

***Interactive comment on* “Testing the relationship between the solar radiation dose and surface DMS concentrations using high resolution in situ data” by C. J. Miles et al.**

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Miles et al. make use of in situ surface DMS concentrations, surface irradiance, and light attenuation data in latitudinal transects throughout the Atlantic to test the regional validity of a proposed global relationship between DMS and the solar radiation dose, with the aim at suggesting an improved relationship for predictive purposes. The authors also make use of climatological data to explore if the use of in situ data makes the difference between their observed pattern and that reported in the global study. They conclude that there is indeed an apparent proportionality between in situ DMS and SRD, and an interestingly similar relationship between DMS and the UVR dose,

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but either one provides further predictive capability than that of the mixed layer depth alone.

The paper is well written, succinct and effective at communicating the authors' point. Overall, it provides a very interesting discussion on the fundamentals of any potential (or apparent) relationship between DMS and solar radiation. It also provides a criticism examination of our Vallina & Simó 2007 (hereafter VS07) data analyses and reasoning. In this sense, the manuscript goes along with other recent works (Belviso & Caniaux 2009, Derevianko et al. 2009). I like the effort made by the authors and am supportive of publication. However, I have some concerns and questions about their handling of the data and the way they refer their work to VS07. I will go point by point on the most important aspects throughout the manuscript, and then I will touch more specific questions.

1. For SRD (or UVR), do the authors use the daily (or noon) irradiance on the DMS sampling day, or the day before? This is another important issue, because the authors claim they are using in situ data, but the daily average irradiance on a certain day when you have sampled in the early morning may have little to do with the observed DMS. In local studies, what we do is to calculate the average irradiance over a 24 hours cycle that ends at the sampling time.

2. To compare both in situ and climatological with VS07, the authors calculate the SRD using the MLD criterion of de Boyer Montégut et al. 2004 (a 0.2°C departure from the T at 10 m). But in VS07 we used a different criterion and re-worked the de Boyer Montégut MOLD climatology accordingly. The criterion used was a 0.1°C departure from the T at 5 m. This can be found in the Supplementary material of VS07 and in Vallina et al. Global Biogeochem. Cycles 2007.

3. I am surprised by the high SRD obtained (Fig. 1), as high as 350-400 W m⁻². Being close to the equinox (12h day : 12h night), and with MLD by definition ≥ 10 m, these high SRD require noon I₀ of the order of 1800 W m⁻², which is beyond the solar

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constant. Something must be wrong. My guess is that the authors averaged only light hours, whereas SRD definition á la VS07 is the 24 hours average. This may change the slope of the relationship quite significantly.

4. I celebrate the effort made by the authors to incorporate the UVRD in the DMS analysis. We already tried that before the VS07 but realized that only the noon irradiance at 380 nm could be obtained. I have several concerns here: (a) It is not clear to me if the authors used noon UVA irradiances measured by TOMS on the very same days (and year) of the DMS sampling, or those extracted from a climatology (average of several years). They mention a TOMS climatology in line 20 of p. 3069. This has to be clarified, as it is extremely important for the subsequent analyses of “in situ” UVRD data. (b) The authors use the noon UVA irradiance and make no daily averaging of it, parallel to what they do for I0. That is, they do not take into account the differences in day length as they change latitude and season. (c) They take late UVA (380 nm) from TOMS but use an attenuation coefficient (0.16 m^{-1}) that is more appropriate for UVB. They can take a look at e.g. the review by Tedetti & Sempéré, *Photochem. Photobiol.* 82: 389-397 (2006) where it is shown that in clear open ocean waters late UVA typically has a k of approx. 0.10 m^{-1} . Another possibility would be to derive k_{380} from in situ k_{PAR} .

5. The authors take too long to underline that the VS07 study (or the part of it they use the most) is a global approach. Actually, they do not make it clear until the first paragraph of the discussion. This is not a trivial issue, as it has profound implications for the intended comparisons of both the results and the tools used. In VS07 we wondered a lot if the use of a fixed k and a fixed derivation of the I0 ($0.5 \times \text{ITOA}$) for the global ocean were appropriate. We tried to use a global climatology of k derived from chlorophyll a concentrations, and a global climatology of I0 derived from a PAR climatology with implemented ICCP clouds, but realized that we were introducing noise by using derivations that were subject to considerable uncertainties. Two aspects of the VS07 work are critical for this and further comparison attempts: (a) Since we do not have in situ physical data associated with each DMS measurement in the global

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database, there is no point to try to find a relationship between DMS and anything based on individual datapoints. Only climatologies (collapsed year) or spatial (coarse grid) averages can be used to derive global relationships. (b) We in VS07 also report on local studies where in situ data are used. In these studies (Blanes Bay and BATS) the scatter of the DMS vs SRD relationships was much smaller than that of the global relationship with climatological data. (c) The motivation of VS07 was NOT to derive a predictive algorithm for surface DMS but to show there is a proportionality of DMS on the SRD that holds over MOST of the global oceans. We did not intend to get to a “tell me what I0 and MLD you have and I’ll tell you how much DMS there is”, but unveil a mechanistically-based emergent property by which DMS tends to accumulate in highly irradiated waters.

6. I fully agree with the statement made on lines 17-19 of p. 3075: “The estimated I0 could then represent the background potential for exposure to incident surface radiation whilst variations in MLD control the dose.” In my opinion this resumes nicely the key to the interpretation of VS07.

Specific comments:

7. P. 3066, lines 25-26. It is not exactly like that. Reduced S demand does not lead to reduced DMS consumption, as it has been shown that DMS does not supply much S to its consumers, but the S ends up as DMSO and sulfate. The suggested effects of UVB on DMS as far as the bacteria are concerned are: (a) UVB damages cells, lowers bacterial production, lowers bacterial S demand, lowers bacterial DMSP-S assimilation (all this proven), increases bacterial DMSP cleavage, increases DMS production (unproven). (b) UVB damages cells, lowers bacterial production, lowers bacterial DMS consumption (proven: e.g. Toole et al. Deep-Sea Res. I 53:136-153 (2006)).

8. P. 3067, lines 9-10. Kniveton et al. indeed demonstrated that extreme increases in UV can cause a reduction in atmospheric DMS but they could not attribute that to underwater effects. Direct (UV mediated) photodestruction in the atmosphere seems a

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more plausible explanation.

9. P. 3068, line 22 and throughout the manuscript. Montegut et al. 2004 should read de Boyer Montégut et al. 2004.

10. P. 3072, line 13. Better cite Toole et al. (2006) than Herndl et al. (1993), since the latter does not link DMS and incident UVR.

11. P. 3073, line 23. The approach is not “the same” unless the authors correct for the MLD criterion and the SRD calculations.

12. P. 3076, lines 19-23. It seems appropriate to cite Toole & Siegel 2004 (who found a relationship between DMS and UVR) here and comment accordingly.

13. Is Derevianko et al. an accepted/published manuscript?

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