

## ***Interactive comment on “Diurnal, seasonal and long-term variations of global formaldehyde columns inferred from combined OMI and GOME-2 observations” by I. De Smedt et al.***

**Anonymous Referee #2**

Received and published: 21 May 2015

This paper provides a consistent long time record of satellite HCHO data from OMI and GOME-2 using an updated version of retrieval algorithm. The authors then validate the new HCHO retrievals using global ground-based DOAS HCHO observations regarding total HCHO columns, HCHO vertical profiles as well as seasonal and diurnal variations in HCHO columns. Finally, interesting global and regional trends in satellite HCHO columns are examined and identified based on the validated retrievals. The paper is generally well written and describes space-based observations and trend clearly and thoroughly.

I think it is certainly appropriate for publication subject to the following addi-

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tions/modifications.

1 It needs to clarify whether aerosol scattering has been taken into consideration in computing scattering weights. If not, why? Along the same line of scattering weights, could the authors comment on uncertainties due to cloud? And to what extent could (changing) aerosol load impact the retrievals and eventually affect the HCHO trend seen from space in China and India?

2 In my opinion, it would be great to have some comparison between the updated HCHO retrievals from this paper and other available HCHO products, e.g., OMI HCHO from González Abad et al. [2015] and OMPS HCHO from Li et al. [2015]. Some numbers in hotspot regions and background from different products are enough.

3 I don't understand why GOME and SCIAMACHY HCHO retrievals are necessarily consistent with new OMI and GOME-2 retrievals. GOME and SCIAMACHY data were retrieved using old algorithm, if I understand correctly. Will difference in retrieval algorithms lead to uncertainties when combine all the data in looking at the long-term trend?

4 GOME-2 has a larger footprint than OMI. Does this mean that GOME-2 is easier to be affected/contaminated by cloud? If so, it may not be fair to directly compare OMI and GOME-2 retrievals. This might also lead to bias in getting reliable diurnal HCHO profile, which is one of the main purposes of this paper, bracketed by GOME-2 and OMI. Can the authors comment on this?

5 I don't understand why 100 km is used in getting average HCHO columns. Some back-of-envelope calculations based on HCHO lifetime and local annual wind speed will be appreciated.

6 I'm not totally convinced by some potential driving factors of HCHO trend proposed in the paper. Temperature should be the dominated driver of the interannual variability (IAV) in HCHO columns. The authors may want to acknowledge more that trends in

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temperature would have played a role in the changes/trends observed. The authors may also want to clarify why the instrument aging only plays a negligible role in the observed trend.

Some minor comments:

Page 12243, Line 17-18 “constrain NMVOC emissions in top-down inversion approaches” Not all of cited work involve inversion approaches, some of them instead simply assume linear relationship between HCHO columns and NMVOCs from a CTM to constraint NMVOC emissions.

Page 12245, Line 21, “Section 3” “Sect. 2”, to be consistent.

Page 12246, Line 4, “at 13:30 LT” “around 13:30 LT”, more accurate

Page 12246, Line 13, “which enables daily global coverage” “which enables almost/nearly daily global coverage”, more accurate

Page 12247, Line 21, “Since 10 years . . .” Can the authors rewrite this sentence?

Page 12248, Line 11, “model a priori profiles is used at the” “model a priori profiles is sampled at the”, more accurate

Page 12255, Line 7, “The model time step is set to 4h” Can the authors clarify at which time period is GOME-2 and OMI a priori sampled?

Page 12255, Line 26, “about 71 and 26 % of the global sink” HCHO loss due to OH oxidation and photolysis is about half to half. Can the authors say a little more about the two loss pathways? Maybe with some references?

Page 12260, Line 7-8, “has been developed to retrieve tropospheric trace gas total columns and profiles” Somewhere in the introduction part, the authors may want to clarify what they have retrieved are tropospheric HCHO columns.

Page 12262, Line 27-28, “the slopes of the regression lines between” Is this RMA

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regression? It's better be since you have errors in both X and Y. Also it seems to me that the intercept has been forced to zero, right? If so, why?

Page 12263, Line 11-12, “bringing the satellites and ground-based observations to a satisfactory agreement within 15%.” I'm not convinced. It seems to me that you have used information from observations in getting vertical profile. Of course you can get a better agreement.

Page 12263, Line 24-26, “The effect of the rather coarse resolution of the global CTM on the modelled profiles (here  $2.5^\circ \times 2.5^\circ$ ) needs to be further investigated, as well as possible other effects of vertical transport and chemical processes.” Just a comment on this. The coarse CTM may be doing OK for regional background from biogenic sources (e.g, HCHO from isoprene) but may never be able to model urban/point source right on top of biogenic background.

#### References

González Abad, G., Liu, X., Chance, K., Wang, H., Kurosu, T. P., and Suleiman, R. (2015), Updated Smithsonian Astrophysical Observatory Ozone Monitoring Instrument (SAO OMI) formaldehyde retrieval, *Atmos. Meas. Tech.*, 8, 19–32, doi:10.5194/amt-8-19-2015.

Li, C., J. Joiner, N. A. Krotkov, and L. Dunlap (2015), A new method for global retrievals of HCHO total columns from the Suomi National Polar-orbiting Partnership Ozone Mapping and Profiler Suite, *Geophys. Res. Lett.*, 42, doi:10.1002/2015GL063204.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 15, 12241, 2015.

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