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Atmospheric Measurement Techniques Discussions

Interactive comment on "Development and characterisation of a state-of-the-art GOME-2 formaldehyde air-mass factor algorithm" *by* W. Hewson et al.

Anonymous Referee #1

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The manuscript by Hewson et al. is interesting and potentially holds useful messages for the community. I am enthusiastic about the calculation of per-pixel AMF errors, but why not push through and provide a complete error analysis for tropospheric HCHO columns? The authors also make an important point at the end of section 5.2: data users focusing on regional studies, and there will only be more of them, should aim to recalculate AMF using profile information which can resolve the spatial characteristics of their target domain. This is a good point, also with an eye on the future 7x7 km2 TROPOMI instrument. This was done by e.g. Vinken et al. [2014] and Lin et al. [2014], who improved on coarse TM4 profile by using GEOS-Chem 0.5 x 0.67 degrees to better

resolve shipping lane/Chinese emissions effects in the AMF calculation for OMI NO2.

Although the paper is clearly structured and well written, I'm having a hard time with many of the main conclusions:

1. First of all, I find it difficult to believe that the AMF errors are "dominated by uncertainties in the HCHO profile shape". The method to compute the profile uncertainty contribution to the AMF error is not described clearly. Yes, HCHO below and above certain model levels are manipulated, but based on which hypothesis? How realistic are the perturbed profiles? I agree with reviewer#3 that a comparison with aircraft profile variability as done by Millet et al. [2006] makes much more sense.

2. Then on the albedo-related AMF errors; in FRESCO+ cloud retrievals, the MERIS albedo climatology is used, but for the HCHO AMF a completely different climatology is used based on TOMS (360 nm). Using wavelength-corrected (412 → 340 nm) MERIS values would improve consistency in the retrieval approach and in the error analysis. I don't see any benefit in using the TOMS albedo or the 'improved' OMI climatology: it holds for a different time period, 1979-1993, or a different time-of-day (13:40 hrs), and both have been retrieved from a different sensor (i.e. different viewing geometries), and the TOMS dataset is spectrally not representative for 340 nm. The authors must have weighty arguments why they prefer the TOMS or OMI albedo climatology over the MERIS 412 albedo set, which could easily be spectrally scaled to 340 nm using the GOME Koelemeijer albedo climatologies. I recommend to either replace the TOMS/OMI UV albedo's with MERIS 340 nm equivalent albedo's, or the authors should convince the readers why the OMI albedo may still be useful for GOME-2 retrievals at 340 nm.

3. The lack of consistency between the clear-sky albedo and the albedo used for deriving the cloud fraction introduces additional errors in the HCHO AMF. The cloud fraction retrieved in FRESCO+ holds, given the surface albedo used in the FRESCO+ retrieval. Any error in the MERIS surface albedo would normally be compensated by

the retrieved effective cloud fraction (if albedo is biased low, a high-biased cloud fraction still explains the TOA reflectance), but only as long as the MERIS database is used for the clear-sky AMF. Since the TOMS or OMI albedo climatologies are not consistent with the MERIS climatology, these compensating effects collapse, leaving the authors with an unknown contribution from albedo inconsistencies in their AMF values. The best would be if the authors resolve this issue by using MERIS albedo's at 340 nm for the clear-sky AMFs, but if they think that OMI-MERIS inconsistency poses no problem, they should explain why that is.

4. The above effects also apply on terrain height. A more sophisticated terrain height description for the HCHO AMF only makes sense if it is also applied to the cloud retrieval. From the manuscript, it is unclear if FRESCO+ accounts for terrain height in a manner consistent with what is proposed for the clear-sky AMF.

5. The quoted uncertainty on the surface albedo is very large (0.05), and would imply that most frequently occurring albedo values over relevant areas are 100% uncertain. How did the authors arrive at this estimate for albedo uncertainty? More importantly, if they used this value, the contribution from the albedo error to the AMF error should be much larger than the $\pm$10% values over tropical forests displayed in the upper panel of Figure 8, as the sensitivity of the AMF to the local albedo is strong for low albedo values over tropical forests. I urge the authors to re-evaluate their methods, and especially the sensitivity of the AMF to the local albedo and they should explain clearly why they find so much lower albedo-related AMF errors than e.g. the 20-30% errors quoted for albedo-related AMF errors in the case of NO2 by Boersma et al. [2004], who used a much smaller albedo uncertainty of 0.02.

6. The cloud error shown in Figure 8 is also very small over areas with a lot of HCHO; 5-10% at most. Especially for low cloud fractions, one expects a strong sensitivity of the tropospheric AMF to the cloud fraction (if this is calculated following the independent pixel approximation as stated by the authors), and hence much higher errors than quoted here. Is it possible that something is amiss with the calculation of AMF sensitiv-

ity to cloud fraction? The authors could include some typical dependency curves that illustrate the sensitivity of HCHO to albedo, cloud fraction, cloud pressure, and aerosols to convince readers that the error calculation is being done in a proper manner.

Specific comments:

P1113, lines 13-18: the description of w(z) is incomplete because no mention is made of how w(z) is computed for the cloudy part of the pixel. This omission should be repaired.

P1114, L21: suggest to add with observation times "and viewing geometries" different from . . .

P1115, L18-20: can artificially enhance the retrieval of tropospheric columns

Section 3: I was surprised not to read about including O3 as a potential AMF dependence. Does it need to be done or not?

P1116, L14-17: some other (SAO) retrievals do not need a background correction (K. Chance, personal communication, AGU 2014), why is it needed here? How large are the biases in the slant columns?

P1118, L9: I don't see how 340 nm AMFs are consistent with 360 nm LER estimates. Why is a wavelength-dependency correction not applied to scale the LERs from 360 to 340 nm?

P1119, L4-5: to what types do the SSA values quoted correspond?

P1123, L24-25: the BRDF effect for HCHO AMFs at 340 nm is probably even less relevant than for NO2, given the stronger Rayleigh scattering at 340 nm screening surface effects much stronger than at 440 nm.

P1127, L14-17: it should be clarified how aerosols affect the FRESCO+ effective cloud fraction retrievals.

P1129, L3-9: this whole part is unclear to me. It is supposed to describe how you tested for aerosol effects on the HCHO AMF, but you lost me.

P1140, Table 2: I think a distinction should be made between the global retrievals listed here by Boersma et al. and Valks et al., and the regional retrievals by Lin et al. and Russell et al. Furthermore, for surface albedo, the DOMINO v2.0 uses the 440 nm values from the Kleipool et al. [2008] climatology, and not the 479.5 nm values.

P1141, Table 3: the cloud approach is missing from Table 3. It should be incorporated.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 1109, 2015.