

Interactive comment on “Evaluation of regional isoprene emission factors and modeled fluxes in California” by Pawel K. Misztal et al.

Anonymous Referee #2

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Overview:

Based on airborne data from flight tracks over California, this study presents the evaluation of Basal Emission Factors, derived from measurements using different land-cover maps, and emission estimates, for the description of biogenic isoprene source. Land-covers considered in three different biogenic volatile organic compound emission models (BEIGIS, MEGAN 2.04 and MEGAN v.2.1) are considered for the calculation of Basal Emission Factors, and isoprene emission fluxes calculated by the CARB's hybrid model are evaluated.

This manuscript is well written and focuses on a very valuable and important work which totally falls in the scope of ACP topics. Biogenic source of volatile organic compounds are indeed still only crudely quantified, and model estimates associated with a

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high uncertainty. Only a few studies presenting model-data comparison at a regional scale have been published so far, and I therefore both enjoyed the originality and the scientific contribution of this study and appreciated the work performed. However I strongly believe there is still a room for improvement in the presentation of this work in order to clarify the methodological approach and to present a deeper analysis of some of the results which are only quickly described. Here are some feedbacks on your manuscript and suggestions for improvements that I would really like to be considered before publication in ACP.

General comments:

The positioning and central objective(s) of this work have to be clarified, and homogenized in the manuscript. It is stated at the beginning of section 3.3 that “The primary goal of the study was to verify the accuracy of isoprene emission estimates used by CARB (...) simulated by CARB's hybrid model”. And yet, very little space is eventually given in the manuscript to the evaluation of CARB's hybrid model results, especially if we do not consider the sensitivity tests for temperature, radiation and LAI, which to me are not really part of a model evaluation. This evaluation is presented as an independent work in the abstract, with only a few lines dedicated, while much more room is given to the BEF evaluation. All these different aspects of the work are really valuable and interesting to me, and are all worth being presented, but with the room entitled considering the main objectives given.

The agreement between measurements and model, regarding BEF and emissions, is somewhat overstated when described as “remarkable” (section 3.2.1) and “extremely good” (section 3.3). I agree that the main characteristic depicted by the measurements are generally well captured by the model (or model parameters i.e. BEF), which is already very encouraging considering the uncertainty in biogenic VOC emission estimates generally, but several emission peaks or BEF regional variability are still not captured by the model / model parameter. The comparison of isoprene fluxes simulated by CARB's hybrid model with measurements would really benefit from a deeper analysis:

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what are the possible explanations for model/data disagreement regarding peak simulation, for instance? The objectives of the sensitivity tests should also be clarified: is the range of variability used for temperature/radiation representing the range of variability observed in the field? Moreover, regarding BEF especially, plots, rather than regional maps, comparing the BEF values for the same location along the different flight tracks would make the comparison analysis more visible.

It is stated, in the introduction and conclusion especially, that biogenic VOCs play a key role in California regarding air quality. What is exactly the contribution of biogenic sources to VOC emissions in this region? Please add some quantification.

Introduction, page 3, line 4: please clarify and detail “as well as the preceding meteorological history”. Do you mean the past 24h or 10-days conditions for temperature and radiation, as taken into account in the MEGAN model?

Supplementary material and information are mentioned several times in the manuscript but unfortunately did not seem to be actually integrated in the supplementary document (which only presents 2 supplementary figures). So... either I missed a document well-hidden or some updates and corrections need to be performed. To properly understand the approach adopted for this work, it is indeed really important to find the information regarding “More methodological details” (page 8, line 15), “Further details on the application of the inverse algorithm” (page 8, line 28) and “Input variables tested” (page 9, line 21), supposed to be in a supplementary material, while Figure 8 does not seem to be part of this additional material anymore. Moreover the Figure S2, addressing the impact of fires on isoprene emissions, is indeed interesting but is not cited or used anywhere in the manuscript. I understand that supplementary information are not meant to be described in full details but at least should they be cited or used even shortly somewhere in the core of the manuscript, or deleted.

Many details, sometimes bringing a bit of confusion and that could be synthesized, are given regarding the different datasets used to build the landcover maps used by

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the different models, but some of the differences and specificities of the models, which can affect significantly isoprene emission estimates, are not given clearly. For instance how many vegetation categories are considered by each model? How is LAI considered (grid-average or for each vegetation type) and which year or climatology is used? Adding a table summarizing all these information, together with model spatial and temporal resolutions, would really help. The CARB's hybrid model is described in section 2.1.4 as an adaptation of MEGAN v.2.04 to include MEGAN v.2.1 enhancements. So what are the actual differences between CARB's hybrid model and MEGAN v.2.1?

Section 3.1 and Figure 2: Landcover is shown to be a critical driving variable and yet only the oak woodland distribution is illustrated. It would be really worth and important to show the landcovers for all the different vegetation categories as well, simplified if needed depending on the number of vegetation categories, for each of the models' landcover, and not only the calculated BEF distribution.

Specific comments:

Page 1, line 5: change “Basal Emission Factors (BEFs) distribution” to “Basal Emission Factor (BEF) distribution”.

Page 3, line 3: change “Large changes in temperatures” to “Large changes in temperature”.

Page 4, line 5: add “to” in “this can contribute TO uncertainties in isoprene emission estimates”.

Page 5, line 4: change “modeling with 1 km²” to “modeling with 1 km²”.

Page 5, line 28: change “MEGANv.2.1” to “MEGAN v.2.1”, adding space.

The long name MEGAN v.2.1 landcover v.2.2 used several times in the manuscript could be shortened.

Tables and Figures:

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Figure 1: The list of ecoregions is very long and hardly readable, and may not need to be presented in full details. This information should therefore either be simplified or enlarged/presented differently to be fully readable. If all these categories have to be presented, they could be listed in one table given in the supplementary material for instance.

Figure 2: Please check and correct the caption at this figure does not only give the “Landcovers used by the models”, as stated at the beginning of the sentence, but also BEFs. Please also detail what “dtiso+eiso” means.

Figure 3 and section 2.3.2: The optimal approach regarding temperature accuracy was found to be using the 4 x 4 km WRF model nudged by CALMET or CALMET directly. This result is not illustrated in Figure 3, and should be added.

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