



EGUsphere, referee comment RC2
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Comment on egusphere-2022-222

Anonymous Referee #3

Referee comment on "Biogenic isoprene emissions, dry deposition velocity, and surface ozone concentration during summer droughts, heatwaves, and normal conditions in southwestern Europe" by Antoine Guion et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-222-RC2>, 2022

This manuscript focuses on describing WRF/CHIMERE simulations covering SW Europe in summer 2012-2014. In these simulations, the responses of MEGANv2.1 biogenic VOC emissions and dry deposition to heat and water stresses as well as biomass changes were represented differently. The study compares O₃ mass concentrations and exceedances, HCHO columns, biogenic isoprene emissions, dry deposition velocity and O₃ stomatal conductance from these various simulations during heatwaves, isolated droughts, combined droughts, and normal periods. In-situ observations of 2m temperature, O₃, and isoprene and satellite (OMI) HCHO column data were used for model evaluation.

This study addresses a topic that has become increasingly popular, and their modeling tools have not been used in previous works to address such a topic. More efforts were devoted to modifying the biogenic emission scheme. The paper falls within the scope of ACP/EGUsphere. Please see below my comments.

Paper structure:

-Introduction contains information that is not directly relevant to what this work addresses and should belong to discussions on the limitation of this work in the end, for example, monoterpene emissions, and the impacts of the drought phases on biogenic emissions of various species.

-Section 2 should include some information on NO₂ observations (chemiluminescence analyzer?) and their uncertainty used for model evaluation. Please consider moving L321 here.

-Section 2 should contain more information about isoprene observations from "different experiments" (methods, uncertainty) rather than simply providing a link. It seems that data from only one station (Ersa) were used? Can the location of this station also be indicated in a map?

-The authors should clarify that the E-OBS dataset mentioned in Section 2 (Cornes et al.) is a 0.25 degree gridded product which was regridded for model evaluation shown in Figure S5 (please confirm).

-Section 3.2, descriptions on MEGAN scheme (a process in CHIMERE) could better be merged into 3.3.

-Why isn't Section 3.3.4 (model validation results) a part of Section 4 (results)?

Modeling and analysis approach:

-Section 3.1.1: Physics schemes used in WRF simulations should be specified. How were these simulations initialized for atmosphere and land? Please also clarify: what was the reanalysis data used as lateral boundary conditions, ERA-Interim (L165) or a NCEP product (L627)? These can all strongly affect your WRF results. See: Huang et al. (<https://doi.org/10.5194/gmd-10-3085-2017>).

-I am confused about the vertical layers of WRF/CHIMERE, 46 layers (L164) or 15 layers (L241)?

-WRF-Noah has been mentioned a few times but it is not clear enough how this can be directly comparable with WRF-ORCHIDEE based analysis because Noah and ORCHIDEE are quite different land models and they may yield quite different soil moisture fields. In the gamma SM-emiss experiment why couldn't soil moisture from ORCHIDEE be used instead of Noah?

-I found that the busy box plots in Section 4 are slightly difficult to follow as the categories have overlaps, not clearly linking their results with the phases/severity of droughts and heatwaves, nor are they directly linked with previously shown time series plots.

-Figure S1: soil type USGS? Please double check. USGS seems to be the source of the land cover input.

-In Section 3.3.1, please consider discussing the drought impacts on biomass burning emissions because this pathway also affects O₃ variability. Was soil NO emission included, and if so, was it from MEGAN? Also the model chemical initial/boundary conditions and their quality should be mentioned. The model errors due to chemical initial/boundary conditions during stagnant and dynamic atmospheric conditions, which have connections with droughts and heatwaves, may be different.

-Section 3.3.2: How about nonstomatal terms, perhaps Wesely? The authors have recognized that there are other approaches in literature (including Lin et al. and Clifton et al. that have already been cited) to represent soil moisture and vegetation impacts on dry deposition, as well as its stomatal and nonstomatal terms. They should point out (e.g. at L410-415, when comparing their results with Lin et al.) that the choice of the dry deposition scheme can strongly affect one's findings. The authors may like to be aware that, based on the dry deposition scheme used here, Anav et al. (<https://doi.org/10.5194/acp-18-5747-2018>) have also quantified the soil moisture impacts on g_{sto} and ozone; and Huang et al. (<https://doi.org/10.5194/acp-22-7461-2022>) quantified the soil moisture impacts on dry deposition and ozone based on different schemes.

-L283-289 and Figure S3: Please clarify the sources of LAI. Specifically, is LAI interannual variability based on ORCHIDEE or some type of satellite data? Does "year dependant" mean summertime averaged or annually averaged for different years? Is the constant LAI used in dry deposition modeling from a climatological product, and if so, what is it? Could the model-based LAI be presented in maps and if possible, be evaluated?

-Figure S7: Definition of chemical regime parameter is not clear. Do low-NO_x regime and high-NO_x regime refer to NO_x limited and NO_x saturated regimes, respectively? What numbers are considered low and high, respectively? It's hard to find such information from the link provided in the figure caption.

-L425: "Surface O₃ remains high above the sea due to transport and the absence of dry deposition" - please confirm the (lack of) treatment of dry deposition over the water. How does this contribute to modeled O₃ errors over land via sea-land breezes and large-scale flows?

Uncertainty associated with their results and conclusions:

-As the authors acknowledged, uncertainty due to the outdated land cover and soil type (along with soil-type-dependent wilting point) inputs reduces the robustness of their results and that updated versions of input data are available. Many studies have assessed the impacts of land cover inputs (and LAI) on MEGAN biogenic emissions which may be cited. In the supplement, it'd be helpful to show a land cover input (of WRF/ORCHIDEE and MEGAN) map in comparison with MODIS to help understand the statements at L495-500.

-PLA(T2 and SD) is developed based on model results which are uncertain. While model absolute T2 values (which are not shown in maps) are evaluated with E-OBS, the performance is hard to be directly linked to PLA(T2). Also could PLA(T2 and SD) be evaluated against, for example, independent, widely used drought indicators/indexes (see <https://doi.org/10.1175/BAMS-D-20-0087.1> for some examples)? How may the uncertainty in your drought/heatwave classifications affect the conclusions?

-Model-OMI HCHO discrepancies look quite large, so are the model-obs isoprene discrepancies at one site. Can other satellite HCHO products be used to help determine the OMI HCHO uncertainty? Can more information be provided on data screening and the regridding approach (L146)? Also, note that modeled NO_x uncertainty can contribute to the model-OMI HCHO discrepancies. Can in-situ NO₂ bias that has been noted be corrected according to Lamsal et al., or modeled NO₂ columns be compared with satellite data?

Inaccurate language, statements needing more supporting evidence, typos, etc.

-Title and abstract:

The title does not accurately reflect the paper contents which cover normal periods as well. The focused model processes could be specified.

L3: met conditions not only modify photochemistry activity and vegetation states so this statement is not accurate. It also contradicts with L37 that met conditions also drive transport and the fact that met conditions drive biogenic emissions and dry deposition magnitude and variability that they study.

L4: "lack of interactions between biosphere and troposphere" is vague. It should be made clear whether you refer to the biosphere-atmosphere exchanges of water/energy that concern land/weather conditions, or the direct soil and vegetation controls on chemical processes such as biogenic emissions and dry deposition. This comment also applies to the "Interactions between the terrestrial biosphere and atmosphere" phrase in the title.

L10-11: key factor of what? Isn't biomass decrease a result of drought/heat stresses as well? Please consider rewording this sentence.

L17: "in agreement with HCHO satellite observations": this conclusion is questionable according to the results presented in the paper; "favourable" for what?

L21-24 does not reflect the highlights of this studies and may be removed or rewritten.

-L68: "because of" should be "partially because of"

-The connections between extreme weather and drought conditions in Section 1 are very nice and may be reorganized/sharpened with more citations from the land-atmosphere interaction communities being included, in terms of mechanisms, observation evidences and model capability/limitations, e.g., Miralles et al. <https://doi.org/10.1029/2012GL053703> ; <https://doi.org/10.1111/nyas.13912>; Hirschi et al. <https://doi.org/10.1038/ngeo1032> . The authors may want to note that land influences on atmosphere through evapotranspiration are included in their coupled WRF/ORCHIDEE systems and are not perturbed in this study.

-L294: Can this fitting function from Bonn et al. be written out here? For isoprene only?

-L326: uncertainty in PBLH is not supported by any sort of analysis. I am assuming that the authors meant to refer to the nighttime poor performance of PBLH that has been seen in many models. Please confirm.

-L332: change "norhtern" to "northern"

-L419-420: This sentence seems to be misplaced and should belong to the motivation of this study?

-L587: "as" is the wrong word?

-L588: PLA should be defined on its first occurrence in the paper.

-throughout the paper, many acronyms need to be defined on their first occurrences in the paper.