Report on "Essays on interbank interest rates" by Kamil Kovář

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Summary

The dissertation analyses under which circumstances and to what extent the level of interbank rates in the euro area depends on the quantity of excess liquidity (EL), in particular the amount generated by quantitative easing (QE). The first two chapters look at aggregate series from the euro area and propose a semi-structural model for the evolution of interbank rates that offers better in- and out-of-sample fit than alternative models. The third and fourth chapters, though interesting per se, are somewhat unrelated to the title of the dissertation and my area of expertise, so my comments will focus on the first two.

The topic of the dissertation is of high policy relevance and the chapters are considerably articulated, exploring extensively and expertly various facets of the issue. One can easily see that a great amount of work has gone into the dissertation. In what follows I report some comments that I would find important to address ahead of the defense, and others that could prove useful to improve the papers. I then conclude with an overall assessment.

Key comments to be addressed for the defense

1. The author reports that the link between the EL created via QE and interbank rates is relatively unexplored, which would grant the need to label this channel as the 'forgotten channel of quantitative easing'. While indeed more academic papers would be welcome on this topic, the relation between EL created via QE and interbank rates is extensively acknowledged and explored in central banking circles. It is actually an integral part of the way in which the policy instrument (asset purchases) is transmitted to the real economy. There is plenty of documentation accessible to the public illustrating this channel, including most recently in the context of the Strategy Review (Altavilla et al. [2021], Table 1). For a more in-depth discussion of monetary policy in the euro area, please refer to Rostagno et al. [2021]. Hence, I would suggest to modify the way in which references to this channel of transmission are presented, without talking about a new or forgotten channel of monetary policy transmission, let alone of a side effect, and to focus instead on the quantitative assessment.

2. When referring to asset purchase programmes by the ECB, it would be better to stick to the aims stated in ECB's official press releases and documentation. The "primary objective" of the QE programme announced on 22 January 2015, e.g., was not to simply impact bond yields (as stated for instance at page 11 of the dissertation), but rather to address the risks of a too prolonged period of low inflation for the fulfilment of the price stability mandate by making access to finance cheaper for firms and households.

Main comments to improve the paper

1. The point that both papers fall a bit short to make, with an eye on potential future publication of the two chapters, is that there is a channel of impact of the EL created by asset purchases above and beyond the direct impact of quantitative easing (QE) on the yield curve. The monetary policy instrument is asset purchases, not really excess liquidity, and the transmission mechanism relies exactly on the ability of such purchases to impact, among others, money market rates. When the author characterises the dependence between interbank rates and EL, be it alone or as instrumented by asset purchases, it is difficult to understand what is the additional kick that asset purchases give to interbank rates via the sole creation of EL rather than more generally through the multifaceted composition of their transmission channels.

2. The first chapter is somewhat in between trying to offer a general framework for the behaviour of interbank rates and a quantitative evaluation of a specific channel. Yet, neither of the two are presented on the basis of an actual, micro-founded theory of how interbank rates are determined. The paper would then benefit from a considerable streamlining and refocus on just a few key take-away messages, perhaps concentrated in the quantitative aspects. I would also opt between the linear and non-linear model as the benchmark, and present the other as part of the robustness checks. This is all the more appropriate given that, if expression (1.9) in section 1.4.1 is a "very good fit" as reported in page 36, then expression (1.10) is intrinsically mixing the impact of EL with the level of the deposit facility rate (the difference between IIR and DR is not independent of the level of DR).

3. The case for the exercises in the second chapter to be useful for analysts is predicated on the basis of the higher accuracy of forecasts that are conditional on excess liquidity projections. Yet, the difficulty for (especially private) analysts to forecast money market rates is to have in real time reliable out-of-sample conditioning variables, be them policy rates or excess liquidity projections. A model that predicts quite precisely a wrong pattern of interbank rates based on a wrong assumption on the future evolution of excess liquidity might fare worse in terms of forecast accuracy than a

potentially simpler model that relies less on potentially wrong information. This could be more clearly stated in the paper, and some considerations added as to the uncertainty surrounding the projections in real time. The author could actually also illustrate this quantitatively, showing how large the bias or uncertainty in the assumptions on the future evolution of excess liquidity could be before making the proposed model less accurate than the other models.

4. In both chapters policy rates or excess liquidity or asset purchases are treated as exogenous policy instruments that the central bank activates discretionally. Yet, policy rates and the dimension of the central bank's balance sheet are reactions to fundamental threats to the primary objective of price stability: the exogenous determinants are the actual, structural shocks that exert downward or upward pressures on inflation rates, to which the central bank reacts. This could be acknowledged and discussed in one of the chapters, and perhaps even expanded upon using only policy surprises on the yield curve as instruments (as in Altavilla et al. [2019]) to determine the part of the policy decisions that is not already priced in by markets based on fundamentals. The use of policy surprises would actually offer a level playing field for decisions on policy rates and asset purchases to affect money market rates.

5. Labelling risk and term premia of interest rates as "disequilibrium" components risks to trigger unnecessarily biased and heated reactions from readers. I would advise not to add considerations on whether these premia are the result of equilibrium vs. disequilibrium dynamics, and to remain more factual on what they actually measure, that is, compensations of risk and term characteristics of an asset.

6. I am somewhat confused by the discussion in Subsection 1.3.3. The definition of "effect of the QE (EQE)" appears to me tautological, as it reads a bit like as "if we suppose that there are no biases in other coefficients, then the only coefficient whose bias we should be concerned about is our coefficient of interest". For instance, there is no reason to believe that the other β's in the definition should be the same between the scenario with QE and the scenario without QE. I would clarify or eliminate this consideration.

7. Appendix 1.C is not well suited to "focus on periods in which expectations do not affect the IIRs": expectations on monetary policy decisions shape money market rates in the lead up to monetary policy decisions just as much if not more than the decision itself, as market participants position themselves with 'bets' on what the monetary authority will eventually announce before the actually announcement; paradoxically, the announcements especially in the short end of the yield curve tend to reflect the opposite sign to what one might expect, as in the moment that the decision is announced some bets are priced out and that is what we observed as the reaction of market rates

on that date, while movements in market rates before the announcements are at times more in line with the actual policy decision then what is measured on the very same day of the decision.

8. The RHS of Figure 1.13 of section 1.4.2 is possibly one of the most interesting charts of the dissertation. The author should elaborate more on the interpretation of that threshold, especially given that it is much smaller than what one might gather from a quick visual inspection of Figure 1.7.
9. It would be good to have uncertainty bands in Figures 1.17 and 1.18 of section 1.5.2.
10. The comparisons of forecast accuracy throughout the second chapter would benefit from considerations on the statistical significance of differences in forecast accuracy, for instance via Diebold-Mariano tests.
11. The use of the label "unconditional" to refer to forecasts that are simply based on a different set of values for the conditioning variables (not the actual realised values but some projected values) is a bit misleading. Similarly, the comparison between purely unconditional forecasts like those for models that do not rely on conditioning information and conditional forecasts based on univariate, exogenous projections on conditioning variables is a bit improper. Perhaps the author could reformulate the "structural" models with a law of motion for conditioning variable also for the case of the conditional forecasts, inverting the equation to extract the set of forecast error necessary to yield the actual sequence of EL.
12. In general, I found the list of references on the short side, especially when it comes to assessing effectiveness and modelling of monetary policy measures. The author could draw from the list of references in Altavilla et al. [2021] for what concerns QE programme(s), NIRP and TLTROs.

Minor comments to improve the paper

1. It would be good to have the number of observations in every table that reports regression results.
2. I am not sure that the term "analogical", which is used several times across the dissertation, conveys the meaning that the author seems to have in mind, that is, something like "similar".
3. Typo in Table 1.3 of section 1.4.1: the coefficient β_1 should refer to ER_{t-1}, not DR_t.
4. The reference to a Google Search in Footnote 2 in chapter 2 is a bit odd, at least the indication of when the search was realised would help.
5. The first column (first forecast horizon) in Tables 2.2, 2.3 and 2.4 is missing for the bottom panel with the overall summary statistics. It is not clear why from either the notes to the table or the main text.

Assessment

Based on the comments above, I would conclude that the dissertation satisfies formal and content requirements for a PhD in Economics. Hence, I recommend the dissertation for a defense, especially if the key comments mentioned above are addressed.

References

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Massimo Rostagno, Carlo Altavilla, Giacomo Carboni, Wolfgang Lemke, Roberto Motto, Arthur Saint Guilhem, and Jonathan Yiangou. *Monetary Policy in Times of Crisis: A Tale of Two Decades of the European Central Bank*. Oxford University Press, 2021.