Review of the doctoral dissertation prepared by Mr. Abhishek Anand, entitled „Procyclical effect of credit risk of banks – econometric analysis for EU financial system"

(supervisor: Prof. Dr. Hab. Mateusz Pipień)

The formal basis for preparing the review is the resolution of the Scientific Council of the *Economics and Finance* discipline of the University of Economics in Krakow, appointing me as a reviewer of the doctoral dissertation of Mr Abhishek Anand.

From a formal and ethical point of view, there is no conflict of interest between the Reviewer and the Doctoral Student. The review follows the requirements specified in the Law on Higher Education and Science of July 20, 2018 (Journal of Laws of 2023, item 742, as amended), particularly regarding Article No. 187.

Assessment of the validity of taking up the topic

The dissertation prepared by Mr. Abhishek Anand is placed clearly within the discipline of *economics and finance*. It discusses the issue of procyclicality in the banking sector across European Union countries, paying attention to the three core areas: loan supply, loan loss provisions (LLP), and capital buffers. The topic is very interesting and important from an empirical and theoretical point of view, especially in the context of the (global) financial crisis of 2007-2008. The sources and the course of this crisis, one of the most disastrous episodes in history, have raised questions about the procyclicality of banking activity. The phenomenon is, obviously, not a novel one. Still, it must be stressed (what the author also notices) that the general perception of banks – and more generally, the financial sector – within mainstream economics before the crisis was, surprisingly, rather poor. Contrary to the heterodox approach, financial institutions (and even money itself) have been generally omitted in the mainstream models and analyses. This resulted in an insufficient understanding of economic mechanisms, leading to the crisis and inadequate assessment of banking risks. All this had serious macroeconomic consequences, manifesting in growing instability and uncertainty.

Thus, after the crisis, there has been a renewed interest in the role of procyclicality in the credit markets, especially in the context of capital regulations, incentives

of banks, and links between the nominal and real spheres of an economy. The financial cycle has been studied more and more thoroughly.

Therefore, the dissertation topic is interesting cognitively and theoretically, as well as in terms of economic practice and policy recommendations. The author properly justified the selection of the topic, pointing to the role of financial variables in business cycles. He pays special attention to the three variables – loan supply, loan loss provisions, and capital buffer – to examine the procyclicality in the banking sector of EU countries.

Assessment of the objectives, hypotheses, and research gaps

The main objective of the thesis is "an empirical analysis of the procyclicality in the Banking sector and cross-country heterogeneity with Seemingly Unrelated Regression Equation system for selected European Union countries". Yet, there is also another statement by the Author that "the main goal of the doctoral dissertation is to examine the procyclicality of the banking sector across three dimensions: loan supply, loan loss provisions, and capital buffer". The question is about the consistency of the dissertation's main goals, as the two named are, of course, similar and closely connected, yet, still not the same. Despite these small discrepancies in this matter, the main objective is adequate and may be treated as achieved through theoretical considerations, as well as through research conducted by the Author, i.e. the successful application of the SURE (Seemingly Unrelated Regression Equations) model to test procyclicality in the banking sector and cross-country heterogeneity for EU countries.

There were also formulated five detailed (partial) objectives. These detailed goals, corresponding with individual chapters of the dissertation, are: (O1) presentation and discussion of main ideas of procyclicality and conceptual relationship with the banking sector; (O2) empirical verification of the procyclicality hypothesis of loan supply for the banking sector and its heterogeneity across the analyzed EU countries; (O3) empirical verification of the procyclicality hypothesis of loan loss provisions for the banking sector and its heterogeneity across the analyzed EU countries; (O4) empirical verification of the procyclicality hypothesis of capital buffers for the banking sector and its heterogeneity across the analyzed EU countries and (O5) application of the SURE methodology for procyclicality analysis.

The goals are adequate; it must be noticed, however, that goals (O2)-(O4) are, in principle, just the decomposition of the main goal. If we take into account the second

take on the main goal (mentioned above), they are only its direct components. Moreover, goals defined in such a way as (O2)-(O5), are rather research tasks, not objectives. Regardless of these doubts, it may be stated that all these detailed goals have been achieved. The theoretical frameworks of procyclicality and its conceptual relationship with the banking sector were described and introduced (O1). Verifying the procyclicality hypothesis of loan supply for EU countries was completed (O2), as well as verification of the procyclicality of loan loss provisions and capital buffer for EU countries (O3 and O4). Finally, a new approach (SURE specification) was introduced to test the procyclicality in the banking sector and cross-country heterogeneity (O5).

There are also some inconsistencies referring to the hypotheses. Namely, on page 14 there are listed four "main" hypotheses of the dissertation: (H1) loan supply of banks is procyclical; (H2) banks hold lower loan loss provisions following higher economic growth; (H3) capital requirements for banks, as mandated by banking regulators, create procyclical effects and (H4) the Seemingly Unrelated Regression Equations (SURE) model is an empirically important generalization of the panel regression in procyclicality analysis.

Yet, on page 70 it is mentioned that the "central hypothesis of the dissertation is that procyclicality varies significantly across countries due to diverse economic and institutional factors, challenging the assumption of uniformity in its effects". And on page 158 another hypothesis, labeled as "primary research hypothesis", is formulated, as "the procyclicality effect is country-specific, exhibiting significant variability among different economies".

The question arises: what are the relations between these categories of hypotheses? The "central" and the "primary" are, in principle, very similar, but what about these four "main" hypotheses? It seems that they are rather subordinated, technical ones, yet it should be precisely expressed.

Yet, it may be stated that the main (central, primary) hypothesis of the thesis was confirmed. The significant variability in results across all analyzed EU countries suggests that procyclicality in the banking sector should be viewed considering the cross-country heterogeneity rather than a single approach applicable to all countries. With regard to "technical" hypotheses, hypothesis H1 is tested positively: loan supply has a significant positive relation with GDP growth for 7 out of 8 countries where coefficients were found to be significant. On the other hand, hypothesis H2 is confirmed for only 4

out of 9 countries with significant coefficients. Hypothesis H3 is supported by the procyclicality of capital buffers, which was found to have a significant positive relation for 7 out of 8 countries where coefficients were found to be significant. Hypothesis H4 has also been validated; the SURE model proves to generalize panel regression and functions as a significant empirical alternative.

Concept of work (including methodology)

The concept of the dissertation is logical and coherent. The considerations are carried out in accordance with the principle 'from general to specific'. It can be seen that the work is conceptually well-thought-out, and the considerations conducted show the author has a pretty good orientation in the field of the subject matter. The way of presenting issues and leading the reader through the dissertation and planned research plan is transparent, and the linkages between individual chapters are clear and justified. The narration is, in principle, logical and precise, and the Author delivers all necessary parts, building the ground for empirical research. The research gap is properly identified and the dissertation fills it, presenting thoughtful and mature research.

Theoretical and empirical parts of the dissertation are clearly indicated and isolated in the structure. The presented theory constitutes a clear framework for subsequent empirical research, which is the essence of the dissertation, as the application of a specific method is at the very center of the Author's considerations. As he stresses, previous analyses of the issue being the topic of the thesis have focused on a particular country or a panel of countries modeled using panel regression techniques. Analyses of country heterogeneity of the procyclical effect seem omitted. Hence, he decided to use the mentioned already Seemingly Unrelated Regression Equations (SURE methodology), which can be assessed highly positively. The application of the methods is correct, and the whole research procedure can be positively assessed.

General remarks

1. The Author notices the paradox that banking, being naturally a procyclical industry, prior to the global financial crisis played in the mainstream literature only a side role. The notion is of crucial importance here, in the context of this crisis, the response of the supervisors, links between nominal and real spheres of the economy, and, finally, designing the best regulatory frameworks. It is, of course, closely connected with the question about the very nature of the crisis

– whether it was only the aftermath of wrong political decisions, wrong incentives on the side of banks, insufficient regulatory frameworks, or just a phenomenon typical and inevitable due to the cyclicality of economic processes? Or, from a broader perspective, is it an obvious consequence of inherent flaws of finance-dominated capitalism identified with the financialization process? In this context, studying (potential) procyclicality of banks' activity is valuable and insightful, especially in the case of the EU.

2. Taking this into account, perhaps it would be also purposeful to depict the broader context: the functioning of the entire economy, together with a more thorough description of the financial systems of the EU countries (models of financial systems, model of supervision, etc.), as well as the description of external conditions and processes (e.g. evolution of approaches to supervision, the progress of financialization, emergence of shadow banking). It would be welcome, giving a solid background for empirical research. For example, the evolution of regulations and recommendations in subsequent Basel Accords (mentioned often in the dissertation) shows clear changes in the approach to risk and its management.

3. One might agree with the Author who states that the main contribution of the dissertation lies in the results and the methods used to obtain them. It can even be stated that the thesis is a little methodologically and instrument-biased. It is typical for dissertations with strong statistical and econometrical parts and, of course, cannot be perceived as any flaw. Quite the contrary, it is advisable and valuable. Yet, even given this remark, the literature part is a bit synthetic and probably could be elaborated more, especially as it regards difficult and complex issues.

4. The introduction is too long, containing many theoretical elements which would have suited better in the chapter 1. Some of them are still discussed (or elaborated) there, which makes an impression of repetition.

5. The conclusions part is, to a large extent, just a summary, with a relatively small share of the Author's own insights and thoughts. Moreover, in a dissertation on such topics, some explicit policy recommendations would be welcome.

6. Perhaps a good idea – to give some theory-methods balance, add even more clarity into considerations, and stress at the same time the novelty of some theoretical issues and input of the author – would be introducing an additional

theoretical chapter on financial cycle, banks, banking risk, lending, etc. Now, some parts of these issues are put in different parts of the dissertation. It is even more important, as the financial systems of the EU countries are not homogenous.

7. But an even more important question, not stressed in the dissertation, is the distinction between the eurozone members and countries that do not have a common currency. It is disputable whether the two categories can be compared and researched as one sample, even taking into account the specificity of the adopted research method. Perhaps there are arguments for such a choice, but a discussion of this issue should be included (probably in section 1.6 or 2.3).

Structure and content of the dissertation

The thesis comprises five chapters, an introduction, and conclusions. The first chapter focuses on conceptual and theoretical issues; the second is a description of the main working horse used by the Author in the dissertation, i.e. SURE framework. Chapters 3-5, analogous in the structure, are empirical, referring to the potential procyclicality of various categories: respectively credit supply, loan loss provisions, and capital buffer. What is important, they provide empirical results with and without the SURE model.

In this section, the content of the individual chapters will be described and discussed in more detail. It must be stated that the remarks and comments here are mainly of polemic and discussion character and do not necessarily refer to flaws or shortages of the dissertation content.

Chapter 1 deals with the theoretical aspects of the business cycles and the concept of procyclicality. The covered topics include the origin and progress of the procyclicality concept. The chapter also includes a vast literature review of studies concerning procyclicality and empirical works performed related to procyclicality in the banking sector. The chapter is correctly prepared and interesting.

Chapter 2 presents the empirical model used for procyclicality testing. It deals with the assumptions of Seemingly Unrelated Regression Equations (SURE) and methods of estimation. Then, it is shown how the procyclicality hypothesis can be verified within this framework. The chapter adequately presents the method. However, there could be perhaps pointed more explicitly also potential limitations of the exploited approach.

Chapter 3 deals with the question of procyclicality of loan supply for a selected sample of 15 EU countries. The research shows that loan growth is typically positive during economic booms and negative during recessions, affecting the quality of loans. Banks may (and usually do!) ease credit standards during booms, leading to riskier loans and potential defaults during downturns. The credit supply is influenced by bank-specific factors (like profitability, credit risk, and deposit base) and macroeconomic variables (such as GDP growth, inflation, and unemployment). Research showed that loan supply generally increases with GDP growth and decreases with rising unemployment, which is rather not surprising. The impact of inflation impact was mixed, while the relationship between bank deposits and loans has evolved with financial innovations. Macroeconomic variables like GDP growth, inflation, and unemployment vary considerably across countries, with mixed impacts on loan supply. Fixed-effect panel regression results initially showed less significant relationships between loan supply and macroeconomic variables, but System of Equations (SURE) models revealed substantial cross-country diversity. Significant financial linkages between sampled countries were also observed, with the SURE model providing more precise estimates than simple regressions. It points to an adequate choice of such an approach by the Author.

Chapter 4, similar in structure to the previous one, concerns the procyclicality analysis of loan loss provisions for a sample of 15 EU countries. Loan loss provisions, are crucial for banks' risk management (building loan loss reserves (LLR), thus protecting capital during credit losses), impact profitability and future capital, provide transparency in financial statements, unlike non-performing loans (NPLs), and signal credit risk to market participants, conveying information about future incomes. The SURE model revealed significant cross-country diversity in the relationship between LLP and economic variables. GDP growth negatively impacted LLP in France, Luxembourg, and the Netherlands, while it positively impacted Portugal, Spain, and Sweden. Inflation was significant in 11 countries, all positively related to LLP. Unemployment showed mixed impacts across countries. Bank-specific variables (loans, profits, deposits) significantly influenced LLP. The SURE model indicated a positive relationship between loans and LLP in 13 countries and a predominantly negative relationship between profits and LLP. Deposits had mixed impacts on LLP across countries. Thus, the Author concludes here that research indicates the importance of considering cross-

country heterogeneity and financial linkages in analyzing LLP determinants. The chapter is well prepared, and the analysis made in it is satisfactory and interesting.

Finally, chapter 5 deals with another issue, namely the procyclicality of capital buffers (the crucial category in regulatory frameworks after the crisis of 2007-2009) in 15 EU countries. The Author notices that links between regulatory capital and the economic cycle vary across banks and countries and are influenced by bank-specific (profits, loan supply, non-performing loans, and ROE) and macroeconomic (real GDP growth, inflation, and unemployment) variables. The growth of the real GDP generally has a negative relationship with capital buffers, indicating procyclicality, while banking sector variables correlate more with capital buffer than macroeconomic factors. The analysis reveals that reporting weak relationships for macroeconomic variables is incorrect, as cross-country heterogeneity is crucial. Fixed-effect panel regression finds only lagged capital buffer and inflation significant. The SURE model highlighted diverse, significant relationships across countries, showing stronger and more precise estimates than independent regressions. The chapter, similarly to the two previous, empirical chapters, is adequate and concise.

Among selected detailed remarks and comments on the content of the individual chapters one might list the following:

1. P. 21 – Figure 1 should not be included literally, moreover, there should be used more scientific source.

2. P. 22-23 – too much technical discussion on identifying tipping points and phases of the business cycle.

3. More literature (not mainly works by Borio) on the financial cycle and its relations with the business cycle would be welcomed (p. 26-28).

4. Why is Juglar's analysis characterized on the basis of secondary sources? It refers also to the presentation of Scumpeter's ideas (s. 30-32). The references should be here to the works of these authors directly.

5. Concerning the Austrian school – the description of its theory of cycle is rather synthetic, at the same time, it is one of the most interesting propositions (even though it did not explain the Great Crisis, it gained more accuracy in subsequent decades). Especially the concept of non-neutrality of money would be worthy of characterizing in the context of procyclicality. Moreover, not the setting of interest rates. (s. 34-35) is crucial in the Austrian analysis., but also money supply.

6. The same refers to Keynes – its concepts are presented too briefly.

7. "However, oil peak in 70's and other external shocks, resulting with high rates of inflation exposed the flaw in the Keynesian framework" (p. 37)– true, but it should be stated, that there were also many other reasons, even more important from the dissertation's topic point of view (e.g. crash of Bretton Woods system, rapid growth of financialization).

8. Very valuable is presented in Chapter 1 also Hyman Minsky's hypothesis – sometimes omitted, yet explaining very well the origin and course of the (global) financial crisis. The same applies to characteristics of the modern theories of the cycle (section 1.3.4), as they are still not widely known.

9. Debate on procyclicality in section 1.4 is valuable and very interesting, yet it misses some institutional considerations, like the creation of the banking union within the EU (along with its consequences). At the same time, some questions seem to be a little overrated (e.g. the role of accounting standards).

10. The issue of credit risks, discussed in section 1.4, should be presented rather earlier (see also general remark no. 6).

11. Distinction between EU countries with and without the euro, as was mentioned, should be included in consideration, also in the context of the research gap (section 1.6).

12. "The crisis posed a threat of widespread bank failures in EU countries and near collapse of their financial systems" (p. 83) – the statement is rather exaggerated. Moreover, it implies homogenous treatment of these countries. It must be stated that there is no such thing as the financial system of the EU. The domestic systems, of which the Author is by no means aware, are, despite similarities and some common frameworks, quite diverse.

The formal side of the dissertation, including the use of sources

The dissertation is carefully edited and formatted. There are very few linguistic and technical errors; there are repetitions or single-sentence paragraphs. Generally, however, the work is written in clear language. The equations and mathematical notations are correct and numbered. The author, despite describing sometimes very technical issues, managed to avoid excessive jargon.

The author correctly uses the sources, expertly presenting the views formulated in the literature and referring to them in an adequate manner. In some parts, as was mentioned, the ideas of some authors are presented on the basis of secondary sources, which is strange, as the original works are widely accessible. Yet, the dissertation uses broad and current (including recent years) literature. However, several so-called 'canonical' works in the field of financial stability and bank crises are lacking).

There are some minor editorial and editorial errors, e.g., fragments of text not assigned to any section/subsection, lack of dots at the end of sources under graphic elements, and some typos (Ludvig von Moses on page 34, "counties" instead of "countries" on page 85).

Conclusion

My overall assessment of the doctoral dissertation of Mr. Anand is positive. This is reflected in the comments contained in the review, which are mainly polemical in nature and suggest possible improvements, which are not, in principle, of a critical nature, pointing out any evident errors.

When pointing out the advantages of a dissertation, several elements can be mentioned. Firstly, the issues of the thesis are important and very current, and at the same time addressed in a comprehensive way and concerning an interesting group of countries. Secondly, the dissertation provides an original solution to the scientific problem posed (both thanks to the theoretical framework and impressive and solid empirical verification). The author demonstrates general theoretical knowledge in the discipline of *economics and finance* and the ability to independently conduct research (meeting the statutory requirements – Article 187.1 – for obtaining a doctoral degree). Thirdly, the work has great application value and – taking into account the importance of the research problem – it may now constitute a valuable source of knowledge for entities interested in the procyclicality, financial crises, resilience of financial systems to crises, the role of regulations and macroprudential policy. It also opens up room for comparisons between different groups of countries constituting the EU. This may contribute to developing the best possible solutions in the field of macroprudential policy and thus reducing the risk of another financial crisis.

Taking into account the above, it should be stated that the doctoral dissertation of Mr. Abhishek Anand, entitled „Procyclical effect of the

credit risk of banks – econometric analysis for EU financial system" written under the supervision of Prof. Dr. Hab. Mateusz Pipień, meets the requirements specified in the Law on Higher Education and Science of July 20, 2018 (Journal of Laws of 2023, item 742, as amended), particularly regarding Article No. 187 and can be the subject of defense and further procedures related to awarding Mr. Anand with a doctoral degree in the discipline of *economics and finance.*

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