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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of satellite-derived HCHO using statistical methods" *by* J. H. Kim et al.

Anonymous Referee #1

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In this paper, the authors apply statistical methods (empirical orthogonal function and single value decomposition analyses) on various HCHO column satellite data sets to highlight the most significant spatial and temporal variations in the HCHO global fields. Focus is on tropical Africa and South America regions where the correlations of the HCHO spatial and temporal patterns with biomass burning activity are examined in an attempt to better understand the origin of the HCHO signal. In addition, the same statistical tools are applied to CO observations from MOPITT, which are used as an indicator for biomass burning activity.

Although the subject of the paper is well within the scope of ACP, there is a list of major issues that prevent the publication of this article in its current shape. Overall, the message is not always clear and there are statements that need to be further

discussed and supported. To my opinion, considering these issues will make the paper significantly different, requiring consequently a new submission procedure.

1. Barkley et al. (GRL, vol. 36, L04803, 2009) presented a very similar analysis based on GOME and SCIAMACHY HCHO data sets. The added-value of this paper compared to the Barkley's paper is not really clear. In any case, the authors should compare and discuss their results to those from Barkley. In particular, Barkley et al. attribute part of the HCHO signal in Amazonia to biogenic sources on contrary to the present study. Also, they use the three first modes to explain most of the HCHO variability. In the present manuscript, the authors only use the first mode which represents a small fraction of the GOME and SCIA HCHO variability. Can the following modes provide useful information?

2. The title of the manuscript and the abstract are misleading. They suggest that a validation of various HCHO data sets based on a novel method is realized. To me, the statistical tools presented in this work are used to interpret seasonalities and spatial patterns of HCHO and to establish links with possible sources. Very little is done in terms of intercomparison of the different data sets. The fact that the various data sets show roughly the same structures in the EOF mode 1 is too qualitative to be considered as validation. In addition, interpretation of the features of the EOF mode 1 is missing for the different data sets. For example, why does this mode represent 50% of the OMI variability and only 20% of that of GOME and SCIA? Why is the GOME EOF mode 1 signal much weaker compared to the other instruments? Why is the amplitude of the corresponding expansion coefficient much weaker? In the Barkley's paper, the time series of the principal component of mode 1 shows only a small discontinuity when SCIAMACHY replaces GOME.

3. The authors compare the spatial and temporal variations of CO and HCHO observations to those of fire counts from ATSR. There is no reference for this product. Since the spatial and temporal patterns of the fire count product is largely used and discussed by the authors, a figure illustrating these features should be added.

4. It is not clear how the longer CO lifetime can explain the lag between the peak in the ATSR fire count product and the maximum in the CO observations. Since CO is directly emitted by fires, the two maxima should be in phase whatever the CO lifetime. This lag could be caused by fires emitting CO which are undetected by ATSR. The seasonality in the fire count product could be compared to the GFED inventory. Another explanation for this lag could be that the maximum in the CO observations is partly due to alternative sources. A large part ($\sim$50% at the global scale) of CO originates from oxidation of CH4 and NMVOCs (e.g. Hooghiemstra et al., ACPD, 11, 341–386, 2011). The authors do not discuss this secondary source and establish a direct link between fires and CO maximum without any convincing argument for the lag.

5. The same comment is true for HCHO. In principle, a maximum in the HCHO observations should be detected when the biomass burning activity is the largest. For example, in the auxiliary material of the Barkley's paper, a figure shows the clear temporal correlation between fire counts and high HCHO columns. The lag between ATSR fire count and HCHO maxima can not be explained by the HCHO lifetime as claimed by the authors.

6. In the "introduction" and "data" sections, the authors suggest that the same algorithm is applied to OMI, GOME and SCIAMACHY data. To my knowledge, the data provided on the "temis" and "mirador.gsfc.nasa" websites are not retrieved in the same way. A description of the GOME and SCIA products is probably missing. On the TEMIS website is provided a GOME-2 data set. This instrument has a better spatial coverage than GOME and SCIAMACHY and could have been considered in this study. Also, the MOPITT CO product is not described and there is no reference for it.

7. The discussion about the retrieval error sources in the introduction should be clarified and moved to the next section. The description of the statistical methods should be extended. In particular, the differences between the EOF and SVD analyses should be further explained, especially because the contribution of the results from the SVD analysis is not clear in the discussion, these results appearing very similar to those

from the EOF analysis.

8. The EOF analysis allows detecting area with high variance compared to a mean value. Possible spatial and temporal correlations of the EOF signal for HCHO or CO with biomass burning activity does not mean that biomass burning is the strongest source of the corresponding specie but that biomass burning is the main cause for its variability. The authors should be more cautious in the formulation they use for the conclusions they draw from their analyses. In particular, their conclusions are in opposition to several studies showing that biogenic emissions are an important source for HCHO production in tropical regions (e.g. Stavrakou et al., ACP, 9, 1037–1060, 2009; Guenther et al., Atmos. Chem. Phys., 6, 3181–3210, 2006).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 8003, 2011.