

I have two main issues:

First, the revised MS does not address my comments substantively. The revised MS shows only a few significant changes. I do not consider that the structural problems in the MS raised in the review have been addressed - the focus is on model response but there is not enough time spent quantifying at the process level the impact of DA. Second, there's been no restructuring to tighten up the focus to the subject mentioned in the title, and so this remains a paper where the apparent subject of the paper is lost in the details of comparison between model and observations and the simple sensitivity studies that would give a much better process-level understanding of the impacts of DA of SM on model performance are not there.

In lots of places the discussion in the manuscript remains rather qualitative. The revised MS spends too much time discussing factors not included in the study (e.g. L532-538, discussion of possible modifications/missing processes in O₃ deposition), and too little time is spent on discussion of the model itself (e.g. L529-L530 which is all that's said on the results regarding O₃ deposition). I'd suggest removing as much as possible of this speculation about model inadequacies that does not add to the interpretation of model response, and refocusing the MS by adding a separate detailed section on what processes are susceptible to modification by SM DA and on quantifying the change in these at the process level (that is, for instance, at the emissions level rather than on the impact on the very small change in ozone levels).

A MS that had a section on 'model responses to SM DA' moving figures S3 and S4 into the main text and then a section on 'comparison with observations' would allow the reader to assess better how the impact of SM DA propagates through to model skill in simulating ozone. Then add significant extra text to 'model responses to SM DA' section in the MS discussing these S3 and S4 figures and similar process level responses of model inputs or other parameterisations before considering the impact on O₃.

Consider L220 of the revision where extra text has been added. The revised discussion is not based around how the biogenic emissions varied with the assimilated SM conditions in this study, or even quantifies the difference. In L515 finally the authors state that the MEGAN emissions have no dependency on SM, but again 'do not anticipate' that emissions were changed. This is not a sufficient response to the review. Please insert a quantitative discussion as to how SM DA affected, and why, the biogenic VOC and NO emissions in your study.

Consider, as a second, but not final example, dry deposition, L240/L535 again does not make clear how the deposition velocity is affected by SM DA - is it solely via modification to surface temperature? Again, it would help to use the Wesely scheme to estimate the response of the deposition velocity to the temperature. This would help to understand the S4 figure panel on deposition. At present, deposition has only been addressed with extra text stating qualitatively how things might be (pg L221, L516, L535), but the calculations/diagnostics are not there. Again, what drives the change? Again, please add a quantitative discussion as to how SM DA affected deposition.

Similar comments pertain to my other major points where I asked for quantitative discussion. In a study such as this where you are trying to unpick how SM DA affects O₃, these process-level attributions are essential to the success of your study.