

## ***Interactive comment on “Tropospheric methanol observations from space: retrieval evaluation and constraints on the seasonality of biogenic emissions” by K. C. Wells et al.***

### **Anonymous Referee #2**

Received and published: 28 March 2012

This study investigates the seasonal cycle of natural emissions of methanol using a combination of satellite remote sensing and atmospheric chemistry and transport modeling. The methodology is sound and described very clearly. The findings make a significant contribution to our understanding of methanol. It is a nice example of how satellite measurements of atmospheric composition support the study of large-scale biogeochemical cycles. I have a few comments that need to be carefully addressed but shouldn't pose any insurmountable hurdles to the authors.

### GENERAL COMMENTS

Looking at Figure 1 it is clear that the signal of methanol is of the same size is that

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of other remaining residuals. The question is what variables contribute to that signal, and whether there could be any leakage of seasonal variation having to do with those factors into the retrieval of methanol. I imagine it would be easy to test the seasonal cycle at sufficient distance of continental sources to exclude the possibility of any external factors playing a significant role. There is little discussion about the filtering of data, except for a DOF criterion. For example, some procedure must be used to avoid influences of clouds, etc. Some further explanation is needed.

The description of IASI is very brief about the use of the averaging kernel to sample the model. It suggests that a procedure is followed that is similar to that used for TES, which receives much more attention. If this is so it should be mentioned explicitly. Otherwise a description is needed at a comparable level of detail.

Looking at Figure 4 it is unclear why the level of methanol is lower for TES than IASI. According to the text the averaging kernel of TES peaks closer to the surface, which means closer to the sources. Therefore I would have expected higher methanol concentrations.

Discrepancies between the model and the measurements are explained exclusively in terms of biospheric emissions, whereas several other factors influence atmospheric concentrations such as chemistry and transport. It is unclear how realistic photochemistry is represented in the model. For example, CTM's have difficulty maintaining sufficient oxidizing capacity under conditions of low NO<sub>x</sub> and high VOC emissions. Such factors require further attention.

It is unclear what seasonality is assumed for remaining emissions. It is known, for example, that fossil fuel emissions in the extra tropics show a distinct seasonal cycle. It is clear that the biosphere dominates in summer. However, this study focuses on the early spring when the seasonal signature of anthropogenic and natural emissions may be comparable.

SPECIFIC COMMENTS

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Page 3958, line 28: Some further explanation is needed about the optimization procedure.

Page 3959, line 11: What motivates the choice of LAI=2?

Figs 8, 9, 10: More quantitative information is needed about the agreement between measurements and model simulations (for example in terms of correlation and rms difference) to support claims in the text that the agreement has significantly improved after optimization.

#### TECHNICAL CORRECTIONS

Page 3961, line 26: the meaning of “higher slope” will be unclear to all those readers who will only read the abstract and the conclusions section.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 3941, 2012.

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