

## ***Interactive comment on “Airborne lidar measurements of surface ozone depletion over Arctic sea ice” by J. A. Seabrook et al.***

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### **Major Comments:**

This manuscript should be published after revision. In particular, the authors should qualify the claims that no ozone depleted air was observed aloft with the statement that this was from an extremely small sample of data.

The authors should take care: a hasty reading of lines 10 to 17 of the Conclusions would suggest that they claim that the interpretation of McElroy et al (1999) is unfounded. They do not actually say this, but the tone of the writing, the word “suggested”, and the juxtapositions, all contribute to this impression. It is very unfortunate that this is based on less than 2 hours’ data.

C30

It is yet more unfortunate that the interpretation by McElroy is in fact very well founded, and has been substantiated by others in the Arctic and Antarctic – see my own work at Atmos. Meas. Tech. Discuss., 5, 5419–5448 (2012), the review by Ross Salawitch that accompanies it, and the references within each.

Hence the statements in the Introduction (p1437 lines5 to 9) and Conclusion (p1443 lines 10 to 17) should be amended to include references to this other work, to emphasise the shortness of the authors’ data set, and to remove the unfortunate tone and phrasing.

### **Minor Comments:**

p1443 line 13 – if there is more data, why not show it? The strength of my points above would be diminished if the extra data was convincing and on widely separated dates. And there is plenty of space, this is currently a short paper by ACP standards.

p1437 line7 – the thesis of McElroy et al (1999), like that of Roscoe et al (GRL 28, 2911–2914, 2001), is not that ODEs can occur in air not “directly” in contact with sea ice. Rather, it is that ODEs can occur in air not currently in contact with sea ice. Hence “currently” or “immediately” should be inserted in here and in p1443 line11, and the tone of the arguments amended.

p1439 – does the signal scattered from the ground not tend to saturate the PMTs so that they remain affected during reception of the following laser pulse? I would assume that authors of such high standard of technical excellence as these, have satisfied themselves on this point. But a few words of reassurance here might be appropriate.

### **Editorial Comments:**

p1436 lines 5 and 6 – use one term for surface, not two. Furthermore “ground” is inaccurate for the surface of sea ice.

p1436 lines 16 to 19 – “e.g.” should preface these references, as there are many more.

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p1436 lines 22 and 23 – “bromine” should be all lower case, and they are bromide ions not bromine ions.

Fig. 2 – the back trajectory line cannot be seen on the figure, and the caption should include the times and dates of the flight segments.

Fig. 3 – the colour scale should be expanded so we can tell 10 ppbv from 20 – the top 15 ppbv of the existing scale are not used. And the caption should tell us what time is on the top axis – is it GMT, solar time, etc?

Fig. 6 caption – the date style differs from all others; and A–C also corresponds to labels in Fig. 5; and we should be told what are the various colours and symbols (presumably the heights of Fig. 7).

Fig. 7 caption – A–C also corresponds to labels in Figs. 2 and 5.

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