

Interactive comment on “Sources and seasonality of atmospheric methanol based on tall tower measurements in the US Upper Midwest” by L. Hu et al.

Anonymous Referee #2

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The aim of this paper is to present one year of continuous atmospheric methanol measurements obtained from a site located in Minneapolis, US, and to interpret the observations using a global atmospheric model. The main conclusions can be summarized as: (i) the anthropogenic emission of methanol at this site accounts for 40% of the observed methanol mixing ratio in winter and 10% in summer; (ii) the data suggest that the assumed dependence of biogenic methanol emissions on temperature in the MEGANv2.0 emission model is accurate enough; (iii) the model underpredicts the data by about 35% in summertime, pointing to flaws in the basal emissions assumed in MEGANv2.0.

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The subject of the paper is well within the scope of Chemistry and Physics Journal. The article is written in a clear way and the conclusions are interesting. The manuscript can be accepted for publication only after the following points are adequately addressed and elucidated.

Major comments:

1) In this study, the model is found to significantly underestimate the methanol measurements. However, from two previous modelling studies we get a different message. In fact, when using GEOS-Chem model and a NPP-based parameterization for methanol plant emissions, Millet et al. (2008) concluded that a significant decrease of biogenic methanol emissions over the US is required in order to match boundary layer methanol concentrations from in situ and aircraft observations. Furthermore, the use of the latest version of MEGAN (MEGANv2.1, Stavrou et al., 2011) in the IMAGES global model resulted in important overestimations of aircraft, in situ and satellite observations in the US. This point merits to be addressed in the revised version.

2) The authors should drive the model with other available inventories for biogenic methanol emissions. How does the MEGANv2.0 used in this paper compare with the NPP-based inventory used in Millet et al. (2008), and with the MEGANv2.1 inventory (<http://accent.aero.jussieu.fr>)?

Minor comments:

1. p. 17474 : The measurement period must be mentioned in the abstract and the introduction section.
2. p. 17481 : The GFEDv2 biomass burning inventory is, to my knowledge, not available after 2008. Are the simulations performed for the measurement period?
3. p. 17500 : Please plot also the simulated annual cycles for CO, benzene and toluene in Fig.3.

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4. p.17483, l.27: "but make up" : replace by "and make up"
5. p.17487 : Section 3.4 does not bring new insight in the interpretation of the measurements and could be either omitted or shortened.
6. p.17488: l.15: replace "over 1 year" by "over one year"

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