

Interactive comment on “Large estragole fluxes from oil palms in Borneo” by P. K. Misztal et al.

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Pg. 7 line 24 CO₂ and H₂O are exchanged across leaf surfaces, not from leaf surfaces.

Pg 8 line line 15 Why do you refer to monoterpene concentration gradients? Monoterpenes are not the topic of this paper.

Pg 8 lines 15-18 I don't buy your argument that we should ignore concern about differences in the ambient-to-leaf concentration gradients when using charcoal-filtered air. You seem to suggest that because the influences on leaf-to-air concentration gradients are complex, we should assume that the influences will tend to average out to be negligible. In the end, you really don't know what the effect of using charcoal-filtered air is on your emissions measurements, and you should simply state that, along with the assumption you have used that other factors are larger. That assumption may be wrong, but you have to go with it given your design. Don't stretch to try to convince the

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reader that this assumption is valid, because you really don't know if that's true.

Pages 11-13. The modeling seems quite uncertain. I'm not sure which coefficients were borrowed from Guenther et al. (2006) for the temperature and light dependence curves, but there appears to be an assumption in this approach that the coefficients transfer with similarity from terpenoid compounds to estragole. The uncertainties carried in this assumption are then transferred to the calculation of the deposition velocity, and from there to the calculation of the canopy conductance to estragole emission. The aerodynamic conductance of the canopy is likely to carry considerable uncertainties, as approaches to derive this term from wind profiles carries inherent uncertainties. Additionally, the leaf boundary layer conductance is estimated for a 'standard' leaf assuming diffusive exchange. Within-canopy turbulence is ignored, with also introduces high uncertainties into the modeling of the transport resistances. When all of this is considered, the modeling comes across as more of 'back-of-the-envelope' stuff than a rigorous effort to understand the controls over exchange. I just don't think the modeling can carry much legitimacy.

Overall, the paper provides some very interesting and valuable data on estragole emission observations. The modeling summarized in Figure 10 is so uncertain as to be of low utility for interpretation. I recommend eliminating Figure 10 and the section on deposition modeling. This aspect of the study should await a future, more nuanced approach to this issue of emissions versus deposition, and the issue as to how the Guenther models for isoprene line up with regard to the temperature and PAR dependence of estragole emission.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1517, 2010.

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