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Thomas B. King and Kurt F. Lewis

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Credit Risk, Liquidity, and Lies [1]

Thomas B. King [a] and Kurt F. Lewis [b]

[a] *Federal Reserve Bank of Chicago, Chicago, Illinois 60604*

[b] *Federal Reserve Board, Washington, DC 20551*

Abstract

We reexamine the relative effects of credit risk and liquidity in the interbank market using bank-level panel data on Libor submissions and CDS spreads. Our model synthesizes previous work by combining the fundamental determinants of interbank spreads with the effects of strategic misreporting by Libor-submitting firms. 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.

Key words: LIBOR, Liquidity, Credit Risk, Misreporting, Bank Funding
JEL: E43, G21, L14

Email addresses: `thomas.king@chi.frb.org` (Thomas B. King), `kurt.f.lewis@frb.gov` (Kurt F. Lewis).

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During the financial crisis that began in 2007, bank funding markets came under extraordinary pressure, threatening both the functioning of the financial system and the transmission of monetary policy. Perhaps the most frequently cited measures of the strains in these markets were the spreads between the London Interbank Offer Rate (Libor) and comparable-maturity rates that could be considered "risk free," such as the rate on overnight index swaps (OIS). Indeed, at the height of the turmoil, the one-month Libor-OIS spread, which was typically less than 10 basis points prior to mid-2007, rose to nearly 300 basis points. In response to these strains, the Federal Reserve and central banks abroad introduced a number of short-term lending facilities for injecting liquidity into funding markets.

Because of the critical role of bank funding in the functioning of financial markets and the stability of financial institutions, several papers have studied the behavior of this market during the crisis, and the policy responses to its breakdown, both in the United States and abroad. In one of the first and best known of these studies, Taylor and Williams (2009) argued that wide interbank spreads mostly reflected 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] The stakes of this debate are high, as the outcome could dictate the nature of the policy response to the *next* banking crisis. Under the liquidity view, the Federal Reserve and its counterparts abroad essentially got it right—strains in funding markets can be relieved by massive injections of liquidity. Under the credit-risk view, policymakers wanting to resolve funding stress should instead focus their efforts on improving the solvency of the banking system through recapitalization, purchases of distressed assets, or loan guarantees. In addition to informing the design of policies, understanding the dynamics of funding-market stress is also important for assessing theoretical models of the interbank market in which credit risk and liquidity play a role.[3]

In this paper, we study this question using matched micro data on individual banks' USD Libor quotes and credit default swap (CDS) spreads across a spectrum of maturities over

[2] Afonso et al. (2011), Angelini et al. (2011), and Smith (2012), like Taylor and Williams, have emphasized 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 (2014) estimate a large liquidity premium in interbank spreads; Acharya and Merrouche (2013) document liquidity hoarding by large settlement banks; and Wu (2011), Rai (2013), and Christensen et al. (2014) all find that central bank liquidity facilities significantly reduced bank funding rates.

[3] E.g., Eisenschmidt and Tapking (2009), Heider et al. (2015), and Acharya and Skeie (2011).

the period 2007 - 2013. The cross-sectional dimensions of our data introduce a considerable amount of information that has been largely unexploited by previous papers attempting to differentiate the effects of credit risk and liquidity in interbank markets. In particular, the relationship between bank-level CDS and Libor spreads enables us to identify the pricing of credit risk independently of the time-series correlations between these series. Indeed, our estimation employs a time-varying parameter model that allows the relationship between CDS and Libor spreads to change at each point in time. Afonso et al. (2011) and Angelini et al. (2011) present evidence that lending banks may have paid more attention to credit risk at some times during the crisis than at others, but previous studies employing aggregate Libor and CDS spreads have assumed the relationship between those variables to be constant.

We also confront another issue that has been ignored by the above-mentioned studies but that becomes particularly important when moving to the bank-level data: Libor is based on a survey of banks that essentially report on an honor system. While this method of reporting has the advantage that Libor data is available even in the absence of regular volumes in the interbank lending market, it also comes with a cost. Specifically, it is now well known that at least some of the 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.[4] A few papers have recently begun to model aspects of the game played by banks when choosing how to report, but none has yet considered the implications for the credit-liquidity debate. In our analysis, we estimate and correct for the effects of lying about Libor by assuming that the costs and benefits of misreporting have a simple linear-quadratic structure. Our model of reporting incentives is similar to Youle (2014), Snider and Youle (2012) and Chen (2013), who also adopt a quadratic cost of deviating from the truth, but we add to their framework the possibility that banks face a cost of submitting a Libor quote that is much different from the average of other banks' quotes on the same day. This piece of the model, which captures a bank's reluctance to appear as an outlier, provides one mechanism through which Libor quotes can be highly correlated with aggregate credit risk even though they are not highly correlated with individual credit risk—a striking and otherwise puzzling feature of the panel data.

Our results generally underscore the importance of liquidity in determining funding costs. On average over our sample, the estimated liquidity premium constitutes the majority of the funding spread 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

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

liquidity facilities were in force, 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: in late-2007, every 1 basis point change in a bank's CDS spread translated into a 4.3 basis-point change in its funding costs. But for most of our sample period—in particular, since 2009—this sensitivity was close to zero. Consequently, although we estimate that fluctuations in credit risk were responsible for some of the largest movements in interbank spreads during the crisis, on average this component makes up only about a fifth of the total Libor-OIS spread.

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. The average effect is a more-modest -6 basis points across all maturities, but we show that controlling for it is nonetheless important for obtaining the credit and liquidity results discussed above. In terms of the parameters of our structural model, we estimate that banks consistently perceived the cost of reporting a value different from the average of other banks as being significantly greater than the cost of reporting a value different from the truth, explaining why the time-series correlation between Libor quotes and CDS spreads is much higher than the cross-sectional correlation.

Our paper generalizes previous credit-liquidity decompositions of aggregate bank-funding spreads in three ways. First, we use substantially more data. Our use of panel data greatly increases the power and flexibility of our identification. [5] Our inclusion of data through 2013 allows us to examine the post-crisis behavior of these markets, including their response to the strains that began in Europe in 2011, in contrast to most previous studies, which end in 2008 or 2009. Second, we correct for misreporting. This correction is crucial to fitting the cross-sectional dimension of the data and, as we show, also has first-order effects on the aggregate results. Finally, we allow the sensitivity of Libor to credit risk to vary over time,

[5] A few papers in the credit-liquidity literature (e.g., Filipović and Trolle (2013), Christensen et al. (2014)) use aggregate Libor data at multiple maturities but 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.) The only paper in the credit-liquidity literature to use the bank-level micro data is Gefang et al. (2011), but they do not match individual CDS and Libor spreads as we do. Rather, they model credit risk as a single unobserved factor, which drives both Libor and CDS with different loadings at each bank. Thus, their identification comes mostly from the time-series component of the data. Finally all of these 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 important for the liquidity results in particular.

whereas previous studies of this relationship—for example, the many papers that simply regress aggregate Libor spreads on aggregate CDS indices—have treated it as constant. Our finding that sensitivity varies significantly suggests that those models are misspecified. Our results in this dimension complement those of Afonso et al. (2011). In particular, using entirely different data, we confirm their result that attention to credit risk in the interbank market increased in the immediate aftermath of the Lehman Brothers default, even though, as noted above, we also find that this attention eventually returned to negligible levels.

We begin in the following section by reviewing the construction and matching of the Libor and CDS data, and we motivate our theoretical and empirical model of misreporting by pointing out certain features of these data that are hard to reconcile.

1 Background and Data

1.1 Construction of the Data

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. 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) and five currencies.[6] During our sample, the individual bank-level quotes were also immediately made public, although this practice changed in 2013.

The primary data used in this paper consist of the daily bank-level Libor submissions and matched CDS spreads on a subset of the dollar-Libor-panel banks for maturities up to one year. We obtain dollar-denominated CDS quotes for the senior debt of the banks from Markit and the Libor data from Bloomberg. 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.[7] We refer to

[6] The composition of the survey panel varies across currencies and also 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.
[7] For example, see Taylor and Williams (2009). OIS rates are also obtained from Bloomberg.

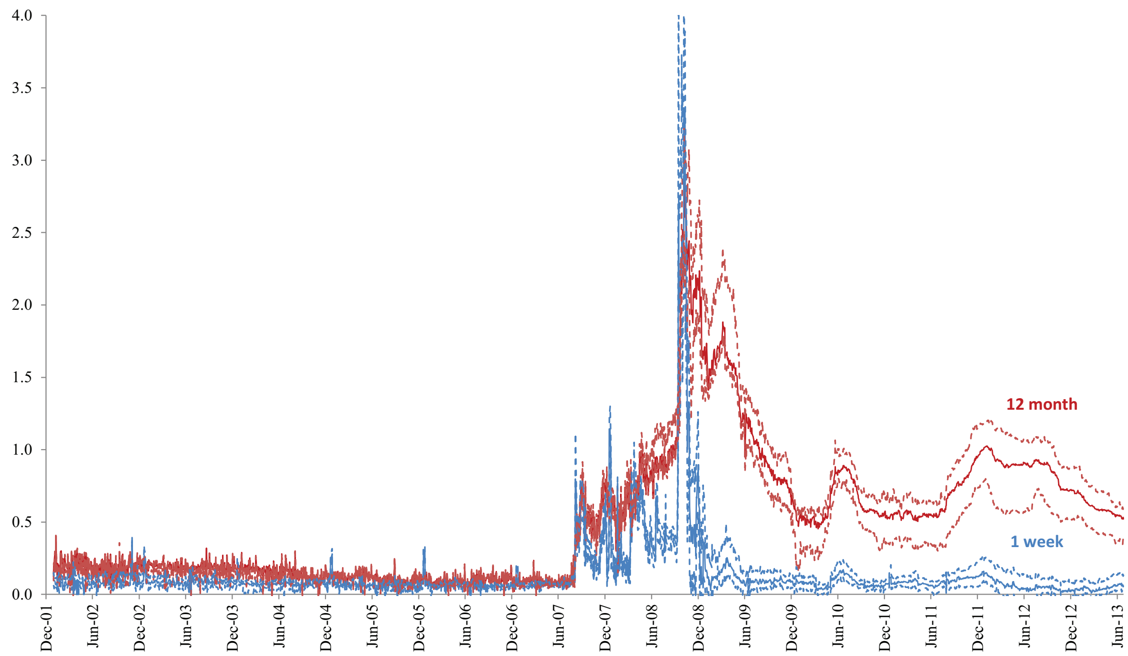
the difference between Libor quotes and OIS rates as "LOIS" spreads.

In our empirical work, we start the sample in August 2007. Prior that date, as shown below, 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 reporting procedure instituted at that date. Over the intervening period, 21 different banks participated in the panel at some point, with 16 to 20 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 Markit does not report quotes for contracts that are not traded in sufficient depth, and, in order to ensure accuracy at short maturities, we exclude any observation for which Markit does not provide quotes for both a 6- and 12-month contract. In total, we have 5,268 bank-week observations of LOIS spreads, of which we drop 12 percent because they come from banks for which short-maturity CDS were never traded. In the remaining sample, 9 percent of the observations are missing CDS data. When estimating our model, we treat those observations using the interpolation procedures available within the Kalman filter, as described later. The online appendix provides additional detail on the composition of the Libor panel and our sample.

For accurate comparison, we need to match the maturity of the CDS and Libor submissions, but CDS are not generally quoted for maturities shorter than six months. Therefore, on each day, we fit a Nelson-Siegel (1987) curve to the full term structure of each bank's CDS (6 months to 30 years), weighting the estimation by inverse maturity. The absolute fitting errors for these curves average just 1.9 and 3.0 basis points at the six- and twelve-month horizons, respectively, indicating that they accurately capture the short end of the term structure. We use fitted values from these curves to extract implied CDS spreads at maturities of one week through one year. See the online appendix for more detail.

A potential difficulty with using the CDS data at short maturities is that poor liquidity in that segment of the market may introduce measurement error. Several considerations mitigate this concern. In general, CDS written on large banks are among the most heavily traded single-name CDS contracts. For example, during the week of July 16, 2010 (approximately the mid-point of our sample), DTCC data show 59,961 outstanding contracts written on our sample banks; eight of our banks were among the 100 most-traded corporate CDS names by gross notional amount. Although much of this volume reflects activity in the highly liquid five-year tenor, we should nonetheless expect that short-maturity contracts for these banks are more liquid than is typical. In addition, as mentioned above, Markit performs a preliminary filtering of the data to exclude quotes that are based on too few market makers or

Fig. 1. Dispersion of Libor-OIS Submissions (percentage points)



that appear stale. Indeed, our CDS sample exhibits significant time-series and cross-sectional variation, indicating that market participants are actively pricing these contracts.

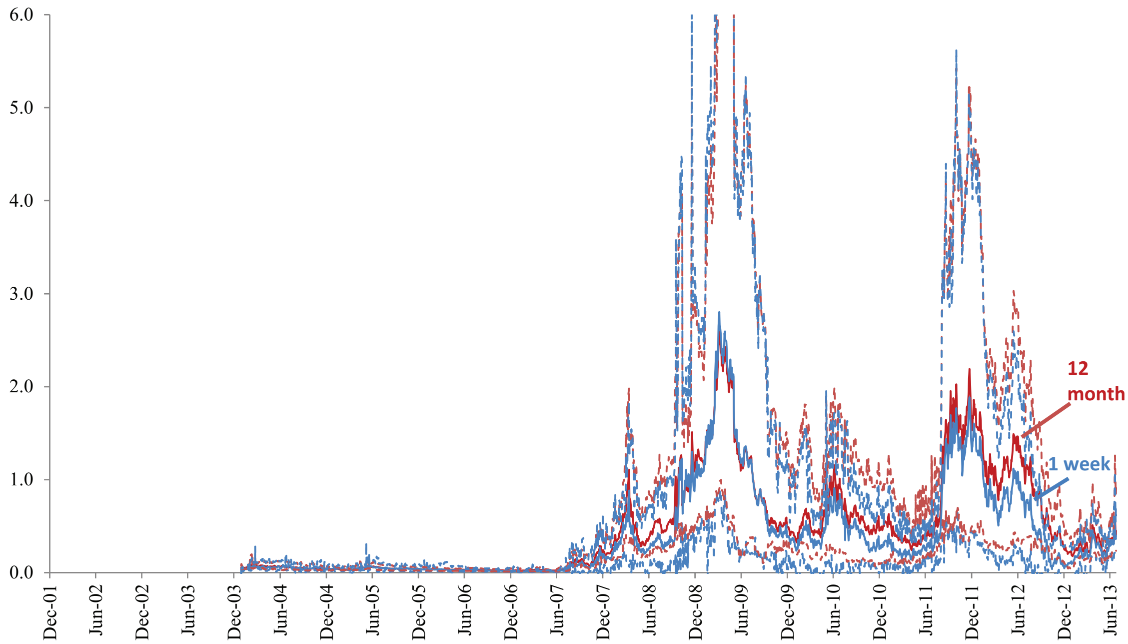
1.2 Empirical Features of Libor and CDS Spreads

To illustrate some key features of these data, Figures 1 and 2 plot 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. They both are very close 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 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 signifi-

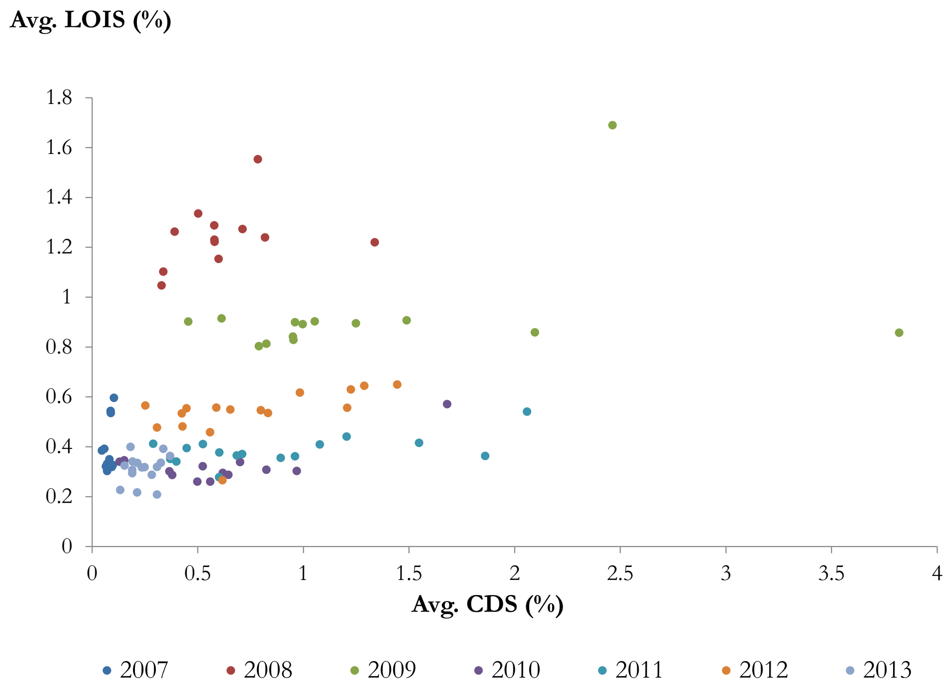
Fig. 2. Dispersion of Nelson-Siegelized CDS Quotes (percentage points)



cantly, 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 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, while that in CDS spreads 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 does not peak until mid-2009; and 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 does increase 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.

Figure 3 illustrates the cross-sectional relationship between LOIS and CDS by scatter-plotting the yearly average of the two series, year by year, for the six-month maturity. It is clear from these figures that the cross-sectional correlation between LOIS and CDS spreads

Fig. 3. Cross-sectional Relationship between 6-month Libor-OIS and Corresponding CDS



is weak at best.[8] Indeed, daily cross-sectional correlations between LOIS and CDS spreads average less than 0.15 at all maturities.

The weak correlation between LOIS and CDS spreads in the cross section is puzzling because, broadly speaking, interbank lenders to a given bank and the writers of CDS contracts on that bank face losses in the same states of the world.[9] This strict correspondence may be muddled by variations in failure-resolution statutes across countries and in corporate structure across institutions. (For example, the relationship between the Libor-reporting banking entity and the parent company on which CDS are typically written may differ depending on both factors.) Nevertheless, a bank's default on one claim is nearly always associated with defaults on other claims, particularly those of similar maturity, so we would still generally

[8] Michaud and Upper (2008) also note the weak cross-sectional correlation between CDS and Libor quotes.

[9] 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, at least in principle, receive the same *pro rata* recovery rate. See Federal Deposit Insurance Act Section 11(d).

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 overall risk of the banking sector but not for the risk of the particular banks they are lending to.

It is tempting to appeal to information asymmetries to explain these discrepancies, as in the model of Heider et al. (2015). But this cannot be the whole story because all interbank lenders generally have access to the same 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 very banks that are also making markets in CDS, and the same information sets should thus be priced into both instruments. 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.

1.3 Libor Misreporting

In the spring of 2008, the *Wall Street Journal* published a series of articles calling into question the veracity of Libor quotes.[10] 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

[10] 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.

and traders that have come to light through the recent legal investigations, in which Libor submitters repeatedly comply to requests from traders at their banks for particular configurations of Libor quotes.[11]

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.[12] Meanwhile, U.K. regulators have undertaken a set of reforms of the reporting process intended to discourage future misreporting [Wheatly (2012)]. A few academic studies have attempted to uncover evidence of the misreporting *ex post* in the data [Abrantes-Metz et al. (2012); Gandhi et al. (2013); Kuo et al. (2012); Snider and Youle (2012)]. Yet these studies have had less to say about the quantitative effects on the aggregate Libor rate and fairly little to say about how those effects could vary over time or how they might affect our interpretation of the interbank-market fundamentals considered in this paper.

1.4 Deviating from the Truth vs. Deviating from the Pack

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); Youle (2014)].[13] 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. However, one possibility is that bankers were at least as concerned about appearing to be different from their peers as they were about lying *per se*. Indeed, investigations into the Libor scandal support the suggestion 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.[14] In a phone conversation with an analyst at the Federal Reserve Bank of New York in April 2008, a Barclay's trader stated, "And so we just fit in with the rest of the crowd, if you like.... So,

[11] 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. (2012) estimate that the panel banks collectively gained $23 billion from position-driven manipulation over the 2005-2009 period.

[12] We note that some of the penalties and prosecutions have to do with Libor misreporting in currencies other than U.S. dollars, which we do not analyze.

[13] As we do, these studies focus on the cross-sectional variation in Libor panel submissions.

[14] These examples are drawn from transcripts and other documents released by government agencies. See the references in Arvedlund (2014) for the specific sources.

we know that we're not posting, um, an honest Libor. And yet—and yet we are doing it, because, um, if we didn't do it, it draws, um, unwanted attention to ourselves." In another 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...

The bankers in these examples display little concern for sticking to the truth. Instead, they appear mainly worried about drawing scrutiny by reporting numbers different from other banks—either too high or too low. [15]

If reporting banks perceive a cost to showing up as outliers, they have an incentive to write down numbers that are close to the numbers they believe other banks are going to write down on any given day. As a result, average CDS spreads can act as an attractor for LOIS spreads, even if individual bank CDS are only weakly correlated with individual LOIS. Thus perceived costs of being an outlier may explain why the Libor quotes are less dispersed than the CDS quotes and how LOIS and CDS spreads can be strongly correlated in the time series even though they are not strongly correlated in the cross section. It is difficult to match these features of the data in a model that has only a cost of lying. [16]

2 A Model of Credit Risk, Liquidity, and Misreporting

In this section, we develop a model of Libor determination to help motivate and interpret our empirical tests. Although it is quite stripped down, the funding-cost portion of the model shares the intuition of many other models of the interbank market, with banks choosing their reserve holdings to cover expected liquidity needs [e.g., Allen et al. (2009); Eisenschmidt and Tapking (2009)]. The misreporting portion borrows features from Snider and Youle (2012) and Chen (2013), but, as noted above, also introduces an incentive for banks not to deviate

[15] On yet another occasion, a different UBS senior manager explained that misreporting occurred because "...the whole street was doing the same and because [UBS] did not want to be an outlier in the LIBOR fixings, just like everybody else."

[16] An additional reason that banks may have attempted to report Libor numbers similar to their peers is that they were engaged in outright collusion in an attempt to influence aggregate Libor rates in order to boost their portfolio returns, activity that has also come to light in post-crisis legal investigations. The model we present in the next section is also consistent with this possibility.

too far from other banks when they report.

Consider a representative risk-neutral bank with unit measure of assets to invest and two investment opportunities: holding cash reserves or lending to I potential borrowing banks. We assume that the aggregate level of reserves is fixed by the central bank and that reserve holdings are remunerated at the gross risk-free rate R. To conserve notation, we develop the model for a contract of a single maturity (one "period"). We also suppress time subscripts for the moment, although in our estimation the structural parameters of the model will be allowed to change in every period and to differ across maturities.

The lending bank faces two sources of risk. First, any funds it lends in the interbank market may be defaulted on. Specifically, a loan to bank i defaults with probability ρ_i. In the event of default, the lending bank loses a fraction δ^L of the loan and interest. (For simplicity, we assume that these conditional loss rates are identical across borrowing banks, though this is not essential.) Thus, the expected return on a loan is

$$r_i = \frac{\left(\rho_i + (1-\rho_i)\delta^L\right)}{p_i^L} \tag{1}$$

where p_i^L is the price of an interbank loan to bank i that promises to pay \$1 in the non-default state. Collect the returns across borrowing banks in the vector $\mathbf{r}$ and the default probabilities in the vector ρ.

The second source of risk faced by a lending bank is a random cash outflow during the period over which its interbank loans are outstanding. Let this outflow be distributed over the support $[0, 1]$ with density f. If the cash outflow exceeds the level of reserves that the bank has chosen to hold, the bank pays a cost k. This cost could reflect the cost of asset fire-sales, borrowing from the central bank, or, in the extreme, bankruptcy. The implications of our model are essentially unchanged if we allow for multiple lending banks, each with a different distribution f.

Each lending bank chooses a fraction of its assets x to hold as reserves and, among the $1-x$ portion lent in the interbank market, it chooses portfolios weights $\mathbf{w} = \left(w_1 \; ... \; w_I \right)$ for each potential borrower. Thus, its objective function is

$$\max_{x,\mathbf{w}} \left\{ xR + (1-x)\,\mathbf{w'r} - k \int_x^1 f(\xi)d\xi \right\} \tag{2}$$

subject to $0 \leq x \leq 1$, $0 \leq w_i \leq 1 \; \forall i$, and $\sum_{i=1}^{I} w_i = 1$. The first-order condition immediately implies that the equilibrium price of a loan to any bank i satisfies

$$p_i^L = \frac{1 - \delta^L \rho_i}{R\left(1 + kf\left(x^*\right)\right)} \tag{3}$$

where x^* is the lending bank's optimal reserves holding.

Meanwhile, following similar logic, a CDS contract that insures \$1 of bonds against default for one period is priced as

$$p_i^C = 1 - \delta^B \rho_i \tag{4}$$

where δ^B is the conditional loss rate on bank bonds.[17] Note that we assume that bonds and interbank loans default in the same states of the world but may have different loss rates given default.

Since interest rates are given by negative log loan prices, for empirically relevant values of the parameters the spread of interbank loans over the risk-free rate can be written as

$$L_i \approx \lambda + \phi C_i \tag{5}$$

where $\lambda \equiv kf(x^*)$, $\phi \equiv \frac{\delta^L}{\delta^B}$, and $C_i = -\log p_i^C$ is the CDS spread for bank i. If one measures the risk-free rate as the OIS rate, (5) is essentially the specification estimated by Taylor and Williams (2009) and others, who have interpreted λ as reflecting funding-market liquidity and ϕC_i as reflecting counterparty credit risk. Our model gives the parameters of this reduced form a bit more structural footing. Specifically, in this stylized model, λ is the expected marginal cost of a cash shortfall among lending banks, and ϕ is the relative conditional loss rates for bondholders and interbank lenders.[18]

To allow for potential misreporting in this framework, we distinguish between each bank i's "true" funding cost L_i and the cost that it reports for Libor purposes $\widehat{L}_i$. The bank's reported value for Libor purposes may reflect several considerations, including that the bank

[17] In the presence of market frictions, of course, equation (4) might not hold exactly as a description of the relationship between CDS prices and actual credit risk. However, given the relative liquidity of the CDS market discussed above, alternative measures based on cash instruments (e.g. bonds, commercial paper) appear to contain at least as much credit-risk measurement error. For this reason, this literature has almost exclusively relied on CDS-based measures of credit risk for the banks in the Libor panel.

[18] Note that, so long as banks choose reserves such that cash outflows less than x^* are more likely than outflows greater than x^* [i.e., $f'(x^*) > 0$], the central bank can always (locally) reduce the liquidity premium by increasing the level of reserves available, since prices must adjust to make banks willingly hold these reserves in equilibrium. This is one mechanism through which liquidity facilities could help to lower interbank borrowing rates.

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 $\widehat{L}_i$ at each point in time. Thus, in any period, both incentives to misreport are captured by an expression of the form

$$\text{misreporting benefits}_i = \gamma_{0,i} + \gamma_{1,i}\widehat{L}_i \tag{6}$$

In theory, the marginal benefit of misreporting $\gamma_{1,i}$ 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, in part, the idea that the probability of a lie being detected is likely lower when there is more heterogeneity in the market. Thus, we have:

$$\text{cost of lying}_i = \frac{\gamma_2}{2}\frac{\left(\widehat{L}_i - L_i\right)^2}{\text{std}\left[\widehat{L}_i\right]} \tag{7}$$

For simplicity, we assume that the parameter γ_2 is the same across banks, although this assumption could be relaxed.

As discussed above, we also 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. We capture this incentive with a second cost function:

$$\text{cost of being an outlier}_i = \frac{\gamma_3}{2}\frac{\left(\widehat{L}_i - \overline{\widehat{L}}\right)^2}{\text{std}\left[\widehat{L}_i\right]} \tag{8}$$

where γ_3 is a cost parameter and $\overline{\widehat{L}}$ is the cross-sectional mean of reported Libor spreads. We assume that the moments $\overline{\widehat{L}}$ and $\text{std}[\widehat{L}]$ 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.[19], [20]

Each borrowing bank chooses $\widehat{L}_i$, to maximize benefits less costs in each period. One can show (see appendix A) that this maximization produces bank-level Libor quotes 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 (ignoring the approximation error due to Jensen's inequality in equation (5)):

$$\widehat{L}_i = \lambda + \phi C_i + \beta_{1i}\sigma^C + \beta_2 \left(C_i - \overline{C}\right) \tag{9}$$

where $\overline{C}$ and σ^C are the cross-sectional mean and standard deviation of CDS spreads, $\beta_2 = -\phi\gamma_3/(\gamma_2 + \gamma_3)$, and β_{1i} is a complicated function of the structural parameters, with the cross-bank variation deriving from γ_{1i}. It is equation (9) that will be the target of our estimation.

If banks always told the truth, $\widehat{L}_i$ would simply be equal to $\lambda + \phi C_i$. Thus, the last two terms in (9) reflect the bank-level misreporting bias. Note, however, that since the last term of (9) always averages to zero, the *aggregate* bias in Libor is simply equal to $\bar{\beta}_1\sigma^C$, where $\bar{\beta}_1$ is the cross-sectional average of the β_{1i} terms. The result that the size of the bias should increase with the cross-sectional dispersion of true funding costs also appears in Chen (2013), although it arises through a slightly different mechanism.[21]

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

[19] 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."

[20] Since we interpret γ_2 and γ_3 as costs, we expect them to be non-negative. While we will not impose it in our estimation, our results will indeed turn out to be statistically consistent with this restriction.

[21] Since our estimation will allow us to infer λ and ϕ on each day and at each maturity, equation (9) 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_i in real time. Of course, in this case, it would not make sense to consider reputational benefits of misreporting, but equation (6) still holds as an approximation to possible trading gains.

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_3. Consequently, we expect our model to produce unbiased and meaningful estimates of λ and ϕ even when there are no underlying interbank transactions.

3 Estimation

We estimate the model by treating equation (9) as a measurement equation for each bank at each maturity and allowing the parameters of these equations to vary over time. We introduce the subscript m to indicate a contract's maturity and the subscript t to indicate calendar time. (Thus, for example, bank i's m-period reported LOIS spread at time t is denoted $\widehat{L}_{imt}$.) Specifically, we estimate

$$\widehat{L}_{imt} = \lambda_{mt} + \phi_t C_{imt} + \beta_{1,it}\sigma^C_{mt} + \beta_{2,t}\left(C_{imt} - \overline{C}_{mt}\right) + \epsilon_{imt} \tag{10}$$

where ϵ_{imt} is a normally distributed iid error with variance matrix $\mathbf{R}$, which we assume to be diagonal. 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, $\beta_{1,it}$, and $\beta_{2,t}$ (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.

Note that we have imposed a few cross-equation restrictions to reduce the dimension of the system. First, we assume that ϕ_t is the same across all maturities. This assumption follows from our structural interpretation of ϕ as the ratio of recovery rates, together with an assumption that, given default, the recovery on a given claim is independent of the maturity of that claim.[22] Second, we assume that β_{2t} is the same across all maturities. This reduced-form parameter is a combination of ϕ, γ_2 and γ_3 in our model. Since we have already assumed that ϕ_t is independent of maturity, the assumption that β_{2t} does not vary with m is

[22] We also ran the model allowing ϕ to vary across maturities but found that the estimated extent of this variation was small and that the other results were little changed.

tantamount to assuming that the cost parameters γ_2 and γ_3 do not vary with m. We think this is a reasonable restriction if these costs reflect primarily reputational and regulatory penalties, since there is no obvious reason that markets or regulators should care about which maturities banks are lying about. Finally, we assume that β_{1it} is the same across all maturities for each bank. Given our other assumptions, maturity variation in this parameter could only come from variation in γ_{1i}. While it is in principle plausible that misreporting benefits could differ across maturities for a given bank, we also ran the model allowing this term to vary over m (expanding the number of state variables from 24 to 92), and the results of that estimation were very similar to those we report below, albeit with somewhat larger standard errors.

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:

$$\theta_t = \theta_{t-1} + \nu_t \tag{11}$$

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). The online appendix provides additional detail on the estimation procedure.

In our baseline results presented below, 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. 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.[23]

4 Results

4.1 State Variable Estimates

Figure 4 shows our smoothed estimates of the liquidity premia $\lambda_{m,t}$ 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 1 and 2, 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 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 through the end of the sample, the 6- and 12-month liquidity premia remain quite elevated relative to historical experience.

Figure 5 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 (based on the corresponding element of $\mathbf{Q}$) is 0.22.[24] Specifically, credit risk sensitivity spikes during two relatively brief episodes: at the beginning of the financial crisis, when we estimate ϕ_t to jump to a value of about 4.3, and immediately following the Lehman bankruptcy in 2008.[25] We discuss these results further in Section 4.3.

[23] Running the model on the daily data yields results that are qualitatively similar in most respects to those we report below, with the main exception being that the model has more trouble distinguishing between credit risk and misreporting at the height of the crisis. We have also run the model using monthly averages, rather than weekly, and again we find very similar results.

[24] This time-variation in the sensitivity to borrower credit risk could be consistent with the model of Heider et al. (2015) in which the incentive to monitor depends on the level and dispersion of credit risk.

[25] 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.

Fig. 4. Estimated path of liquidity premia (λ_t), by maturity, with 5th and 95th percentiles (shown in percentage points).

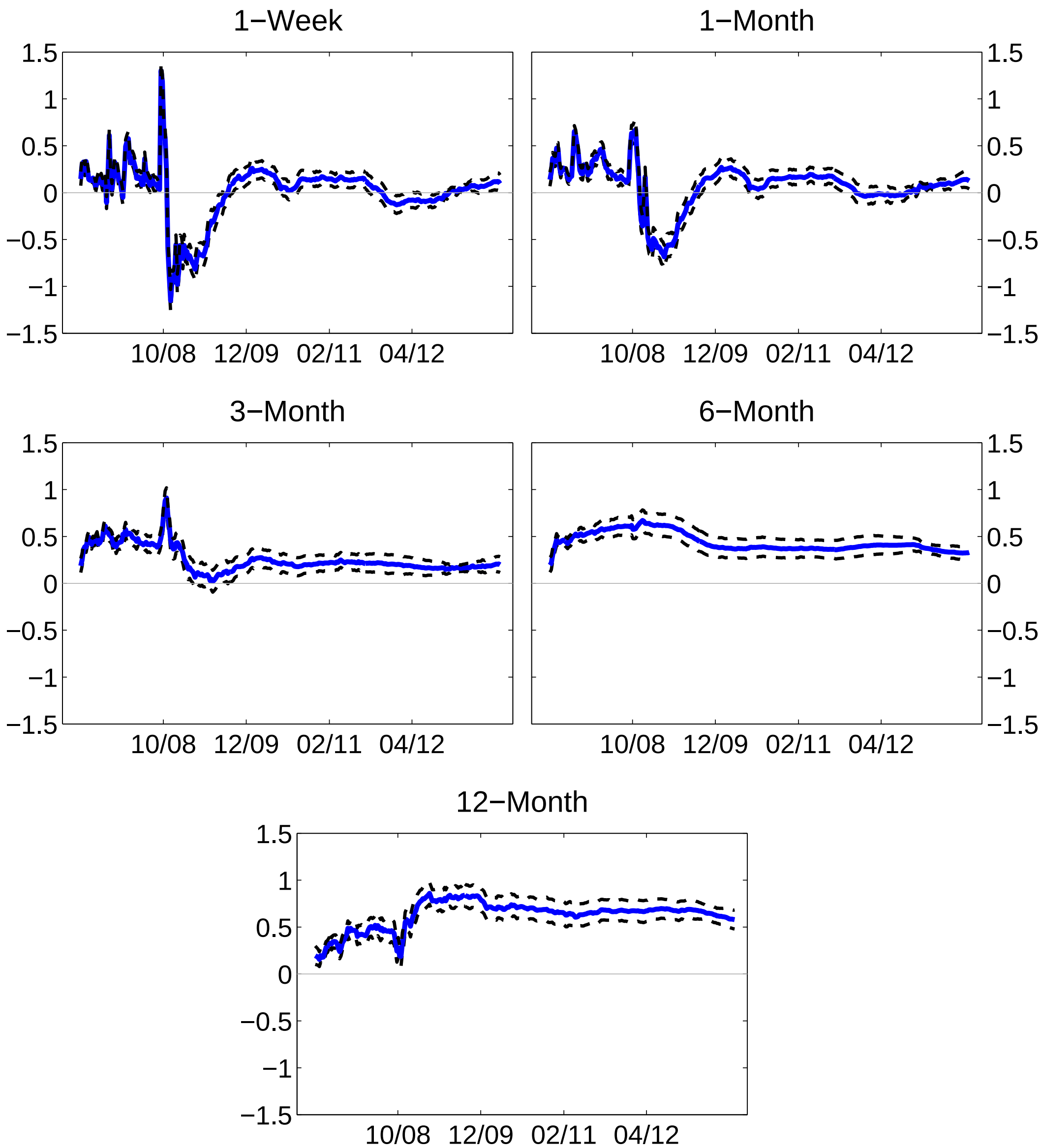
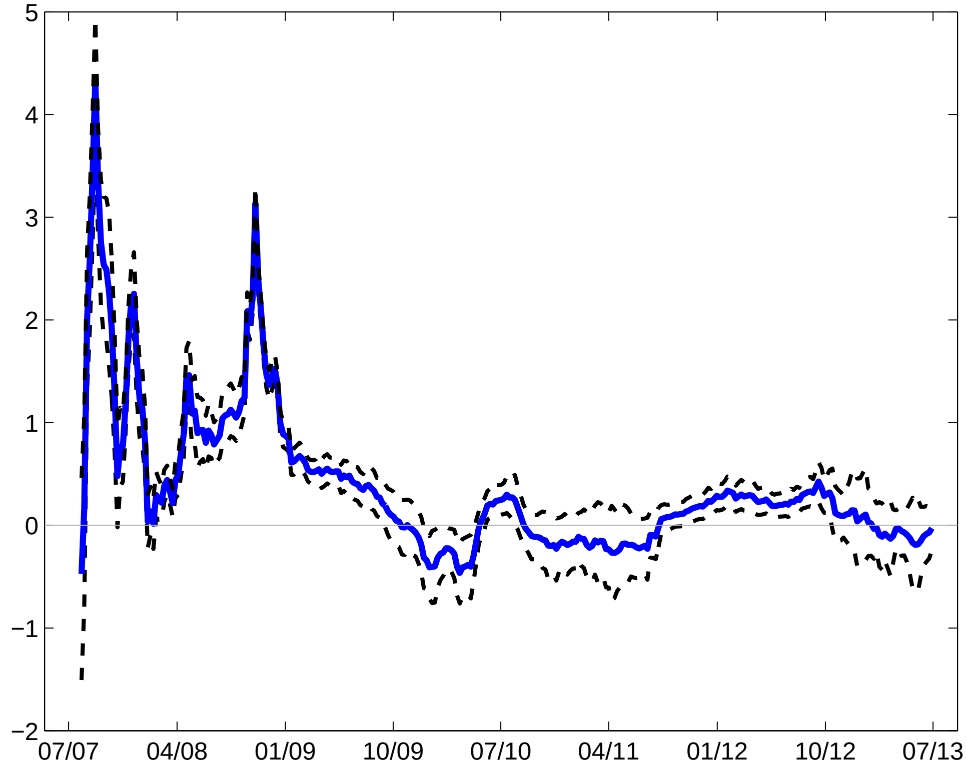


Figure 6 combines our point estimate of ϕ_t with the CDS data to produce the aggregate credit-risk components $\phi_t \overline{C}_{mt}$. Unlike liquidity, credit risk exhibits virtually no differences across maturities. This result is a consequence both of our assumption that relative conditional loss rates do not depend on maturity and the shape of the observed CDS curves, which, as noted earlier, tend to be quite flat at these maturities. Although the credit-risk spread contributes substantially to LOIS spreads during certain episodes, the time variation in 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

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

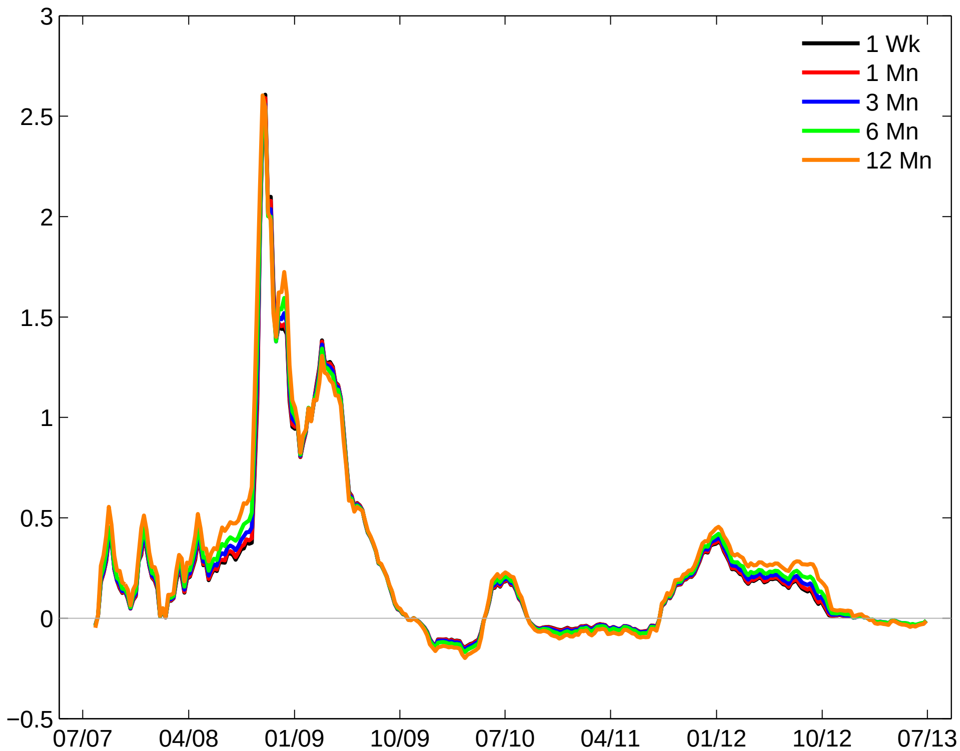


basis points.

Figure 7 shows the average misreporting bias $\overline{\beta}_{1,t}\sigma_t^C$ over time. We find that misreporting varies considerably across banks, maturity, and 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 about -6 basis points at all maturities. It attains its largest magnitudes of -30 to -35 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) (who finds an average misreporting of about -8 basis points) and Kuo et al. (2012) (who identify misreporting based on bids at the TAF and Fedwire microdata and find similar magnitudes relative to our own during 2008-2009). The finding that the bias was greater during the crisis period is consistent with opportunistic misreporting. However, we also estimate that misreporting biases were considerably smaller in the stress period of 2011 than they were in 2008-2009, even though the levels and dispersion of CDS spreads were similar in both periods, perhaps suggesting that banks have made more of an effort to report correctly in an environment of enhanced attention to this problem.

Recall that our estimate of the reduced-form parameter β_{2t} allows us to infer the ratio

Fig. 6. Aggregate Credit-Risk Component, $\phi_t \bar{C}_{mt}$ (shown in percentage points).

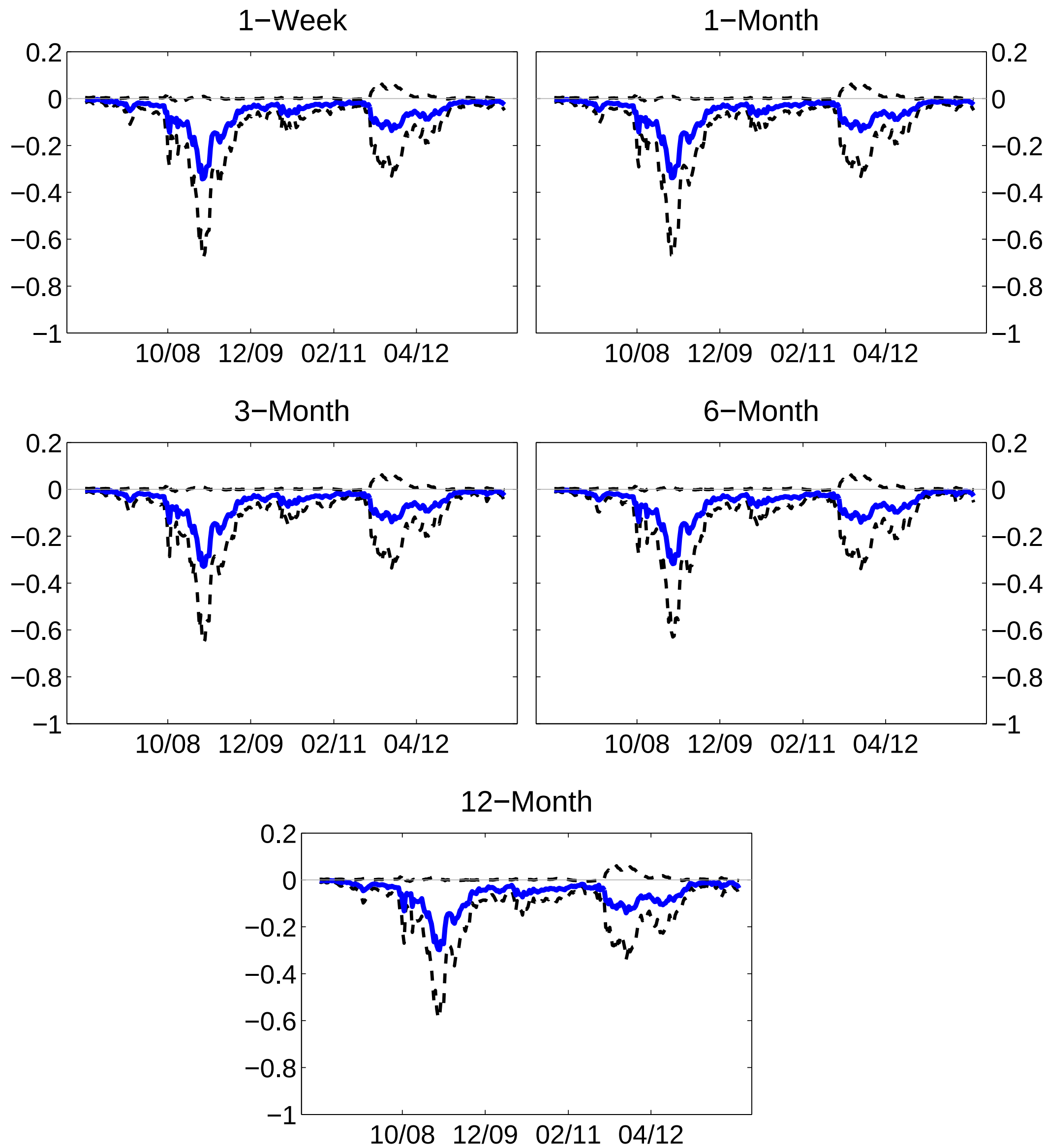


of the perceived cost of lying to the perceived cost of deviating from other banks ($\gamma_{2,t}/\gamma_{3,t}$). Figure 8 plots this ratio. The estimate is always statistically and economically close to zero, implying that the perceived cost of differing from other banks is much greater than the perceived cost of lying *per se*. This finding 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_2 close to zero implies that banks do not vary their reported Libor quotes commensurately with their credit risk; meanwhile, a high value of γ_3 implies that all banks want to report similar values. The statistical and economic significance of the ratio rises a bit on average after 2010, around the time that regulators began to investigate Libor misreporting in depth.

4.2 Decomposition of LOIS spreads

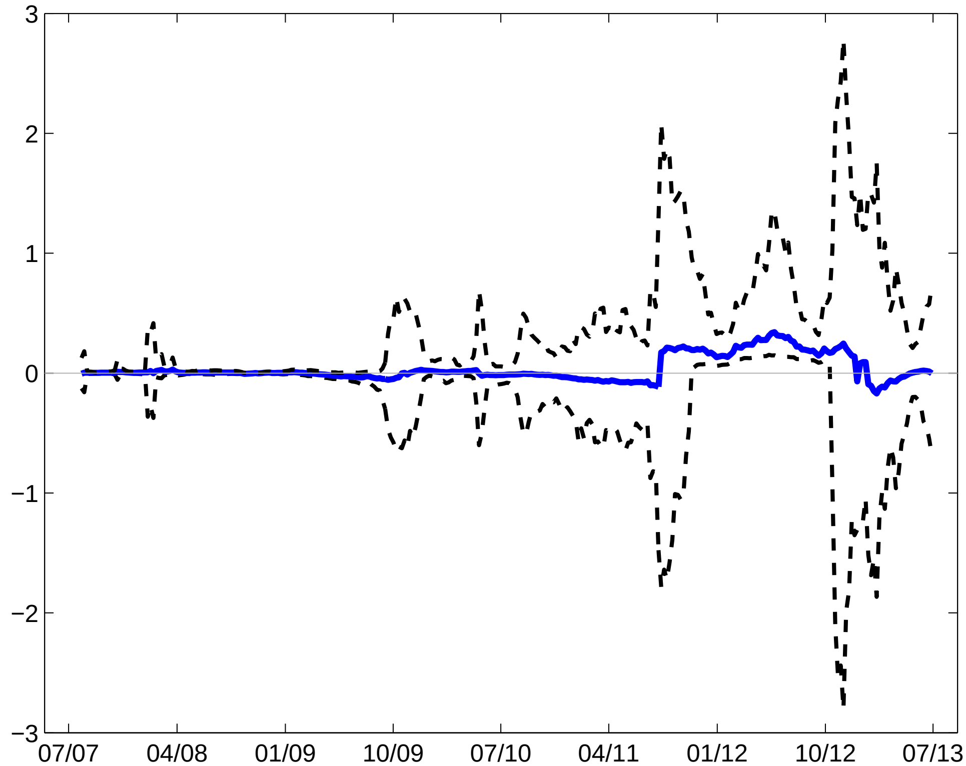
Table 1 summarizes the relative contributions of the liquidity, credit-risk, and misreporting components based on our median point estimates of λ_{mt}, ϕ_t, and $\bar{\beta}_{1,t}$. The top panel reports these results in terms of the average values of those components. The bottom panel reports the average values of the ratios of the three components to the "true" LOIS spread. (Truth is defined as the estimate we obtain by subtracting the estimated misreporting bias from the

Fig. 7. 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).



reported quote.) The liquidity component dominates the true LOIS spread at all maturities greater than one week. At the 12-month horizon, it accounts for 80% of the level of that 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 (57% versus 44%) even though its average value is smaller (7.9 basis points versus 24.4 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 liquidity-risk component.

Fig. 8. Estimated path of the ratio of misreporting costs ($\gamma_{2,t}/\gamma_{3,t}$), with 5th and 95th percentiles



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 spread itself it is less important at longer maturities.

Table 2 decomposes the time-series variance of the LOIS spread, making use of the approximation

$$\begin{aligned}\text{var}\left[\overline{L}_{mt}\right] \approx & \ \text{var}\left[\lambda_{mt}\right] + \phi^{*2}\text{var}\left[\overline{C}_{mt}\right] + \overline{C}_m^{*2}\text{var}\left[\phi_t\right] \\ & +2\left(\overline{C}_m^{*}\text{cov}\left[\lambda_{mt},\phi_t\right] + \phi^{*}\text{cov}\left[\lambda_{mt},\overline{C}_{mt}\right] + \phi^{*}\overline{C}_m^{*}\text{cov}\left[\phi_t,\overline{C}_{mt}\right]\right)\end{aligned} \quad (12)$$

where ϕ^* and $\overline{C}_m^*$ are linearization points chosen to minimize the mean squared error of the linear approximation with respect to the aggregate LOIS data.[26] Each column of the table sums to the variance of the (true) LOIS spread at each maturity, apart from approximation error from the linearization. Again, this decomposition is performed using the medians of the distributions of our estimated state variables.

Apart from the one-week maturity, the variance of LOIS spreads is dominated by the

[26] The time-series RMSEs of the approximation range from 26 to 28 basis points across maturities.

Table 1
Relative contributions of model components to level of LOIS spread

	1 wk	**1 mn**	**3 mn**	**6 mn**	**12 mn**
Average value (pp)					
liquidity	0.014	0.079	0.255	0.439	0.627
credit risk	0.242	0.244	0.250	0.258	0.275
misreporting	-0.059	-0.059	-0.059	-0.059	-0.061
Average fraction of "true" LOIS spread					
liquidity	37%	57%	80%	82%	80%
credit risk	72%	44%	22%	19%	21%
misreporting	-32%	-24%	-15%	-9%	-6%

Notes: 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.

Table 2
Variance decomposition of the LOIS spread

	1 wk	**1 mn**	**3 mn**	**6 mn**	**12 mn**
λ	0.088	0.054	0.019	0.010	0.023
$\overline{C}$	0.030	0.030	0.028	0.026	0.021
ϕ	0.043	0.046	0.049	0.052	0.066
Covariance terms	-0.077	-0.040	0.029	0.038	-0.035

Notes: 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.

credit-risk component (the sum of the ϕ and $\overline{C}$ terms). That is, 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.

At the one-week horizon, and to a lesser extent at the other horizons, the covariance terms in equation (12) contribute significantly to the variance of LOIS spreads. Most of this covariance derives from correlation between λ_{mt} and $\overline{C}_{mt}$. In particular, the negative covariance at shorter maturities primarily results from the drop in short-term liquidity premia in late 2008, at the same time that aggregate CDS spreads were rising. As we explain in the next section, the improvement in liquidity at the short end was likely caused by the ramping up of Federal Reserve lending programs during this time.

4.3 Event Studies

The previous section discussed decompositions of the full-sample variation in LOIS spreads. This section zooms in on some specific episodes that drove much of this variation. In particular, Table 3 reports the decomposition of changes in aggregate LOIS spreads around five events that garnered wide attention by showing the three fundamental components of those spreads (λ_{mt}, $\overline{C}_{mt}$, and ϕ_t).

		Beginning of crisis		TAF		Fed Facilities		SCAP		European debt crisis	
		Weeks of 8/1 - 8/29/07		Weeks of 12/5/07 - 1/2/08		Weeks of 10/1 - 11/5/08		Weeks of 4/29 - 5/20/09		Weeks of 7/27 - 9/7/11	
LOIS	**1 wk**	0.496		-0.273		**-2.007**	***	-0.046		0.009	
	1 mn	**0.529**	**	**-0.653**	***	**-1.004**	***	-0.124		0.042	
	3 mn	**0.541**	***	**-0.347**	*	**-0.633**	***	**-0.310**	*	0.109	
	6 mn	**0.589**	***	-0.222		**-0.356**	*	**-0.333**	**	0.134	
	12 mn	**0.454**	***	-0.106		**-0.277**	*	**-0.350**	***	0.169	
λ	**1 wk**	0.187		-0.037		**-1.825**	***	0.271		-0.093	
	1 mn	0.194		**-0.414**	***	**-0.726**	***	0.181		-0.059	
	3 mn	**0.199**	***	**-0.109**	*	**-0.291**	***	-0.006		0.000	
	6 mn	**0.216**	***	0.017		**0.066**	***	-0.023		0.003	
	12 mn	0.000		**0.148**	***	**0.272**	***	-0.011		0.018	
$\overline{C}$	**1 wk**	0.004		-0.013		0.015		**-0.801**	***	**0.980**	***
	1 mn	0.005		-0.011		-0.002		**-0.807**	***	**0.991**	***
	3 mn	0.007		-0.007		-0.044		**-0.818**	***	**1.013**	***
	6 mn	0.010		-0.003		-0.098		**-0.824**	***	**1.034**	***
	12 mn	0.014		-0.001		-0.180		**-0.817**	***	**1.046**	***
ϕ		**3.897**	***	**-1.037**	*	-0.241		-0.064		0.201	

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.

Table 3. Decomposition of model results across various time periods

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 1. The table shows that, during the month of August 2007, LOIS spreads at most maturities exhibited increases that were in the 99th percentile (given the distribution of such changes in our sample). Our decomposition shows that a small part of these increases were due to a deterioration in liquidity. Instead, most of the increase can be explained by the rise in ϕ.

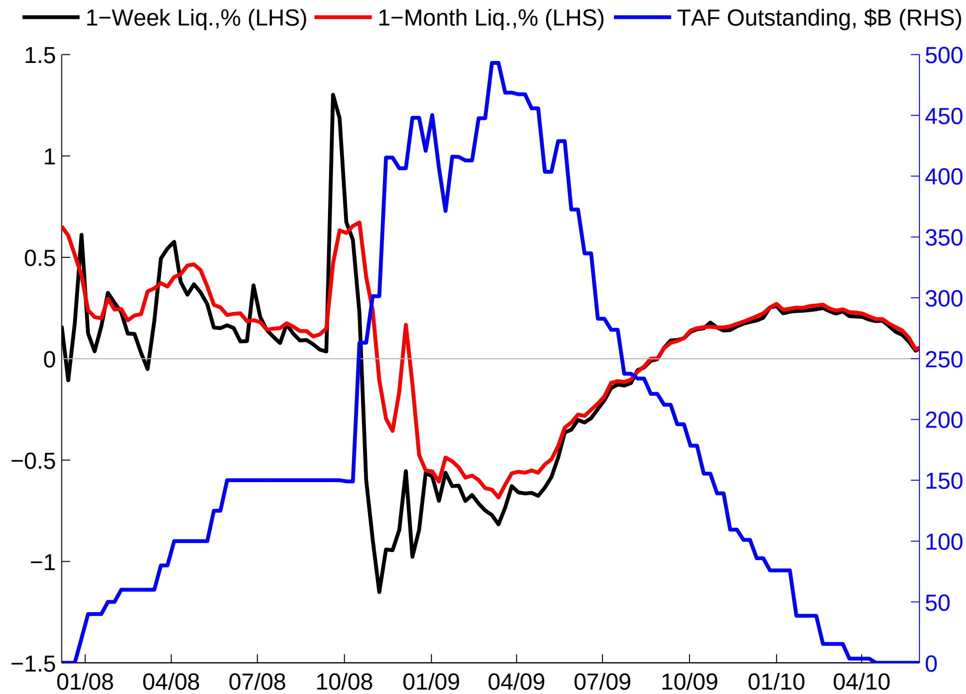
In contrast, the second and third columns of the table show the breakdown for two episodes in which the Federal Reserve responded to the crisis by introducing liquidity facilities. First, in December 2007, the Fed introduced the TAF. In the weeks following the TAF announcement, LOIS spreads fell across 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.) Similarly, the Fed expanded the TAF and introduced a host of new liquidity facilities targeted at relatively short maturities in October 2008.[27] As shown in the third column, LOIS spreads narrowed significantly around this time, and, at maturities of up to three months, our model attributes most of this narrowing to an improvement in liquidity, consistent with the Fed's measures having the desired effect. 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, liquidity deteriorated during these periods for the 6- and 12-month maturities. This result likely reflects 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. For example, the longest-maturity TAF loans ever offered were just 84 days, and even these constituted a relatively small fraction of outstanding TAF balances.

To see the effects of the TAF on short-term liquidity spreads more clearly, Figure 9 plots our estimates of $\lambda_{1wk,t}$ and $\lambda_{1mn,t}$ against TAF balances outstanding during the time this facility was operating. The decreases during the two TAF-related windows considered in the table are evident, but the correspondence between the series also appears to extend over the entire life of the facility. (Again, no such relationship exists with the longer-maturity liquidity spreads, not shown.) Since we did not use information about the TAF, or other borrowing-volume data, in our construction of the liquidity premia, this correspondence also serves as an external validation of the estimates produced by the model.

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"). These were generally regarded as successful, 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

[27] 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.

Fig. 9. The estimated path of the short-maturity liquidity premia (λ_t) and the amount of outstanding TAF loans at the Federal Reserve



bank CDS spreads widened dramatically. Moreover, the CDS widening did not pass through to a significant degree into LOIS spreads because our estimate of ϕ was near zero around this time.

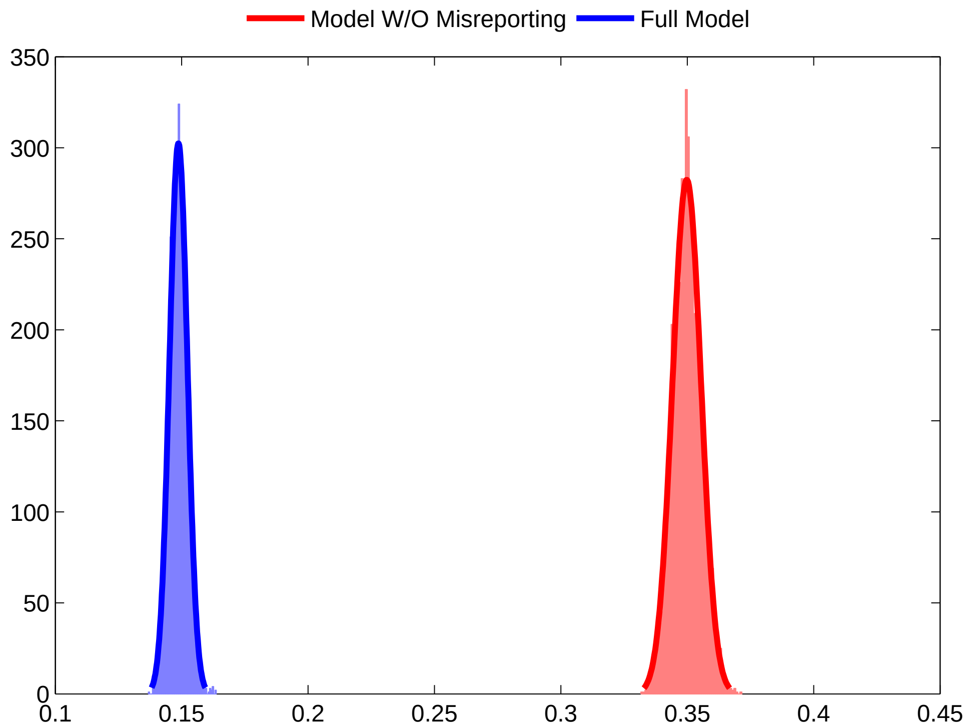
4.4 Does Accounting for Misreporting Matter?

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_1 and γ_3 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_1 and β_2 terms in equation (9) equal to zero. Consequently, the model that we now estimate contains only the liquidity and credit-risk terms.

Figure 10 plots the estimated posterior trace of $\mathbf{R}$ for this restricted model versus the baseline model estimated above. This is a summary measure of how closely each model is able to match the LOIS quotes. (Our prior distribution in both cases is essentially flat over the region depicted.) Including the misreporting terms reduces the modal estimate of the trace by more than half, indicating that these terms are extremely important for fitting the data. To see this another way, the estimated RMSE of the model across all banks and maturities falls from 6.4 basis points to 4.1 basis points when including the misreporting terms.

Controlling for the misreporting terms is also important for getting the right results with

Fig. 10. Posterior distributions of the trace of **R** under the baseline model (blue) and the model with misreporting terms set to zero (red)



respect to credit and liquidity. Tables 4 and 5 reproduce Tables 1 and 2 for the model that excludes misreporting. Without this correction, the 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.

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 (10) 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 Conclusion

This paper develops a simple 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. In doing so, we merge two rapidly-growing strands of the literature and place the important decomposition of credit and liquidity factors in interbank

Table 4
Relative contributions of model components ignoring misreporting

	1 wk	**1 mn**	**3 mn**	**6 mn**	**12 mn**
Average value (pp)					
liquidity	0.197	0.246	0.440	0.647	0.851
credit risk	0.000	0.000	0.001	0.001	0.003
Average fraction of "true" LOIS spread					
liquidity	94%	97%	100%	100%	100%
credit risk	6%	3%	0%	0%	0%

Notes: Using the model that excludes the misreporting terms, 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.

Table 5
Variance decomposition of the LIBOR-OIS spread without controlling for misreporting

	1 wk	**1 mn**	**3 mn**	**6 mn**	**12 mn**
λ	0.123	0.154	0.227	0.236	0.165
$\overline{C}$	0.000	0.000	0.000	0.000	0.000
ϕ	0.000	0.000	0.000	0.000	0.000
Covariance terms	-0.003	-0.003	-0.003	-0.003	-0.002

Notes: Using the model that excludes the misreporting terms, 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. The contributions of λ and ϕ are calculated using the medians of the posterior distributions of the estimates.

markets 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 is due to the credit-risk component and much of the fluctuations in that component stem from movements in Libor's sensitivity to credit risk, rather than from changes in the level of credit risk *per se*.

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. 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.

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A Derivation of Measurement Equations

Each bank's first-order condition is:

$$\gamma_{1,i} - \gamma_2 \left(\frac{\widehat{L}_i - L_i}{\text{std}\left[\widehat{L}\right]} \right) - \gamma_3 \left(\frac{\widehat{L}_i - \overline{\widehat{L}}}{\text{std}\left[\widehat{L}\right]} \right) = 0 \tag{A.1}$$

Solving for $\widehat{L}_i$ and substituting equation (5) gives

$$\widehat{L}_i = \frac{\gamma_{1,i}\text{std}\left[\widehat{L}\right] + \gamma_2\left(\lambda + \phi C_i\right) + \gamma_3 \overline{\widehat{L}}}{\gamma_2 + \gamma_3} \tag{A.2}$$

Taking expectations and rearranging delivers

$$\overline{\widehat{L}} = \lambda + \phi \overline{C} + \frac{\overline{\gamma}_1}{\gamma_2}\text{std}\left[\widehat{L}_i\right] \tag{A.3}$$

where $\overline{\gamma}_1$ is the cross-sectional mean of $\gamma_{1,i}$. Substituting back into (A.2),

$$\widehat{L}_i = \lambda + \phi \frac{\gamma_2 C_i + \gamma_3 \overline{C}}{\gamma_2 + \gamma_3} + \left(\frac{\gamma_{1,i}}{\gamma_2 + \gamma_3} + \frac{\gamma_3 \overline{\gamma}_1}{\gamma_2(\gamma_2 + \gamma_3)} \right) \text{std}\left[\widehat{L}\right] \tag{A.4}$$

The cross-sectional variance is therefore

$$\text{var}\left[\widehat{L}\right] = \left(\frac{\phi\gamma_2}{\gamma_2 + \gamma_3} \right)^2 (\sigma^C)^2 + \frac{\text{var}\left[\widehat{L}\right]\text{var}\left[\gamma_1\right]}{\left(\gamma_2 + \gamma_3\right)^2} = \frac{\left(\phi\gamma_2\right)^2}{\left(\gamma_2 + \gamma_3\right)^2 - \text{var}\left[\gamma_1\right]} (\sigma^C)^2 \tag{A.5}$$

Thus, we can write equation (9), by defining

$$\beta_{1i} = \left(\frac{\gamma_{1,i}}{\gamma_2 + \gamma_3} + \frac{\gamma_3 \overline{\gamma}_1}{\gamma_2(\gamma_2 + \gamma_3)} \right) \frac{\phi\gamma_2}{\sqrt{(\gamma_2 + \gamma_3)^2 - \text{var}\left[\gamma_1\right]}} \tag{A.6}$$

and $\beta_2 = -\frac{\phi\gamma_3}{\gamma_2 + \gamma_3}$.

B Data and Estimation Details

This appendix describes in detail our raw data, our handling of those data, including the procedure we use to extrapolate CDS spreads to short maturities, and the estimation procedure we employ.

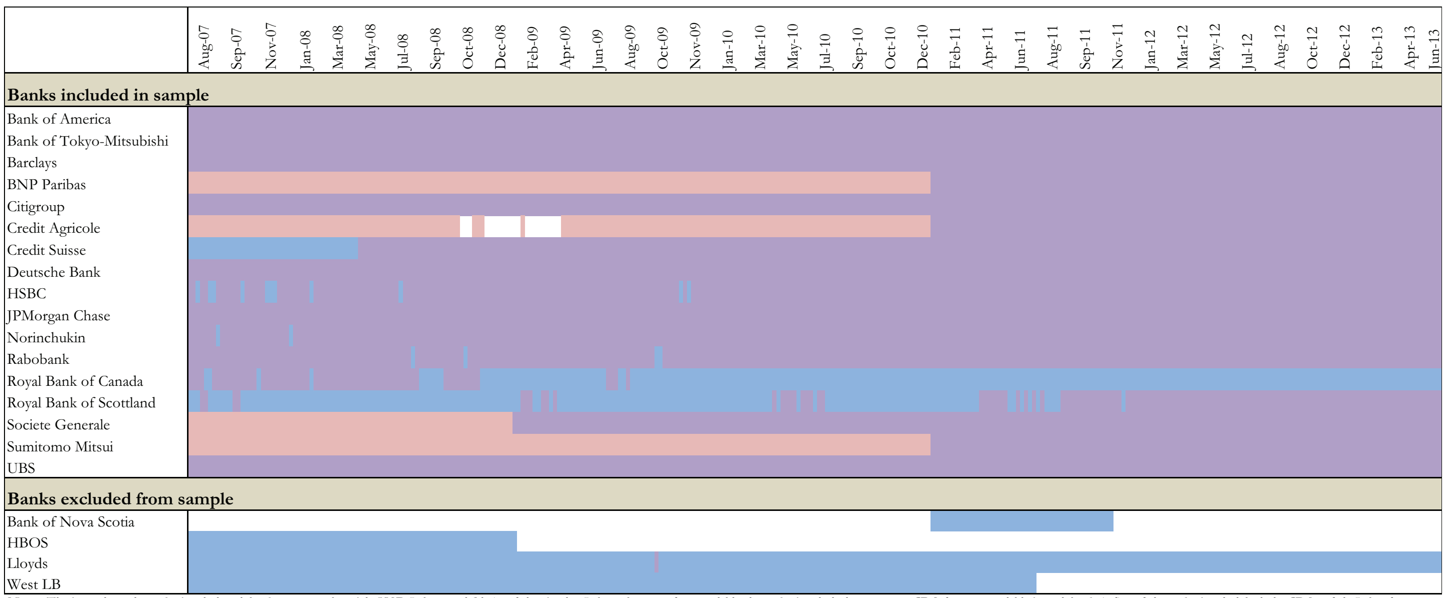
B.1 Data Sources and Coverage

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 B.1 shows the composition of the Libor panel during the sample period used in the estimation, July 30, 2007, through June 28, 2013. (Figure 1 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 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 B.1 indicate the weeks during which we have CDS observations for each bank after applying these filters. (Figure 2 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.

Fig. B.1. Data summary



Notes: The figure shows the weeks for which each bank was a member of the USD Libor panel (blue) and therefore has Libor-submisison data available; the weeks for which short-maturity CDS data are available for each bank (red); and the weeks for which both the CDS and the Libor data are available (purple). Blank cells indicate that a bank neither was in the Libor panel nor had CDS data avilable in that week.

Table B.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%

B.2 CDS Curve Fitting

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

$$C_{imt} = \beta_{0,it} + \beta_{1,it}\frac{1-e^{-m/\tau}}{m/\tau} + \beta_{2,it}(\frac{1-e^{-m/\tau}}{m/\tau} - e^{-m/\tau}) \quad \text{(B.1)}$$

where C_{imt} is bank i's CDS quote at maturity m on day t, and $\beta_{0,it}$, $\beta_{1,it}$, $\beta_{2,it}$, 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 B.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. [28]

Table B.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 curva-

[28] 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 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 B.2
Differences between CDS term-structure features in extrapolated and raw data

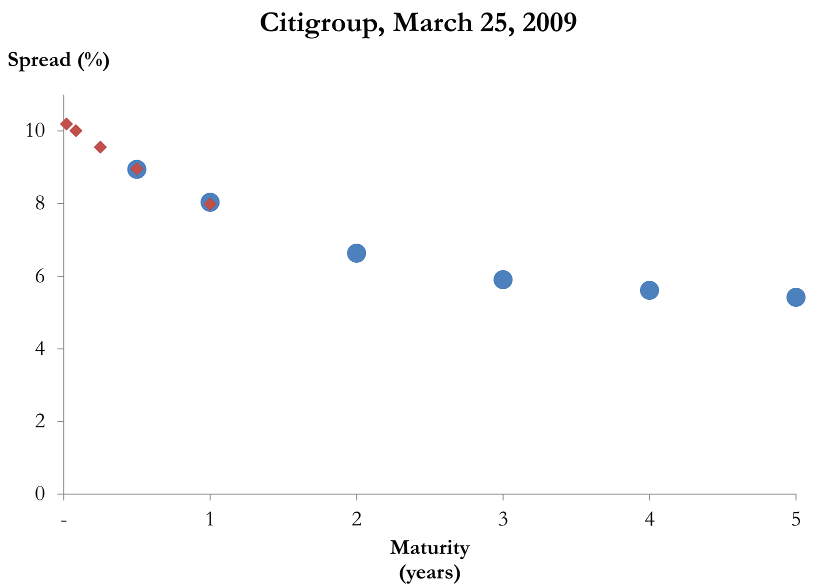
	Percentile						
	1	5	25	50	75	95	99
Level	-23.4	-8.3	0.1	6.4	13.6	26.3	34.7
Slope	-17.3	-9.8	-3.1	0.5	4.4	11.8	24.6
Curvature	-80.9	-30.1	-8.1	-1.8	4.7	19.9	56.4

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.

ture (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.

It is also instructive to examine the extreme cases. Figure B.2 shows the raw and imputed CDS spreads for the observation associated with the greatest difference in the raw and imputed level measures—Citigroup on March 25, 2009. The six- and 12-month fitting errors are negligible, and the curve is quite smooth. The 125-basis-point difference between the one-week and six-month spreads does not appear unrealistic. Figure B.3 shows the raw and imputed CDS spreads for the observation associated with the greatest difference in the raw and imputed slope measures—Citigroup on March 3, 2009. While the fitting errors are relatively large, the Nelson-Siegel curve essentially smoothes through the bump in the raw 12-month data and again does not appear unreasonable given the rest of the curve. Finally, Figure B.4 shows the raw and imputed CDS spreads for the observation associated with the greatest difference in the raw and imputed curvature measures—UBS on October 3, 2008. Perhaps not coincidentally, this observation also has the largest fitting errors at both the six- and twelve-month maturities of any of the 23,055 observations in our sample. Even so, the fitted curve captures some of the non-convexity of the data, and, again, it does not appear unrealistic at the very short maturities. Given that even these largest outliers look reasonable, we conclude that the curve-fitting procedure does not likely induce a large amount of measurement error.

Fig. B.2. CDS curve observation with greatest difference between imputed (red diamonds) 1-week and raw (blue circles) 6-month level



B.3 Estimation Procedure

Our estimation applies the Kalman filter to the linear state-space system described by the measurement and state transition equations (10) and (11). 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 (10) can be written compactly as

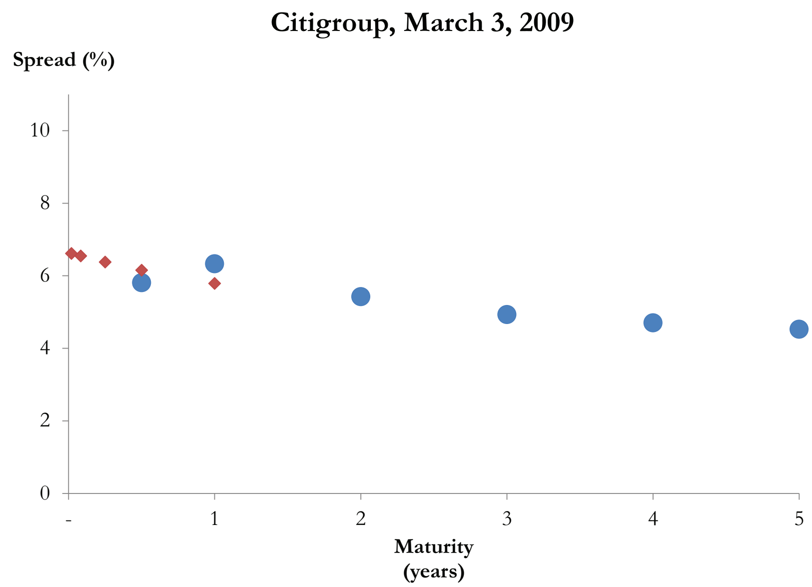
$$\widehat{\mathbf{L}}_t = \mathbf{X}_t \theta_t + \varepsilon_t \tag{B.2}$$

where $\widehat{\mathbf{L}}_t$ is the 85×1 vector of Libor submissions 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

$$\mathbf{X}_t = \left(\mathbf{I}_5 \otimes 1_{17} \;\; \Sigma_t^C \;\; \mathbf{C}_t \;\; \left[\mathbf{C}_t - \overline{\mathbf{C}}_t \right] \right) \tag{B.3}$$

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

Fig. B.3. CDS curve observation with greatest difference between imputed (red diamonds) 1-week/6-month and raw (blue circles) 6-month/12-month slope



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).

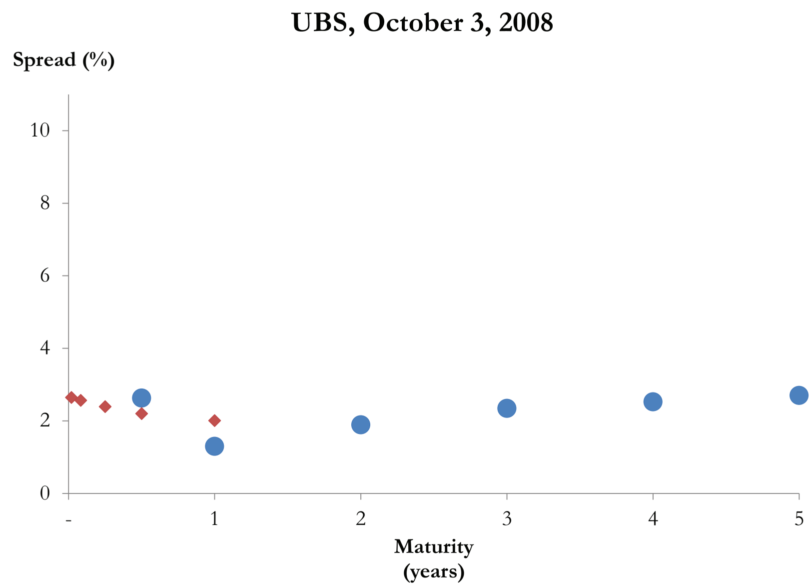
To deal with missing data, we follow the procedure of Aruoba et al. (2009). This process starts with the initial data that has missing values and uses a matrix, noted W_t in that paper, to eliminate missing observations, creating a situation where the left- and right-hand variables in the observation 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 (11), where $\mathbf{Q}$ is the 24×24 covariance matrix of innovations in the state transition equation. This law of motion can also be represented as the driftless random walk

$$\theta_t = \theta_{t-1} + \nu_t \tag{B.4}$$

where $\nu_t \sim N(0, \mathbf{Q})$. 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

Fig. B.4. CDS curve observation with greatest difference between imputed (red diamonds) 1-week/6-month/12-month and raw (blue circles) 6-month/12-month/24-month curvature



$\mathbf{R}$ and $\mathbf{Q}$ themselves are also diagonal.[29]

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 initial mean of $\beta_{1,i}$ 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 reported below.) The prior parameterization of the hyperparameters $\mathbf{R}$ and $\mathbf{Q}$ 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

[29] The assumption of a diagonal $\mathbf{R}$ greatly facilitates the MCMC procedure in the presence of missing data. Note that, since $\mathbf{R}$ is diagonal by assumption, the measurement-error RMSE mentioned in Section 4.4 is equal to $\sqrt{tr[\mathbf{R}]/\mathbf{k}}$.

take 50,000 Gibbs draws, of we we discard the first 45,000 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.