. The red and purple regions in Figure A.1 indicate the weeks during which we have CDS observations for each bank after applying these filters. (Figure 1b in the text shows some of the daily data dating back to when they first become available, although the summary statistics shown there are based on fewer banks as they go further back in time.)

We have at least some CDS quotes for 17 of the 21 banks that were ever in the Libor panel during our period, but the four remaining banks never show up in our CDS data. (One of these banks, Lloyds, does have CDS quotes on a single day, which we drop as an anomaly.) While our Kalman filtering procedure can handle missing data, as described below, it does not add information to the estimation to include banks for which the independent variables are always missing. We therefore exclude these four banks altogether. Because three of them were only in the Libor panel for part of the sample period, the associated data amounts to only 12 percent of all bank-week LOIS observations.

For each bank in our CDS data, we fit a curve of the Nelson and Siegel (1987) form to the cross-section of CDS quotes on each day. Specifically, we estimate

$$C_{imt} = b_{0,it} + b_{1,it}\frac{1-e^{-m/\tau}}{m/\tau} + b_{2,it}(\frac{1-e^{-m/\tau}}{m/\tau} - e^{-m/\tau}) \tag{A.1}$$

where C_{imt} is bank i's CDS quote at maturity m on day t, and $b_{0,it}$, $b_{1,it}$, $b_{2,it}$, and τ_{it} are bank-day-specific parameters. We fit the curve to the full term structure of CDS quotes in our data, minimizing the weighted sum of the squared errors across maturities, where the weights are proportional to $1/m$. We use fitted values from these curves (on the days that we can estimate them) to match the maturities of our LOIS data for each bank.

Table A.1 displays the average absolute fitting errors from this procedure, both in absolute terms and as a fraction of the level of the raw CDS spreads. The curves generally fit quite well at the short end. Nonetheless, one potential concern is that, especially given the exponential terms in the Nelson-Siegel specification, our procedure could generate explosive behavior out of sample and thus introduce substantial measurement error for very short maturities. While we of course have no way of verifying the accuracy of our extrapolation, we can check that it does not generate values that appear implausible, in the sense of being too far out of line with the observed data at the short end. To do this, we use the raw six-month, 12-month, and two-year CDS data to compute measures of the level, slope, and curvature of the short-maturity CDS curve. We then compare these to the level, slope, and curvature computed using our extrapolated Nelson-Siegel data. Specifically, for level, we compare C_{6mn} in the raw data to C_{1wk} in the fitted values; for slope, we compare $(C_{1yr} - C_{6mn})$ in the raw data to $(C_{6mn} - C_{1wk})$ in the fitted values; for curvature, we compare $((C_{2yr} - C_{1yr})/2) - (C_{1yr} - C_{6mn})$ in the raw data to $((C_{1yr} - C_{6mn}) - (C_{6mn} - C_{1wk}))$ in the fitted values.

Table A.2: Differences between CDS term-structure features in extrapolated and raw data

	Percentile						
	1	5	25	50	75	95	99
Level	-23.4	-8.3	0.1	6.4	13.6	26.3	34.7
Slope	-17.3	-9.8	-3.1	0.5	4.4	11.8	24.6
Curvature	-80.9	-30.1	-8.1	-1.8	4.7	19.9	56.4

Notes: The table shows percentiles of the distributions of the differences between the Nelson-Siegel-imputed CDS curves at very short maturities and the raw CDS curves at slightly longer maturities. Specifically, the "level" row compares raw 6-month quote to the imputed 1-week quote; the "slope" row compares the difference between the 6-month and the 12-month quotes in the raw data to the difference between the 1-week and 6-month imputed quotes; the "curvature" row compares the 6-month/12-month/2-year second difference in the raw data (adjusted for the different period lengths) to the 1-week/6-month/12-month second difference in the imputed quotes.

Table A.2 shows the distributions of the differences between the extrapolated curve features and those at the short end of the raw data. The first row shows that the imputed one-week spread differs from the raw six-month spread by less than 27 basis points for more than 90 percent of the observations. (For context, the middle 90 percent of the raw six-month CDS spreads is 9 to 201 basis points.) The second row shows that the imputed 1-week/6-month slope differs from the raw 6-month/12-month slope by less than 12 basis points for more than 90 percent of the observations. The final row shows that the imputed 1-week/6-month/12-month curvature differs from the raw 6-month/12-month/2-year curvature (adjusted for the difference in period lengths) by less than 31 basis points for more than 90 percent of the observations. These statistics are not suggestive of wild swings in the extrapolated data. As an additional measure to guard against noise potentially introduced by our curve fitting, in the estimation we drop all observations for which the percentage fitting error at either the 6- or 12-month maturity is greater than 25%.

B Measures of CDS Liquidity

To further investigate concerns about the potential for CDS illiquidity to contaminate our results, we constructed two measures of CDS liquidity. These measure are used in the validation exercises in Subsection 5.3.2 of the paper. Here, we briefly describe the data.

First, we exploit the errors in our Nelson-Siegel CDS curves. As discussed above, we calculate the absolute value of the percentage deviation of each raw 6- and 12-month CDS quote from the fitted curves on each day. This measure can be computed for each bank in our sample on each day when CDS spreads are reported. To construct a time-series index, we take the median across banks, on each day at each of the two maturities. Fitting errors from similar curves are often taken to be proxies for market functioning and liquidity, since we would generally expect arbitrage to result in relatively smooth term structures (e.g., Hu et al., 2013; Musto et al., 2018).

The second measure of illiquidity we use is the bid/ask spread on CDS contracts. Markit constructs average bid/ask spreads, for each CDS name at each maturity on each day that they receive sufficient quotes. Unfortunately, Markit only began collecting these data in 2010, and only for the dominant currency for each CDS name. In practice, the latter condition means that we only have observations for the three US banks in the Libor panel. Nonetheless, one might expect CDS liquidity to be highly cross-sectionally correlated across banks. Therefore, we construct indices of bank-CDS liquidity at the six- and twelve-month horizons by averaging the three bid/ask spreads that we observe at each of those maturities on each day. Summary statistics for the two short-term CDS liquidity measures are presented in Table B.3 below.

Table B.3: Summary Statistics for CDS liquidity Measures

		Mean	Std. Dev.	Min.	Max.
Median abs. % fitting	6m	3.50%	4.20%	0.00%	28.90%
error from NS curve	12m	4.10%	2.20%	0.10%	11.20%
Ave. bid/ask spread across	6m	0.25%	0.10%	0.13%	0.59%
3 banks (starts 2010)	12m	0.14%	0.06%	0.07%	0.34%

C Estimation Procedure

Our estimation applies the Kalman filter to the linear state-space system described by the measurement and state transition equations equation (8) and equation (9). The fixed parameters are estimated via Gibbs sampling, following Kim and Nelson (1999).

The specific structure of the estimated model can be written as follows. Stacking the data across firms and maturities at each point in time, the measurement equations of the state-space representation equation (8) can be written compactly as

$$\widehat{\mathbf{L}}_t = \mathbf{X}_t \theta_t + \varepsilon_t \tag{C.1}$$

where $\widehat{\mathbf{L}}_t$ is the 85×1 vector of LOIS across banks and maturities (recall that we are using data from five different maturities for each of 17 firms), $\mathbf{X}_t$ is the matrix of independent variables, and θ_t is the vector of time-varying coefficients. $\mathbf{X}_t$ is 85×24 and has the structure

$$\mathbf{X}_t = \begin{pmatrix} \mathbf{I}_5 \otimes 1_{17} & \Sigma_t^C & \mathbf{C}_t & [\mathbf{C}_t - \overline{\mathbf{C}}_t] \end{pmatrix} \tag{C.2}$$

where $\mathbf{I}_k$ is the k-dimensional identity matrix, 1_{17} is a vector of ones of length 17, Σ_t^C is an 85×17 matrix that consists of 5 stacked copies of the 17×17 diagonal matrix with the cross-sectional standard deviations of CDS spreads for maturity m at time t (σ_{mt}^C) along the diagonal, $\mathbf{C}_t$ is the 85×1 vector containing the C_{imt}'s, and $\overline{\mathbf{C}}_t$ is a 85×1 vector containing the $\overline{C}_{mt}$'s stacked on top of each other (each $\overline{C}_{mt}$ is a 17×1 vector repeating the average CDS for the given maturity at the given date).

To deal with missing data, we follow the procedure of Aruoba et al. (2009). This process starts with the initial data that has missing values and uses a matrix, noted W_t in that paper, to eliminate missing observations, creating a situation where the left- and right-hand variables in the observation equation within the filter are of a different size in each period. W_t is created by beginning with a identity matrix of size 85×85, and then keeping only the rows from that matrix which correspond to the observed elements within the data for date t. Thus, we then use the Kalman filter and Gibbs procedure on $\widehat{\mathbf{L}}_t^W = W_t \widehat{\mathbf{L}}_t$ and $\mathbf{X}_t^W = W_t \mathbf{X}_t$, and note also that $\varepsilon_t^W = W_t \varepsilon_t$. More details are available in Aruoba et al. (2009) and included references.

We treat the coefficient vector as a hidden state vector that evolves according to equation (9), where $\mathbf{Q}$ is the 24×24 covariance matrix of innovations in the state transition equation. We assume that the measurement errors $\varepsilon_{\mathbf{t}}$ are identically and independently distributed normal random variables with mean zero and covariance matrix $\mathbf{R}$, and, in order to reduce the dimensionality of the estimation, we follow standard practice by assuming that $\varepsilon_{\mathbf{t}}$ and ν_t are uncorrelated. Further, we assume that the covariance matrices $\mathbf{R}$ and $\mathbf{Q}$ themselves are also diagonal. Note that, since $\mathbf{R}$ is diagonal by assumption, the measurement-error RMSE mentioned in Section 5.4 is equal to $\sqrt{tr[\mathbf{R}]/\mathbf{k}}$.

We assume that the hyperparameters $\mathbf{R}$ and $\mathbf{Q}$ and the initial state θ_0 are independent from each other, that the initial state is a normal random variable with mean $\bar{\theta}_0$ and covariance matrix $\bar{\mathbf{P}}_0$. We set the initial mean $\bar{\theta}_0$ to line up with a world in which the true LOIS spread is derived from the individual-firm CDS with identical recovery rates between bondholders and interbank lenders. That is, we set the mean of ϕ_0 equal to 1. In light of results by Youle (2014) and others, we set the mean of β_{1i0} equal to -1, implying a small amount of misreporting on average. However, we make these initial distributions quite flat, with a covariance of matrix of $\bar{\mathbf{P}}_0$ that has values of 10 along the diagonal and zero along the off-diagonals. (Reasonable variants on these choices do not change the qualitative results.) The prior parameterization of the hyperparameters **R** and **Q** are also set to diffuse values; each element of the diagonal is an inverse gamma with a single degree of freedom and shape parameters of 10^{-4}. By making these priors very flat, we allow the data to drive the shape and position of the posterior distributions. We use a two-step Gibbs algorithm: (1) states given hyperparameters and (2) hyperparameters given states. See Kim and Nelson (1999) for details concerning the construction of the posterior distributions.

All estimates reported in the text are based on posterior draws of the smoothed state vector (i.e., the distributions of the state conditional on the full-sample information). We take 250,000 Gibbs draws, of which we discard the first 5,000 as a "burn-in" sample. We checked convergence by increasing and decreasing the number of draws, by changing the starting values, and by examining the time series of the individual variables.

In the results presented in the main paper, we estimate the model on weekly averages of the daily data. If our specification were the true data-generating process, the frequency of the data used should make no systematic difference for our estimates. However, if the model is misspecified—say, because the true state variables do not follow pure random walks—different choices for the time aggregation can matter. Our choice of weekly data as the baseline is driven by several considerations. First, the daily data are not reported at a constant frequency because of weekends and holidays. This creates difficulties for the Kalman filter, which assumes that observations are equally spaced over time. Second, particular observations in the daily data may be driven by idiosyncratic events, such as quarter-end or reserve-maintenance reporting dates, that our model does not capture. Finally, the timing of the data (for example, when a day's CDS quotes are reported, relative to when Libor is posted) and the information flows among market participants at the intra-day horizon is somewhat unclear. In our model, we have assumed that banks have knowledge of the distribution of other banks' quotes when choosing their own, but at a daily frequency this might be unrealistic. By averaging across days, the weekly data smoothes out this microstructure variation, as well as other potential sources of high-frequency noise, while still allowing the parameters to move rapidly enough to realistically capture abrupt changes in market conditions.

C.1 Extra Step for Estimating Fixed-ϕ Model

Subsection 5.5 of the paper discusses an alternative model in which credit-risk sensitivity is fixed rather than time-varying. This model is estimated by adding another step to the Gibbs algorithm. That is, we extracted ϕ from the θ_t vector, creating θ_t^* (and Q^*), which continues to evolve according to equation (9) in the main paper, and then a fixed ϕ parameter was drawn in a separate step. The algorithm (once initial draws are obtained from the prior distributions) is:

1. Draw path of $\theta_t^{*(i)}$ for all t, conditional on the data, $R^{(i-1)}$, $Q^{*(i-1)}$, $\phi^{(i-1)}$;
2. Draw $\phi^{(i)}$ conditional on the data, $R^{(i-1)}$, $Q^{*(i-1)}$ and the path of $\theta_t^{*(i)}$ for all t;
3. Draw $R^{(i)}$ conditional on the data, $Q^{*(i-1)}$, $\phi^{(i)}$ and the path of $\theta_t^{*(i)}$ for all t;
4. Draw $Q^{(i)}$ conditional on the data, $R^{*(i)}$, $\phi^{(i)}$ and the path of $\theta_t^{*(i)}$ for all t.

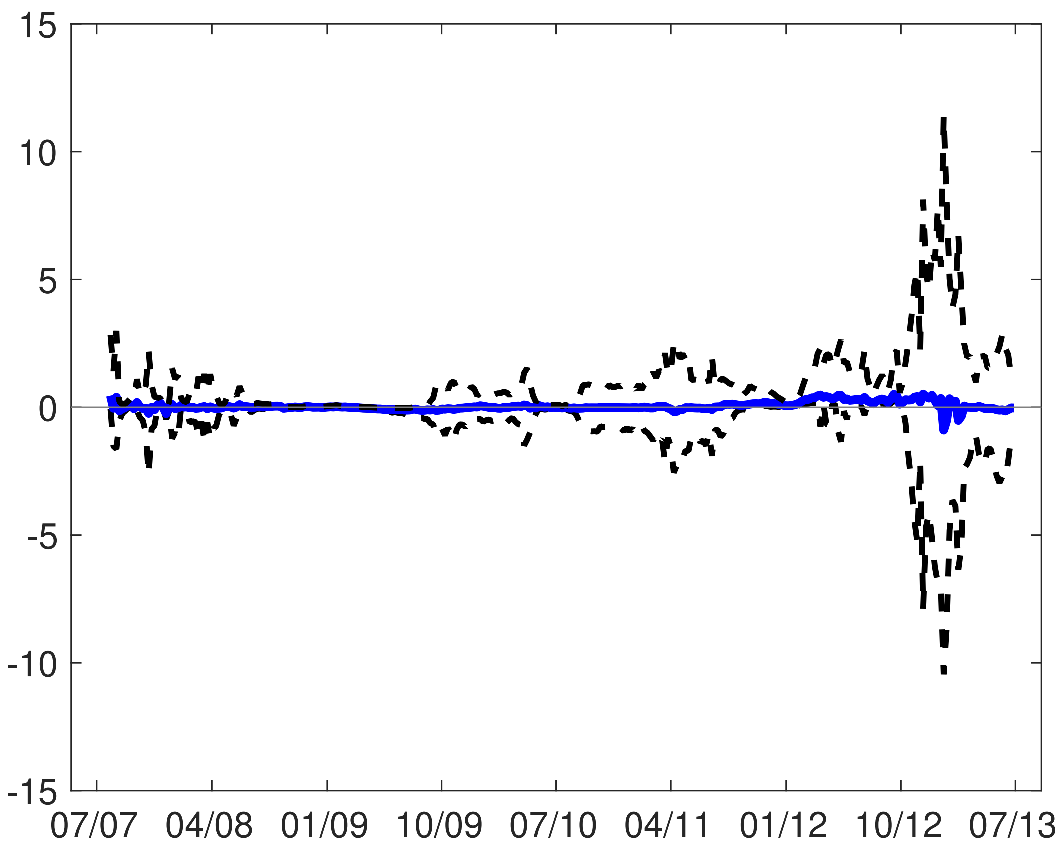
This algorithm adds step 2 to the original process and allows us to find the posterior distribution for the fixed ϕ parameter, shown in the main text as Figure 6.

D Additional Results and Robustness Checks

D.1 Ratio of Misreporting Costs in Baseline Model

Subsection 5.1 one the paper reported on the fact that the perceived cost of differing from other banks was always much greater than the perceived cost of lying *per se*. All else equal, a value of γ_{2t}/γ_{3t} close to zero would imply that banks do not vary their reported Libor quotes commensurately with their credit risk, but that they would all want to report similar values. (Because only the ratio can be identified, all we can say from this is that $\gamma_{2t} \ll \gamma_{3t}$.). Figure D.1 shows the time series estimate of the γ_{2t}/γ_{3t} ratio.

Figure D.1: Estimated path of the ratio of misreporting costs ($\gamma_{2,t}/\gamma_{3,t}$), with 5th and 95th percentiles.



D.2 Liquidity Spread Validation Regressions

Subsection 5.3.1 of the paper contains regressions of our model's liquidity premium estimates on external measures of funding-market liquidity. Because of the high serial correlation of the errors, in the text we report the results using Cochrane-Orcutt estimation. For completeness, in Table D.1 we report the same regressions using OLS with Newey-West standard errors. (Note that the R^2 in these regressions is biased upward.)

Table of the paper, and Table below include joint estimation of the affect of T-bill spreads and TAF total capacity. Given that they are both being used as measures of external validation of our liquidity proxy, concerns about multicolinearity are diminished by examining the regressions in two steps. The first two columns for each maturity show regressions where the T-bill spread and the TAF capacity are performed independently, with the final column showing the joint regression. The results are generally similar.

Table D.1: External Validation of λ, using OLS with Newey-West

	1wk			1mn			3mn		
Intercept	0.02	0.13***	0.08***	0.03	0.18***	0.06***	0.12***	0.28***	0.11***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
T-bill spread [a]	0.19***		0.22***	0.43***		0.45***	0.92***		0.89***
	(0.06)		(0.06)	(0.05)		(0.05)	(0.05)		(0.06)
TAF Total Capacity		-0.52***	-0.55***		-0.35***	-0.41***		0.31*	0.19*
		(0.09)	(0.08)		(0.09)	(0.07)		(0.17)	(0.10)
Adj. R^2	0.05	0.15	0.21	0.28	0.08	0.39	0.62	0.06	0.64
Durbin-Watson	0.2	0.24	0.23	0.14	0.09	0.17	0.24	0.03	0.24
N. Obs.	308	308	308	308	308	308	308	308	308

	6mn			12mn [b]		
Intercept	0.34***	0.41***	0.27***	0.72***	0.63***	0.65***
	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)
T-bill spread	1.61***		1.48***	-0.38		-0.47*
	(0.17)		(0.15)	(0.44)		(0.25)
TAF Total Capacity		0.74***	0.70***		0.64***	0.64***
		(0.06)	(0.05)		(0.03)	(0.03)
Adj. R^2	0.32	0.39	0.66	0	0.84	0.85
Durbin-Watson	0.11	0.02	0.2	0.01	0.08	0.09
N. Obs.	308	308	308	264	264	264

NOTE: Regression performed using standard OLS with the Newey-West procedure to account for serial correlation in the errors. Asterisks indicate significance at the 10(*), 5(**) and 1(***) percent levels, respectively. *a*: In the regression for the 1-week horizon, we use the 1-month Treasury note-bill spread, which is the shortest maturity available, all other maturities are matched exactly. *b*: 12-month data only available since 2008.

D.3 Sensitivity to CDS Liquidity Metrics

Subsection 5.3.2 of the paper contains regressions of our model estimates on external measures of CDS liquidity. Because of the high serial correlation of the errors, in the text we report the results using Cochrane-Orcutt estimation. For completeness, in Table D.2a we report the same regressions using OLS with Newey-West standard errors. (Note that the R^2 in these regressions is biased upward.)

Table D.2: Checking for Contamination from CDS Illiquidity

(a) Procedure: OLS with Newey-West

	λ^{6M}		λ^{12M}		ϕ	
	Full sample	Since 2010	Full sample	Since 2010	Full sample	Since 2010
Intercept	0.55**	0.27***	0.80***	0.65***	0.20	0.14
	(0.27)	(0.03)	(0.11)	(0.06)	(0.15)	(0.18)
6m fit err.	−1.33	0.05			−1.38	0.81***
	(1.58)	(0.11)			(3.64)	(0.24)
6m b/a		28.70***				−6.84
		(11.00)				(58.60)
12m fit err.			−2.72 *	−1.18 *	1.46	−2.95 *
			(1.42)	(0.66)	(9.83)	(1.69)
12m b/a				−16.90		54.50
				(20.50)		(142.10)
Adj. R^2	0.05	0.27	0.15	0.39	0.01	0.40
Durbin-Watson	0.02	0.08	0.06	0.07	0.11	0.16
N obs	309	168	309	168	309	168

(b) Procedure: Cochrane and Orcutt (1949)

	λ^{6M}		λ^{12M}		ϕ	
	Full sample	Since 2010	Full sample	Since 2010	Full sample	Since 2010
Intercept	0.01***	0.01***	0.02***	0.01***	0.01**	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
6m fit err.	−0.01	0.02			0.39	0.23*
	(0.12)	(0.03)			(0.48)	(0.14)
6m b/a		−0.15				−0.15
		(1.31)				(0.28)
12m fit err.			−0.08	−0.11 *	−0.52	−0.76
			(0.13)	(0.06)	(0.65)	(15.50)
12m b/a				1.41		3.18
				(2.62)		(27.90)
Error AR(1)	0.99	0.99	0.97	0.99	0.95	0.96
Adj. $R^{2\ a}$	0.00	0.01	0.00	0.17	0.01	0.12
N obs	308	167	308	167	308	167

NOTE: *a*: Adjusted R^2 for Cochrane-Orcutt procedure excludes contribution of lagged error term. Asterisks indicate significance at the 10(*), 5(**) and 1(***) percent levels, respectively.

D.4 Estimation Using Only 5-Year CDS Spreads

Table D.3 presents a version of the results shown in Table I for the specification where the full term structure of CDS were replaced with just 5-year CDS.

Table D.3: Model Results – Using Only 5-Year CDS

(a) Relative Contributions of Model Components

	1wk	1 mn	3 mn	6 mn	12 mn
Average value					
Liquidity	0.054	0.122	0.304	0.497	0.702
Credit Risk	0.384	0.384	0.384	0.384	0.384
Misreporting	-0.248	-0.248	-0.248	-0.248	-0.248
Average fraction of "true" LOIS spread					
Liquidity	28%	39%	58%	69%	74%
Credit Risk	72%	61%	42%	31%	26%
Misreporting	-64%	-58%	-44%	-31%	-22%

Note: The top panel shows the average level of each of the indicated components of the average LOIS spread at each maturity, reported in percentage points. The second panel shows the unweighted average value of each component when normalized by the contemporaneous value of the bias-corrected LOIS spread. The contributions are calculated using the medians of the posterior distributions of the estimates.

(b) Variance Decomposition

	1 wk	1 mn	3 mn	6 mn	12 mn
Liquidity	0.097	0.067	0.031	0.017	0.012
Credit Risk	0.219	0.219	0.219	0.219	0.219
ϕ only	0.185	0.185	0.185	0.185	0.185
$\bar{C}$ only	0.02	0.02	0.02	0.02	0.02
Liq/Credit risk Cov	-0.186	-0.116	0.016	0.063	0.027

Note: The table shows the approximate contribution of each of the indicated components to the overall time-series variance of the average LOIS spread at each maturity. Units are percentage points squared. Contributions are calculated using the medians of the posterior distributions of the estimates.

D.5 Raw vs. Smoothed CDS quotes

We do not find large fitting errors for the 6- and 12-month CDS spreads, even on the days of the greatest dislocation in our sample. For horizons less than 6 months, we use the curves to extrapolate, and there is no direct way to validate the accuracy of this procedure since CDS quotes shorter than 6 months do not exist. However, the good in-sample fit of the curves gives us some comfort, and the slope and curvature of our fitted curves at the short end do not behave in erratic ways.

To test whether our results were being driven by the smoothing procedure, we input the raw 6- and 12-month CDS quotes into our estimated model, in place of the smoothed quotes at those maturities. If the model were very sensitive to the Nelson-Seigel approach, applying the model to the raw quotes instead of the Nelson-Seigelized quotes would give significantly different predicted values for the corresponding maturities of LOIS. Figure D.2 shows that this is not the case by showing the *very small* differences in implied credit risk under raw vs. Nelson-Siegel curve CDS results. At most, the estimates of the credit-risk and misreporting components differ briefly by a few basis points. Figure D.3 shows that the effect of exchanging raw for Nelson-Siegel curve CDS is similarly minuscule for the measure of misreporting as well.

Figure D.2: Credit Risk Measure when replacing Nelson Siegel CDS spreads with raw CDS spreads

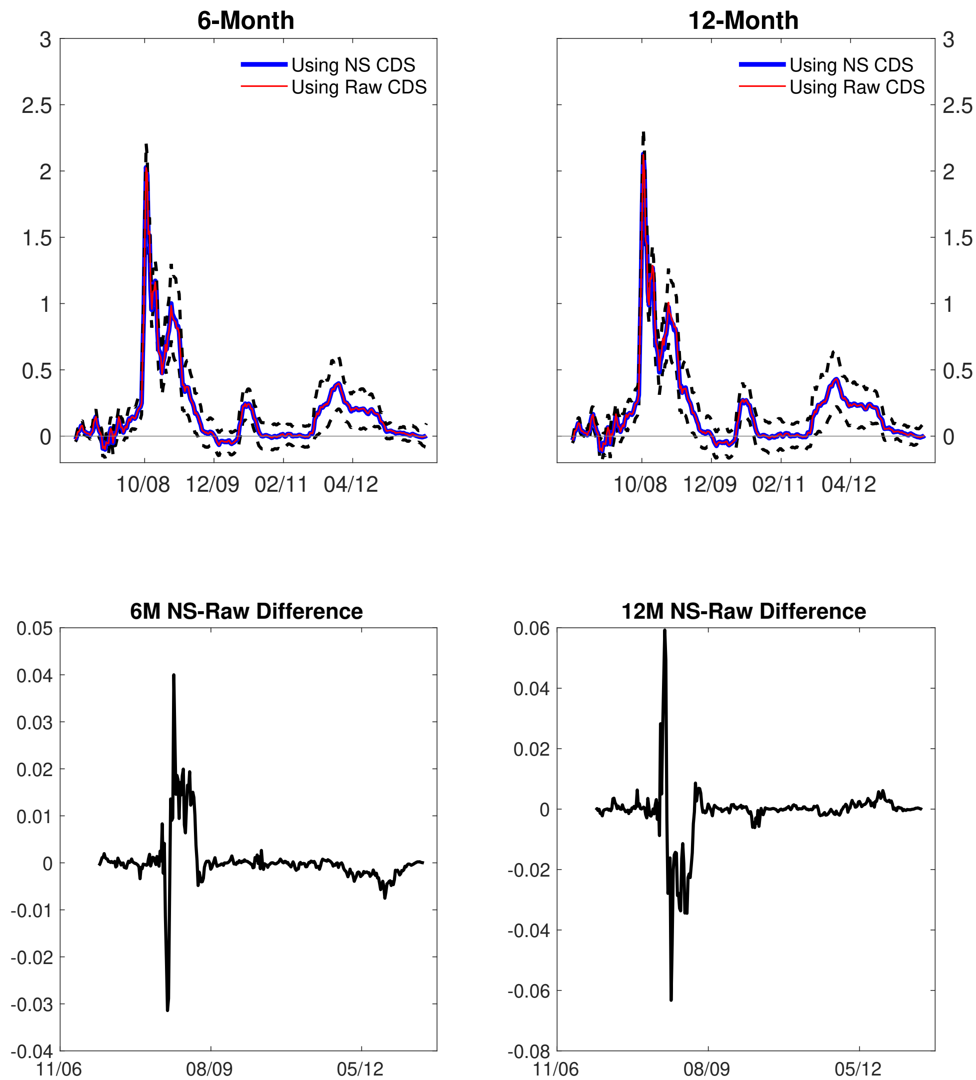
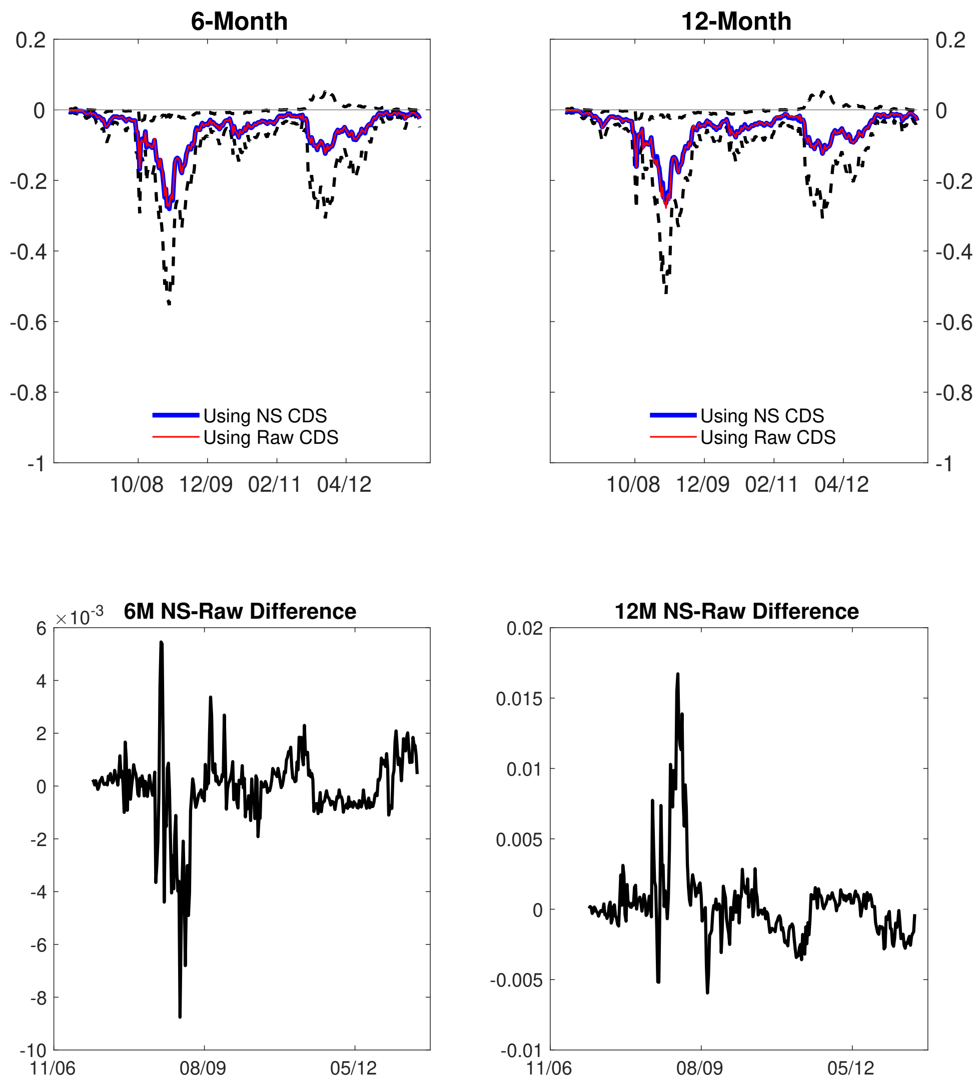


Figure D.3: Misreporting Measure when replacing Nelson Siegel CDS spreads with raw CDS spreads



D.6 Fixed-ϕ Estimation

Table D.4 presents a version of the results shown in Table I for the model with fixed credit risk sensitivity.

Table D.4: Model Results – Fixed-ϕ

(a) Relative Contributions of Model Components

	1wk	1 mn	3 mn	6 mn	12 mn
Average value					
Liquidity	0.098	0.164	0.342	0.528	0.718
Credit Risk	0.145	0.146	0.151	0.16	0.178
Misreporting	-0.067	-0.068	-0.068	-0.069	-0.072
Average fraction of "true" LOIS spread					
Liquidity	19%	36%	59%	74%	80%
Credit Risk	75%	61%	39%	25%	19%
Misreporting	-36%	-29%	-18%	-11%	-8%

Note: The top panel shows the average level of each of the indicated components of the average LOIS spread at each maturity, reported in percentage points. The second panel shows the unweighted average value of each component when normalized by the contemporaneous value of the bias-corrected LOIS spread. The contributions are calculated using the medians of the posterior distributions of the estimates.

(b) Variance Decomposition

	1 wk	1 mn	3 mn	6 mn	12 mn
Liquidity	0.11	0.14	0.18	0.173	0.11
Credit Risk	0.015	0.015	0.015	0.015	0.015
ϕ only	0	0	0	0	0
$\bar{C}$ only	0.015	0.015	0.015	0.015	0.015
Liq/Credit risk Cov	-0.012	-0.002	0.038	0.061	0.068

Note: The table shows the approximate contribution of each of the indicated components to the overall time-series variance of the average LOIS spread at each maturity. Units are percentage points squared. Contributions are calculated using the medians of the posterior distributions of the estimates.