EGUsphere, referee comment RC1
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Comment on egusphere-2022-748

Anonymous Referee #1

Referee comment on "Updated isoprene and terpene emission factors for the Interactive BVOC (iBVOC) emission scheme in the United Kingdom Earth System Model (UKESM1.0) " by James Weber et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-748-RC1, 2023

SUMMARY AND GENERAL REMARKS

Biogenic Volatile Organic Compounds (BVOCs) play a key role in the composition of the atmosphere. They control the oxidizing capacity (abundance of OH) and contribute substatially to the formation of secondary organic aerosol (SOA). Therefore, acurate representation of BVOC chemistry and emissions in state-of-science Earth system models is imperative.

Weber et al. present a very comprehensive study of the inteactive BVOC emission model iBVOC in UKESM1. They identify a significant shortcoming in the selection of Emission Factors by mass (EFmass) for isoprene (IEFmass) and terpenes (TEFmass) for one particular Plant Funtional Type (PFT), namely C4 grasses. This leads to a massive overprediction of isoprene and terpene emissions from this PFT, while the global total budget remains relatively uneffected (compared to the literature). So, one could say right for the wrong reasons.

Weber et al. then analyse several alternative options and approaches of deriving improved EFs and evaluate the impact of their alternative EFs on emissions and atmospheric mole fractions of isoprene and terpenes. Some sensible reccomendations for fututure direction wrap up this very useful paper.

I think that this work represents a very valuable and useful contribution to the growing literature on the UK community Earth System Model UKESM. The authors have identified a significant shortcoming in the BVOC emission model and offer reasonable and useful alternatives. They also present a very detailed analysis of their suggested changes on UKESM, albeit limited mostly to the emissions and atmospheric abundance of BVOCs themselves. This is in my view where the paper falls a bit short of its potential. BVOCs pla

a key role in tropospheric ozone production, the atmospheric oxidising capacity (OH abundance), and the formation of secondary organic aerosol (SOA) with substatial impacts on the radiation budget. This is a bit of a missed opportunity I feel, although I concede freely that the amount of work required is likely massive and probaly beyond the scope of this paper. I hope that future work along the lines detailed above will follow.

With the above said I recommend the publication of this paper in GMD after the minor issues, which I will outline in the following, have been addressed.

SPECIFIC REMARKS

p1l12: "future changes in land use and land cover (LULC) and change."; redundant, please revise.

p2l50: citation "Cao et al., 2021" does not appear to be in the list of references.

p4ls99&100: please provide some refrences for JULES and UKCA.

p8l234: link; Is there perhaps another way of referencing these parameters (paper or technical documentation) that has a DOI? There is a potential risk that this link could break in future.

p11l333: reference to Table 3 should be to Table 4, I believe.

p11l349: "andorganic" should read "and organic"

p11l349: citation Mulcahy et al., 2020, doesn't appear to be in the list of references

p11l355,356: The first sentence in this paragraph seems a bit redundant, because it basically repeats what has been said in the previous paragraph.

p12l375: should read "timeseries **of** anthropogenic"

p12l379: citation Sellar et al., 2019, doesn't appear to be in the list of references

p13l392: should read "PFTs **as** discussed"

p15l366: should read "**are** at the lower end"

p15l367: "total emissions" is redundant

p15l498: "land use cover" --> either "land use" or "land cover" or "land use and land cover"

p16l495: "Fig2b" should be Fig3b, I believe

p16l502-504: how does this decrease of terpene emissions from needleleaf trees square with other models and observations, e.g., from Hyytialäa? Haven't needle leaf trees always been thought to be important for terpene emissions at high latitudes. Please comment here and, if appropriate, in the text.

p16l508-511: how much could the bias be due to differences in climate between sim and obs? are those sims nudged? I believe they are, but maybe a cooment on that fact may help. Even if nudged, there will be significant differences in climate. Important?

p17l530: The observations in are introduced without prior discussion. No references are offered. Please add references and discuss backgound a bit.

p17l335: I am not sure I understand the argument for introducing the scaling factor here. Isoprene emission are zero at night both at the observation sites and in iBVOC (there is a diurnal cycle in the model representing real world conditions). So, why is the scaling need - and what data is scaled (I presume the reference is to the model). Please elaborate.

p19l609-611: This sentence needs discombobulation and revision.

p20: Data and Code availability. I appreciate that there are certain restrictions on the UM code and thus sharing is very complicated. However, this is not the case for all the data

(and the code to process them), that heas been used in the plots and tables of this paper. All the data used to produce the plots and tablesas well as the code used to produce them must be made available publicly according to GMD policy. Ammend data availability section accordingly.

p27: reference Weber et al., 2021 --> needs to be updated to Weber et al., 2022, I believe.