Response to the comments on MS 3185:

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

Thanks the reviewer for this constructive and thoughtful report. I deeply appreciate the efforts and helpful comments from the reviewer which contributes significantly to improve this paper. Please find my response to reviewer point by point as shown below.

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.

Answer: Thanks for the suggestion. I will refer to the possible transitional growth effects to motivate the empirical analysis in order to allow for negative effects of consumption taxes.

р. 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.

Answer: Yes, ideally it would be better for an exact comparison with the results of Kneller et al. (1999) by using the same time period over 1970-1995. Unfortunately, when I check the data used by Kneller et al. (1999) in the IMF, Government Financial Statistics Yearbook, the fiscal data used by them for the 1970s are no longer available. Therefore, I incorporate the (nearest) available data starting from 1980 (that are available currently) in this paper.

р. З

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.

Answer: Theoretically, in contrast to the Barro (1990) model, expenditure taxes (whether constant or time-dependent) become distortionary and have a negative effect on growth when leisure is entering the utility function (in other words, labor supply is elastic) as in the Mendoza et al. (1997)

model. Therefore, this paper aims to use this theoretical model augmenting concurrent conditions of increased debt/GDP and globalization to explain negative growth effects of consumption taxes.

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.

Answer: This is a good point that I should claim before the equation. Specifically, I choose the same variables used in Kneller et al. (1999), which is also motivated by the theoretical model in section 2 in their paper.

р. 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?

Answer: I use the ratio of real GDP at constant 2011 national prices (in mil. 2011US\$) to total population as a proxy of GDP per capita (in 1000US\$). The data are taken from the PWT 9.0. In terms of investment ratios, I think both data (coming from PWT and WDI) are highly correlated.

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.

Answer: The reason why the number of observations differs if the number of countries is the same as in Kneller et al. (1999) is that some data are missing or what we say "unbalanced" panel data. The sentence "A one standard deviation increase in distortionary taxation by one percentage of GDP ..." is just a traditional way to indicate if the effect of key explanatory variable on the dependant variable is sizable. The investment share is entering the regression in the ratio of investment to GDP as in Kneller et al. (1999). The way that GDP per capita used in empirical analysis is also following the specification in Kneller et al. (1999).

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?

Answer: Yes, in the revision version I can discuss the long-run effects.

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.

Answer: Yes, in the revision version I can report in one table with relevant alternative specification, and the detailed variants of alternative specifications could be reported in appendix tables.

р. 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).

Answer: Yes, you are right. Easterly and Rebelo (1993) find that the significance of fiscal variables in their regressions is sensitive to the inclusion or otherwise of initial GDP. The removal of this term collapses Eq. (1) to a simple form of growth accounting equation. As in Table 5 in Kneller et al. (1999) this is to examine if the significance of fiscal variables in the growth regression is sensitive to this change in specification, and it shows not. If you think this is really a problem, I can remove it or leave it in appendix.

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?

Answer: Yes, you are right. The problem with difference GMM is that the instruments are weak. As a result, I think that this is why Kneller et al. (1999) do not incorporate this method. If you think this is a problem, I can remove it or leave it in appendix.