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By studying 3-months LIBOR futures, this paper evaluates the consequences four scandal-related events have had on liquidity and volatility in LIBOR markets. The goal is to document the market disruption, or lack thereof, caused by the manipulation and discontinuation and to draw the relevant policy lessons. One finding is that the liquidity outflows necessitated by the discontinuation and the associated volatility increases were confined to a period of a few weeks before the discontinuation, easing potential concerns that market transitions of the scale of LIBOR could only be done at the cost of major and prolonged disruption.

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The Effects of the LIBOR Scandal on Volatility and Liquidity in LIBOR Futures Markets

Kilian Bachmair[1]

Faculty of Economics, University of Cambridge, Austin Robinson Building, Sidgwick Avenue, Cambridge CB3 9DD, UK

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Abstract

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[1] Corresponding author. *Email address*: kb686@cam.ac.uk
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I. Introduction and Motivation

On 27 June 2012, Barclays was the first financial institution to admit to manipulation of the London Interbank Offered Rate (LIBOR); in the subsequent weeks, it became clear that investment banks and LIBOR submitters across Europe and the U.S. had systematically fixed rates (BBC, 2013). As a result of the scandal, all GBP LIBOR and the one-week and two-month USD LIBOR maturities have been terminated at the end of 2021, whilst overnight, 1-month, 3-month, 6-month and 12-month USD LIBOR continue to be reported until the end of June 2023 (Bank of England, 2018); LIBOR users were encouraged to transition away also from these USD LIBOR rates by 31/12/2021 at the latest (Board of Governors of the Federal Reserve System et al., 2021).

In the case of the LIBOR discontinuation, some $300tn worth of financial contracts which LIBOR used to underpin (BoE, 2018) in five currency areas are discontinued alongside LIBOR and replaced by instruments benchmarked on alternative reference rates. This paper analyses to what extent the uncertainty and capital outflows caused by the following four key events related to the LIBOR scandal have influenced liquidity and volatility in LIBOR markets:

- 16/04/2008: Mollenkamp and Whitehouse's (2008) Wall Street Journal article is the first to raise suspicions that LIBOR may have been systematically manipulated.
- 27/06/2012: Barclays is the first bank to officially admit to LIBOR manipulation.
- 27/07/2017: The Financial Conduct Authority's (FCA) then-Chief Executive Andrew Bailey first hints at the possibility of discontinuing LIBOR (FCA, 2017).
- 31/12/2021: All GBP LIBOR rates are discontinued, and users of LIBOR rates that are still being reported (e.g., 3M USD LIBOR) are recommended to stop using it.

Exactly these events were chosen as the focus of this study since they are salient, well-defined events which, assuming the absence of perfect anticipation, reveal new relevant information to the market (first three events) or bring about the most radical conceivable structural change to this market: its cessation. In all cases, economic theory suggests at least the potential to have an effect on trading activity or volatility.

The markets under consideration are futures on three months (3M) GBP and USD LIBOR. Out of all LIBOR derivatives, futures have been chosen for two reasons: first, derivative traders attempted to earn money through dishonest rate submissions mainly with swaps and futures

(Ashton and Christophers, 2015). Second, 3M LIBOR futures were among the most widely traded futures in the world, and so played an important role in the international financial system. Thus, for the remainder of this paper, e.g., “GBP LIBOR liquidity” or “volatility of USD LIBOR” will be used as shorthand for “liquidity in 3M GBP LIBOR futures markets” and “volatility in 3M USD LIBOR futures markets”.

The hypothesis motivating this paper is that the uncertainty and disruption caused by all four events, and users’ reorientation to alternative instruments necessitated by the discontinuation have caused *decreases* in liquidity and *increases* in volatility. This paper analyses whether this has indeed been the case and if so, how exactly: which of the above four events have had measurable impacts? Was liquidity or volatility affected the most? And what was the nature of these impacts – were they long-lasting or transitory?

Studying these questions is interesting for multiple reasons. Lower liquidity and greater volatility cause distortions in allocation and price forming mechanisms as well as a worsening of systemic stability and balance sheet risks. In addition to documenting the consequences events related to this scandal have had on a market with impact on millions of consumers in the Western currency areas, this paper analyses the consequence of the regulatory response to the LIBOR scandal, which can inform future policy decisions in similar situations.

First, consider policymakers’ decision in the case at hand to discontinue LIBOR altogether and replace it with alternative rates that no longer have the loopholes that enabled large-scale rate rigging; doing so was certainly the strongest possible response and an effective way to break with the institutional arrangements that paved the way for rate rigging. On the other hand, keeping and reforming LIBOR and its calculation methodology would have spared market participants the costs and disruption associated with moving out of LIBOR and into alternative benchmarks. Clearly, the extent of disruption a discontinuation causes should be a consideration in regulators’ decision whether in similar cases of manipulation and market abuse, the affected market should be reformed or, as a more extreme measure, abandoned altogether, as has been done here. This paper serves as a case study to shed light on this: if we find the discontinuation has caused strong and prolonged disruptions in the LIBOR markets, regulators should seriously think about the costs on market functioning of such a measure. If, on the other hand, volatility and liquidity were not significantly affected by the discontinuation, or only over a brief period of time, we know it is possible to close down even large financial markets without causing catastrophic and extended turmoil in the markets – exactly what constitutes a catastrophic and extended degree of turmoil is open to interpretation, of course. As we shall see in this paper, statistically significant effects of the discontinuation on volatility and liquidity become visible only a few weeks before the termination date – probably significantly later than would a priori be conceivable.

Second, in the event that policymakers choose a market shutdown over reform, perhaps *the* central question of the implementation is how to communicate this. From a theory perspective, it is conceivable that liquidity outflows and increased volatility begin already with the discontinuation announcement. In this case, policymakers must trade-off giving market participants sufficient time to move out of the discontinued market with minimising the time period they want to keep a disrupted market in existence. If, on the other hand, there is little to no turmoil after the announcement, and liquidity and volatility deteriorations occur only around the time of the discontinuation's execution, policymakers do not need to worry about causing turmoil prematurely by allowing for a longer time between the announcement and execution; firms can be given more time to organise the shift away from the discontinued market. This is one lesson that can be drawn from the analysis of the discontinuation announcement on 27/07/2017.

In short, this paper's goals are both to historically document the LIBOR scandal's effects on two key aspects of financial markets and to shed light on the lessons that can be drawn from it regarding the regulatory response's consequences on financial market stability.

II. Literature Review

This paper sits at the intersection of two strands of research: first, the literature on the LIBOR manipulation scandal, its causes, and consequences; and second, studies on LIBOR futures and their properties. Thus, a review of both will be given, including an outline of what this paper contributes to each strand. In addition, one might wonder how the cessation of LIBOR and its derivatives markets, or rather, a study on its effects on these markets, differs from studies on previous security delistings; this will be addressed at the end of this section.

Literature Strand I: the LIBOR Scandal

Existing research on the LIBOR scandal can roughly be categorised into two sub-branches: the first addresses whether there was manipulation at all, and if so, how it happened; the second focuses on the consequences and how to make LIBOR more robust against manipulations.

Literature Sub-Branch I: did manipulation occur, and if so, how? Mollenkamp and Whitehouse (2008) were the first to raise suspicions that panel banks were deliberately underreporting rates. This sparked a series of academic articles that attempted to *prove* rate fixing, most notably by looking at LIBOR's link to related rates. Articles include Gyntelberg and Wooldridge (2008), Taylor and Williams (2008a and 2008b), Snider and Youle (2010), Abrantes-Metz et al. (2011 and 2012), Monticini and Thornton (2013) and Gandhi et al. (2019). Most authors find evidence for manipulation; the last doubts have disappeared between 2012 and 2015, when Barclays, Deutsche Bank, UBS and others agreed to settlements in the US, UK and Euro Area.

The logical next question was on banks' motives for submitting false rates. These have been found to be twofold: first, to suggest higher creditworthiness and lower risk than was true by submitting artificially low rates (e.g., Monticini and Thornton, 2013 and Kuo et al., 2018); second, traders have asked submitters to send in rates that would benefit their positions in LIBOR-based derivatives (e.g., Duffie and Stein, 2015; Snider and Youle, 2012 and Vaughan and Finch, 2017). Gandhi et al. (2019) suggest that the latter motive was the dominant one.

Finally, an interesting question that has not been completely settled to this day concerns the role of financial regulators. Treanor (2012), Scott (2012) and a recording of an internal phone call between Barclays employees published by the BBC (Verity, 2017) suggest that Bank of England officials have actively *encouraged* Barclays to submit artificially low LIBOR rates during the financial crisis. The BoE's alleged motif was to bring Barclays' LIBOR rates in line with those of other international banks to prevent financial markets from interpreting Barclays' high LIBOR submissions as a sign of financial distress. It should be noted, however, that no other banks appear to raise these allegations and they have never been confirmed in court.

Literature Sub-Branch II: Consequences of the Scandal and Required Reforms. Having established *that*, *how* and *why* there was manipulation, the second literature branch focuses on the repercussions and how to make LIBOR more robust against manipulations. Several authors look at the effect of misreporting rates on LIBOR's level and find that, relative to alternative borrowing rates such as large Certificates of Deposit, LIBOR rates between 2005 and 2007 were significantly lower than pre-2005 (e.g., Abrantes-Metz et al., 2012; Snider and Youle, 2012; Monticini and Thornton, 2013; and Kuo, et al., 2018). This systematic downward bias in the rate is indicative of banks' attempt to signal higher-than-actual creditworthiness. Berkovitch et al. (2020) evaluate the consequences of the scandal on firms with public debt. They find that whilst average firms are not substantially affected in terms of returns on equity and debt, firms with low credit ratings are. Finally, Eisl et al. (2017) try to quantify the maximum effect an individual manipulated rate submission could have on the overall LIBOR rate's mean and standard deviation under multiple rate setting methodologies.

Major official inquiries into the required reforms include the review by the then-Chief Executive-designate of the Financial Conduct Authority Martin Wheatley (2012) and the Final Report of the Market Participants Group on Reforming Interest Rate Benchmarks (2014). Academic articles on potential reforms include Duffie et al. (2013), Duffie and Stein (2015) and Coulter et al. (2018); all focus on possible improvements either in the way LIBOR and its calculation is structured to prevent future manipulation. Gandhi et al. (2019) find that if prospective punishments are sufficiently severe and enforcement is credible, criminal prosecution can make a meaningful contribution to preventing manipulation.

In this second literature branch, whilst possible reforms have been amply discussed, and whilst legal trials against banks involved in the scandal give some indication on the order of magnitude of the damages, relatively little research has been done on the consequences of the scandal on *financial market functioning*. Gensler (2012) emphasises that between 2007 and 2012, during the financial crisis, whilst the volatilities of most short-term interest rates increased substantially, the volatility of USD LIBOR rates has significantly decreased relative to comparable rates. Hou and Skeie (2014) point out that it was this relatively low volatility of LIBOR that gave rise to the manipulation suspicions in the first place. Huang and Todorov (2022) report that USD and GBP LIBOR forward rate agreements have experienced declines in trading volume of more than 90% between April 2019 and April 2022; the authors attribute this to the fact that as trading in other LIBOR derivates decreases in the wake of the phase-out, so does the need for hedging LIBOR fixing risks through forward rate agreements.

This hypothesis and the fact that their discussion focusses on an annual analysis in steps of three years, with the two most recent datapoints being the above-mentioned April 2019 and April 2022 provides an excellent motivation for this study: can a similar decline in liquidity be seen also in other LIBOR derivates like futures, as the authors suggest? If yes, what does the time path of this change in liquidity look like on a more granular basis? And what does this mean for volatility? This article aims to contribute towards closing this research gap.

Literature Strand II: LIBOR Futures Markets

A major theme in the literature on LIBOR futures markets has been modelling LIBOR futures – both their statistical properties and valuation (e.g., Heath, Jarrow and Morton's, 1992; Brace et al., 1997; Miltersen et al., 1997; Goldys, 1997; Jamshidian, 1997; and Rutkowski, 1999). All these papers are of a theoretical nature and focus on the appropriate model to describe LIBOR rates and related derivatives. Piazzesi and Swanson (2006) and Ferrero and Nobili (2009) investigate the expectations hypothesis in LIBOR and LIBOR futures markets and broadly find evidence against it.

The only study known to this author that empirically analyses LIBOR futures markets' volatility over time as this paper does is Neely and Winters (2006): they explore seasonality patterns in 1M USD LIBOR futures' prices and volatility between June 1991 and March 2001, that is, seven years before our sample begins. One reason for the dearth of market property studies on LIBOR might be the fact that the events which make the investigation of LIBOR markets' properties interesting have taken place only very recently or are still in the process of materialising – as is the case for some USD LIBOR tenures, including the 3M one. Closing this research gap is another goal of this paper.

Previous Studies on Effects of Security Delistings

Whilst there has not been much research on the effect of LIBOR-scandal related events on liquidity and volatility in affected markets, several studies have investigated these properties in the context of the cessation of other financial instruments: Sanger and Peterson (1990) investigate the effect a delisting has on the liquidity of common stocks on major American stock exchanges. Comparing average liquidity in the eight weeks before the delisting and in the eight weeks after, they find delistings tend to cause a significant liquidity decline; the authors do not investigate the time path of liquidity within these 16 weeks in greater detail, however. Harris et al. (2008) find that the stocks of delisted firms suffer significant reductions both in value and liquidity as well as sizeable increases in volatility; these effects materialise across a period of multiple months around the delisting, being greatest on the delisting day itself. Finally, Kawaller et al. (2001) evaluate trading volume and volatility of S&P500 futures in the ten days surrounding the expiry of the next-to-expire contract and the transition of trading activity to the second-to-expire contract. They find a negative relationship between trading volume and volatility. Studies confirming this inverse association of volatility and liquidity in futures markets include Daigler and Wiley (1999) and Tauchen and Pitts (1983). On the other hand, Bessembinder and Sequin (1992), Cornell (1981), Foster (1995), Grammatikos and Saunders (1986) and Rutledge (1979) all suggest volatility and liquidity are positively related in futures markets. Given these contradictory results in the literature, it will be interesting to see how volatility evolves as trading volume declines with LIBOR futures' phase-out.

Two important aspects distinguish this study from the above-mentioned papers on the effects of security delistings. First, the LIBOR discontinuation is unprecedented in size and significance: given its role as a global interest rate benchmark, with contracts and derivatives of a dollar volume benchmarked on it which are orders of magnitude larger than that of any individual stock, LIBOR plays both quantitatively and qualitatively a completely different role in the global financial system than the instruments investigated by Sanger and Peterson (1990) or Harris et al. (2008). Second, LIBOR's discontinuation is due to a major financial scandal involving some of the most important financial institutions and ensuing regulatory reform, rather than the delisting of an individual firm, or the regular and recurring transfer from one contract to another as investigated by Kawaller et al. (2001), with correspondingly greater risks and uncertainty. This is also reflected by the concerns of both practitioners and regulators that have pointed to the risks from the LIBOR discontinuation: the SEC warns the discontinuation may have a "*significant impact on the financial markets and may present a material risk for certain market participants, including public companies, investment advisers, investment companies, and broker-dealers*" (SEC, 2019); the Reserve Bank of Australia (2021) and the Alternative Reference Rates Committee (2021) point to the potential disruption and the risks to financial stability, both systemically and to financial and non-financial firms, from a disorderly transition. Finally, at least for emerging markets, Bruggia and Rank-Broadley (2021) predicted that the LIBOR transition will create disruption in affected markets. This paper aims to evaluate to what extent these risks of disruption have materialised and how exactly.

III. Methodology

There are multiple conceptual approaches in which volatility and liquidity measures can be grouped. To ensure robustness of results, this paper uses at least two tests of each chosen conceptual group and a minimum of three measures for each aspect of market functioning. Indeed, as we shall see below, all measures display a high degree of concurrence regarding the statistical significance of the events in question, the effect's direction, and on whether liquidity or volatility is affected more strongly. This is the case both within and across both currencies considered, suggesting the conclusions are robust to a range of different methodologies. This section justifies the methodology choice; appendix A outlines liquidity and volatility measures' econometric details.

How do we know this similarity of results is not due to an excessive structural similarity in the measures used? Appendix A shows this concern is unfounded: not only are the measures used structurally different, as explained in more detail below, but tables A.1 to A.4 show that when considered across the entire sample period, measures exhibit a significant degree of disagreement. 17 out of 26 cross-correlation pairs between measures' values are below 2/3, 12 are less than 0.5; only three cross-correlation pairs are above 80%, two of which are two versions of one and the same measure, where we would expect a high degree of correlation.

Several of the measures used in this study rely on daily returns r_t. For a given day t, these are calculated as the difference in the logs of the closing prices on t and the day before, $t-1$:

$$r_t = \ln(P_{C,t}) - \ln(P_{C,t-1}) \tag{1}$$

where t refers to weekdays only. As a result, weekend returns are included in returns from Friday to Monday, so we are implicitly working with the trading time hypothesis.

Liquidity Measures: Justification of Choice

Four liquidity measures were chosen: two price impact and two bid-ask spread measures. This choice has been made based on two criteria. First, their data requirements could be satisfied with the obtainable LIBOR futures data. Second, they were found to be the "best" measures in Goyenko et al.'s (2009), Marshall et al.'s (2012) and Fong et al.'s (2017) benchmarking studies, where "best" is to be understood as follows: the chosen statistics have yielded results closest to a set of high-frequency liquidity benchmarks as measured by correlation with, and mean squared prediction error relative to, these benchmarks. Since these benchmarks have very demanding data and computational requirements, they were not used in *this* paper.

For price impact measures, the Amihud (2002) measure and volatility over volume were used; the Roll measure and the effective spread measure were chosen as spread proxies. The former is a widely used model-based measure and was found to be among the measures with the consistently highest correlation with high-frequency benchmarks' results in Goyenko et al. (2009). The latter is a popular model-free measure. The two measures complement each other in at least two ways: first, the Roll measure is an implicit measure of the bid-ask spread; it derives its estimate from the properties of the serial covariance in prices under spreads of different sizes. The effective spread, on the other hand, directly and explicitly measures the difference between bid and ask prices. Second, as already discussed above in the context of volatility measures above, model-based measures can only be as good as the underlying assumptions; the effective spread as a direct measure of the bid-ask spread does not rely on a theoretical model.

We might wonder, then, why we should invoke the Roll model to estimate the *implicit* bid-ask spread in the first place, when we could also use another empirical bid-ask spread measure instead. This is because officially quoted spreads can diverge from true effective spreads for a number of reasons (Roll, 1984 and Campbell, Lo and MacKinlay, 1996): first, liquidity providers do not always update their quotes perfectly in time; second, they may offer favourable bid and ask prices in order book rebalancing efforts in the case of an excess of buy or sell orders; finally, Eikeboom (1993) and Glosten and Milgrom (1985) suggest that market makers may offer advantageous prices to non-insider traders. In their study on Kansas City Board of Trade wheat futures, Shah, Brorsen and Anderson (2009) find a large difference between the average quoted and bid-ask spread estimated by the Roll measure: the average value of the former is more than three times the mean value of the latter. This shows that the two may well differ significantly also for financial futures and motivates *estimating* the spread, rather than simply working with the quoted spread.

Volatility Measures: Justification of Choice

Volatility measures can be classed into two groups: the first group, historic volatility, measures variation in historical data; the second group is forward-looking and so can be used to forecast volatility. The perhaps most well-known examples of the latter group are implied volatility, based for instance on Black and Scholes' (1973) option price model, and Bollerslev's (1986) GARCH model. The disadvantage of forward-looking measures is that they are only as correct as their underlying models are, and in the case of implied volatility they are also somewhat subjective: they measure the market's volatility *expectations*, which might well turn out to differ from realised volatility ex post. Given these limitations, and as the focus here is on the effect of the LIBOR scandal on realised or historical volatility, forward-looking measures were not used in this study.

The perhaps most immediate choice for capturing volatility, i.e., price dispersion, would be a standard measure of statistical dispersion, such the variance or standard deviation. The first measure, Realised Squared Returns, is closest to the variance formula and as such it is simple and intuitive; as a result, there is also no obvious cause for bias or error (Garman and Klass, 1980). A disadvantage of the Realised Squared Returns measure is that it is not robust to noisy data; whilst Bloomberg's data on a widely traded instrument such as LIBOR futures are likely to be very accurate, it is probably unrealistic to expect there to be no noise at all (see also the discussion on *Treatment of Extreme Spikes in Liquidity and Volatility Measures at the End of the Samples* in section *V. Estimation and Specification*). In the case of noisy data, the closely related realised kernel estimator due to Barndorff-Nielsen et al. (2008) is better-suited. Since this requires high-frequency data, however, which were not available, this measure was not used.

The flipside of the Realised Squared Returns measure's simplicity is that it fails to make use of information other than returns data even though doing so improves estimator efficiency. The Garman-Klass (1980) estimator was developed to achieve efficiency gains relative to Realised Squared Returns without having excessively demanding data requirements; it is calculated using daily trading volume, opening and closing prices, and intra-day high and low prices, all of which were available in this study. In addition, the estimator also includes a drift correction which the standard deviation does not account for (Chou et al., 2010). A drawback of the Garman-Klass measure relative to simpler measures is the fact that it relies on the assumption that the underlying return process follows Brownian motion with normally distributed increments (Linton, 2019).

Finally, the Schwert (1989) measure was used as it allows for the potential presence of autocorrelation in return data. As already hinted at in the discussion of the Roll measure above, this can be caused by non-synchronous trading, especially at lag order one (Schwert, 1989).

Regression Analysis: Disentangling Scandal's Impact and Market-Wide Developments

Suppose we have produced time series for the liquidity and volatility measures above and we observe changes in these in the temporal vicinity of some scandal-related event. At this point, two questions arise: first, is the observed change in liquidity or volatility statistically significant? Second, we shall see in more detail below that the main mover of both liquidity and volatility in LIBOR appear to be events that cause market-wide uncertainty. Thus, we need to ensure we do not confound the effects of the scandal-related events in question with other events that have the potential to affect liquidity and volatility in the way observed.

To address these two points, liquidity and volatility measures' values are regressed on a dummy indicating the event window and the closing price of the Chicago Board Options Exchange's CBOE Volatility Index (VIX) as a control variable, often interpreted as the "fear index" which captures the degree of uncertainty among financial market participants.

Moreover, visual analysis suggests there is some seasonality to both liquidity and volatility. Finally, an additional factor influencing liquidity, and thereby potentially volatility, is residual time to maturity. To capture these two effects, time dummies were used. Since LIBOR futures mature always in the same months of the year, time dummies also proxy for residual maturity. Specifically, to allow for multiple changes in liquidity for each futures contract used – each individual contract is considered for a length of three months in this study, see *Estimation Under Multiple Futures Contracts Running in Parallel* in section V. for more information – whilst being parsimonious with degrees of freedom, monthly dummies have been used.

The ceteris-paribus effect of a scandal-related event of interest was then estimated by the following regression:

$$measure_t = \beta_0 + \beta_1 VIX\_close_t + \beta_2 event\_window_t + \boldsymbol{\delta' monthdummies_t} + \varepsilon_t \quad (2)$$

where VIX_close_t refers to the daily VIX closing price, and $\boldsymbol{monthdummies_t}$ is a vector of eleven dummy variables from January to November, which are equal to unity if t is a day in the respective month and zero otherwise. Thus, the intercept β_0 corresponds to the month December. $event\_window_t$ is a dummy variable equal to 1 if day or week t is in the event window in question and zero otherwise. Thus, the coefficient of interest capturing an event's effect on liquidity or volatility, whilst controlling for market-wide uncertainty, seasonality, and residual contract maturity, is β_2.

$measure_t$ is the daily value of any of the liquidity and volatility measures estimated in this paper. In addition to total liquidity or volatility as captured by the standard application of these proxies, the analysis is repeated with the dependent variable $measure_t^{unexpected}$ that reflects abnormal or unexpected liquidity and volatility, following Kawaller et al.'s (2001) approach: first, the data series of daily values of liquidity and volatility measures is calculated that is also used in the standard analysis of tables 3 to 5. Second, an AR(5) model is estimated for each of these measures, which is then used to forecast expected liquidity or volatility during the event window. Finally, these fitted values $measure_t^{AR(5)\,fit}$ are subtracted from the realised values $measure_t$ to obtain unexpected liquidity and volatility:

$$measure_t^{unexpected} = measure_t - measure_t^{AR(5)\,fit} \quad (3)$$

This setup attempts to eliminate anticipated liquidity or volatility changes and so serves as an additional control for liquidity or volatility fluctuations that are due to factors other than the discontinuation. Financial return time series tend to exhibit autoregressive heteroskedasticity: volatility clustering. To formally verify this impression, i.e., to test for ARCH effects in the liquidity and volatility measures' time series, Engle's (1982) Lagrange multiplier test was applied to all eight measures for both GBP and USD LIBOR; this was done both on a standard OLS estimation of (2) and on a constant-only model for each measure. In all resulting 32 regressions, the null of no ARCH effects was rejected at all imaginable significance levels.

To address volatility clustering, a GARCH specification for the error terms ε_t was used. Thus, following Bollerslev (1986), the variance σ_t^2 of the error terms ε_t was modelled as:

$$\sigma_t^2 = \alpha_0 + \alpha_1 \varepsilon_{t-1}^2 + \cdots + \alpha_q \varepsilon_{t-q}^2 + \gamma_1 \sigma_{t-1}^2 + \cdots + \gamma_p \sigma_{t-p}^2 \qquad (4)$$

The error model's GARCH order p and ARCH order q were chosen as follows: all combinations of GARCH models that have $p \leq 3$, $q \leq 3$ were estimated; then among all models with significant GARCH coefficients, the one with the lowest Bayesian Information Criterion (BIC) was chosen. As Hansen and Lunde (2003) suggest, in many cases, the best or second-best (see appendix D) model was the GARCH (1,1).

IV. Data

Variables Analysed, Data Availability & Time Frame

Every year, four futures contracts on 3M GBP and USD LIBOR mature: in March, June, September, and December. Thus, whist there is a well-defined maturity date for these contracts, of course, it should be noted that they do not have a sharply defined starting date: in principle, there is no reason why a liquidity provider could not enter a futures contract on 3M LIBOR maturing multiple years in the future. On the other hand, liquidity in these contracts will of course decrease substantially as the time to maturity increases. This paper uses data of the next future to mature at a given point in time, so each individual futures contract is considered for a total of three months.

For each rate, daily spot closing prices were collected as well as the seven items on the associated futures contracts outlined in table 1, as required by the tests used in this study. Out of all available tenures, 3M LIBOR was chosen as it is among the most widely used and reported LIBOR maturities (Intercontinental Exchange, 2022). All data were downloaded on 23/03/2022 through the Bloomberg Terminal. The data time frames for the variables used in this study are as follows. For all variables and both GBP and USD LIBOR, the earliest datapoints are 01/01/2008. This was chosen such that the first event, the reports in April 2008, was comfortably included. The final datapoints for GBP LIBOR were limited, naturally, by the discontinuation date. Even though most LIBOR settings, including all GBP LIBOR rates, were officially discontinued only on 31/12/2021 (Bank of England, 2021), Bloomberg stopped reporting them already on 17/12/2021, making this the final observation date for all GBP LIBOR variables. The most recent date used for the USD LIBOR futures, which will be active until mid-2023, was the day before the data collection date, 22/03/2022.

Note that as a result, for GBP LIBOR, we have the chance to investigate the evolution of the market functioning aspects we are interested in – liquidity and volatility– from before the manipulation scandal became public, across the entire period of the scandal unfolding up until the point until its consequences were final and implemented: its cessation. Table 1 provides summary statistics of the variables used.

Table 1. Summary Statistics of Sample Data Used

LIBOR Rate	Variable	Obser-vations	Mean	Standard deviation	Q25	Q50	Q75
GBP	*Spot Price* P_t	3,530	99.030	1.329	99.191	99.411	99.489
	Opening Price P_O	3,530	99.046	1.259	99.15	99.39	99.49
	Closing Price P_C	3,592	99.049	1.255	99.15	99.39	99.485
	End-of-Day Ask Price P_A	3,149	98.978	2.205	99.14	99.36	99.5
	End-of-Day Bid Price P_B	3,148	98.970	2.205	99.13	99.35	99.5
	Intra-Day High Price P_H	3,530	99.061	1.249	99.16	99.4	99.5
	Intra-Day Low Price P_L	3,530	99.033	1.268	99.14	99.38	99.48
	Daily Trading Volume V	3,530	47302	41429.7	20597	36442	60078
USD	*Spot Price* P_t	3,511	99.078	0.928	98.682	99.562	99.738
	Opening Price P_O	3,574	99.076	0.879	98.67	99.54	99.715
	Closing Price P_C	3,574	99.077	0.878	98.67	99.54	99.715
	End-of-Day Ask Price P_A	3,480	99.102	0.858	98.681	99.555	99.72
	End-of-Day Bid Price P_B	3,477	99.010	0.859	98.675	99.55	99.715
	Intra-Day High Price P_H	3,574	99.092	0.870	98.675	99.555	99.72
	Intra-Day Low Price P_L	3,574	99.062	0.887	98.660	99.525	99.71
	Daily Trading Volume V	3,574	194823	128242.5	107290	167782	250660

All variables reported here are rounded to the third decimal place, except where variables were reported to fewer places in the original dataset. Note that the reported statistics are sample moments rather than population moments. Appendix B gives further details on the variables.

Note that for both interest rates, the numbers of observations for the spot prices and the seven variables collected on futures differ for some variables. This also influences the number of observations we have for the different measures. The main driver of the smaller number of bid and ask price observations is the relatively large number of trading days on which Bloomberg did not report them. However, as there did not seem to be a systematic pattern to these unreported days, this does not appear to affect results in a meaningful way. Finally, as a result of values not being reported for all variables on all days, liquidity or volatility measures using multiple variables were calculated based on the subset of days in the sample period on which an observation for every variable needed for a given measure was reported.

Table 1 also shows that for both GBP and USD LIBOR, the means and standard deviations of all prices are very close to one another, with the bid and ask prices falling out of line most. This is indicative of a fair degree of efficiency and market maturity: in an efficient market, where futures prices tend to be in line with spot prices, their moments should be equal on average as well. Comparing coefficients of variation, we see that USD spot and futures prices are somewhat less dispersed than their GBP counterparts; within these, all prices exhibit a relatively similar coefficient of variation, apart from P_A and P_B. Since their dispersions are lower than those of the other prices for GBP LIBOR and higher for USD LIBOR, this may be due to P_A and P_B's smaller sample sizes, rather than any systematic difference in dispersion.

Finally, looking at trading volume V's coefficient of variation, we see it is orders of magnitude more dispersed than all price series. V's mean value also reveals that in terms of average volume, the US LIBOR market is more than three times as large as the UK LIBOR market. It will be interesting to see whether the scandal-related events in question have different effects in different-size markets.

V. Estimation and Specification

Identification Assumptions: VIX Unaffected by Events, LIBOR Scandal as Natural Experiment and Regulators' Aim to Minimise Disruption

A matter at the heart of this paper is the question on identification: if changes in volatility or liquidity were to be found, can these exclusively be attributed to the manipulation scandal, its becoming public and the regulatory responses to it? We have already seen in section III. above that the main candidate alternative driver of changes in liquidity and volatility, market-wide uncertainty, is controlled for using regression (2).

VIX_close_t is an effective control variable only if it is not endogenous in (2), of course. That is, this paper assumes that the S&P 500 options market, which the VIX is based on, is sufficiently large and unrelated to LIBOR that the uncertainty caused by the four scandal-related events in question does not spread to VIX or cause any significant reaction in it. Even if this assumption does not hold and S&P 500 options are somewhat unsettled by the manipulation scandal, however, we will *under*estimate the effect of the events in question, i.e., β_2 will be biased towards zero. Thus, findings that an event has a statistically significant effect on volatility or liquidity are not threatened if VIX_close_t is not perfectly exogenous.

In addition, this paper assumes that the manipulation of LIBOR was an exogenous event, and so changes in these aspects of market functioning are exogenous as well. Specifically, it is assumed that the drivers of the manipulation scandal were twofold: first, the scope for manipulation, created by LIBOR's calculation methodology that solicited submissions by bankers without the requirement of providing evidence, and second, bankers' incentive to take advantage of this possibility, as manipulation was either beneficial for their bank's perceived creditworthiness or for their own profits. The fact that the LIBOR methodology was designed such that this manipulation was possible is assumed to be random, rather than somehow endogenous, i.e., influenced by factors affecting both LIBOR's regulation and the evolution of LIBOR futures' volatility and liquidity over time. Thus, the first reports on the possibility of manipulation and Barclays official admission are taken to be exogenous events.

The regulatory responses and their timings, on the other hand, will likely have been chosen also with a view to the market's current state as well as its potential reaction. This paper assumes that regulators' intention was to minimise disruption in LIBOR markets and that they are competent enough to time their intervention at least better than chance would. Thus, if anything, the coefficients measuring the effects of the third and fourth events – the announcement of the possibility of a discontinuation and the actual or recommended discontinuation for GBP and USD LIBOR, respectively – are biased towards zero. Thus, the significance of effects we may find is not threatened. In interpreting coefficients' *sizes*, we should keep in mind, however, that without regulators' careful timing, the events in question may have the potential of effects larger in absolute magnitude than this study finds. In addition, coefficient sizes depend on the definition of the event window, which in the case of the discontinuation necessarily is somewhat arbitrary; see section VIII. for a discussion.

Estimation Under Multiple Futures Contracts Running in Parallel

Whilst there exists only one (end-of-day) spot price on each day, for the futures, there are always multiple contracts of different maturities actively being traded at a given time. To create one time series per liquidity or volatility measure used, we need exactly one numerical value per day for futures' prices and volume (per currency used, that is). Thus, we need to choose which one out of the multiple contracts active at a time to use. On every trading day under consideration, this study follows what seems to be the standard solution to this in the literature: using the data of the futures contract that is to mature next, i.e., the first generic contract. It should be noted that doing so introduces an additional assumption into our analysis, however: poolability of data from different futures contracts (Baltagi, 1995).

Treatment of Extreme Spikes in Liquidity and Volatility Measures at the End of the Samples

In the original version of the data, for some of the six measures of liquidity and volatility, extreme spikes could be seen in the very final two to eight days of both the GBP and the USD LIBOR samples. See Appendix C for the example of the Amihud measure; the spikes in the other measures were both similar in magnitude and covered the very same days in the samples.

As a first response, it was confirmed that these spikes are not due to miscalculations, but instead genuinely in the data. This turned out to be the case: for the GBP LIBOR sample, the spikes were driven by a single extreme price jump on 15/03/2022; for the USD LIBOR sample the spikes were driven by two extreme values in reported trading volume on 14/03/2022 and in daily returns on 15/03/2022 – this is within the final seven observations of the sample.

The facts that these spikes occur in the very final week of both samples, that they do not occur on the same dates since the final dates of the samples do not coincide, that USD LIBOR was not discontinued at the end of its sample, and that the spikes are extreme in size, suggests they may be due to limitations of data reliability in the final days of the sample. Specifically, this might be because in the case of US data, the sample's final data have been only a few days old at the time of retrieving them. Since Bloomberg retrieves its data often from multiple sources, its data is "*scrubbed, verified and continually updated*" (Bloomberg Finance L.P., 2022). Similarly, once Bloomberg stopped reporting GBP LIBOR after 17/12/2021, it has likely also stopped its process of "scrubbing, verifying and updating" GBP LIBOR data; as a result, the extreme values driving the extreme spike at the end of the sample might be somewhat unreliable. Thus, the analysis in this paper was conducted by dropping the final two observations for the GBP LIBOR sample and the final seven observations of the USD LIBOR sample. Importantly, doing so does not affect the finding of a marked increase in illiquidity and volatility beginning towards the end of 2021.

If the spikes in liquidity and volatility measures at the very end are spurious, this raises the question whether the entire increase in illiquidity and volatility beginning at the end of 2021, to be discussed in more detail below, is spurious. This can be ruled out almost certainly, however, for the following reasons: first, the increase in illiquidity and volatility is multiple weeks long, in the USD sample even multiple months, so the data should have been updated and be reliable by the time of data collection. Moreover, this increase appears to begin at different time for GBP and USD LIBOR: for the former, it this is in early October, for the latter, in early December 2021. Thus, the increase cannot be to do with its position at the end of the sample, which however seems to be the driver of the extreme spike.

Finally, since all three volatility measures were dominated by a few extreme, albeit genuine, volatility spikes in 2008, which dwarfed the fluctuations in the remaining time series, all three measures were winsorised at the 0.5th and 99.5th percentiles.

Usage of Logarithmic Data

When running regression (2) in level data, naturally, the magnitude of the event window coefficient β_2 depends on the scale of, and data frequency used for, the relevant liquidity and volatility measure. To create better comparability between coefficient sizes, the formal regression analysis was conducted using logarithmic transformations of all variables except dummies. Moreover, this allows for an elasticity interpretation of coefficients.

VI. Results

Visual Analysis

Liquidity: Figures C.3 to C.10

Graphs of all liquidity and volatility measures' values across the entire sample period are displayed in appendix C. Figures C.3 to C.10 show the four (il)liquidity measures' evolution over time - recall that higher numerical values signal lower liquidity. Several conclusions can be drawn: first, most liquidity measures experience recurring spikes in regular intervals. These spikes occur on the very final days of a given futures contract's lifetime, when trading activity shifts from the currently next-to-expire contract (the lead contract) to the second-to-expire contract, which is about to become the new lead contract. As explained above, contracts are staggered in timeframes of three months, so liquidity transitions and the associated spikes, occur in intervals of three months. Not only across measures, but also within measures, these recurring spikes are of different magnitudes. It seems that in times of greater general market turmoil, such as the global financial crisis or the first Covid wave, transitions from the lead contract to the second-to-expire contract created greater liquidity squeezes than in calmer times.

Second, most of the irregular, more pronounced spikes coincide for the UK and US: large and prolonged spikes during the global financial crisis around and after 2008, several smaller spikes in the crisis's aftermath in 2010 and 2011, the first Covid wave in March 2020, and finally, the phasing out of many LIBOR maturities towards the end of 2021. On a closer look, however, we can see that for all measures, the end-of-2021 spike systematically begins earlier for GBP LIBOR than for USD LIBOR. Zooming in shows that this increase starts roughly in early October for the former and in early December for the latter. One possible explanation for this difference is the fact that whilst GBP LIBOR was going to be irrevocably terminated on 31/12/2021, for USD LIBOR, this was only the *recommended* termination date for LIBOR users. USD LIBOR users knew they could continue using it if they did not manage to complete the transition process by the end of 2021, so they might have felt less pressed for time in discontinuing usage of the rate.

Second, there is no clearly visible long-term trend in liquidity brought about by any event. Instead, the spikes and higher levels of illiquidity are always relatively short-term and transitory; no event appears to have had a lasting change or to introduce a subsequent trend. The most persistent event is the financial crisis; yet also in this case, after its peak around the end of December 2008, illiquidity quickly improves again and is back to relatively low levels by January 2010. Comparing the two rates, it appears that the USD LIBOR has slightly more pronounced liquidity spikes than GBP LIBOR.

The first three events under consideration seem to have had neither a long-term impact, as can be seen from the unaffected overall time series, nor a short-term effect: in some cases, an event occurs just after a peak, in others just before, and sometimes in-between two spikes. It is probably naïve to think the manipulation suspicions, Barclays' official admission or Bailey's discontinuation announcement were a surprise to *all* market participants: not only were the major players who conducted the manipulations aware of them in general, but we have also seen in the literature review above that regulatory authorities were even alleged of *encouraging* LIBOR's fixing rather than preventing it (Treanor, 2012; Scott, 2012; and Verity, 2017). Thus, we cannot rule out that large and well-connected players were aware of the first three events before they officially became public; as a result, it is conceivable that part of their effects materialised already before the event date. It seems implausible, however, to think that *all* market participants anticipated the first three events such that there should be no temporally coinciding effect at all. Moreover, in the case of anticipation by some players, we would expect to see a reaction building up in the days or weeks before the event, and it coming to full force on the day of the event. We see no such pattern for any measure applied to any of the first three events.

To summarise, for each of the first three events, we would expect the timings of an event and its effect to coincide, potentially with a build-up in the days or weeks before the event caused by insiders' reactions. In the rare instances where spikes and event timings do coincide, spikes appear to be neither of economically significant size compared to their immediate neighbours and they tend to be only very brief. The perhaps most important insight appears to hold independent of anticipation considerations: the time around the discontinuation announcement, even if expected by some market participants, did not bring a meaningful and lasting deterioration in liquidity. In other words, regulators did not spark four-and-a-half years-long period of ever worsening liquidity by announcing the discontinuation already in mid-2017. Giving market participants plenty of notice about a market transition does not necessarily imply creating an equally long period of illiquid markets.

Indeed, clearly visible increases in illiquidity seem to have occurred only in the final weeks and months leading up to the (recommended) LIBOR discontinuation in December 2021. For three of the four measures, this effect seems to be of a similar order of magnitude as the effect of the first wave of the Covid-19 pandemic. The fact that this holds also for USD LIBOR might at first be surprising since 3M USD LIBOR will not be phased out until June 2023. Recall, however, that regulatory authorities including the financial conduct authority (FCA) in the UK and the Federal Reserve Board and the Office of the Comptroller of the Currency in the US have advised banks to terminate using USD LIBOR for new contracts by 31/12/2022 (Board of Governors of the Federal Reserve System et al., 2021). If financial institutions have followed this recommendation, and moved their capital out of USD LIBOR futures, a marked increase in illiquidity as observed would indeed be expected.

Evaluating whether the discontinuation-induced illiquidity is large and extended or not is somewhat in the eye of the beholder, of course, but it seems we can say at least the following:

the end-of-sample illiquidity spikes begin about four weeks and three months before the discontinuation date for the USD and GBP LIBOR, respectively. Given that the discontinuation date had been known since 2017, liquidity disruptions could well have started significantly earlier. For both currencies, three out of the four liquidity measures used display illiquidity spikes roughly in the same order of magnitude as those caused by the first Covid wave. Thus, whilst the disruptive costs of a discontinuation appear to be temporally limited to a few weeks, in magnitude per day they are among the largest disruptions in the sample, consistently outsized only by the disruptions caused by the financial crisis.

To conclude, we cannot see any effect of the first three events. The lack of a systematic occurrence of spikes in their temporal vicinity also makes it difficult to define an event window in which to even try to prove their significance; an additional difficulty is the general noise the illiquidity series exhibit. Thus, the formal analysis below focusses on the effect of the discontinuation.

Volatility: Figures C.11 to C.16

The observations one makes from the visual analysis of the three volatility measures are similar to those of the liquidity measures: some clearly discernible spikes appear to fit the three month-pattern of the lead contract redesignation, although the differences in sizes of these spikes both across measures and within are much more pronounced than for the liquidity time series. The link between the lead contract redesignation and volatility seems to be weaker than is the case for liquidity. Irregular spikes, i.e., those that appear not to be associated with the quarterly contract expiry, also coincide for the GBP and USD samples, although at times with different intensities and relative magnitudes. All three measures tend to yield very similar results on the timing of spikes, except for the spike towards the end of 2021, which here too begins around early October for GBP LIBOR and around early December for the USD rate. The relatively high degree of agreement both across measures and between samples lends some confidence to the following conclusions:

First, as is the case for liquidity, there appears to be no long-term trend or structural change caused by any event; the temporally longest development is the post-financial crisis reduction in volatility. The events with largest impacts, in descending order, appear to be the global financial crisis in 2008 and the first wave of the Covid pandemic in early 2020; the measures disagree on the event with the third largest impact: either spikes in the aftermath of the global financial crisis in 2010 and 2011, or the termination of GBP LIBOR and recommended discontinuation date for USD LIBOR users in December 2021.

Second, the first three key events also do not seem to have a short-term effect. Zooming in on the measures' time series suggests that there appears to be no systematic relationship between these events and the timing or magnitudes of short-term spikes – as above, sometimes spikes occur before, sometimes after the events, and never does there seem to be a spike in the vicinity of these events that is different in magnitude to the spikes the series exhibit throughout. As with liquidity, it seems policymakers did not need to worry about causing prolonged volatility by announcing the discontinuation four and a half years before implementing it.

An increase in volatility beginning in October and December 2021 for GBP and USD LIBOR respectively is clearly visible in all measures, on the other hand. These spikes also appear to be more pronounced relative to the remainder of the time series than is the case for the liquidity measures and they last until the end of the samples, also for the USD sample. Again, whilst being only a few weeks long, the end-of-sample volatility spikes are among the largest spikes in the sample; unlike for liquidity, however, they tend to be significantly smaller than those related to the Covid disruptions.

The finding that the first suspicions of manipulation in 2008 and Barclays' admission to rate rigging in 2012 fail to increase volatility is consistent with Gensler (2012) and Hou and Skeie (2014), who, recall from above, point out that USD LIBOR rates' volatility between 2007 and 2012 was *lower* than that of comparable short-term interest rates.

It appears that only the immediate prospect of the termination, once it was less than three months and less than one month away, for GBP and USD LIBOR respectively, has impacted market functioning. Analysing and confirming the discontinuation's effects' statistical significance will be the focus of the next section.

Formal Analysis

For almost all measures, the largest spikes occur around the global financial crisis and the first Covid wave in the first half of 2020. Thus, clearly, liquidity and volatility in LIBOR futures are affected by events that cause uncertainty and disruption in the wider financial markets. The critical identification question is thus whether the spikes beginning towards the end of 2021 can be attributed to the LIBOR discontinuation or if they are due to an altogether different event causing uncertainty in the financial markets. This motivates the regression analysis described above on the Chicago Board Options Exchange's *CBOE Volatility Index* (VIX), following regression (2).

The regression results on overall volatility and liquidity are reported in tables 3 to 5; tables 6 to 8 report unexpected liquidity and volatility. Following the observation that the end-of-2021 spike in all measures begins in early October for GBP LIBOR and in early December for USD LIBOR, the chosen start dates of the discontinuation event windows are 1 October and 1 December 2021, respectively. The choice of the discontinuation event window is justified, and its implications for the interpretation of the event window coefficients are discussed in greater detail, in section VIII.

With the event window defined as above, we can see that for the USD LIBOR, for all measures of both volatility and liquidity, the coefficient on the discontinuation event window *post1/12/2021* is significant at the 1% level in six cases and at the 5% level in the remaining two cases. Moreover, it is positive for all measures. For the GBP LIBOR regressions, the coefficient on *post1/10/2021* is positive and significant at the 1% level in all seven cases.

Comparing the sizes of the event window coefficients, we find that for the liquidity measures, they are consistently higher in the USD regression than those in the GBP regressions. Specifically, for GBP and USD LIBOR respectively, the event window sees an increase in the Amihud measure of about 16% and 23%, of 30.5% and 73% in volatility over volume, and a rise in the effective spread of about 10% and 47%. The original Roll measure increases by about 17.5% and 24%, and the absolute value-based Roll measure rises by roughly 15% and 21%. Thus, it seems that whilst the effect of the discontinuation on USD LIBOR began to materialise two months later than that on GBP LIBOR, once it did take place, liquidity was affected more strongly in the USD market. This appears to be in line with Huang and Todorov’s (2022) for LIBOR forward rate agreements (FRAs): the authors find that out of CHF, GBP, JPY and USD-denominated FRAs, the latter experienced the largest drop in trading volume of more than 97% between 2019 and 2022.

Among volatility measures, again for GBP and USD respectively, the Garman-Klass measure rises by 99% and 57% during the event window, whilst the Schwert measure increases by 212% and 60%, and the Realised Squared Returns by 53% and 84%. Both the large differences in magnitudes as well as the fact that there is no perfect way to define the discontinuation event window (see section VIII) make it difficult to draw precise conclusions on the size of the discontinuation’s effect on volatility. Nevertheless, this appears to be reasonably strong evidence that volatility has been affected more strongly than liquidity, in the sense that it has experienced a greater relative increase.

An additional interesting finding is how the coefficients on *VIX_close* compare for liquidity and volatility measures: whilst it is somewhere in the vicinity of unity in four out of five liquidity regressions for both currencies, it is significantly above unity for all volatility measures, in four out of the six volatility regressions even around 2.5 – suggesting a 2.5% increase in USD LIBOR volatility for a 1% rise in market-wide volatility. Thus, everything else equal, LIBOR illiquidity seems to move roughly proportionally with market-wide uncertainty, whereas LIBOR futures are significantly more volatile than the VIX.

Tables 6 to 8 with unexpected liquidity and volatility as dependent variables confirm all these conclusions: using these yields very similar results, for many regressions the only significant difference appears to be in the regression constant. To summarise, it seems we can confirm the impression from the visual analysis that the end-of-2021 spikes in (il)liquidity and volatility are statistically significant, also when controlling for other events that cause movements in these two across financial markets – this holds both for overall liquidity and volatility, as well as for measures of their unexpected or abnormal component developed by Kawaller et al. (2001).

Interpretation & Results' Economic Significance

Events 1&2: suspicions and confirmation of widespread manipulation

Neither the 2008 WSJ article raising the first manipulation suspicions (Mollenkamp and Whitehouse, 2008), nor Barclays' official admission to this in 2012 appear to have had a discernible impact on liquidity and volatility in the markets under consideration. Economic theory suggests at least two mechanisms these events *could* have had an impact: first, by creating uncertainty in these markets. Investors become concerned about what the revelations mean for the stability of the market, withdraw their funds and their concern becomes a self-fulfilling prophecy: liquidity decreases and volatility increases.

Second, a more sophisticated mechanism involves market participants' perceptions about the presence of insider traders. Informed trading models in the spirit of Glosten and Milgrom (1985) and Easley and O'Hara (1987 & 1992) suggest that the bid-ask spread, a measure of liquidity also used in this paper, increases in the share of insider traders in the market. Intuitively, if insiders know about the intrinsic value of a security and, naturally, only make trades they think advantageous to themselves, market makers make a loss when trading with insiders. If market makers cannot distinguish insider traders from non-insiders, they need to compensate for a higher share of insiders through a wider bid-ask spread; see the papers for more details. In the context of the manipulation revelations, informed trading models would suggest the first two events might have the following effect on liquidity and volatility.

Suppose there was a number of traders that was not aware of the manipulation suspicions before the 2008 WSJ article, and a number of traders that did not believe in these suspicions with certainty before Barclays' 2012 admission. In both cases, upon the revelation of this new information to the market, these traders update their beliefs; realising there was a larger share of money invested by insiders than previously thought, so that market conditions are more adverse to them than initially assumed, some non-informed traders leave the market in the aftermath of these two events. This not only reduces overall trading activity, but more importantly in this model, increases the share of informed traders. In addition, if market makers do not have perfect knowledge about the share of informed traders in the market, these two events are also likely to make them update their beliefs to include a higher share of insiders, over and above the change in the actual share of insider traders caused. Both effects will cause market makers to impose a larger bid-ask spread, i.e., liquidity to decrease.

Now, none of our liquidity measures, including the effective spread and Roll measures, suggest either of these two events have had an effect on liquidity. So where does the mechanism described above fail? In our mind, there are two possibilities: First, some non-informed traders did leave the market and market makers did update their beliefs to a higher share of insiders, but the relationship between the share of insiders and the bid-ask spread as Glosten and Milgrom (1985) and Easley and O'Hara (1987 & 1992) suggest does not hold. See Dolgopolov (2004) for a detailed discussion of this possibility. Second, neither traders' nor market makers' perceptions of informed trading were significantly changed, perhaps because the widespread presence of manipulation was an open secret already before. As a result, there was also no significant shift of trading activity away from our markets.

The fact that none of the four liquidity measures suggest changes in trading activity might be interpreted as evidence of the latter option. The logical follow-up question then is whether non-insiders have gradually dropped out of the market already in the years before as market participants became aware of the manipulations; after all, some authors believe manipulations had been occurring already since the early 1990s (Keenan, 2012). Such a long-term, gradual change might be very difficult to identify, however, and certainly is beyond the scope of this paper. Nevertheless, it might be an interesting question for further research.

Events 3&4: announcement and execution of discontinuation

The analysis of the discontinuation's announcement in 2017 and its factual or recommended execution at the end of 2021 for GBP and USD LIBOR, respectively, sheds light on how markets react in case of a discontinuation or large change of important reference rates. First, we have seen that once effects in liquidity and volatility start to materialise, this happens close to simultaneously, and whilst liquidity decreases, volatility decreases. In the debate between authors finding a positive relationship between volatility and liquidity in futures markets (recall from above: Bessembinder and Sequin, 1992; Cornell, 1981; Foster, 1995; Grammatikos and Saunders, 1986; and Rutledge, 1979), and those finding an inverse association (Daigler and Wiley, 1999; Kawaller et al., 2001; and Tauchen and Pitts, 1983), this puts the paper on the side of the latter group. Second, we have seen that liquidity and volatility deteriorate abruptly rather than gradually, and only in the final weeks before LIBOR's (recommended) termination. This second finding can inform the design of similar policy interventions on reference rates:

The LIBOR scandal was a prominent example of a choice regulators often have when confronted with the abuse of some financial instrument or market: the choice between discontinuation and reform. Two major advantages of an outright discontinuation are, first, the strong signal and commitment this shows towards rectifying what is wrong with the discontinued instrument – in this case, manipulation potential of LIBOR – and second, that a discontinuation is the most thorough way of eradicating all the structural problems which enabled the instrument's abuse in the first place. These advantages must be weighed against the downsides of course, the perhaps most important of which are the financial stability and disruption risks. Recall from above that discontinuation sceptics among both regulators and practitioners have warned about these risks also in the case of the LIBOR discontinuation (including Alternative Reference Rates Committee, 2021; Bruggia and Rank-Broadley, 2021; Reserve Bank of Australia, 2021; and SEC, 2019).

This paper's results shed light on the extent these risks have materialised: the fact that the 2017 discontinuation announcement did not cause significant disruptions in liquidity and volatility, at least compared to those in the weeks leading up to the discontinuation, shows that giving market participants plenty of notice about a market's termination does not necessarily come at the cost of an equally long period of disrupted markets; policymakers do not face an inescapable trade-off between giving markets sufficient time to conduct the transition and minimising the period of turmoil initiated by the announcement.

Instead, the financial disruption risks critics have pointed to have materialised only in the about one and three months leading up to the discontinuation, during which considerable liquidity and volatility deteriorations can be seen for USD and GBP LIBOR, respectively. Whether this is deemed prolonged or not is a much more subjective question, of course; what seems clear, however, is that these disruptions could in theory have started much earlier. In terms of these disruptions' severity, almost all measures suggest that the illiquidity and volatility spikes were the third largest in the 15-years sample, after those during the global financial crisis and during the first Covid wave. Thus, whilst disruptions were non-trivial, they were surpassed by the disruptions caused by two other events – rather than dwarfing everything the market had seen before. As with the duration of these disruptions, their severity could in theory have been much greater than turned out to be the case.

Table 2. Liquidity and Volatility Tests' Summary Statistics

(IL)LIQUDITY MEASURE	LIBOR Rate	Obser-vations	Mean	Standard Deviation	Q25	Q50	Q75
Amihud	GBP	3,591	3.24e-11	3.72e-11	1.31e-11	2.38e-11	3.80e-11
	USD	3,573	6.81e-12	7.72e-12	7.72e-12	4.46e-12	7.95e-12
Volatility/Volume	GBP	3,592	6.59e-06	.0000147	1.99e-06	3.42e-06	6.11e-06
	USD	3,574	2.99e-06	4.23e-06	7.84e-07	1.59e-06	3.26e-06
Effective Spread	GBP	3,147	.000098	.0001222	.0000505	.0001001	.0001011
	USD	3,567	.0000772	.0000862	.000041	.0000505	.0000761
Roll Measure	GBP	3,592	.0001553	.0003469	.0000253	.000076	.0001543
original	USD	3,574	.0001304	.0002675	0	.0000449	.0001301
Roll Measure	GBP	3,592	.0001808	.0003493	.0000499	.0000922	.0001774
absolute values	USD	3,574	.0001694	.0002852	.0000327	.0000773	.0001688

VOLATILITY MEASURE	LIBOR Rate	Obser-vations	Mean	Standard Deviation	Q25	Q50	Q75
Realised Returns²	GBP	3,591	9.44e-08	3.69e-07	2.61e-09	7.46e-09	2.83e-08
	USD	3,574	7.97e-08	5.05e-07	6.15e-10	2.55e-09	2.26e-08
Schwert Measure	GBP	3,592	7.20e-08	2.97e-07	6.06e-10	4.48e-09	2.43e-08
	USD	3,574	5.80e-08	3.33e-07	5.41e-10	4.07e-09	1.79e-08
Garman-Klass	GBP	3,530	1.64e-07	5.47e-07	9.18e-09	3.18e-08	8.77e-08
	USD	3,574	2.58e-07	1.37e-06	9.10e-09	2.27e-08	8.68e-08

This table reports the descriptive statistics of the outcome measures. Q25, Q50 and Q75 refer to the 25th, 50th and 75th percentiles, respectively. Recall from table 1 the varying numbers of observations in the raw data. Since the different measures employed in this paper also use different variables, this also results in varying numbers of observations in the liquidity and volatility measures, as can be seen here. We will see in tables 3-5 below that this is most conspicuous perhaps in the case of the original Roll measure in column 5 of tables 3 & 4, which sets periods with positive return autocovariance equal to zero, and so creates missing values in its log version. This is the major driver of the smaller number of observations in the Roll regression. As a robustness check, Roll's absolute value-based version, equation (11), which uses also positive autocovariance values, was calculated in this study as well. In addition, all regressions in tables 3-5 below have a slightly fewer observations than the liquidity and volatility measures reported here due to a small number of missing observations in the *VIX_close* sample.

Table 3. Regression (2): GBP LIBOR Liquidity Results

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.937***	0.932***	0.746***	0.842***	1.067***
	(0.0143)	(0.0121)	(0.0112)	(0.0180)	(0.0224)
post1/10/2021	0.161***	0.305***	0.0962***	0.175***	0.154***
	(0.0512)	(0.0338)	(0.0355)	(0.0676)	(0.0506)
Reg. Constant	-6.312***	-15.13***	-2.473***	-11.38***	-12.12***
	(0.0454)	(0.0376)	(0.0395)	(0.0558)	(0.0695)
Observations	3,502	3,503	3,132	2,805	3,461

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. For space reasons, the monthly dummies and GARCH model coefficients are not reported here, but available upon request. All non-dummy variables, i.e., the dependent variables and VIX_close were used in log form.

Table 4. Regression (2): USD LIBOR Liquidity Results

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	1.004***	1.178***	0.387***	0.903***	1.122***
	(0.0122)	(0.0121)	(0.0147)	(0.0280)	(0.0274)
post1/12/2021	0.232***	0.727***	0.471***	0.239**	0.207**
	(0.0370)	(0.0376)	(0.0389)	(0.108)	(0.100)
Reg. Constant	-8.167***	-4.661***	-1.854***	-5.260***	-5.828***
	(0.0337)	(0.0388)	(0.0453)	(0.0870)	(0.0945)
Observations	3,567	3,570	3,565	2,517	3,454

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Regression (2): GBP & USD LIBOR Volatility Results

VOLATILITY MEASURE	GBP Realised Squared Returns	GBP Schwert Measure	GBP Garman Klass Measure	USD Realised Squared Returns	USD Schwert Measure	USD Garman Klass Measure
VIX_close	1.714***	2.619***	2.377***	2.066***	2.504***	2.541***
	(0.0236)	(0.0483)	(0.0592)	(0.0308)	(0.0662)	(0.0596)
post1/10/2021†;	0.531***	2.124***	0.988***	0.836***	0.599***	0.571***
post1/12/2021‡	(0.0507)	(0.188)	(0.183)	(0.0743)	(0.113)	(0.157)
Reg. Constant	-23.61***	-25.69***	-24.61***	-10.98***	-12.36***	-11.23***
	(0.0736)	(0.160)	(0.199)	(0.0953)	(0.223)	(0.196)
Observations	3,503	2,950	3,448	3,540	2,957	3,574

Std. error in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

† regressor used for the GBP sample; ‡ regressor used for the USD sample.

Table 6. Regression (2): GBP LIBOR Unexpected Liquidity Results

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.928***	1.016***	0.742***	0.842***	1.082***
	(0.0147)	(0.0159)	(0.0108)	(0.0180)	(0.0227)
post1/10/2021	0.172***	0.134***	0.0986***	0.175***	0.241***
	(0.0511)	(0.0396)	(0.0354)	(0.0676)	(0.0613)
Reg. Constant	-2.544***	-2.844***	-2.270***	-2.171***	-2.919***
	(0.0474)	(0.0468)	(0.0382)	(0.0558)	(0.0704)
Observations	3,502	3,503	3,565	2,805	3,461

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. For space reasons, the monthly dummies and GARCH model coefficients are not reported here, but available upon request. All non-dummy variables, i.e., the dependent variables and VIX_close were used in log form. The larger number of observations in tables 6-8 compared to tables 3-5 are due to gaps in the original data that would be filled by the prediction of $measure_t^{AR(5)\,fit}$.

Table 7. Regression (2): USD LIBOR Unexpected Liquidity Results

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.999***	1.178***	0.426***	0.904***	1.055***
	(0.0105)	(0.0121)	(0.0130)	(0.0280)	(0.0269)
post1/12/2021	0.234***	0.727***	0.386***	0.239**	0.286***
	(0.0348)	(0.0376)	(0.0262)	(0.108)	(0.0939)
Reg. Constant	-2.805***	-2.716***	-1.405***	-2.762***	-3.142***
	(0.0294)	(0.0388)	(0.0425)	(0.0870)	(0.0894)
Observations	3,567	3,570	3,565	2,517	3,454

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Regression (2): GBP & USD LIBOR Unexpected Volatility Results

VOLATILITY MEASURE	GBP Realised Squared Returns	GBP Schwert Measure	GBP Garman Klass Measure	USD Realised Squared Returns	USD Schwert Measure	USD Garman Klass Measure
VIX_close	1.898***	2.619***	2.453***	2.066***	2.504***	2.542***
	(0.0285)	(0.0483)	(0.0649)	(0.0308)	(0.0662)	(0.0609)
post1/10/2021†;	2.659***	2.124***	0.786***	0.836***	0.599***	0.438**
post1/12/2021‡	(0.0889)	(0.188)	(0.216)	(0.0743)	(0.113)	(0.199)
Reg. Constant	-5.854***	-6.713***	-7.453***	-6.133***	-7.312***	-7.635***
	(0.0871)	(0.160)	(0.217)	(0.0953)	(0.223)	(0.203)
Observations	3,503	2,950	3,448	3,540	2,957	3,574

Std. error in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

† regressor used for the GBP sample; ‡ regressor used for the USD sample.

VII. Robustness Tests

We might wonder to what extent regression (2)'s results are dependent on the choice of ARMA lags in the error model. For each measure in both currencies, tables D.1 to D.3 in appendix D report the results of regression (2) for the ARMA specifications with the second-lowest BIC: in some cases, this requires significant changes in the choice of ARMA lags. We see that all regressions' results are very robust to the ARMA model choice: for both the coefficients on *VIX_close* and *post1/10/2021* or *post1/12/2021*, signs and significance levels are *exactly* the same in all regressions across both currencies, except for the Roll measure in the GBP sample, where the *post1/10/2021* is significant at 1% in table 3 and at the 5% level in table D.1. Coefficient sizes are very similar, too: across the eight liquidity measures for two currencies, we have a total of 32 estimations of coefficients on *VIX_close* and *post1/10/2021* or *post1/12/2021*; in seven out of these 32 cases is the difference in coefficient magnitudes between the best and second-best ARMA model larger than 0.05; it never exceeds 0.2. Coefficient signs, significance levels and magnitudes are also very similar across ARMA model choices for the monthly dummies – these are not reported here for space reasons, but available upon request.

Additional support to our conclusions is lent if we solve the problem of heteroskedasticity and serial correlation in residuals using an alternative method to GARCH modelling: tables D.4 to D.6 report regression (2) estimated by generalised least squares (GLS). We see that for most regressions, the size of the coefficients on *post1/10/2021* or *post1/12/2021* stay in the same order of magnitude, often remarkably similar. Only in two cases does coefficient significance change: in the Volatility over Volume regression in the USD sample, *post1/12/2021* is no longer significant; for the GBP Realised Squared Returns regression, *post1/10/2021* is now highly significant with a p-value of 0 to the third decimal place. This appears to strengthen our observation from before: whilst precise coefficient magnitudes change across measures, currencies and regression specification, the positive sign and significance at 5% or more is very robust.

Tables D.7 to D.9 in appendix D report the results of regression (2) with time dummies for quarters instead of years. This is a less granular control for seasonality; more importantly, as each individual futures contract is considered for three months in this study (see V. Estimation and Specification for more details), this does not control for residual maturity. For the GBP LIBOR liquidity tests, we see that the coefficients on *post1/10/2021* are all in the vicinity of the results in table 3, with deviations of at most 0.1. With the exception of the effective spread measure, which had the lowest significance level also in the original regression of table 3, $\hat{\beta}_2$ is positive and significant at the 1% level. A similar picture holds for USD liquidity in table D.8: the results for $\hat{\beta}_2$ are in the same order of magnitude, with no systematic decrease or increase and all remain positive and significant at the 1% level.

Turning to the volatility tests, for both GBP and USD LIBOR, the coefficients on the discontinuation event window in the regressions using quarterly dummies in table D.9 are close to those in table 5. All remain positive and significant with the exception of the original Roll measure, which had a p-value of 13.8% in the regressions with monthly dummies and a p-value of 17% in those with quarterly dummies. The coefficients on VIX_close_t are remarkably similar across all regressions.

Finally, we might wonder about the high volatility our liquidity and volatility series exhibit across the entire sample. How does this affect the significance of the liquidity and volatility deteriorations this study finds – could what this study describes as the discontinuation's effects partly or wholly really be driven by the general volatility in the data? To address this question, tables D.10-D.15 repeat regression (2) on winsorized liquidity and volatility measures. Both the winsorization of the top 10% and top 20% of values confirm our results from before: whilst coefficient magnitudes vary both across measures and between specifications, their consistently positive sign and high significance is robust. In fact, the discontinuation event window coefficients are significant at the 1% level in each of the 32 regressions. Comparing results from the data winsorized at 10% to our main results, the discontinuation coefficients become larger in seven out of ten regressions for liquidity and in five out of six cases for volatility. Winsorizing at 20% appears to have no systematic effect on discontinuation coefficient sizes relative to 10%; some become slightly larger, some smaller. In short, it seems the outliers and spikes our time series exhibit have no effect on the conclusions from our main findings. Finally, the findings of appendix D also provide confirming evidence that volatility appears to be more strongly affected by movements in VIX than liquidity.

VIII. Limitations

Potential criticisms and limitations of this study include the following three. First, recall our identification assumption that the fraudulent LIBOR submissions are a natural experiment and so exogenous to other factors driving liquidity and volatility in LIBOR futures markets. Similarly, the points in time at which the first suspicions of manipulations were raised, at which the first banks officially admitted to manipulation, at which the possibility of a discontinuation was first discussed and the chosen dates for GBP and USD LIBOR's discontinuation are assumed to be exogenous. It might be argued, however, that the probability of detecting signs of manipulation is higher at times where manipulation occurs to a particularly stark degree; in other words, Mollenkamp and Whitehouse (2008) might have been written and published at a time where manipulation was especially uninhibited. Likewise, banks' first admissions to manipulation, the first hints at a potential discontinuation and the discontinuation dates might have occurred under the pressure of scrutiny caused by intense manipulation. As a result, we may think the events are not perfectly exogenous to the development of liquidity or volatility in LIBOR markets.

The main drivers of the above four key events do not seem to be directly related to liquidity or volatility developments, however: Mollenkamp and Whitehouse's (2008) main piece of evidence is the divergence of the LIBOR rate from the rates in the default-insurance market that had been going on for almost four months when the article appeared at the end of May 2008. Barclays' admission to manipulation and its concomitant agreement with the US Department of Justice to a settlement of $160 million ensued after multiple years of criminal investigations. Financial Conduct Authority (2017) justifies the prospect of discontinuing LIBOR by the end of 2021 with the difficulty of implementing reforms that have been suggested three years earlier, in 2014. Finally, at the time of coming into effect, the LIBOR discontinuation had been settled and announced for multiple years. To put it in a nutshell, these four events appear to be driven by long-term observations and developments independent of liquidity or volatility in their temporal vicinity; this paper maintains the exogeneity assumption.

Second, even though the LIBOR scandal was uncovered only in 2012, recall from above that systematic manipulations by LIBOR submitters might have occurred and remained undetected since the early 1990s (Keenan, 2012). Thus, when interpreting our results, we should remember that we cannot draw firm conclusions on the behaviour of an unmanipulated market from these data; indeed, as discussed above, to key players in the market, some events might not have come as a surprise. Similarly, we have assumed that first, regulators aim to minimise market disruption and are competent in doing so and that second, VIX is unaffected by the LIBOR scandal. If the former assumption holds and the latter fails, we underestimate events' effects on LIBOR futures liquidity and volatility. Whilst this strengthens the conclusion that the discontinuation has had a significant effect, it weakens the conclusion that the first three events have not. Nevertheless, since out of these three events, regulators had direct control over the timing of the third event only, and as there does not appear to be any systematic relationship between these events' timings and the occurrence of spikes of *any* size, this conclusion does not seem altogether untenable.

Third and finally, the start dates of the discontinuation event window were chosen so as to fit the end-of-2021 increase in liquidity and volatility observed in the visual analysis. Thus, it might be criticised that the event window is somewhat reverse engineered. Whilst this is true, it is also in the nature of an event which has been announced for several years rather than coming as a surprise that it is not perfectly clear how to define its start date. One obvious possibility is to look at the announcement date – which has been done in this study, and no effect was found at this point. Conversely, looking only at the very day of the discontinuation not only fails to give justice to the fact that the volatility and liquidity increases are multiple weeks long, but also is statistically infeasible: it would result in a sample size of one and, recall from above, in the case of GBP LIBOR, data are available only until 17/12/2021. Thus, how else to define the event window if we do not want to choose an altogether arbitrary starting date?

To our mind, the best way to deal with the fact that there is no non-arbitrary way of defining the discontinuation event window is (i) to acknowledge the arbitrariness of any event window definition, (ii) to appreciate that whilst the discontinuation's β_2's size changes when the discontinuation event window is altered, its statistical significance at the 5% level or higher does not (not reported, but available upon request) and (iii) as a result of this, to interpret the coefficients as indicative of a statistically significant deterioration in volatility and liquidity in the final weeks of 2021, rather than pinning down the precise magnitude of this effect. Obviously, with an effect that starts to materialise at some point in time, its size is also a function of the event window definition.

IX. Conclusion

This paper analyses to what extent four key events related to the LIBOR scandal have influenced two aspects of financial market functioning – liquidity and volatility – in LIBOR markets and attempts to draw the relevant lessons on regulators' decision to respond by altogether discontinuing LIBOR rather than merely reforming it. Three out of the four events do not appear to have an economically or statistically discernible effect on either liquidity or volatility: the publication of the first suspicions that LIBOR may have been manipulated systematically and on a grand scale in April 2008; the admission of the first large financial institution to these suspicions, Barclays, in June 2012; and the first public discussion of the possibility of an eventual discontinuation of LIBOR by the head of the FCA Andrew Bailey in July 2017. The discontinuation of GBP LIBOR, and the recommended end date for USD LIBOR users, on the other hand, appears to have caused a significant deterioration in both liquidity and volatility. In the context of our initial hypothesis, this means that market participants and the liquidity they provide would reorientate away from LIBOR markets on a grand scale only once its irreversible end was nearing.

The effects on GBP LIBOR's and USD LIBOR's volatility and liquidity associated with the discontinuation appear to start materialising in early October 2021 and early December 2021, respectively. Potentially, this difference is due to the inalterability of the GBP LIBOR discontinuation, whereas USD LIBOR users that failed to meet the recommended end date on 31/12/2021 could continue to use LIBOR, if needs be until June 2023. This may have resulted in a greater sense of urgency to leave the GBP market than in its USD counterpart. Visual analysis suggests that for all measures and both currencies, the effects were significantly smaller than during the financial crisis, and for almost all, they were smaller than during the first wave of the Covid pandemic in spring 2020.

In the context of the two policy questions we posed in the introduction, we can thus draw the following conclusions: first, the disruptions policymakers induce by discontinuing a set of large financial markets like the LIBOR markets, whilst being of significant magnitude, need not be many months or even years long. Relatedly, announcing a market's eventual discontinuation does not necessarily induce an exodus of liquidity or an increase in volatility right away. Policymakers do not need to be concerned that every month of notice they give to market participants creates an additional month of deteriorated liquidity and volatility in this market. More generally, it seems opposing a market termination in favour of a reform that keeps a market in existence on the grounds of excessive risks to disruption in liquidity and volatility, as the critics of the discontinuation did, holds up to scrutiny only if the alternative reform causes very little disruption indeed.

Given these results, two more pieces of information required to evaluate regulators' trade-off between reforming a financial instrument prone to abuse or altogether discontinuing it are required: first, what are the costs in terms of market disruption, in volatility, liquidity and otherwise, of a reform that does not go so far as to abolish a market? Second, do reforms tend to be less effective in rectifying what is wrong with an instrument than an outright cessation and replacement, and if so, to what degree? These are interesting questions for further research; together with the results of this paper, their answers could help policymakers choose between reform and abolishment of a financial instruments in need of this in the future.

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Appendix A. Liquidity and Volatility Tests' Econometric Details

Liquidity Measures: Econometric Details

This section outlines how the liquidity measures used in this paper are calculated and what variables have been used for this. For detailed derivations, please refer to the original papers that developed them; references are provided.

Price Impact Measure I: Amihud (2002) Illiquidity

In its daily data-based version, Amihud's statistic takes the average of the ratio of the absolute value of daily returns and trading volume data; intuitively, this gives us the change in security prices for a unit-change in the order flow:

$$Amihud_T = T^{-1} \sum_{t=1}^{T} \frac{|r_t|}{V_{\$,t}} \tag{5}$$

Given trading volume in the denominator, small values of $V_{\$,t}$ can lead to $Amihud_T$ being strongly affected by small changes in r_t; avoiding this is the reason for taking an average over multiple weeks. Following Amihud (2002), for T, this paper uses the average number of trading days in a month, T=21. $V_{\$,t}$ is calculated as the dollar (or GBP) volume, i.e.:

$$V_{\$,t} = V_t * P_{C,t} \tag{6}$$

The reason this is a measure of *illiquidity* rather than liquidity is that larger values of the statistic imply returns (and so prices) are moved relatively more by a given trading volume, i.e., a larger value implies an asset is *less* liquid. Intuitively, the smaller is $Amihud_T$, the smaller is the price change for a given order size, and so the more liquid is the market according to the price impact definition of liquidity.

Price Impact Measure II: Volatility Over Volume

The volatility over volume measure, due to Fong et al. (2017), calculates the variance of daily closing prices σ_t^2 following the standard formula and normalises them by dollar or GBP volume $V_{\$,t}$, which continues to be calculated following equation (6). Taking the square root, result is essentially a trading volume-weighted standard deviation:

$$VoV_t = \sqrt{\frac{\sigma_t^2}{V_{\$,t}}} \tag{7}$$

σ_t^2 was calculated daily based on a rolling seven-day sample with three lead and three lag terms.

Bid-Ask Spread Measure I: Effective Spread Measure

The effective spread measure, used among others in Goyenko et al. (2009), complements the other liquidity measures used here in that it relies not just on closing prices P_t, but also on the end-of-day bid and ask prices $P_{B,t}$ and $P_{A,t}$. Implicitly assuming a symmetrical bid-ask spread, the effective spread measure calculates the security's "true intrinsic value" as the mean value of P_B and P_A and subtracts this from the daily closing price:

$$Effective\ Spread_t = 2 * \left|\ln(P_t) - \ln\left(0.5 * \left(P_{B,t} + P_{A,t}\right)\right)\right| \tag{8}$$

Given the relatively small difference, doing so in variables' logarithmic form yields the approximate percentage difference between the closing price and the intrinsic value. The absolute value is taken since depending on whether the final trade is a buy or a sell order, this difference will be positive or negative, respectively. Since a given transaction only incurs half the bid-ask spread, the expression is multiplied by two. The effective spread measure is calculated daily here.

Bid-Ask Spread Measure II: Roll (1984) Measure

This measure is derived from Roll's (1984) model of an end-of-day security price P_t which is composed of the fundamental security value V_t and an effective spread S:

$$P_t = V_t + \frac{1}{2} S Q_t \tag{9}$$

where Q_t is a binary variable that indicates whether the final trade of the day is a buy order ($Q_t = 1$) or a sell order ($Q_t = -1$). It can then be shown that the first-order autocorrelation in the *differenced* prices $\Delta P_t = r_t$ equals

$$Cov(r_t, r_{t-1}) = \frac{1}{4} S^2 \tag{10}$$

See Roll (1984) for a detailed derivation. Then, a simple rearrangement of (10) yields a spread estimator in terms of the autocovariance of price differences:

$$S_t = 2\sqrt{-Cov(r_t, r_{t-1})} \equiv Roll_{orig,t} \tag{11}$$

Note that (11) is undefined (or rather, complex-valued) if $Cov(r_t, r_{t-1}) > 0$; thus, observations of positive autocovariance are disregarded by this measure. To address this case, Roll (1984) modifies (11) and defines an alternative spread estimator as

$$S_t = -2\sqrt{|Cov(r_t, r_{t-1})|} \equiv Roll_{abs,t} \tag{12}$$

The problem with this, however, is that in Roll's model, the bid-ask bounce can only induce negative first-order serial correlation. Conversely, positive serial correlation would imply a negative bid-ask spread, i.e., bid prices exceeding ask prices. However, positive first-order autocorrelation is frequently observed in practice – indeed, Reinganum (1990) finds that 41% of autocovariance in NASDAQ and NYSE security data he analyses is positive.

Thus, both versions of the Roll measure are calculated and reported in this paper: the "original" version $Roll_{orig}$ and the absolute value-based version $Roll_{abs}$. Both are calculated daily, using a rolling sample of seven observations for the calculation of $Cov(r_t, r_{t-1})$.

Whilst $Roll_{orig}$ is more easily explainable with Roll's model, $Roll_{abs}$ considers all observations and does not close its eyes to the fact that positive serial correlation is a robust phenomenon in security price data. This dilemma is a reminder of the fact that, as with all model-based measures, we must keep in mind the fact that the statistic rests on the underlying model's assumptions. As explained above, to address shortcomings of individual measures and to ensure robustness of results, multiple liquidity measures from two different conceptual groups are used here.

Volatility Measures: Econometric Details

Volatility Measure I: Realised Squared Returns

The *Realised Squared Returns* measure is usually the sum of squared returns over a given period. Like the variance from standard statistics, Realised Squared Returns in a given period need to be calculated from multiple observations within this period; here, it is formed with m=7 using daily returns:

$$RV_t = \sum_{i=1}^{m} r_i^2 \tag{13}$$

Note that this formula is not demeaned; this is because of the small size of the mean returns in daily data (Linton, 2019).

Volatility Measure II: Garman-Klass (1980)

To form the estimator, we need the following four prices on a daily basis: opening and closing prices $P_{O,t}$ and $P_{C,t}$, respectively, and high and low prices $P_{H,t}$ and $P_{L,t}$, respectively. Note that in the use of these four within-period prices, the Garman-Klass estimator differs from the other volatility estimators used here which apply price differences between periods. Moreover, unlike other intra-day volatility measures, this estimator uses only data that can be found "in the financial pages of the newspaper", as Garman and Klass (1980) put it, which are likely to be the main source of information many investors base their decisions on (or at least they were, at the time). It is then formed as:

$$\begin{aligned} GK_t = {} & \Big[\big[\ln(P_{H,t}) - \ln(P_{O,t})\big] - \big[\ln(P_{L,t}) - \ln(P_{O,t})\big]\Big]^2 \\ & -(2\ln(2) - 1) * \big(\ln(P_{C,t}) - \ln(P_{O,t})\big)^2 \end{aligned} \tag{14}$$

where time t typically refers to days. This is because although in principle, the Garman-Klass estimator can also be applied to more frequent data, this would require having the high and low price for each period considered, which due to data availability constraints can be difficult. Here too, GK_t was calculated daily.

Chen et al. (2006) point out the rule of thumb that if the security under consideration is traded less than 1,000 times per day, GK_t is biased. With the heavily traded LIBOR futures, this is not a problem, fortunately: the minimum daily volume in the GBP LIBOR sample is 1,069; USD LIBOR has a minimum daily volume of 6,461; mean trading volumes are much higher (see table 1).

Volatility Measure III: Schwert (1989) Measure

Similarly to the variance, volatility in a given period needs to be calculated from multiple observations within this period; thus, following Schwert's notation, if *within* a given period t of interest, we have N_t sub-period returns, the Schwert volatility estimator is formed as follows:

$$Schwert_t^2 = \frac{1}{N_t}\left(\sum_{i=1}^{N_t} r_{it}^2 + 2\sum_{i=1}^{N_t-1} r_{it}r_{i+1,t}\right) \tag{15}$$

Note that as for realised squared returns, this formula is not demeaned; this is because of the small size of the mean returns in daily data. Schwert's formula relies on intra-period data. Since the highest-frequency data we have is daily, we need to determine the frequency for which we calculate it. With daily data, a standard frequency would be monthly (Linton, 2019); however, to be able to find short-term effects of key events related to the LIBOR scandal, as well as these effects' duration, the estimator was calculated for biweekly data, i.e., for $N_t = 14$. As a robustness check for this decision, the Schwert estimator has also been calculated for monthly data: as may be expected, this yielded very similar results, in that the monthly Schwert estimator looks like a smoothed version of its biweekly counterpart.

Liquidity and Volatility Tests' Correlation Matrices

Tables A.1 to A.4 display the correlation coefficient between measures' time series across the whole sampling period: from 01/01/2008 to 17/12/2021 for GBP LIBOR and 01/01/2008 to 23/03/2022 for the USD LIBOR.

Table A.1 GBP LIBOR Liquidity Measures Correlation Matrix

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
Amihud	1				
Volat. Over Volume	0.5593	1			
Effective Spread	0.3156	0.2404	1		
Roll Measure original	0.4665	0.7903	0.2855	1	
Roll Measure alternative	0.5550	0.8332	0.3228	0.9603	1

Table A.2 USD LIBOR Liquidity Measures Correlation Matrix

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
Amihud	1				
Volat. Over Volume	0.6929	1			
Effective Spread	0.4601	0.4262	1		
Roll Measure original	0.3627	0.6017	0.3628	1	
Roll Measure alternative	0.5548	0.7581	0.4759	0.8890	1

Table A.3 GBP LIBOR Volatility Measures Correlation Matrix

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread
Amihud	1		
Volat. Over Volume	0.7505	1	
Roll Measure alternative	0.3200	0.4143	1

Table A.4 USD LIBOR Volatility Measures Correlation Matrix

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread
Amihud	1		
Volat. Over Volume	0.7551	1	
Roll Measure alternative	0.7440	0.5884	1

Appendix B. Details on Variables & Spot Rate Summary Statistics by Month of the Year

Table 1 displays the following variables:

- *Opening Price* P_O. The price of the reported day's first trade.
- *Closing Price* P_C. The price of the reported day's last trade.
- *End-of-Day Ask Price* P_A. Final ask price on a trading day.
- *End-of-Day Bid Price* P_B. Final bid price on a trading day.
- *Intra-Day High Price* P_H. Highest price at which contract traded on a trading day.
- *Intra-Day Low Price* P_L. Lowest price at which contract traded on a trading day.
- *Daily Trading Volume V.* Reported as the total number of a security's shares on the day the security had its most recent settlement or trade price. If an exchange does not send information on the volume, this is reported as 0. However, this is never the case for either rate considered here.

Table B.1 Spot Rate Summary Statistics by Month of the Year

LIBOR Rate	Month	Observations	Mean	Standard deviation	CV	Q25	Q50	Q75
GBP	Jan	296	1.101	1.384	1.257	0.520	0.601	0.922
	Feb	283	1.086	1.366	1.258	0.521	0.616	0.870
	Mar	303	1.012	1.323	1.307	0.520	0.591	0.843
	Apr	279	1.094	1.450	1.325	0.527	0.651	0.825
	May	281	1.006	1.370	1.362	0.506	0.609	0.823
	Jun	298	0.981	1.396	1.423	0.507	0.587	0.825
	Jul	312	0.967	1.400	1.448	0.509	0.632	0.825
	Aug	294	0.905	1.340	1.481	0.388	0.682	0.773
	Sep	301	0.926	1.425	1.539	0.379	0.586	0.767
	Oct	310	0.959	1.487	1.551	0.404	0.563	0.790
	Nov	299	0.822	1.018	1.238	0.523	0.558	0.792
	Dec	274	0.778	0.751	0.965	0.519	0.577	0.792
USD	Jan	301	1.061	1.094	1.031	0.254	0.572	1.722
	Feb	288	0.984	0.919	0.934	0.262	0.507	1.663
	Mar	312	0.946	0.859	0.908	0.274	0.553	1.269
	Apr	279	0.997	0.917	0.920	0.276	0.627	1.177
	May	280	0.926	0.893	0.964	0.274	0.467	1.186
	Jun	298	0.883	0.878	0.994	0.273	0.533	1.250
	Jul	298	0.885	0.892	1.008	0.265	0.460	1.307
	Aug	294	0.870	0.873	1.003	0.265	0.409	1.315
	Sep	287	0.867	0.944	1.089	0.252	0.336	1.321
	Oct	308	0.965	1.143	1.184	0.242	0.321	1.364
	Nov	285	0.836	0.833	0.996	0.238	0.334	1.419
	Dec	281	0.837	0.797	0.952	0.247	0.311	1.498

The mean is reported as the implied spot *rate*. Thus, to get the spot *price*, convert it according to equation (15). CV, Q25, Q50 and Q75 denote the coefficient of variation and the 25th, 50th, and 75th percentile, respectively.

Appendix C. Liquidity and Volatility Measures Graphs

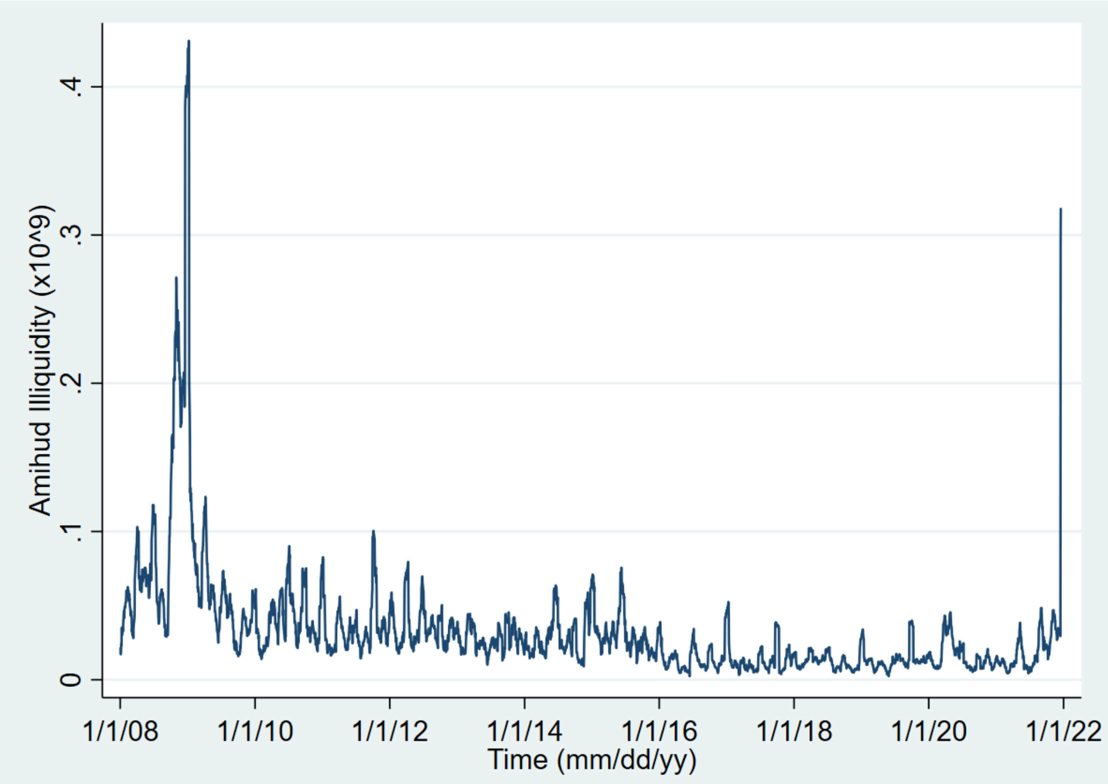

Figure C.1 GBP LIBOR Amihud with potentially spurious extreme spike on the final two days

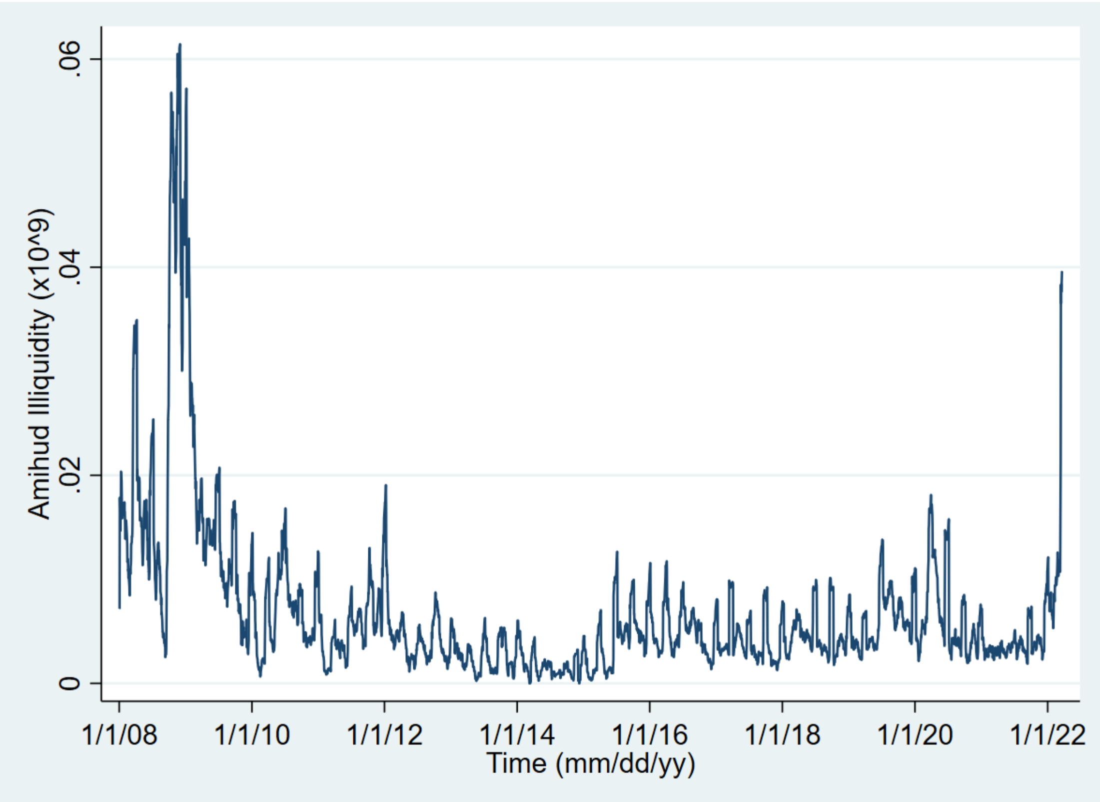

Figure C.2 USD LIBOR Amihud with potentially spurious extreme spike on the final eight days

Figures C.3 to C.16 display both the entire sample period beginning in 2008 and zoom in on the dates of the first three events of interest ±30 days. A red vertical bar in the middle of each close-up marks the event date. The time around the final event, the discontinuation, whilst it is highlighted by a red circle as all other events, is not displayed as a close-up for two reasons: first, unlike the first three events, the discontinuation was announced, so it is not perfectly clear when its effects would begin to show. Second, in the case of GBP LIBOR, it is in the nature of the event that there are no data 30 days after the event. Moreover, the discontinuation is discussed in detail in the formal statistical analysis.

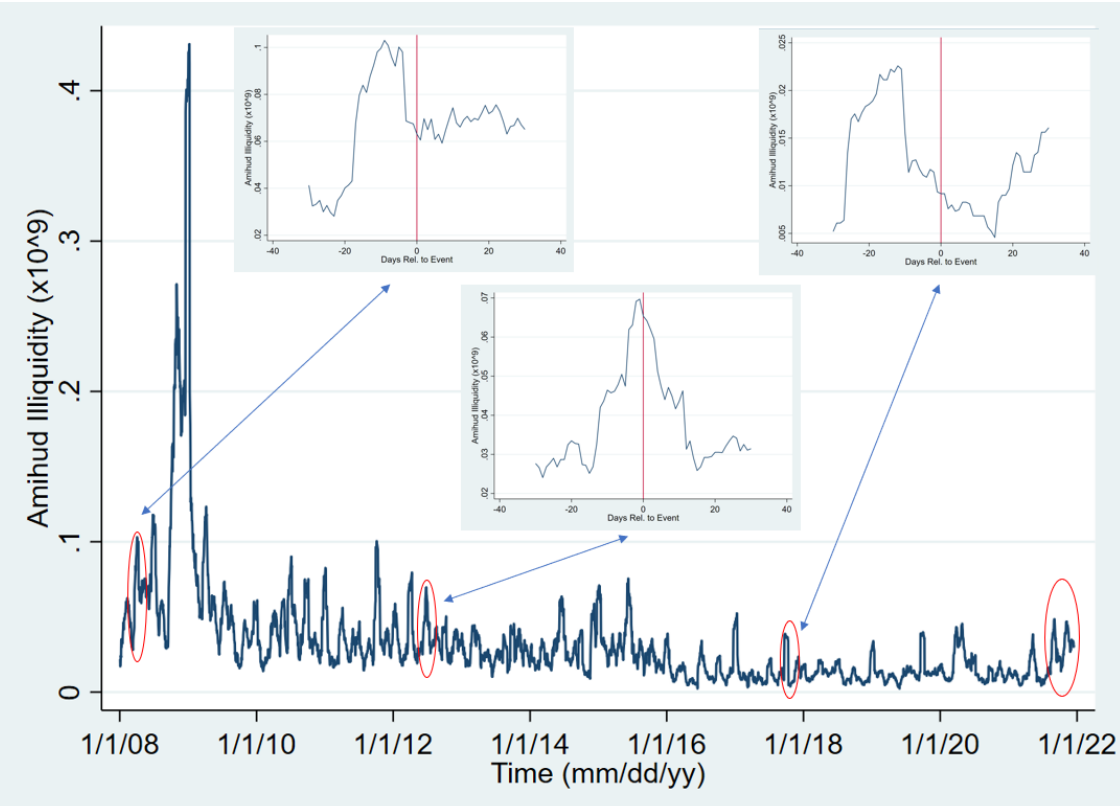

Figure C.3 Amihud Measure on GBP LIBOR. In this and the subsequent figures, the vertical red line in the close-up highlights the event date: first manipulation suspicions on 16/04/2008; Barclays' admission to manipulation on 27/06/2012; Andrew Bailey's first hint at a possible discontinuation of LIBOR on 27/07/2017; and the spike associated with LIBOR's (recommended) discontinuation on 31/12/2021.

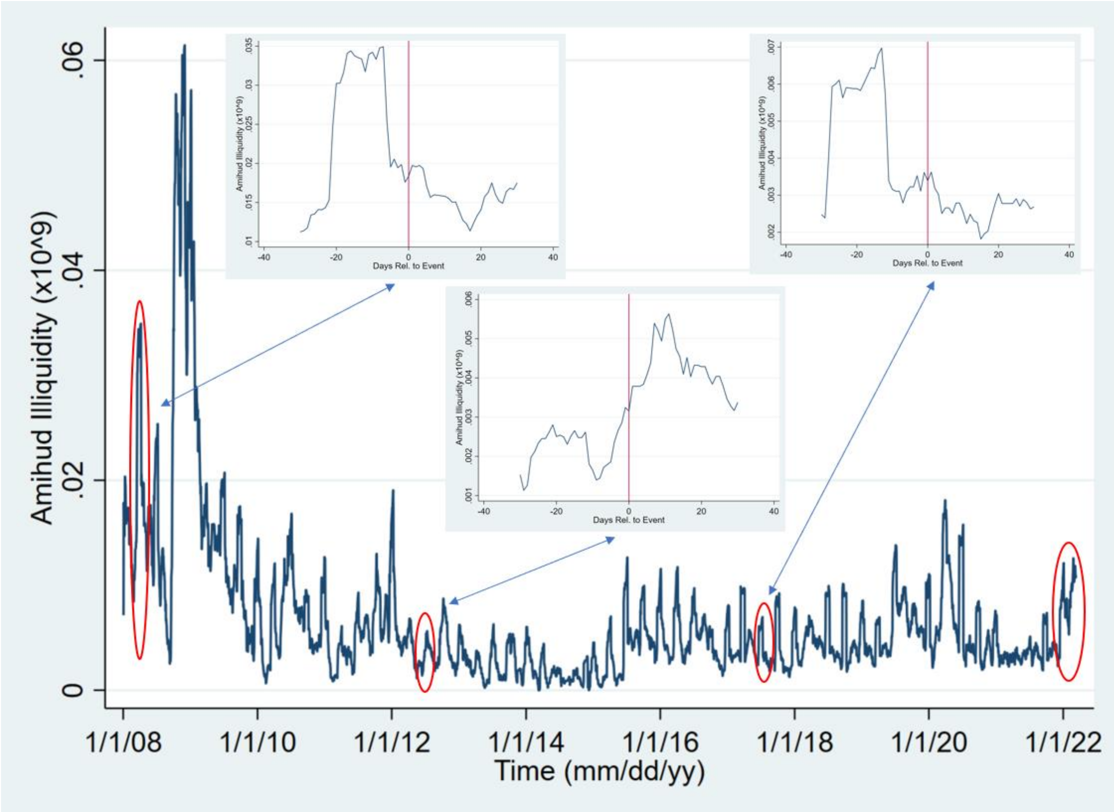

Figure C.4 Amihud Measure on USD LIBOR

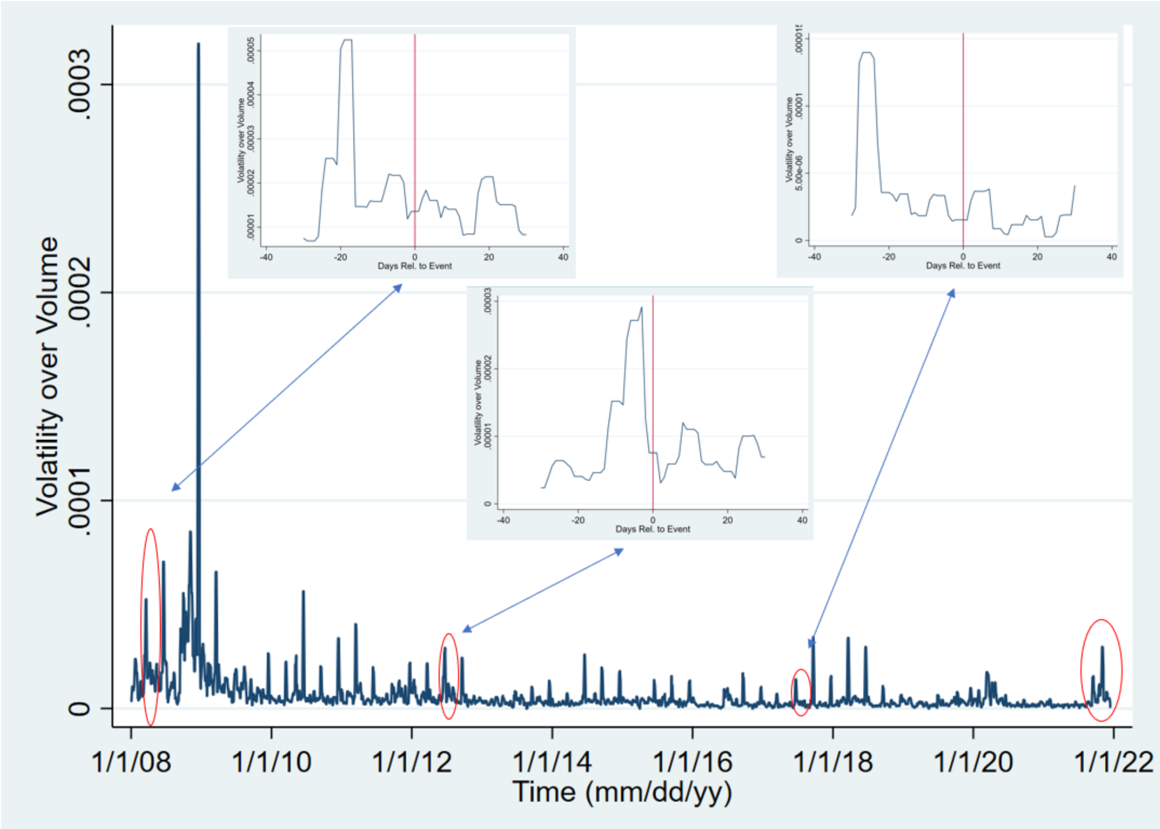

Figure C.5 Volatility over Volume on GBP LIBOR

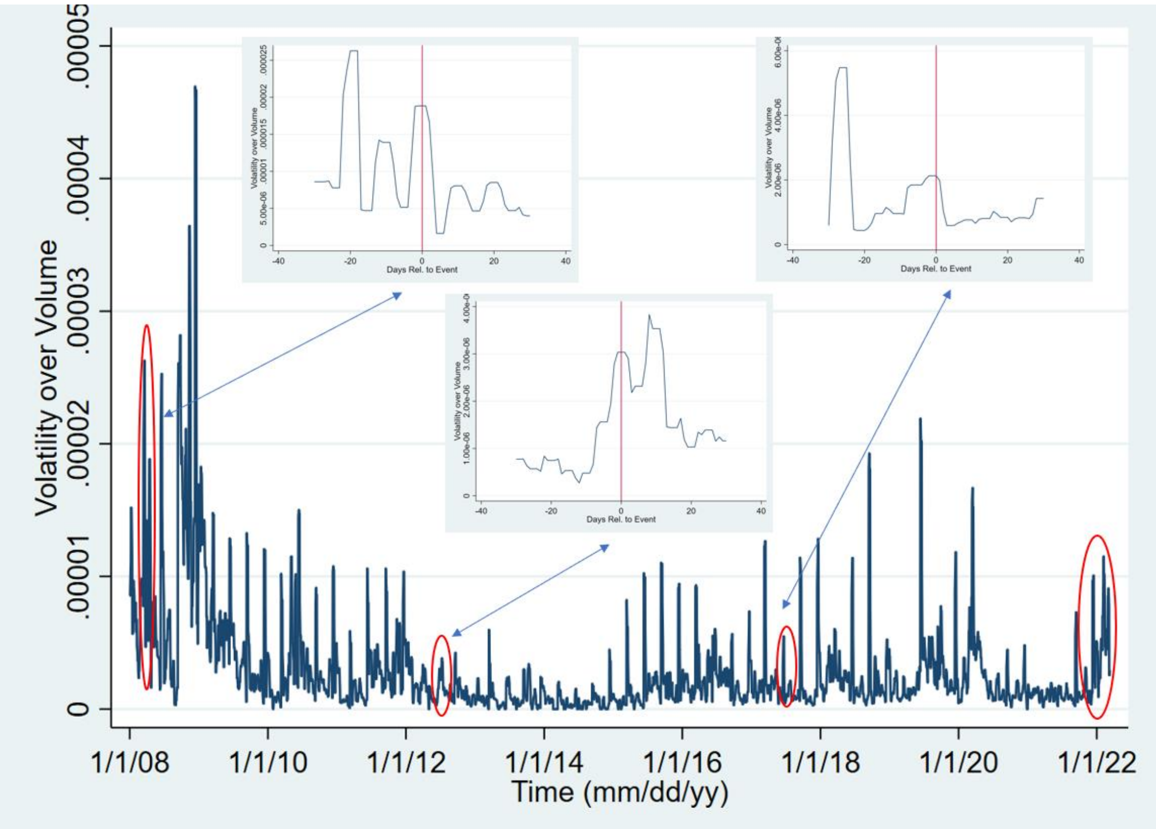

Figure C.6 Volatility over Volume on USD LIBOR

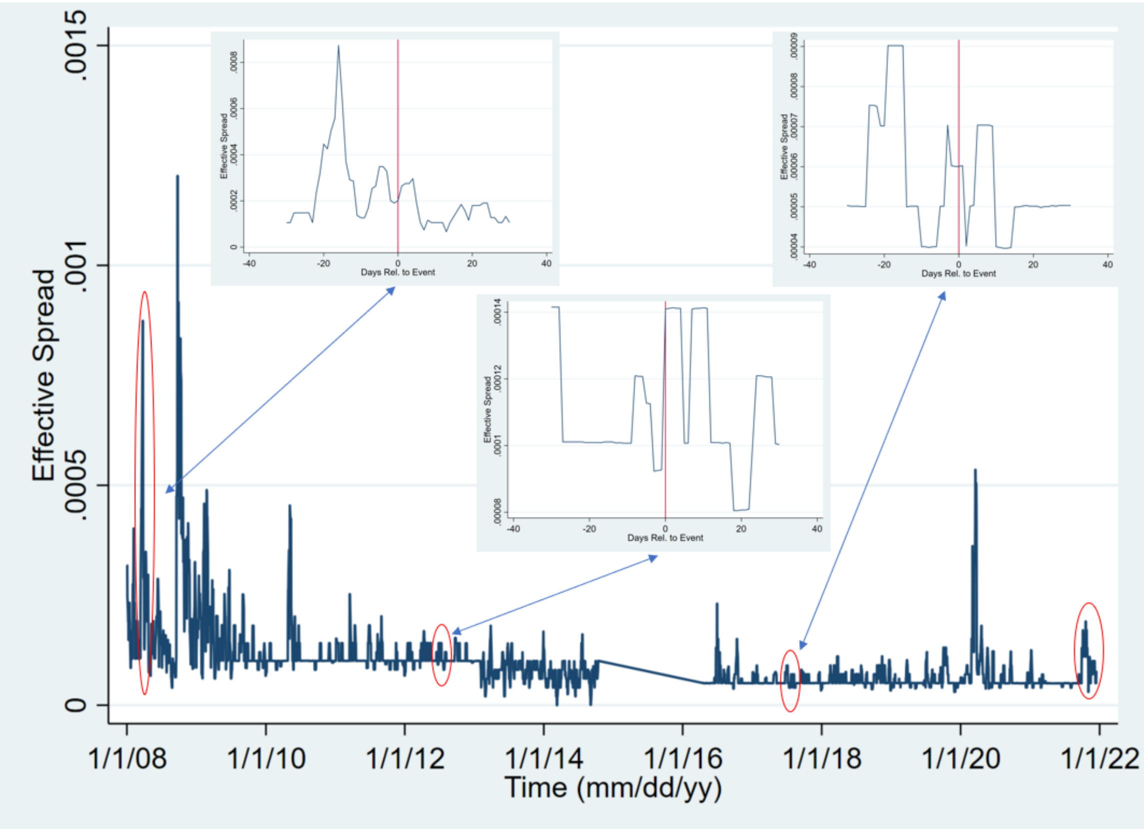

Figure C.7 Effective Spread Measure (7-Day Moving Average) on GBP LIBOR. The straight line between end of 2014 and mid-2016 is due to bid- and ask prices not being reported by Bloomberg in this period.

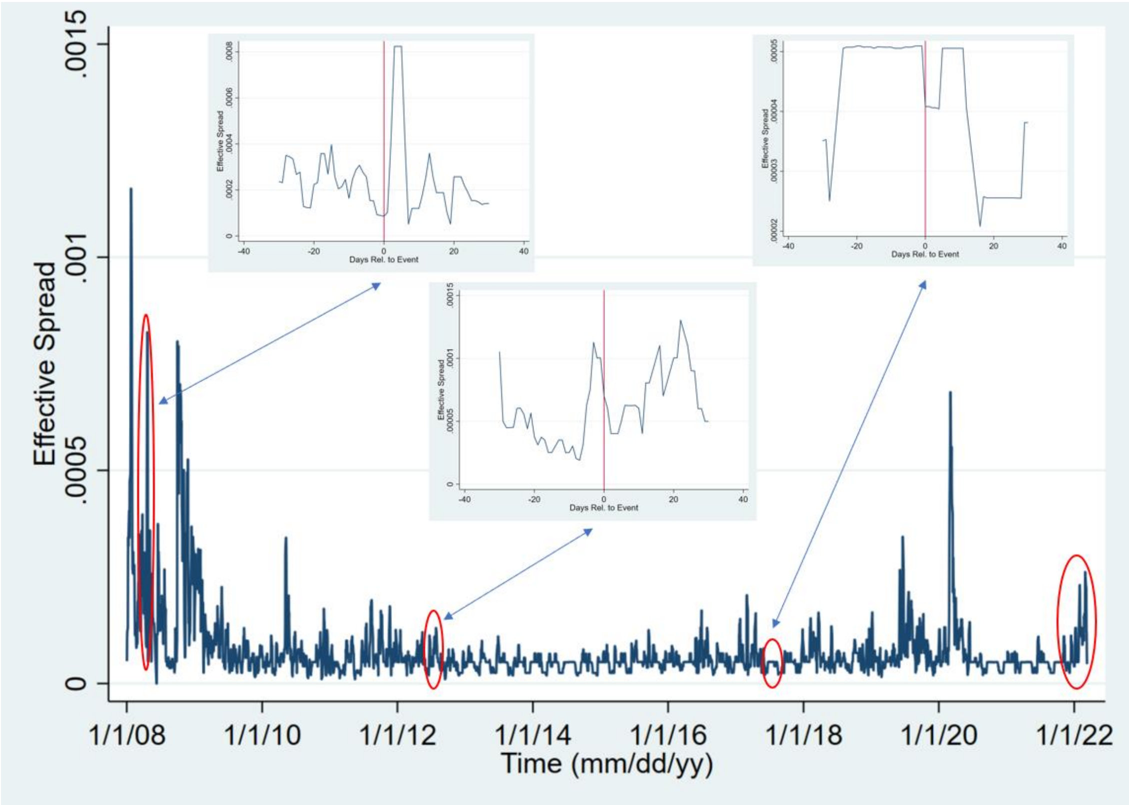

Figure C.8 Effective Spread Measure (7-Day Moving Average) on USD LIBOR

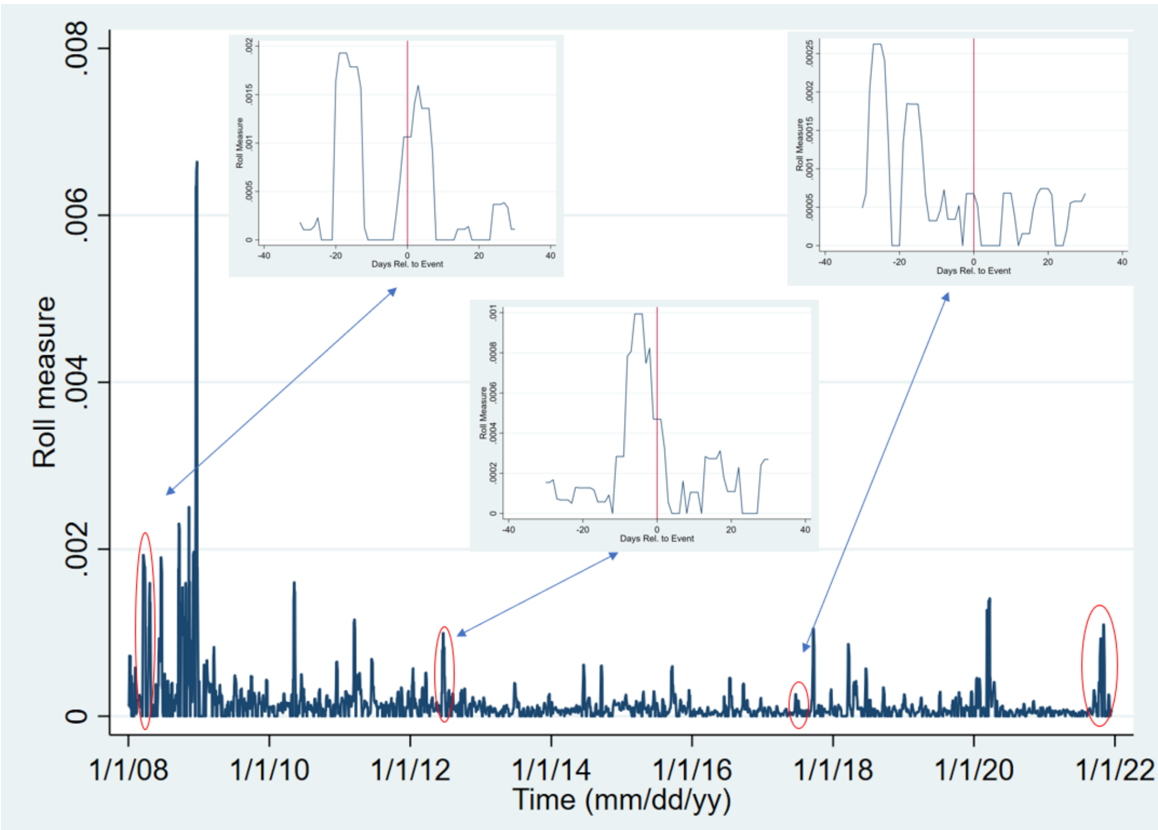

Figure C.9 Roll Measure on GBP LIBOR

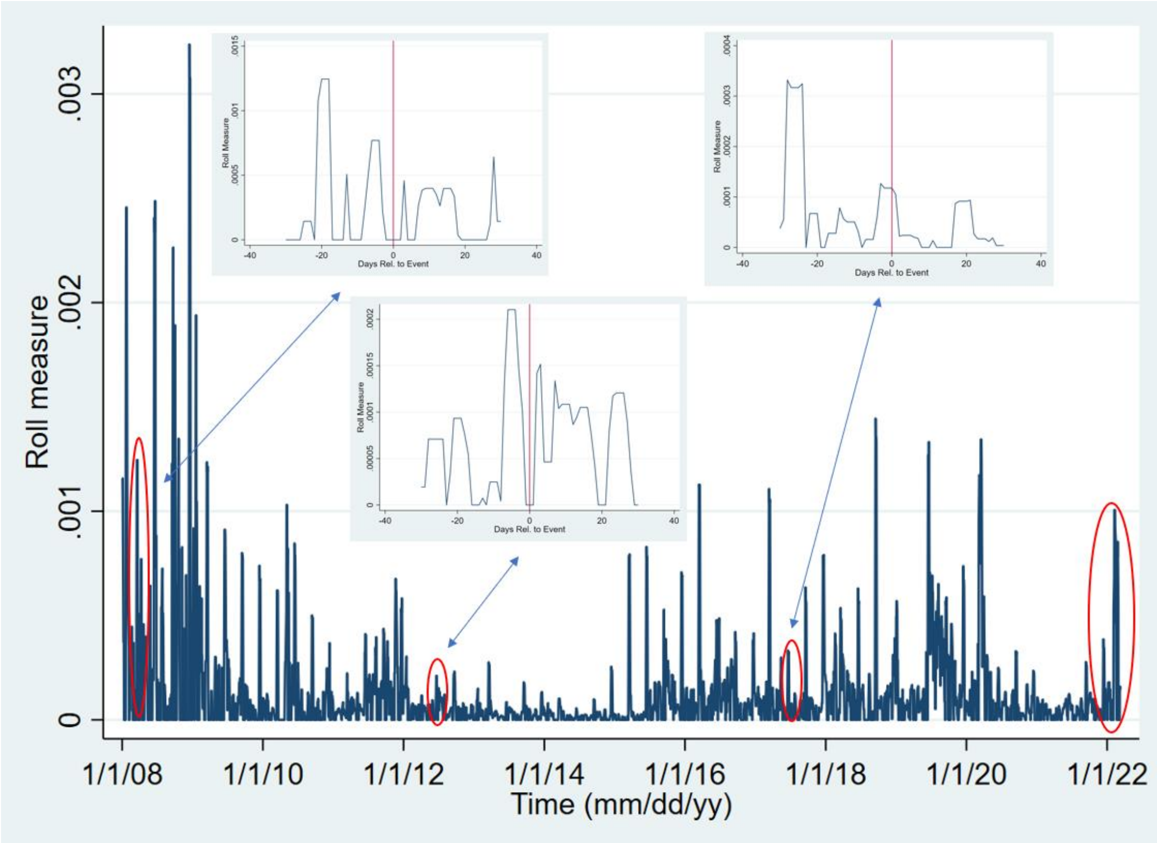

Figure C.10 Roll measure on USD LIBOR

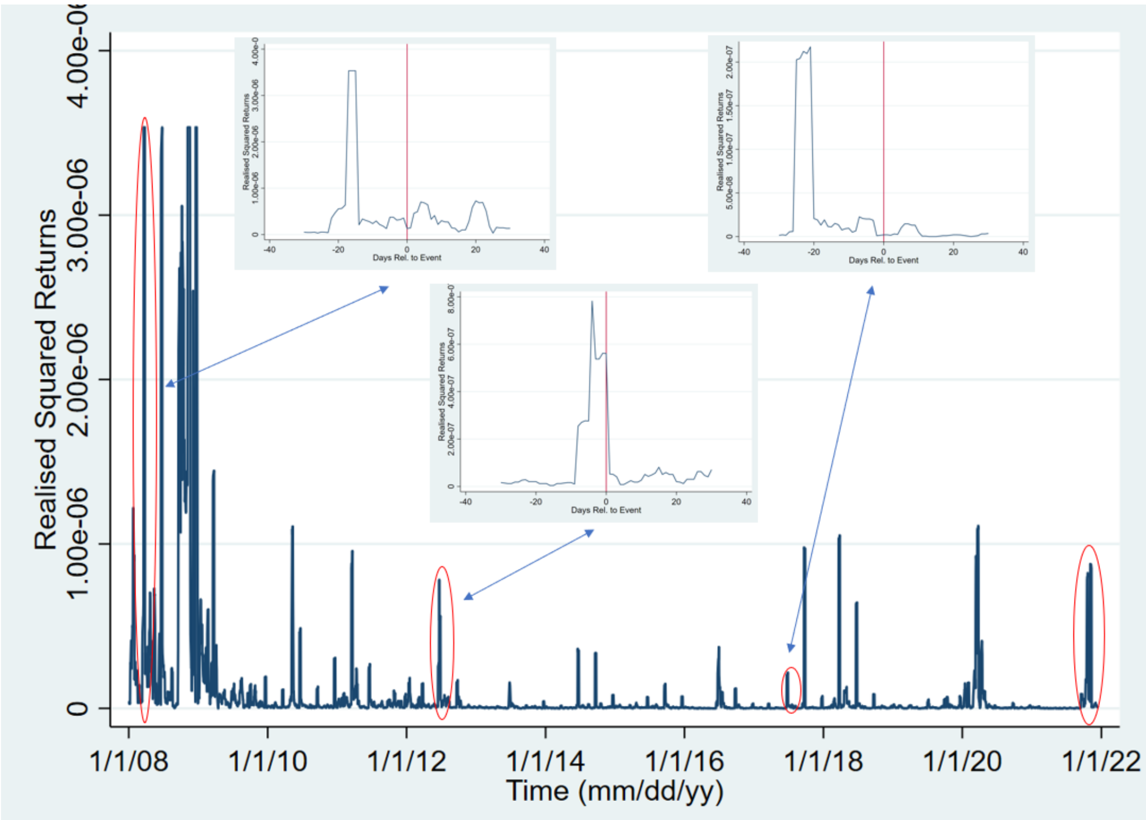

Figure C.11 Realised Squared Returns Measure on GBP LIBOR (winsorized at 0.5% and 99.5%)

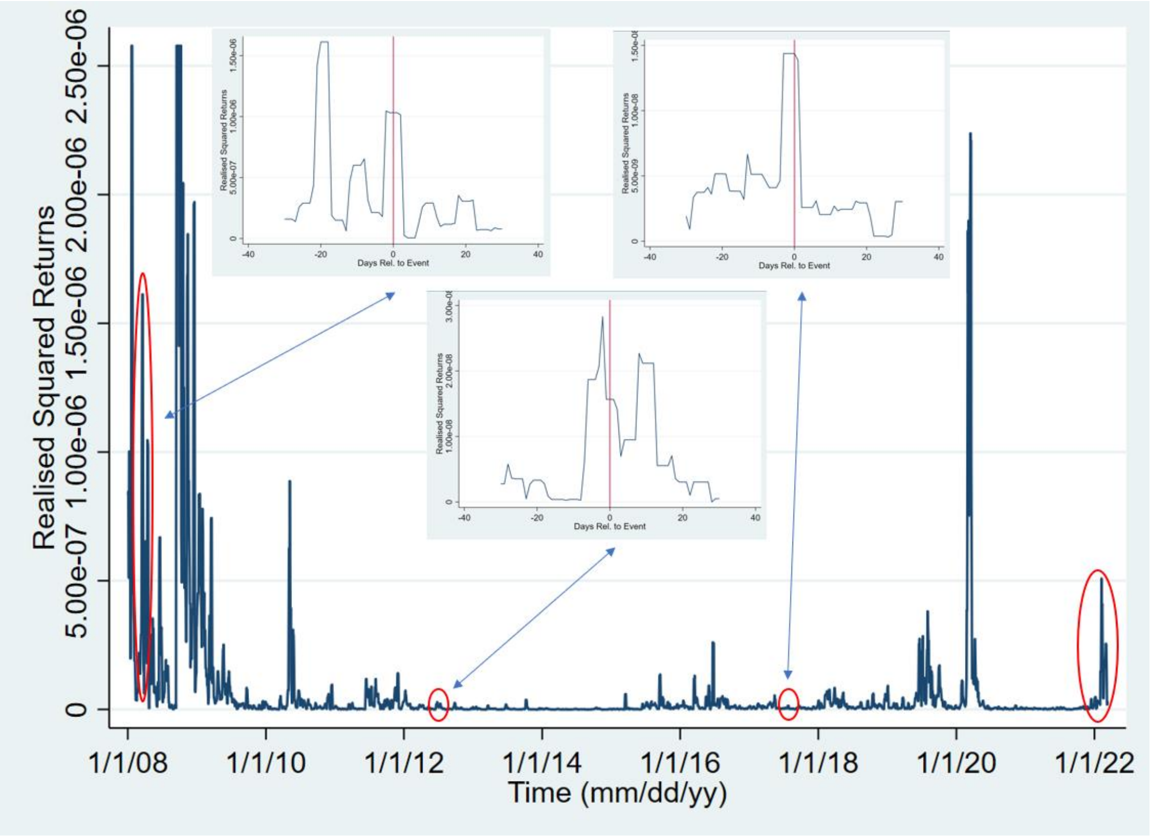

Figure C.12 Realised Squared Returns Measure on USD LIBOR (winsorized at 0.5% and 99.5%)

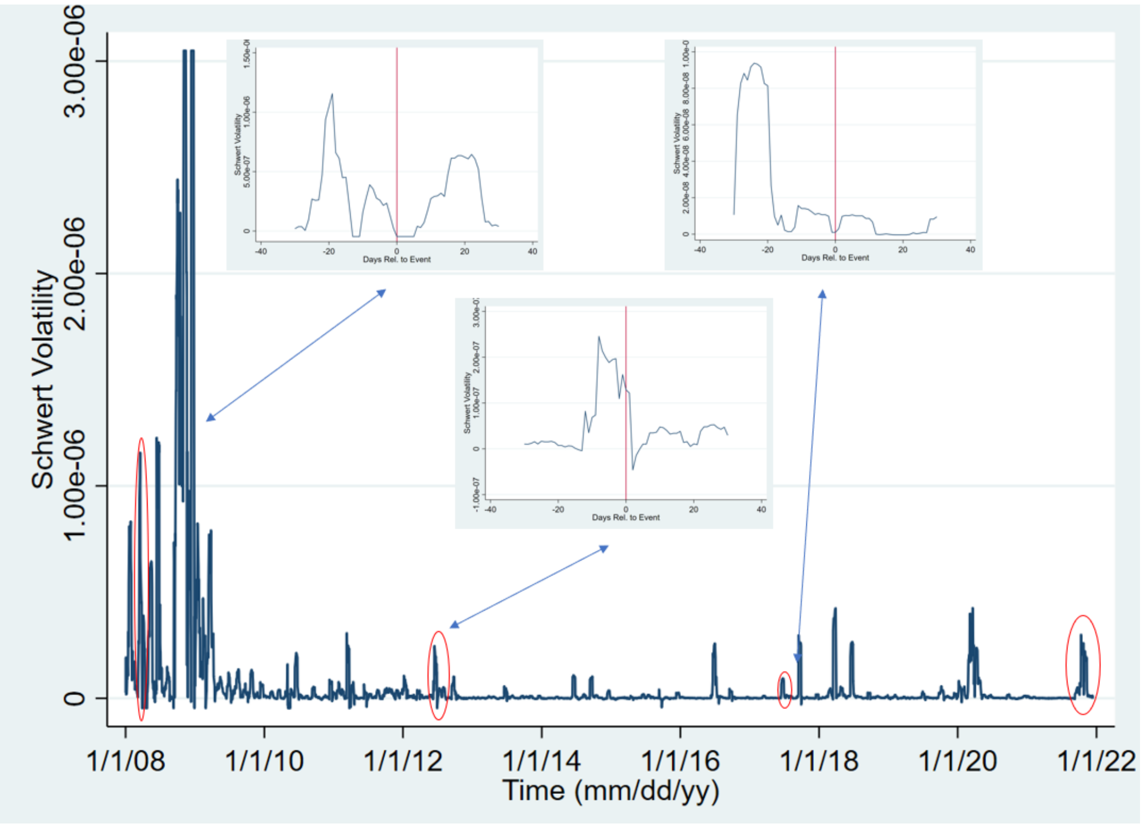

Figure C.13 Schwert Volatility Measure on GBP LIBOR (winsorized at 0.5% and 99.5%)

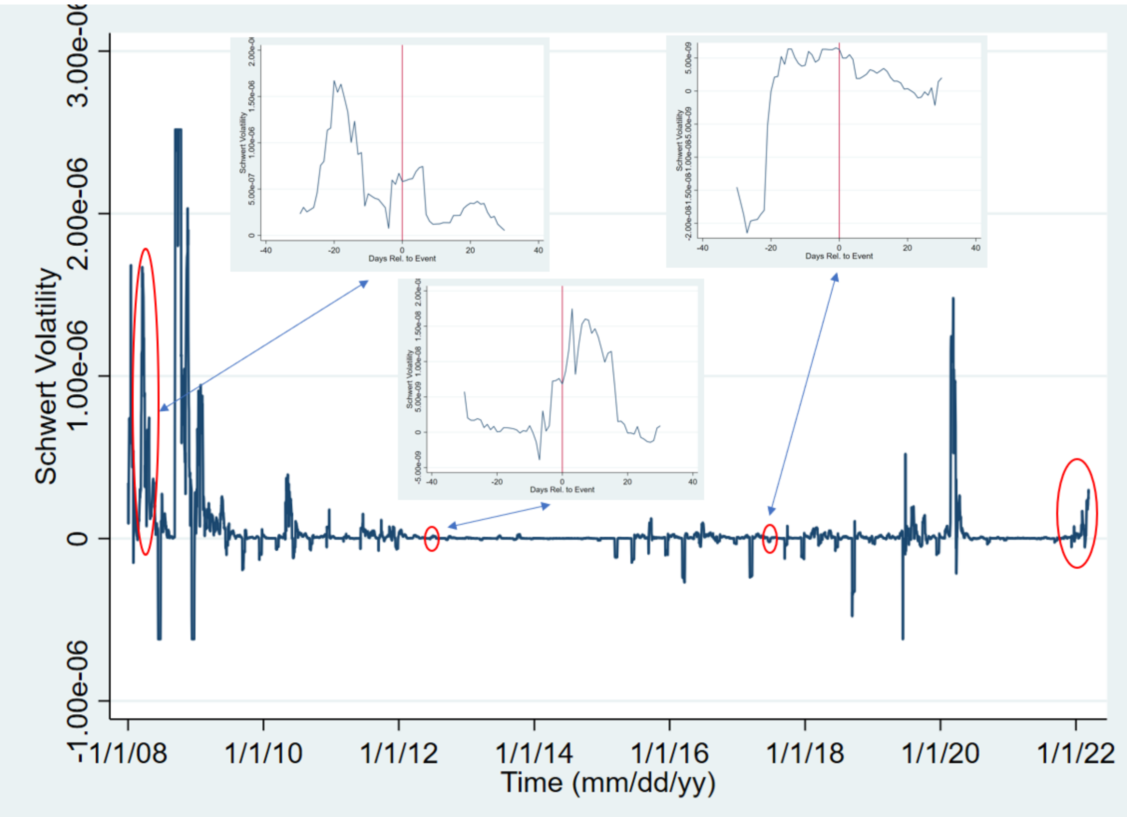

Figure C.14 Schwert Volatility Measure on USD LIBOR (winsorized at 0.5% and 99.5%)

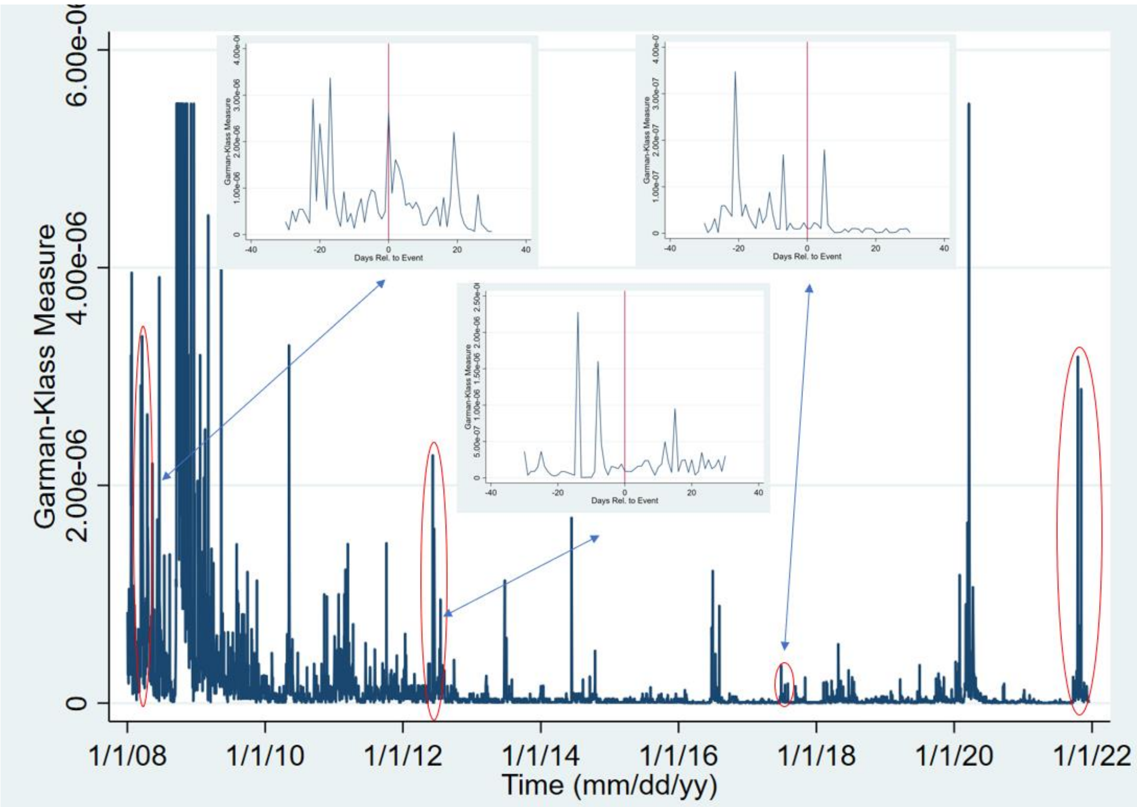

Figure C.15 Garman-Klass Measure on GBP LIBOR (winsorized at 0.5% and 99.5%)

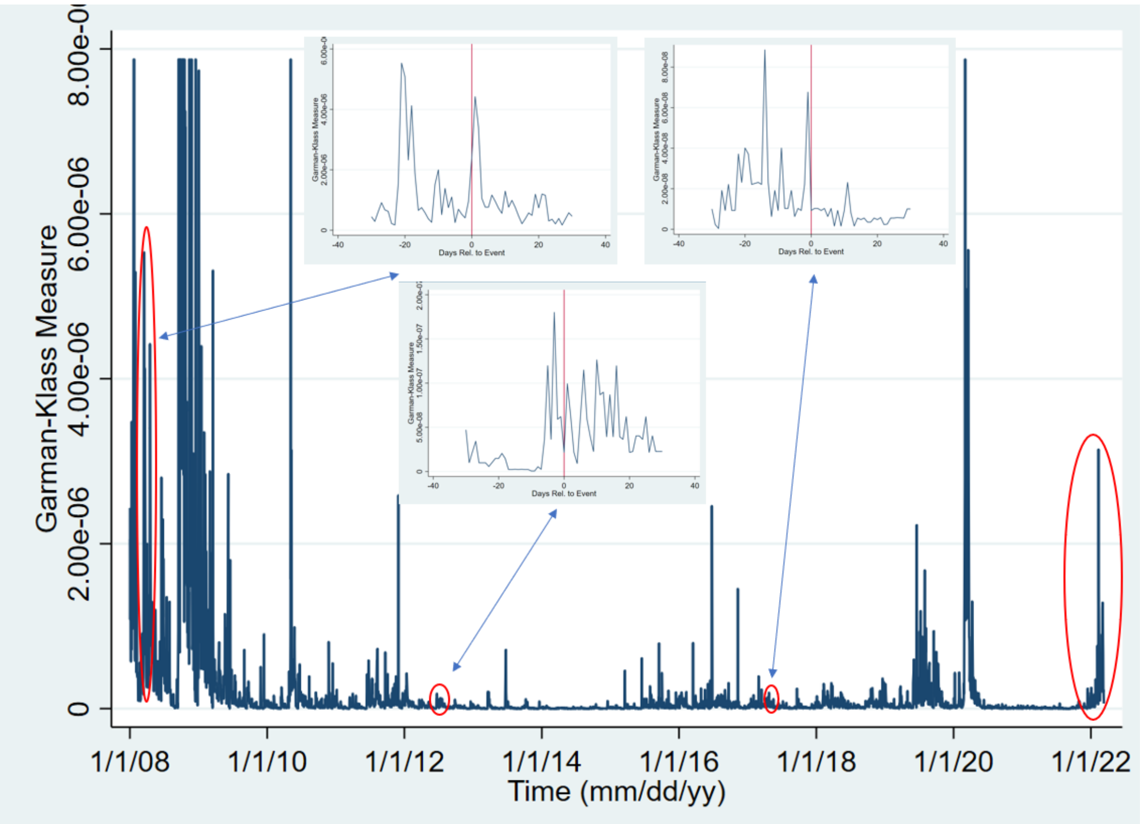

Figure C.16 Garman-Klass Measure on USD LIBOR (winsorized at 0.5% and 99.5%)

Appendix D. Robustness Tests

Table D.1 Regression (2) with 2nd best ARMA model: GBP LIBOR Liquidity

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.928***	1.020***	0.742***	0.863***	1.043***
	(0.0147)	(0.0160)	(0.0108)	(0.0183)	(0.0210)
post1/10/2021	0.172***	0.143***	0.0986***	0.206**	0.129***
	(0.0511)	(0.0398)	(0.0354)	(0.0933)	(0.0463)
Reg. Constant	-27.03***	-15.37***	-2.458***	-11.43***	-12.05***
	(0.0474)	(0.0484)	(0.0382)	(0.0571)	(0.0652)
Observations	3,502	3,503	3,132	2,805	3,461

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Second-best model in terms of BIC. For all tables of appendix D: *Measure* and *VIX_close* are still in logarithmic form. For space reasons, the monthly dummies and GARCH model coefficients are not reported here, but available upon request.

Table D.2 Regression (2) with 2nd best ARMA model: USD LIBOR Liquidity

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	1.004***	1.179***	0.387***	1.099***	1.076***
	(0.0117)	(0.0117)	(0.0143)	(0.0395)	(0.0267)
post1/12/2021	0.249***	0.727***	0.457***	0.241**	0.231**
	(0.0373)	(0.0369)	(0.0352)	(0.119)	(0.0992)
Reg. Constant	-8.205***	-4.665***	-1.858***	-5.064***	-5.720***
	(0.0324)	(0.0379)	(0.0445)	(0.123)	(0.0909)
Observations	3,567	3,570	3,565	2,517	3,454

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table D.3 Regression (2) with 2nd best ARMA model: GBP & USD LIBOR Volatility

VOLATILITY MEASURE	GBP Realised Squared Returns	GBP Schwert Measure	GBP Garman Klass Measure	USD Realised Squared Returns	USD Schwert Measure	USD Garman Klass Measure
VIX_close	2.347***	2.581***	2.432***	2.070***	2.629***	2.527***
	(0.0352)	(0.0453)	(0.0639)	(0.0310)	(0.0655)	(0.0623)
post1/10/2021†; post1/12/2021‡	0.304	2.018***	0.838***	0.831***	0.643***	0.607***
	(0.205)	(0.196)	(0.201)	(0.0729)	(0.111)	(0.141)
Reg. Constant	-25.40***	-26.64***	-24.71***	-10.99***	-12.70***	-11.24***
	(0.122)	(0.140)	(0.215)	(0.0960)	(0.220)	(0.202)
Observations	3,456	2,950	3,448	3,540	2,957	3,574

Standard error in parentheses, *** p<0.01, ** p<0.05, * p<0.1. †in the GBP regression; ‡ in the USD regression.

Table D.4 Regression (2) estimated by GLS: GBP LIBOR Liquidity

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.943***	1.152***	0.734***	1.016***	1.089***
	(0.0297)	(0.0410)	(0.0205)	(0.0461)	(0.0443)
post1/10/2021	0.248**	0.563***	0.150**	0.614***	0.549***
	(0.0965)	(0.133)	(0.0652)	(0.157)	(0.143)
Reg. Constant	-6.353***	-15.61***	-2.466***	-11.98***	-12.31***
	(0.0961)	(0.133)	(0.0674)	(0.146)	(0.143)
Observations	3,502	3,503	3,132	2,805	3,461

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table D.5 Regression (2) estimated by GLS: USD LIBOR Liquidity

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	1.241***	0.509***	0.772***	1.120***	1.187***
	(0.0302)	(0.0161)	(0.0234)	(0.0619)	(0.0500)
post1/12/2021	0.236***	0.0487	0.154**	0.362**	0.383***
	(0.0853)	(0.0455)	(0.0662)	(0.166)	(0.139)
Reg. Constant	-9.028***	-1.060***	-2.951***	-5.550***	-5.911***
	(0.0971)	(0.0518)	(0.0753)	(0.193)	(0.161)
Observations	3,567	3,574	3,565	2,517	3,454

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table D.6 Regression (2) estimated by GLS: GBP & USD LIBOR Volatility

VOLATILITY MEASURE	GBP Realised Squared Returns	GBP Schwert Measure	GBP Garman Klass Measure	USD Realised Squared Returns	USD Schwert Measure	USD Garman Klass Measure
VIX_close	2.359***	2.744***	2.396***	2.636***	2.933***	2.624***
	(0.0713)	(0.0888)	(0.0648)	(0.0732)	(0.0815)	(0.0702)
post1/10/2021†;	1.541***	1.865***	0.915***	0.763***	0.617***	0.508**
post1/12/2021‡	(0.231)	(0.272)	(0.209)	(0.206)	(0.222)	(0.198)
Reg. Constant	-25.36***	-26.64***	-24.61***	-12.97***	-13.93***	-11.74***
	(0.231)	(0.292)	(0.210)	(0.236)	(0.273)	(0.225)
Observations	3,456	2,950	3,448	3,540	2,957	3,574

Standard error in parentheses, *** p<0.01, ** p<0.05, * p<0.1. †in the GBP regression; ‡ in the USD regression.

Table D.7 Regression (2) with quarterly dummies: GBP LIBOR Liquidity Results

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.993***	0.787***	0.770***	0.892***	0.996***
	(0.0139)	(0.0173)	(0.0104)	(0.0204)	(0.0245)
post1/10/2021	0.148***	0.239***	-0.0127	0.268***	0.221***
	(0.0492)	(0.0417)	(0.0393)	(0.0886)	(0.0847)
Reg. Constant	-6.507***	-14.83***	-2.446***	-11.63***	-12.03***
	(0.0358)	(0.0513)	(0.0330)	(0.0625)	(0.0755)
Observations	3,502	3,503	3,132	2,805	3,461

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table D.8 Regression (2) with quarterly dummies: USD LIBOR Liquidity Results

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.810***	1.011***	0.330***	0.656***	1.002***
	(0.0127)	(0.0149)	(0.0112)	(0.0288)	(0.0274)
post1/12/2021	0.425***	0.496***	0.441***	0.260***	0.372***
	(0.0292)	(0.0434)	(0.0327)	(0.0998)	(0.102)
Reg. Constant	-7.727***	-4.986***	-1.629***	-4.452***	-5.606***
	(0.0350)	(0.0427)	(0.0311)	(0.0881)	(0.0869)
Observations	3,567	3,570	3,565	2,517	3,454

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table D.9 Regression (2) with quarterly dummies: GBP & USD LIBOR Volatility

VOLATILITY MEASURE	GBP Realised Squared Returns	GBP Schwert Measure	GBP Garman Klass Measure	USD Realised Squared Returns	USD Schwert Measure	USD Garman Klass Measure
VIX_close	2.329***	2.621***	2.391***	2.074***	2.499***	2.486***
	(0.0348)	(0.0462)	(0.0599)	(0.0288)	(0.0650)	(0.0634)
post1/10/2021†;	0.278	2.228***	1.043***	0.850***	0.499***	0.537***
post1/12/2021‡	(0.202)	(0.192)	(0.206)	(0.0675)	(0.107)	(0.135)
Reg. Constant	-25.31***	-26.49***	-24.36***	-11.04***	-12.32***	-10.80***
	(0.115)	(0.137)	(0.184)	(0.0842)	(0.199)	(0.194)
Observations	3,456	2,950	3,448	3,540	2,957	3,574

Standard error in parentheses, *** p<0.01, ** p<0.05, * p<0.1. † regressor used for the GBP sample; ‡ regressor used for the USD sample.

Table D.10 Regression (2): GBP LIBOR Liquidity Results - 10% winsorization

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.774***	0.757***	0.622***	0.905***	0.949***
	(0.0117)	(0.00377)	(0.0101)	(0.0163)	(0.0166)
post1/10/2021	0.292***	0.229***	0.114***	0.503***	0.235***
	(0.0496)	(0.0171)	(0.0318)	(0.116)	(0.0483)
Reg. Constant	-5.874***	-14.66***	-11.30***	-11.56***	-11.77***
	(0.0398)	(0.0141)	(0.0318)	(0.0508)	(0.0522)
Observations	3,502	3,503	3,132	2,805	3,461

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. In this and all subsequent tables, the winsorization was conducted only from above as the focus of this analysis is on the effect of the positive outliers – informally, the regularly occurring spikes in volatility and liquidity.

Table D.11 Regression (2): GBP LIBOR Liquidity Results - 20% winsorization

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.645***	1.029***	0.441***	0.619***	0.768***
	(0.00962)	(0.0137)	(0.00953)	(0.0149)	(0.0183)
post1/10/2021	0.396***	0.125***	0.231***	0.808***	0.817***
	(0.0403)	(0.0368)	(0.0223)	(0.0696)	(0.0761)
Reg. Constant	-5.712***	-15.38***	-10.88***	-10.79***	-11.32***
	(0.0326)	(0.0410)	(0.0285)	(0.0469)	(0.0560)
Observations	3,502	3,503	3,132	2,805	3,461

Std. errors in parentheses, *** p<0.01, ** p<0.05, *p<0.1.

Table D.12 Regression (2): USD LIBOR Liquidity Results - 10% winsorization

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.887***	1.077***	0.509***	1.024***	1.096***
	(0.0103)	(0.0115)	(0.0128)	(0.0273)	(0.0253)
post1/12/2021	0.293***	0.723***	0.410***	0.253**	0.327***
	(0.0349)	(0.0326)	(0.0308)	(0.104)	(0.0890)
Reg. Constant	-7.870***	-4.552***	-2.181***	-5.616***	-5.824***
	(0.0297)	(0.0323)	(0.0422)	(0.0843)	(0.0851)
Observations	3,567	3,570	3,565	2,517	3,454

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table D.13 Regression (2): USD LIBOR Liquidity Results - 20% winsorization

(IL)LIQUIDITY MEASURE	Amihud	Volat. Over Volume	Effective Spread	Roll Measure original	Roll Measure alternative
VIX_close	0.700***	0.814***	0.410***	0.888***	0.906***
	(0.00848)	(0.00714)	(0.0110)	(0.0234)	(0.0220)
post1/12/2021	0.388***	0.533***	0.260***	0.331***	0.452***
	(0.0286)	(0.0357)	(0.0300)	(0.0851)	(0.0739)
Reg. Constant	-7.373***	-3.891***	-1.869***	-5.233***	-5.336***
	(0.0244)	(0.0245)	(0.0335)	(0.0729)	(0.0737)
Observations	3,567	3,570	3,565	2,517	3,454

Std. errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table D.14 Regression (2): GBP & USD LIBOR Volatility - 10% winsorization

VOLATILITY MEASURE	GBP Realised Squared Returns	GBP Schwert Measure	GBP Garman Klass Measure	USD Realised Squared Returns	USD Schwert Measure	USD Garman Klass Measure
VIX_close	2.096***	2.451***	2.025***	2.059***	2.247***	2.157***
	(0.0336)	(0.0439)	(0.0495)	(0.0292)	(0.0553)	(0.0635)
post1/10/2021†;	0.335**	2.071***	1.121***	0.861***	0.618***	0.519***
post1/12/2021‡	(0.165)	(0.213)	(0.161)	(0.0628)	(0.0937)	(0.163)
Reg. Constant	-24.61***	-25.55***	-23.72***	-11.00***	-11.65***	-10.31***
	(0.0958)	(0.135)	(0.175)	(0.0909)	(0.188)	(0.201)
Observations	3,456	2,950	3,448	3,540	3,574	2,957

Std. error in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

† regressor used for the GBP sample; ‡ regressor used for the USD sample.

Table D.15 Regression (2): GBP & USD LIBOR Volatility - 20% winsorization

VOLATILITY MEASURE	GBP Realised Squared Returns	GBP Schwert Measure	GBP Garman Klass Measure	USD Realised Squared Returns	USD Schwert Measure	USD Garman Klass Measure
VIX_close	1.593***	1.894***	1.708***	1.583***	1.664***	1.526***
	(0.0286)	(0.0337)	(0.0551)	(0.0122)	(0.0481)	(0.0536)
post1/10/2021†;	0.730***	2.011***	0.987***	0.892***	0.676***	0.467***
post1/12/2021‡	(0.128)	(0.113)	(0.159)	(0.0513)	(0.0853)	(0.156)
Reg. Constant	-23.32***	-24.74***	-22.70***	-9.679***	-10.11***	-8.558***
	(0.0821)	(0.100)	(0.177)	(0.0456)	(0.164)	(0.171)
Observations	3,456	2,950	3,448	3,540	3,574	2,957

Std. error in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

† regressor used for the GBP sample; ‡ regressor used for the USD sample.