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Biogeosciences Discussions

Interactive comment on "The emission factor of volatile isoprenoids: stress, acclimation, and developmental responses" *by* Ü. Niinemets et al.

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The Ninnemets et al. manuscript titled "The emission factor of volatile isoprenoids: caveats, model algorithms, response shapes and scaling" and the companion paper "The emission factor of volatile isoprenoids: caveats, model algorithms, response shapes and scaling" review biogenic VOC emission model components, discuss their shortcomings, and make some recommendations for future efforts. Anonymous referee# 1 states that this is an authoritative review by prominent scientists in the field and I fully agree. The referee goes on to say that the paper is untimely and should be postponed. I don't fully agree with this since the paper does synthesize the information in the individual papers in the Trends in Plant Science and (along with the companion paper) makes a few additional important points. In particular, the manuscript

discusses how progress could be made in the development of quantitative algorithms that describe emission response to drought and heat stress. However, I believe readers would benefit if the two papers were combined, redundant material eliminated, and review material that is presented elsewhere were made more concise.

If the definition of an emission factor includes the growth conditions (e.g., Guenther et al. 2006) then there is little variation in Es for different light regimes. So for the Guenther et al. (1999) model referred to on page 1544, line 10, there is no within-canopy variation in Es- instead the variation is associated with the long-term light response emission activity algorithm. This is an important point for text throughout the manuscript which focuses on the need to turn a constant emission factor into a variable emission factor. But since the emission factor is just the part of the model for which you can't (yet) explain the variability with a numerical algorithm (and so need to assign a mean value representative of the population mean and an associated standard deviation representing the variability) then Es does not vary for those driving variables (although you can run the model with a best estimate and an lower and upper bound). So (for example in section 2.4) you shouldn't have an Es that varies with season. Instead you should include an emission activity algorithm to characterize this variability and define the emission factor for a certain season. The authors view of Es as the emission rate adjusted only by instantaneous light and temperature is incorrect if additional emission activity algorithms are included and the emission factor is defined for specific conditions. So instead of saying (page 1551 line 12) that they have "attempted to chart a path forward for including Es as a dynamic term in future modeling efforts" I would suggest that instead they are attempting to reduce the variability associated with Es (the population average emission factor) by adding algorithms that account for this variability.

P 1539, line 16-19: It would be useful to include some suggestions of what investigators could use to characterize biotic and abiotic stresses.

Page 1548, line 21: the canopy emission factor can be experimentally assessed if you

know (or can estimate) the leaf age and past temperature and light. Also page 1550 line 1: why is it “essentially impossible” to do this?

Interactive comment on Biogeosciences Discuss., 7, 1529, 2010.