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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.

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This paper describes whole-canopy isoprene fluxes measured above five European forests, including both published and unpublished studies, and assesses approaches for using these measurements to parameterize the emission potential used in isoprene emission models and considers the associated uncertainties. This is an important activity that has received little attention. The paper also provides a brief mention of a community BVOC flux database that, as far as I know, has not been described in the scientific literature. The manuscript is well-written, the approach is generally valid and

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the manuscript provides a substantial scientific contribution with useful guidance for several important points. I recommend publication in Biogeosciences after addressing the following comments:

General comments

1) The most important comment that I have is that the authors should consider an approach that recognizes that measurements similar to the standard conditions should be used to determine the emission potential. I recognize that this may not result in the best estimate of the daily total emission or the emission at conditions dissimilar to the standard conditions but I would argue that is not the "job" of the emission potential. The emission potential, by definition, is the emission at specified standard conditions and so the best estimate is made by either selecting measurements within some fairly narrow range of conditions or perhaps weighting measurements by how close they are to the standard conditions. It is the "job" of the emission algorithm to go from the emission potential at standard conditions to the emission at other conditions. If the emission algorithm does not do a good job of this then there will be errors but you shouldn't bias the emission potential to try account for this. Instead you should work on developing a better emission algorithm. There will be relatively little difference in the emission potential calculated by different emission algorithms if the emission potential is based on measurements made under conditions similar to the standard conditions. Of course, the problem with applying this approach to canopy scale flux data is that we can't control the measurement conditions and there may not be any that are similar to the standard conditions. If that is the case then the emission potential should be reported for some standard condition that is within the range of the observed conditions. You could then leave it up to the developers/users of a given model to convert this to an emission potential for the standard conditions of their model. Looking at Figure 1, it appears that 3 of the sites would have some measurements at T=30, PPFD=1500 while T=25, PPFD=1500 might be appropriate for the other two. You could report the emission potential for T=25 as the measured emission potential and then also report

one or more calculated emission potentials for T=30 along with an exact specification of the model approach used to get there.

2) I assume that there is at least some landscape heterogeneity at some of these sites. The authors should consider binning measurements for different “footprints” associated with different wind directions that represent different oak fractions. This could provide “replicates” with emission potentials for a larger range of oak fraction values that may provide some insights into the value and variability in the leaf-level emission potential. Of course, this assumes that there is some information on the landscape heterogeneity at these sites.

Specific comments

Page 2, line 28: “In the Guenther algorithms, isoprene emission rates are modelled by assessing the emission potential”. This is not something specific to these algorithms- all isoprene emission models include some term of this type, although they may not call it an emission factor.

Page 4, line 31: delete “to” in “Castelporziano has to a Thermo-Mediterranean”

Page 6, line 18 and line 33: Be more specific about how the tendencies for studies to use a big leaf approach and using leaf temperature equals air temperature. For example, how many of the studies listed in line 16/17 do this? It may be useful to consider that at least one reason investigators do is because of the considerable effort involved in applying the full inverse canopy algorithm to their dataset and it would be useful to have an easier way to do this. For example, Yu et al. al 2017 (http://doi.org/10.1016/j.scitotenv.2017.03.262) calculated emission factors using an aircraft flux measurement dataset by using the single point version of MEGAN2.1 that you mention on page 7 line 9, and is relatively easy to use, and compare this with emission factors estimated using the regional MEGAN2.1 FORTRAN code, which is relatively difficult to use. A possible recommendation from your study is that BVOC emission modelers should provide a single point version of their code that can more easily be used to derive emission potentials from tower and aircraft flux data.

Page 8, line 32-36: The statement that "Measurements of the emission potential made using leaf-cuvette systems on a single leaf or branch gives a direct measurement of the isoprene emission rate that inherently excludes the deposition process." seems inconsistent with "but it may still be offset slightly as some of the isoprene may undergo dry deposition to leaf surfaces". The leaf cuvette measurement excludes deposition to other leaves and to the soil but there is the possibility of uptake by the emitting leaf including by phyllosphere microbes on the leaves.

Page 10, line 27: what is the basis for the 10% uncertainty assigned to species composition and 15% to LAI? Does this consider landscape heterogeneities and the uncertainty associated with differences in the LAI and species composition within the footprint of each measurement in comparison to the average for the whole area?

Page 10, line 34: The specific leaf mass that you use to convert canopy scale emission potentials to leaf-per-mass emission potentials are arguably as uncertain and variable as isoprene emission potentials. A 25% uncertainty for specific leaf mass may be a reasonable value but you should justify this number and mention how this makes it difficult to compare canopy and leaf scale measurements. This uncertainty could be eliminated if the investigators making leaf level measurements would report emissions as both "per mass" and "per area" leaf emission potentials (i.e., they should provide the specific leaf mass for each measurement) and I suggest that this be a recommendation. If some of the leaf level data that you refer to include measured specific leaf mass (and so direct measurements of per-area leaf emission potentials) then you should make this more direct comparison that does not suffer from the large uncertainties in specific leaf mass estimates (you could do this in addition to the comparison you have already made with the per-mass leaf emission potentials.

Page 11, line 29: Define what you mean by a "wide" range. The range given here of 6750 +/- 1150 is equivalent to +/- 17% which is well within the uncertainties that you discuss. Should that be considered a wide range?

Page 12, line 22: “regional or VOC global” should be “regional or global VOC”

Page 13, line 26: MEGAN2.1 allows users the option of using a constant value for each of the 15 PFTs but the recommended approach is to use the MEGAN2.1 isoprene emission factor map that accounts for the fraction of isoprene emitters in each landscape based on plant species composition and the species specific emission potential for each location.

Page 14, line 1: The MEGAN2.1 canopy-scale emission potential for high isoprene emitters is 24000 ug m-2 h-1. The global average temperate broadleaf deciduous tree PFT isoprene emission potential of 10000 thus represents a canopy composed of 41.6% high isoprene emitting trees which is high but not “primarily composed” as stated in the text.

Page 14, line 17-22: As is pointed out in this manuscript, canopy-average leaf-level PPFD values are considerably lower than above canopy values. Even sun leaves have a PPFD that is typically 50% or less than the above canopy PPFD since they are, on average, at an angle to the sun. The MEGAN2.1 standard condition for the past 24 and 240 hour PPFD refers to the leaf-level value and it is not appropriate (i.e., it just doesn’t work) to use the above-canopy value (i.e., a big-leaf model) with this equation (G06). That the G06 past 24/240 hour algorithm should not be used with the big leaf model is an important point to make in this paper but then going on to compare the MEGAN leaf-level PPFD standard condition with the measured above-canopy PPFD in figures S5 to S9 is comparing “apples and oranges” and may be confusing to some readers. It should be made clear that this is a comparison of two different parameters (above canopy PPFD and leaf-level canopy-average PPFD) and the main point is that the above-canopy value should not be used in the past 24/240 algorithm.

Page 14, line 28: Why not just conclude/recommend that the G06 algorithm should not be used with a big leaf model?

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Page 14, line 23 (and Figure 1 and Figure 7): Check on the values of PPFD shown for Castelporziano. They appear to be higher than what would be expected at the top to the atmosphere. Also, note that PAR should always be expressed in units of W/m2 while PPFD is the appropriate term when you use units of micromol/m2/s.

Page 14, line 37: This sentence is confusing.

Page 14, line 40: This may be because the Castelporziano PPFD solar radiation value is incorrect as mentioned above.

Page 15, line 4-6: or when they are measured under conditions similar to the standard conditions

Page 15, line 9: Leaf-level isoprene emission potential varies considerably between the top and bottom of the canopy and also depending on the past light and temperature environment. Are the leaf level emissions representative of the canopy average, as is the case for the canopy scale measurements, and is the past light and temperature similar? If this is not known, and it is often not reported for leaf-level studies, then this point could be included in the discussion of uncertainties for this comparison.

Page 15, line 23: As discussed above, an alternative approach is to select only measurements that are close to the standard conditions.

Page 16, line 37: This is an important point and a good opportunity for you to provide some recommendations for the standardization of flux measurements.

Figures 4-6: You were generally consistent in referring to “emission potentials” but these figures refer to “emission factors”. Either can be used but be consistent.

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