Author response to reviews

The authors appreciate ACP Editorial Office, Dr. Müller, and both Referees' continued efforts. Following Dr. Müller's guidance, we further improved the paper along the lines suggested by the Referees. The changes include adding "take-home" messages in the conclusions as well as revising related sentences in the abstract. Please see below our response (in blue) to Referee #2's remaining comments (in black), which are also in line with Referee #1's general suggestion regarding making "the most of the opportunity afforded to improve our understanding of soil moisture on air quality". Quoted text from the revised manuscript is *in italic*. A "tracked-changed" version of the manuscript is submitted together with this document.

Response to Report #1 by Referee #2

The authors have made changes to the manuscript to address the comments of both reviewers, although these are not particularly convincing. The paper still suffers from the key problem that I identified in my original review: that it is descriptive rather than analytic. It is a very competent report of the research performed, but it remains short on the type of new insight needed for a useful scientific paper. There is a lot of explanation and justification of model performance, but it is not clear what we learn from this. The topic is certainly of interest to the readership of ACP, and worthy of publication, but the reader is made to work hard to extract the key messages of the study.
The research covered by this paper aims to bring in new insights into the added value of assimilating satellite soil moisture data to modeling atmospheric conditions in a regional-scale coupled modeling system comprising the widely-used Noah land surface model. The "atmospheric conditions" here not only refer to weather fields (which have been topics of many previous studies) but also air quality related states and processes, as noted in Section 1 of the paper. Explanations and justification of the model behaviors are necessary for understanding these Noah-related results. They also help lead to the expectation that the value of soil moisture data assimilation would be larger in modeling systems where soil moisture-sensitive processes are more realistically represented, and other sources of model uncertainty are better addressed. Including key messages in the conclusions, as this referee suggested below, also help address this comment.

I requested a clear, overarching conclusion for the reader to take away. The response is a lengthy sentence in the abstract that lists uncertainties but provides little useful information to highlight the take-home messages. There are minor adjustments to detail in the conclusions, but these provide no overview of the bigger picture that might highlight the wider value of the study. This still needs to be resolved. The final sentence of the abstract advertises future work, but this appears to underscore the rather inconclusive nature of this study, which needs to stand on its own.
That added sentence has been broken down into three sentences and reworded. Now this part reads as: "*In the cases that the DA improved the modeled SM, weather fields and some O_3-related processes, its influences on the model's O_3 performance at various altitudes are not always as desirable. This is in part due to the uncertainty in the model's key chemical inputs, such as anthropogenic emissions, and the model representation of stratosphere-troposphere exchanges. This can also be attributable to shortcomings in model parameterizations (e.g., chemical mechanism, natural emission, photolysis and deposition schemes), including those related to representing water availability impacts.*"

The outcome from this work, based on the Noah land surface model, can stand on its own, although it's also noted that the value of soil moisture data assimilation would be larger in modeling systems where soil moisture-sensitive processes are more realistically represented and other sources of uncertainty of the model are better addressed. The concluding paragraph has been modified to more clearly highlight the key messages:

"*…It was demonstrated that, via changing the model's weather fields that drove its chemistry calculations online, the SM DA influenced various O_3-related processes, O_3 concentrations and exceedances modeled by WRF-Chem. In some locations/times, these influences were large and resulted in improved model performance. To further improve the modeled chemical fields via applying the SM DA at various scales, it is not only important to improve the model representations of anthropogenic emissions and trans-boundary transport, but also to address shortcomings in model parameterizations, e.g., to realistically reflect the impacts of water availability on biogenic emissions and dry deposition, and for longer simulations, to include O_3 damage to vegetation*…"

The descriptions in the text would still benefit from some polishing to make them clearer and thus more accessible to the reader. For example, on lines 287-8, the synoptic conditions and drought conditions "can be closely linked to" regional O3 variability. I assume the intended meaning here is "partly explain"? There are a number of places like this where some relationship is identified but where the direction or magnitude of the relationship isn't clear.
We have changed this text to "*partially explain the regional O_3 variability*".