Author's response on the comments by Anonymous Referee # 3 on
"Tree water relations trigger monoterpene emissions from Scots pine stem during spring recovery" by A. Vanhatalo et al.

We thank Referee # 3 for relevant and constructive comments. Below, we respond to them one-by-one.

The manuscript represents another interesting and useful contribution from one of the Finnish field sites, here the Scots pine ecosystem. It is a representative, heavily instrumented environment, and the new work is an innovative study relating plant seasonal dynamics to potential atmospheric impacts. The experiment is well described, and a series of relevant auxiliary measurements were conducted to aid in interpretations. The weaknesses of the study lie in the lack of reproduction (only one tree studied), and the associated speculation concerning the results and their drivers. Since this is likely ongoing work, I recommend including another season (spring 2014, 1015 ?) and possibly more trees into the manuscript when ready, and/or reduce the amount of speculation by focusing on the most likely reasons for the observations, clearly indicating what is known and what is speculation. The title should be changed accordingly.

Response: Thank you for this suggestion. We agree that our data is limited by the lack of biological replicates, but in our opinion it still offers a novel insight and important information on a previously unknown relation between VOCs and tree water transport. We have two years of data from the same tree, which gives us confidence that the phenomenon is not a measuring artefact or an anomaly of one time. Given that other published studies on this issue don't exist, we believe that this study could already with the present datasets serve as a key for other researchers to plan measurements confirming our results. In the future, we'll be able to confirm whether this phenomenon is a general feature of Scots pines.
The title was not changed.

Specific comments:
1. I agree that terpene emissions from tree stems of terpene-storing species is a worthwhile study subject. However, it should be more prominently compared to the other, presumably more relevant sources of terpenes to the atmosphere. The authors compare only to the dominant source of emissions, namely foliage. They should include the other identified sources that add/modulate the total terpene source: (i) leaf litter on the soil, (ii) herbivore impacts, and (iii) forest management, aka selective thinning/removing and harvesting. All these have been studied and published works exist. If the current study's findings indicate that stems are minor but significant sources, either via the demonstrated short-term effect or via all-year-round emissions, then this should be related to the other minor sources. The comparisons are, in my opinion, much more relevant here than the yet still speculative nature of the origin and drivers of stem emissions; future work could instead focus on the more relevant sources.

Response: We understand the question very well, and agree that the full analysis of VOC exchange between a forested ecosystem and atmosphere should include all potential sources, for example soil and leaf litter, and the biotic and abiotic disturbances, in addition to the traditional tree canopy approach. At our field site we have performed long-term measurements of different ecosystem components influencing the terpene budget at stand scale. We are also pioneering in the stem VOC exchange measurements under field conditions. This paper was narrowed to deal only with the springtime events in stem VOC exchange, in order to focus on the *in situ,* transient

physiological processes linking the monoterpene emissions and tree water transport. This was a conscious decision as otherwise the paper would have been expanded to several new aspects of field and the paper already utilizes lots of datasets. The seasonal cycle of stem-originating VOC emissions and their role in ecosystem-scale VOC budget will be discussed in other papers which we are currently working with.

2. Related to #1, above-canopy flux measurements, which have been done at this site in the past, should be included if available to put the observations into context.

Response: Comparison to the ecosystem scale flux measurements was done, but no clear correlation between ecosystem scale fluxes and chamber measured fluxes could be seen. This is most probably because of many reasons: the trees are individuals with different genomes and thus they show different reactions to environmental factors, which produces variance in timing and strength of the physiological processes. Moreover, though trees grow in the same forest stand, they experience slightly different environment: some get more light than others, at some points snow cover is thinner and melts faster etc. Summing up, the timing of the rather transient (about 12 h) monoterpene burst might vary within the forest and thus its effect cannot necessarily be seen in ecosystem-scale measurements. Furthermore, the footprint of the ecosystem-scale flux measurements is large and within the footprint there are several tree species and site types. Moreover, monoterpenes react in the atmosphere and may not be detectable any more above the canopy with eddy covariance or other micrometeorological measuring systems.

3. The results are well summarized but could be combined with the discussion, realizing that some of the discussion is speculative. Comparative evaluations should be considered when discussing the fluxes. The same units of flux should be maintained, and switching like on page 7791, first paragraph, is discouraged.

Response: We have revised the Discussion and believe that most of the speculative issues are now either removed or clarified. For comparison purposes, also the shoot flux data is presented. By showing fluxes per day we wanted to point out how the peaks affect cumulatively to the fluxes. Nevertheless, the paper aims to weight the relation between the fluxes and the water transport, not the absolute levels of fluxes or water transport.

4. The discussion part is where most issues are. I think most of the discussion in section 4 until page 7795 is reasonable. It stems from the observations and is related to what is known about tree physiology during that time of the season. However, I strongly encourage the authors to consider eliminating, or at least drastically shortening sections 4.1 to 4.3. Temperature is obviously not relevant in the burst other than triggering seasonal sap-flow recurrence / end of dormancy, section 4.2 appears entirely speculative, and section 4.3 is so at least in parts. I am not even sure that the offered explanations are exclusive of additional possibilities, such as, I speculate, the reallocation of monoterpenes through sap-flow from roots to other tissue in spring (which could have been tested by an additional enclosure lower on the tree stem). It appears to me that much more research is needed to evaluate the most likely source and drivers of the monoterpene burst, and I think the authors should be satisfied with having discovered it, and linked it to the physiological

changes the tree underwent as it recovered from winter dormancy. I thus recommend shortening section 4 appropriately.

Response: Since the manuscript is concentrating mainly on the unexpected high monoterpene emission peaks, we'd like to include a discussion of three potential mechanisms triggering them (Ch 4.1–4.3). In our opinion, this analysis is valuable in interpreting the role of different physical and physiological phenomena in the monoterpene emissions from the stems. With this analysis, we can rather confidently rule out a direct effect of temperature, which is the main driver for emissions from foliage during most of the year. Due to lack of detailed monoterpene concentration measurements inside the tracheids, it is impossible to reach a final conclusion, but at the moment the refilling of embolized tracheids seems to be the most likely cause for these high peaks.
We have revised and shortened the Discussion to remove speculation and to clarify the most important findings. Reallocation from roots was included as one potential (albeit unlikely) source.

5. The conclusions need to address the relevance of the discovery, such as for spring atmospheric BVOC emissions and/or spring herbivore vulnerability or attacks. If the study can be reproduced, a focus on this relevance maybe useful in improving study design and auxiliary measurements.

Response: Conclusions are revised to include this aspect as well.