Commentary on Harttgen and Klasen, “Well-being of Migrant Children and Youth in Europe”

The theoretical section frames migration decisions in terms of push-pull factors: real wage (pull) and demographic (push) differentials between origin and destination countries. This framework is quite narrower than the framework developed later in the paper for evaluating children’s wellbeing. To the extent parents decide to migrate not just for economic reasons (higher wages), but also based on their expectations for their own children if they migrate or if they stay, I feel the theoretical framework should be broadened and made more consistent with the evaluation framework. For instance, push and pull factors may not account for a parent’s decision to move so her children may get a better education (in terms of their future life chances) in another country. Moreover, I’m not sure push factors (e.g., population growth) are still predictive of migration flows, especially in Europe. Many former East European countries are among the main origin countries of migrants to the West, and they have lower (more negative) population growth that some of their destination countries.

The authors argue that the economic impact on receiving countries is positive for several reasons. I don’t think this is entirely agreed upon at this time. At least Coleman and Rowthorn (2004), which the authors cite elsewhere, argue against this view, and their dissenting, albeit minority, view should probably be represented. The argument that “migration also plays an important role for the social development of the sending countries” is plausible but would also need to be supported by some evidence. I do not know of any study documenting this, other than the study by Frank and Hummer (2002) cited in the paper on the developing countries. The other interpret their finding of a positive effect of migrants’ remittances, net of household effect, as an effect of migration on health knowledge and behavior.

The framework for measuring the well-being of migrant children is very comprehensive and nicely thought-out. As mentioned above, it expands beyond the earlier theoretical framework, and
unfortunately, probably much beyond the empirical availabilities, as noted by the authors. Although one is hard pressed to find anything important missing in this framework, I would only add one human right consideration: the deportation risk that migrant children may face if committing offenses. I can only agree with the authors, however, on the difficulty of comparing social exclusion among children and between countries. In the list of indicators provided pp.46-7, it is unclear if any could be disaggregated by migration status. On education, the point that we shouldn’t just focus on educational attainment but also on whether migrant children and migrant youth can realize their skills is a great insight, but investigating this would likely require going beyond the current age range of 0 to 18 years.

On data sources, it is a little hard to see what would come out of comparisons at the aggregate level, because migrant and non-migrant children are likely to differ on many individual characteristics other than place of birth. Having data at the micro level would seem essential to get to the equity issue of whether migrant children are worse off net of those individual characteristics, or which of those contribute most to potential differences. As mentioned by the authors, there are several international survey programs with cross-sectional data at the individual level. Although I agree that the cross-sectional nature of these datasets limits our ability to draw causal inferences from them, panel data might not be the panacea either. Their design is more appealing to be sure, but in the context of migration, some of it short term, some of it illegal, one would have to worry about differential coverage and re-interview rates over time. Another limitation of the available cross-national datasets, however, is that they are designed to document particularly well one dimension of child wellbeing by investigators who are experts in the corresponding field, typically at the expense of other relevant factor. The Trends in International Math and Science Study (TIMSS) for instance, provide no less than 10 “plausible” values of a student’s math and science achievement, but has no variable for household income. The assets that investigators included in the questionnaire are those thought to affect learning directly (e.g., number of books, personal computer at home). These items allow constructing a possession index, which is related
to wealth, but doesn’t allow testing separately for an income effect. Finally, the authors note that “there are hardly any comparable micro data on health”, but WHO’s Health Behavior of School-Age Children (HBSAC) study would seem to have relevant survey items on mental health and behaviors (e.g., getting drunk, smoking).

The point on the heterogeneity of the migrant experience is obviously an important one, as well as the distinction between short-term and long-term effects, but those won’t be easily captured in the databases mentioned above. In most surveys, it is likely that only a variable such as “country of birth” might be available. It might well be missing in some countries (e.g., France in TIMSS), perhaps for political reasons, but in any event, the availability of additional variables to tap into the diversity of migration experiences is quite unlikely.