

Answers to the Referees' comments:

Referee 1

Review of Do transfer costs matter for foreign remittances? A gravity approach

Achievements

This paper uses data on remittances from 23 countries to Pakistan from 2001 to 2013 to analyze the effects of transaction costs of remittances. It argues that distance is an inadequate proxy for remittance costs and that the remittance costs are negatively correlated to remittances from a country. The paper presents new evidence on a generally underexplored phenomenon and is also well written.

Major Comments

Despite this I have a number of comments to the paper. These relate to the interpretation of the results of the paper, measurement issues, data and the presentation of result

1.) Interpretation – I am very concerned about the author's causal interpretation of the results. **In particular my concern is that with respect to the central variable of interest (transaction costs) what the authors actually measure is self-selection. Immigrants that want to remit more (for unobserved reasons) are all else equal likely to self-select countries that offer low transaction costs.** In addition given that the authors explain how important remittances are for Pakistan in section 2 I also have serious doubts as to the exogeneity of the bilateral exchange rate, GDP at home and credit to the private sector. The authors therefore need to think much more carefully about their identification strategy, potential bias resulting from these assumptions needs to be discussed and to be more convincing the authors should also see whether they can improve their identification. In this context **I wonder whether the Pakistan Remittance Initiative in 2009, and the National Cash Remittance program in 2012 cannot be used to identify effects of reducing remittance costs.**

ANSWER: Concerning the interpretation of the results as causal, we do not think that the self-selection problem is affecting the results. Indeed, immigrants do not select the countries where they migrated according to the cost of remitting, but according to the prevalence of migrant-networks or relatives in the destination country and the easiness of the migration process. The avenue followed in the paper to identify the causality is the use of panel data. The inclusion of country fixed effects in the "fixed effects" model controls for sources of endogeneity related to unobservable heterogeneity that is country specific and time-invariant. Moreover, the Hausman Taylor estimation, in which the GDP variables are considered as endogenous, shows that the results remain the same.

2.) Measurement – I do not understand why remittances are measured in absolute value rather than per capita in equations (1) through (4). It seems to be clear to me that if remittances are measured in absolute values then they will be highly correlated with the migrants residing in a certain country and this should be a very important variable in all specifications. Indeed the authors' results in tables 3 and 4 are very suggestive of this. The migrant stock is significantly positive with a coefficient that hardly ever differs statistically significantly from 1 (except for in the Hausman Taylor estimates, which may however, be due to the weaknesses of these methods).

2 – Also it is not really clear whether the Z variables in equation 1 are measured in absolute values or in logarithms (equation 1 suggests they should be in logs, the tables 3 through 5 suggest they are in levels, and the text does not clarify). The authors need to justify better why they use the dependent variable or move to a measure of per capita remittances.

ANSWER:

Remittances are measured in absolute terms to make the results comparable with the related literature. However, results with the dependent variable in per capita term could be presented in the revised version of the paper. As indicated in the Notes below the Tables of results, all the variables are measured in natural logs except the dummy variables and bilateral exchange rate.

3.) Data quality – Given that a substantial part of the data is extra- and intrapolated the authors should make their assumptions for extrapolation much clearer and should also try to justify these (i.e. to state only the most obvious, why is Switzerland with its huge banking sector like Norway in terms of transaction costs?) It would also be important to see how the results are affected by the intra- and extrapolations, by an extra regression using only actually measured data.

ANSWER:

We also run regression using the actually measured data and the results indicate similar estimates but most variables loss statistical significance. We could present these results in an Appendix in the revised version of the paper.

4.) Presentation of results – I really do not see why three tables of regression output are needed I would much rather the authors would • **use only one (or at most two) panel estimators**. Since there is little variance across different estimators it could be mentioned that the other estimators lead to similar results • reduce on the number of specifications. In particular here I do not understand why in the results for equation (4) presented in table 5 the migrant stock, which is highly significant in all other specifications as well as credit to private sector are dropped. This sort of non-nested design makes me very skeptical of the robustness of the results in table 5 to including the obvious missing variables in the specification. The authors should therefore reduce the reported output and include the missing variables (migrant stock, credit to private sector). Also could the central point of the paper that distance is a bad proxy for transaction cost not be highlighted by simply regressing distance on transaction costs.

ANSWER:

We agree with the suggestion, we can follow this advice and present only the main results in the text and move the rest to the Appendix. The reason why the migrant stock and the credit to the private sector are not used in Table 5 is because they are used to predict the cost of remitting. When distance is regressed on transaction cost the results indicate that there is a positive correlation, we could show the results in the revised version of the paper.

Minor comments

Between table 3 and 4 GDP(host) changes too GDP (source) and GDP (home) to GDP recipient In a number of cases the authors state that the sign of a variable may depend on the remittance motive,

without explaining further how this is. As somebody who has not worked on remittances before I would need an explanation. This applies to - P12: passage referring to GDP per capita at home - P13: passage referring to exchange rates - P18: passage on institutional variables

ANSWER:

As indicated in page 14: "Pakistani migrants send more remittances when the economic conditions improve at home, which supports the portfolio investment motive", on the other hand, the altruist motive will be supported if the migrants send more remittances when the economic conditions in the home country deteriorate or when there is no correlation between the economic conditions at home and remittances. On the other hand, in page 16 is stated: "However, remittance flows to Pakistan do not seem to respond to the source country's economic conditions. This is in contrast to the findings of Schioppa and Siegfried, (2006), Vargas-Silva and Huang (2006) and Kemegue et al. (2011) who argue that remittances are more responsive to the host country's economic conditions than to the economic conditions of the home country. The results can be explained by considering the extent of the migrant integration into the formal sector of the source economy."

2 Reviewer of manuscript: "Do transfer costs matter for foreign remittances? A gravity model approach" by Ahmed and Martinez-Zarzoso

The paper addresses a potentially interesting question: how sensitive is the magnitude of foreign remittances to the costs of remitting from abroad. The authors analyse this question for the case of remittance flows to Pakistan. They use data on bilateral flows for a panel of 23 host countries of migrants from Pakistan. Their measure of transaction costs is an original prediction based on regressing the available price of remittances on the stock of migrants and the financial development in the respective host country. The assumptions made for this imputation are strong but rather plausible. The estimated panel models are decently justified. The results are mostly as predicted. The host country GDP and its geographical distance from Pakistan are insignificant under all specifications. The models reported in table 5 hardly illuminate the effects of the proxy for transaction costs on remittances. Their estimated coefficients are statistically significant and have a negative sign under all specifications. But this opens more questions than it provides answers. Apart from potential sources of bias due to the constructed measure of costs, we cannot distinguish between the effect on overall flows and the switch to informal, unrecorded channels. As the authors indicate in section 3, less than 40 per cent of remittances to Pakistan were transferred via the banking system. They give somewhat contradicting statements on the differences between the costs of using the formal and the informal methods (in particular huwala and hundi - which are presumed cheaper in footnote 4 but not in the first paragraph on page 6). The only way to address such issues and to properly engage with the question of the paper would be to use micro data - like Gibson, McKenzie and Rohorua (2006) do for Pacific islands and Anwar and Mughal (2012) for Pakistan.

ANSWER: The results in footnote (4) and in the first paragraph of page 6 and indeed not contradictory, since the text refers to the result found in Arif (2009), which is specific for remittances sent from Saudi Arabia to Pakistan. This fact will be clarified when revising the paper. We agree with the comment arguing that we are not able to address in this paper the switching between formal and informal channels when sending remittances, however the use of micro-data is outside the scope of the paper.

Section 3 also includes some details on the Pakistan Remittance Initiative (PRI) was launched in 2009 and on the National Cash Remittance Program to enable NADRA centres to process remittances using smart national ID cards. The impacts of these programs are not discussed in the empirical part. But particularly the latter would provide an almost experimental setting to address the question of the paper. I recommend major revisions of the current draft.

These should consider the points above and: • restructure and simplify sections 1 to 4 formulating explicit predictions for the models and stating the preferred theoretical justification; • consider the use of micro-data or include more countries - all variables used are easily available for a cross-country analysis; • could some of the host country variables be used as instruments to correct for biases arising from measurement errors (e.g. if informal remittances decline as a result of lower costs)? Are remittances endogenous to the macro controls used? • provide a better intuition on the average cost-elasticity of remittances and calculate the expected per cent increase in formal remittances from lowering the cost of sending money through banks or operators to levels found in competitive markets.

ANSWER:

Thank you very much for this suggestion, we will take it on board and analyse whether there is a significant difference before and after 2009, concerning the amount of remittances sent through formal channels. We also aim to present some test to check for the endogeneity of remittances.