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Atmospheric Chemistry and Physics
Discussions

Interactive comment on “Global biogenic volatile organic compound emissions in the ORCHIDEE and MEGAN models and sensitivity to key parameters” *by* P. Messina et al.

Anonymous Referee #2

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This is an interesting manuscript presenting an alternative global emission model to MEGAN as well as some insights into the mechanisms of such models. It is well suited to the journal and overall well written although grammar and language should be improved in some occasions. There are, however, a couple of open questions/problems that should be clarified along with a number of minor issues before accepting it for ACP.

First, I disagree with the argument that the current available information about Emission Factors is sufficient for statistical significance as stated in the introduction (P33971, L19). As far as I can see this is only valid for very few species while for many others only very few measurements can be found. The question is, however, if the available

EFs are sufficient to characterize the representative species for a PFT. Although the authors point out the difficulties of PFT parameterisation (and among these I miss the one that PFTs are of variable species composition) they are obviously of the opinion that they have overcome these difficulties. But how were these PFT specific EFs actually derived? All what is presented is Table 3 showing one EF per PFT and a list of references with varying detail. I would like to illustrate this point: In the ORCHIDEE model description, the authors say they have determined an isoprene emission factor of 0.5 for the boreal needleleaf deciduous PFT (=Larches) based on Levis et al. 03 (EF 0.0), Guenther et al. 06 (EF 0.7), Karl et al. (EF 0.0), Steinbrecher et al. 09 (EF 0.0), and Steinbrecher et al. 13 (only oaks in here). So how does this work out? One of the problems seems to be that only secondary sources are used which in turn partly use the same original investigations. It would be more logical to fall back on primary literature sources – preferably new ones or at least complemented by new ones (e.g. Ruuskanen et al. 07, Ghirardo et al. 2010). So, which measurements from which species were used to derive which PFT and how is it done? This is probably an issue for a supplement.

Second, I am a bit surprised that LAI is more or less stated to be wrong in ORCHIDEE already in 2011 (P33990, L1) but has not been improved since although the deviation to measurements is very large and it is discussed (and demonstrated) to be a very important driver for emission. There is a bit of discussion about uncertainties in measurements but I feel that the paper doesn’t dare to claim that the ORCHIDEE simulations are as valid as the MODIS derived values. However, if the MODIS data are considered ‘state of the art’, then I see three options to proceed: 1. Improve the LAI simulations, 2. Improve the argumentation to a degree that the reader can accept ORCHIDEE simulations as equally likely as MODIS data, or 3. Run all simulations with MODIS derived values only. Option 3 seems the most feasible to me.

Third, I would like to see a bit more model descriptions and information about setups. For ORCHIDEE, the activity factor is mentioned to depend on leaf age but it is not clear how it is derived and how it is different for different PFTs? It is not used in the comparisons of model simulations although it may pose a difference to MEGAN, particularly if it is decreasing the emission of PFTs with high leaf longevity. Furthermore, it is clear that drought and CO2 is changing in the simulations but it is not clear if one or both are considered for emission calculations. Regarding MEGAN, respective functions exist as options because emission is quite sensitive to both (e.g. Seco et al. 15, Acosta-Navarro et al. 14). With respect to the setup, I think that given the large differences in the PFT covered areas between the MEGAN and ORCHIDEE runs it would make sense running the models with each other's land-cover scheme to demonstrate the effect of this issue separately.

In addition, I would recommend avoiding repetitions throughout the manuscript (e.g. P33977 last paragraph, P33983 L18/19, P33996 last paragraph) and re-structure the analysis of LAI impacts, i.e. differentiating more clearly between the effect of size vs. dynamic and between emission area and light (and temperature) modifying impact (see also Keenan et al. 11). In this context, it is perhaps critical to state that some LAI are so large that there 'is no more light available' (P33992, L17). If this would be true, photosynthesis couldn't work and leaves wouldn't make any sense at all.

Literature referenced which is not in the paper:

Ghirardo A, Koch K, Taipale R, Zimmer I, Schnitzler J-P, Rinne J. 2010. Determination of de novo and pool emissions of terpenes from four common boreal/alpine trees by 13CO2 labelling and PTR-MS analysis. Plant, Cell & Environment, 33: 781-792.

Keenan T, Grote R, Sabaté S. 2011. Overlooking the canopy: The importance of canopy structure in scaling isoprenoid emissions from leaf to canopy. Ecological Modelling, 222: 737-747.

Ruuskanen TM, Hakola H, Kajos MK, Hellen H, Tarvainen V, Rinne J. 2007. Volatile organic compound emissions from Siberian larch. Atmospheric Environment, 41: 5807-5812.

Seco R, Karl T, Guenther A, Hosman KP, Pallardy SG, Gu L, Geron C, Harley P, Kim S. 2015. Ecosystem-scale VOC fluxes during an extreme drought in a broad-leaf temperate forest of the Missouri Ozarks (central USA). Global Change Biology, 21: 3657-3674.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 33967, 2015.