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Biogeosciences Discussions

Interactive comment on "Gross community production and metabolic balance in the South Pacific Gyre, using a non intrusive bio-optical method" *by* H. Claustre et al.

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I congratulate the authors on an analysis of cp for determining daily productivity. Their text implies that they see this as a first use of this method, even though it was introduced in 1989 by Siegel et al. The interval of time since Siegel et al. was published, however, was not for lack of trying to move things forward, and many others have contributed. For example there is substantial moored sensor data that has been (and still could be) used for these kinds of analyses. I don't find the criticisms of the method (not covered here) to be of sufficient magnitude that the method should not be employed. Under many circumstances, although not all, it can provide useful information, as they

demonstrate.

The criticisms for using cp as a proxy for productivity are (1) that the carbon attenutation cross-section (essentially, C:cp) is not constant on a diurnal basis, and (2) that cell division cycles will confound any analysis. Criticism (1) was pointed out by Stramski et al. (1992, JGR?), in measurements on laboratory cultures of one or two organisms. A few datapoints suggested an inconstancy, but not enough in my opinion to negate the method. The second criticism, pointed out, I think, by Durand and Olson (1996, Deep Sea Res. II, 42, 891-906, 1996.) has more validity, but if this phenomenon were important, it would be seen in the data. Clearly, a minimum in cp at dawn, and a maximum near dusk strongly suggests that cp is measuring something related to productivity. Growth is a harder variable to estimate since you need an estimate of the photosynthetic biomass. All these issues have been discussed in Marra (1994, In: Ocean Optics, ed. by R.W. Spinrad, K.L. Carder, M.J. Perry, Oxford Univ. Press, New York, pp. 189-201) and Marra (1995, Phil. Trans. Royal Society Lond. B 348, 153-160).

This brings up the question of what is really being measured by cp over the photoperiod. The cells could be dividing continuously, which leads to an increase in scattering as the cell numbers increase. Such a change would mean an exponential increase, and we can further suppose that growth rates are too low to depart significantly from the observed near-linear change. The second way to change cp over the day is to have the cells change size as they assimilate carbon. This would show a linear increase over the photoperiod.

Sometimes the method doesn't work. This was pointed out, also in Marra (1995), where a series of day-night cycles in the North Atlantic showed a clear diurnal signal in cp, and followed by another series where the such signals cannot be discerned.

Regarding comparisons, (discussion on p. 3100), the PRPOOS program in 1985 did a pretty good job, although the results are in scattered publications. Marra (2002 in Williams et al.'s bood) shows data on diurnal POC variations which although not exactly

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contemporaneous, are similar to the 14C productivity values. The use of cp will only be as good as its correlation with POC. Marra (2002) also shows good agreement between O2, 14C, cp, and diurnal CO2 drawdown for the North Atlantic Bloom in 1989. This is the most complete comparison I am aware of, and the incubated techniques compare well with an in situ geochemical method and with in situ cp. These authors cannot make that claim.

Some detailed comments:

1. p. 3092, near top. Hansell et al. (2004, Limnol. Oceanogr., 49(4), 2004, 1084–1094) is also a 'large scale geochemical analysis' of the North Atlantic, and they find that the metabolic balance is autotrophic but very close to 1.

2. p. 3098, lines 7-10. This is a new definition of gross community production. Gross production is supposed to include respiration losses, and diurnal cp will have losses built in, whether they are respiration or grazing.

3. p. 3098, lines 15-17. It looks as if CL is constant day or night. Is there evidence for this?

4. p. 3098, line 25. I'm not at all sure what 'net carbon stock' is.

5. p. 3100, lines 5-15. First, this analysis (like all O_2 based incubations) assumes that the dark bottle gives an accurate representation of the actual respiration rate during the day. This might not be true, and ofter, comparisons of GPP from dissolved O_2 analyses with ^{18}O incubations shows significant differences. The correct comparison with cp should be the increase in O2 in the light bottle during the day, the only unambiguous measurement of the dissolved oxygen method.

6. p. 3100, lines 18-19. I don't think you can make the categorical statement that GPP will be less in the South Pacific Central Gyre. Light penetration is also

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greater, so there is a deeper euphotic zone, and more depth over which to integrate production.

7. p. 3100, lines23-24. Too bad there are no geochemical methods to use in the comparison. It might be wise to point out that they don't always work at these time scales because of the variability in mixed layer depths, and other confounding factors. It is interesting that the most stable water column is best for this analysis, and incubation techniques do the same thing by hold samples at prescribed depths.

8. p. 3101, p. 3102. I would like to be sure, for the discussion of deep productivity, that the water column was stable for these measurements. That is, there were no internal waves or intrusions that contaminated the cp signal. I don't see any hydrographic data to go along with Fig. 3.

9. p. 3103, lines 22-23. I think the 'light shock' they mention is unlikely in current practice. Not to say it hasn't happened before, but it has become well-recognized that keeping the samples in Niskin bottles is ok, and storing until filtration in dark nalgene bottles is ok, too.

10. p. 3105. The psi models deserve a further look. It was always difficult to determine to what depth you integrate to. That is, if the chl-max is beneath the 1%E(0), it is difficult to see that it is a productive layer.

Interactive comment on Biogeosciences Discuss., 4, 3089, 2007.