The study by Friess et al uses two decades of tropospheric BrO measurements in Antarctica, performed at two sites, to investigate the links between trop. BrO and other geophysical quantities, either measured or modelled. From this and the temporal variability of trop. BrO, the authors draw conclusions on the source regions and formation mechanisms of this species. The authors also indentify an interesting event of BrO transport across the continent. The paper presents a unique database which is seriously analyzed, it is also well structured and perfectly in the scope of ACP so this work should be published.

On the content, my main remark is that on several occasions (e.g. L 326, L.485 and L 592, see below), when interpreting measurements with models, the authors assume the models more realistic and this is not discussed. If they are reasons to think the models are good enough in Antarctica, this is an interesting info to add in the paper since one may expect them to be limited by the available sampling there. If not, maybe a few words of caution about the models could be added in the interpretation.

Other than that, my remarks mainly aim to improve the readability.

P5. Table 1 should also include AH and NM.

P7 L180 The sentence starting with 'DSCDs' is complicated and misleading (it describes SCDs not dSCDs after the i.e.), I suggest to break it in two and put the subjects closer to the verbs.

P8 L193 'non linear constant intensity offset'. I think the authors mean that this is a non
linear term in the DOAS fits because it is additive on the intensity. If so 'Non-linear constant' reads weird. Maybe replace 'non-linear' by 'additive'? Or explain more otherwise.

P8 L206. The sentence is long and a bit messy. Consider dropping '(which are ....)' or breaking in two.

P8 L209. 'Most probable atmospheric state'. This reads an overstatement, even if the sentences after bring some clarifications. An OEM algorithm maximizes a metrics, but there is subjectivity involved in many ways (gridding, forward model, covariance matrix, convergence criterion etc...), not talking about the metrics itself. I would just remove 'most probable' unless the authors want to develop.

P 9 Table 3 should include the surface albedo.

Using layers of different thickness in the retrieval is interesting. I do not follow however why the small altitude difference explains that it does not work for AH. Did the authors do some tests to check this statement? If so explain, if not, reformulate the very affirmative 'most probably', as it could come also from different S/N in the DSCDs, or from the different elevation sequences.

Figure 2: please add the corresponding date so that we can relate the AKs to a given angular sequence. It is mentionned that the AK are for 'clear-sky', but what about the aerosol load? I ask that since I find weird the minimum sensitivity at the altitude of observations for AH. If the authors can further develop this last point, it's worth.

p 11 L.265 et seq. 'This illustrates that obs ...' - I do not follow here. Maybe I miss something, but the described improvement in dofs is for adding 1° elevation above the horizon, right? And the scope of the statement seems very large, but at NM, you have special conditions with the high surface albedo. It seems to me that you should describe the effect on the dofs of removing the negative angles, and give arguments for the fact that this is also true at low albedo (e.g. refering to other studies in warmer areas).

p 11 L.274 and very often later in the paper: there is a dot after the day number in the date, please remove

Figure 3: Please add 'HYSPLIT' at the beginning of the caption. The date, here and often after in the paper, does not follow copernicus standards, and add UTC or LT for the time.
Figure 4: legend and axis text should be larger

p 15 L.318 'Figure' should be 'Fig.' (here and across the paper) except at the beginning of a sentence, according to Copernicus standards.

L.326 "The difference ... can be attributed to the lower sensitivity of the MAX ..." There are also errors in the back trajectory modeling. Is there a reason why the authors do not consider it here?

'no BrO is detected above the detection limit' -> 'BrO remains under the detection limit'?

L 344. I would add ', in this place, ' before the second BrO.

In Fig. 7 et seq. (or in the caption) I would indicate the studied periods for the two sites.

L. 485 et seq., again, the model error for the wind speed over sea ice is not considered. It seems safe to assume that a measured wind speed is more accurate than a modelled one, especially in Antarctica. Unless I missed something, maybe the authors could add a few words on that?

L. 499 to 502. Break the sentence in two.

P 26 L 534-535 'This area' -> Seems to refer to 'the coastline east of NM' 4 lines before. I think this would help the readability to be more explicit e.g. 'this coast'

Across the whole Section 3.4, it would help the reader to have the important places for each site on the maps of fig 12 13 14 15. One of the suplots of fig 12 and 14 could pinpoint the important areas for NM, the same for AH on fig 13 and 15 (e.g. with symbols described in the caption)

L 592 again, the model seems implicitly assumed more realistic that the measurements.

L 619: 'am and pm' -> 'morning and afternoon'?
- L 637 missing 'from' after released? the whole sentence reads tautological: reactive bromine serves as a source of reactive bromine?

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Minor comments:

- the block letters in the title does not seem all appropriate

- in the author list, it reads weird that the affiliation of the 2nd author is the 3rd one

- L 349 'is' -> 'ice'

- L 426 'indcates'-> 'indicates'

- L 473 comma missing after 'There'?

- L 515 Sander et al should be between parenthesis

- L 582 'resides' -> 'resided'