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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The spatial scale of ozone depletion events derived from an autonomous surface ozone network in coastal Antarctica" *by* A. E. Jones et al.

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We thank both for referees for their comments on the manuscript, which we address here.

Referee 1, Hans-Werner Jacobi

1) We agree that using the abbreviation "h" is somewhat awkward, but this change was made to the ms by ACP, so I guess it needs to stay!

2) We used the word "salinity" as a way to address the fact that the specific nature of the source is not entirely known. Yes, it is linked to sea salt, but there are a number of

potential sources: e.g. salty brine is not specifically “sea salt”, nor are frost flowers... For this reason, we prefer to stick with the wording of “salinity” to keep things a little open at this stage.

3) You are right, that the mechanism seems well understood (when writing, we were thinking in broader terms). We have adjusted the sentence to use the word “source” rather than “mechanism”

4) Regarding flights over the sea ice zone in spring, there are sometimes flights in later spring, but the point is that there have been no observations of ODEs from aircraft. To clarify this, we have adjusted the text to read: “However, there have been no measurements from manned aircraft to probe the extent of ozone depletion over the springtime Antarctic sea ice zone due to the harsh environmental conditions during the ODE-season.”

5)a. The stated accuracy of 0.5 ppbv and detection limit of 1 ppbv for 1-min averages only applied to the dual-cell instrument at Neumayer. We have checked the data, and the statements are correct. Please note, however, that these values do not apply to any of the other instruments referred to in the paper.

b. Full calibration tests were done pre- and post-deployment, so a comparison can be done for before/after the experiment, but none were done in the field. We are therefore not able to provide they type of estimate of instrument performance in the field that you suggest.

c. The 2B Tech instruments were modified so as to work at -40oC – they were not simply off-the-shelf instruments. As a result of these modifications (described in Bauguitte et al., 2011), they were quicker to stabilise and were tested for this in freezers at BAS pre-deployment. Noise was found to be significant over the first 3 minutes, but generally after that time, the signal to noise was sufficient to produce valid data. When this was not the case, additional filtering was done, as described in the paper. To address this point, we have amended the manuscript to read: “Following such down times, the

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instruments required a short time for the signal to stabilise again. Such recoveries were aided by instrument modifications that had been made to deal with the low anticipated ambient temperatures (described in Bauguitte et al., 2011)."

d. Zero ozone measurements and span checks were done at the start and the end of the deployment period, as described in Bauguitte et al., 2011 (section 4.1), but not during autonomous operation. The following statement is now included in the section 2.2 Technical/instrumentation: "Zero ozone measurements were taken at the start and the end of the deployment period, as described in Bauguitte et al. (2011)."

e. At the end of section 2.2 there is a clear statement that "All data presented here, for both the autonomous sites and manned stations, are 10 min averages".

f. Yes, data from the manned stations were filtered to remove the influence of local pollution. This point is now clarified in the paper (section 2.2): "Data presented from the manned stations are filtered to remove any influence of local pollution."

6) The results from sites B and C indicate that the air measured up to 1000 km altitude has been influenced by bromine chemistry. They do not demonstrate that BrO is present at these altitudes. Ozone-poor air might be advected to these altitudes without advection of BrO. We know that there is no consistent correlation between BrO and O3 (from work by many authors). And we know that BrO can exist with considerable structure in its vertical profile. To really know whether BrO was present below 1000 km we need ground-truthing for the satellite data, which of course we don't have. To expand the discussion somewhat further in the paper we have added to section 3.3.1: "We note that the BrO VCD is also enhanced over the transect area during the October ODE (Fig.6), although, again, we have no information on its vertical profile and thus the relation to the ozone-depleted air masses."

7) We have made this change, as suggested by the Reviewer.

8) The amount of ozone depletion is calculated as described in the caption for Fig 8.

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This information is now included in the paper, and the other changes suggested by the Reviewer are also made. (i.e. revised captions for Tables 2 and 3, and explanation in the manuscript of how the surface anomalies were calculated).

9) We have changed “square kilometres” to “area” as suggested.

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Response to anonymous Referee 2

We thank Referee 2 for checking through the references, but we find that both the Richter et al (1998) and the Read et al (2008) are in fact already included in the reference list of the ACPD manuscript.

Similarly, “above sea level (asl)” is already defined on line 26 of the abstract. The term “UAV” was indeed not defined in the .pdf we originally submitted, but is defined in the on-line ACPD-version of the manuscript. We have therefore not altered the ACPD-version.

And sorry, but in the on-line ACPD-version of the manuscript, both of these time references are given as 1 min; again, the inconsistency was picked up at the proofs-stage.

Lines 172 and 173 have been amended to: “The structure of the BrO VCD observations, with high VCD evident at the edge of the satellite swaths, suggest that areas between the satellite swathes (as well as further poleward) where no data are reported, also have some enhanced BrO.”

The inlet set-up was designed to eliminate snow/ice in the inlet as far as possible. Its effectiveness was tested during the winter on the Cairngorm mountain in Scotland (the closest we have to Antarctic conditions in the UK – cold, snowy and windy). Under these conditions, the inlet set-up was effective. In answer to the question, no, snow and ice accumulation was not monitored by these remote systems in Antarctica – I’m not even sure whether/how this could be done. However, we have been extremely careful not to over-interpret our data, and any biases of a few percent would not negate the

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conclusions of this paper.

Zero ozone measurements were taken at the start and the end of the deployment period, as described in Bauguitte et al., 2011 (section 4.1), but not during autonomous operation. This information is now included in section 2.2: ""Zero ozone measurements were taken at the start and the end of the deployment period, as described in Bauguitte et al. (2011)."

The 2B Tech instruments were modified so as to work at -40oC – they were not simply off-the-shelf instruments. As a result of these modifications (described in Bauguitte et al., 2011), they were quicker to stabilise and were tested for this in freezers at BAS pre-deployment. Noise was found to be significant over the first 3 minutes, but generally after that time, the signal to noise was sufficient to produce valid data. When this was not the case, additional filtering was done, as described in the paper. To address this point, we have amended the manuscript to read: "Following such down times, the instruments required a short time for the signal to stabilise again. Such recoveries were aided by instrument modifications that had been made to deal with the low anticipated ambient temperatures (described in Bauguitte et al., 2011)."

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27555, 2012.