Responses to referees' comments

"Banking Concentration and Financial Stability New Evidence from Developed and Developing Countries"

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We would like to thank reviewers for their insightful comments on the paper, as these comments led us to improve it. We took into consideration all reviewers comments and addressed them in the paper. Detailed responses to reviewers are given below:

Point 1:

Reviewers' comments: "In the introduction the authors should state more clearly the contribution(s) to the existing literature. In this regard, they should shortly explain what exactly they mean by direct and indirect effects and how they are able to discriminate between the two. They mention that they use a very large panel compared to other studies. However, this fact alone might not necessarily deliver "more robust empirical results than those so far existing in the literature".

Response to reviewers

The authors improved the introduction and the literature review to highlight the existence of the current literature and the main contribution of our paper. Moreover, we explained in the paper and highlighted the difference between the direct and indirect channel and how each channel act (page 4).

The statement "*more robust empirical results than those so far existing in the literature*" have been changed in the text by the following : "Second, it considers the largest panel of 173 developed and developing countries, which will deliver more robust empirical results and helps compare our results with the existent results in the literature".

Point 2:

Reviewers' comments: "With regard to the empirical approach, it is hard to believe that all explanatory variables are really exogenous. For instance, it is very likely that GDP growth in period t is affected by a banking crisis that occurred in the same period. Taking lags of the variables on the right hand side could mitigate this problem to some extent and would also be theoretically more convincing as delayed effects of the explanatory variables on financial (in)stability are more likely. Beyond that, the crisis dummy could be one in period t because of a banking crisis at the very beginning of the year".

Response to reviewers

We have lagged all variables on the right hand side of our model as suggested by the reviewer. The revised version delivered new and more convincing results.

Point 3:

Reviewers' comments: "In addition, the empirical analysis could potentially suffer from an omitted variable bias. Thus, the authors should check the robustness of their results when including additional explanatory variables that are likely to affect financial stability. Candidates could be, for instance, private sector debt, total financial liabilities, a measure of financial development, interest rates, and the monetary policy regime. In order to check if a previous boom period represents an important trigger for a future crisis it could also be reasonable to include a measure of the output gap in the past, for instance. Furthermore, since diversification is mentioned as an important channel in the literature review, it might be worthwhile to take that up again in the subsequent analysis".

Response to reviewers
The authors would like to mention that they took preliminary estimations with candidates variables that may affect financial stability as extracted from the literature. For example, interest rates, private sector debt. The authors reported no interesting results for these sets of estimations. Some other variables were not available for the sample countries used in this study. It's worth noting that with the variables used in this study, our sets of estimation show that our model has the required robustness and do not suffer from any omitted variable bias. However, it would be interesting to develop further research including others potential variables that may affect financial stability as suggested by reviewers.

Point 4:

Reviewers' comments: "The authors mention that they use a GMM approach to estimate equation (3) and equation (4). They should be more explicit about their motivation to do so and explain their approach in more detail. They mention that they examine the validity of instruments without stating what is instrumented by what".

Response to reviewers
In general, a dynamic estimation model, such as GMM, would be the appropriate econometric method, see for example Athanasoglou et al. (2008). Use of a GMM estimator also accounts for possible correlations between any of the independent variables (Baltagi, 2001).

Athanasoglou, P.P., Brissimis, S.N., Delis, M.D., (2008). Bank-specific, industry-specific and macroeconomic determinants of bank profitability. *Journal of International Financial Markets, Institutions and Money*, 18 (2) : 121–136.

Baltagi, B.H. (2001). Econometric Analysis of Panel Data, 2nd ed. John Wiley & Sons, Chichester.

Point 5:

Reviewers' comments: "To get a better feeling about the crisis dummy, it would be interesting to see how many countries in the data set faced no crisis at all and how many countries faced only one crisis in the sample period".

Response to reviewers
We don't see any reason for looking how many countries in our sample faced no crisis at all and how many countries faced only one crisis. We would like to remind reviewers that our

crisis dummy variable (financial fragility in our model) is our dependent variable that we are trying to determine its occurrence in our sample countries. We are wondering how and to what extent this could give "a better feeling about the crisis dummy". However, we improved our results with another set of estimations by splitting the sample to 3 sub periods depending on the absence/existence of crises in these subperiods. The new results are interesting.

Point 6:

Reviewers' comments: "The authors should not only comment on statistical significance but also on economic significance when discussing the regression results. In particular, they should discuss the marginal effect of a change in regressors for the probability that a crisis occurs (see, for instance, Table 3). Otherwise, it is difficult to assess what a coefficient estimate of, say, - 0.0114 actually means".

Response to reviewers
The reviewers can easily see the improvements we made on the paper in this regard. Most of the econometric results have been improved by developing the economic ideas behind each statistic results and also compare it with the existing literature.

Point 7:

Reviewers' comments: "The authors should provide details about the different test statistics they list at the bottom of each Table. For instance, it is probably unclear to most readers what is meant by "chibar2(01)".

Response to reviewers
All the tests have been defined and detailed at the end of each table.

Point 8:

Reviewers' comments: "In the abstract and introduction 173 countries are mentioned. However, in all tables it is stated that the number of countries is 156. This should be clarified. From page 4 the reader gets the impression that per capita GDP (PGDP) is included in the set of macroeconomic control variables (X). However, it is not listed in the regression output in Table 3. This also should be clarified".

Response to reviewers
Actually, we first run the regressions using a sample of 173 countries and based on a feedback from another reviewer we run the regression using a sample of 156 countries to better compare with literature. The updated estimation in this version has been carried out with a sample of 156 countries.

We totally agree with the reviewer comment. (PGDP) variable was deleted accidentally. It is now included.

Point 9:

Reviewers' comments: "Finally, the paper lacks thorough editing and proofreading. Examples: Missing subscripts in all model equations, "T-Student are reported…" (see Table 5), Usage of the term "correlation" (see p.6/7), "profit model" (p.8), Table 6 is presented after Table 8, etc".

Response to reviewers

We would like to thank the reviewer for their comments. The authors sent the paper to a professional for editing and proofreading. The paper gas now much improved. For example see Table 5 the "T-Student coefficients are reported in parentheses" is deleted. The Usage of the term "correlation" in page 1 and page 2 and page 6 and page 7 is edited and corrected (see pages 1, 2, 3, 4, 6, 7). Profit model means using ROA. However, profit model is deleted and replaced by ROA (see page 8).
The main reason for presenting Table 6 after Table 8 is that Table 6 is too big.