

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 #4

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I've been asked to adjudicate between two conflicting reviews of this manuscript and “to provide some guidance to help decide if it has potential or if it is too flawed for publication.” In general, I find myself in agreement with both reviewers in finding numerous flaws in the original manuscript, which I won't reiterate in detail here. In general, the original manuscript provided far too much detail in textual form which could be more clearly and concisely presented in tabular form, as noted by both reviewers. The authors have indicated a willingness to comply with reviewer suggestions which should result in a far more compact and concise manuscript. The remaining issue, on which the two original reviewers were divided, is whether the information presented is sufficiently original and of sufficient significance, to justify publication.

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In essence, the manuscript consists of two separate screening exercises, conducted by two different research groups, using distinct (although overlapping) enclosure and analytical techniques, in two drastically contrasting ecosystems. Though both sets of measurements appear to be of high quality, differences in measurement technique (potted plants vs. plants growing in the ground, leaves vs. branches, GC-MS vs. GC-FID, differing detection limits) complicate efforts to draw comparisons between the two. Nevertheless, it appears as though the authors seek to enhance the potential significance of their individual data sets by comparing the two systems (“... the aim ... was to achieve a description of VOC emissions from poorly described tropical vegetation to be compared with the quite well investigated ... emission from Mediterranean vegetation”). Judged on this criterion, I'm afraid the manuscript fails. Granted that emissions from tropical vegetation are woefully underrepresented, characterization of emissions from 12 Amazonian spp, some of which have already been characterized and 3 of which are in the genus *Hevea*, advances the goal of characterizing tropical emissions hardly at all. And, can we justify further characterization of the ‘quite well investigated’ Mediterranean flora? Finally, what exactly is to be gained by comparing the two? Aside from the extreme contrast in ecological situation, we end up comparing 12 trees from Brazil with 7 trees, 1 palm, several shrubs, herbs and grasses from the Mediterranean. They're different. So what. Having said that, I do find it interesting that the diversity of monoterpenes is apparently so low in the Amazon; this is certainly worthy of mention and further study.

If one rejects the proposed comparison between ecosystems, as I do, then one is left with two fairly small screening studies. Should they be published? On the one hand, one could argue that many of the species investigated, particularly in the Mediterranean ecosystem, have been studied previously. On the other, as argued by the authors, information characterizing emissions of low molecular weight oxygenated compounds and sesquiterpenes is rare, and new data is presented for both ecosystems. Furthermore, the authors present an exhaustive comparison with previously reported emission rates in the Mediterranean flora, and one is struck by the lack of consistency

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between their and previous measurements. Although 'quite well investigated', there is clearly much we do not understand about BVOC emissions from the Mediterranean area. Not too surprisingly, differences in the composition of monoterpenes were found, often quite large, but extremely large difference in emission rates were also found, and several cases reported in which taxa previously characterized as non-emitters were now found to emit (or vice-versa). One can come up with post facto explanations of why these discrepancies might exist (the authors provide several) but the fundamental question is whether the reported differences are, to at least some extent, the result of experimental errors— species identification, enclosure technique, analytical technique, etc.—or whether they are real, in which case the interesting and important question is why the emission characteristics of a given species vary so much in space and time. In either case, these inconsistencies surely represents a cautionary tale for all of us involved in characterizing leaf level BVOC emissions (q.v., Niinemets et al. 2001, *Biogeosciences* 8, 2209), and raise the nagging question of an appropriate sample size for characterizing the BVOC emission capacity for a given taxa. Without going into excruciating detail, the authors might choose to emphasize the differences between their and previous measurements and speculate further on the causes and/or importance.

I may be in a minority, but I agree with the authors that information obtained from careful leaf/branch level emission studies provides a crucial complement to data from tower- or aircraft-based flux studies, particularly with respect to reactive short-lived compounds. These screening studies are not terribly exciting, and are descriptive rather than hypothesis-driven science. Nevertheless, if we as a community are to improve models of BVOC emissions, these efforts remain an important component, and high quality data sets such as these deserve publication. Results should be presented as economically as possible, largely in tabular form, and discussion kept to a minimum, focusing on any truly novel findings. Two such aspects of this study might be the relative lack of diversity in monoterpenes speciation in the Amazon trees investigated, and the large inconsistencies between the Mediterranean data and data previously reported.

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A few additional comments, not mentioned by the previous reviewers, or areas in which I disagree with them.

I too was struck by the fact that although detailed physiological measurements were apparently obtained in parallel with the emissions measurements, they were not discussed in the Results or Discussion sections. However, in contrast to the two reviewers, I don't recommend including these data in a revised manuscript. When conducting emission screening exercises, I too use photosynthesis and stomatal conductance data primarily to ensure that the plant is not obviously stressed. To report this physiological information would, in my opinion, contribute little and add length to an already bloated manuscript. To be clear, I wholeheartedly endorse the importance of including additional physiological data when studying the physiological controls over emissions, or investigating changes in emissions over time, but for a simple screening exercise, a simple statement that measured rates of photosynthesis, conductance and internal CO₂ either did or did not indicate the presence of significant stress should suffice.

In two places, the authors invoke plant age as a controlling factor in emissions of methanol and a possible reason for observed differences. The age of the plant however is largely irrelevant; young, expanding leaves are expected to emit methanol, regardless of the age of the plant.

(-)E-caryophyllene or B-caryophyllene?

Brachypodium frequently misspelled (Brachipodium)

With respect to identifying m/z73: if m/z73 were to represent methylglyoxal (secondary oxidation product) wouldn't you also expect to see MVK and/or methacrolein (primary oxidation products) ? For those spp. characterized by a large proportion of ocimenes, wouldn't you expect this to reflect a stress response?

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

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