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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 #3

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General Comments

I largely found this to be an enjoyable and informative paper. As with many types of ecological models the strength of this paper is not based on the certainty (or lack thereof) associated with the exact absolute output, but rather in demonstrating the potential importance of biomass burning on coastal ocean nutrients and algal growth. This work clearly establishes this as a plausible mechanism and now others can engage in more detailed field sampling to validate the results. This paper suffered from a lack of clearly stated methods. My sense was that the authors have this information but it was just not clearly presented.

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Interactive Discussion

Discussion Paper

Introduction

The case for a possible wide-ranging affect of large-scale biomass burning in areas of SEA on the eutrophication of coastal oceans is justified and clearly stated. A number of statements require literature citations, including page 7781-line 25, page 7782- line 2 and lines 4-15.

Text regarding general modeling principles on page 7782 (lines 22-29) and page 7783 (lines 1-5) is unnecessary. Text on lines 15-22 from page 7783 are also not needed.

Materials and Methods

A detailed, map with sampling sites and geographic features would be helpful. Were all wet and dry samples taken from the same location - did number of samples adequately cover the study period - how many hazy and no-hazy days were sampled? On page 7785 (line 10) how is a hazy day defined for dry deposition. Perhaps it is a matter of semantics, but values in Table 1 are not really deposition, they are aerosol concentrations. Dry deposition is of course modeled using those ambient air values. Is there an analytical distinction made between DON and PON? Should Tables 1&2 be in Methods or Results. Need a citation for the companion paper that is mentioned. Page 7785 (Lines 15-20) - How were deposition rates calculated and is the time period of a year (i.e. $\text{g/m}^2/\text{yr}$) a full calendar year or the September-January sampling period? Again, it might be semantics, but the concentration data do not show a higher AD for dry fallout, of course it all depends on what actually is deposited in the water and what blows by.

Page 7785 (Lines 23-25) - I may have missed this but was the relationship between PSI and wet/dry concentrations established? Figure 1 shows PSI but text says ambient concentrations.

Bottom and page 7785 and top of 7786 - again, much of this may be better placed in results and even discussion (see Page 7786 Line 7-9).

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Not certain that the introduction to the model on Page 7786 (bottom of page) adds to the paper.

This reviewer is not qualified to comment on the specific mathematics used in the NEUTRO Model. However, the expression for dry deposition modeling needs to be explained in more detail. Do the ambient concentrations change over long distances and how is this change accounted for? What about humidity, wind and water surface conditions. Is that state-of-the-art modeling for dry AD this simple? The Model Validation section on page 7791 appears to be limited. It was not discussed well and a single 24-hr validation data set does not seem adequate.

Page 7793, Lines 10-14 - I am confused as to whether this is for the sampling period of the entire year. Samples were only taken for part of the year. Does the 1.835 mg/L value for non-hazy, wet days come from Table 2?

Again, I apologize if it is mentioned in the methods section, but what is the relationship between hazy and non-hazy and wet versus dry. Do you get all four of those combinations in the environment?

Results and Discussion

Page 7794, Line 12 - Flux is not expressed as mg/L. Line 18 - be sure to call this a modeling experiment. What about discussing the possible contribution of TON as a source of DIN after TON is deposited in the water.

Are results in Figure 4 supported by any bioassay experiments in the literature? What about sensitivity to P? Y-axis in Figure should be labeled phytoplankton biomass I believe. Does the phytoplankton respond to P once the steady state response to N occurs?

Page 7795, Line 1 - what is the value for the phytoplankton biomass baseline. Line 11 - what are vertical fluxes? AD can be different since it enters the waterbody from the surface thereby maximizing the potential contact time with phytoplankton.

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Interactive
Comment

Figure 5 - y-axis needs label. Is this nitrate+nitrite? Text on page 7796 (Line 13) implies that Figure 5 represents wet AD. Is this true and if so what about dry deposition? If it is the combination of wet+dry that point should be made clear in the text and in the caption to Figure 5. A better distinction should be made between conservative and non-conservative admixture assumptions.

Page 7798 Section 3.5 Environmental Impacts - Notable results; however, others have found same thing in coastal waters and there this section needs to more fully discuss the Singapore results in light of that literature.

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

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