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Interactive comment on “Characterization of ocean biogeochemical processes: a generalized total least-squares estimator of the Redfield ratios” by V. Guglielmi et al.

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Since we are not native from an English speaking country, we can understand that the manuscript needs a full copy-edit, and we will do it. We will also add more (and recent) citations (including the 3 suggested), throughout the manuscript.

Most of our work consisted in finding a new way to calculate the ratios (sections 3 and 4). Thus, we are somewhat disappointed that these sections were simply skipped and that the comments are mainly about the composition of plankton in surface waters.

We do not understand the disagreement "with the overarching statement, on page

Interactive
Comment

2409, "Today, the only agreed-upon conclusions are both the confirmation of the Redfield's concept (which has also been extended to other ecosystems than the ocean), and the variability of these ratios depending on the ocean area." ". Since the following sentence says : "I do agree with the authors that at depth, there are similarities to the Redfield ratio (once much of the organic material has been remineralized)". That's exactly what we are saying. Since it is well known that there are large variations in elemental stoichiometry of the surface water, we are excluding the upper layer : page 2384 "... below this mixed layer", page 2404 "... recall that we must consider only the data from below the mixed layer".

We are also saying that this concept has been extended to terrestrial studies (Cleveland and Liptzin 2007, McGroddy et al; 2004).

It follows : "..., however, the plankton in the surface water can cause large variations in the elemental stoichiometry based on the taxa present, nutrient availability and plankton physiology.", which corresponds to the end of our sentence "..., and the variability of these ratios depending on the ocean area. ". The large variations in the thin surface layer, averaged over a large time scale, are diluted and mitigated in the large volume of the deeper ocean, and correspond to the measured small variations (Schneider et al. 2005, Anderson and Sarmiento 1994, Shaffer et al. 1999, Hupe and Karstensen 2000, Li and Peng 2002, Schroeder et al. 2010, Placenti et al. 2013).

In other words we do agree with the referee, and we will rephrase this section to make it clear.

As mentioned in our introduction, all the back-calculation methods used to estimate the anthropogenic carbon in the ocean (and consequently ocean acidification) below the wintertime mixed layer depth, are based upon the Redfield ratios. Thus, it is very important to know, as accurately as possible, how they vary, and how to calculate them.

References :

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Cleveland, C. C. and Liptzin D.: C:N:P stoichiometry in soil: is there a “Redfield ratio” for the microbial biomass ?, *Biogeochemistry*, 85, 235–252, doi: 10.1007/s10533-007-9132-0, 2007.

McGroddy M. E., Daufresne T., Hedin L.O.: Scaling of C:N:P stoichiometry in forests worldwide: implications of terrestrial redfield-type ratios, *Ecology*, 85, 2390-2401, doi: 10.1890/03-0351, 2004.

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Comment

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