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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Temporal and spatial characteristics of ozone depletion events from measurements in the Arctic" *by* J. W. Halfacre et al.

Anonymous Referee #2

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This paper reports results from the Arctic O-Buoy program and uses them to make statements about temporal and spatial features of ozone depletion events. It is generally a well-written paper and suitable for publication in ACP once a few things are modified/corrected.

Page 30237 around line 15: true, but there must be salinity in these snowpacks; this point should be made.

Page 30240 line 6: spell out what you mean by "long-term" – these are of the order months, not years.

Page 30242 line 19: Fayt et al is not in the reference list

Section 2.3: I find this section quite confusing in the way that it's written. I think I know what you've done, but it's hard to extract that information from the text. Surely you've run the trajectory according to the duration of the observed ODE. You should then calculate the spatial scale according to the distance between the start and end point of the trajectory. The text (and figure caption for Fig 4) talk about determining the ODE spatial dimension by calculating the maximum distance between any two points along the trajectory. The points along a trajectory are arbitrary, and determined by the resolution of the output. Do you mean that you integrated all these distances..? That would be fine, but it's not what I see shown in Fig 4. Please carefully clarify this section. Also, this entire analysis rests on the observed ODE reflecting transport, rather than local chemistry. Please make this point in the text.

Page 30247 line 6: amend the text to read "38 ODEs were observed between the months of February and June" – because they were not all in the same year.

Page 30247 line 10 – it would be good to show an example plot for 1 case

Page 30247 line 14 – that the lifetime of O3 reached 14 days is not shown in Fig 6a, as this information is embedded in the bar >50 hours. Adjust.

Page 30247 line 18 – ranging from 0.24 to 7

Page 30247 line 20 to 25 – you write what the >50 hour events are not due to – could you suggest why they might be so extended..?

Fig 6: this analysis depends critically on how the ODE start/end time is defined. Did you try other definitions? How sensitive is your result to the choice of definition? Also, please make the legend box on the figure larger!

Section 3.1 – it troubles me that much of the work in this section refers to work by Stephens that is either an PhD thesis form (and thus very hard to get hold of) or in unpublished papers. Have these papers since been published? In particular, the number

3.6 is critical to this paper, and the reader has no way of independently checking it.

Page 30249 line 24 – how sensitive are the results to the assumption that temperature was 248K?

Page 30251 line 15 – keep references to unpublished work to a minimum. Unless now published, remove the Stephens et al 2013a reference here, and use the Saiz-Lopez et al 2007 Science paper (Science, 317, 348-351, 2007). Certainly in the discussion below (line 16 onwards), use the Saiz-Lopez paper, where they report a 4-fold increase in calculated surface O3 loss rates when including IO as well as BrO in their photochemical box model. This is equivalent information to the Stephens et al paper, but it is already peer-reviewed and published.

Page 30251 line 25 – state that it's the enhanced salinity of first year sea ice that could be the reason for enhanced chlorine...

Page 30253 line 22/23 – Fig 8 shows that the median was $\sim$908km, not the mode

Page 30253 line 28 – amend text to "The results presented here..." rather than "These results..." which could be taken to mean the results of Ridley et al and Jones et al.

Page 30254 line 6 – 341 km

Fig S2 – the paper talks, throughout, of the 17 events, but Fig S2 shows 18...

Page 30256 line 2 – which OB1 deployment, 2009, 2010 or both?

Fig 10 – what would the wind rose look like for air masses with no ODE? i.e. is it the wind direction per say that matters, or the sea ice conditions which the air passes over..?

Section 3.3 paragraph 1, and fig 11 - Did you filter your temperature data according to wind speed, so that you looked only at temperatures when wind speeds were low? i.e. to remove temperature data when depleted air masses were being transported, and to focus in on local depletion conditions. Also, why were average temperatures

examined from Hysplit trajectories..? Surely you need to look at the extremes, i.e. the minimum temperatures that the air mass experienced. This information is lost when you calculate averages. Also, state the height of the trajectories – were they all close to ground level..?

Section 4 – update the conclusions depending on what you find when addressing the issues raised above.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 30233, 2013.