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Discussions

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Interactive comment on "Isoprene emission potentials from European oak forests derived from canopy flux measurements: An assessment of uncertainties and inter-algorithm variability" *by* Ben Langford et al.

Anonymous Referee #2

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First, let me ask what you may call a philosophical question: should the editors allow the papers which are so innovative that they change the way we think about biogenic model algorithms? I definitely think so, because this paper not only is a great overview of biogenic models but also allows to understand how the biogenic modeling approaches work when challenged against real-world ecosystem data which can now be properly inverted to back calculate emission potentials for isoprene and maybe in the future for other compounds. While the algorithms started with leaf level measurements, the paper is another strong signal that the new generation models including process-

based models should be sufficiently flexible to also be capable of accepting regional parameter data. With the rising number of these ecosystem-scale measurements, this direction is well positioned to receive further refinement, improve the estimates, and most importantly to enhance process-based understanding at canopy scale. The paper is generally well written and seems incredibly useful for the biogenic flux measurement and modeling community. I would like to recommend its publication, although I do have a few rather minor comments/questions which hopefully can be addressed before publication.

General

1) The authors should try to avoid the confusion between the same parameters derived in a different way/scale/conditions. Alex's point to use the conditions closest to the standard conditions seems like another sensible approach worth evaluating. However, inverting the algorithm even at conditions significantly deviating from standard conditions seems still worth the exercise but must necessarily lead to larger errors from environmental parameters measured simultaneously, and potentially may become inconsistent with original model design or intent. Assuming the measured environmental parameters (e.g. T, PAR) are accurate, the value of inferring about the emission potential at different conditions seems valuable to assess how well the algorithmic activity factor works. If it works well, then the emission potential collected under the conditions close to standard should be similar to that inverted from fluxes measured at different conditions with reverse algorithm within the same footprint. For example, Figure 2 showing stable measured emission potential during the day is unbelievably encouraging, so this approach in my opinion deserves some greater attention.

2) Model parameters which were designed for the leaf level-scale may not always be compatible for comparison or extrapolation with the same parameter obtained from inverting the equation at the ecosystem scale, even if in principle it should work. For this reason, it would be helpful to use a thoughtful system of descriptors for equivalent parameters, e.g. EFcan or EFextrap, so it is clear and distinguishable how the parameter

was obtained. This will help the issue which the authors are trying to communicate to modelers (last paragraph in the abstract) that they should be careful about how these parameters were derived before using them.

3) The abstract seems somewhat heavy for a reader. The take-home message about the differences as large as a factor of four are somewhat scary and confusing. It asks for some further insight as to what exactly causes such a large difference. If you suggest the uncertainties in the inversion of the algorithms are different for different models is it because the inversion does not work perfectly or the specific algorithm does not work well for top-down inference about the emission potential (so would likely not be accurate the other way round – bottom up)? I suggest to focus in the abstract on the major points and progress, and less so much on what you did and technical detail.

Specific

4) P5 L33 G93 "Perhaps the most widely used" – did you mean the most highly cited?

5) P8 L4-5 Why did you leave out Langford et al. 2010 here? Misztal et al. (e.g. 2011, table 3) used approaches to estimate BER from the regression with measurements, as well as from the middle of the day (11:00 LT; which you show also here agrees well). I think you should add Langford et al., 2010 reference here, because they reported BERs as mid-day average. I would also suggest to be more neutral and refrain from subjective statements about which approaches are more popular.

6) Abstract. Seems long and overloaded. In particular the last two sentences are rather pessimistic and might agitate modelers unnecessarily, because it is hard to believe you could really be off by a factor of four if everything is done perfectly or at least it is not sufficiently clear why exactly this is the case.

7) In the concluding remarks, you focus on the way the emission potentials are derived. Do you also want to make a bigger point about how the future models could be enhanced to better assimilate observational data at regional scale?

C3

8) It is great that you include the original definition of emission factor (collected under the standard conditions and leaf scale). I wonder if it would be worth making a distinction between the parameterized algorithm on the full-canopy observations and whether it should be labeled as the same or a modified algorithm.

9) Table 1 – since PTR-TOF-MS was used in Castelporziano, why did you write vDEC? Did you artificially convert the PTRTOF dataset to disjunct to be consistent with other measurements? Either seems fine, as long as it is clear.

10) SI S1.1 Alice Holt – Measurement setup Lag time - as the signal to noise ratio for isoprene was rather very high, why did you use the approach for low signal to noise species? Why did you not use the accurate lag-time from each half hour period?

11) SI S1.1 "to ensure the reduced electric field strength" seems somewhat random and out of context. Also 2.01 mbar suggests that the pressure was stable to 0.01 mbar. This is rarely the case. I suggest you say 2.0 mbar or 2.01+/-0.XX mbar

12) SI S.1.1 P.1 L21-22 Instead of the justification it might be appropriate just to write what the consequences are (if any). I do not think it is necessarily bad to use high resolution measurement if it is appropriately post-averaged unless it leads to counting zeros. Otherwise, can you inform what the difference is between fluxes measured at 50 ms and averaged to 200 ms, as opposed to measured at 200 ms?

13) P8 L16-30. Unfortunately, I am extremely confused by the lack of clarity here. In particular, the weighted IEP is concerning. Why do you average the activity factor across the footprints and conditions before taking the ratio? It does not seem appropriate, because, as you say, these processes are nonlinear. For example, you have to use the model to average PAR accurately. It is more intuitive to average the emission potential, because in principle it should be relatively constant for the same vegetation type (as you show in Fig. 2), and you would not have to average nonlinear processes.

14) Sect. 2.4. Isoprene deposition. Given the large gradient it is interesting that the

C4

authors suggest the deposition can be significant even for isoprene. It would be helpful to provide the percentage range of isoprene deposition relative to total flux, in addition to canopy resistance. As Alex wisely points out, you need to be aware of epiphytic microbes whose role is not yet well understood in affecting emission and uptake of isoprene.

15) Sect. 2.6 how do you differentiate between the effective LAI and the tree cover area fraction?

16) P10 L28 As you did not calibrate isoprene on gas standard at Alice Holt, you had to estimate the concentration from relative transmission. I am generally fine with the approach, but it should be clear in the text whether you have accounted for isoprene fragmentation (mostly to m/z 41) because as you probably know isoprene sensitivity is significantly deviating from transmission estimate vs non-fragmenting species (e.g. MVK). Not accounting for fragmentation would result in underestimating the concentrations but perhaps you derived a fragmentation correction factor for proton reaction rate constant (effective k) in the post-campaign calibration? In either case it is not clear so you should add appropriate detail to SI.

17) Sect. 3.2 Figure 2a,c is incredibly super cool, and the diurnal emission potential seems relatively constant as expected, except for the morning and evening times. Did you try to filter for low u-star to see how this would affect the diel trend? Maybe you could plot the low ustar data in grey. Do you know why you could not reproduce this stability with G93 as beautifully as with G12?

18) Figure 7. This is also an incredibly beautiful figure. In particular the temperature activity factor works shockingly well. In panel a, it might also be useful to add the parameterized G06 line which would better fit the gamma for PAR. It would be nice to further discuss these differences because they show major results from this study.

Technical:
19) G93, G95, G06, G12 need to be defined on their first use and used consistently (e.g. G93 in the abstract).

20) add page numbers in SI

21) Sect. 2.1.1-2.1.5 Significant figures in the coordinates of the locations vary from 3 to 10. Please be consistent.

22) P6 L13 the unit of the gas constant seems incorrect. Maybe a typo or maybe you intended to refer to 1 mole.

References:
Langford, B., Misztal, P. K., Nemitz, E., Davison, B., Helfter, C., Pugh, T. A. M., MacKenzie, A. R., Lim, S. F., and Hewitt, C. N.: Fluxes and concentrations of volatile organic compounds from a South-East Asian tropical rainforest, Atmos. Chem. Phys., 10, 8391-8412, https://doi.org/10.5194/acp-10-8391-2010, 2010.

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