

## ***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 #3**

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### General Comments

The paper described some interesting data regarding nutrient deposition in response to biomass burning in Southeast Asia. I would consider changing the title ‘Impacts of biomass burning on ocean water quality.....’ since ‘surface water’ could include freshwater bodies such as lakes whereas the main focus is on oceans. I believe the manuscript could be substantially improved through a number of editorial changes since it is often difficult to read. Oftentimes, the text exactly repeats the numbers that are presented in Tables/Figures which is unnecessary in my opinion. The results would be much easier to read by focusing on the main trends and patterns. Oftentimes,

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chemical species are abbreviated or completely spelled out. I would suggest to use the abbreviations throughout the text after defining these as soon as possible; i.e., ammonium (NH<sub>4</sub>). The introduction could use some restructuring since the fact that nutrient inputs into oceans are important for water quality including eutrophication is repeated several times; once is enough. I get the feeling the authors are overstretching their data a bit since the number of samples/sample periods is very small so extrapolating these results can be a bit risky. I was not sure why seawater concentrations were presented in Table 1 since aerosol concentrations are expressed per square meter of air if I understand it correctly while seawater concentrations are expressed per liter of water. I would suggest removing these from the Table unless you can connect the two better. Also, I would suggest transforming Table 1 into a figure using box and whisker plots if you want to retain most of the information presented. Throughout the text the authors take some liberty with the use of significant digits where the means sometimes have fewer significant digits than the standard deviations. I suggest being consistent with the number of significant digits based on the precision of the measurements. The order in which the figures are discussed is inconsistent with the order in which the figures are numbered. For instance, figure 7 is discussed before figure 5 and 6 and there are other examples like this. This has to be resolved. Finally, the figures are small and often hard to read.

### Specific comments:

Page 7746: Consider rewriting lines 14-19 since these are a bit difficult to read. I suggest to describe the TN and TP data into separate sentences.

Page 7747: Line 11 seems redundant.

Page 7747: Line 24-26 seems redundant

Page 7748: Line 3-5 seems redundant

Page 7748, line 9: replace ‘by’ with ‘derived from’

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Page 7748, line 20: Delete '137 km north of the equator'

Page 7748, line 22: replace 'around' by 'surrounding'

Page 7749, line 5: What do the authors mean by 'maximum wind speeds ranging from 5-10 m/s?'

Page 7749, line 7-8: I would just simplify this sentence by giving latitude and longitude; the 'North of the Equator' and 'East of the Prime Meridian etc.' are redundant.

Page 7749, line 17 and further: This paragraph seems presented awkwardly and the order in which the methods are described does not seem logical.

Page 7750, line 3: Replace 'the lowest readability' with 'detection limit'.

Page 7750, line 5 and line 12: How long were samples stored before analysis?

Page 7750, line 9: Were bottles pre-cleaned with DI water, HCl, or both?

Page 7750, line 11-12: I assume you mean to say that precipitation events yielding less than 1 mm of rain were not considered in this study. If so, you probably need to rewrite the sentence since it is a little cryptic. Also, what do you mean by 'analytical convenience'?

Page 7750, line 14-17: How far was the meteorology station removed from the sampling site?

Page 7750, 7751 section 2.2.1: Here I would define the chemical name, i.e., NH<sub>4</sub>, NO<sub>3</sub>, TN, etc. and use those throughout the remainder of the paper.

Page 7751, line 17-18: What was the temperature of the ultrasonic bath and what was the ambient temperature?

Page 7752, section 2.3.1: There seems to be an awful lot of detail in the description of the dry deposition calculations. If it is a standard method than I don't believe all the background information is needed.

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Page 7753, line 6: What do you mean by 'most limiting' parameter. Is that the same as 'most uncertain'? If so, use the correct terminology.

Page 7753, line 20: Replace 'metrological' by 'meteorological'.

Page 7753, line 24 to Page 7754, line 8: This seems more appropriate for the results and discussion section.

Page 7754, line 13: m is not a rate. I assume it may be annual precipitation. If so, please clarify.

Page 7754, line 21-26. If I understand this section correctly, the cumulative precipitation during the study represents approximately one third of the annual precipitation. It was not clear to me however how annual fluxes were calculated and why is the average annual precipitation rate mentioned?

Page 7755, line 1-13: How does the information presented here relate to this study? This probably would fit better in the results and discussion section. It is still not clear to me how in this study annual fluxes were calculated given that no year-round measurements were taken.

Page 7755, section 3.1. It seems like the first paragraph of this section can be condensed quite a bit especially those related to the PSI and the API since other data sources are used. It seems like the patterns from the different sites are saying similar things so it could probably be simplified. Also, Fig 3c is missing.

Page 7756, line 19 and line 24-25: What do you mean by 'intermediate rainfall'? Also, the observations made referring to fig 4 are hard to judge since the figures are small. Finally, the figure does not show fire activity and intensity as far as I could tell.

Page 7757, line 8 to page 7758, line 12: This section is a little difficult to follow since the figures are very small and hard to see.

Page 7758, line 10-12: Do you have any evidence to support this statement?

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Page 7758, line 18: Why do you mention seawater concentrations? Not sure why you are including these. I'd suggest deleting these from the text and Table 1.

Page 7758, line 19-23: All this information is presented in Table 1 so I would not repeat this in the text but rather focus on trends. In addition, it may be worthwhile converting Table 1 into a Figure (for instance using box and whisker plots) for easier comparison between haze and non-haze concentration similar to the figures related to rainwater concentrations.

Page 7758, line 23 and further: I would first discuss nutrient concentrations in rainwater before discussing the ratios. The discussion on ratios appears to come too soon. In addition, how do you interpret the differences in ratios between the various nutrients? Also, without error bars it is hard to determine if these differences are significant or not.

Page 7759, line 1-4: This information is already presented in the table. Focus on the main patterns rather than repeating the information presented in the table.

Page 7759, line 9-18: Check the significant digits in this section. There are inconsistencies between averages and standard deviations as well as between different elements.

Page 7759, line 19: The order presented for the N species is not consistent with the patterns displayed in Fig. 5.

Page 7760, line 15-19: In this and other sections you discuss the differences between nutrients, yet there is no discussion why these patterns are present. Perhaps it is difficult to explain but without an explanation this is left hanging a bit.

Page 7760, line 25-27: You mention that the precipitation measured during the study period was about 800 mm which represented approximately one third of the annual precipitation. Again, it is unclear how you come up with annual fluxes.

Page 7761, line 16-19: I was not sure how this statement related to the rates presented in the figures. Especially since one third of the rainfall fell in about one third of a year so calculating fluxes for a whole year should yield about the same flux expressed per

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year. Again, this goes back to the previous comment relating to my confusion on how fluxes were calculated.

Page 7761, line 24-27: Your explanation sounds reasonable but can you really make this conclusion based on such a short measurement period?

Page 7763, line 27-28: This is the first time you mention the companion paper. It would probably be good to state it in the introduction that this paper is part of a two-paper set.

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

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