Sunburned plankton: Ultraviolet radiation inhibition of phytoplankton photosynthesis in the Community Earth System Model version 2, by Joshua Coupe et al.

This paper presents modifications made to CESM2 to enable the simulation of UV-B (280-315 nm) and UV-A (315-400 nm) propagation through the ocean and UV inhibition of phytoplankton photosynthesis. Biological weighting functions (BWF) that determine UV inhibition were incorporated into the Tropospheric Ultraviolet and Visible (TUV) model, in order to calculate spectrally-integrated BWFs at an hourly frequency. The weighting functions of inhibition were then applied in the ocean biogeochemistry model MARBL, which represents 4 phytoplankton PFTs – small phytoplankton, diatoms, diazotrophs, and coccolithophores; and 2 zooplankton PFTs. In MARBL, coccolithophore shell thickness can be impacted by CO2 levels.

The damage functions were based on the few available studies of productivity responses to changes in UV radiation. There are several pertinent applications and needs for Earth system models to represent these impacts, and this study is an important step forward for the CESM community and would be of interest to other modelling groups. The paper describes the modelling approach clearly and the results are described with an appropriate level of detail. My main comments relate to some inconsistencies in the description of the study and results, organization, and framing of the results.

Major comments:

1. Scale of UV changes:

a. How do the PI UV radiation levels compare to present-day? Since occasionally the results are compared to existing studies, it would be helpful to add a discussion of the differences between present-day and preindustrial UV, and how these might impact the results.

b. 20xPI sensitivity study: This is a huge impact (95% global reduction in stratospheric ozone), if Pinatubo reduced NH ozone by 10% and Hunga Tonga reduced tropical ozone by 5% (also see Fleming et al. 2024 for more recent estimate of Hunga Tonga impacts on ozone from the water vapor injected into the stratosphere). I suggest returning to these discrepancies in the discussion, to highlight that the changes discussed would not be expected on a global scale (perhaps unless a very extreme case like asteroid impact or nuclear war), but instead they display the model sensitivities to changes in UV radiation.

c. The discussion in the supplement of ozone hole stress would fit well into the main discussion of the manuscript and further helps put these results into context of historical impacts.

2. Further discussion of the attenuation of radiation with depth: Equations 2-3 and the values of K imply that most UV radiation attenuates by 16 m with low chlorophyll concentrations. But Figure 5 shows changes down to the 30-40m layer (Layer 4). Does the attenuation of PAR with depth follow a different trajectory? It seems to me that if <1% of surface PAR reached below 16m, there would be very minor impacts in the 3rd and 4th layers. What are the changes to PAR at depth in these experiments? Has there been any validation of depth profiles of PAR vs biomass at these depths? (Minor point: Line 374: This states there is an increase in NPP in the 3rd layer (25m), but Figure 5 shows the 4th layer (35m) – it would be better if the text and figures referred to the same layers)

3. As this is a first step in modelling these impacts, I think the authors could draw even more attention in the discussion to the additional modelling and observational studies that would help inform future developments or address any of the limitations in this study. Consider devoting a short sub-section or paragraph in the Discussion to future research priorities to help progress this work.

4. Description of methods: The abstract states that you conducted simulations to calibrate estimates of sensitivity of phytoplankton productivity to UV radiation. But this is not reflected in the paper, since the experiments discussed only used one value of the biological weighting functions. It would be more accurate to say you did these experiments to 'understand' the sensitivity, unless some calibration was done that is not discussed in the paper (in which case, that should be explained in the manuscript). Also Line 485: I do not see evidence of a large parameter space of E*_inh being investigated in this study.

Minor points

Organization: The results seem out of order to me. Figure 4 is helpful for understanding where the different PFTs live, and the absolute impacts of UV radiation on their NPP. This helps with the understanding of the globally averaged results shown in Figure 2. I suggest rearranging and adding subsections, ie: (1) regional impacts of UV radiation on phytoplankton NPP; (2) globally integrated impacts of UV radiation; (3) vertical distribution of impacts; (4) effect of enhanced CO2.

Lines 151-154: What is the motivation for the sensitivity studies with different configurations of MARBL? They are not discussed in the manuscript (with exception of a very small mention of the 3p1z experiment in the results). So either remove the mention of these additional experiments, or update manuscript to include these in your results/discussion and more fully explain how they contributed to the understanding of phytoplankton sensitivities to UV radiation. (Moving the supplement into the main text would help, as this is relatively short and very relevant to the validation of your results)

Equation 1: Over what interval of lambda are these calculated? (What is Δlambda?)

Line 333: Increase in productivity – is this in relative or absolute terms? Only relative terms are shown in Figure 2.

Figure 3: How is the total E_inh calculated, is it weighted by PFT distribution?

Lines 355-360: Here, and elsewhere in the results, I would suggest you keep the description of the patterns and other direct results from these experiments, but leave the comparison to other studies for the Discussion.

Lines 453ish: Mention here the amount of increased UV radiation in this region in these experiments compared to ozone hole or other historical events.

Lines 458-459: Provide a reference here and clarify – this is not clear – the responses are on the high end of observed values of productivity or observed values of productivity changes?

Line 465ish: In addition, I would think that phytoplankton are adapted to ambient UV levels (this is the case for terrestrial plants), which would have an impact on the spatial variation of the inhibition factor / BWFs. Would it be important to consider the ambient levels of UV to which phytoplankton are accustomed, before considering impacts of changes to those levels?

Lines 425-427: are you sure it's this way around? Could it be that the decreases in small phyto and diatoms are making it easier for coccolithophores to live here?

Line 436-437 – provide a reference here, but also consider moving comparisons of results to other studies to the discussion.

Paragraph from Lines 428-438: When discussing PIC/POC scaling, is this for the case of 900 ppm? This is implied since it references Figure 8c. Please clarify.

References

Fleming, E. L., Newman, P. A., Liang, Q., & Oman, L. D. (2024). Stratospheric temperature and ozone impacts of the Hunga Tonga-Hunga Ha'apai water vapor injection. Journal of Geophysical Research: Atmospheres, 129, e2023JD039298. https://doi.org/10.1029/2023JD039298