

Interactive
Comment

***Interactive comment on* “Impact of biomass burning on surface water quality in Southeast Asia through atmospheric deposition: field observations” by P. Sundarambal et al.**

Anonymous Referee #1

Received and published: 12 April 2010

Review: “Impact of biomass burning on surface water quality in Southeast Asia through atmospheric deposition: field observations”

General/recurring comments:

Phosphorus is generally not a limiting nutrient in coastal/marine waters. If phosphorus is limiting in the marine systems discussed here, please provide a reference to that effect.

Be careful with significant figures. In several instances, more significant figures are reported that are probably warranted.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Overall, the manuscript suffers from poor clarity and is difficult to read. Suggest revising to make it easier to read.

Superscripts: throughout the document, superscripts of chemical compounds (e.g. NO₃) are missing.

Specific comments:

How long were samples stored at 4 degrees C? Nutrient samples are more commonly stored frozen (-20 C) unless there is a very quick turnaround time (<24 hours) for analysis. What were the filters stored in? What were the HPDE bottles cleaned with (HCl and DI)?

What QA/QC procedures were used for both lab and field methods? Were field blanks used to insure that samples were not contaminated (see comment below regarding P).

Page 7749, line 3: “millions” should be “million”

Page 7753, line 26: “Dominance of” should be “Dominance by”

The sections on DAD and WAD flux calculations are overly complicated and could be simplified, especially the WAD section.

It is surprising that organic P dominates the P flux, even during non hazy conditions (p. 7762, line 14 and Figures 5, 6). It is most commonly reported in the literature that either atmospherically deposited P is generally low, or that it is dominated by mineral P (i.e. dust). Do the authors have an explanation for why OP is high, even during non hazy conditions? Others have used organic P as an indicator for sample contamination (e.g. bird droppings, pollen). Without any explanation for why OP would be high, I'd be suspicious that you are seeing contamination here.

It seems like the WAD flux numbers are based on a very small number of samples (p. 7759, line 24), i.e. 6 samples total. I would be very careful about drawing conclusions from such a small number of samples.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



In the reference section, there are at least two journal titles which are wrong. First, “The Science of the Total Environment” is just “Science of the Total Environment.” (p. 7768, line 8). Second, “Environ. Interpret” is “Environment International” (p. 7769, line 22).

Table 1: It’s not clear to me why the seawater concentrations are being shown here. I assume it is to show that the concentrations of DAD are greater than those in seawater to show its importance. The concentration of the aerosols aren’t the critical factor to determining if it’s important, it’s the flux rate. If these are being shown for stagnant boundary level reasons, this will not be the determining factor in determining depositional velocity in marine systems. Suggest either removing the numbers for seawater, or better explain why this is important.

Several figures (3,4,5,6) are very small and hard to read.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7745, 2010.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

