**Referee Report on MS 2017412, “Quantifying the Life-Cycle Benefits of a Prototypical Early Childhood Program”**

*Reviewer #1*

This manuscript offers impactful new findings that expand the emerging literature that involves re-analysis of longitudinal data from experimental (and non-experimental, in some cases) studies of early childhood educational investments. Perhaps as important, as these findings, themselves are for public policy and the vigorous debate about relative benefits from early childhood investments are the advances in the sophisticated methodology represented in this article. That is, the authors provide exemplary detailed specification of their formulae and their accompanying assumptions that underlie and guide their computed models along with thoughtful presentation of alternative assumptions and their consequent effects on findings. The authors bring many sophisticated ideas and methods to bear (although one can still argue with some of these, as discussed below) and extend the previous reports that they and other teams have presented in several notable ways. Specifically, this team extends the analyses to estimate economic impact across the life cycle, rather than limiting the projections to the earlier age periods when certain datasets were obtained. Further, they add to their estimates the impact of the early childhood program on the parents of the study participants (i.e., maternal employment/income effects) - often overlooked; the health benefits - also rarely included in other return-on-investment and cost: benefit papers (by incorporating actual treatment costs incurred in childhood and projecting cost savings based on the fairly recent health indicators obtained from study participants when they were in early middle age); and a more expansive set of cost estimates associated with crime. Finally, this paper presents, to this reviewer's knowledge, the most detailed and up-to-date calculations of the program's costs in 2014 dollars. These are definite strengths.

Despite many strengths of this paper, there are few errors and omissions that warrant correction and that will improve the value of this worthwhile set of analyses.

First, the title is misleading. The ABC/CARE projects do not in any way represent "prototypical" child care. The title is misleading and might suggest that one could expect comparable benefits from almost any form of child care. This was center-based child care that offered low staff to child ratios, active monitoring, used a curriculum, etc. The title should be revised to be more accurate. Similarly, the program should be referred to more accurately as a child development center form of childcare throughout the paper and its features briefly reiterated in the discussion.

Second, this paper unduly relies on citation of the authors' own unpublished analyses and sometimes the unpublished work of others. This is not necessary in most cases. The authors fail to cite in the appropriate locations the peer-reviewed publication(s) that make(s) the same point about many foundational issues, such as the study's original design and prior seminal findings. For example, the fact that the children in the control condition were not prevented from attending center-based childcare in the community appears in major prior articles and the benefits of this intermediate, non-randomized treatment "substitute" has been effectively documented (notably, see Burchinal, Lee, and Ramey, 1989 - Type of day-care and preschool intelligence in disadvantaged children, Child Development). Why would the authors cite an unpublished source of their own (specifically, Garcia et al, 2017, unpublished) for this fact? Actually, the Garcia et al (2017) is cited so often that one wonders how much it overlaps with this paper and what is needed from that not readily available paper to understand this current paper? This strongly detracts from the current paper. Similarly, specific previously published findings about effects of the early educational intervention at age 30 and 21 should be cited more directly in the introduction and at intermittent places later so that readers can more fully appreciate the context of this new economic forecasting analysis. To the extent that the current authors notice subtle differences or include additional data can be mention, when needed, but not to the exclusion of providing readers more details about these studies and previous findings.

Closely related is that earlier reports sometimes appear at odds with the assumptions and perhaps the data incorporated into this new, expansive economic analysis. A prime example is that there were no significant effects on earnings or crime at age 30 of the original treatment, at least as previously analyzed. This should not be ignored -and if differences occur, the authors should offer reasons for these discrepancies.

Finally, the paper lacks a much-needed section on limits and a contextual interpretation of these two studies. The projects occurred in a small university town that had high income, high education families and a school system deemed excellent by almost all parameters. Further, the proportion of minority and low-income families was very low, as noted in many prior publications about these studies. More importantly for a lifetime analysis, most of these study participants remained nearby and the Research Triangle Park has many job opportunities, at all levels, thus increasing employment opportunities - probably for both groups of study participants. Thus, although many of these findings may generalize, they need to be contextualized for cohort and geography. Another issue is that the gender differences that favor males in one way, but females in another way warrant greater discussion. These cannot be disaggregated from complex U.S. issues around race and gender, expectations, racism, opportunities, etc. A thoughtful mention of this will much improve the contribution of this important paper.

Recommendation: Accept with minor revisions and expanded references and discussion to be submitted.