Atmos. Chem. Phys. Discuss., 15, C5349–C5353, 2015
www.atmos-chem-phys-discuss.net/15/C5349/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Oceanic bromine emissions weighted by their ozone depletion potential" *by* S. Tegtmeier et al.

Anonymous Referee #2

Received and published: 29 July 2015

This paper presents ODPs for bromoform, and as such is interesting for the readership of ACP. This is obviously a complex topic but the authors have not adequately addressed or discussed this complexity. The science presented is incomplete. The introduction and abstract are poorly written, miss some significant points with regards to the complexity of convective changes with climate change and omit significant papers in the field concerning VSLS and climate. The contribution of bromoform due to anthropogenic sources (aquaculture) is not separated – and this would be possible, see below. Without these major issues in presentation and science in this paper being addressed I believe this work is currently not of sufficient quality for publication in ACP. The general approach and the aim of establishing emission weighted ODPs makes this work very relevant and I encourage the authors to make these changes in order to

improve the quality and scientific integrity of this work.

[Leedham et al., 2013] state that 2% of current emissions are due to aquaculture in Malaysia, growing to 20% in the next decade. 94% of aquaculture in SEA occurs in Indonesia and Philippines – a more satisfactory discussion, and estimate for ODPs from aquaculture could be presented, making this paper's discussion of anthropogenic ODP's relevant for the Montreal protocol.

Title – the paper does not deal with all oceanic bromine emissions' ODPs – so is overstated – change bromine to bromoform. This is particularly important for inorganic product gas washout, which is quite different for CH2Br2 relative to CHBr3. [Liang et al., 2014] discuss how the different wet deposition processes of weaker convection favours CH3Br versus CH2Br2 due to PGI washout. The results presented in this work are therefore only relevant for CHBr3 and cannot be extended to all oceanic bromine as the authors have done.

The abstract is far too general and requires tightening and quantification: in some places is very vague. For example the first sentence: "At present, anthropogenic halogens and oceanic emissions of Very Short-lived Substances (VSLS) are responsible for stratospheric ozone destruction." Is an oversimplification and ignores the roles of N2O, CO2, CH4, water vapour, aerosol etc in stratospheric ozone depletion, which are all very relevant in determination of future ozone (Chap 4, WMO ozone assessment, 2014).

Line 6, page 14644: "Emissions of VSLS are, on the other hand, expected to increase in the future." and the next sentence can be combined and shortened. Or essentially lines 1- 8, page 14644 of the abstract could be removed as really introduction material and inadequately described in this abstract.

Exemplary used throughout could be replaced within a case-study framework, as this is really what is presented in this paper.

Line 22, page 14644: larger convective activity is not descriptive enough or accurate (IPCC chapter 12 states less frequent, more intense convection with increased stability due to a higher tropopause). The explanation given by [Hossaini et al., 2012] is that while mid-tropospheric mass fluxes are depressed the higher tropopause in 2100 increases the 'depth' of the tropical convection and is accompanied by an increased mass flux to UT. This point needs to be discussed more completely in the paper – references within [Hossaini et al., 2012], [Rybka and Tost, 2014] and [Liang et al., 2014] would be helpful.

[Hossaini et al., 2015] is an extremely relevant reference for ozone depletion and the climate implications of VSLS that has been overlooked by the authors. This paper describes the vertical ozone loss seen by bromine, chlorine and iodine VSLS and how it is quite different than that of long-lived halogen species. The ozone losses in the Hossaini paper could be directly compared with the values found in this work. The [Hossaini et al., 2015] paper, combined with the [Tilmes et al., 2012] describe how the effect of VSLS on ozone is amplified under high stratospheric aerosol conditions. This is an important consideration for establishing ODPs for the VSLS that is neglected in the current work, especially as the background loading of stratospheric aerosol has increased in recent times [Solomon et al., 2011].

Page 14646, line 10 The simplistic model of [Schofield et al., 2011] shows that these uncertainties due to emissions inventories are inferior to those of modeled transport or wet deposition processes.

Page 14647, lines 5-15 - ODPs is again introduced in this paragraph, this should come earlier and the repetition of the definitions removed (this would improve the flow of the introduction). Each paragraph should deal with a separate and new point.

Page 14651, line 15 (and elsewhere), only using the updraft mass-fluxes will lead to an error in the concentrations, as shown by [Frey et al., 2015] the downdraft mass fluxes are also very important 5-15% in determining composition at different levels. This will

be important for oxidative capacities and ODPs as ozone is transported downwards from the stratosphere.

Page 14652, line 24 The active chlorine is relevant for ozone loss predominantly in the polar regions, whereas active bromine is relevant at all latitudes (i.e. [Lee et al., 2002] – $\sim$80% of Bry is in radical form at midlatitudes). Therefore even if the assumption that stratospheric circulation would influence chlorine and bromine similarly holds (which it doesn't due to lower and upper branches of the Brewer-Dobson circulation being impacted differently by climate change), the ODPs of chlorine and bromine can not be equated in this way – see also [Hossaini et al., 2015] for the altitude difference in ozone losses between VSLS and long-lived halogens.

Page 14647, line 16 – inside in should be insight into Page 14648, line 7 introduce should be introduced

Discussion would benefit from a comparison with ozone assessment for CHBr3.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14643, 2015.