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Dissertation Evaluation
“Measuring the Impact of Microfinance”

by Nargiza Alimukhamedova

Dear Prof. Slobodyan,

I’m pleased to serve as an external referee for CERGE-EI with regard to the dissertation of Nargiza Alimukhamedova. In my view this dissertation on “Measuring the Impact of Micro-finance” is both novel and interesting. However, I also believe that the manuscript still requires some revision before it is acceptable as a PhD. dissertation. In the following I outline my reservations with respect to each of the three papers in the dissertation.

While I think substantial revision is needed, I do not see any issues which cannot be addressed by the author within a reasonable period of time. I would therefore suggest that my main comments should be addressed (in a revision and/or letter) before the PhD. degree is awarded to Ms. Alimukhamedova.

Yours Sincerely
Prof. Dr. Martin Brown

Comments on Chapter 1 “What do we know about Microfinance at Macro Glance?”

This chapter examines the impact of microfinance on macroeconomic performance using data for 84 countries over the period 1995-2011. The author applies VAR framework which accounts for the mutual influence between country-wide penetration of microfinance, financial depth and real economic growth. The author also splits the sample into countries with different institutional and economic conditions to examine how the relation between microfinance and growth depends on the economic environment of a country.

Comment 1: The author claims to look at the relation between microfinance and economic growth. But her indicator of real economic performance captures the level of real per capita income. In my view this is a mistake. If the research question is to examine how microfinance affects growth then the author should (as is common in the literature, e.g. King and Levine 1993) use an indicator of real growth instead of real income levels.

Response 1: Following the comment, additional estimations using dependent variable as a growth rate of real GDP per capita were generated and included in the text. Additional explanations / narrative for new tables were also added.

Comment 2: The presentation of the data is not sufficiently clear for the reader to get a picture of the raw correlations in the data. The author should include a table in the Appendix showing the main variables (microfinance penetration, broad money, GDP growth, GINI coefficient) by country.

Response 2: Following the comment Table 11 with correlation of main variables with microfinance was generated and added into Appendix.

Comment 3: Scatterplots (with country-years or country averages as observations) relating micro-finance to broad money, GDP growth and inequality would also be helpful in understanding the data.

Response 3: Following the comment, the scatterplots were added.

Comment 4: Table 1 should include information on how many countries / years the main variables are available for.

Response 4: Table 1 was augmented with additional column with country / year observations.

Comment 5: I would suggest integrating the presentation of the data and methodology into one section. It is very cumbersome to read the methodology section without knowing what data are being used

Response 5: It is a good suggestion, however for the coherence of the chapter the data and methodology sections can't be merged.

Comment 6: Clustering of countries: I really like the idea of clustering countries in order to look at the heterogeneity of the effects across economic conditions. I'm worried though that there may not be sufficient observations in cluster 1 (only 14 countries). The author should discuss whether this is an issue or not for her VAR approach. If it is maybe it would make sense to merge 1 & 2 and contrast with cluster 3.

Response 6: It is a good suggestion. However, it is not possible to merge the data of cluster 1 with other clusters as cluster 1 represents the most stable and developed countries. As can be observed in Table 6a, indicators for Cluster 1 and Cluster 2 (3) differ quite substantially. Therefore Cluster 1 can't be combined with others as it would indicate combining different countries. The low number of country observations affects only the power of the test.

Comment 7: It is not clear to me how I should think about the impulse-response analysis of microfinance. What would the microfinance "shock" be? Is it the entry of a large foreign microfinance bank or a big government program? How big could such a shock be in reality? What does a 5% shock for 12 periods mean in the impulse response analysis? Would the microfinance pro-gram /bank withdraw after the 12 periods? Are these 12 years or 12 months? This needs to be discussed in much more detail.

Response 7: Following the comment, the section on impulse-response analysis was revised and improved in the following way: "As a conclusion to the empirical analysis, we visualize the impact of microfinance by plotting impulse response functions. The initial level of the impulse of microfinance is set at 5%. Potential example of shock coming from microfinance could be outside donor supporting microfinance in the country, i.e. large donor supported microfinance investment into the country and its impact on macroeconomic and financial indicators. The period of the impact of the shock is chosen for 12 periods as being the reasonable time for evolution of the impact. We visualize the effect of microfinance disabling across three clusters. Given that the effect of microfinance is primarily captured through its outreach, we present the impulse response functions for microfinance borrowers only. We also model the microfinance market as finite and assume a saturation point at around 3% of population".

Comment 8: The literature review does not cover the most recent experimental studies on the impact of microfinance (which are then mentioned in the third paper of the thesis) and completely ignores the non-experimental evidence on microfinance impact (e.g. Pitt and Khandhker 1998).

Response 8: In fact, the literature covers prominent experimental studies particularly conducted by Banerjee et al. (2010). Other non-experimental impact studies are not deliberately covered in the paper given the robustness of impact methodology and finding the impact.

Comment 9: The information in Figure 1 is already shown in Table 2

Response 9: Figure 1 is presented for illustrative purposes to highlight the difference across clusters.

Comment 10: Table 1 includes the variable "Financial Depth" which is not used in the analysis.

Response 10: Following the comment the Table 1 was revised and updated. In particular, the variable "Financial Depth" was eliminated from Table 1.

Comment 11: Tables 3-5 could be merged to one table

Response 11: Table 3 and Table 5 content a bit different data, therefore they can't be combined.

Comment 12: I would suggest reporting the clustering approach in the data and methodology section rather than in the results section.

Response 12: For coherence purposes of the chapter, the clustering approach was preserved in its original place.

Comment 13: Why does the number of observations for cluster 1 (but also the other clusters) vary for MF borrowers and MF portfolio in Table 6a-8a. Why doesn't the number of observations for the 3 clusters add up to those for the whole sample.

Response 13: This is due to the nature of original base. In particular, some observations for particular MFIs is missing, some MFIs report only for particular year.

Comment 14: Tables 6c-8c need to be discussed or dropped

Response 14: Following the comments, Tables 6c-8c were dropped from the paper.

Comment 15: Some references are missing.

Response 15: References list was revised and updated.

Comments on Chapter 2 “Microfinance Environment in Uzbekistan: Analysis of Supply and Demand”

This chapter provides an analysis of microfinance supply and demand in Uzbekistan. The main contribution of this chapter is to empirically examine the location decision of MFI's across different regions of the country.

Comment 1: As in the first paper, the presentation of the data is not sufficiently clear for the reader to get a picture of the raw correlations in the data: The author should show a map of MFI penetration by region. This should also be compared to the penetration of ordinary commercial banks and that of the Mikrokredit bank.

Response 1: A chart with distribution of MFIs across all regions in the country has been included. Unfortunately, the data on penetration of commercial banks and in particular of MikrokreditBank is not available. Given the state priority, the branches of commercial banks including of MikrokreditBank is purposely allocated in each region and most of the districts.

Comment 2: Some of the data in Table 7 seem unreasonable (or wrong) and warrant more discussion. For example there seem to be 152 hospitals per 10'000 inhabitants on average. The share of SME in gross regional product is extremely low and needs to be discussed. Industrial production seems to be only 0.57% of GRP on average should this not be 57%?

Response 2: The data provided in Table 7 is as of year 2001 which is considered as early transition period of Uzbekistan which then is translated to the regional statistics. Additional explanations have been corrected for indicators, i.e. not “hospitals” but rather “medical points”. The latter term is considered as a broad definition of “access to medical services” and includes hospitals, medical points, beds, medical receptions, doctors and other health infrastructure. As for GRP, it is related to early transition period.

Comment 3: The variables in Table 7 are not all well explained: How are the variables “density of roads” or “density of population” measured? Why isn’t trade scaled by population or GRP? Why isn’t the economically active population scaled by total population?

Response 3: Additional explanations for “density of roads” and “density of population” is provided. The scaling of trade and population is not applicable in our case given that we use district population among other indicators.

Comment 4: A univariate comparison of the main variables for regions covered / not covered by MFI’s (with simple t-tests) would be very helpful.

Response 4: Following the comment the new Table 7a with comparison of main variables has been generated.

Comment 5: In the empirical analysis of the location decision, it might matter whether the MFI’s are competing with other financial providers. In particular, the presence of Mikrokreditbank might affect the location decision of other MFI’s. The author should really control for the branch network of this key market player in each region.

Response 5: It is very good point. However, according to a qualitative research conducted by the author in 2011, Mikrokreditbank and non-bank MFIs constitute quite different type of micro lending. The lending mode differ both by method (i.e. size, collateral, documents, procedures). Therefore presence of any commercial bank and/or Mikrokreditbank branches is not considered creating substantial competition to non-bank MFIs as they have their own micro lending niche. Moreover, given the state priority, the branches of commercial banks including of Mikrokredit Bank is purposively allocated in each region and most of the districts. Therefore there is no variation of bank branches across regions.

Comment 6: The analysis of “excess demand” in section 4 is incomplete, and makes (in my view) no contribution to this chapter. A detailed analysis of demand would be based on firm-level survey data which documents which types of firms in which regions don’t apply for credit, apply for credit and get it, and apply for credit and get rejected. The current analysis is a superficial analysis of rejection rates based on some interviews with MFI staff. The author does not even say how many MFI staff were interviewed. I would suggest dropping this part of the chapter and focusing on the supply side analysis.

Response 6: This is a constructive comment. However, for Dissertation and holistic presentation of the chapter, the section on “excess demand” is being decided to be remained. The critical comments have been incorporated. For further journal publication version, the section will be eliminated.

Comment 7: I would like to know more about who owns / runs the Credit Unions and Microfinance Companies and what their objectives are. This is important to understand the main “location” analysis. For example do local politicians or businessmen support the opening of MFI’s in their home region?

Response 7: Both CUs and MCOs are considered to be private entity. To open non-bank MFI license should be obtained from the Central Bank, however, geographical location is under free choice and decision of the owners. More details on ownership, objectives of non-bank MFIs is provided in section 2 of given Chapter.

Comments on Chapter 3 “Better Access to Microcredits: Does Geographical Proximity Matter?”

In this chapter the author uses survey data for 1086 households to examine how access to microfinance affects household consumption and entrepreneurial activity.

Comment 1: There is almost no information on the underlying survey: Who designed the questionnaire, who paid for the survey and who ran it? How were respondents chosen from among clients of CU and MCO? Were the MFI's involved in the selection of borrowers? How were the non-borrowing respondents sampled? The author should provide a reference to the questionnaire (e.g. has an online appendix in English with the questionnaire).

Response 1: Following the comments a separate Appendix on data collection, field logistics, funding grant agencies was added. In addition, the survey questionnaire was also added.

Comment 2: The presentation of the methodology is very confusing and I think inconsistent. In section 4.2 it is argued that the author is estimating the ATT (average treatment on the treated) effect: This would be the average effect of borrowing on consumption and entrepreneurial activity of those which borrow. My understanding of the ATT is that one needs to compare consumption and business activities of borrowers to that of non-borrowers. Given the non-random assignment of borrowing, respondent distance to the next MFI could then be used as an instrument for borrowing in an IV framework or a conditioning variable in a matching framework of borrowers vs. non-borrowers.

Response 2: Following the comment the methodology section is being revised. In particular, the parameter to be estimate is being refined as to “being similar to ITT with different intensity to treat proxied by geographical proximity”.

Comment 3: The alternative approach (which is followed by the experimental studies that are cited in this chapter) is to estimate the ITT (intention to treat) effect. This would imply comparing the consumption and business activity of households which are close to MFI to those which are not, independent of whether they borrow or not. Here the challenge is to account for non-random location of MFI branches. A matching procedure based on region-level characteristics (identified in the previous chapter) would help here.

Response 3: Following the comment the methodology section is being revised. In particular, the parameter to be estimate is being refined as to “being similar to ITT with different intensity to treat proxied by geographical proximity”.

Comment 4: Currently my view is that the author is mixing these two approaches: She is estimating the ATT effect with a matching methodology. But as far as I understand from the chapter, she is not matching borrowers versus non-borrowers, but instead close versus non-close households. But maybe I'm just really mixed-up by the very confusing exposition of the data and methodology in this chapter. In this case, the author should integrate the data and methodology sections and improve their description.

Response 4: Following the comment the methodology section is being revised. In particular, the parameter to be estimate is being refined as to “being similar to ITT with different intensity to treat proxied by geographical proximity”.

Comment 5: It is unclear to me what the dependent variables are in the analysis. The author discusses the retrospective methodology in detail, which apparently gives her information on consumption and business activities over the past 10 years. But then she does not tell the reader for which period the main dependent variables for business activity (business revenue, business profit, business size) or consumption (household expenses, income and assets) are measured. Are these end-of-period measures, or average measures over the entire period? The author should add an appendix detailing the definitions of these main variables.

Response 5: The dependent variables represent the average values for year 2010, given that the survey was conducted during January – March 2011. Respective explanations were added to the text. Additional explanations are also provided in respective sections of the methodology section.

Comment 6: The author could cite the broader evidence on distance and lending (Degrees / Ongena, Hauswald / Marquez). The author could relate her approach to two recent non-experimental papers which examine the impact of geographical distance to an MFI on financial inclusion

o http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2109492

o http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2226522

Response 6: Following the comment, mentioned two studies were included into literature review section.

Comment 7: What are the variables “pre-treatment” in Table 3?

Response 7: These are in fact “Initial Covariates”, i.e. confuse in terminology of matching technique. Respective corrections have been done in Table 3.

Comment 8: The notes to Table 5 are copied from Table 4 and as a consequence are not correct.

Response 8: The notes in Table 5 are revised and corrected.