

Comments on MS 3185:

"Composition of Taxes and Growth: Evidence from OECD Panel Data"

The paper attempts to replicate results reported by Kneller et al. (1999), with a dataset that covers more countries and more years. The reported results differ from Kneller et al. (1999), especially with respect to a reported negative growth effect of consumption taxes (which are held to be non-distortionary).

The paper uses theoretical arguments from Kneller et al. (1999) to motivate a growth effect of the composition of taxes. I do not find this motivation helpful. It is correct that the composition of taxes does affect the growth rate in models of endogenous growth, but these models usually predict a permanent growth effect of a change in policy variables, which appears questionable. It is also correct that the composition of taxes does not affect the steady state growth rate of the Solow model, but it should affect the transitional growth rate on the way to the steady state path. The latter appears to matter when using data for a relatively short time period, as in the present paper. Hence, it could be more reasonable to motivate the empirical analysis with reference to the possible transitional growth effects of the composition of taxes, which would allow for negative (transitional) effects of consumption taxes as well. As it turns out, the empirical specification employed in section 3 actually allows for transitional growth effects, which is difficult to reconcile with a model of endogenous growth.

p. 2

For an exact comparison with the results of Kneller et al. (1999), it would be helpful to use the same time dimension 1970-1995 as well. In the present paper, it remains unclear whether (unnoticed) differences in the specification of the regression equations may account for the different results (see below). The fiscal data used by Kneller et al. (1990) are from the IMF, Government Financial Statistics Yearbook. Are the fiscal data used by Kneller et al. (1999) for the 1970s no longer available? Footnote 1 (see also p. 4) is unclear in this respect, especially because the paper uses the same source for its 1980-2015 sample.

p. 3

The paragraph that aims to explain negative growth effects of consumption taxes is rather speculative and unrelated to the empirical part in section 3. It reads as an ex post rationalization due to the (unnecessary) motivation of the study with reference to models of endogenous growth.

p. 5

The empirical model (equation (1)) is not motivated by an explicit growth model. Hence, the estimated coefficients cannot be interpreted in terms of the parameters of a production function, but only in terms of statistical significance. It remains unclear whether equation (1) has also been used by Kneller et al. (1999), which is important to know for an exact comparison of estimation results.

p. 6

I am puzzled by the reported values for GDP per capita reported in Table 2. A mean value of \$29.95 cannot be right, and it cannot be a log value. Moreover, which version of PWT has been used? Which GDP series has been used to calculate the growth rate? Why has the investment share not been taken from PWT?

Tab. 3

It remains unclear why the number of observations differs if the number of countries is the same as in Kneller et al. (1999) (as stated) and the years are 1970-95 vs. 1980-2005. I do not understand the sentence "A one standard deviation increase in distortionary taxation by one percentage of GDP ...". Are the control variables, say, the investment share, entering the regression equation in logs? The last paragraph on p. 9 suggests that GDP per capita does not enter in logs. Such a specification would differ from the standard in growth empirics.

The paper only discusses the short run growth effects of taxation. The long-run effects could be calculated by taking the coefficient on lagged per capita income into account as well. Do the results suggest that investment does not matter for economic growth?

Maybe the main result of the paper, the presumably robust negative growth effect of consumption taxes, could be reported in one table with relevant alternative specification. The detailed variants of alternative specifications could be reported in appendix tables.

p. 9

I do not understand why the removal of the initial income term "collapses the basic regression to a simple form of growth accounting equation". A growth accounting equation would have growth rates on the RHS (while a development accounting equation would have a level variable on the LHS).

Tab. 10

In the context of growth regressions, the main problem with Diff GMM is that the instruments (levels) will be weak if they are time persistent. The trade share has not been instrumented? Has the number of instruments been limited?