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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new physically-based quantification of isoprene and primary organic aerosol emissions from the world's oceans" *by* B. Gantt et al.

Anonymous Referee #1

Received and published: 5 March 2009

General Comments

The manuscript by Gantt et al. estimates emissions of isoprene, and primary and secondary organic aerosol from the oceans to the atmosphere. Several new and interesting aspects to the field are incorporated. These include additional laboratory measurements of isoprene production rates from several phytoplankton strains, the application of several variations of global maps of phytoplankton species/class/functional type reconstructed from satellite data, sensitivity studies of the speciation method and mixed layer depth, estimates of both marine primary organic aerosol (linked to the water insoluble organic aerosol fraction) and secondary organic aerosol (linked to isoprene),

and emission estimates for both the sub- and super-micron aerosol size fractions.

The paper is a valuable contribution to ACPD as it adds to the body of knowledge on marine emissions which is thought to be very important contributor to atmospheric composition, chemistry, and climate issues but as yet is very poorly understood. It is also important because it addresses varying spatial and temporal scales, including diurnal and hourly periods, as well as compares recent efforts in the field to estimate marine emissions of isoprene and organic aerosols.

The paper is generally well written and well structured, although in a few places the text could be more succinct. One example is the title; it could be shortened by use of 'marine' rather than 'from the world's oceans', which does not add necessary content. In addition, there are some inconsistencies or looseness in terminology that should be standardized. For example, choose emissions or fluxes, but not both. Similarly, choose tropical or equatorial waters, but not both. While these have slightly different definitions, they appear to be used a bit loosely here. Similarly, choose ocean-emitted or ocean-derived or marine.

After addressing the specific comments listed below this paper should definitely be published in Atmospheric Chemistry and Physics.

Specific Comments

1. The use of "physically-based" in the title and throughout the text is unclear as this term is never defined. Virtually all parameterizations are based on physical observations, by their nature. Why is this highlighted?

2. It is not clear until much further into the manuscript what is 'new' as stated in the title. This is an important and necessary work that builds our body of knowledge in new ways. However others have also combined new laboratory data on emission rates with satellite maps of phytoplankton functional type. The authors should clearly identify and

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highlight the novel aspects of this paper (of which there are several, as listed above in General Comments).

3. P2939, L5 – The list of species tested (from the methods section) does not match that in Figure 1. Mention of values taken from the literature is needed in the legend to Figure 1. Currently this is mentioned in the text, but not until several paragraphs after this sentence.

4. P2939,L10 – A wide range of emission rates can be indicative of many things, only one of which is photoinhibition, that can vary by species. Please rewrite the sentence to address this more completely.

5. P2939, L13 – What is the definition of "young" – earlier in the growth curve, or a batch closer to the initial transfer of a stock culture? If there is data to support this it should be mentioned (even just in this sentence is fine if it doesn't warrant a table). If not, and this is an anecdote, it should be removed. Sufficiently high variability exists in such biological systems that more substantial proof is required for inclusion.

6. P2939, L15 – Differences in emissions for various nutrient replete and deplete conditions is a very interesting observation. I recommend adding further comments/data to support this if available, as well as potential implications. For example, were nitrogen-limited cultures slower growing and smaller isoprene producers?

7. P2939, L16-26 – This section is confusing, and requires reordering of the sentences to first explain that the emission factors were fitted to a curve, secondly how this fitting was done.

P2945, L12 – Why is it important to capture diurnal variations in emission rates in this work if you are calculating monthly average emissions?

Presumably the emission factors used already account for both day and night emissions. If not, then a rough estimate can be achieved by adjusting the emission factor, assuming 0 production at night and averaging over a typical light:dark hourly cycle. It

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seems the diurnal variability is of most interest for the purpose of testing the emissions relationship as a function of light level on an hourly basis. If so, please clarify.

8. P2947,L8 – Table 1 is a very useful summary of recent work, including this manuscript. A brief several sentence comparison in the text is also warranted, including hypotheses as to why there is such a range in values for the various metrics.

P2948 – A brief statement should be added about why these particular parameters (speciation and MLD) are the most relevant to test.

P2949, L13 – This is a very interesting and neat way to determine the sensitivity to speciation. A small clarification is needed to the statement, in that differences in the magnitude of 1 phytoplankton class are estimated here. In reality, all phytoplankton classes will have similar issues, and thus this would be a minimal error estimate.

P2050, L16 – The diurnal variations could at most affect values by a factor of 2, whereas your table 1 shows differences on the order of factor of 10. So this is likely not a primary driver of the discrepancy.

Technical Corrections

P2934 “Main” is not a proper descriptor of phytoplankton types. Please specify as most abundant, representative of certain regions, etc. or remove.

P2934,L8 and L14 – “were scaled up” and “were used”

P2934, L21-22 – Is this a peak or average value?

P2937, L1 – Please add comma after “organic aerosols”

P2938, L27 – Please add “selected” before “diatom”. It is important to recall that there can be dramatic variation in biological parameters by species, or even strain, within a given class.

P2940, L2 – Please change to “all types of phytoplankton tested in this work or retrieved

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Interactive Comment

from the literature to use in this work", or something similar.

P2940,L6 – The sentence stating "Amongst the diatoms" is a result, and should be moved elsewhere.

P2940,L8 – This is an unusual choice for error bars, and needs to be mentinoed in the Figure legend.

P2940, L19-20 – What are the emission factor units?

P2940,Section 2.3, L12 – Capitalize and italicize *Phaeocystis*

P2942, L12 – "have considerably"

P2942, L2 – Delete "strong'

P2945, L4 – Data is only presented for January and July, not throughout the year. How can there be elevated solar radiation throughout the year? Please reword sentence.

P2945, L29 – "...emissions can likely be attributed..."

P2946, L22 – "Sect. 3.4"

P2947, L27 – "...in these polar regions."

P2947, L9 – It has been shown that marine-source SOA is very large, but the link to CCN effects is just a hypothesis. Please reword.

P2949 – Sections 3.5 and 3.6 would more appropriately be labeled 3.4.1 and 3.4.2

P2950, L7 – "performed"

Throughout – "tropical waters" and "polar waters" are referred to without mention of latitudinal bands or other identifier (e.g. abstract, P2947).

The authors cite Spracklen et al. 2008, but this is not in the reference list.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2933, 2009.