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Interactive comment on “Peru upwelling plankton respiration: calculations of carbon flux, nutrient retention efficiency and heterotrophic energy production” by T. T. Packard et al.

Anonymous Referee #2

Received and published: 10 February 2015

From ETS measurements Packard et al. derive several useful parameters concerning the ocean carbon cycling, i.e. respiration, the carbon flux and energy production. They propose that measuring the respiratory electron transport system is able to estimate aerobic as well as anaerobic forms of respiration. The overall idea seemed to be to (re)introduce ETS as a valid tool to estimate aquatic respiration, however, the text lacks precision and in part asks too much background knowledge on the parameters presented without a hint on their importance. Initially I had difficulties to read and even understand parts of the paper, mainly because some of the terminology the authors use was unusual and some definitions of parameters and explanation of thoughts was distributed between different parts of the text. Additionally I missed more detailed

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explanations on several suggestions the authors brought forward - they generally abbreviated these explanations with '... as expected...'

I agree with referee #1 that the paper contains interesting ideas and concepts, however I suggest to go over the text again, clarifying and editing these issues (see detailed comments) before proceeding to the next publication step.

Introduction:

page 16178, line 24-25: Although for insiders it might seem trivial, I suggest to add some citations here.

page 16178, line 26: Even in anoxic seawater it produces CO₂ ... – by it do you mean respiration or the ETS?

page 16180, line 5: The paper by Giering et al. is not unequivocally accepted in the community mainly because they were not measuring respiration but derive it from other parameters. Thus, I would not use this paper as the hook to introduce the concept of deriving the carbon flux from respiration measurements.

page 16179, line 21: Is the review by Aristegui et al. 2009 not the better citation here?

page 16180, line 13: Is the abbreviation EMF really needed; it does not appear anywhere else.

Methods:

In general I suggest to extensively edit the methods for clarity.

The first paragraph (page 16181 – research site) was confusing to me as it was mainly some history of the site that to my mind is better explained in the introduction. I could not find a reason why the authors report C-line numbers and separate station numbers. I suggest to report either C-line or station numbers throughout the text. If either number is important for special purposes (that the authors do not mention) they are in table 1 anyway.

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At the end of the methods section (page 16185) there is a paragraph on ‘ocean setting’ where some of the abbreviations in the first paragraph (e.g. CUEA) are explained. I suggest that in the methods the authors purely introduce their research site (stations, coordinates, etc.) leaving aside all the history of the upwelling area. The ocean settings paragraph should go into the results to my mind. The question that arose from this is whether this paper represents a reanalysis of data. If so, it should be stated more explicitly.

Also I miss at what depth ETS measurements have been taken and how. The sampling depth can be derived from tables but I think it should be stated more explicitly in the text.

page 16182, line 18-20: Why have two methods been used? What is the difference between them and how was ETS measured. I suggest a brief explanation of the method(s) here.

page 16182, line 25: How was RN2 calculated? I suggest to briefly show the calculation here.

page 16183, line 1: Please explain the numbers in the equation. Do not just refer to another paper.

page 16183, line 5: Similar to referee #1 the normalization step was not clear to me. What is it good for?

page 16183, line 6: Please define R_m , z_m and b .

page 16183, line 8-10: Why was R calculated differently in the E_z and in the aphotic zone?

Results:

As is, the results are more a mix of results and discussion. I think the data would lend itself to be it that way. Thus, I suggest to combine the results and discussion.

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page 16185, line 23: I think RO2 appears for the first time here! Please explicitly define in the methods section.

page 16186, line 12: Table S3 should table 3?

page 16186, line 16-20: This part should definitely go into the methods section.

Discussion:

page 16189, line 8-9: Why should HEP reflect RCO2? Please explain more explicitly.

page 16190, line 5-15: What is the main message in this paragraph? I suggest to bring forward your proposed importance of measuring/estimating HEP. What would be the implications if the ratio of ATP/2e- is different?

page 16190, line 25-27: Please be more explicit when stating that HEP should be a small fraction of the solar energy input. Why should this be the case?

Tables and Figures:

Table 1: I don't understand why the dates are so arbitrary and do not correspond to the stations Is surface respiration the average over Ez?

Table 2: As for referee #1 describe the calculation of potential R from ETS activity.

Figure 3a Panel 2: Sort the bars according to depth from left to right, i.e epipelagic upper meso, lower meso etc. I suggest to write out the 'C' to carbon.

Interactive comment on Biogeosciences Discuss., 11, 16177, 2014.

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