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## ***Interactive comment on “Emissions of BVOC from Lodgepole Pine in response to Mountain Pine Beetle attack in high and low mortality forest stands” by T. R. Duhl et al.***

**Anonymous Referee #2**

Received and published: 21 August 2012

Duhl et al. measured BVOC emissions from pine trees at two sites in the Rocky Mountains. The goal of the study was to screen for BVOC emissions and determine if future investigations are warranted. The authors selected two sites: 1) Chimney Park Wyoming (CP), and 2) The Mountain Research Station (MRS) in Colorado. At each site, branch enclosures were used to concentrate samples of branch and needle emissions from trees at various stages of beetle attack. Samples were analyzed in a laboratory with gas chromatography. Multiple samples were taken from MRS at three different periods corresponding to different attack stages. The authors interpreted the results as differences between each site and speculate that trees at the MRS may be more resistant to beetle attack. The authors concluded that more measurements are indeed

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warranted to determine BVOC emissions from beetle attacked trees.

Because of the current epidemic of bark beetle infestations in the West, and the general lack of information of the response of BVOC emissions to attack, this article is a timely and important contribution. I found it to be well written, with pertinent references throughout. The experimental design, sampling methods, and analytical methods are appropriate.

My main concern with the article is the comparison of CP and MRS and subsequent interpretation of results. I assume that these sites are very different and have undergone very different beetle pressure. I am skeptical that the MRS trees have more heterogeneous emissions because of tree resistance to beetle pressure or blue stain fungus. Additional information in the discussion section would strengthen this aspect of the paper. As writing now, a few questions remain unanswered: Can the authors quantify that the sites had similar beetle pressure? For example, were number of pitch tubes counted on host trees? Were beetle flight traps used during attack? How confident are the authors in the baiting used at MRS matched actual beetle pressure at CP? These data may be available in the baiting paper referenced; however a more detailed description in this paper is needed. On the other hand, can the differences in MT emission be due to site-to-site variability, or another mechanism? What is known about MT emission variability among Lodgepole pines in different soil types, precipitation, and temperature regimes? If this is an open question to be answered in a follow up study, I suggest the authors remove references to MPB resistance throughout the paper. I also suggest that the authors add other possible explanations for site-to-site variability.

Minor comments / questions:

- 1) The authors could provide more detail on quantitative measures of tree selecting at MRS for attacked trees. As written, readers are referred to a separate paper; however, a few additional sentences in this paper would be helpful in my opinion.
- 2) How does tree to tree communication alter results at CP? Because the beetle attack

**BGD**

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was more severe at CP, could tree to tree communication result in homogeneous emission? Perhaps this is too speculative, but what would the critical measurements be to determine this?

3) What were the general meteorological conditions during each measurement period, and would site differences prior or during measurements impact emissions? For example, was cloud cover similar at each site?

4) Why was PRISM data selected instead of on-site measurements?

5) A table showing the level of mortality at each site / stand and its timing would be beneficial for comparing with other studies. As well as some description of beetle pressure.

6) Information on drawbacks and potential errors associated with using enclosures may be helpful for some readers.

7) The authors briefly discuss the Amin et al., 2012 study published in Environmental Science and Technology in terms of SOA potential. Can the authors also discuss potential reasons why Amin et al., 2012 found a clear increase in MT from the trunk of beetle attack trees, and a similar increase was not found in the branches of this study? Does this result from additional resin production in the truck of trees during attack, and perhaps leaf emissions may not be as significant?

Editorial comments:

1) page 9151, line 27: should this be “MPB resilience” not “MBP”?

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