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Interactive comment on “Ozone stress as a driving force of sesquiterpene emissions: a suggested parameterization” by E. Bourtsoukidis et al.

Anonymous Referee #3

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The authors describe the use of a branch enclosure technique to investigate how BVOC emission from a Norway spruce branch responds to environmental conditions and they describe the development of a parameterization to describe the observed responses. In particular, they observe elevated sesquiterpene emissions that are correlated with ozone. This is a topic that is of interest to readers of Biogeosciences.

The manuscript is generally well written but needs careful editing to correct a few spelling errors (e.g., page 12, line 27 *enchased* should be *enhanced*; page 13, line 10 *individual* should be *individually*; page 16, line 3 *responce* should be *responses*; page 16 ,line 12 “*found*” should be “*was found*”; etc). More importantly, there are some issues with references that don’t seem to say what the authors imply. For example, page 3, line 14 states that Heiden et al. found that “mainly SQT” was induced by ozone

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but Heiden et al. says tobacco plants increased their SQT emissions while pine trees increased MT emissions. On page 3, line 29 the text states that Penuelas and Staudt report elevated SQT under ozone stress but Penuelas and Staudt only mention the sum of MT and SQT. The authors then go on to suggest that their finding that SQT responds to stress, while MT do not, fits with the literature but this does not seem to be the case. They also state that MT are not critical for oxidative stress (page 14, line 13) but I don't see this supported in the literature.

The dynamic branch enclosure and the VOC analytical techniques employed by the authors are appropriate and they appear to be high quality measurements. I am concerned that they don't have more frequent measurements of SQT speciation since this could have a major impact on their assumed flux given the very different O₃+SQT reaction rates which could result in major errors in emissions given the large term to "correct" for O₃+SQT losses. They state that the few measurements they do have suggest that SQT speciation doesn't change but I am unconvinced since the existing literature suggests that we should expect to see changes in SQT speciation with different season and stress. At a minimum, I suggest including a sensitivity study where they assume some possible changes in the SQT speciation and show how this could influence the results. One ancillary measurement that appears to be missing is leaf terpenoid content which would have been a worthwhile addition.

The major weakness of this study is that there are no replicates- they only made measurements on a single branch. The authors justify this by saying that "Back et al. (2001) found that in contrary with MT, SQT emissions do not defer significantly between the investigated coniferous trees (Scots Pine) and therefore smaller uncertainties would be induced when applying the algorithm". This sentence is a bit unclear but it seems to say that we can trust this algorithm because Back et al. found little variation in SQT emission factors among different Scots Pine. However, the Back et al. result on emission factors is not very relevant for this study on emission response and may just say that SQT emissions from Scots Pine are not very important. What is relevant is that

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Heiden et al. found that Scots Pine exposed to ozone emit more MT but not more SQT. This suggests that there is considerable variability in SQT response to ozone among different species and does not rule out differences among individual spruce trees. I suggest two possibilities for making this manuscript acceptable for publication in Biogeosciences: 1) include some replicates (other individuals and species), or 2) remove the text on the quantitative algorithm and instead discuss how to go about making a quantitative algorithm and what would need to be done (i.e. more measurements) to do this.

Specific comments:

Page 9, line 22-23: The text says there are different beta values but they aren't statistically different

Page 9, line 26: why should this influence SQT emissions?

Page 11 and elsewhere: is the relative humidity correlation just due to temperature?

Page 12, line 2-3: the temperature is always below 30C. Is this really a stress?

Page 12, line 10: how does this explain the rise in basal rates?

Page 15, line 3: clarify this sentence

Page 17, line 7: since you didn't measure production rates or even pool size then how do you know this?

Interactive comment on Biogeosciences Discuss., 9, 7661, 2012.

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