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Atmospheric Chemistry and Physics Discussions

Interactive comment on "New constraints on terrestrial and oceanic sources of atmospheric methanol" *by* D. B. Millet et al.

D. B. Millet et al.

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Response to comment by J. Rinne

Our responses are given below in bold italics.

This is an interesting and well written paper, which contributes to the understanding of the sources and sinks of atmospheric methanol.

The estimate for the terrestrial biogenic methanol emission given in the paper is 80 Tg/y. Compared to the estimations of global isoprene emissions of 400-500 Tg/y this is about 16-20%. There is a number of ecosystem scale measurements of methanol emissions along with other VOCs from various ecosystems published in recent years. It could be worthwhile to see if the observed methanol emissions and ratios of methanol to terpenoid emission are consistent with the conclusions of this paper.

An interesting point. However, isoprene emission rates vary strongly with vegetation type. Methanol emissions appear to do so as well. Since their emission rates probably do not vary in the same way across vegetation types, there is no reason to expect ecosystem-scale emission ratios to resemble the global average.

We did compare simulated emission rates with flux measurements at the University of Michigan Biological Station (UMBS), Duke Forest NC, and Blodgett Forest CA. These comparisons just confirm what we find by comparing concentrations: the model is too high at Duke Forest, too low at UMBS, and too low at Blodgett. This reinforces our finding that "the variability between surface sites is not captured by the model" and that "the relationship of methanol emission to NPP is weaker and more variable than previously thought".

Page 7621, lines 15-16: "...the large ocean source could cause detectable structure S5963 in atmospheric concentrations..." I have to slightly disagree with this sentence. More important than the production is the net exchange between the atmosphere and ocean. Only if the oceanic source strongly modifies this exchange will it lead to detectable structure in the atmosphere.

Agreed. We have modified the text to clarify this point.

Page 7621, 26-27: "...these high observed values may result from down-mixing of free tropospheric air". Is there any evidence on this, for example using ozone concentrations?

Good point. Yes, ozone is slightly elevated during this period. We added a parenthetical comment to this effect.

Minor comments

Page 7616, line 4: "...with a beta factor of 0.09..." The meaning of this may not be obvious for a reader not familiar with the MEGAN modelling.

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We replaced that phrase with the following: "(emissions scaled as exp[β(T − 303)], with β = 0.09)"

Page 7629, line 17: "...atmospheric lifetime is 4.7 days, lower than..." I would change "lower" to "shorter".

Done.

Figure 6: The scale of the x-axis makes it hard to see any structure in the vertical profiles. I suggest scaling so that the structure is clearly visible, for example 0-5 ppb scale.

Done.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 7609, 2008.