REFEREE REPORT

The paper is not very clear regarding the objective of the paper or the key differences of the results with respect to the existing literature (of which the author has good knowledge, as shown in the review of the literature). The paper is not even clear about what are the key results of the paper.

Given this lack of clarity, I will try to identify what I think are the key results that seem to arise from the econometric results, and discuss them.

Result 1) the relationship between debt and growth is linear; nonlinearities arise only when current debt ratios are used rather than past ratios (table 2). Note that institutions do not matter, despite the author's claims on p.8: what eliminates the significance of the nonlinear relation in table 2 is the timing of the debt ratio (comparing columns 3-4 versus 5-6), not the presence of institutions (in fact, comparing 3 versus 4 and 5 versus 6 yields no difference in results)

Result 2) The relationship is linear even when splitting the sample by LIC and MIC (table 3)

Result 3) Debt services crowds out total investment only in LIC, but not in MIC (table 4).

Result 4) external debt raises investment only in MIC (table 5).

Regarding results 1 and 2)
If the non-linearity depends on the level of the institutions, as shown in the cordella et al. paper (very well acknowledged by the author), entering institutions linearly in the regression, splitting the sample by income, or checking different slopes by income, would not uncover than non-linearity. The author needs to inspect the deep non-linearity arising from the interaction between institution and a non-linear function of debt in order to claim that the non-linearity does not matter.
Also, if lagging the debt ratio is so crucial for the results, the author needs to deepen the analysis. Cordella et al. discuss the fact that lagging the ratios would automatically introduce the opposite bias of the one arising from current debt ratios, if income is mean reverting to a trend (an income collapse in current 5-year period--say due to terms of trade changes--is likely to be followed by an increase in income during the next 5-year period, so that, for a given debt stock, the mechanical increase in the current debt ratio will be associated with future higher growth). Smoothing the denominator would be a better solution.

Regarding result 3)
This effect is not due to public investment (Table 5), which means it must be due to private investment. Can we check?
At a minimum, table 5 should show another 3 columns for the private investment regressions, so that we can check that this holds (and both should exclude the other type of investment to avoid identification issues; see more below). The absence of these regressions would otherwise raise the suspicion that the results are not robust.
The fact that private and public investment are substitutes is a serious identification problem in these regressions. But it cannot be used as an argument to run only public investment regressions and place private investment on the RHS (as done in table 5). One could in fact run the same regression switching the two variables and wish to interpret it as a private investment regression.
If the author does not want to solve the identification problem of which regression (private or public) is actually being estimated, the simplest solution would be to run separate regressions for both private and public investment regressions without the other investment in the regressions, and

acknowledge the introduction of an omitted variable bias. This would need to be followed by an inspection of the results and a discussion of the extent (and direction) to which the omitted variable bias would affect the results (on the basis of the correlations of the other investment with determinants).

Regarding result 4)
This result is not discussed in the conclusions or in the introduction. Also, the discussion of the interpretation of the result is inconclusive and incorrect. P.11 states that this result is consistent with the fact that dropping investment from the growth regressions reduces the coefficient. The latter result is not shown in the paper but other contributions have shown that dropping investment entails a small increase in the absolute value of the negative coefficient of debt on growth. As investment has a positive effect on growth, this means that debt must have a negative (not positive!) effect on investment. This is indeed the result that is normally found in the literature. Hence the author needs to acknowledge and discuss this difference.
Finally, the author claims that this result is consistent with the idea that "debt spurs public investment until a certain threshold, above which its positive effect vanishes" (p.11). Why?
Does the author believe result #4? If so, the author should discuss it more explicitly in the paper and reconcile this result with other results of this paper and of the existing literature. If not, the result should be dropped.

Overall, the paper needs to clarify the objective of the paper and the key results, and needs to spell out the key differences of the results with respect to the existing literature. And it needs to address the comments above.