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Biogeosciences Discussions

Interactive comment on "Diel variability of heterotrophic bacterial production and UV doses in the South East Pacific" *by* F. Van Wambeke et al.

Anonymous Referee #1

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The Ms of Wambeke et al try to address dial variability of bacterial production (BP) and UV doses in the South East Pacific. It is not clear from the Ms what exactly the authors did as the material and method section is very poor and unbalanced, with very little explanation on sampling, depths, incubations, determination of BP (about a page for all these topics) and a lot explanations on the measurements and calculations of UVR (3 pages). This was apparently done in order to relate the BP with the UVR doses within the UML. As the author clearly stated, the cells are moving within the UML and thus the mean irradiance is adequate to evaluate the effects of UVR. However, their data seems not to support this idea and they show higher variations at 5 m and not in the UML. The author should consider why this is happening? One of the potential explanation is the cells are moving within the UML al a lower speed than their sampling

frequency. That means the a complete circulation within the UML takes longer than the three hours between their sampling, so they are sampling cells with different light acclimation history (this is clearly seen in the profiles presented in Figure 4, at least for MAR and EGY). In addition, and looking at Table 3, the depth of the UML was highly variable at GYR, so a mean value for this depth is not good. So a different approach should be used to present their data.

Specific comments: Abstract:

1) BP is expressed in %, it is also necessary to include ´real´; values as Carbon, so the reader will have and idea of the BP rates. 2) The authors measured leucine incorporation, and they made a relation to CPD as if this was obvious and routinely. I disagree with this as they involve two different targets for UVR as well as different metabolic and timescales. It is possible to infer a indirect relation, but not as the author did assuming from BP and UVR levels the amount of CPDs.

Material and methods

3) It is not clear here that they sampled every depth and what were the depth, it only seems so after looking at figure 4. 4) As mentioned above this section is poorly described and the author devote a lot of work explaining the measurements of UVR, etc. 5) There is NO statistics with the exception of few correlations that are poor and in the end the authors present more variability with depth and at 5 m that in relation to the UML.

Results

6) This section is confusing as the author mix results with discussion 7) Through the text the author used words like ´great magnitude´; ´variations. . .very low´, but most important the reader would like to know if the observed variations were significant or not (statistically). 8) One of the y-axis in Figure 2 is wrong or have the wrong units as maximum solar PAR at noon is ca 500 W m-2. 9) In the figure

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caption the authors state that the bars are standard deviation, however, in M&M they mentioned only duplicate samples and ´half difference between samples´. 10) Figure 3 seems to be based on the actual profiles of BP, such as the ones presented in Figure 4, so the authors should present first the real profiles and then the contour plots. Even though contour plots are nice to see, they ´suffer´ from the potential variability according to the way used to calculate them. It is often see that differences arise from the power or method used for the gridding, so more explanations should be available to the reader. 11) There are a lot of repetitions between the text and what is shown in the Tables.

Discussion

12) This section is highly speculative and many parts are repetition of the results. 13) What do you mean by ´volumetric surface (5m)´? One can not consider the 5m sample as surface sample. 14) Please see above my comments for BP and CPDs. 15) The authors tried to evaluate the effect of UVR by taking samples and they relating their response to the UV R levels measured during a 3-hour period. This could be an interesting approach, but it is not clear what the authors related, for example, what dose did they use for the calculations, the actual dose occurring during the incubation or the previous one so they also account for the previous light history? In addition, this was calculated for every depth or just integrating the effect in the UML? 16) The final statement or conclusion is highly speculative.

Interactive comment on Biogeosciences Discuss., 5, 435, 2008.