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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Seasonal variation of tropospheric bromine monoxide over the Rann of Kutch salt marsh seen from space" *by* C. Hörmann et al.

Anonymous Referee #2

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This manuscript describes measurements of BrO above the Rann of Kutch in India/Pakistan from satellite remote sensing. The work is interesting and well presented, although some use of English should be improved. The scope of the work fits in the journal.

General comments:

The manuscript describes potential albedo effects on the retrieval of tropospheric BrO and tries to argue that albedo is not a contributor to the enhanced BrO observed in this region. That discussion needs further consideration. It is clear from the albedo maps (Fig. 4) that the "white desert" of the Rann of Kutch is higher albedo than surroundings, or during Monsoon is similar to surroundings. The text description indicates

that the albedo is lower during the time of the April/May peak, which is true in the sense that of the absolute albedo in the Rann compared to other times of year, but the albedo contrast between the Rann and surrounding areas appears largest during the Feb-May time period. Significantly, the albedo contrast between the Rann and surroundings during the monsoon is very small, so if albedo affects BrO retrieval, no differential albedo exists during the monsoon and no BrO enhancement would be expected. The method of removing a background surrounding the Rann (Eqn. 1 on page 6) is potentially sensitive to the differential albedo between the Rann region and the surroundings. Therefore, I would suggest plotting on Fig. 5 not the reflectivity in the Rann region, but instead the difference in reflectivity between the Rann and the "background" region used for removal of stratospheric BrO influence. At least by eye, this seems to have a pattern more like the BrO enhancement. However, it does appear that the winter season is different than springtime despite similar albedo difference (Rann minus surrounding background regions). That seasonal difference could be affected by stratospheric annual cycling and should be further considered.

The discussion of GOME-2 data and comparison to OMI is less well developed than other aspects of the work. A number of arguments are made, but none are really fully explored. For example, there is a discussion of the diurnal cycle of BrO that is indicated to potentially be the cause of lower BrO abundance at the time of GOME-2 overpass (morning) compared to OMI (early afternoon). At most clean polar sites, BrO cycles are not highly diurnally varying, which is due to production of Br2 in the prior evening and at night and rapid photolysis of this brown gas in the early morning. Therefore, the supposed cycle at least would differ from polar sites. A reference, Holla et al. (2015), is cited, which does indeed show a peaking of BrO later afternoon. However, the Holla et al. (2015) manuscript also shows NO2 data that are enhanced through trapping of pollution NO2 in the shallow nocturnal / early morning boundary layer. Levels of NO2 above $\sim$1 nmol/mol appear to prevent production of high BrO levels. Therefore, the dirunal cycle at the Dead Sea may not be appropriate to the Raan of Kutch. In fact, the manuscript doesn't consider regional pollution, when it could have an effect on these data. Nearby Karachi has a population of ∼24 million people, and Ahmadabad is ∼7 million. Other effects like boundary layer height, morning fog/clouds, etc. should be fully discussed and this section should be revised accordingly.

The boundary layer height is not treated consistently in this manuscript. In the section about GOME-2 data, 2km (GOME-2 overpass) and 3km (OMI overpass time) are quoted, but the Table 1 and AMF calculations appear to use a 1km ABL height, with alternatives of 0.4 and 2.0 km. This is not consistent and clearly from the sensitivity testing in Table 1, would change the peak BrO mixing ratio significantly.

The manuscript should indicate that this work is motivation for measuring BrO from the ground in this region to verify the space-borne observations. Although the region is clearly remote, it is not inaccessible, nor would measurements via simple MAX-DOAS systems be difficult. Verifying the presence or absence of this space-detected feature would significantly contribute to our ability to connect space-based measurements to ground truth.

A number of sections discuss that the Rann of Kutch "...is probably one of the strongest natural point sources of reactive bromine...", but there is no comparison of this source to other sources. Additionally, the manuscript indicates that there is "supposed to have significant impact on local and regional ozone chemistry", but there is no calculation showing that this impact is significant. It certainly "may" have an impact, but in the absence of some reference indicating significance, the wording appears inaccurate.

Specific comments:

page 1, line 11: indicate that the times are "respectively"

page 1, line 13: replace "former" with "prior"

page 1, line 15: reword "supposed to have a significant influence"

page 1, line 19: Missing "von" from "von Glasow"

page 1, line 21: move "significantly" to after "troposphere"

page 2, line 19: cut the word "ever"

page 2, line 27: are both BrO and IO below 2 ppt? clarify

page 4, BrO retrieval: This section appears to indicate that Level 1 OMI data were reanalyzed by this group rather than use of the OMI BrO product (OMBRO). Can this be made more clear, and the specific sources of the data from OMI data streams should be described fully. If this calculation differs significantly from OMBRO, that should be noted – why was OMBRO not used?

page 6, line 15: This section is not fully clear. Why are slant columns of BrO (S*_trop) being calculated? Which "geometrical AMF"? Doesn't a geometrical AMF assume that the reflector is the Earth's surface, while the actual tropospheric return may be from clouds / fogs / aerosol light scattering?

page 6, Radiative transfer section. This seems like it should use at least 2km layer

page 6, line 28: "...and briefly described in Carn..."

page 7, line 25: Effects of local pollution may be affected by wind direction

page 8, line 18: should say "...not a strong..."

page 12, after line 16: should discuss potential of morning fog and/or NO2

page 13, line 1,2: boundary layer height inconsistent with modeling.

page 13, line 28: why is Salar de Uyuni being discussed here?

page 14, line 11: clarify "supposed to"

page 15, line 14: Wasn't OMAEROG also used, as well as either some level 1 OMI data or OMBRO (unclear).

page 22, caption says "adapted from Fund, W."? Typo?

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