

External reader report on “The Role of Early Intervention on Childhood Skill Formation” by Liyousew Gebremedhin Borga

I enjoyed reading Liyousew Borga’s dissertation. The research is careful and innovative and the writing is clear. The three papers adhere nicely to the overall theme. Given the dominance of randomized control trials in contemporary development economics, it was refreshing to read a dissertation that applied creative techniques to non-experimental data and generated illuminating, and often plausibly causal, results. I found myself learning a lot.

Borga’s research builds on a long line of work that argues that early childhood experiences and investments play a major role in shaping later outcomes. While this broad connection is widely accepted, there remain many open questions about the specific nature of this relationship and the determinants of investments. Borga’s dissertation addresses both of these questions. His first paper sheds light on some questions about the links between investment and outcomes. He uses time use data from household surveys in multiple developing countries to understand how childrens’ allocation of time across activities affects the acquisition of skills, both cognitive and non-cognitive, that are likely to matter for long-run achievement. The second and third papers focus on determinants of investments. Specifically, the second paper provides a theoretical and empirical examination of how household characteristics and differences in innate abilities across siblings affect the allocation of parents’ investment in children. And the third paper uses the time-variation in the rollout of policies designed to empower women in Ethiopia as a way to identify effects on opinions and investments in children.

I have a number of comments on the papers. Some of these are straightforward questions about empirical methods and interpretations—for readers like myself who are unfamiliar with some of these methods, it would be useful to have a fuller, more technical, discussion of how certain specifications address particular identification problems while leaving others unresolved. I think this is generally something the author should keep in mind especially when submitting papers to general-interest journals. I also make suggestions about possible extensions and ideas for new papers. This is a very rich area of research, and while the dissertation in its current form is entirely adequate, there is some low hanging fruit that the author could consider for new papers in the coming months.

I present my detailed comments by chapter.

Chapter 1 demonstrates Borga’s skills in framing questions in novel ways. Indeed, childrens’ own time use should be viewed as investment decisions. The chapter quantifies the ways in

which time spent on work is a bad investment relative to time spent studying, and how these tradeoffs change with age.

I would have liked to see a more rigorous exposition of the relative strengths and weaknesses of the empirical specifications. As it stands, they are applied somewhat mechanically to show that the results are consistent across specifications. But considering that different models could produce different biases, perhaps more could be learned about what is endogenous by comparing estimates across the specifications?

Relatedly, there are several important, and quite interesting, endogeneity concerns in the data. These should be fleshed out—there could be reverse causality; investments and outcomes could be jointly determined by innate ability; a child who enjoys school is likely to do better in school and to spend more time on school-related activities. Some thoughts: (a) We see that fixed effects models deal with the potential endogeneity of input choices and unobserved endowments. But what about endogeneity of input choices and *observed* endowments? Using controls will not solve this problem. (b) Non-cognitive attributes especially must affect inputs (consider self-confidence as example). I imagine there is a psychology literature on this that the author could look into. My suggestions here are meant to enrich the discussion of identification in the chapter. I realize that the data will prevent a perfect resolution of the concerns, and that is fine.

I think the chapter could also be strengthened by remembering that time allocations are the result of *some* optimization problem, however imperfectly implemented. For example, surely there is some agency on the part of households, so when one child spends an hour on schooling while another spends five hours, it is because they perceive different marginal returns? This leads me to a question about how we should interpret coefficients. If the coefficients describe the effect of a particular activity relative to an hour of schooling, how do we interpret them given that an hour of schooling has a different marginal value to different children?

Finally, there are two areas for interesting future extensions. First, it could be instructive to think about spillovers across children. The data in this case might be limited, but if one had access to data with some basic social networks information, it would be interesting to learn how one child's time allocation affects her friends' choices and outcomes. Second, some activities like sleep must have a nonlinear effect on outcomes—at low levels of sleep, more sleep raises the returns to studying, but at high levels of sleep, more sleep simply crowds out studying. I would be curious to see if such nonlinearities show up in the data.

Chapter 2 is a study of a specific incomplete markets problem, where an inability to contract on the returns to investments prevents the singular pursuit of maximized marginal returns. Given that the investment one receives from parents depends on many factors, there are several open questions: what kind of sibling should one want, and how does this depend on household characteristics and birth order? Again, the chapter has some very interesting results and I appreciated seeing compositions of endowments and investments in some detail.

The model demonstrates how underlying parameters could lead to a multiplicity of relationships across sibling investments. It would really help to have the mechanics of the model and results more intuitively explained. In the chapter, the proposition statements are explained in works but the “why” in some cases still remains obscured.

On the top of page 62, the authors write that they have two approaches for dealing with the possibility that endowment is itself driven by parental investment (reverse causality). But only one approach is presented. Perhaps there was a typo or I missed something?

I wonder if the model could be used to greater potential by linking the regression coefficients back to parameter regions in the model. For instance, from results that show parents reinforcing educational inequality but not health inequality, what might we learn about underlying model parameter values?

The third chapter examines the effects of policy changes designed to empower women on, among other outcomes, investments in child health. As the author acknowledges, data limitations and the potential long-run nature of returns to health investments make this analysis somewhat difficult. But I wonder if the data has other variables that could be interpreted as indirect measures of other child-specific investments. If so, it would be useful to look at those too.

While reading about the reforms, a number of questions arose in my mind. It would be useful to have more information, even if it is informal or anecdotal. Were the reforms credible? Are the new rules enforced? Do they apply to formal assets only? If so, what happens to informal assets that are shared by households?

Also, what was the political economy of the timing of implementation? This information is central to thinking about the identification problem, but is not discussed much in the chapter.

The paper could be strengthened by fleshing out the theoretical framework in less generality. For one, this would allow the model to be better integrated with the data analysis.

Furthermore, there are some natural extensions that could be interesting. For example, consider whether the reforms create a disincentive to save/invest (since husbands know that they get to keep less of the returns to investment in the event of a divorce). Could such a policy have inadvertent effects that are counterintuitive but plausible and testable?

I'm happy to state that this work meets the formal and content requirements for a PhD thesis in economics. I recommend the dissertation for a formal defense. Well done.

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