Biogeosciences Discuss., 10, C189–C191, 2013
www.biogeosciences-discuss.net/10/C189/2013/

Biogeosciences Discussions

Interactive comment on "Emission of atmospherically significant halocarbons by naturally occurring and farmed tropical macroalgae" *by* E. C. Leedham et al.

Anonymous Referee #2

Received and published: 3 March 2013

Summary

This paper is generally well-written and contains new data concerning the halocarbon production by tropical macroalgae. The data should be an important contribution to understanding the complexity and variability of biogenic halocarbon emissions. I only have one major concern as described below. I recommend publication, after main concern and some other minor comments have been addressed.

My major criticism of the paper is that the emission estimates of CHBr3 and CH2Br2 from the coastlines of Malaysia and South East Asia are based on rather uncertain values and many assumptions, and so the conclusion is questionable. First of all, the

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

production rates of CHBr3 and CH2Br2 from tropical macroalgae (supplementary Table 1) are highly variable in each class of phaeophytes, rhodophytes and chlorophytes. However, the authors use their average to calculate a production rate per unit area (378 nmol CHBr3 m−2 h−1) without any statistic evaluation . From supplementary Table 1, I calculate the relative standard deviations for CHBr3 production at t24 to be as high as 81%(rhodophytes), 147% (phaeophytes) and 146% (chlorophytes). The errors could be larger, taking the effect of incubation time (t4 and t24) into consideration. Since the value (378 nmol CHBr3 m−2 h−1) is used in all the scenarios, the following calculation illustrates the problem.

Specific Comments

1) p.491 line 20

Each production rate should be given with an error due to the variable incubation results.

2) p.493 line 26 “Fresh weight may provide a more accurate basis for scaling up to emission estimates . . .”

Please state the reason for this.

3) p.495 line 11 “ . . . the ratio of CHBr2Cl : CHBr3 decreases from 18 : 1 at t4 to 11 : 1 at t24.”

With shorter lifetime of CHBr3, the ratio of CHBr2Cl : CHBr3 should increase by time.

4) p.495 line 17

The lifetime of CH2I2 in the seawater is reported to be only 14min (Jones and Carpenter, 2005). Are you sure that the decomposition is negligible in your 24-h incubation under constant illumination?

5) p.497 line 5 “Of these, 5 papers expressed results only per gram of DW and so were removed.”

BGD

10, C189–C191, 2013

Why do you remove those data? You also have the production rates on DW base to be compared (p.489 line 8) .

6) p.498 line 2 “The percentage standard deviation was similar for both halocarbons.”

Please show the number of percentage standard deviation for each halocarbon.

7) p.501 line 14 “. . . For these reasons, our ability to use local biomass data is of distinct benefit to the following estimates.”

Is this true in the scale-up to the whole South East Asia?

8) p.501 line 25 “We averaged production rates for phaeophytes, rhodophytes and chlorophytes . . .”

Please give the error estimates for the averages.

9) What is the point of Conclusion 3? The paragraph could be more concise.

10) Conclusion 4

Is this true after all the errors are considered?

11) The authors first calculate the average production of bromoform from incubated macroalgae, then estimate its concentration in the water, and finally its sea-to-air flux. I wonder why the authors don’t measure bromoform concentration in the water, which should give more direct and reliable estimate for the flux. The water data should also be useful to evaluate the appropriateness of the assumptions for macroalgal biomass and potential bromoform production in the tropical coastal water.

Interactive comment on Biogeosciences Discuss., 10, 483, 2013.