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Interactive comment

Interactive comment on "Detection of Outflow of Formaldehyde and Glyoxal from the African continent to the Atlantic Ocean with a MAX-DOAS Instrument" *by* Lisa K. Behrens et al.

Anonymous Referee #2

Received and published: 12 February 2019

This manuscript describes the detection of HCHO and CHOCHO in African outflow during the COPMAR project in 2016. The authors find elevated HCHO and CHOCHO at higher altitudes, and suggest biomass burning and transport of long-lived precursor VOCs as the source. Overall, the manuscript presents an important set of observations and sufficient preliminary analysis and is suitable for ACP. The following comments should be addressed before publication:

Comments:

1.) Section 4.5 discusses MOZART outputs which show elevated HCHO and CHOCHO between 3000m and 6000m, while MAX-DOAS shows only elevated HCHO. FLEX-

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Discussion paper

PART is then used to investigate potential sources. Why not turn on/off biomass burning in MOZART and see the impact on modeled profiles, or look at MOZART outputs of other tracers?

2.) If MOZART underestimates HCHO south of the equator, how does using incorrect MOZART profiles to calculate MAX-DOAS VCDs influence the retrieval in those regions?

3.) I echo reviewer 1's request to discuss the profile shapes used in satellite retrievals. Ideally, there would be consistency between these profiles and those used in the MAX-DOAS retrievals.

4.) The last two paragraphs of section 5 (comparison with other studies) are important, and it is difficult to interpret the authors' intent.

The authors first state that CHOCHO enhancements are "usually attributed to local production from CHOCHO precursors either originating from marine biota or from transported organic aerosol rich in dissolved organic carbon". They then state that "it seems more likely that on this days [sic] its source is related to transported precursors". Are they referring to their own observations only, or commenting also on previous authors' conclusions?

The next sentences are also problematic. The authors write "The aerosols trap the gases which are lifted and transported together. These stored gases are then re-released to the gas phase by reversible desorption after several days." There is no evidence of this in their work. It should be stated as a potential explanation or hypothesis rather than fact.

5.) In general, there is not enough care given to instrument accuracy. A large portion of the manuscript is dedicated to describing the DOAS analysis. For that reason, uncertainty should be quantified if possible. AOD and Angstrom-exponent measurements are also discussed and compared without any comment on accuracy or precision.

Minor comments:

1.) Page 2, line 11: “A global background concentration of HCHO exists of 0.3 – 2.0 parts per billion”. This is a wide range and it is unclear what is meant by ‘background’. For this manuscript, a useful number would be the concentration (in ppb or column density) over the remote ocean stemming from CH4 oxidation.

2.) Section 2.4: What is meant by ‘the model data are linearly interpolated?’ Interpolated in time? Sampled at the observation location?

3.) Section 3.2: What versions of the satellite products are used? OMI-BIRA? OMI-SAO? The only reference given is a Ph.D. thesis. Either the product should be described, or peer reviewed literature should be sited.

4.) Section 3.6: Is FINN the inventory included in MOZART? If so, it should be in section 3.4.

5.) Page 9 lines 26-27: “For the calculation of this limit, different approaches were used in previous studies, for example, Peters et al. (2012), Sinreich et al. (2010), and Platt et al. (1997). In this study, the detection limit is calculated for each trace gas individually with the method from Sinreich et al. (2010).” If these methods are different, the choice should be justified. If they are not significantly different, the other studies should not be discussed.

6.) Page 15 line 17: “Due to the primary emission sources of HCHO” sounds like direct HCHO emissions, which I do not believe is what the authors mean. Perhaps “photochemical production of HCHO from VOC precursors”.

7.) Page 27 line 1: Clarify what is meant by aerosol ‘type’.

8.) Page 27 line 5: “The present study is the first to confirm the enhanced levels of HCHO and CHOCHO frequently observed from satellites over the Atlantic Ocean using ship-based measurements.” Is this true (could be), or is it the first ship-based MAX-DOAS measurements over the Atlantic?

9.) Throughout: A correlation coefficient of 0.55 or 0.56 is ‘moderate’ (not ‘good’) agreement.

Technical comments:

1.) Page 9 line 14: “In this study, the trace gases are expected to be in elevated layers and from satellite measurements”. Please clarify.

2.) Page 9 line 25: “ground-based” should be “ship-based”.

3.) Page 26, line 18: It is unclear what is meant by “On 3 respectively 2 days”.

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