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Interactive comment on “Tree water relations trigger monoterpene emissions from Scots pine stem during spring recovery” by A. Vanhatalo et al.

Anonymous Referee #2

Received and published: 9 July 2015

The manuscript of Vanhatalo et al. reports of a monoterpene emission burst induced in April in a Scotch pine tree following recovery from winter freezing. Although this manuscripts raised a potentially interesting ecological issue on VOC emission, it is based on measurements run in ONLY ONE TREE! Therefore, this manuscript provides just indications of an evidence occurred for two year consecutively in only one tree because more biological replicates are required to claim for a physiological mechanism. Indeed biological replicates of many (i.e. 3-5) trees are needed otherwise how the authors can be sure that such an event (springtime burst of monoterpenes) identify a mechanism occurring in all the trees or it is just an anomaly happening for some reasons in just this particular investigated tree? On the other hand, if the burst of

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monoterpenes measured by the authors regards (and occur in) all the Scots pine tree of a forest, a validation should be found also at canopy level with eddy covariance flux measurements. In addition, the authors refer many times throughout the text to either summertime or daily/hourly time-resolved measurements, although no summertime and daily/hourly time-resolved data have been shown. Besides, the authors discussed about the 'tree water relations' without showing evapotranspiration flux data that have been measured by the same device that measure the CO₂ exchange (as stated by the authors in line 18 page 4). Therefore, in order to make the manuscript acceptable for publication, I suggest the authors to add some more biological replicates and/or a validation of the monoterpenes burst through eddy covariance flux measurements, and to address my major (and minor) revisions listed below.

MAJOR REVISIONS - All the sub-paragraphs of the 'discussion' section look too much as an introduction or as a chapter for a textbook. I suggest the author to shorten the text and focus all these sub-paragraph more on the explanations that can be supported by the data shown in this manuscript. - Conclusion section must be dramatically reduced to a few sentences without citations. - Lines 24-25, page 9 and lines 26-27, page 14: no statistical treatment have been performed to evaluate to strength of the relationships between the dynamics of the different variables (i.e. ANOVA). - I suggest the authors to cut the Y-axis to enlarge the lower part of figure 3B; moreover, I suggest the authors to merge panel E, F of Fig. 3 to panel A, B of Fig. 1. - I suggest the author to remove Figure 6 as it is redundant, because the same information are shown already in Fig. 4C, D.

MINOR REVISIONS - Lines 12-15, page 1: the author should consider also VOC emission from the soil. - Line 15, page 1: the authors mentioned 'anomaly', but refer to what? How is defined the 'normality'? - Line 20, page 1: no 'transpiration' data have been shown (see my comments above). - Line 21, page 1: again, the authors mentioned 'unusual', 'anomalous', but refer to what? How is defined the 'normality'? - Line 27, page 1: '20-50%' in weight? - Line 28, page 1: '0.5%' in weight? - Lines 17-18,

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page 2: please add Loreto et al. PNAS (1996). - Lines 9-10, page 3: This is absolutely not enough! Because (in addition to what said above), the tree can be visibly healthy, but can have anomalies inside. . . - Line 22, page 4: please indicate which kind of gas standard. - Lines 24-26, page 4: either add a reference or report the formula of the exponential curve mentioned. - Lines 30-31, page 4: replace “molecular masses were measured” with “protonated mass ions were monitored”. - Lines 2-4, page 5: please add more details of PTR-MS calibration; has a gas standard been used? - Line 4, page 5: why ‘other’? - Line 7, page 5: please show the ‘temperature normalization equation’. - Line 12, page 5: ‘for this purpose’, but which one do the author mean? - Line 12, page 7: where these ‘maximum’ and ‘minimum’ data have been shown? - Lines 24-30, page 7: daily data have not been shown (see my Major revision above). - Lines 4-5, page 8: Why this should be a result? - Lines 13, page 8: how the authors can claim for ‘an acclimation response’? - Line 21, page 8: again, no data having such a time-resolution have been shown. - Lines 24, page 8: ‘consistently’? - Lines 13-15, 26-29 page 9 and line 1, 3 page 10: the authors keep referring to hourly, daily, summertime data not shown in this manuscript. - Lines 9-12, page 10: besides this sentence is puzzling, which are the ‘driving forces’? - Lines 12-15, page 10: how can the authors be so sure that ‘winter embolism’ occurred in this particular study case? - Lines 27-28, page 10: why ‘stem CO2 flux anomalies might be related to this phloem activity’ ? - Lines 10-11, page 11: delete this sentence. - Figure 4: why ‘VPD data of 2013’ are missing in Figure 4?

Interactive comment on Biogeosciences Discuss., 12, 7783, 2015.

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