After reading the rationale paper for the conference and the two background papers, I am persuaded that there are many interesting and important issues to be studied related to migrant youth and children of migrants. Much of this research needs to involve clever and careful empirical analysis of high quality data, data that for many questions is in very short supply. Hopefully the program of action that generated the conference will motivate the collection of new data that will be made available to the full community of researchers. While it may be new data collection that is most important to advance our knowledge, I would like to focus on the need for theoretical modeling to guide the design, collection, and analysis of that data.

**Theories of family migration**

One of the important future directions for research in this area is more serious attention to theoretical models of family migration. As pointed out in the Rossi background paper, migration is not randomly assigned to families or to individual members of families. Families move for a reason, individual members of families move for a reason, and children either join their parents or stay behind for a reason. Those reasons may be complex and impossible to observe or measure, but their existence must be kept in mind in any comparison of migrant children with native children or of “left behind” children with parents who live with both parents. Economic models of family migration of the type developed by Mincer (1978) can provide a number of insights about migration decisions. In the case of children, these models need to also think about the motives of parents in making decisions that affect their children. The logical place to start would seem to be a model of altruistic parents who take into account the impact of anything they do on their children. This doesn’t mean that it is only the welfare of children that matters, or even that the welfare of children gets a large weight in parents’ decision process. But the same motivations that cause even poor parents to spend significant fractions of their income investing in their children’s health and schooling must cause parents to give some consideration to the costs and benefits of migrating on their children when they make migration decisions. This has implications for any empirical comparison of migrant
children with native children or of “left behind” children with other children.

One of the points of Mincer’s analysis was that “efficient” family migration decisions will maximize the total net benefits of migration, taking into account all benefits and costs to all family members. The net gains to the winners should be larger than the net losses to the losers, so that it is possible (at least in principle) to compensate the losers and make everyone better off. Benefits and costs should be defined very broadly in considering a move from Point A to Point B. In addition to narrowly defined economic components such as the direct costs of migration and the differences in wages, housing costs, and prices between the two points, the calculation should also include psychic costs and benefits such as emotional attachment to friends and family. The fact that school-aged children are much less likely to migrate than pre-school children suggests that parents do give weight to the interests of children, since it is presumably the disruption to children’s schooling that is the major difference between moving with a five-year old child and moving with an ten-year old child.

Sometimes family migration decisions involve something other than joint family migration, as when one parent migrates and the children stay behind with the other parent. This can be very important in international migration, and drives much of the literature surveyed by Rossi on the impact of remittances on child outcomes. The same basic theoretical approach can be applied to migration that splits families. An efficient family decision about who migrates and who stays behind should be the one that maximizes the total net benefits to the entire family. Whether all or part of the family migrates, it should in principle be possible for all members of the family to be better off if the migration takes place than if it doesn’t. Remittances are exactly the kind of transfers required in a Mincer-type model of family migration to spread the gains of migration to all family members.

As in all economic models of the family, there are a number of important caveats that should be raised. One is that from a policy point of view we may not be satisfied with the weight that parents give to the children’s welfare in making migration decisions. We may be concerned that children experience disproportionate burdens of migration, either when the whole family moves or when one parent migrates and leaves the rest of the family behind. It should also be added that there might be too little, as well as too much, migration from a child’s perspective. Parents may be reluctant to leave rural areas or reluctant to leave their home country even when children would benefit greatly from better education and labor market opportunities in
the city or in another country. Migration is surely one of the great vehicles for intergenerational mobility, and we might find that the migration decisions of parents often generate lower levels of migration than would be optimal from the standpoint of children.

**Bargaining and migration**

Another important caveat to a model in which migration decisions are assumed to maximize total family benefits from migration (which implies some kind of unitary household decision making) is that such a model assumes away many important issues related to family and household bargaining. If a mother and father face different costs and benefits from migration, and if there is no mechanism for the kind of side payments required for a Mincer-type efficient migration decision, then whether migration takes place (and whether by all or part of the family) will depend on the bargaining power of the parents. This may have important implications for children, since the mother may put more weight on the children’s welfare than the father does. This is a potentially fruitful direction for research, although the data requirements are a challenge. Once again it is important to emphasize that putting more weight on the children’s interests need not imply that the family should be less likely to migrate or even less likely to have one parent migrate. Children may be the biggest winners from migration, even when it appears very disruptive to their lives. One of the pitfalls in comparing migrant children and youth with native children and youth is that it is easy to focus on what are often large gaps between migrants and natives. This can be especially true in comparing foreign immigrants to natives, a case in which data limitations often make it difficult to compare the immigrants to some comparable children back in the home country. Large differences between migrant children and native children, who are relatively easy to compare in many data sets, obviously do not mean that the children are necessarily worse off from having migrated.

**Migration and early childhood outcomes**

I direct the Cape Area Panel Study (CAPS), a longitudinal survey of young people in metropolitan Cape Town, South Africa, that has collected four waves of data since 2002. The survey is a collaboration between the University of Michigan and the University of Cape Town funded by the U.S. National Institute on Child Health and Human Development (Lam, Seekings, and Sparks 2006). A number of observations from our CAPS research are relevant
to future directions in research on migration of children and youth. One of our consistent findings in CAPS is that early childhood experiences have an enormous impact on outcomes such as progress through secondary school and early labor market experience (e.g., Lam et al. 2007). This is not surprising, and is consistent with the extensive research in the United States pointing to the importance of early childhood development (Heckman 2006). We find, for example, that early school performance, as measured by number of grades failed and performance on a baseline literacy and numeracy test, is much more important in explaining large racial gaps in progress through secondary school than are the enormous racial differences in quality of secondary schools.

It seems natural to link this growing consensus on the importance of early childhood development with research on child migration. Going back to the theoretical issues discussed above, it is interesting to consider the extent to which parents face a tradeoff between the short-term disruptions of migration for children and the long-term benefits of moving a child to an environment with better education and labor market opportunities. This is especially salient given the fact that young children are significantly more likely to migrate than school-aged children. Since, as shown by Heckman and many others, the pre-school years seem to be extremely important in long-term development of cognitive skills, migration may have high long-term costs to the extent that it disrupts early development. This raises another important caveat to the theoretical model sketched out above. Parents do not necessarily have complete information about the tradeoffs being made in making migration decisions. Migration models have long emphasized this uncertainty in terms of the probability of employment or the future path of wages in the destination region. Another less obvious source of uncertainty is the impact of short-run disruption on the long-run development of children. This uncertain impact must be weighed against future education and labor market opportunities for migrant children that are also uncertain.

**Implications for empirical research**

Thinking carefully about theoretical models of family migration can provide an important foundation for thinking about important issues of selection and endogeneity in any empirical analysis of migration related to children and youth. In addition, these theoretical issues can lead to interesting new empirical questions, although the data challenges are often enormous.
Among these questions are the following:

- To what extent do family migration decisions internalize the costs and benefits experienced by children?
- Do parents give more weight to the interests of some children than others in migration decisions, for example with regard to gender, age, birth order, or perceived returns to migration?
- Do mothers and fathers agree about the desirability of migration in terms of the interests of children, and does bargaining power affect the migration decision that is made?
- Are there negative impacts of early childhood migration on early childhood development, and how are these affected by the age at migration?
- Does the short-run impact of early childhood migration differ from the long-run impact?
- Do parents have accurate perceptions about the short-run and long-run impact of migration on children?

Some of these issues have been analyzed in existing empirical research, although little of that research has looked directly at international migration of children. I think there are fruitful directions in future research that connect research on child migration with research on parental investments in children, intra-household decision making, household bargaining, and the impact of early child development on later outcomes.

**References**


