Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-499-RC2, 2020

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "Satellite soil moisture data assimilation impacts on modeling weather and ozone in the southeastern US – part I: an overview" *by* Min Huang et al.

Anonymous Referee #1

Received and published: 19 October 2020

This study addresses the impact of a more accurate treatment of soil moisture content on WRF-Chem simulations of some aspects of weather and atmospheric composition. The soil moisture of the NOAH land surface model is adjusted using a data assimilation technique to retrieved soil moisture content from the NASA SMAP radiometric measurements. The more accurate soil moisture then modifies moisture, heat and trace gas emissions from their 'base' values, and two WRF-Chem studies are compared for the period of August 2016, one being the base and the other including SM DA. Additional simulations are performed for 2013. Comparisons are between ground- and aircraft-based observations and modelled quantities.

This is a competent modelling study, and the authors have attempted to apply best practice in bringing SM DA and the application of WRF Chem to the study of the continental US. As such it complements, but doesn't much extend, an earlier study by the lead author in 2018. It therefore somewhat lacks novelty.

The SM DA is shown to improve model performance as compared to aircraft observations of air temperature and specific humidity, although there is no reported improvement against ground-based observations of temperature, humidity or wind speed.

For reactive gas phase composition, very small changes in O3 are calculated, with little effect of SM DA on modelled ozone aloft. Some degradation in model skill results, which the authors phrase as being less 'desirable'. I think this means they expect SM DA to improve model skill, but as it stands there are no reasons in the manuscript given.

The study concludes with the effect of including an updated emissions database on modelled ozone.

I would identify this as an interesting region/time period for study being a geographical region with a heterogeneous LULC environment results where there are multiple sampling of edge cases (regions of drought, regions close to field capacity) in the the vegetation modelling framework. .

My main issue with the MS is that it's something of a pot-boiler, and the problem under consideration is not clearly stated. The study is undermined by the majority of the discussion being rather qualitative, despite much quantitative information being in the paper's figures, and the discussion is often focussed on what was not included, rather highlighting the impact of SM DA on model performance.

Some important questions are raised, but no clear direction of travel for this work emerges and the no clear conclusions are drawn as to how and to what extent SM modifies the picture until we reach the concluding remarks. This diminishes its impact and I suggest that the focus of any revised submission should be on the process-level impacts of SM DA on e.g. emissions or deposition processes which result from the improved treatment of SM.

I say this because, at present, the authors bring up aspects of the modelling framework which are unsatisfactory or where the study itself could have been improved and some readers might be left wondering exactly what remains of the SM impact that has been included at the process level. Slightly frustratingly, there are long parts of the discussion on things that can't be addressed (340-345 irrigation. 346-347 rainfall product QC, 358-360 other models that might be used). L210 raises the question of how SM affects convection, but no discussion of the impact is given on e.g. vertical transport.

As a second example, on L167 the critically important aspect of the response of MEGAN to SM is raised, but, after saying that SM effects are not well understood, the discussion moves on, although figure 3 shows the modifications. The authors need to extend this section extensively to quantify the effect of SM on emissions in MEGAN, and to show how MEGAN responds (especially as these NO and isoprene emissions changes are important to the discussion of the ozone response to SM DA).

As a third, L187 raises SM-dependent vegetation properties which might be important to ozone deposition, but it is again not clear what the impact of these effects might be on e.g. ozone. These issues are raised again L379 and again in L396-403 with similar lack of clarity as to their impact. If these important effects can't be at present included, it seems to me that having raised these points repeatedly, the authors should at least estimate what the size of these effects might be on deposition velocity and hence ozone flux to the surface? The manuscript would be improved drastically if these important processes were discussed quantitatively - above, it would be good to know how big are the emissions changes. Here, how large would deposition velocities need to change to produce an effect on ozone?

As a fourth example, the study makes a point about how the signal from the use of different emissions datasets in terms of ozone response is large. This is a worthy point, but does not contribute to the question at hand, and the use of a second emissions dataset doesn't really improve the understanding of the problem. Similarly, the role of strat-trop transport is undoubtedly important, but again moves the discussion away from the SM DA. The reluctance to exclude anything, and to state which factors are dominant, makes the focus of the study very difficult to discern, and really detracts from the potential impact which is to understand how the WRF-Chem modelling framework is improved by SM DA in this mixed LULC environment.

The ozone response appears to be driven by temperature via the coupling of MEGAN to WRF meteorology. Here the manuscript is somewhat successful but this section also gives the clearest indication of how it could be improved. The authors could expand on the description of the results to drill down into the processes at work and how they interact. For instance, Figure 9 shows a very small change in ozone, which receives little comment, the authors preferring instead to concentrate on the maximum value and the correlation. The revised MS could look at regions of positive or negative ozone change, and say whether the small change in O3 is to be expected, or not, and give reasons for this, for instance by unpicking the contribution from emissions, deposition and temperature changes in Figure 3, and to present the results in more detail than is done in L389-392. This approach should be followed for the other aspects of the impact of SM DA on O3 and other atmospheric constituents.

In conclusion, I feel that the impact of the study would be improved if the focus could be narrowed, the depth of discussion improved and the connection of SM to the other inputs to WRF-Chem better quantified.

Specific comments:

L43: missing symbol between 70 ppbv

L55: mid-latitude cyclones are

L56: 'They are'

L138-139: ‘the major chemical species in the FT are’

L148: what do the authors mean by ‘runs’?

L180: rephrase ‘its major component surface resistance’

L307: shown to be consistent

L330: unusual use of dominantly

L533: sentence describing the impact is not clear

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-499, 2020.