Godoy et al Biogeochemical Discussions 8, 5827-5648, 2011

The authors examine the effect of UVB radiation on net community production (NCP) by comparing rates in quartz and presumably (but not specified) borosilicate glass bottles. They observe, what I believe to be correct, that there are no published studies examining this effect. Such a study in long overdue, as we have known for the best part of two decades that there is an effect on photosynthetic rate measurements. Thus the study is welcome.

General and Overall Comments:

i) I cannot judge the quality of the radiation measurements, but the oxygen measurements are of very high quality and as they observe on p.5835, line 7 the differences between the +UVB and -UVB rates are significant.

ii) It seems to me that there are quite a number of inconsistencies between the text and the data in Table 1, I have noted them below and the authors will need to check the text very carefully.

iii) There is also as feature that the authors need to address. Their broad conclusion is that incubations in the presence of UVB give lower rates of NCP than in its absence – this they observe in 4 out of 5 cases. From simple probability theory the statistical case is weak – with the small data set and the distribution of the data non-parametric test is probably most appropriate – my calculation is that the odds of it occurring by chance are 25% or more. If you do a regression analysis of the effect of UVB on NCP (quartz-glass rates) the R^2 is quite high (0.8) and the relation shows a positive relationship between NCP and UVB, not an inhibition. The authors need to give thought to this.

iv) My recommendation is to return the paper to the authors for a rewriting and resubmission. It may be that they need to be more cautious over their conclusions and less ambitious over their projections from the study.

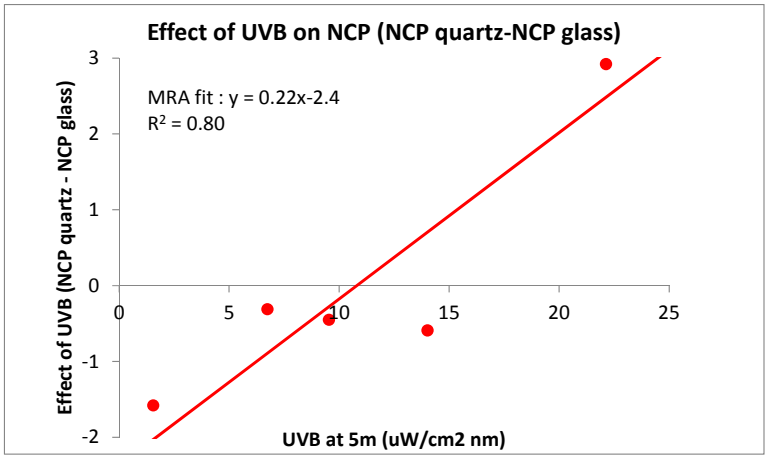
My detailed comments are as follows:

p.5835, line 8 "*Indeed, NCP in the presence of UVB was negative in all but two experiments,. .* ", this does not match the data in Table 1, where NCP is negative in only one instance, and it is negative both in the absence and presence of UVB.

p.5835, line 10 "*with a median decrease in NCP by 2.17 mmol* O_2 m^{-2} d^{-1}*.*", the median decrease as far as I can determine is -0.31, quite different from what they report.

p.5835, line 11 "*NCP increased when plankton communities were exposed to ambient UVB levels in only one station, that at the northernmost station sampled, where NCP increased to 6.75 ± 0.03 mmol* O_2 m^{-2} d^{-1} *compared to 3.48±0.02 mmol* O_2 m^{-2} d^{-1} *when UVB was removed (Table 1*).", this statement is correct, however the point that seems to be missed that this station had the highest UVB - 94 µwatts cm^{-2} sec^{-1} at the surface and by my calculation, despite the higher Kd, still the highest at 5m. This implies an opposite effect on UVB on NCP to their general conclusion.

Take it or leave it, if you plot the change in NCP against the UVB intensity at 5m, then there is a positive relationship between NCP and UVB not an inhibition, with significant relationship ($R^2 = 0.80$).



p.5835, line 24 "*Respiration rates at surface were generally lower and more uniform across stations, resulting in a prevalence of net autotrophic communities (GPP > R) throughout the entire section (Fig. 5). In contrast, the surface waters were supersaturated with CO_2 at all open-ocean stations, supporting, therefore, a net efflux of CO_2 into the atmosphere (Table 1)*." Net heterotrophy is not the only explanation for CO_2 supersaturation. The area is an upwelling area and the upwelling water will be supersaturated with CO_2, further it will warm up – increasing the supersaturation. Exchange of CO_2 across the air sea interface is a slow process and there is a possibility that equilibrium may not have been reached. This needs exploring, if nothing else to eliminate it.

p.5836, line 19 "*All stations occupied had autotrophic plankton communities when incubated in glass, as has been done in the past*." Not all, 4 out of 5 did, and with the same pattern in the case of the incubations in quartz.

p.5836, line 25 "*Exposure of surface (5m) communities to UVB radiation, greatly reduced NCP in all but one community and rendered all, except one of the communities investigated heterotrophic*."

and

p.5837, line 8 "*, the communities in surface waters resulted to be, in general, strongly heterotrophic, thereby acting as a CO_2 source, consistent with the supersaturation in pCO_2 in surface waters driving a CO_2 efflux to the atmosphere."*

and

p.5838, line 28 "*The use of quartz bottles to allow the UVB component of the irradiance field yields net heterotrophic communities in surface waters, consistent with the supersaturation in pCO_2 in surface waters observed along the cruise."*

All three statements are, as far as I can see, incorrect. I think the authors mean is that most of them (4 out of 5) become more heterotrophic, although all remain in the same state of trophic balance – autotrophic communities remain autotrophic.

p.5837, line 22 "*Our results show that, for the communities studied along the Humboldt Current System, removal of UVB increases net community production, by suppressing respiration and possibly increasing gross primary production*." This is patently a wrong claim and inconsistent which their correct observation (**p.5837, line 1**) that "*Our experiments did not allow evaluate (sic) the effect of UVB on respiration vs. that on GP, . . .*".

p.5837, line 3 I don't think the Pringault *et al* reference is relevant to the present discussion, as Pringault *et al* specifically mention (p. 324, line 8) that they used UV-free light: "*Visible light (UV and IR free) was*

provided by two fluorescent tubes (daylight spectrum, Sylvania Luxline plus F36W/860, Sylvania, Germany)." Could the present authors check this.

p.5837, line 24 "*Moreover, the exclusion of UVB from the solar radiation not only inflates NCP rates, but may even alter the NCP, in our case shifting the communities from net heterotrophic to autotrophic."* There isn't a single instance of this in their data set as far as I can see.

p.5838, line 2 (also in the Abstract) "*These results show that UVB radiation, ... , may have suppressed net community production of the plankton communities in the study area, possibly driving plankton communities in the Southwest Pacific toward CO_2 sources."* I think the authors need to qualify this statement, which actually is a speculation, especially as it is in the Abstract. Agreed, they properly use "*may have*" but, formally for what it's worth, the UVB dose response (i.e. the NCP downshift versus UVB irradiance level) is the inverse, so we need some caution in this respect. Secondly, and more importantly, I don't think we understand what controls NCP, so how can we project. We can expect feedbacks between P and R operating within the food web, if the time response of these feedbacks is longer than the incubation time, then short-term experiments cannot be used to make long term predictions. By all means think about what the implications of the study may be, but make the assumptions and limitations clear and also make it clear that you are speculating. Of greatest importance, if you want to put speculations in the Abstract (where there is no indication of the limits to the data) then make it clear both in the text and especially in the Abstract that it is no more than a speculation. Some people may only have ready access to the Abstract, further others may not have the background to judge the reliability statement. In both cases I would recommend "*We speculate that . . .* ",

Peter Williams