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Comment

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

Anonymous Referee #2

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Main comments:

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. The results sections are far too long (as is the discussion) and could be shortened considerably by moving detailed

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results presented as text into tabular format. 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.

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. 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

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

Minor comments: 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. 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. 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

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as the light- and temperature-dependent algorithm developed for isoprene) was used. 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.”? 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. 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.

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

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