**Response to RC1:**
**We would like to thank the reviewer for the time taken to read and comment on this manuscript. The comments have been very helpful to improve the manuscript. We will follow your suggestions in addressing these changes in the revised version. Please find below (in bold) our responses to the reviewer's comments.**

RC1
This paper describes an analysis of the seasonal effects of soil drying on ozone stomatal deposition and surface ozone concentrations. The analysis utilizes the CHIMERE chemical transport model coupled with WRF, DOS3E, and the NOAH soil models. Results show large changes in ozone deposition and surface ozone concentrations in Mediterranean climates in Europe. My main concern is the lack of discussion. The Results section is thin and should be supplemented with quantitative information not readily derived from the maps, for example, differences resulting from the different soil moisture scenarios. Critically missing is a Discussion section, or a combined Results and Discussion, describing the reasoning, importance, and context of the results. For example, the discussion of the change in model performance is just a few sentences long and is entirely descriptive.

RC1
Minor comments:
The manuscript should be edited for grammar and flow. There are numerous grammatical errors.

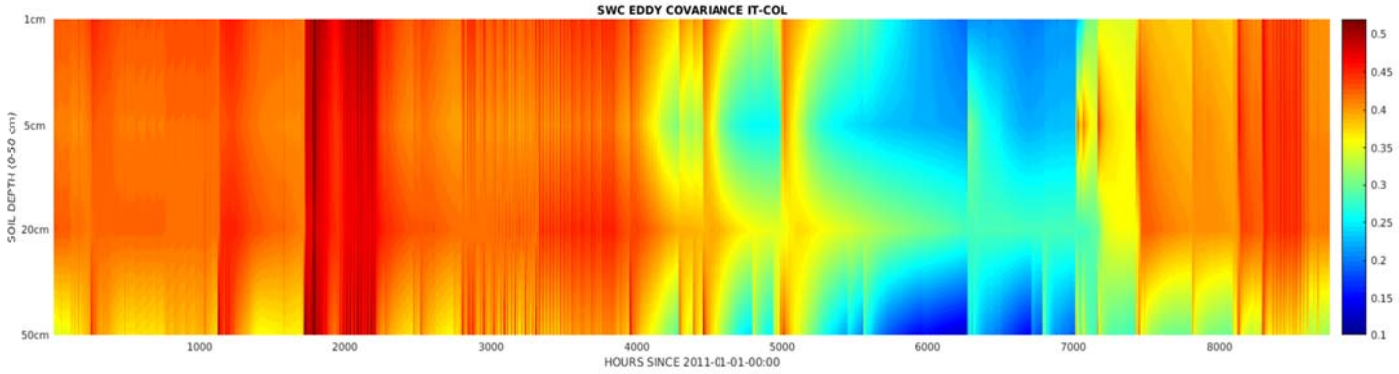
**AC1**
**We have corrected a few typo and grammar errors in the revised manuscript.**

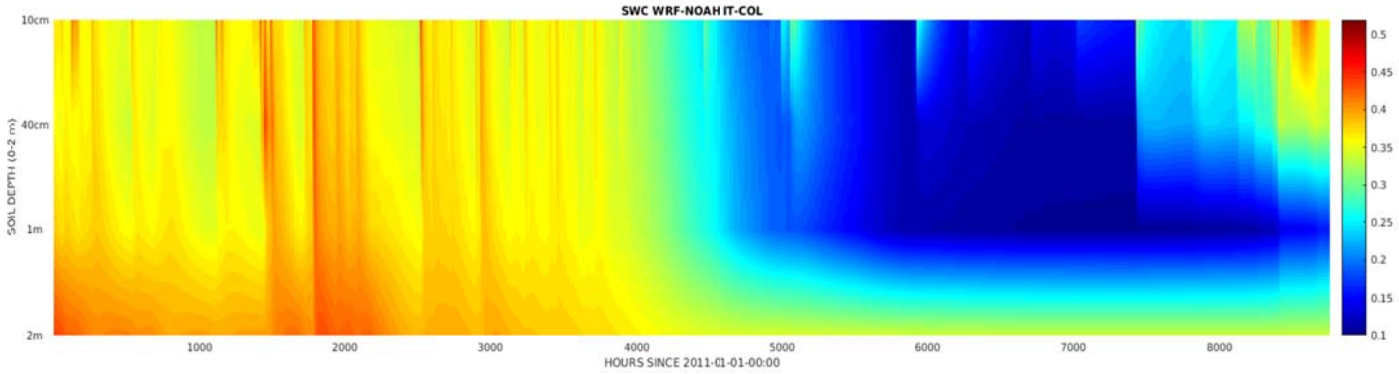
RC1
Figure 1: Increase the font size. The titles should be changed to be more easily understood. The color bars should be labeled.

**AC1**
**We increased the font size in Figure 1 and labeled the colorbar.**

RC1
Can you add measured data to Figure 1? I understand that soil moisture measurements are made at different soil depths than the depths where the simulations are done, but they should still agree qualitatively with the gradients.

**AC1**
**We fully agree that observed soil moisture data would help to understand whether the model reasonably reproduces the soil water; unfortunately, over the selected sites, soil water measurement are too shallow or layers do not coincide making thus the vertical interpolation and subsequent comparison very uncertain and confusing (see for instance the figure below).**



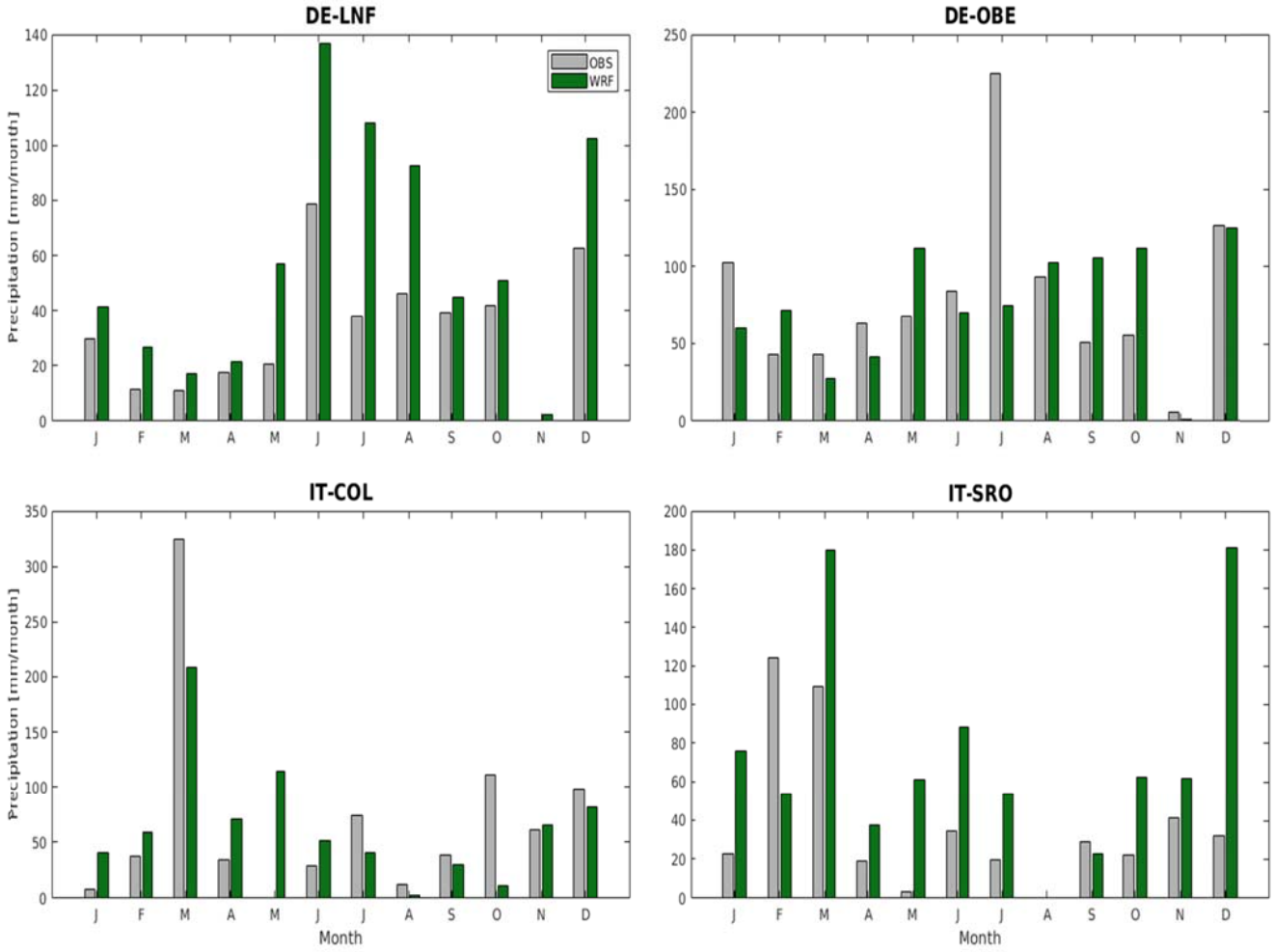

RC1

I find the model and measured precipitation correspondence difficult to discern. To my eye, it is easier to distinguish when the model and measurements do not agree. Is there some other way to represent the data? In the text, you state that the measurements are “well reproduced,” but on what timescale? Weekly? Seasonally? They do not appear to coincide day-to-day.

**AC1**

**We agree that the comparison of hourly data might be confusing and not easy to read, but we also believe that only showing high frequency data allows to fully understand how the water is distributed between the different soil layers as well as evaluate the offset between rainfall events and soil water. Nevertheless, in the figure below we present a more readable comparison between the simulated precipitation and the observations over the four analyzed sites.**

**Finally, we make more clear in the revised version that rainfall events refer to the validation of hourly data.**

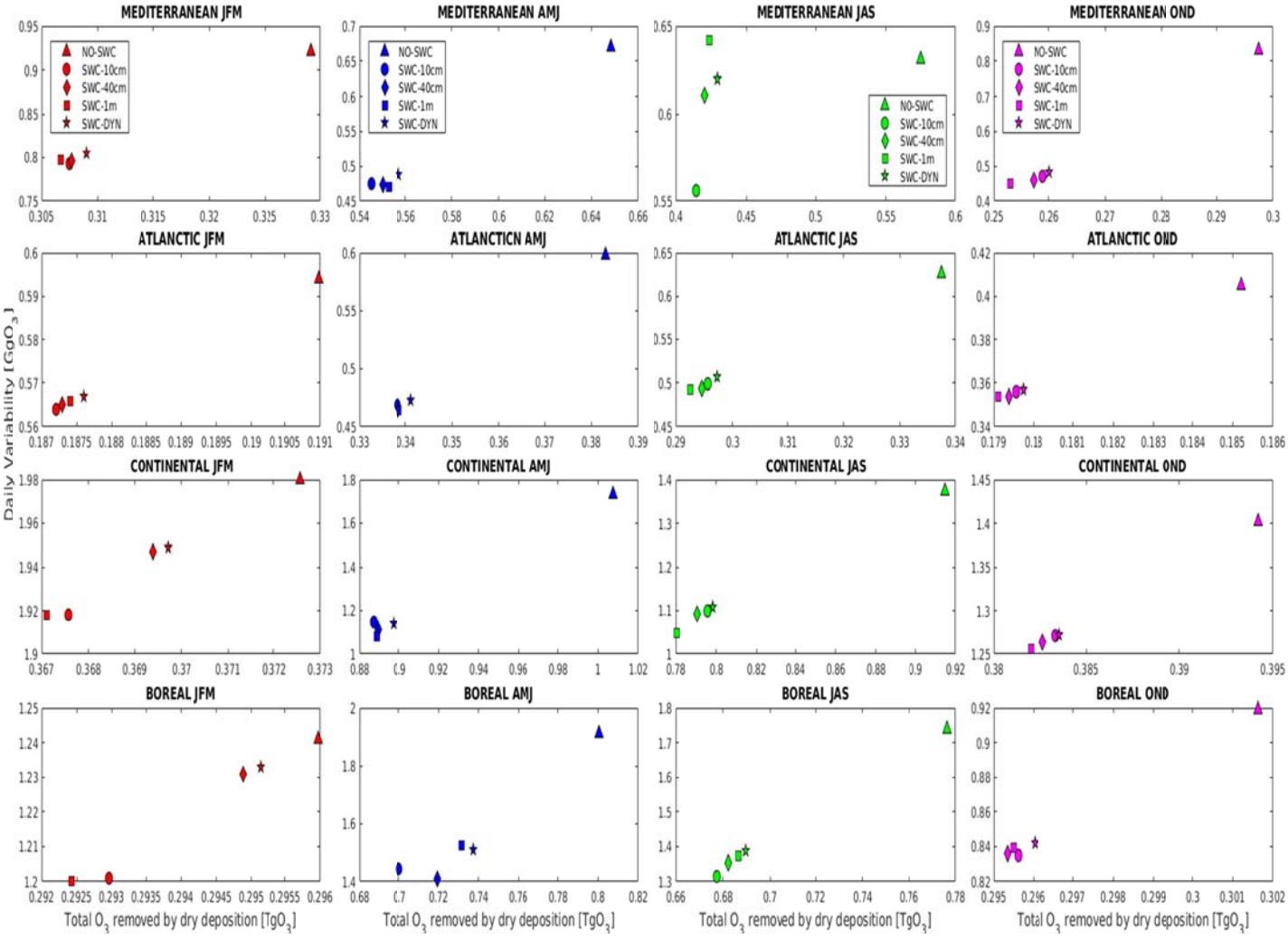



RC1
Is there another variable that can be added to the precipitation panels that makes it visually clear why precipitation does not coincide with soil moisture seasonally?

**AC1**
**Surely, evapotranspiration (or latent heat), runoff and snow cover would help to clarify the water dynamic into the soil; however we believe this analysis is out of the scope of this paper. In fact, the main aim here is to assess changes in atmospheric chemistry when different assumptions of water uptake by roots are used.**

RC1
Lines 342–346: The annual change across Europe is not a very interesting statistic. I recommend highlighting certain regions, especially the portion of Europe with a Mediterranean climate. Second, does the variability in deposition change, rather than just the mean?

**AC1**
**We fully agree the paper would benefit from the inclusion of a regional-based analysis; for this reason we aggregated seasonal data over climatic region derived from EEA dataset (http://discomap.eea.europa.eu/Services.aspx?agsID=9&fID=5477). This analysis allows to easily understand how mean and variability (i.e standard deviation) change between different simulations/regions/seasons. We have added the figure below and relative comments in the revised paper.**

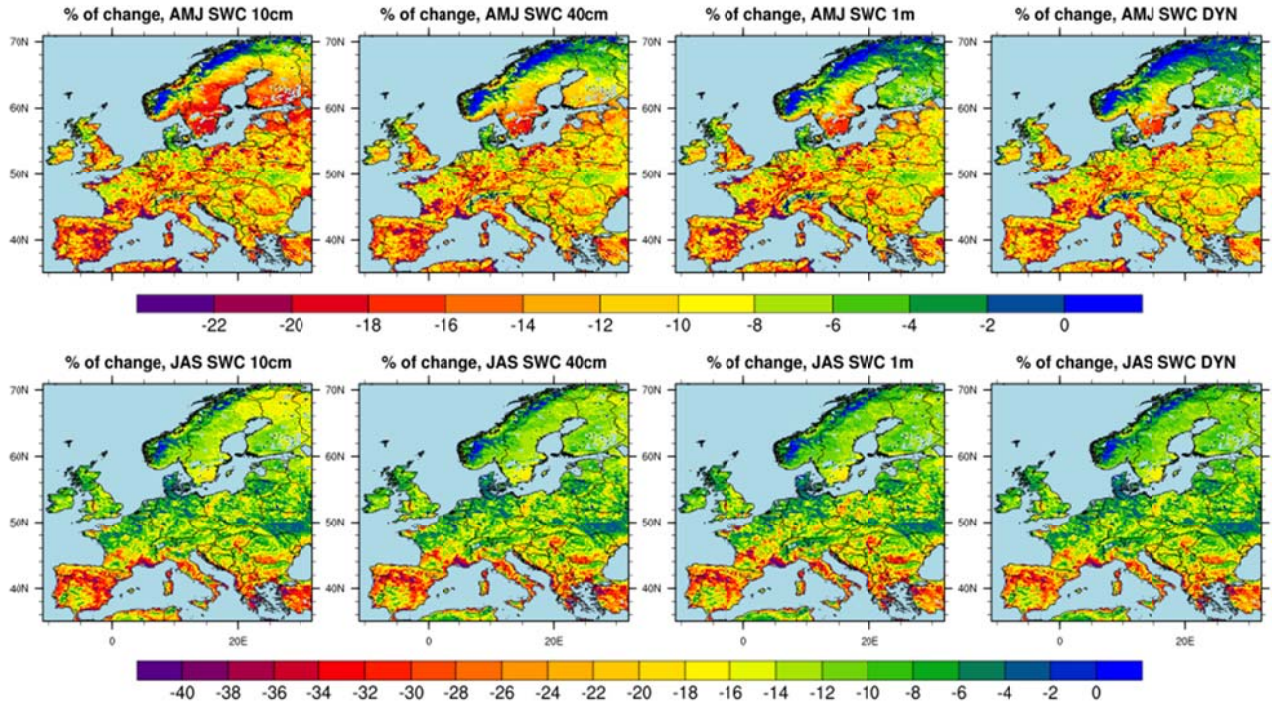



RC1

Figure 2: The color scale saturates over large regions of southern Europe. I'm curious to know how large the observed percent change was.

**AC1**

**Please find below the new figure 2 that we have added in the revised version of the paper; it shows, for both AMJ and JAS, the percentage of change without any cap in the colorbar.**




RC1
There is little to no discussion of whether ozone deposition and ozone concentration differences were observed between soil moisture schemes. These differences, if they exist, are not apparent to me from Figures 2 and 3. Results and discussion to this point should be added.

**AC1**
**Indeed, this discussion is shown in Figure 4 (and relative text in the paper); following also the next comment, we have broadened this discussion in the revised version of the manuscript.**

RC1
I find Figure 4 and the small portion accompanying text to be unconvincing and not useful. I recommend removing this piece of the analysis.

**AC1**
**We believe this figure is very useful for two reasons: 1) it clearly allows to quantify, in absolute units (i.e. not a percentage), the resulting changes in O3 concentration because of the different assumption in water uptake in the rooting zone, and 2) it shows how a process occurring within the soil affects also the concentration of gas in the upper troposphere (up to 650 hPa). According also to previous comment from reviewer, we decided to broaden this discussion in the revised paper.**

RC1
The text concerning changes to ozone measurements and model agreement should be clarified and expanded. It isn't clear to me what the authors are communicating.

**AC1**
**We have broadened this discussion in section 4 of the revised manuscript.**

RC1

Can the authors quantitatively contextualize the change in ozone concentration results in terms of the attainment of European ozone standards?

**AC1**

**We thank the reviewer for this suggestion; we have added in the revised document the figure below which shows the percentage of change in the European standards used to protect vegetation and human health from ozone (i.e. AOT40 and SOMO35, respectively). Results are very interesting as we find a relevant percentage of change, reaching even the 100% in some points. More details can be found in the revised manuscript.**

**Response to RC2:**
**We would like to thank the reviewer for the time taken to read and comment on this manuscript and the positive comments and opinion. Please find below our responses to the reviewer's comments.**

Anonymous Referee #2
RC2
This study investigated the impact of soil moisture on model predicted O3 dry deposition and concentration. This is a good effort in improving current approaches handling the dry deposition process in chemical transport models as well as in studies focusing on assessing O3 impact on vegetation. By including soil moisture effect in stomatal uptake modeling, O3 dry deposition would be reduced by about 10%. While such a difference is somewhat significant, it is much smaller than the known uncertainties in most dry deposition algorithms, which is typically on the order of a factor of 2. For example, Schwede et al. (2011, A.E., 45, 1337-1346) compared one American and one Canadian models used in major monitoring networks for O3 and other gaseous species, and Flechard et al. (2011, ACP, 11, 2703-2728) compared three European and one Canadian models for nitrogen species across the NitroEurope network. Both of these two studies suggested the differences between the commonly used dry deposition models (and thus the uncertainties in most models) being as large as a factor of 2 even on long-term average basis. In this circumstance, including soil moisture in some models may not improve the O3 prediction and may even increase the bias if the models are already biased low. This does not mean that sensitivity studies on soil moisture effects are not needed, but the existing known large bias should first be outlined, and the significance of the present study could then be elaborated. Some other specific comments are listed below.

RC2
1. Remove the introductory materials in the abstract and provide a more concise summary of the major findings.

**AC2**
**We shortened the introductory materials in the abstract as suggested.**

RC2
2. Simplify the discussion of the basic concepts (especially paragraphs 3-7 in this section), and add a brief discussion on the large uncertainties in the commonly used existing schemes (as outlined above).

**AC2**
**We thank the reviewer for suggesting Schwede et al. (2011) and Flechard et al. (2011) papers; we have added a discussion on the uncertainties of existing dry deposition schemes in section 4 of the revised manuscript.**

RC2
3. In Sections 3.2 and 3.3: where possible, first give a brief discussion on how well the original dry deposition scheme performed based on available literature so we would know if the revised version (by including soil moisture) would perform better or worse. This is important because the scientific community would depend on this finding to decide if additional effort is needed in generating soil moisture field and applying it in the dry deposition estimation.

**AC2**
**The comparison of model's performances was already given in section 3.4, thus readers can already easily understand if the modified model would perform better or worse; additionally, in section 3.4 of the revised manuscript we have broadened the discussions comparing our results with former**

**studies. Finally, in the last section we have broadened the discussion on the uncertainty of dry deposition, comparing this study with former publications.**

RC2
4. In section 4, on one hand, it is stated that the dry deposition scheme is improved; and on the other hand, the bias on the model predicted O3 concentration was increased. While it is possible that the increased bias in the predicted O3 concentration was due to the large uncertainties in the other physical and chemical processes in the model, it is also possible that the original dry deposition scheme was already biased low. In the latter case, the scheme is improved in terms of including more processes, but not for the overall predicted dry deposition. Some clarifications are needed here.

**AC2**
**Thanks for this suggestion, we fully agree that further clarifications are needed. For this reason, we have broadened this discussion in section 4 adding some clarifications and references.**