Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-469-RC1, 2016


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Can simple models predict large scale surface ocean isoprene concentrations?" *by* Dennis Booge et al.

**Anonymous Referee #2**

Received and published: 19 July 2016

General Comments: This manuscript describes an evaluation of the Palmer and Shaw, model (which parameterized oceanic isoprene concentrations as a function of chlorophyll concentrations and laboratory isoprene emission factors) with satellite chlorophyll data and in situ ocean cruise measurements. The Booge et al. manuscript then describes subsequent updates and extensions of that model, with evaluations based on the cruise data. The updates included (1) the addition of emission factors representing multiple individual phytoplankton functional types (PFTs) as opposed to a single average value across PFTs, (2) the testing of the model results against individual pigment markers), and (3) laboratory measurement of biological degradation rate with an isotopically labeled isoprene and subsequent inclusion into the model. The results demonstrated large increases in predicted oceanic concentrations, and thus fluxes, which more closely matched the in situ cruise data than the original model. However,



Printer-friendly version

Discussion paper

the fluxes are still insufficiently high to match observed atmospheric isoprene concentrations. The authors conclude missing sources of oceanic isoprene still exist.

This is a very well-performed study which has successfully updated the prior model, which was limited by necessity to representing only a few phytoplankton species and functional types. In the intervening years, a number of laboratory studies have been performed with dozens of additional phytoplankton species and several additional PFTs, thus broadly expanding the information available with which to expand the model. The experiment to make direct measurements of isoprene biological uptake through deuteraetd material is particularly exciting, and it will be important to follow through with that expected publication as indicated.

Booge et al. do a remarkable job at combining all the new data sources and model formulation. The results have increased oceanic concentration predictions substantially, which partially compensates for previously-expected "missing sources", but these are still only of marginal importance to the air concentration underestimate. Additionally, there are clear locations during the cruises where the updated model still fails to reproduce the appropriate concentrations. Despite the fact this mismatch between bottom-up (parameterized fluxes and concentrations) and measured air concentrations still exists, this paper has performed important work, is a major step forward, and needs to be published. It is an important paper to the fields of remote chemistry, and aerosol formation marine regions.

The reporting is descriptive, succinct, and easy to follow. The analytical and measurement methodologies used are all robust and generally have been previously well-proven. The analyses are performed to an appropriate level of detail, the conclusions drawn are well-supported, and the literature is comprehensively cited.

Therefore, I recommend publication with minor revision, and have only minor comments below.

Specific Comments: Page 6, line 15 – If any species identifications beyond PFT iden-

tifcation by pigment (i.e. Figure 5) it would be helpful to point out whether they were species previously tested for isoprene production and present in Table 2 or not. This is particularly important in the areas where isoprene was not reproduced well.

Figure 6, line 20 – This should be Figure 3, not 2

Page 7, line 40 – I agree the physiological conditions can be a major driver of emission rates. A review of the laboratory studies that investigated this issue show a large range of emissions. This subject is worth a brief review of the relevant literature ($\sim$2 sentences).

It is important to provide the caveat that the in-situ data provided is focused on three cruises in two regions of the oceans. It is a good test, but there are many regions that have not yet been tested with the updated model.

There is mention of the time resolution of the assessment being insufficient to capture the phytoplankton and isoprene heterogeneity that can result in large blooms of isoprene-producing species, and thus contributing to the underestimate of air concentrations. A sensitivity study based on bi-weekly or weekly satellite assessments of chlorophyll, as compared to monthly, would be an interesting addition to the manuscript. While it may not be possible to obtain MLD data on these time scales, perhaps there are pigment dat. Reasonable assumptions could be made in a simplistic manner to check what the maximum relative increase possible is in oceanic concentrations, flux, and ambient air concentrations. This would help determine if resolution is really the issue, or if untested high-producing species are the dominant cause of underestimate.

---

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-469, 2016.



Printer-friendly version

Discussion paper