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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.

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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.

Page 7621, lines 15-16: "...the large ocean source could cause detectable structure

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.

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?

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.

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

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.

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