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Atmospheric Chemistry and Physics Discussions

Interactive comment on "First space-based derivation of the global atmospheric methanol emission fluxes" *by* T. Stavrakou et al.

Anonymous Referee #1

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Stavrakou et al. present an analysis of IASI methanol measurements and an adjoint inversion to estimate global methanol source fluxes. They evaluate two representations of the biogenic source (one based on NPP, and one based on the MEGAN model) as a priori terms in the inversion. They derive emissions that are similar globally to those estimated by MEGAN, though with significant regional differences. The retrieved fluxes also result, by and large, with improved agreement with in-situ measurements.

The paper is well-organized and carefully written. The analysis is generally sound and the topic is useful and suitable for ACP. The introduction to the physical processes governing biogenic methanol emissions is very good, and the description of the IASI retrieval is informative and clear. The description of MEGAN and IMAGES are of appropriate length and detail while providing adequate citations for further information.

I have a few comments and suggestions as follows, and recommend publication once they are addressed.

General comments

- my main complaint with the paper is the lack of any real uncertainty analysis. The authors discuss the uncertainty in the IASI measurements themselves (>= 50%) but do not discuss in any real way the uncertainty in their inversion results. I see this as problematic and needing to be addressed before the paper can be published. Otherwise one doesn't know how to interpret the results. For instance, the a posteriori biogenic flux is 100 Tg ... is this 100.00 +/- 0.01? +/- 100%? 200%? I recognize it is not necessarily straightforward to come up with realistic quantitative uncertainty estimates on inversion results and that the comparison with independent observations is one step in this direction. However I think it is incumbent on the authors to do a bit more in this regard. For instance, how do the retrieved fluxes depend on uncertainties in the overall model framework? E.g., model OH concentrations? Meteorological fields? Assumptions regarding reactive uptake during dry deposition (Karl et al., Science 2010)? Etc? Without treating these points the paper risks becoming another inversion study lacking a good-faith assessment of the robustness of the results. It deserves better!

- One point that is not discussed: how well do you expect the retrieval to perform over fire scenes?

- And a related point: to what extent do you really have constraints on the biomass burning flux, given that it is only $\sim$5% of the biogenic flux? This gets back to the a posteriori uncertainties.

- How well are the a posteriori fluxes (biomass burning versus biogenic) really resolved? This is not assessed in the paper. Figure 4 seems to imply some conflation of the two source types. For instance, fires in Alaska and E. Russia appear to be resulting in artificially higher biogenic fluxes in those areas.

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- since you are not doing full retrievals everywhere, is there potential for differential bias depending on the surface properties and hence land cover of the retrieval scene? And then that such bias is erroneously attributed to emission differences? For instance, you discuss specifically an apparent model underestimate in arid regions.

The model underestimation in the free-troposphere is touched upon in multiple places, and the strength of the oceanic source/sink is given as a possible cause. A bit more more could be said on this point. For instance, would a stronger ocean source (sufficient to resolve the discrepancy in the free troposphere) give a realistic simulated vertical profile over oceans?

Specific comments

- a few grammatical problems throughout

- 5223, 15-16: use consistent units to avoid confusion (Tg or TgC)

- font on some figures is too small; hard to read

- Figure 5, 6, and especially 7, suggest a different color and / or symbol for the aircraft observations and the S1 simulation, it can be hard to distinguish the two (for instance, the middle panel at the lowest vertical level)

Section 8, page 25, paragraph 3 (and elsewhere): What exactly is meant by “satisfactory agreement”?

Figure 11: Add in IASI error bars here for consistency

Section 7, page 21: You mention here that the seasonal cycle of IASI in the SW US is similar to that observed at Kitt Peak, but neither the western nor SW US are shown in Figures 5 or 6.

Section 7, last line, reference to Figure 9: Statement is confusing b/c satellite data is not plotted in Figure 9. Do you mean that the OptS2 simulation results in better agreement with the surface observations, thus the satellite data must be consistent

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with those observations? Please clarify text accordingly.

5240, 25: Please be more specific in your discussion regarding Indonesia. Are you referring specifically to the Sumatra plot when you cite the 2x discrepancy over Indonesia? But then you discuss in-situ observations over Malaysia, which means we should be looking at the Borneo plot? Indochina technically should not include either Indonesia or Malaysia, but readers might easily assume you mean to refer to the Indochina plot, which actually shows good agreement for the May-June timeframe (when the in-situ data were taken).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5217, 2011.