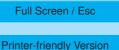**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Fluxes and concentrations of volatile organic compounds from a South-East Asian tropical rainforest" *by* B. Langford et al.

Anonymous Referee #2

Received and published: 3 June 2010

Overall

Collecting good BVOC flux data in the tropics is very difficult, and this paper makes a substantial addition to the literature. The authors do a thorough job putting their results into the context of previous measurements and the MEGAN model. On this basis, I recommend publication after the issues identified below are addressed. My most serious concern is that the dataset collected during the field campaign has been too finely parsed with the goal of producing independent papers. There are numerous citations to other manuscripts that are not yet available (in press), and the value of this dataset has been diminished. I feel the authors have left a lot on the table with the goal

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

of producing additional publications. In particular, there should be better synthesis of the VOC gradient data and the GC results. This point is relevant to the leaf-level data also. It's difficult to review the totality of this research contribution with so many in-press references.

A secondary issue is that more details of methodology should be included. This is emphasized in individual comments below. Because of the complexity of combining PTR-MS and eddy covariance in the tropics, it is essential that all these details are included. In particular, see the comment about the additive errors (page 11984).

NB: I prepared these comments before reviewing the other referee's comments.

Detailed comments (this includes both specific comments and typographical errors listed in order)

11976:22-25 Although these are both valid points, the inference is a bit off. I would not describe BVOC fluxes as a fraction of NEP. Ecosystems that are net carbon sources can also emit BVOCs. The accounting mentioned in the abstract (0.4% of assimilated carbon) is more relevant.

11977:14 Adding "In the presence of nitrogen oxides" before "Finally" might help readers that are unfamiliar with this concept.

11977:23 The following reference includes data from flux measurements in the Amazon basin: Müller, J.-F., T. Stavrakou, S. Wallens, I. De Smedt, M. Van Roozendael, M.J. Potosnak, J. Rinne, B. Munger, A. Goldstein and A.B. Guenther. 2008. Global isoprene emissions estimated using MEGAN, ECMWF analyses and a detailed canopy environment model. Atmospheric Chemistry and Physics, 8: 1329–1341.

11978:7 Again, Muller et al discusses this seasonality issue, backed up by some data.

11978:10 separate also benchmark

11979:1 Should just be degrees, not degrees C in lat/long.

11979:17 "north east" to "northeast"

11979:20 Give the flow rate and length of the 1/2 inch tubing.

11981:6 Since the standard with multiple compounds was available during OP3-III, you could compare the measured sensitivities to the Taipale technique and see if you were justified by its application during OP3-I.

11981:22 I think 2 degrees C is incorrect here. Maybe 200? Maybe a PDF issue?

11981:22–4 There is no discussion of potential atomic mass interferences. For example, furan, a product of biomass burning, has the same molecular weight as isoprene. The manuscript alludes to a GC, but only states briefly its use for compound identification. Here is a specific point where incorporation of another dataset is necessary for publication.

11981:24 Since line 22 has "summary," replace "summarized" in this sentence (or vice versa).

11981:25 – 11982:1 First, what did you use for your scalar concentration (X')? Was this procedure repeated for every mass? It should always be the same lag for every mass, correct? Unless some compounds were sticky in the tube? Next, I assume you lagged the cross-correlation function over the 25 seconds? I don't think that's clear. Finally, you need to be careful when doing a lag and max procedure (if that is what you did). If there is no flux (for example, isoprene at night), you will calculate a small positive flux since you're picking the max point. The procedure works great during the day when there are strong fluxes, and perhaps the quality assessment is accomplishing this same goal. If so, be explicit about that.

11982:17 The 90% rate is after the application of the u* filter described in the preceding paragraph, correct? If so, make clear? How much data were rejected by that filter? Please state.

11982:22 Move the left parenthesis to just before "Helfter."

11982:27 – 11983:6 Can you give a reason/speculate why this site is influenced by such low-frequency eddies?

11984:8 You first estimate that you might be losing 15–20% due to a measurement time that was too short (11983:6), and you then lose "<30%" due to condensation. These losses should be additive, correct? I don't want to be too critical, since you are being very thorough in your error analysis. But please address this point. Also, shouldn't the relatively low pressure in the sampling line reduce the effect of condensation?

11985:12 Give the details on your isoprene lifetime calculation. 40 minutes seems short, but maybe OH concentrations were assumed to be high? Is this number from Karl et al 2004?

11985:29 – 11986:2 Issue of splitting the publication of the GC results. This is another case where it is necessary to include at least enough GC results to confirm the mass identifications.

11986:17 Should be Figures 4a and 4b. Maybe more clear to make two separate figures? Your call.

11987: 12 Another instance where the dataset has been parsed very finely. The issue of the fluxes from the boundary layer breakup is interesting, and you have the information necessary (gradient data) to explore this further.

11988:14 "under storey" to "understorey"

11988:16 "rates" to "ratios"

11988:29 – 11989:8 Another instance where the dataset has been parsed very finely. Here, a significant amount of material is alluded to, and the paper has not appeared on APCD yet. Much of the value of this paper is the discrepancy between the measurements and MEGAN. The leaf-level data are an important piece of information, and the details are not available to assess their merits.

11989:9 Add Muller et al to the results in Table 3.

11990:8 Why was Tmax further constrained? Probably a good idea, but explicitly state the reason.

11990:9–10 Did Owen gather temp & light curves from enclosure measurements? This would be a very interesting comparison.

11990:20–21 There are two Hewitt et al., 2010a's and no 2010b. I assume this refers to the second one. Again, it's difficult to assess this paper with so many references to manuscripts in preparation.

11990:26 Because the x-axis contains the entire dataset (for phase III), it's difficult to detect these interesting discrepancies. I see the high values early on the Jul 9, but not on Jul 10.

11990:23 – 11991:11 While I agree with your reasoning here, you've alluded to other explanations which might explain some of the discrepancy. This reduces the clarity of your current discussion and makes the paragraph moot. Would the hypothetical variations in BER explain this discrepancy? Also, did Owen collect any leaf-level data that shed light on this issue? The current paper seems a bit of a teaser, and the rest of the story will come later.

11991:19–20 Be explicit about why this is "not unexpected" for MTs. Isoprene is also light dependent.

11991:24–26 Although a subtropical forest, it would be interesting to contrast your results to: Lerdau M, Keller M. 1997. Controls on isoprene emission from trees in a subtropical dry forest. Plant, Cell & Environment 20: 569–578.

11992:1–11 This is very important, but worthy of more discussion since it's a major conclusion of your paper. While I completely agree that more measurements are necessary, you should explore why your results are so different from the MEGAN value. Could inter-annual variability be responsible? You briefly mention seasonality in the

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Introduction, and you should return to that point here.

11993:10 Another reference to unpublished work. I'm not sure the flux data alone is sufficient to support the conclusion of this paragraph. I understand you hedge your bet with "suggests," but this is not enough. Also, you find a small net deposition, but is this significant? Because of non-linearities and potential flux loss associated with condensation, could the actual flux be positive? Make the paragraph more cautionary, and address the significance issue.

11993:26 Another reference to unpublished work. In this case, this point isn't central to the rest of the paper, so you can either add more information or leave for the story for the forthcoming publication.

11994:4–19 Since CO_2 fluxes were only measured during the day and only for a relatively small part of the year, any reference to a "net carbon sink strength" is problematic. I understand what you are saying, but the term usually implies temporal summation.

11995:6 What is the rationale behind the selected NO emission rate? The model results will be very sensitive to this parameter.

11995:4–18 I like the effort to carry your results a bit further with the modeling work. But I do have a couple of concerns. You cast this in terms of net carbon exchange, but it's difficult to get excited about 0.04%. Or even the 0.4% that you start with. Be specificâĂŤwhat is the relevance of this section? Just to say that BVOCs are trivial for carbon accounting? Second, are you accounting for biogenic secondary aerosol formation? Is that part of the dry or wet deposition? Do you assume that anything with a high vapor pressure leads to deposition? You should make this point more clear.

12007: Sesquiterpenes and estragole are listed in the table, but not mentioned elsewhere in the paper.

12010: DEC is not defined in the Table caption. vDEC is defined in the paper, so just tidy this up.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

12012: Add a sentence that briefly explains the difference between the x and y axes.

12017: Give more explanation on the bottom wind rose. Both normalized and raw fluxes are displayed, correct?

12018: It's difficult to get a lot of details out of this graph. It would be helpful to display these data as a scatter plot (measured vs. fitted).

12021: Here, you are plotted nighttime CO_2 data, but in the text you've stated the data are no good. Either remove it or add a cautionary note to the figure.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 11975, 2010.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper