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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.**

P. Williams (Referee)

oss074@bangor.ac.uk; pjlw@bangor.ac.uk

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Briefly, the authors use optical measurements, interpreted as POC, in order to estimate carbon balance and flux. They correctly argue, the approach has the advantage of being a non-intrusive in situ method with a whole list of advantages over in vitro O_2 methods. They find a balanced diurnal cycle of "POC" and probably rightly conclude that net community production (NCP) is zero, not negative as in vitro-based studies have suggested. So far in my reading of the paper I have no reason to believe that this overall conclusion is not broadly consistent with their data. They calculate a production rate from their observations of 850 mgC/m2.d. I have no particular problem with the

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scale of the rate itself (=70mmol C/m2.d), numbers of this order were reported by Williams et al (2004) for the North Central Pacific Gyre.

There are, however, a number of major conceptual problems with the paper.

1) Section 3097, l. 10-12. They start with a definition of NCP as NCP=GP-CL, where CL (community losses) is the sum of grazing, viral lysis and respiration. Now, NCP has a very clear meaning in the literature as the difference between Gross production and Respiration, i.e. it is the balance of organic material and organic energy in the system. Grazing, for example, gives rise to growth, which is part of community production and thus not wholly a loss term. (Of course, it would be if they were determining net primary production – but the discussion is of NCP.) Thus, their definition of NCP is at variance with the common and longstanding usage (a recent set of definitions can be found in Karl, 2002). Maybe they are just considering net small particle production, but that is not NCP as there is a comparable flux of DOC – as they will be aware

2) Section 3097, l. 20 onwards. They seem to argue (line 20 onwards) that, as there the rate of day-time rise in POC equals the night-time loss rate (Eq 5), then GCP will equal the time-corrected day-time rise or night-time loss (Eq 6). This would only be the case if there were no particle removal during the day. Maybe this is their conceptual model – but it is at variance with their proposal (Section 3097, sentence starting line 2) "heterortrophic biomass thus appears also stimulated (. . .) to photo-autotrophic processes". They also should note, see (4) below and Section 3096, line 21, that they acknowledge day-time flux of particles. The conventional assumption is that the night-time removal processes continue during the day. This being the case, day-time gross production is the sum of the particulate production and removal. If there is no net production then in the simple case of the day and night-time periods being equal in length, GPP is 2*night-time loss not 1*night-time loss, as their calculation. This is the principal used for all O2 GPP rate determinations. In the case of the O2 approach, some authors contend there is greater flux during the day-time period, this would give a multiplier greater than 2. The uncertainties over the scale of heterotrophic processes

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in the light means that multiplier probably falls in the range between 1 and 3. Given equal day and night rates of particle removal the calculated rates of GPP and losses are essentially twice the reported ones, i.e. circa 1600 mgC/m2.d – that's high, twice those reported at the HOT site in the North Pacific Gyre but that is not grounds to conclude they are wrong.

3) These higher calculated rates mean however, that the argument (Section 3099, l. 13) based on Table 1 of the comparability of the optical and O2-determined losses would no longer stand and gets worse when we consider the implications of (4) below.

4) Section 3096, line 21. "...., it was demonstrated that phytoplankton does not contribute to more than 20% of the cp-derived POC", I presume this refers to the POC increase, the rest of the POC increase being heterotrophic. They note that this avoids "uncomfortably high growth rates". However, it merely replaces the devil with the superdevil. As only 20% (170 mgC/m2.d) of the POC increase comes from the phytoplankton, the remaining (680 mgC/m2.d) must come from bacterial, protozoan and microzooplankton growth. Now, these organism grow with a growth efficiency much less than 1, characteristically somewhere in the region 0.1-0.2. Assume for calculation, a median value of 0.15, then the total carbon flux through these organisms must be 680/0.15 = 4,533 mgC/m2.d, add on the 170 from the phytoplankton and the total organic flux during the day time will be 4,700 mgC/m2.d, 5.5-times greater than the 850 mgC/m2.d reported. If DOC is the source of the growth of the heterotrophs, and the users of the DOC, the bacteria, are actively cropped by the protozoans, then the overall yield of that part of carbon flux is down to 0.15*0.15=0.025 giving a increase in c-demand – all of which must originally come from phytoplankton production. If the heterotrophs are grazing upon phytoplankton, then the intrinsic rate of phytoplankton particle production rockets up from 170 mgC/m2.d to 4,700 mgC/m2.d, and rather having to contend with phytoplankton growth rates giving uncomfortable production rates of 850 mgC/m2.d, one has to contend with rates 5-6 times greater. We still have to add on night-time losses. If one assume the night-time activities of the heterotrophic

population is the same as the daytime then a further 4500 mgC/m2.d is added on. At this stage one is in danger of going through the roof as far as production rates are concerned.

Until one has a much more explicit, and justified, model of what they envisage to be going on in the system, and how it is reflected in optically-determined particle concentrations and fluxes, you can end up with almost any number – most of them worryingly high.

5) Section 3101 line 25 continuing to 3102 line 9. Here they argue that the imbalance observed in in vitro-determined O2 fluxes may be accounted for by anoxygenic photosynthesis processes. The author's basic argument is that anoxygenic photosynthesis produces organic carbon so that the imbalance in O2 does not imply an imbalance of organic carbon (and so energy). The argument is not central to the main thrust of the paper. As I believe the notion is almost certainly flawed, I felt it was useful to put down my reasoning.

The argument in a condensed form runs as follows:

Although we think of the problem of O2 imbalance (net heterotrophy) as one of energy imbalance, in reality, as we cannot measure energy, it is a matter of stoichiometric imbalance. In conventional aerobic waters, where oxygenic photosynthesis prevails, there is a stoichiometric (and energy balance):

$$\text{light} + H_2O + CO_2 = [CH_2O] + O_2 = H_2O + CO_2 + \text{heat}$$

If NCP = 0, then there is neither loss nor gain of O2

Introduce anoxygenic photosynthesis (where something other than water - H2X - donates protons) and the simple stoichiometric balance for O2 is lost, e.g.

$$2H_2X + aCO_2 + bH_2O + bCO_2 = (a+b)[CH_2O] + 2aX + aH_2O + bO_2$$
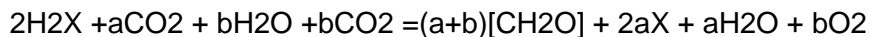
When you come to respire the organic product with O2, the O2 required is a+b mols, whereas only a mols were produced, i.e.

$$(a+b)[CH2O] + (a+b)O2 = (a+b)H2O + (a+b)CO2,$$
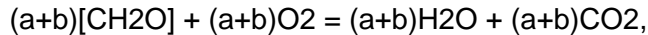
Thus although there is energy and organic balance (NCP=0); oxygen balance would give falsely imply net heterotrophy.

The critical issue with this scheme is the nature and source of the proton donor (H2X). Such a sequence of events can occur in sediments, where H2S is present and acts as a proton donor, but in the aerobic oceanic water column, where the sediments are 3 km or more away, there is no source of H2S – the half-life of H2S in oxygenated water is measured in minutes. The only proton donor that could exist is such waters is DOC and it has been suggested could serve as the proton donor. However, it runs into the buffers if you try use it to redress a significant O2 imbalance. The argument runs like this:

Effectively DOC used as a proton donor is a disproportion reaction, where in order to reduce CO2 you have to oxidise the DOC. I'll illustrate the problem with the classical conversion of molecule with an elemental composition of [CH2O] to [CHO] – the latter could for example be succinate, which is a common product of anoxygenic photosynthesis in sediments. You can write any number of possibilities, they all end up giving much the same basic story, although quantitatively they differ.

The balanced equation looks like this:

$$4[CH2O] +CO2 = 4[CHO] + [CH2O] + H2O$$

Thus we have reduced one mole of CO2 to produce one mole organic material, but need to oxidise four moles of DOC ([CH2O]) to [CHO] to provide the protons. (In

principal you could oxidise the DOC down to CO2 and then there would be a one-to-one proportion, but I've not heard this as a possibility – you can't do any better than this).

The deficit in a 100m water column in the study of Williams et al 2004 was 20-30 mmol O2/m2 d. Taking the 4:1 ratio above, to redress this balance you would need process about 70-100 mmol DOC/m2 d. The total DOC in the 100m water column is 100*80 = 8,000 mmol DOC/m2 d, thus you would run out of local DOC in about 100 days.

You can fiddle with the numbers but you end up confronting the same problem. You have to end up arguing for import of DOC. This in essence was embedded the proposals of del Giorgio/Duarte et al: thus we have gone full circle. The arguments for extensive DOC import are not convincing because the central oligotrophic gyres, by their very nature, are isolated systems – if you import DOC from productive areas, you would import inorganic nutrients at the same time and the gyres would not be oligotrophic. (This was the point made by Williams and Bowers, 1999.) If anything, as the present authors observe, the DOC is high in these oligotrophic areas so the horizontal gradients would be in the opposite direction and would give rise to DOC export.

The question one asks oneself is, if the organisms are able to extract protons from this DOM and transfer them to NAD; then why, when oxygen is present, do they use the NADH to reduce CO2, rather than oxygen as in conventional respiration – which clearly they must have the opportunity and capability to do. It's for this broad reason one's inclined to adopt the view of the physiologists that these bacterial anoxygenic systems drive proton pumps, and so they can spare organic metabolism but not supplement it, and so cannot redress the O2 imbalance.

Thus, the notion that anoxygenic photosynthesis can reconcile the net heterotrophy, at least to my mind, doesn't hold water – some other explanation must prevail.

In conclusion, the paper offers a novel, interesting and potentially valuable approach to the study of organic metabolism, free from problems of associated with in vitro in-

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cubations and also problems of air-sea exchange which complicate the analysis of in situ O2 changes. Thus, one would like to see the technique brought into use, but, as it stands, there are major problems with the interpretation of the data.

It seems to me that there are two options open to the authors. Either, they can limit their conclusions to diel particle flux, where the analysis will be fairly straightforward. Alternatively, if they want to take the analysis further, to interpret optically-determined particle flux in terms of overall carbon metabolism, then they face a slew of problems. If they chose to do this, then my recommendation to them is not to start with their optical observations and try to define rates from them – which has given rise to the problems we face with the present paper. My suggestion would be to start with the fundamental ecological processes, connections and definitions and see how they can fit their observations into these, and what assumptions have to be made in order to analyse the data in terms of the defined ecological processes (GPP, NCP etc). An instructive exercise would be to use a published flow model for the plankton – Fig 4a, p.136 in Nagata (2000), although it looks rather untidy, would be a good start as it has all the fluxes you need; true it is not for an extreme oligotrophic site but it is open ocean. It would be trivial matter to reconstruct the day time and night time POM and DOM fluxes from the information given in Nagata's figure; the authors can then see what assumptions have to be made to analyse the diel POM changes. I suspect they will find that it is far from the straightforward analysis offered in the present paper.

In would recommend to the Journal that the paper is returned to the authors for major surgery. If my arguments are correct, the present analysis is flawed and need a hard looking at. My intuition is that their observation that phytoplankton only gives rise to 20% of the particle increase will turn out to be a major headache; it certainly is not the blessing they suggest.

Peter J L Williams
21/09/07
pjlebw@bangor.ac.uk

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