

***Interactive comment on* “Technical Note: Ensuring consistent, global measurements of short-lived halocarbon gases in the ocean and atmosphere” by J. H. Butler et al.**

Anonymous Referee #1

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The manuscript reports on the outcome of a workshop on organohalogen measurement strategies, which took place in Feb. 2008. The outcome was – in brief: The variability of organohalogen measurement could, at least partly, be due to deficiencies in the measurements and the inter-calibration of instruments. As a remedy intercomparison measurements, basically after the pattern of GAW or CarboEurope, were suggested. Two strategies appeared to be promising: 1) circulate large tanks 2) distribute small containers It was suggested that (despite major potential problems) the latter approach should be preferred, since it promises to yield results much faster than strategy 1).

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This reviewer basically shares the view of the authors that better intercomparison might be needed. However, it appears that the manuscript has very little substance, saying on 5 pages little more than its abstract or the text above. Also, the reason for the recommendation of strategy 2) is not compelling, here more details and a more thorough discussion would be desirable. Also it would be helpful to give a brief list of the target species. Why are chlorine species not included? Are iodine species important when there is virtually no I found in the stratosphere? In summary: The manuscript makes a potentially important point, but fails to give hard evidence that this is actually the case, also details of the approach to be chosen are missing. Moreover there are many minor points (see below) that make the manuscript unnecessarily hard to read. The figures could probably be deleted. I suggest major revisions of the manuscript.

In detail there are several points that need attention: 1) The terms “long lived”, “short lived”, and “very short lived” are not defined (except “short lived”, meaning less than about 6, but no reference given). 2) p. 11288, line21: “. . . sizeable fraction of oxidation . . .”, oxidation of what? 3) p. 11289, lines 3-6: This sentence is grammatically not correct. 4) p. 11289, line 14: Figures 1, 2 show many measurements at different sites and times, they do not provide information on the variations being real or instrumental artefacts. One Fig. should be more than enough to illustrate the amount of variability. 5) p. 11289, line 18: It is argued that better organohalogen measurements are required to quantify the relative contributions from organic/inorganic sources. But are the inorganic halogen sources any better known than the organic ones? 6) p. 11289, lines 22/23: “. . . calibration issues continue to limit the ability . . .” this is a central statement, but no evidence for it to be true is given. 7) It is not made plausible that the observed spatial and temporal variations of measured organohalogen levels are actually due to instrumental shortcomings (rather than being a natural phenomenon). 8) p. 11291, line 3: literature reference missing. 9) p. 11291, lines 11ff: Properties of “small containers” are not given.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11287, 2009.

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