

Dissertation Review – Comments by Pietro Biroli

“The Role of Early Intervention on Childhood Skill Formation”

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I think this is a very strong dissertation: the research ideas are innovative and nest well within a growing field of applied economics, the data analysis is rigorous, the graphs and tables are informative and well crafted, the writing flows well. I believe that the content of the three chapters satisfies the necessary standards for awarding a PhD degree; however, before defending the dissertation, I believe that the first paragraph of the introduction should be re-written.

Here below I provide more detailed feedback with questions and suggestions.

Chapter 1

Although I understand that this chapter of the thesis is forthcoming in the Journal of Development Studies, I would like to ask a few questions to understand better why certain modelling decisions were made. Many of the following questions also apply to the following chapter.

1. Following the literature spearheaded by Cunha and Heckman [2007, 2008], Cunha et al. [2010], why didn't you estimate factor scores to proxy for cognitive abilities and socio-emotional skills?
2. I liked very much the variety of methods used to approach the estimation of the achievement production function delineated in equation (1.1); the corresponding graphs that rank the time input coefficients (Figures A.3 to A.6) are very informative. However, in your estimation of the cognitive production function, why didn't you control for the noncognitive measures – and vice-versa, why didn't you control for cognitive abilities in your estimation of the noncognitive production function? This would allow you to control for time-varying observables that are certainly correlated with the regressors (current ability θ_{ija-1}) and also estimate ‘cross-productivity’ of these skills.
3. It would be interesting to have a discussion of causality. The approach is set up to minimize potential concerns relating to endogeneity, and the graphs and the different estimation methods go a long way to ensure us of the robustness of the results. However, all of these estimates are ultimately correlational. I believe they are informative, but what can we learn from them, and what else is left to be proven?

4. Among the many estimation methods used, it would be interesting to see also an application of the one proposed by Agostinelli and Wiswall [2016b,a]

Minor points:

- Where are the non-linear results where a second degree polynomial is used? Did you also try a different functional form, such as a CES, translog, or others suggested in Griffin et al. [1987]

Chapter 2

I find the topic of reinforcing or compensating parental behaviour very interesting and important for the field to move forward. I very much appreciated the thorough review of the theoretical and empirical literature in the field. A few comments and suggestions:

1. I believe that there are many commonalities with the paper by Yi et al. [2015], and it would be useful to be more specific regarding your contributions to the literature as compared to this paper.
2. When describing compensating or reinforcing investments in different domains, it might be useful to compare the trade-off between the two using a unique budget constraint: is the total amount of resources invested in each child similar? Are the (reinforcing) educational investment perfectly offsetting the cost of the (compensatory) health inputs? Should the discussion regarding equity between siblings take this into account?
3. What about noncognitive skills? Do the results change or take a different connotation once these skills are considered?
4. A discussion of external validity would be useful: are there single-child household in the data? How different are they? Can these results be used to shed light on countries with different levels of child labor, fertility, etc?

Minor points:

- The introduction could benefit from a better description of how you “construct a measure of human capital at birth that is plausibly net of maternal investments during the prenatal period.” This is a crucial point of the paper, and not very clearly defined from the outset.

Chapter 3

This final paper tackles a different yet very important topic by focusing on women empowerment and leveraging policy-level exogenous variation to identify the parameters of interest. I think this is a very promising paper, but compared to the previous two chapters, it might require some more time for polishing and fine-tuning.

1. A strong stance is taken against the traditional model of unitary household, but how many (applied or theoretical) researchers truly believe in the validity of this model? Isn't the common understanding of the literature that the unitary household model is flawed, yet simple and useful as a thought experiment or possibly a benchmark (see for instance Chiappori and Naidoo [2017]? The introduction might benefit from discussing this evidence.
2. Instead of estimating factor scores in a first step, and then a difference-in-differences estimation in a second step, couldn't you estimate everything jointly in one step?
3. Why there is only a low and non-significant correlation between empowerment and decision making (Table 3.2)? What does that tell you about the validity of your exploratory factor analysis (EFA)? It would be useful to give more information about how this EFA was performed.
4. Would your result change if controlling for multiple hypothesis testing (see for instance Romano and Wolf [2017])?

Minor point:

- The introduction does not mention what results are found by Gray (1998) and Stevenson and Wolfers (2006).
- Put standard errors in Figures 3.2 and 3.3
- Table 3.5 does not report the results for the empowerment index
- Page 104 reads "The results largely remain unchanged after controlling for the empowerment indicators of the mother" but that is not the case. Also the statement "the fact that the height-for-age indicator was significantly affected by the reform is an indication that, while wasting related problems may be addressed effectively with time, stunting related problem, may be more challenging to address" is not corroborated by your tables, since wasting is not affected by the reform, but stunting is!

- Why do you control for empowerment in panel B of Table 3.6? Isn't empowerment affected by the reform and therefore endogenous – i.e. a “bad” control? How come the coefficient in column (1) *increases* after controlling for empowerment?

Smaller Points

1. In the introduction, you mention that “Returns to investments made in early childhood are comparatively superior to investments made later in life for the mere reason that beneficiaries have a longer time to reap the rewards;” however isn't this statement true only under certain assumptions? For instance, I believe this is true only if the returns to investment in early childhood are greater than the returns to other potential investments (for example, the return to investing a dollar in the stock market and then using the capital plus interest to invest in adolescence, or bequeath the money later in life). Another important distinction is the presence of critical or sensitive periods of investment in early childhood.
2. Although very common in the economics literature, I find the term ‘noncognitive’ quite misleading, since there are a lot of ‘cognitive’ processes are involved in self-control, pro-social behaviour, grit, and the like. While initially the term was used to define the residual from a regression of a certain outcome on a measure of cognitive skills – and therefore was orthogonal to cognitive measures by construction – the measures used in your dissertation are often correlated with cognition. I would suggest using a different terminology, such as socio-emotional skills or soft skills, but it is a matter of taste.

References

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