Review of ACPD MS acp-2016-1102-revised

"Sensitivity of the tropical stratospheric ozone response to the solar rotational cycle in observations and chemistry-climate model simulations" by R. Thiéblemont et al.

I have had time now to read through the revised manuscript and the responses to my earlier review. Publication can now be recommended after revision to make the following minor but important (in my opinion) remaining modifications.

(1) While the statement made in section 2.3 (p. 7) of the first manuscript ("We do not take into account the direct effect on heating rates generated by UV variations because previous modelling studies have already shown that the stratospheric ozone response to solar variations is almost entirely driven by the effects of UV changes on the photolysis rates, in particular the photolysis of molecular oxygen (Swartz et al, 2012).") has thankfully been removed, it has been replaced with the following statement (p. 8, lines 7-8 of the new manuscript): "Note also that on timescales of the 11yr cycle, Swartz et al. (2012) found that their photolysis-only simulation captured almost all of the solar cycle effect on ozone." This statement is also untrue in the upper stratosphere. It appears to be based on a statement in the Swartz et al. paper (p. 5942, bottom of first column): "The photolysis-only simulation captures almost all of the solar cycle effect on ozone." However, these authors were probably referring in this sentence only to the ozone response in the lower stratosphere (below 10 hPa). Looking at their Figure 2, it is clear that the statement is not true in the middle and upper stratosphere, which is the main area of interest here. As mentioned in my first review, at 2 hPa, the ozone response is reduced by the direct heating effect by about 30%. At 1 hPa, the reduction is about 40%. So, please remove this sentence as it will lead to confusion on the part of readers who may be led to believe that temperature feedback effects on the ozone response can be neglected on all time scales.

(2) Despite the comparisons presented with the results of Sukhodolov et al. (2017), who used a CCM that included the direct effect of solar UV variations on the heating rates and the temperature response, it remains unclear to this reviewer that this component of the temperature response can be neglected when calculating the ozone response on the 27-day time scale. Although it appears that increased hydroxyl production is the main cause of the negative ozone phase lags in the present model simulations in the upper stratosphere, is the same true in other models (such as that employed by Sukhodolov et al.)? Models can differ in their photolysis and chemistry schemes as well as their radiation schemes. The only way to investigate this is to improve the radiation scheme of the LMDz-Reprobus CCM and carry out sensitivity studies using both CCMs. Fortunately, there is already a statement in the summary section of the revised manuscript (p. 17, lines 26-27): "Nonetheless, we recognize that to quantify properly the impact of the neglected solar-induced temperature feedback on our results, additional CCM experiments including the direct radiative effect of UV on heating rate should be performed." What is missing in this last section, however, is a statement that such an improvement is necessary for ultimately applying the model to investigate the top-down sun-climate mechanism (noted in the Introduction), which is based on dynamical consequences of the upper stratospheric thermal response. There are apparently quite significant dynamical responses on the 27-day time scale that can have non-negligible tropospheric consequences (e.g., Hood [GRL, v. 43, p. 4066, 2016]). Any

hope of simulating the latter effects will depend on an accurate simulation of the direct UV forcing in the upper stratosphere. So please add such a statement.