

Interactive comment on “Bromofom in the tropical boundary layer of the Maritime Continent during OP3: the contrast between coast and rainforest” by J. A. Pyle et al.

Anonymous Referee #2

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Summary.

Pyle et al. present measurements of bromofom (and C2Cl4) spanning a number of days from two sites (coastal and rainforest) in Borneo. They interpret the measurements in terms of similarities and differences observed between the two stations, and conclude that background bromofom concentrations at the coastal site (during the 3-4 days of sampling) were somewhat higher than at the rainforest site, but that excursions above background were dramatically higher, indicating a nearby/local coastal source. The enhancements did not correlate with C2Cl4, consistent with a non-anthropogenic

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source. The authors go on to compare their measurements with output from an Eulerian and a Lagrangian model, and conclude that both are unable to reproduce the high CHBr3 enhancements at the coastal site, but that they need to scale down the emissions in the Eulerian model by 2-6X to match the observed background levels.

The paper presents interesting data and the topic is appropriate for ACP. It is fairly well written. I see it as borderline in terms of whether the material and analysis as presented are sufficient to merit a standalone paper. The authors refer in the text to ongoing measurements, and that they now have 2 annual cycles of measurements from Borneo (the same inland site and a different coastal site). I find it confusing that the authors don't choose to include any of that information, and instead carry out a limited analysis based on just a few days of data.

Comments.

Abstract. “a lower global source of 190Gg Br/yr”. Given that the spatial and temporal distribution of bromofom emissions is not well-constrained, the authors should steer away from giving a global total based on these limited data in one location. Warwick et al. (2006) showed 7 emission scenarios with varying spatial distribution; picking one of these and scaling it down to arrive at a global flux estimate based on these few days of data is not really useful. The overall point the authors are making is fine “... point to the difficulty for short-lived species of extrapolating local measurements to a global scale”. Do any of the other Warwick scenarios perform better?

Section 2.2. The model description is inadequate, particularly for TOMCAT. Without saying what's driving the models, the data-model comparisons turn into a non-informative black box exercise. You don't need to list all model details, but the salient ones should be included: how are the bromocarbon emissions computed / distributed? Resolution? Time step? How do we know the OH levels are reasonable?

In several places there is a lack of numbers or statistics that are needed to back up statements in the text. For instance, - P14978, L20-21. “the concentrations at Danum

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are usually less than at Kunak, sometimes by a factor of two or more". Give numbers, e.g., X% of the time Danum is lower, and on average bromoform at Danum is Y% of Kunak. The statement is not clear from Figure 2. Visually they look very similar, and Danum is higher than Kunak on several occasions. However it's not clear what you mean in the Figure 4 caption about the scaling. - P14978, L23-24, "slightly higher variability", what is that? Give SD or variance. Again this point is not apparent from the figure. - P14976, L27 "do not correlate with the bromoform peaks", this one is actually visible from the figure but you could still give an R value - P14980, L10, "produced identical measurements", need number, e.g. within 5%.

On a related note, P14979 L12-14, "The above has shown ..." these actually haven't been shown convincingly, as per the previous comment.

P14980 L22-23, "substantial gradients between the coast and inland can be expected..." I can maybe see this in Fig 5, but not in Fig 4, need to be quantitative in both cases.

P14981 L9 and L11, "good agreement", "agrees well", what does this mean?

Technical comments.

P14971, L23-24. Production is not controlled for all halocarbons, rephrase

Section 2.1. Mention a few key details about the instrument, e.g. adsorbent(s) and column(s) used.

P14977, L6, "and C2Cl4", phrasing is misleading since you don't look at C2Cl4 with the model

P14978, L22, "the modeled difference increases if a coarser grid is used", but earlier you said "lower resolution would not allow us to distinguish between Danum and Kunak" so this is confusing, is it a typo?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14969, 2010.

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