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Atmospheric Chemistry and Physics
Discussions

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 #2

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This paper describes the impacts on the representation of meteorological variables and ozone in the southeastern US in WRF-Chem of assimilating soil moisture into the Noah land surface model. It demonstrates that soil moisture has an influence on these variables and provides a useful indication of the magnitude of the effects. The paper addresses an interesting topic, shows elements of novelty, is mostly of satisfactory quality and is within the defined scope of ACP.

My principal criticism is that while it is an interesting and competent description of a sensitivity experiment on soil moisture, with justification and explanation of results, it does not in its present form provide the analysis and deeper insight needed to sub-

stantially improve current understanding. This is largely because the focus is on the effects of assimilation rather than on the wider effects of soil moisture on the model atmosphere. This provides little new process understanding, may not be applicable to other models, and depends heavily on the performance of the underlying Noah land surface model, which is not explored in any detail here. While it is clear that this is an exploratory study, frequent statements in the results and discussion such as "future efforts should be devoted to..." and "... need further evaluation" point to topics that should have been explored more thoroughly here. This is particularly the case where key processes or feedbacks are acknowledged to be missing (e.g., soil moisture controls on VOC emissions from MEGAN, or on deposition processes and vegetation uptake).

The quality of the data assimilation needs to be assessed more thoroughly before the atmospheric impacts can be explored. If data assimilation of soil moisture has a large effect it suggests that there are either substantial biases in the Noah land surface model or major uncertainties in the retrieved values. This uncertainty needs to be summarized to aid the reader in interpreting the results.

Much of the paper is descriptive rather than analytic, and this needs to be addressed before the paper is suitable for publication. The methods section in particular is too long. The results section describes comparisons, supported by a large number of figures, but the explanations are largely speculative and provide little new insight into the governing processes. The comparison with aircraft observations is somewhat cursory, and given that the improvements may not be significant (although this is not assessed rigorously) then it is not clear what value the comparisons bring.

The sensitivity study on anthropogenic emissions (Section 3.5) does not fit well with the main focus of the study on soil moisture, and it is not clear why this was included. I would recommend removing this section and the associated comments in the conclusions (lines 538-542) which are of little relevance to data assimilation of soil moisture.

The paper concludes by investigating the impacts on the upper troposphere and potential effects downwind. While it is valuable to explore the wider implications of soil moisture assimilation, the effects on ozone are very small (less than 1 ppb) and are much less than the biases associated with poor representation of stratospheric contributions due to lack of upper boundary conditions. The value and significance of this comparison is therefore unclear. This should be established before the potential consequences for ozone over distant regions such as Europe is considered.

In summary, the paper needs some reformulation to bring out key messages. The weaknesses identified here could be addressed in a number of ways. A simple sensitivity study altering soil moisture uniformly across the domain could be very useful to confirm the impact on different processes (e.g., lightning, convection) and would allow a more authoritative interpretation of the complexity of varying biases associated with assimilation. Tightening the methods and results sections by replacing description with explanation or analysis would be helpful. Further specific comments and suggestions are included below.

The English language is acceptable but is awkward in places, and the text would certainly benefit from some polishing.

Specific Comments

Title: the paper addresses the impacts on meteorological variables, not on "weather" in a conventional sense, and the title should be adjusted to reflect this.

Abstract, line 17: "dense vegetation, complex terrain, unmodeled water use" These issues are included in the abstract, section 3.2 and conclusions but are results from previous work, not the outcome of analysis in the present study.

Abstract, lines 23-27: These two sentences should be rephrased. The focus needs to be on the importance of the processes rather than the importance of quantifying them, and accurate assessment of the SMDA impacts on model performance is less important than understanding the importance of correctly-represented SM.

Line 59: clearer phrasing is needed: trapping in the upper troposphere rather than anticyclones established there?

Line 65: Soil moisture has other influences on the atmosphere (e.g. indirectly through vegetation) so perhaps add "principally" or "most greatly" here.

Line 81: The term "semicoupled" is not meaningful, as it remains unclear which components are coupled and which are not. Is this a form of one-way coupling or a coupling of only some variables? A clear but concise description is needed to explain this to the reader.

Line 125: What is the justification for the bias correction described here, and how much impact does it have?

Line 167: If soil moisture influences are not well represented in Megan, will the responses to its assimilation be meaningful or useful? The effects are only indirect through other meteorological variables. Similarly, what are the consequences of the lack of VPD treatment in the deposition scheme? This is only briefly mentioned in the text at l.400.

Line 169: "curves" would be clearer as "vertical profiles"

Para 230: Are these observations published? If so, please provide citations.

Line 331: A table of model performance with and without DA is needed here to provide a stronger quantitative underpinning of this discussion.

Line 343-345: There is no clear signal from the assimilation of a bias associated with irrigation in the regions indicated; why is this? Is this difference swamped by other uncertainties, or is the effect washed out by the bias correction applied before assimilation?

Line 356-358: this explanation for model problems with evaporative fraction is vague and unconvincing!

Line 367: The impacts of the data assimilation on temperature and humidity are very small. Are these changes significant?

Fig 6 shows the observations, the model simulation and the impacts of assimilation. However, it does not show whether the base simulation matches the observations or whether the assimilation improves the model bias, and these are the two factors that the reader is most interested in! Some of this information is provided in Figure 7 on a temporal not vertical basis, but please reconsider which panels to show in Fig 6.

Line 405: It would be worth pointing out that these RMSE changes are positive and that model performance is less good with assimilation.

Line 426: The points made in this paragraph highlight compensating model errors for ozone, but the lack of any stratospheric influence in the WRF-Chem runs remains an issue to be addressed.

Line 451: lightning is mentioned in the abstract, conclusions and a number of places through the paper, but the effects are not quantified anywhere. Does the soil moisture assimilation have any significant effect on lightning NO emissions? If so, please quantify it.

Line 495: Evaluation against SEAC4RS observations is not thorough here. Assimilation "led to better model agreements" but no numbers are provided in support of this. Some indication of the biases or RMSE values are needed in the text or a table, or alternatively a scatter plot of simulations against observations should be added to Figure S5. While this attempt to put the results of the study in context is valuable, the comparison is not convincing, and the explanations are highly speculative.

Line 510: Improvements in T2/RH/WS in 50% of locations is not a convincing demonstration of the value of assimilation. The improvements in MDA8 against AQS and CASTNET (42%, 51%) are of very similar (negligible?) magnitude, but these details are omitted from the concluding discussion.

Fig S1: The panels in this figure are too small, please make them larger so that they are legible (as in Fig 1).

Typos and Minor Issues

The language needs substantial polishing, e.g., line 103 "of the used modeling system" better as "of the modeling system used". (and Line 341)

Line 111: acronym SRTM30 is not defined. Line 139: is -> are Line 340: better phrased more clearly without use of "unmodeled"

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