

## ***Interactive comment on “Long term halocarbon observations from a coastal and an inland site in Sabah, Malaysian Borneo” by A. D. Robinson et al.***

**Anonymous Referee #1**

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The article presents halocarbon data collected over a year from two sites in Borneo, South East Asia. The article and particularly the data set is likely of interest to a number of other scientists in the field. A brief description of the instrument and some hypotheses of the trends observed are given in the text which help to present this neat, compact study and perhaps more importantly highlights this valuable data set to other scientists in the field. I recommend this paper be accepted and published, but ask the authors to consider the following (minor) changes which may help to make the paper clearer to other readers.

Suggested changes/comments:

Page 1922, line 25: “On balance it seems likely that the short-lived bromocarbons are

C175

responsible for the apparent deficit.” It should be more clearly stated whether there is consensus among the referenced authors that the short-lived bromocarbons are responsible for the deficit or if this is the summation of the authors here.

Page 1924, lines 4-6: Delete the sentences “In Sect. 2.1 we briefly describe the instruments used for this work and discuss how we monitored instrument performance throughout the deployment. In Sect. 2.2 we discuss the calibration methodology used for each instrument.” This summary of the methodology section is not necessary, the titles make this clear enough.

Page 1924, line 12: “Briefly, each sample is pre-concentrated using a Carboxen trap.” Specifically which Carboxen adsorbent is used (mesh size etc)?

Page 1925, lines 3-9: It should be clarified how many sites the instruments were operated at Bukit Atur, Danum and Tawau are all mentioned here yet only Bukit Atur and Tawau are described in more detail in the field deployment section on pages 1927-1929

Page 1926, line 6: “are always completely retained on the lighter Carboxen at the front of the adsorbent bed” Is this a multi-adsorbent trap? If so this should be stated in the brief instrument description given earlier along with the type of Carboxens used (see earlier comment).

Page 1932, line 21 onwards: Discussion of the discrepancy between measurements of perchloroethene at the Bukit Atur and Tawau sites during the first few months of instrument deployment is explained by a drift on the calibration gas used at the Bukit Atur site. This does indeed appear to be the most likely explanation for the differences observed, however, little text is devoted to these differences within the text when the time-series and seasonal averages are explained. The text should make it clear that the perchloroethene reported at Bukit Atur between December and March is likely an upper estimate of the actual mixing ratios at that time. This should be reiterated when describing the seasonality in figure 8 otherwise the text and plots could be considered misleading to the reader.

C176

Pages 1937 line 24 to page 1938 to line 7: During discussion of correlation between CH<sub>2</sub>Br<sub>2</sub>\* and CHBr<sub>3</sub> varying between months/different periods the authors should mention the possibility of interference from a changing CHBrCl<sub>2</sub> component of the CH<sub>2</sub>Br<sub>2</sub>\* signal. If this is not thought to give any significant interference then it should be stated why in this section.

Page 1938, section “5.4 Methyl iodide”: In previous sections where other compounds measured have been discussed, an approximate atmospheric lifetime of each compound was given, this would be useful here, making the sections more consistent and help the reader to better interpret the results.

Page 1940, line 20 onwards: The authors should exercise care here and consider re-writing some of the text. The fact that the larger variability observed at the coastal site is not captured by the model is ultimately a short-fall of the model and its insufficient resolution. The better comparison of measurement and model at the inland site is indeed encouraging, but the differences at the coastal site are equally important and shouldn't be disregarded. A sentence or two to clarify this would be worthwhile here.

Page 1941, line 6: The authors introduce the acronym VSLS without definition. While this term is common in the field it should be defined before first use in the text.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 1919, 2014.