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Interactive Comment

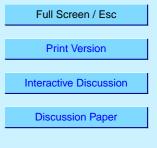
Interactive comment on "Oxygenated compounds in aged biomass burning plumes over the Eastern Mediterranean: evidence for strong secondary production of methanol and acetone" by R. Holzinger et al.

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The paper presents interesting results on the potential effect of aging in biomass burning plumes leading to an increased abundance of volatile organic species. The authors point out that the loss of CO could potentially increase observed emission ratios in aged biomass burning plumes. Indeed it appears that the MeOH/acetonitrile ratios listed in table 2 agree much better with previously published results than the MeOH/CO ratios. If true it questions the usefulness of CO as a conserved tracer in aged biomass burning plumes. In particular initial HO concentrations in biomass burning plumes are poorly constrained and the assumed 24% loss further downwind could potentially be higher.



On the other hand it highlights the importance of characterizing initial emission factors right at the source in order to be able to distinguish primary vs secondary formation of oxygenated compounds.

Figure 3 shows that on average acetonitrile concentrations decrease in the marine boundary layer. Doesn't this suggest that on average the Mediterranean Sea acts as a sink for acetonitrile and is not in equilibrium? The authors might not be aware of a study on the Mauna Loa Observatory [IJMS, 223 (1-3): 527-538 JAN 15 2003] where we have observed systematically lower acetonitrile mixing ratios in marine boundary layer air during a 5 week measurement period, suggesting that the ocean can be undersaturated and an effective sink for acetonitrile on longer timescales. In light of various reports on acetonitrile concentrations in the marine boundary layer I would agree that it remains an open question whether the ocean can act as a sink or can be considered in equilibrium on a global scale.

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