I found both the paper by Kenneth Harttgen and Stephan Klasen and the paper by Andrea Rossi to be very useful in laying out a large number of issues related to migrant youth and children of migrants. Both are well written and cover large and diverse literatures. I will discuss the two papers jointly, sometimes focusing more on one paper than the other.

One thing that I was hoping to get out of the papers was some sense of what we know about how migrant youth and the children of migrants fare on a number of outcomes. In that regard I felt that I got a bit more interesting detail from the Rossi paper. That might be because it had a brief that was somewhat easier to connect to existing empirical literature. There are a number of papers that do things like estimate the impact of remittances on children’s health and schooling back in the sending region, and Rossi’s paper does a nice job of surveying those literatures. I did not feel like I learned as much from the Harttgen-Klasen paper about empirical evidence on the well-being of migrant children and youth in Europe. Perhaps there just isn’t much research in this area, which in turn may be due to the lack of good data. Even some very crude estimates would have brought the paper to life a bit, if only as a way of illustrating the kinds of policy questions we might be interested in and the kinds of methodological problems we face in trying to answer them.

While both papers do an excellent job of discussing conceptual issues, I found myself thinking about a number of theoretical issues throughout the papers. I will organize that discussion in terms of three questions: 1. Who migrates? 2. Why do children migrate? 3. Why do children stay behind?

Who migrates?

Whenever I hear a discussion about child and youth migration I think about the classic and age profiles of migration that have been documented for decades. I thought that either of the papers could have benefited from showing such a profile. I generated two age profiles using census data from the IPUMS-International web site (Minnesota Population Center 2008). These are shown in Figures 1 and 2 at the end of this note. (As an aside, I thought census data should have been given more attention in discussion of data, since it often includes questions such as those used for Figures 1 and 2). Figure 1 shows the proportion of individuals at each single year
of age who lived in a different state or a different country five years before the 2000 census. For those under 5 it shows if the proportion born in a different state or country than their residence at the time of the census. The bottom panel shows a similar figure for France based on the 1990 census, where the reference period is the 1982 census. Both figures show the pattern we would see in almost any population at any point in time. The highest migration rates are observed among young children and among young people roughly aged 18-30. This is especially true for internal migration, where there are generally few legal restrictions on migration. But it is striking that we see roughly the same age profile for international migration. Children 0-5 are more likely to be recent immigrants than almost any other age group other than 18-30 year-olds.

While the patterns in Figures 1 and 2 are well known to researchers who study migration, they seem important to the discussion for several reasons. First, they remind us that migration of pre-school children, both internal and international, has long been a fundamental component of migration. Second, they are a useful starting point for thinking about the reasons for migration of children and youth. Child migration almost by definition involves migration decisions being made by someone other than the migrant. They are usually accompanying their parents, who are coming out of the other bulge in the age-migration profile. Whether or not the interests of the child are taken into account, an issue discussed below, a child migrant is unlikely to be the decision maker. Starting around age 15 we start to see youth migration that may be very different. Youth migrants are much more likely to be the decision makers. Even if they are not the decision maker, they may well be the focal person for the migration decision. That is, the migration is very likely to be about them and their education, work, or marriage, in a way that is seldom the case for children.

**Why do children migrate?**

Although both papers mention many reasons why children migrate, I felt that a number of important issues did not get the attention they deserve. In this regard I was struck by what seemed to be one glaring omission from the very impressive lists of references included in the two papers and in the “New frontiers” rationale paper. That paper is Jacob Mincer’s “Family Migration Decisions” (1978). Mincer’s paper is mostly about joint migration decisions of husbands and wives, which is why it may not be widely cited in discussions of child migration, but the paper is relevant to child migration in a number of important ways. First, it developed the notion of “tied migration.” Although Mincer applied it to spouses, we can think of broadly as referring to family members who move because the total gains to migration for the collective family are positive,
even though they are zero or negative for the tied migrant. I think it is useful to think of child
migrants in this way, at least for those cases in which the migration is a voluntary move made
because expected benefits are thought to exceed expected costs for the joint family (considering
both costs and benefits very broadly).

The second way in which Mincer’s paper is relevant is that it develops a useful theoretical
model for thinking about family migration decisions. The literature discussed in the Harttgen and
Klasen paper is mostly about individual adult migration. Although H-K have a good discussion of
the “new economics of migration” that incorporates the idea that families may use migration as a
means for insurance and consumption smoothing (for example by sending young men to the city
to earn wages and send remittances back to the family in the rural area), these are not really
models of family migration in which families move with their children. Mincer’s model is a better
way to think about migration in which children get dragged along when one or both parents move.
Indeed Mincer talks about the role that children of different ages may play in migration decisions.

To the extent that the papers discuss why children migrate, the papers tend to give the
impression that children are mostly an afterthought in whatever migration decision caused them to
move to a new country (or to be left behind). This seems like an important issue. If we assume
that parents care about their children, then they presumably give at least some weight to the
interests of the children when migration decisions are made. In the extreme case, migration may
take place primarily for the benefit of the children. Parents may want their children to have better
education and economic opportunities that are available in the destination country, and may accept
lower income and worse living conditions for themselves in order to improve the lives of their
children. At the other extreme, parents may impose enormous disadvantages on the children in
order that one or both parents (or perhaps some other adult guardian) may have access to better
jobs or better services. Most child migration is probably in between, with children’s welfare being
neither the primary motivation for family migration nor being totally ignored.

*Why do children stay behind?*

As both papers point out, we are interested in children who are left behind as well as children
who are tied migrants. This seems like a very important issue to focus on, and a very nice survey
of existing studies is provided in the Rossi paper. One of my reactions to research on the impact
of migration on children left behind is symmetric with my reaction to research on children who
accompany their migrant parents – why are the children left behind and how should we think
about the counterfactual in analyzing their outcomes. The typical research in this area, as is clear from Rossi’s very good survey, does some kind of a comparison of “left behind” children with other children, or estimates the effect of remittances on outcomes such as health or schooling. The problem with these studies is that we don’t know why some parents migrate and others don’t, we don’t know why some children are left behind, and we don’t know what would have happened to a particular “left behind” child if the migrating parent had stayed home (or if the child had gone along with the parent). In the case of research looking at remittances, is the counterfactual having a migrant parent who sends no remittances? Is it having a migrant parent who sends a bit more or less remittances? Or is it having the parent back at home? While these are perhaps obvious questions, the literature often does not seem very careful in thinking about them.

The Rossi paper has a nice discussion of the endogeneity of migration decisions. These are so fundamental that I would like to have seen the discussion much earlier in the paper. I found it hard not to think about them throughout the discussion of the literature on the impact of remittances or the impact of parental migration on children left behind. I would also like to have seen a similar discussion in the Harttgen and Klasen paper. When researchers compare migrant children and youth to native children and youth, they may have a number of questions in mind. For some purposes, such as planning the provision of schooling or public services in the host country, we may simply want to know how the groups compare, without regard to whether the migrants are positively or negatively selected relative to their origin country. For other purposes, however, we presumably are interested in questions such as whether the migrant children or youth are better off than they would have been if they had not migrated. This is very difficult question to answer, but seems like a fundamental counterfactual if we want to really understand international migration of children and youth. I think we will be more likely to find the answer to that question if we use a model such as Mincer’s to model family migration.

References


Figure 1. Location 5 years ago by age, United States, 2000 census

Figure 2. Location at 1982 census by age, France, 1990 census

Note: if born after last census, location at last census indicates place of birth