

The editor asked me to write a report based on two questions: (i) Is the contribution of the paper potentially significant? and (ii) Is the analysis correct?

**(i) Is the contribution of the paper potentially significant?**

The authors start by suggesting on pp. 2-3 that: "The literature regarding "tax revenues-literacy" nexus is relatively poor. Whatever, there are several results in this direction. Some authors suggest that there is a significant connection between tax revenues and literacy level (Riezman and Slemrod, 1987; Ghura (1998); Book, 2003; Kenny and Winner, 2006; Kirchner et al., 2008; Mahdavi, 2008; Aidt and Jensen, 2009; Chaudry and Munir, 2010; Marti et al., 2010; Profeta and Scabrosetti, 2010; Aidt and Eterovic, 2011; and Dioda, 2012), while other researchers do not find any correlation in this way (Sartori, 2000)."

First, I am puzzled by the claim that the the entire literature is "poor" without substantiating this further. Further, as the authors review some of the paper on the list, it turns out that some of them are more about whether education and literacy affect the composition of tax revenues. Second, it is striking that only one study (the one by Sartori) is cited as finding evidence against the existence of a tax revenue-literacy correlation. In fact, there is from this list at best only one researcher who argues against the view that literacy levels and tax revenues are correlated. To my own surprise, Sartori does not produce a single correlation in a table or a graph as far as I can read from a book in italian. If I am right, this brings us down to zero researchers who produce empirical evidence against the tax revenue-literacy nexus.

Yet, while the authors declare the literature poor, they go on to test whether there is a relationship using a panel of countries. The authors do not bother to explain why they expect this relationship. This should have been easy given the extensive literature that they cite. Moreover, they never bother to explain the value added of their analysis as compared to the extensive list of papers that they mention in the quote above.

As frustrating as this might be for the present reader, I do think that the authors are using a relevant measure of literacy which may compare favourably to some of those used otherwise e.g. by Kenny and Winer (2006) and Aidt and Jensen (2009) who use more general education variables, and perhaps the authors could motivate their paper better if they stress their data more. A more significant contribution would be to look at how their literacy measure correlates e.g. with the education variables in Kenny and Winer (2006), and whether the same conclusions that these authors drew can be confirmed with a more direct measure of literacy.

As the paper stands it is not a significant contribution, though there may be potential in stressing the alternative measure used in the present paper

and how this relates to existing findings. Yet, this would not be a "big" contribution.

**(ii) Is the analysis correct?**

Essentially, the authors carry out panel regressions to understand the (partial) correlation between tax revenue and literacy. Unsurprisingly, they find that the simple linear relationship is positive. They also show some non-linearity which are potentially interesting, but they never provide an explanation of why the nonlinearities belong in the model. Also, why they include cubic terms in some specifications is poorly motivated. Thus, while the econometric model is reasonable for digging out the robust correlations, it remains unclear what we gain from this analysis pointing back to question (i) about the significance of the contribution.

Though, the analysis appears correct, it is poorly motivated and the positive relationship unsurprising..