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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 #3

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The paper by Hewson et al. presents an improved algorithm to compute AMFs for GOME-2 HCHO retrievals. The algorithm is not particularly innovative but it brings together a number of incremental improvements from the recent literature and evaluates their effects on the AMF computation in a consistent framework including error estimates. The paper is well written. This is in my opinion a valuable contribution worth publishing in AMT.

I have one major comment that I think is critical to address. I am troubled by the error characterization presented here and the lack of comparison to the work of Millet et al. JGR2006 (cited in passing but not in that context). The authors find that the shape

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factor is the dominant source of AMF error but this is based on seemingly arbitrary +/- 25% perturbations to the HCHO profiles – without better characterization of the error on the HCHO profiles it seems to me that the inferred AMF error is of little value. Even more of concern, the finding disagrees with the previous work of Millet et al. , which provided in my opinion a much better estimate of AMF error because it was based on a large collection of aircraft vertical profiles over the eastern US in summer including simultaneous HCHO, cloud, and aerosol measurements. They reported a much smaller AMF error (15-24%, vs. 50-60% in this paper), and found that clouds were by far the largest source of error, which makes a lot of sense to me (and even more so for GOME-2 because of its large pixel size). By contrast, the authors here find clouds to be only a small source of error (smaller than aerosols, which makes no sense to me), but it's not clear to me how they estimated the cloud error. Cloud errors seem to get short shrift in this paper – they are discussed less than aerosol errors, which should be much smaller. I think that it is essential that this paper carefully compare their results to the experimentally-based AMF error analysis by Millet et al., and if they disagree to give a solid reason why. Millet et al. should be an important reference for this work. In particular there has to be a resolution of why this paper finds a 50-60% error, vs. 15-24% in Millet et al.

I have a number of other specific comments (page, line).

(1110, line 16) The authors advertise their computation of AMF errors for individual scenes. However, for individual scenes, isn't the spectral fitting error by far dominant? In my experience, this is why HCHO satellite data are reported only as monthly or seasonal averages. The spectral fitting error averages out but AMF errors tend to be more systematic. My point is that I'm not sure that an estimate of AMF error for individual scenes is valuable. Maybe the authors could give a better argument of why they think it is.

(1110, lines 17-18) I am skeptical of the 50-60% error and of the dominant contribution from the HCHO profile, for reasons stated above. If that statement is to remain in the

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abstract it needs to be better supported.

(1111, 4) It would be good idea to cite Palmer et al. JGR 2003 since they were the first to infer isoprene emissions from HCHO satellite observations.

(111, 21) I don't understand the 're-normalisation'.

(1112, 7) It makes no sense to me that aerosols would be a larger source of error than clouds. I believe that the 50% aerosol effect comes from an extreme case of an overhead biomass burning plume and is not really representative. Millet et al. 2006 should be prominently cited in these error estimates – it seems to me that they did a much better job at quantifying the AMF error than anyone else.

(1112, 11) I would think that model PBL depth is the largest source of error in the HCHO profile. This should probably be mentioned.

(1114, 9) References given for surface albedo contribution to AMF error are for NO2. NO2 is more sensitive to surface albedo error than HCHO (longer wavelengths), as pointed out by Palmer et al. 2001. This should be acknowledged.

(1114, 17) This section talks about surface albedo, HCHO profiles, aerosols, and LUTs as sources of AMF error but doesn't mention clouds.

(1120, 17) I would have expected the fine distribution of clouds to be a major factor of difference in the AMF calculation for individual scenes between model simulations at different resolutions. Maybe this gets washed out in temporal averaging, but if the goal is to have an error estimate for individual scenes. . .

(1128, 17) The statement that biogenic emissions greatly affect the shape factor seems weird. Considering that almost all the HCHO is in the PBL in any case, it seems to me that the shape factor would be largely insensitive to the magnitude of biogenic emissions and instead respond most strongly to the PBL depth.

(1130, 6) I question the punchline in the summary about the critical importance of the

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vertical profile; see my major comment above.

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