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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.

Anonymous Referee #1

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The manuscript presents a method for estimating the N:P:-O₂ stoichiometric ratios of exported organic matter in the ocean. The method presented would be correct IF the stoichiometric ratios were constant, but there is more and more evidence that they are not. Measurements of the stoichiometric ratios in suspended particles (Martiny et al. 2013;2014) show large coherent large-scale variability. Inverse models also estimate large stoichiometric variability in the exported organic matter (Teng et al. 2014; De-Vries and Deutsch 2014; Weber and Deutsch 2010). That the N:P:-O₂ ratios appear nearly constant in the interior of the ocean after correcting for the preformed components, reflects the circulation averaging of the respired material. Indeed the authors themselves suggest as much on page 2409:

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"This variability seems natural: biogeochemical processes do not occur in the same way everywhere, even if they are somewhat homogenized by large-scale ocean circulation."

It might very well be that the constant stoichiometry assumption is valid for some purposes, but it isn't clear from the manuscript if this is the case or not. It seems as if the authors aim to apply the method to separate the respired CO₂ (biological pump) from the anthropogenic CO₂ that is assumed to enter the ocean via the solubility pump. If this is the case then the paper needs to explain carefully why the constant stoichiometry approximation is acceptable for this purpose. I cannot tell from the paper.

Another important weakness of the paper is that interior ocean water masses are never really formed by the mixing of a small number of end members (e.g. Gebbie and Huybers, 2011). The problem is always an underdetermined problem and limiting the number of end-members is a rather artificial way of regularizing the problem.

Also the writing is uneven and not always clear. I list a few examples below but they are not the only cases

(1) At one point the paper concludes that the ratios they estimate are in "good agreement with Redfield's concept" but later they state that they estimate significant variations of the ratios with latitude and depth. Which is it? Either the ratios have no significant variations and Redfield's concept is correct or they do have significant variations and the concept is not correct.

(2) It seems to me that much of the business in section 3.1 could be dispatched by referring to the concept of the Singular Value Decomposition (SVD) of a matrix, which is fairly well known. Even by oceanographers.

(3) There is a detailed discussion of the salts in seawater that is not particularly relevant to the paper and a detailed discussion of the concept of potential temperature, which is well known.

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(4) On page 2385 the paper states that oxidation is the only way in which nitrate concentrations can change below the surface. In fact nitrate is both added by the remineralization of organic mater and removed by denitrification in regions where oxygen is sufficiently low i.e. in bottom sediments for the Atlantic Ocean , but also in the water column for parts of the Pacific and Indian Ocean basins. While both reactions can be referred to as "oxidations" they have different implications for a water-mass analysis aimed at inferring the remineralization ratios of organic mater.

References:

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