Author's response on the comments by Anonymous Referee # 2 on
"Tree water relations trigger monoterpene emissions from Scots pine stem during spring recovery" by A. Vanhatalo et al.

We thank Referee # 2 for devoting the time to review our paper. We have done our best to response the comments (see below).

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 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.

Response: Thank you for this comment. We are well aware that our arguments would be better justified if we had data on more trees. While our measurement site is well instrumented and usually has replicate measurements, in this case we can unfortunately only present data from one tree. Since we use here many measured tree-scale parameters, the combination of all necessary measurements was available on only one tree at the time. However, despite of the lack of biological replicates, the measurements from two consecutive years show fairly similar features, and therefore we are confident that our observations are related to the intrinsic seasonal physiology of the pine trees and that they are significant in the springtime BVOC dynamics of the trees. We have tried to clarify this in the text.

Earlier at the same site, high monoterpene emissions from pine shoots have been observed in springtime (Hakola et al. 2006, Aalto et al. 2014). Comparison to the ecosystem scale flux measurements was done, but no clear correlation between ecosystem scale fluxes and chamber measured fluxes could be seen. Since the ecosystem scale measurements upscale the emissions of the whole stand, such transient physiological features related to emission changes in individual trees are easily lost in the measurement noise. As the environmental factors vary within the stand (most importantly melting of snow cover and sunfleck-related rapid temperature changes), and the tree individuals exhibit naturally somewhat different responses to these factors, this produces variance in timing of the physiological processes within the stand. Furthermore, the footprint of the ecosystem-scale flux measurements is large and within the footprint there are several tree species (Norway spruce, Common juniper and several broadleaved species) and also moister and drier site types. Moreover, since many monoterpenes are very rapidly reacting in the atmosphere especially

under spring conditions they may not be detectable above the canopy with eddy covariance or other micrometeorological measuring systems.

The referee is correct that transpiration data is not shown in figures though it is discussed shortly in the text. To attain good quality transpiration data the relative humidity of air must be rather low. In natural boreal conditions in spring this is not too often the case, and thus good quality data lacks in many cases, especially in night-time. This is why we show sap flow and VPD instead, which both reflect nicely how much a tree loses water to the atmosphere, also when relative humidity is high. The transpiration mention was removed from the abstract and it is mentioned once in the results section with a note that data is not showed in any figure.

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.

Response: Thanks, you have a good point here. However, as the paper is discussing a totally novel finding, we wanted to explore the most important physiological processes possibly affecting it. That is why we present some parallel explanations. We have now revised the Discussion to overcome such an interpretation, and condensed the text in order to concentrate on the most plausible issues.

Conclusion section must be dramatically reduced to a few sentences without citations.

Response: Revised according to suggestion.

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).

Response: Due to lack of replicates (as the referee correctly noticed) and the nature of the dataset showing phenomena occurring only in a specific time, it is impossible to do statistics to verify the results. However, as the phenomenon was repeated in two consecutive springs with similar timing and very similar environmental responses, we suggest that at least a close relationship between the monoterpene burst and the presented variables exists.

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.

Response: Fig 3B was revised. However, Fig 3 shows conditions inside the enclosure affecting emissions during the measurements, while Fig 1 shows the ambient conditions affecting the tree spring recovery at stand scale. Therefore we feel that the right place to enclosure temperature is in Fig 3, and have not merged the figures.

I suggest the author to remove Figure 6 as it is redundant, because the same information are shown already in Fig. 4C, D.

Response: It is true that the same data is partly presented in two figures, but figure 4 shows short-term dynamics, whereas the figure 6 points out the seasonal change until summer and the onset of sap flow around the time of monoterpene burst.

MINOR REVISIONS

Lines 12-15, page 1: the author should consider also VOC emission from the soil.
Response: During the time of observed monoterpene burst, the soil was covered with snow and there were no ongoing VOC measurements yet: the soil enclosures to study VOCs are installed only after snow melt. From earlier studies (e.g. Aaltonen et al. 2011, 2012) we know that snowpack hinders the emissions and once snow has melt, the soil emits VOCs at a rather high rate. However, we added the soil and understory vegetation as potential large sources for VOCs in the abstract.

Line 15, page 1: the authors mentioned 'anomaly', but refer to what? How is defined the 'normality'?
Response: Changed 'anomaly' to 'high emission rates'.

Line 20, page 1: no 'transpiration' data have been shown (see my comments above).
Response: This issue is discussed above.

Line 21, page 1: again, the authors mentioned 'unusual', 'anomalous', but refer to what? How is defined the 'normality'?
Response: We have revised this to 'non-systematic'.

Line 27, page 1: '20-50%' in weight?
Response: Yes, per weight basis. This was added.

Line 28, page 1: '0.5%' in weight?
Response: Yes, per weight basis. Text was revised.

Lines 17-18, page 2: please add Loreto et al. PNAS (1996).
Response: Added.

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…
Response: We added wording indicating potential biotic or abiotic damages as sources for high transient emissions. However, here we mainly refer to emissions that are not triggered by any external factors.

Line 22, page 4: please indicate which kind of gas standard.
Response: The following text was added: 'The replacement of the gas analysers did not cause any irregularity in the H_2O and CO_2 exchange data because the calculation of gas exchange is primarily dependent on concentration difference instead of absolute concentration. Both analysers were also calibrated for CO_2 using a comparable calibration method and standard gases containing ca. ambient concentration of CO_2. For more details on CO_2 and H_2O calibration protocol used at SMEAR II, see Keronen et al. (2014).'

Lines 24-26, page 4: either add a reference or report the formula of the exponential curve mentioned.
Response: The formulas for the exponential curves were added.

Lines 30-31, page 4: replace "molecular masses were measured" with "protonated mass ions were monitored".
Response: Replacement done.

Lines 2-4, page 5: please add more details of PTR-MS calibration; has a gas standard been used?
Response: Revised, and the following text added: 'A mixture of several VOCs (e.g. α-pinene as a representative of monoterpenes) in nitrogen was used as a gas standard. The mixture was further diluted with volatile-free air from a zero air generator to attain concentrations below 20 parts per billion by volume, i.e. around the ambient atmospheric concentrations.'

Line 4, page 5: why 'other'?
Response: Word 'other' removed from the text.

Line 7, page 5: please show the 'temperature normalization equation'.
Response: The equation was added to the text.

Line 12, page 5: 'for this purpose', but which one do the author mean?
Response: We mean that the chamber was tailored to study reactive gas fluxes from tree stems. The text is now specifying this.

Line 12, page 7: where these 'maximum' and 'minimum' data have been shown?
Response: The daily pattern of emission rates are shown in Figure 3A,B and Fig 4A, B. Reference to the figure 4 was added which led to change the order of the figures: previous figure 4 is now 5 and vice versa.

Lines 24-30, page 7: daily data have not been shown (see my Major revision above).
Response: The daily emission data is shown in Figure 4, and a reference to this figure has been added.

Lines 4-5, page 8: Why this should be a result?
Response: This was moved to the materials and methods section.

Lines 13, page 8: how the authors can claim for 'an acclimation response'?
Response: The text was revised and 'acclimation response' was left out.

Line 21, page 8: again, no data having such a time-resolution have been shown.
Response: The time lag approximation was removed from here.

Lines 24, page 8: 'consistantly'?
Response: Word removed as unnecessary.

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.
Response: The hourly resolved data is shown in Fig 5 a, b. Summer-time data on radial change fluctuations is not presented in this manuscript as the focus is in spring-time phenomenon.

Lines 9-12, page 10: besides this sentence is puzzling, which are the 'driving forces'?
Response: We have modified the sentence. 'Driving force' is transpiration.

Lines 12-15, page 10: how can the authors be so sure that ‘winter embolism’ occurred in this particular study case?

Response: We cannot be sure, but winter embolism formation is known to be a common occurrence in trees, including Scots pine, (e.g. Sperry, John S., and David J. Robson. "Xylem cavitation and freezing in conifers." *Conifer cold hardiness*. Springer Netherlands, 2001. 121–136). The text was edited and the reference added.

Lines 27-28, page 10: why ‘stem CO2 flux anomalies might be related to this phloem activity’?

Response: Because phloem takes part in embolism refilling. Refilling requires metabolic activity which should increase stem respiration and CO_2 efflux rate.

Lines 10-11, page 11: delete this sentence.

Response: Done.

Figure 4: why ‘VPD data of 2013’ are missing in Figure 4?

Response: Figure is supplemented with more VPD data.