Comments on the overview paper “The Impact of Migration on Children in Developing Countries” by Andrea Rossi

By

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1. Introduction

The overview paper by Rossi was commissioned by the Rockefeller Foundation to map the existing state of research on the topic of “child and youth migration in …developing nations”\(^1\). It was intended to indicate the strengths and weaknesses of the current knowledge base as a means for evaluating the circumstances of child migrants in the area surveyed, and to suggest areas for future research on the topic. The paper executes the assignment extremely well. It is lucid, well structured and nuanced. The introduction usefully sets the stage, introducing clear definitional frameworks and raising key points (e.g., the need to consider in-country as well as international migration, the problem of counterfactual evidence in assessing the benefits of migration). The structure of the paper is transparent, many of the arguments advanced are compelling and the research conducted appears to be thorough and carefully evaluated.

There is perhaps some imbalance between different sections of the paper – the section on remittances is particularly impressive, more detailed and precise than the section on forced migration and south-south migration – but this may to some extent be an artifact of the scope of the assignment and the marked differences in data available. No doubt the fact that study of remittances implicates migrants in the developed countries (and is not therefore strictly speaking a “south-south” issue) has affected the extent of research interest. The section on forced migration is rather compressed and does not fully survey the extensive literature on the topic – perhaps because this was not thought to be germane to the assignment (I will not comment on that section though clearly a lot could be added to the very succinct points in the paper). The excellent point made in the introduction (p.4) that the study of south-south child migration might shed light on non monetary factors impinging on the migration decision (since earning disparities between the sending and receiving country are likely much smaller) is not explored further, though this would seem to be an important suggestion. The concluding discussion on the definition of households, and migrant households in particular, could perhaps be revisited to pinpoint more clearly the relevance of these definitional debates to research on children affected by migration, since as currently presented, this is not immediately obvious: why should

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research in this area use “household” rather than “family” as the unit of analysis? If this is an artefact of current national surveys, then the consequences need to be addressed. But these are minor points: in general I found the paper most engaging, and I look forward to discussing some of the questions it raised.

My comments follow the structure of the paper.

2. The Effects of Migration on Children left behind

Given the magnitude of the phenomenon of children left behind, it is notable that so many key research questions remain unanswered. Is the negative U shaped relationship between international remittances and income inequality that is referenced on p.13 a widely accepted phenomenon? What are its implications for child wellbeing over time? More generally, what is the impact of remittances over time on migrant families? What impact do immigration policies and laws have on them? Does facilitation of family reunion increase the likelihood of consistent remittance income because migrants expect to maintain long term contacts with their families, where restrictive policies have a chilling effect because migrants adopt new families in the destination state? What are the main factors that determine whether and when a left behind child will be brought to the migrant parent’s destination state? Some of these general questions are raised by the literature review and would, in my view, merit further exploration in the discussion and perhaps research attention in future. The legal framework is particularly crucial.

The real lacuna in the data, as the paper stresses, is the impact of south-south migration. And yet, according to the figures presented in the paper’s introduction, this is a hugely significant phenomenon, both numerically and socio-politically. For the purposes of correcting this information deficit and devising a viable strategy for future research, it might be interesting to explore the reasons behind the dearth of research, especially given the plethora of work on development, anthropological questions and other related topics in the south. Is the fragility of the states involved and their inability to collect adequate basic survey data a fundamental issue? Is the lack of financially sound stakeholders in the topic and therefore a shortage of research support a reason? Is the relative informality of the immigration systems, and the consequent lack of concern about the precise scale of migrant flows a factor? A clearer picture of the reasons for the lacuna would assist in devising strategies to address it.

I found many of the observations in this section of the paper fascinating: the differential impact of household income vs remittances on reducing low birth weight (p. 16); the allocation of remittances to particular forms of child education (choice of private schools to combine access to higher quality education with institutional child care, p. 18); the complex data on the impact of the prospect of future child migration on investment in education in the home country (p. 19); the data on seasonal maternal migration (p. 24) and more generally on maternal remittance behaviour (sending funds to children rather than spouses p. 27) and finally the perceptive comments about the impact of remittances on female use of time (does it lead to more child care or not? P. 27). Some points struck me as less compelling or persuasive: what evidence is there that migration improves
children’s health by increasing mothers’ health knowledge (p. 17) – given what we know about migrants’ health outcomes in the early stages of migration, and the absence of linguistically appropriate health facilities in many contexts, one would welcome more reflection on how the transfer of “new information and values” works in practice. Also, I would welcome some discussion on the “culture of dependence” argument (p. 19). This would seem to raise some of the same counterfactual evidentiary problems that the paper references in connection with other migration related outcomes. Finally, it was not clear to me why remittances should have different influences on the extent of child work for children working within or outside the household (p. 22) – increased land or equipment could expand the scope for child work rather than reduce it. This part of the argument could perhaps be expanded and clarified, since it is a critical aspect of the overall assessment of the impact of migration on child wellbeing.

3. Migrant Children in Developing Countries

The paper boldly attempts to provide a preliminary remedy to the lack of meaningful global estimates of child migration, and in particular south-south child migration, by developing several immigration indicators. As a lawyer with limited statistical sophistication, I am not well qualified to comment on the value of the data presented. I will confine myself to a couple of generic observations. Firstly, I was not persuaded that the indicators added much useful information to the discussion. In the absence of more targeted and focussed data, it was not clear to me that these ballpark figures could be useful to understand the structure of child migrations or the factors influencing them. I agree with the paper’s claim that this data is useful for stimulating discussion at the Bellagio meeting – where a more refined set of frameworks might be developed. Second, one might need to have a clearer sense of what constitutes a “developing country” – listing Poland, Lithuania and Argentina alongside Sudan and Nepal requires some explanation. Third, it was not obvious to me what the value of conclusions such as “for countries such as Uganda, El Salvador, Poland and Mexico, the impact on child population is bigger than the impact on youth population” was (p. 37). Impact on what? For whom? When? As the paper correctly indicates, the presence of sizeable populations of undocumented (or unregistered) migrants might also reduce the value of the official data.

Turning to the rest of the paper, as I have already indicated, there seems to be some compression of data and analysis which might benefit from more careful attention. For example, on p. 39, there is a discussion of the differential impact of migration on health care opportunities for rural and urban migrants, but conflicting findings aren’t addressed or explained, so the data adds little to an understanding of the broader phenomenon. There is also mention of the beneficial impact of rural-rural migration in the case of Uganda, likely a result of the prolonged conflict and the danger to children (risking recruitment as child soldiers) in many parts of northern Uganda – without this background the findings are hard to interpret.

4. Empirical Challenges and Data Requirements
The only substantive observation I can offer on this section is to question whether the distinction between migrant and non-migrant families and individuals is indeed as radical, systematic and enduring as the paper suggests. It seems implausible to suggest, given the multiple factors that impinge on migration decisions, the contingencies that arise and the variations observed in the data, that there really is so radical a divide between the two groups. For example, while it is certainly the case that migrants are not usually “the poorest of the poor”, there are many exceptions to that general fact. One only has to consider the thousands crossing the US/Mexico border in the deadly heat of the desert, or the indebtedness of rural Chinese families struggling to keep up with payments to “snakeheads” to note that migrants are not always wealthier than their non-migrating counterparts (the historical example of Irish migration to the UK as a result of the potato famine comes to mind). It is not clear whether one can reliably assert that “those who migrate have a higher than average educational level” – higher than the national average, than the average for their community, their age and gender group? In the search for causal effects of migration, it might be acceptable to compare migrating and non-migrating populations from the same region and community, particularly given the difficulties (as the paper points out) of conducting “before and after” surveys of migrants themselves. I look forward to discussing this and the many other insightful points raised in the paper.