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Atmospheric Chemistry and Physics Discussions

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 #2

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General Comments

Overall, the paper was of interest and reasonably well structured. However, I think there are many editorial improvements that could be made to improve its flow and clarity and to maximize the value to the scientific community.

As I read the manuscript, I had recurring thoughts of "Why is this included?" It wasn't until the very end that I realized that this paper was part of a pair of papers that also included modeling. This knowledge explained why some information and statements that seemed out of context were in the paper (e.g., nutrient concentrations in sea water, sea surface roughness). That linkage ought to be noted early in the paper but, even

so, some of the material remains ancillary to the main focus of this paper and could be dropped.

The authors need to be consistent in their use of terms. For example, the primary data segregation is between hazy and non-hazy days or periods, not "haze and non haze periods" as occasionally stated. The number of significant figures must also be used consistently. The mean cannot be to one decimal place and the associated standard deviation to three decimal places. The authors need to review their measurements and methodologies to ensure the proper number of significant figures is reported for each nutrient and deposition type throughout the report.

The lack of context and small size of the figures made several difficult to read and interpret.

Specific Comments

In line 5 of p. 7747, iron is specified as a potentially important nutrient. However, line 14 on p. 7748 only lists N and P species as the objective of the field study. The authors should document/justify why they did not analyze for iron.

In lines 19 & 20 on p. 7748, is this level of precision necessary in describing the location? If so, insert "between" before the first latitude and longitude. If the latitude bounds are given, it is not necessary to specify "137km north of the equator"?

In line 5 on p. 7749, I am not sure what was meant by the range of "maximum" wind speeds. Do the authors intend "mean daily maximum" over various seasons of the year or does the range represent the absolute maximums from the SW and NE wind directions?

In lines 8 and 9 on p. 7749, "N" and "E" are sufficient for describing the latitude/longitude location.

The first paragraph under Section 2.2 seems disorganized (sentences out of sequence – for example, the sentence starting with "TSP" might "fit" better at the beginning of the paragraph and the sentence starting with “The mass” might “fit” better at the end of the paragraph).

In line 23 on p. 7749, I assume the filters were conditioned prior to weighing rather than prior to and after sampling as stated?

In line 11 on p. 7750, 4 deg C is warm for sample storage; how long were samples typically stored before chemical analysis?

In line 14 on p. 7750, how near was the meteorological station (NUS) to the SJI deposition site? No scale was shown in Figure 2. Presumably the precipitation rates at the two sites are similar.

In line 18 on p. 7751, what temperature was the ultrasonic bath and to what “ambient” temperature was the extract cooled?

In line 18 on p. 7752, Wesley and Hicks (2000) could be listed as a reference on dry deposition (M.L. Wesley and B. Hicks, A review of the current status of knowledge on dry deposition. Atmospheric Environment 34 (2000), pp. 2261–2282.).

In line 5 on p. 7754, what is the basis for coarse PM having an upper diameter limit of 18 microns? My understanding is that hi-volume TSP samplers have an upper sampling size diameter of 25-30 microns.

In line 13 on p. 7754, “m” is not a rate. Insert “annual” before “precipitation” and change “m” to “m/year”.

In lines 24 & 25 on p. 7755, the PSI and API indices are introduced. Why not present material and discuss solely from the original concentration measurements and just note in line 22 of p. 7756 that the air quality was moderate or unhealthy during October 2006 (if only used PSI data, confirm that based on TSP measurement)?

In lines 24 & 25 on p. 7756, rainfall is referred to but the figure reference is 3b rather than 3c. Because Figure 3c is missing, it is difficult to know whether the adjective of

the rainfall is “intermediate” as written (vague term) or “intermittent”. Figure 4 is small and difficult to read but does not appear to indicate “fire activity and intensity”.

In lines 28 & 29 on p. 7756, the number of samples for the hazy (4) and non-hazy (16) days analysis are presented. The number of samples would be useful information to provide in the Abstract or Introduction to help guide the reader’s decision to read and, if so, the interpretation and confidence in the results, especially for hazy days as is based on a limited number of samples.

In line 23 on p. 7757, the text refers to “larger fire/hotspot clusters” in Figure 4 but the current scale of the figure makes it difficult to see.

In line 27 on p. 7757, also specify the “low altitude” of transport. Also, at the current scale of the figure, I cannot see that one trajectory came from the Indian Ocean (the two trajectories look similar to me. It seemed to me that the discussion did not follow the sequence of the plots in Figure 4 (i.e., plots not referenced in the same sequence that they were presented).

Beginning on line 10 on p. 7758, the last sentence seems speculative and poorly worded. Can the authors provide some supporting evidence of why they believe this statement to be true and rephrase the sentence more precisely?

Beginning on line 17 on p. 7758, the sentence about concentrations in seawater is out of context. The seawater concentrations in Table 1 are out of context and do not appear to be needed for this paper.

Beginning in line 23 on p. 7758, the nutrient concentration ratios from hazy to non-hazy days requires clarification and more discussion. What is the significance or implication of the rankings? The text appears to be referring to atmospheric concentrations but the cited Fig. 7 shows DAD fluxes. Ratio rankings are again shown at the end of the dry deposition discussion “with respect to their concentrations and fluxes.” The order of the two specie ratio rankings were different and the rankings implied the difference between some specie ratios were significant. However, in Fig. 7 the TN and the NO2+NO3 DAD ratios are not significantly different. A careful review and rewrite of this section is needed.

In line 24 on p.7758, Figure 7 is referenced before Figures 5 or 6 are referenced.

In lines 8 & 10 on p. 7759 and in line 2 on p. 7761, the notations for reactive N and reactive P are not obvious to the lay reader.

In lines 9 through 17 on p. 7759, some of the results have inconsistent significant figures. Also, showing the results in a table might be easier for the reader to interpret and remember.

In line 27, the similarity of nutrient specie concentrations in rainwater and seawater, especially with a limited number of wet deposition samples, could be coincidence. How spatially and temporally variable are concentrations in coastal areas? More evidence and discussion is needed to support the insinuation of the runoff of pollutants after storms affecting concentrations in seawater. Perhaps this is an appropriate place to introduce the modeling effort.

In Table 1, suggest changing “aerosol during hazy and non-hazy days” to “aerosol samples on selected days with hazy (October 2006) and non-hazy (November 2006 – January 2007) conditions”. Suggest changing “Haze” to “Hazy” in the “Period” column. When the table is first referenced, there is no clue as to why seawater concentrations are included. The min ON concentration on a hazy day was 5.15 in the text. Although a reference for the seawater concentrations is provided, it would be helpful to know the number of samples, location of sampling, depth of sampling, and seasonality of samples. In other words, how representative are the seawater numbers and what level of confidence can be placed in them?

In Fig. 2, it would be helpful to have a larger spatial context provided. Is it possible to show (identify) Sumatra and Kalimantan, which are referenced on p. 7755, as well as

the sites mentioned in Fig. 3?

In Fig. 3, Fig (c) is missing. In Fig (a), why not show all data as concentrations, rather than including less informative PSI and API numbers? In Fig (b), how were 3-h PSI values developed (e.g., BAM or TEOM or other continuous PM monitoring method)? Data from how many sites formed the basis of the PSI number? It might be interesting to show a plot of matched 24-hr TSP concentrations from these sites versus the SJI site.

In Fig. 4 (a-c), the geographical context of the trajectory maps is impossible to know without labeling or another map. In Fig. 4 (d), the caption should include a reference as to the source of the graphic or data from which it was generated. Even with enlargement, the “hotspot” locations are difficult to see.

In Fig. 6 (a), an indication must be provided to help the reader understand the spatial extent, representativeness, and uncertainty included in the seawater measurements.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C1862/2010/acpd-10-C1862-2010-supplement.pdf

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