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Biogeosciences Discussions

Interactive comment on "Leaf level emissions of volatile organic compounds (VOC) from some Amazonian and Mediterranean plants" *by* A. Bracho-Nunez et al.

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Reply to Anonymous Referee #2

Main comments:

REFEREE: In this manuscript, results are presented from two screening surveys of leaf-level biogenic volatile organic compound (BVOC) emissions from common plant species of Mediterranean and tropical ecosystems, measured emission rates are compared between plants and locations, and a compendium of somewhat-detailed BVOC emissions information for a wide variety of BVOC chemical species for ∼12-16 vegetation species (depending on the site) is given. In my opinion, a few major revisions and

some minor changes should be made prior to publication in BG.

AUTHORS: We greatly acknowledge the work and time the referee invested into this review. This support is encouraging us to intensify our work on this paper. We agree with the referee that there are some major improvements to be made prior to acceptance. Please find below our answers to the referee comments broken down to separate chapters.

REFEREE: The results sections are far too long (as is the discussion) and could be shortened considerably by moving detailed results presented as text into tabular format.

AUTHORS: We checked this proposal (which is repeated along several examples below) and we will rewrite and shorten along the relevant comments.

REFEREE: Figures 1-5 could also be removed and replaced with the data in tabular format, which would be more informative and useful to readers who might want use the emission rate data for quantitative purposes such as modeling/sensitivity studies. Additionally, this would shorten the text considerably because the authors would not have to go through and give fractional contributions to the compound classes in the text (as the authors do on lines 11-21 on P. 15292 and again from line 11-25, P. 15294, throughout section 3.2, etc.). These sections should instead only contain the most important revelations from the emission rate and compound ratio comparisons, such as the observation that MT profiles were diverse in the Mediterranean species and maybe highlight the most dominant chemical species detected.

AUTHORS: We principally favored the graphical solution, but we now agree with the referee to transform data into tables. We agree that doing this, the data will be easier usable by readers. Furthermore, rewriting with tables will shorten the text and make it at least not worse but even better to understand. We count on the understanding of the editor, that we need some time to perform this work.

REFEREE: One issue that needs to be properly addressed in the paper is the fact that, given the high biodiversity of tropical ecosystems, this study has probably screened only a tiny fraction of 1% of the total vegetation species present, which is why flux measurements are sometimes used instead to infer ecosystem-wide emission factors. The authors would do well by giving the reader a more accurate picture of what fraction of total Amazonian vegetation species were screened in this study rather than just briefly stating that “small numbers cannot lead to a final view but the results do indicate a trend and will at least improve data bases”, which sounds weak, especially if the authors want to stick with the current premise in the paper that emission inventories need to be improved.

AUTHORS: The referee touches a critical point of studies on primary emissions from some species within forest of extreme biodiversity. We will not be able to screen all tree species. Taking into account individuals with >10 cm diameter at breast height (dbh) the number of tree species/ha can be estimated at 250 (terra firme), 172 (Varzea) and 79 (Igapo) according to Wittmann et al. (2002, 2010, 2012) and Montero et al. (2012). In view of these numbers we screened more than 1 % but definitely not enough. We will briefly mention this point in the revised manuscript.

Montero, J.C., Piedade, M.T.F. and Wittmann, F. (2012) Floristic variation across 600 km of inundation forests (Igapo) along the Negro River, Central Amazonia. Hydrobiologia DOI 10.1007/s10750-012-1381-9.

Wittmann, F., Anhuf, D., and Junk, W.J. (2002) Tree species distribution and community structure of central Amazonian varzea forests by remote-sensing techniques. Journal of Tropical Ecology (2002) 18:805–820. DOI:10.1017/S0266467402002523.

Wittmann, F., J. Schöngart, and Junk, W.J. (2010) Phytogeography, species diversity, community structure and dynamics of Amazonian várzea forests. In Junk, W.J., M. T. F. Piedade, F. Wittmann, J. SchoÂÍngart & P. Parolin (eds), Ecology and management of Amazonian floodplain forests. Ecological Series 210, Springer, Berlin: 61–102.

Wittmann, F., Householder, E., Piedade, M.T.F., de Assis, R.L., Schöngart, J., Parolin,

P., and Junk, W.J. (2012) Habitat specifity, endemism and the neotropical distribution of Amazonian white-water floodplain trees. Ecography 35: 001–018, doi: 10.1111/j.1600-0587.2012.07723.x.

REFEREE: Instead, if the focus is on better understanding leaf-level processes, why didn’t the authors analyze the emissions data using the transpiration and gas-exchange information that was described in the methods? Very few BVOC emissions campaigns monitor transpiration and photosynthesis and I was excited to see that these parameters were monitored and then surprised that no further mention was made of the data, including presenting the data themselves or performing any analyses using this information. I get the impression that the authors went through a whole lot of effort to comprehensively measure not only emissions but also a lot of other important potential drivers of said emissions but then don’t show that anything was done with all this data. Given the length of the results section, this paper could include some great analyses but instead the results are just the figures translated into text format. If the prospect of performing a bunch of additional analyses sounds daunting, why not just remove the descriptions of transpiration and photosynthesis measurements from the methods. Without more rigorous analyses, this paper should be about half as long as it currently is.

AUTHORS: We agree. We were performing measurements of gas-exchange and transpiration in order just to see whether the plant behaved normally. However, the referee is right. This data should not kept locked. We are now working to include gas-exchange and transpiration data to give better information about the current state of the primary metabolism of the investigated saplings.

REFEREE: Also, not enough text is devoted to the statistical analyses, and these should be expanded.

AUTHORS: We will check for missing or incomplete statistical analysis.

REFEREE: In the discussion section, a lot of space is used making qualitative comparisons with literature-derived emissions. This is extremely lengthy and should be condensed, in my opinion there are numerous unnecessary comparisons made with other studies. It would also be nice to see some speculative discussion of the impacts of this study (e.g., a back-of-the-envelope estimate of BVOC fluxes made using this data and a bottoms-up approach and a comparison with flux-measurements or satellite-derived estimates of emissions strengths, or implications on regional chemistry through SOA formation, etc.) rather than just comparisons with other studies included in the discussion.

AUTHORS: We do not believe that or measurements should be exploited to estimate regional or even global emission strengths. This is something which might be performed with supporting modeling studies, which is definitely not the goal of our work. Nevertheless, we will discuss the screening data in view of improving global estimates and implication on atmospheric chemistry. However, we have to keep in mind that such PTR-MS studies even if accompanied by GC analysis will not be able to explain all atmospheric reactivity. OH sources and sinks in forests seem to be poorly understood and field campaigns have presented clear evidence for missing OH sinks (see Nölscher et al. 2012). Therefore, screening of tree species can contribute to understand these findings and to push more intensive investigations to identify ecotype related emission qualities. We will more discuss these issues in the revised ms.

Minor comments:

REFEREE: 1. Greater emphasis in the text should be made on the fact that potted saplings, and not mature naturally growing trees were used in the measurements and therefore the results may not reflect actual emissions.

AUTHORS: We thought this was sufficiently described, but we will improve mentioning this fact.

REFEREE: 2. Be consistent throughout the ms with respect to reporting not only the model type of equipment used in the study but also the manufacturer name and place

of manufacture. For example on P. 15286, line 25, only “V25” is given in reference to the control unit used during some of the measurements.

AUTHORS: Thanks, we will check that.

REFEREE: 3. P. 15290: The authors state that the phenomenological algorithm from Guenther (1993) was used to determine basal emission rates but it is unclear if compounds with both light- and temperature- dependent emissions (e.g. isoprene and some light-dependent MT) were normalized using that algorithm, which was developed for monoterpenes with temperature-only dependencies. The authors should be explicit about which algorithms were used to normalize emissions for which compound groups, and if isoprene and light-dependent monoterpenes were normalized this way, the authors should justify why this algorithm (and not others such as the light- and temperature-dependent algorithm developed for isoprene) was used.

AUTHORS: We agree that is a critical point. We always used the light and temperature driven algorithm. Most monoterpene emissions, except those from storage pools such as coniferous resin ducts or herbal glands, are light and temperature driven. There is no difference between isoprene and monoterpene emission. We thought this was written clearly enough in the discussion paper on page 15290 lines 15-21. But we will try to improve it now.

REFEREE: 4. The paper could be less wordy if somewhat clunky sentences such as (p. 15291, lines 12-15) “In the case of ten tropical plant species (Garcinia macrophylla, Hevea brasiliensis, He-vea guianensis, Hevea spruceana, Hura crepitans, Pachira insignis, Pseudobombax munguba, Scleronema micranthum, Vatairea guianensis and Zygia jurana), out of the twelve plant species screened, we were able to identify VOC emissions.” were shortened. How about, “VOC emissions were detected in ten of the twelve tropical species (Garcinia macrophylla, Hevea brasiliensis, He-vea guianensis, Hevea spruceana, Hura crepitans, Pachira insignis, Pseudobombax munguba, Scleronema micranthum, Vatairea guianensis and Zygia jurana) screened.”?

AUTHORS: We agree and will reword.

REFEREE: 5. Also on P. 15291, I'm curious why the authors chose emissions >10 mcg/g-1 hr-1 as the cutoff for species emitting isoprene to be considered high emitters? Something higher, like perhaps 50 might be a better threshold since high emitters such as oaks can easily emit >100 mcg/g-1hr-1. AUTHORS: Well, this is matter of discussion and feelings. Some oaks are emitting very high rates of isoprene. But if you go for monoterpenes, 10 might be a better threshold. We would like to point out the strong monoterpene emitter Fagus sylvatica, for example (Dindorf et al 2006). A general view is given by Monson et al. (2012).

Dindorf, T., Kuhn, U., Ganzeveld,L., Schebeske, G., Ciccioli,C., Holzke, C., Köble,R., Seufert, G., and Kesselmeier,J. (2006) Significant light and temperature dependent monoterpene emissions from European beech (Fagus sylvatica L.) and their potential impact on the European VOC budget. J. Geophys. Res., VOL. 111, (D16):16305. doi:10.1029/2005JD006751.

Monson RK, Jones RT, Rosenstiel TN, Schnitzler JP. (2012) Why only some plants emit isoprene. Plant Cell Environ. 2012 Sep 21. doi: 10.1111/pce.12015. [Epub ahead of print]

Referee: 6. In the Discussion (P. 15297) the authors state that plant species were chosen "more or less by chance" while in the abstract, it is stated that "common" species were selected. The authors should decide which case is accurate and stick with that.

AUTHORS: This may be a matter of a native language feeling? We used the term "common species" to describe the general distribution. But when going out to the field, the individuals were taken more or less by chance. We will rewrite.

REFEREE: Minor edits: 1. Page 15281, line 18: change "are" to "is" to read "the number : : : is limited" 2. Page 15282, line 3: place "Singh et al., 2001" in parenthesis 3. Page 15282, line 6: change "the missing" to "unmeasured" 4. Page 15298, lines

11-12, "Kesselmeier and Staudt, 1999" should be placed in parenthesis.

AUTHORS: Thanks! We will correct these mistakes.

Interactive comment on Biogeosciences Discuss., 9, 15279, 2012.