Essays on interbank interest rates

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The thesis consists of four chapters. The first two chapters deal with modelling and forecasting of interbank interest rates in the euro zone. The third chapter describes Kamil's Eviews add-in, which is employed to carry out exercises presented in the second chapter. Finally, the fourth chapter, which is coauthored by other two authors, covers a distinct topic – experimental evidence within the field of rational inattention.

In my report I will focus mainly on the first two chapters, which seem to constitute the core of the dissertation. Regarding the third chapter, I do not know what criteria use to assess a documentation of program code. Therefore, I do not comment the third chapter at all. Actually, I am not sure whether this chapter should be part of the dissertation – it is quite usual that dissertations are based on authors' own codes/toolboxes and documentation of such codes is not included as part of dissertations. Finally, I will provide only a few comments related to the fourth chapter. Those comments, however, should be discounted a bit because the chapter is outside of my area of expertise.

Overall, the author demonstrated ability to identify interesting economic questions either of an applied nature relevant to business and central banks (first two chapters) or from the academic perspective (chapter 4). He acquired quantitative skills required to effectively address the posed research questions.

On the other hand, I have several reservations. First, the dissertation is not written concisely. The exposition contains a lot of unnecessary digressions from the main massage and it takes time before it gets to the point (and in consequence the dissertation is too long relative to the contribution it provides). I provide several examples of such digressions related to Chapter 1 below. It looks like the dissertation papers have never gone through a working paper stage, which would surely streamline the text. Second, the dissertation seems to be finished in a hurry[1] and it would benefit from a careful language check. Third, I have also several methodological concerns that are discussed in detail below.

In my view, the presented reservations should be somehow addressed by the author. At least, I strongly suggest the author to comment my concerns in detail during the defense, especially to the points 1.3, 1.4, 2.1, and 2.2, and to discuss consequences of his responses for the contribution of the dissertation. *Then, I can confirm that the thesis satisfies formal and content requirements for a PhD thesis in economics and I recommend the dissertation for a defense.*

[1] For example, the abbreviation IIR is explained after several use of such abbreviation (p. 10), the structure of the Chapter 1 described in the last paragraph of the introduction (p. 13) refers to different numbering than actually used. There are missing units in Table 1.6, sources of data reported in figures in Section 1.5 are also missing. Figure 1.1 ends by 2016 although the period of interest is to 2019 etc.

Chapter 1: Negative policy rates and interbank interest rates: The forgotten channel of Quantitative Easing

Chapter 1 examines a very intuitive idea that when there is a plenty of liquidity in the banking system, the interbank overnight rate is close to ECB's deposit rate i.e. banks lend each other for the rate which is close (from above) to the rate offered by the ECB for banks' deposits. In such case, the ECB's main refinancing rate loses its anchoring nature – nobody needs to borrow from the central bank. The central bank is not a liquidity provider anymore; it becomes a liquidity absorber.

In addition, the more the liquidity in the banking system (excess reserves), the lower the spread between the interbank rate and deposit rate – this is the main hypothesis of Chapter 1. Such hypothesis is not surprising: the higher the supply of liquidity, the lower the price of liquidity on the interbank market. Put it differently, when there is plenty of liquidity, the spread between interbank rate and deposit rate, which represents the risk premium (the Eonia rate represents the price of unsecured overnight lending between banks), is lower simply because there is lower probability of a negative liquidity shock.

Focusing on the ECB, the chapter aims to model the abovementioned economic intuition using a very simple framework. Moreover, it estimates the effect of ECB's QE on short-term interbank rates by the means of counterfactual analysis.

Comments:

1.1) As I mentioned above, here I provide several examples of what I mean by my characterization that the thesis is not written concisely. Several additional examples could be found also in other chapters, but I do not explicitly state them in the report:

- When introducing the model (Section 1.3), the author starts with a simple two-regimes model. Then he argues that during the period of interest (i.e. ECB's QE), the model collapses into single-regime model. And finally, he states that he uses ARDL approach (together with FMOLS), which means yet another model specification. Why not to go directly to single-regime ARDL specification and save a few pages of text?
- When discussing the estimation procedure (Section 1.4), the author starts with the OLS and TSLS. Then he argues that the variables included in the model form a cointegration relationship and suggests the use of appropriate approaches by Pesaran and Shin (1998) and Phillips and Hansen (1990). Why not to get directly to the estimation approaches, which deal with cointegration? I do not see the point to present estimation results from approaches, which do not make econometric sense. If OLS and TSLS estimates are to remain in the paper, it should be explicitly and very clearly argued why those approaches are employed and discussed (robustness considerations are not enough to justify the use of OLS for cointegrated variables).
- In the Subsection 1.3.3, the author argues why he focuses on QE period (it is because of a) possible endogeneity of excess reserves and b) absence of policy rates in the model for the Euribor). However, then he presents several pages with results related to the whole sample (including the period immediately after September 2008 when the causality was indeed reversed and excess reserves were driven by ECB's policy measures to deal with heightened interbank market rates due to elevated risk premia). Again, I do not see the purpose of such exercise when the author himself admits the possible presence of endogeneity bias and the problem with missing expected policy rates in the model for the Euribor.

1.2) The main hypothesis of Chapter 1 is that the volume of excess reserves is negatively related to the spread between interbank interest rates and ECB's deposit rate. When estimating the model over the whole sample (Subsection 1.4.2), it seems that the model specification for the 'normal' regime becomes just a constant. The author dropped the excess reserves presuming that they do not play any role in 'normal' regime. However, this is what the main hypothesis is indirectly asking. If one is interested whether excess reserves play a role in one regime, it should be also tested whether they play any role in the other regime (regardless it is the regime with very low volume of excess reserves). Next, if the model for the 'normal' regime is a constant, I am surprised by the very high R2 for the whole model.

1.3) In general, quantitative easing has been employed for two reasons. First, as an immediate reaction to the credit crunch (after the collapse of Lehman Brothers) to address to heightened risk premiums and to make the credit markets working again – this happens when interest rates were far from its zero/effective lower bound. Second, as a monetary policy tool when a policy interest rate is not available anymore due to the zero/effective lower bound. That is also the reason why interbank rates are not discussed so much in the literature on the QE effects – why to discuss rates, which are at their lower bound? The subject of the chapter relates to the lower bound on the nominal interest rate very closely. However, the existence and effects of the lower bound on interbank interest rates are not discussed in the chapter almost at all.[2]

1.4) I am wondering why the interbank interest rates with longer maturities (Euribor rates) are not modeled in a standard way i.e. as discussed in the literature dealing with the yield curve modeling (for US see, for example, JMCB paper Wu and Xia, 2016)? The author decomposes Euribor into the equilibrium component (Eonia rate) and spread (the rest). This is rather unusual. Defined in this way, the spread includes expected short rates over the period covered by longer rates, which need not be related to "*disequilibrium variation in the risk component*" as the spread is characterized on p. 29. In the subsequent analysis, the spread is replaced by a stress proxy (equations (1.6) and (1.7)), which captures something different than expected short rates.

1.5) Counterfactuals based on multi-equation models are estimated quantitates. As such they are surrounded with uncertainty that should be accounted for (together with the uncertainty from the estimation of the equation for the interest rate/spread) in the second stage when the counterfactual paths of interest rate (spread) are computed.

Chapter 2: Forecasting euro zone interbank interest rates in the presence of excess reserves

Chapter 2 focuses on the forecasting performance of the modelling framework from Chapter 1. It evaluates the forecasting performance of conditional and unconditional forecasts of interbank interest rate (and spread) with respect to various modeling approaches (univariate, multivariate). The results are as one would expect – the two-regime model outperforms linear models if we use the information on the observed excess reserves, the performance is not so

[2] In the literature, there are several estimates of the so-called shadow interest rate, which capture how the short term interest rate would look like if there is no effective lower bound on the nominal interest rate. Estimating the models from Chapter 1 with the shadow policy rates could reveal the role of the effective/zero lower bound.

good if exogenous variables in the models (policy rates and excess reserves) are forecasted as well. The subsequent analysis discusses (rather obvious) fact that two-regime models are appropriate if the forecasted phenomenon exhibits a structural change.

Comments:

2.1) My main concern relates to the comparison of forecasting performance of various models. Differences in forecasting performance of models are not statistically tested (e.g. by the Diebold-Mariano test). The subsequent discussion of specific models in terms of their forecasting performance superiority is thus weakened and conclusions need not be relevant. In addition, the period of forecast evaluation is so short that I see a pitfall that the differences between models in their forecasting performance are not indeed statistically significant.

2.2) To construct unconditional forecasts, the author forecasts exogenous variables (policy rates, partially also excess reserves) as a random walk. Such choice includes several obstacles. First, it ignores the existence of the zero/effective lower bound on policy rates (excess reserves). Next, if working with models in differences such exogenous variables are simply a random disturbance. Then, such a model is equivalent to a model without the exogenous variable. How can be such a model estimated?

Chapter 3: Developing forecasting models using the SpecEval add-in for Eviews

Not commented.

Chapter 4: Responding to the inattentiveness of others: Experimental evidence from a cooperative environment

The chapter takes a proposition from unpublished manuscript Plazonja (2018) and empirically tests it. The hypothesis is as follows: given that it is costly to collect/absorb information due to limited attention then in the cooperative game of two players each agent should increase its attention when the other does so. The experimental evidence presented in the chapter confirms the hypothesis.

Comments:

4.1) The main equation which translates the experimental data into the confirmation/rejection of the main hypothesis is as follows (see equation (4.2) on page 263):

$$AM_{i,t}^{x} = \alpha_i + \beta^{x} f\left(AAM_t^{p}, AAM_i\right) + \delta_t + \epsilon_{i,t}^{x},$$

where $AM_{i,t}^{x}$ is an absolute error from a computational exercise that proxies rational inattention of player i who plays role x (Sender, Receiver) at round t. *AAM*s stands for average absolute error from a preliminary set of computational exercises that suggest what type the player i or his/her partner p is (attentive or inattentive).

While the individual characteristics of player i are controlled for by α_i, I do not see how are such characteristics controlled for in the case of partner p. Consider, for example, mathematical skills of partner p, which are not necessarily related to inattention, but presumably affect AAM_t^p. While mathematical skills of player i are controlled for by α_i, they are not controlled for the partner p. The effect of AAM_t^p on $AM_{i,t}^x$ could be different from what the main hypothesis states.