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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.

**B. Gantt et al.**

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Note: All reviewer comments in italics. All responses by the authors in normal font.

We would like to thank reviewer for his/her comments. We have done our best to address each of the points as detailed below.

General comments

*1. The present manuscript aims to derive the global oceanic budget of isoprene (based on new laboratory isoprene emissions rate determination) and its impact on the OC global budget. This subject is of importance because, as pointed out by the authors, although minor at the global scale and on a yearly basis, the marine source of isoprene*

*can have regionally an impact on the secondary formed OC. The paper presents a complete study which includes new laboratory isoprene emissions, use of phytoplankton distribution satellite derived data to globalize these emissions and finally an estimation of primary and secondary organic carbon. The paper is clearly written and the different steps used for the global source determination are generally well explained. However, as papers have been published very recently on very similar subjects (see Spracklen et al. (2008), Roelofs et al. (2008) and especially Arnold et al., ACP, 2009), it is suggested that the current manuscript should provide a through comparison with these papers before publication in ACP (see main comments).*

Some of the recent papers were not available during the manuscript preparation. In the revised version of the manuscript, we discuss the methods and conclusions of these recent papers to provide a better comparison to the new methods we used to estimate the different emissions.

Main comments

*2. A very recent paper (Arnold et al., ACP, 2009) also aims to derive a global budget of oceanic isoprene and its impact on organic carbon (OC). Although this paper by Arnold et al. is quoted in the present manuscript, similarities and discrepancies are only superficially commented. The derived emissions (from scaled up laboratory emissions) is similar to the bottom-up approach of Arnold et al. (using a different dataset of new laboratory emissions but a similar way to scale up using an approach based on remote sensing- one of the method used -PHYSAT- is the same for both papers). Therefore each step of the global source estimation (the emission rates for the different phytoplankton groups from laboratory experiments, the PHYSAT phytoplankton distribution -which should be the same if the same version of the method is used- and finally the resulting global isoprene source) should be compared in more details and carefully discussed. Improvements should be clearly pointed out (ex separation between WIOC and WSOC). The global source of OC is estimated as well in this paper (20 TgC/yr) and compared in table 1 with previous estimates but almost no comment is made on the*



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*significant differences observed between these results. As the present paper comes after the study of Spracklen et al. (2008) and Roelofs et al. (2008) it should give the reader the necessary information to compare the different approaches and results.*

We agree with these concerns and comments of the reviewer. Several sections of the revised manuscript have been edited to describe previous studies in much greater detail in order to highlight the differences involved in our emission estimates.

*3. The global oceanic source of isoprene is given as ranging from 0.31 to 1.09 Tg C yr-1 and is "within the range of estimates of 0.19-1.68 TGC yr-1 proposed by previous studies";. Examining the budget proposed by Arnold et al. (from 0.31 Tg yr-1 with the "bottom-up" approach and 1.9 Tg yr-1 with the "top-down" approach), it seems that the statement would be right if the same units were used but once it is Tg C yr-1 and once Tg yr-1. Please clarify.*

To make all the emission units consistent, in the manuscript we report all values (isoprene, SOA and primary OC) in units of Tg C yr-1. Global emission numbers in Table 1 have been adjusted accordingly.

*4. P2939 Studies have shown that there is a significant variability of isoprene emission rates from phytoplankton from a same group (see Table 1 from Arnold et al. 2009). Therefore values given here as "emission factors" representative for diatoms, coccolitophorids and which are based on measurements on a limited number of species) are certainly associated with a significant uncertainty which should be given/estimated. This would help to know if the difference of emission factors between the phytoplankton groups is significant or not.*

We agree with the reviewer. Isoprene production rates are highly variable within the same class of phytoplankton. Even within the same species of phytoplankton, the production rate can also vary based on the environmental conditions such as light intensity, temperature and perhaps many more factors that have not yet been identified. Figure 1 shows the total range of measurements from each group for variable light



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intensity only. The error bars shows the measured uncertainty due to variations in one parameter only. Future studies are needed to better constrain the effects of numerous parameters affecting organic hydrocarbon emissions from marine plankton.

*5. P2936 Terpenes can as well be emitted by phytoplankton (see Yassaa et al., 2008, Colomb et al. 2009)*

Discussion about the terpenes has been added to the revised manuscript.

*6. Section 2.1 Please explain how the studied species were chosen (Three diatoms species were studied although this specific group is only dominant in certain regions as high latitudes and upwelling regions)*

Diatoms were chosen because i) they are the major species in high [Chl-a] waters and ii) according to most published data, diatoms have some of the highest isoprene production rates of all the different plankton groups. Therefore, it is expected that isoprene emissions from diatoms and dinoflagellates will contribute the major fraction of total marine isoprene emissions.

*7. P2938 L29 What was the background ? (value and uncertainty) What were the typical mixing ratios measured? Were they well above the detection limit?*

Background levels in the laboratory were below the detection limit (< 3ppb). Measured mixing ratios were between 5 and 20 ppb. This information has been added to the revised manuscript.

*8. P2939L23 The estimation of the isoprene production (emission factor) in function of the light intensity is not completely convincing to me. It is almost uniquely based on the diatoms variations (considering the uncertainty on the first point for coccolithophorid the isoprene production could be as well independent of the light intensity above a certain value).*

Our Figure 1 is not in conflict with the reviewer's comment. Published lab and environmental data shows that the isoprene production from phytoplankton depends on light



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intensity. However, when a certain emission value is reached for a given light levels (as shown on Figure 1) further change in the light intensity does not affect emission rates considerably.

*9. Section 2.3. The scale-up of the isoprene emissions is based on two different remote sensing methods which according to the authors show "similar spatial and seasonal distribution of phytoplankton speciation". A figure would help to better identify similarities and differences of the two methods.*

Figure has been added to supplemental material.

*10. Then it is stated that "the PHYSAT model is used as a default for the assessments of the global marine-isoprene emissions", why ?*

The following description has been added to the text: "In this study, the PHYSAT model is used as a default for the assessments of the global marine-isoprene emissions because it directly represents the dominant phytoplankton groups rather than using a proxy as in the NDT method. In addition, the PHYSAT method differentiates between Prochlorococcus, Synechococcus, and Nanoeukaryotes, while NDT does not."

*11. Finally it is said that sensitivity calculations using NTD method have been performed to estimate how distinct methods and the uncertainties in phytoplankton composition can affect results for total global oceanic emissions of isoprene; this is in contradiction with the previous statement that both methods show "similar spatial and seasonal distribution of phytoplankton speciation";. If these two approaches are used for sensitivity analyses purposes, then a more detailed discussion on PHYSAT and NTD methods should be provided.*

More detailed description of PHYSAT and NDT are provided in a revised manuscript.

*12. P2948 Figure 7 It is somehow surprising to observe on figure 7 almost no seasonality in this "snapshot" midday impact. Does it mean that the seasonality of isoprene emissions play almost no role on the midday production? If so, it contradicts the state-*



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*ment "figure 7 highlights the need for improved assessments of the marine isoprene emissions."*

The snapshot figures are for the percentage contribution of marine isoprene SOA to total OC emissions. Since primary OC emissions scale very strongly with the local meteorology (i.e., surface wind speed), Figure 7 would not reveal the contribution of seasonality to midday marine isoprene emissions. Text has been modified to avoid the confusion.

Technical comments

*13. Section 2 Where were the laboratory experiments performed?*

Text has been modified to describe the experimental location.

*14. P2941 L 5 "Photosynthetically"*

Changed.

*15. Section 3.1 is named "global isoprene emissions" and begins with "global production of SOA. "It should be clearer to focus first on the isoprene emissions (figure and text)*

Global production of SOA has been moved to Sect. 3.4.

*16. P2950 L 7 "performed"*

Fixed.

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