

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

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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

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would be much easier to read by focusing on the main trends and patterns. Oftentimes, 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_4). 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 overstressing 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.

Authors’ response:

We thank the reviewer for the constructive comments.

We have changed the manuscript title to “Impact of biomass burning on ocean water quality in Southeast Asia through atmospheric deposition: field observations.”

The clarity of the manuscript is improved by making the necessary editorial changes and removing repeated contents in the text. The chemical species are abbreviated in

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the text when they appear first and then the abbreviations are used throughout the text.

The introduction is restructured by making the text concise.

The concentration of seawater was shown to indicate the baseline seawater concentration of nutrients and to compare the trend with that of atmospheric nutrients. The seawater concentration is now removed from Table 1. It is explained in the companion paper "Impact of biomass burning on ocean water quality in Southeast Asia through atmospheric deposition: eutrophication modeling".

The number of significant figures is checked for their consistency, and they are revised where needed to be consistent with the precision of the analytical measurements.

The order of the figures discussed in the text is checked for its consistency and corrected accordingly.

The figures in the manuscript are replaced by clear figures with a larger size to improve their readability.

Specific comments:

Comment 1: 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.

Authors' response:

The sentences are rewritten as per the reviewer's suggestion.

Comment 2: Page 7747: Line 11 seems redundant.

Authors' response:

The redundant line 11 is removed.

Comment 3: Page 7747: Line 24-26 seems redundant

Authors' response:

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The redundant line 24-26 is removed.

Comment 4: Page 7748: Line 3-5 seems redundant

Authors' response:

The redundant line 3-5 is removed.

Comment 5: Page 7748, line 9: replace 'by' with 'derived from'

Authors' response:

It is replaced in the revised manuscript as suggested.

Comment 6: Page 7748, line 20: Delete '137 km north of the equator'

Authors' response:

It is deleted

Comment 7: Page 7748, line 22: replace 'around' by 'surrounding'

Authors' response:

It is replaced.

Comment 8: Page 7749, line 5: What do the authors mean by 'maximum wind speeds ranging from 5-10 m/s'?

Authors' response:

The above range represents the mean daily maximum wind speeds from the SW and NE wind directions. It is clarified in the revised manuscript.

Comment 9: 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.

Authors' response:

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The sentence is rephrased as suggested.

Comment 10: Page 7749, line 17 and further: This paragraph seems presented awkwardly and the order in which the methods are described does not seem logical.

Authors' response:

We apologize for this mistake. The paragraph is rewritten logically.

Comment 11: Page 7750, line 3: Replace 'the lowest readability' with 'detection limit'.

Authors' response:

It is replaced.

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

Authors' response:

Although -20° C storage is optimum for long term preservation, we observed that 4° C is sufficient for short term storage. However, our samples are analyzed as soon as possible, within 1 or 2 days. Our QA/QC protocols are discussed in our previous publications (Karthikeyan and Balasubramanian, 2006; Karthikeyan et al., 2009).

Comment 13: Page 7750, line 9: Were bottles pre-cleaned with DI water, HCl, or both?

Authors' response:

HPDE bottles (250 mL, Nalgene®) were used for storing the homogenized rainwater samples. These bottles were filled with 2 N HNO₃ for 3 days and subsequently with ultrapure water for 3 days. Finally, they were rinsed with ultrapure water three times and kept safely in plastic bags until use. No residual nitrate was found in the rinse water after washing. (Karthikeyan et al., 2009)

Comment 14: Page 7750, line 11-12: I assume you mean to say that precipitation

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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'?

Authors' response:

The sentence is rewritten for improving its clarity.

Comment 15: Page 7750, line 14-17: How far was the meteorology station removed from the sampling site?

Authors' response:

The distance between NUS and SJI is less than 10 km. The map in Figure 2 is replaced by a new map with an appropriate scale in the revised manuscript. The precipitation rates at the two sites are similar during storm events.

Comment 16: Page 7750, 7751 section 2.2.1: Here I would define the chemical name, i.e., NH₄, NO₃, TN, etc. and use those throughout the remainder of the paper.

Authors' response:

In the revised manuscript, we refer to the chemical name of nitrogen and phosphorous species when they appear for the first time and the corresponding abbreviations are used throughout the remainder of the paper.

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

Authors' response:

The temperature of the ultrasonic bath was 60° C and the "ambient" temperature was 27° C. The sentence is modified with inclusion of the exact temperature.

Comment 18: 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

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believe all the background information is needed.

Authors' response:

We provide the most relevant information for dry deposition calculations so that the readers can understand their implications.

Comment 19: 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.

Authors' response:

The sentence is rephrased as "The most important parameters required for estimating V_d are. . . .".

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

Authors' response:

It is replaced.

Comment 21:Page 7753, line 24 to Page 7754, line 8: This seems more appropriate for the results and discussion section.

Authors' response:

We would like retain this information in the current section as it is related to the dry deposition velocity calculation.

Comment 22: Page 7754, line 13: m is not a rate. I assume it may be annual precipitation. If so, please clarify.

Authors' response:

It is corrected in the revised manuscript as "the product of annual precipitation rate (P in m/year)". Please see our response to comment #23.

Comment 23: Page 7754, line 21-26. If I understand this section correctly, the cumu-
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lative 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?

Authors' response:

We thank the reviewer for the valuable comment.

We actually estimated the atmospheric deposition fluxes during the study period (3 months, October through November 2006). Due to an oversight, we indicated the flux per year. Appropriate corrections are incorporated into the revised version of the manuscript. Both DAD and WAD fluxes are now expressed as mg/m²/day in the revised manuscript instead of g/m²/year. The flux calculation formulae in sections 2.3.1 and 2.3.2 are modified and all the DAD and WAD fluxes in Figures and text are also amended accordingly in the revised manuscript.

Comment 24: 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.

Authors' response:

We agree that this information is not necessary. We delete these lines in the revised manuscript.

Comment 25: 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.

Authors' response:

The section dealing with the PSI and the API is condensed and simplified in the revised

manuscript.

The missing Figure 3 (c) is now included.

Comment 26: 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.

Authors' response:

Intermediate rainfall is replaced by rainfall distribution in Fig. 3c; This figure which is currently missing is given in the revised manuscript.

The quality of Fig 4 (a-d) is improved with inclusion of hotspot maps and with a larger size.

Comment 27: 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.

Authors' response:

Please see our response to Comment 26.

Comment 28: Page 7758, line 10-12: Do you have any evidence to support this statement?

Authors' response:

Koe et al. (2001) is referenced to support the last sentence. The last sentence is rephrased more precisely as follows: The biomass burning-impacted air masses contained elevated levels of airborne particulate matter compared to those originated from other sources of air pollution.

Comment 29: 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.

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Authors' response:

The seawater concentration presented in Table 1 is removed in the revised manuscript and it is explained in the companion paper "Sundarambal, P., Tkalic, P., and Balasubramanian, R. Impact of biomass burning on surface water quality in Southeast Asia through atmospheric deposition: Eutrophication modeling, Atmospheric Chemistry Physics Discussion, 10, 7779–7818, 2010.,".

Comment 30: 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.

Authors' response:

We appreciate the reviewer's suggestions. We remove the redundant information in the text and focus on trends in the revised manuscript. We would make an attempt to convert the information in Table 1 into a suitable figure.

Comment 31: 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.

Authors' response:

The ratios are discussed after the discussion on the nutrient concentrations in the revised manuscript. We would reinterpret the differences in ratios between the various nutrients, as suggested.

Comment 32: 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.

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Authors' response:

The main patterns and trends are discussed as suggested.

Comment 33: 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.

Authors' response:

The significant digits in this section are checked and corrected for their consistencies. The significant numbers are decided based on the precision of the analytical measurements.

Comment 34: Page 7759, line 19: The order presented for the N species is not consistent with the patterns displayed in Fig. 5.

Authors' response:

The order presented for the N species is corrected to be consistent with the patterns displayed in Fig. 5.

Comment 35: 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.

Authors' response:

The difference between nutrients is briefly explained in the revised manuscript.

Comment 36: 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.

Authors' response:

Please see our response to Comment #23.

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Comment 37: 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 year. Again, this goes back to the previous comment relating to my confusion on how fluxes were calculated.

Authors' response:

Please see our response to Comment#23.

Comment 38: Page 7761, line 24-27: Your explanation sounds reasonable but can you really make this conclusion based on such a short measurement period?

Authors' response:

Please note that the conclusion derived from a short measurement period is consistent with earlier studies (Balasubramanian et al., 1999; See et al., 2006) according to which most of the chemical species (including inorganic ions) were higher on hazy days as compared to clear days. These air pollution episodes affected the local air quality in Singapore, and contributed to the increase in total nitrogen content in precipitation samples (Karthikeyan et al., 2009).

Comment 39: 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.

Authors' response:

Thank you for the suggestion. We will make the amendment accordingly.

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