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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: eutrophication modeling" *by* P. Sundarambal et al.

Anonymous Referee #1

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General comments

This paper focuses on the effect of Southeast Asian biomass burning on seawater nitrate levels in the Singapore Strait. The authors sampled nutrient deposition during days they designated as either influenced or not influenced by biomass burning. Based on high- and low-end nutrient deposition data they collected, they ran 20-day biogeochemical model simulations in the Singapore Strait to assess the effects of biomass burning on surface N & P concentrations. For comparison, they also sampled nutrient concentrations in the Strait during days influenced and not influenced by biomass burning.

While this is a very interesting and worthwhile topic, there were some major flaws in the study methodology. Some of the apparent flaws could stem from an incomplete description of the methods and results, and so the authors should more thoroughly describe and substantiate their methods (see specific comments below). In addition, the modeling aspects of the study were incompletely validated. If these issues can be addressed satisfactorily, then the authors can further improve their paper by comparing/contrasting their results with other relevant studies in other regions, and by paying more attention to appropriately referencing their statements. They should explicitly discuss some of the limitations of their assumptions. I would also strongly suggest that the authors get a native speaker to correct the manuscript. Additionally, the manuscript needs to be better organized, as currently portions that should be in the methods or results are in the wrong section (see technical comments).

Specific comments

1) The authors determined whether a day was influenced by biomass burning based on atmospheric haziness alone, which I did not find convincing. The authors should more thoroughly describe and substantiate why they used haziness as a metric for biomass burning. The PSI index was used to define haziness, but the PSI index itself was never defined. The authors should talk about the strengths and weaknesses of using this index. How do they separate pollution haze from biomass burning haze? Pollution is another very important source of atmospherically deposited N and so by using haziness as an index, they may overestimate the impacts of biomass burning. I suggest using other methods to validate their biomass burning day classification. These may include back trajectories coupled with satellite images of point biomass burning sources, aerosol optical thickness, FLAMBE models, etc. If they have any chemical characteristics in their samples that would help trace the degree of biomass burning influence (such as K+), that would also be ideal.

2) The authors sampled changes in concentrations of marine nutrients on hazy vs. non-hazy days, but in the Methods section, they indicated only very little about where

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they sampled. Based on 14 samples taken at an uncertain/poorly described location, time, and depth, a correlation between haziness and surface seawater nutrient concentrations was supposedly observed (although based on Fig. 1, I am not convinced it is a strong correlation at all). From this very small sample size, the authors draw the conclusion that on hazy days, atmospheric deposition is increasing surface seawater nutrient concentrations. However, even if there were a good correlation, a correlation does not indicate a causal relationship. Their interpretation of these data is particularly difficult to believe as a reader because the sampling area is inadequately described. The authors do not provide any indication on how surface nutrients vary normally over time, and thus how we can believe that the change observed was not just normal environmental variation. They should also provide more details about what their analytical precision/accuracy is. Finally, in their modeling results, they see a 1% increase in surface NO3 due to high-end N deposition (I think, see comment #5). A 1% increase is not likely to produce observable differences in surface seawater, and this discrepancy is never addressed.

3) The details about the atmospheric and surface seawater sampling are never provided. We are referenced to many other sources for this information, e.g. on p. 7785, l. 5 they direct us to a companion paper (without reference). Without a reference, its difficult to evaluate whether the data collected were sampled well enough to be even be used in the context of the study. The paper would be better if the authors made it more of a stand-alone work.

4) The model validation section needs a lot of work. For validation, the authors provide a conference proceeding abstract reference (P. 7790, l. 26). They also compare nutrients from a model run from one day with field observations (Fig. 3). However, we don't know the details about that those field observations. The authors only state that these samples were gathered "at a monitoring location in the East Johor Strait." We are not provided with any information about how representative these values are, nor are we told over what model domain the comparisons are averaged over for comparison

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purposes. We are not provided with any information about how good the hydrographic current representations are. The authors should show more model validation data.

5) The authors saw at the most, a 1.36% increase in NO3 "total mass" in the water column (Fig. 4) due to atmospheric N deposition. Firstly, they should explain what they mean by "total mass." This is not a commonly used or intuitive unit (total mass of what?). Secondly, I am confused because in the abstract they say that "computations showed that atmospheric fluxes might account for up to 17–88% of total mass of nitrate nitrogen in the water column during hazy days and 4 to 24% during non-hazy days." On p. 7797 they say that, "atmospheric fluxes might account for an increase of nitrite + nitrate nitrogen concentration in water column in the range of 1–16% (mean 9.3%) and 5–76% (mean 45%) during non-haze and haze periods, respectively." Why are these numbers inconsistent? Because of this confusion, I have difficulty interpreting their statement in the conclusions from p. 7795, l. 5 that "Increased atmospheric nutrient fluxes, even as much as 100 times above the typical atmospheric nitrogen flux, could cause eutrophication in nearshore waters of Singapore and surrounding waters and also areas where tidal action is low"? This conclusion needs much stronger support/explanation (and the sentence itself needs to be re-written).

6) Section 2.2. Although wet and dry deposition data were apparently sampled, it is later stated that only wet deposition was simulated (e.g. p. 7793). The authors should justify why they only simulated wet deposition and they should add something about how this may affect their results.

7) I understand that only NO3 was added to the model from deposition. However, biomass burning is a large source of water soluble organic N, as well as NH4+. The authors should talk a little bit about the uncertainty of excluding these pools of potentially bioavailable nutrients may affect the interpretation of their results.

8) The title is inappropriate for the papers' focus- while biomass burning aerosols presumably enter the study region from all over SE Asia, the oceanographic region of

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focus is only the Singapore Strait. Therefore, it is misleading to indicate that the paper addresses water quality in all of SE Asia.

9) The authors should provide more background in the introduction on the magnitudes of fluxes of nutrients from biomass burning vs. the concentrations of nutrients in the water column.

a. First, how does biomass burning compare with other sources of atmospherically deposited nutrients? The authors state in P. 7781, l.25 that "Most of local knowledge regarding contamination due to forest fires (biomass burning) originates from earlier studies conducted elsewhere, at various parts of the world (e.g., The United States, Australia, Brazil, Mexico, Africa). However, the results of these studies are of little use in assessing the environmental impacts of the resulting pollution since their main objective was to quantify the flux to the atmosphere of various trace gases such as CO2, CH4, and N2O from biomass burning." This is not correct, there are plenty of relevant data that they can and should cite. A more thorough literature review should be conducted and then the authors should discuss in the introduction the relevance of these studies in context of their study region.

b. The authors should also provide the reader with some idea about the existing concentrations of nutrients in the Singapore Strait. They should provide more justification on why it is reasonable to believe that atmospherically deposited fluxes of biomass burning N can affect the existing nutrient pool in this region.

10) The study was based on a 20-day model simulation, but the impact of anthropogenic N inputs to ocean areas will probably have more important long term impacts based on longer-term accumulation rather than on episodic impacts. Why were only short-term changes investigated? The authors may consider mentioning longer-term impacts as a future issue, although not one addressed in this study.

11) Food webs were not really discussed, and so I think the authors should either take any mention of food webs out of their abstract or discuss food webs more in the results

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and discussion/conclusions sections.

12) This study needs to be put in better context of other relevant studies (e.g. Krishnamurthy et al., 2007, 2009, 2010; Zamora et al., 2010, etc.). For example, their observation of P enrichment due to atmospheric deposition is in direct contrast to these other studies. Similar studies modeling the affect of atmospheric nutrient deposition to the ocean and coastal areas should be referenced, and compared/contrasted to the results.

Technical comments

1) As mentioned previously, there are numerous errors in the English. For example, in the abstract, it is “plankton” not “planktons.” However, these errors are too numerous to point out- I leave that responsibility to the authors of finding a native speaker to correct these errors.

2) Section 2.8, p. 7793: The authors state that Case I indicates the effects of physical oceanography, Case III indicates the effects of atmospheric deposition + physical oceanography, and Case II indicates the affects of atmospheric deposition. So therefore Case II=Case III-Case I. Why run Case II in the first place?

3) P. 7782, l. 2: reference?

4) P. 7782, l. 5: “SEA surface waters receive a large nutrient supply of which a substantial portion is of anthropogenic origin.” Reference? Is this for coastal or open ocean waters in SE Asia?

5) P. 7782, l. 6“Accelerated eutrophication and its subsequent effects such as nuisance algal blooms and reduced oxygen levels pose significant problems for coastal waters and aquatic ecosystems in SEA. Algal blooms resulting from complex coupled physical/biological processes are steadily increasing in coastal waters.” Reference?

6) 7783, l. 1: “Besides the advection-diffusion redistribution, a series of terms for the biochemical interactions between non-conservative quantities is considered.” Redistri-

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bution of what?

7) Section 2.1: There is a lot irrelevant information in here, the authors should take out anything not directly related to their study, and add more about the oceanographical/hydrological region of focus. For example, they should talk about the relevant currents, depth of the Strait, whether rivers impact the area, any freshwater lenses that may develop after a rain event to prevent immediate mixing of rain with the rest of water column, etc. They should add latitude/longitude to their map (Figure 2).

8) Section 2.2: The first two sentences belong in section 2.1.

9) P. 7785 l. 16- units of years should not be compared with units of days in the same sentence.

10) Section 2.2. Any discussion of results (e.g. regressions) should not be in the methods section, but rather should be in the results.

11) P. 7788, l. 25: the printed flux equation is wrong, the correct version is: F=settling velocity x concentration. There is no surface area component that I am aware of, and the units don't work out if you add in surface area anyways

12) Section 2.3.2., lines 7-17: this should be in the methods section site description

13) Where is the reference for the 2136 mm/yr rainfall rate?

14) P. 7794, l. 17-23 belongs in the methods section. The authors should state why they used the NO3 concentrations indicated on line 23 and how these values are environmentally relevant, particularly with respect to biomass burning

15) P. 7797 l. 25: reference to Fig. 6 is a typo.

16) P. 7798, l. 1: Put in a qualifier, algal growth in the marine environment is not always N limited.

17) P. 7800, l. 9: how was NEUTRO enhanced and why is this relevant?

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18) The authors state in their conclusions that "It was found that nutrient loading onto the coastal and estuarine ecosystems of the Singapore and surrounding countries from the atmospheric wet and dry deposition during hazy days was remarkable, the contribution being between 2 and 8 times that of non-hazy days." If they still have reason to believe this after more rigorously confirming that those days were primarily influenced by biomass burning, they should state here actual concentrations, compare those concentrations to other locations, and state that this conclusion is based off field data, not model data. Does this conclusion belong in the companion paper?

19) P. 7800, l. 25: "The results of the present study depict that the impacts of nitrogen species through AD onto the coastal region are more significant than phosphorus species." The authors should spend more time talking about P and Fig. 7 if they want to make this point in their conclusions. Otherwise, they should take this out.

20) P. 7801, l. 1: Sewage is never mentioned. Talk about it earlier or take it out.

21) P. 7780, l. 24: "fixed and organic N" doesn't make sense. Organic N can be fixed N- fixed N is N that was originally N2 in the atmosphere but was captured and transformed into reactive N by diazaotrophs.

22) P. 7780, l. 26- Add Mahowald et al., 2008.

23) P. 7781, l. 19- El Niño

24) Table 2: seawater baseline units?

25) Figure 2: Show sampling sites on the map.

26) Fig. 4: "C" for concentration is easily confused with C for carbon/biomass. Use "DIN" instead.

27) Figs. 6,7,8: scale is difficult to read, adjust and make the font larger.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7779, 2010.