Referee Report on Pavel Dvořák’s “Essays on the Microeconomics Banking”
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I have read this dissertation with great interest. Overall, I think that it has a potential to be a very strong thesis.

On the technical level, I found this a very well executed piece of work. The author shows a very good command of the modern banking literature. The theoretical models and the empirical estimations testify to a high degree of competence, and provide valuable insights that add to the literature. I found the empirical part (i.e., the fourth chapter) particularly interesting, and would be glad to see the author submit it to Finance a uver/Czech Journal of Economics and Finance, where I am an editor.

I have a number of comments on issues where I think the dissertation could be strengthened. My comments belong to two groups. First, I have comments on the motivation of the papers and the interpretation of the results. Second, I have a number of comments on the technical issues surrounding models themselves.

Chapter 1

• To provide a motivation for the study, it is important to present some basic data on the international comparison in bank fees. The author keeps referring to “vast international differences” in retail bank fees, without showing a single table or at least a chart illustrating his case. In fact, the cross-country differences in nominal fees are relatively small.

• Page 15: Much is made of the alleged cross-country disparities in fees divided by GDP per capita. What is the underlying rationale for dividing by GDP per capita? It would seem a more natural to start by comparing nominal fees (e.g., in euros or in dollars). In the nominal comparison, the disparities that are alleged (but not shown or analyzed in this paper) become much smaller. This is an important point, because it is used as a motivation for the paper.

• Relatedly, to provide a persuasive motivation, it would be useful to show some scatter-plots of cross-country data on nominal fees versus GDP per capita, Gini coefficients, and a proxy for “noise” in the economy (given that this plays an important role in the paper). Perhaps the data from Chapter 4 could be used here.

• Page 12, last para. before the heading: it is claimed here that the model has general implications for all types of fees, even though it deals with only loan-related fees. This needs to be better explained. The mechanics of the model is based on lending and does not seem to be easily transferred to deposits and other banking transactions. In this context, also the reference on page 35 to fees representing an important part of
consumers’ regular expenses in the transition and developing countries is somewhat misleading, as the model deals only with loan-related fees.

- Page 12, bottom: the text stresses heterogeneity of banks’ pricing models by country, but there is also substantial heterogeneity among banks in the same country. In fact, the bank-by-bank heterogeneity may well be more substantial in some cases: internationally active banks usually deploy the same pricing model across its subsidiaries and affiliates in all the countries in which the bank operates.

- The introduction includes some very general statements that would need to be backed up (e.g., “retail bank fees have always been very important source of income of banks around the world”).

- Also, the paper contains many non-attributed statements that would require proper attribution. For example, page 16, first full paragraph: “explanation often put forward by bankers themselves” … which bankers, where? Also, page 16, the last paragraph: “two prominent factors believed to be affecting the level of bank fees” … believed by whom? Similarly, page 17, beginning of the first full paragraph: “differences in fees are often believed…” by whom?

- One rationale for bank fees is that they provide an additional cushion for banks against risks, in particular market risks. Banks’ net interest income is usually much more sensitive to changes in market conditions than fee income. Moving away from interest income towards fee (and other) income can therefore be seen as a way of limiting exposures to market risk. This also means that if a bank operates in an environment with more volatile interest rates, it has stronger incentives to move more towards fee-based income. This issue could be recognized in the paper.

- Another possible explanation for fees is that of a competition softening device. Interest rates are usually subject to much closer monitoring (e.g., because of monetary policy considerations—they play an important role in monetary policy transmission) than fees. For fees, it is therefore much more difficult to other market participants, banks’ counterparties, and regulators, to make a comparison. This can provide substantial help to soften competitive pressures on individual banks.

- The explanation of the fees in the paper seems to assume self-selection by the borrowers (page 18, last paragraph, third line). But in practice, banks devote substantial efforts to screening the borrowers, actively trying to sort the good ones from the bad. Assuming that banks do not screen and rely on self-selection is not very realistic.

- Page 18, last paragraph, first sentence: the banks’ temptation to increase fees may also be limited by competition among banks.
• Page 19, penultimate paragraph, last sentence: This sentence is misleading, in an important way. Koellinger, Minniti and Schade (2007) do show that entrepreneurial overconfidence is different in different countries (which is not very surprising); however, their paper does not provide evidence for a significant negative link between overconfidence and the level of development (e.g., the United States, one of the most developed countries in the sample, also have one of the highest levels of entrepreneurial overconfidence). For the argument to work, the authors would need to find a significant and large negative link, which is not the case.

• It is not clear what to make of the notion of fees as “a type of collateral” (pages 20 and 21), given that there are some important differences between the two. For example, collateral is returned to the borrower if the loan is repaid; fees are not.

• Given the importance of the numerical illustrations (emphasized in the conclusion), it would be useful to move a summary of the numerical calculations into the main text rather than keeping them only in the appendix.

• Page 35, bottom: the text suggests that income inequality in “young” emerging economies is relatively high and can be expected to decrease. The available studies suggest an increase in inequality compared to the centrally planned economy, but even after the increase, the average Gini coefficient in emerging markets is reportedly close to the OECD mean (e.g., Branko Milanovic: “Income, Inequality, and Poverty during the Transition from Planned to Market Economy”, World Bank, 1998). The case for expecting inequality to decline is therefore not clear-cut.

Chapter 2

• The key notion of this chapter is that information sharing is “an effective device for facilitating tacit collusion between banks.” I do not find the idea that information sharing “softens competition between banks” (page 54) particularly persuasive. The opposite argument could be made (and has been made) that information sharing makes the market more transparent and ultimately strengthens competition among banks. The author should explain better the reasoning underlying this central notion. For example, how to explain that in a number of countries, banks themselves, in particular large banks, oppose creating/extending credit information sharing bureaus? If it helped collusion, they would be expected to push for it. Perhaps a distinction needs to be made between mandatory (government-imposed) and voluntary information sharing mechanisms.

• To make his case more persuasively, the author could illustrate that countries with credit bureaus tend to have weaker indicators of competition (e.g., H-statistics or the Boone indicator) than comparable countries without such bureaus, after controlling for other factors that influence competition. The author’s notion seems to be that there
is a negative relation between the presence of a credit bureau and competition—something that could be usefully shown as part of motivation for this paper. Additionally, from the discussion in the paper, one would expect the relation to be stronger for systems based on relationship banking, and weaker (but still significant) in systems with prevalence of transactional banking. Can this be illustrated? Also, is there a difference between voluntary and mandatory bureaus?

- The key result about the importance of the credit bureau as a “competition softening device” is driven by very specific assumptions about the screening/scoring process employed by banks. The screening technology is assumed to produce only two outcomes: 1 (accept) or 0 (reject). However, in practice, banks assign a range of different scores to accepted borrowers rather than a single score. They can use financial and other information they obtain about the borrowers to discriminate among them, and they can use this as a basis for differentiated pricing (one could model it as a score on the continuous interval from 0 to 1, with 1 being the highest bonity and 0 being the lowest bonity).

- The screening technology is assumed to be costless, exogenous, and the same for all banks (page 63). If this were the case, a bank could do as much screening as it wished (because it costs nothing to screen) and get information that would be as good as the information from other banks (because the technology is the same for all banks). What would be the point of information sharing? The only reason for information sharing in this setup is that the tests performed by different banks are assumed independent (page 64), while presumably the tests by the same bank are assumed to be always the same (which is not clearly stated, but implied in footnote 8). Why should that be the case? Perhaps a solution would be to assume that screening is costly (which it usually is) rather than costless.

- The starting point for the model in this paper is Broecker (1990). But why is Broecker a useful starting point? The model does not inspire much confidence. For example, it equates the degree of competition with the number of banks, a notion that has been challenged by subsequent theoretical and empirical literature.

- Even if credit bureaus were indeed associated with “softer” competition, it is not clear what the policy implication of the paper is. Is it something policymakers should be worried about? Does it make the banks more or less stable? Does it make the customers better off or worse off?

- If credit bureaus are indeed associated with “softer” competition among banks in terms of lending, what does it mean for their competition on the deposit/funding side?
• The proof of the equilibrium seems to require the existence of “menu costs” for banks, mentioned in passing in the appendix on page 74. This assumption should be mentioned in the main text.

• At the end of the paper, the author does present some policy recommendations, but those do not seem well thought-out and well linked to the main text. For example, the author claims that the increase in the number of banks active in the market with information sharing may have a destabilizing effect on the local banking industry.

• As a minor practical matter, the subscripts are a bit confusing, since the subscript “b” is not used for “bad” borrowers (denoted “a”), but for the good ones.

Chapter 3

• Technically, the paper is well executed, but the motivation and policy conclusions could be presented more persuasively.

• The paper seems to be making the case for deposit ceilings, because they supposedly prevent banks from “severe competition” that might lead to “excessively high interest rates on loans.” That is a very controversial measure that would require a much more in-depth analysis and discussion. Even if the high lending rates were a concern, and even if there were a link between lending and deposit rates, would a ceiling on deposit rates be the most efficient and effective measure? For starters, putting a ceiling on lending rates (which is what some governments tried to do) seems to aim at the same objective more directly (leaving aside that it is politically also easier to “sell”).

• The problem with ceilings on interest rates, whether deposit or lending, is that they are aimed at the symptoms rather than the substance of the problem, which relates to protecting financial stability. Also, the ceilings are attempts to regulate prices in contracts between private parties, and as such have been bypassed in many ways, including by charging various fees (positive, or negative, i.e. “gifts”) or transferring business from more regulated entities to less regulated entities or booking business in different jurisdictions. Side-effects may therefore include decreased transparency of the market and decreased informational value of interest rates. For these reasons, country authorities now primarily use regulatory and supervisory measures (such as capital adequacy and liquidity regulations) rather than arbitrary interest rates ceilings.

• The author claims that “many governments” have imposed such ceilings on deposit rates. But the only example it gives is that of the U.S. Regulation Q, which was phased out more than 20 years ago. If there indeed were many governments imposing such ceilings, the author should list them.
• The existence of a link between interest rates on loans and those on deposits is a rather well accepted empirical regularity. Plots of these rates over time normally reveal a close (albeit not perfect) relation between the two. So, showing that such a positive relation can exist in theory is not particularly revealing.

• What would be interesting instead is to investigate the relationship between deposit rates and banks’ financial soundness. This relationship is indeed found empirically, (e.g., in Kraft and Galac, “Deposit interest rates, asset risk and bank failure in Croatia,” Journal of Financial Stability, Volume 2, Issue 4, March 2007, Pages 312-336—please update the reference in the dissertation), and it has some policy relevance. However, making the case for a regulatory ceiling on deposit rates is tricky, and perhaps not necessary. Supervisors have other alternatives, such as establishing an “early warning system” with thresholds for deposit rates.

Chapter 4

• The author needs to address endogeneity issues associated with the estimate. For example, increases in fees are likely to influence the structure of the demand.

• Key to the approach in this paper is the definition of the “representative client.” For the paper to be persuasive, the author should provide a better motivation for the selection of this client. What type of client is it? Does the client has any loans? (Table 4.12 does not provide much help in this regard). Why should a retail client in Slovakia be considered representative for all types of clients in all the countries?

• Relatedly, there seems to be a disconnect within the paper: on one hand, cross-country differences are presented as vast; on the other hand, a Slovak client is a considered a “representative client” for the whole region (even including Austria).

• Is it appropriate to analyze fees without analyzing interest rates at the same time? These are often two parts of the same (this can be seen in practice for example when banks are forced by a regulation to charge lower lending rates than they would otherwise do: they usually increase their fees).

• Where are the potential explanatory variables suggested in Chapter 1? Based on the theoretical discussion in Chapter 1, I would for example expect the Gini coefficient to play a significant role. Also, the author should try include a proxy for “noise” in the economy, which plays an important role in Chapter 1.

• Interpreting banks’ earning on assets as a measure of the bank’s “riskiness” (top of page 111) is not well explained. Higher earnings may indeed reflect greater risk taking, but they may also reflect other factors.
• The author uses market share and Herfindahl-Hirschmann to approximate the degree of competition in the banking market (page 118). However, this approach has been discredited in the recent banking literature.

• Putting the Austrian banks together with the other country banks is somewhat of mixing applies with bananas. This disparity is illustrated e.g. by Figure 4.15.

• The author should provide a more in-depth discussion of the estimation methodology. For example, the presented estimates combine micro- and macro- explanatory variables (i.e. those that are bank-specific with those that are the same for all banks in a country). This creates a clustering of standard errors, which raises some econometric issues (in general, it biases the coefficient p-values upward). How were these issues addressed?

• To what extent does the analysis address cross-border issues (e.g., cross-border lending)? Are the bank data consolidated or unconsolidated (if the former, then the Austrian bank data also include data for their subsidiaries, and substantial double-counting may be involved).

• The limited time dimension of the dataset is a concern, which should be more clearly highlighted. It is strange that the author did not observe dynamic asset growth in the observed banks, given that the period has been characterized by rapid credit growth in most of the Central and Eastern European banking markets.

• The author has assembled a unique database on fees from a number of Central European countries. Given its uniqueness, it would be useful to make this database as a supporting file to other researchers. It will also be important to make the underlying data available so that the results can be independently replicated or re-examined. (If there are confidentiality issues, it may be possible to “anonymize” the data.)

• Why does the exclusion of e-Banka decrease the number of observations by 2, from 122 to 120 (Table 4.4)?