

Comment on os-2022-6

Anonymous Referee #2

Referee comment on "Net community production in the northwestern Mediterranean Sea from glider and buoy measurements" by Michael P. Hemming et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2022-6-RC2>, 2022

This manuscript presents a detailed account of glider and buoy measurements of dissolved O₂ and inorganic carbon (DIC) concentrations, which, together with physical fluxes due to horizontal advection, air-sea exchange, and mixing, are used to calculate net community production (N) in the euphotic zone of the NW Mediterranean Sea in spring 2016. Both the data, which are or will be openly accessible through BOUSSOLE, MISTRALS and BODC databases, and the derived N rates are important contributions to our knowledge on the biotic contribution to the ocean carbon sink, which is an important and timely scientific topic. The presentation of methods is detailed and comprehensive, including calibration of sensor, and results and calculations are also clearly presented. I am however less enthusiastic about the conclusions and discussion.

Conclusions seem a little vague to me, mainly focus on what has been done, rather than what has been observed. E.g., "this was the first time that high-resolution vertical profiles covering the wider DyFAMed area provided insights into the biogeochemical and physical processes during a spring bloom." However which are those insights provided by this study is not mentioned. I particularly don't agree with the conclusion that "this study demonstrates the capability of estimating N using measurements obtained by an autonomous glider". I do fully support the approach and agree that gliders have a unique potential to provide estimates of N at spatial and temporal scales that are unsuitable for other methods/platforms. In this regard, I think that the paper is a very substantial scientific contribution. I also acknowledge the conscientious calculations of both rates and uncertainty, and their detailed description in the paper. However I feel that the results obtained here do not demonstrate the capability of gliders for N estimation (which also has consequences for the scope of the paper as a whole):

- Ng (DIC) is 14 times higher than Ng (O₂) converted to DIC assuming a Q_p of 1.45 (85 vs 6 mmol m⁻² d⁻¹) (Table 2). Both cannot be correct, and yet both are estimates of N that use measurements obtained by the autonomous glider.
- Ng (O₂) during the bloom period (19 March-3 April) is 9±36 mmol m⁻² d⁻¹ (Table 2). This is quite a low rate, not significantly different from zero and much lower than N estimations in other blooms (e.g., Goldman et al. 2015 doi: 10.1111/nph.13125;

Seguro et al. 2019 doi.org/10.1016/j.pocean.2017.12.003); it is even lower, despite representing the spring bloom period, than the average daily rate in the area of study throughout the year of $25 \text{ mmol m}^{-2} \text{ d}^{-1}$ (derived from the 20 years annual mean N (O_2) of $9.2 \text{ mol m}^{-2} \text{ a}^{-1}$ in Coppola et al, 2018, as cited in the manuscript).

- The large uncertainty in N_g (DIC) implies that the values calculated for the whole sampling period ($44 \pm 94 \text{ mmol m}^{-2} \text{ d}^{-1}$) and during the bloom ($85 \pm 98 \text{ mmol m}^{-2} \text{ d}^{-1}$) are also not different from zero. This means that neither of the two N estimates based on glider data [N_g (DIC) and N_g (O_2)] are different from zero within their uncertainty.
- The dynamics of N_g (O_2) do not agree with either the dynamics of DIC captured by the glider [N_g (DIC)] or the dynamics of O_2 in the buoy [N_b (O_2)] (Fig. 9). Specifically, there is a period (ca. 25-31 March) when the bloom seems to be well established as shown by sustained high rates of N_g (DIC), N_b (O_2) and N_b (DIC), when N_g (O_2) remains low (often very close to zero within its uncertainty).
- Such differences in magnitude and trends lead to stoichiometric relationships falling far from the physiological range. Different time scales of N, P, C and O dynamics, the limited N and P dataset and broad assumptions and uncertainties in the stoichiometric calculations may partly explain the discrepancies. However, when the average QP is calculated based only on glider O_2 and DIC measurements, the result is 0.14 ± 0.81 , that is zero within its uncertainty.

I am by no means trying to undermine the importance and potential of the approach or the data, but only to underline some contradictions that put into question the conclusion above.

In this regard, I think that the manuscript requires an extended discussion on the potential causes of these different results. I agree with the reviewer #1 in that discussion is comparatively too short, and I would say that it is also incomplete. This study can be very useful in constraining the poorly known variation of N, however the lack of discussion on which of the contrasting N estimates can be taken as best guess means that we are left with a range of N during the bloom between 9 ± 36 and $128 \pm 90 \text{ mmol m}^{-2} \text{ d}^{-1}$. I think that the paper would improve with a discussion on the causes for the large differences between the different N estimates, which are the rates that better explain the backscatter and chlorophyll-a build-up in the region, and a more complete comparison of bloom N estimates with the literature (including blooms elsewhere). Results here are only compared with Coppola et al. (2018) and Copin-Montégut (2000), yet it is acknowledged that such a comparison is not possible "because each study is focused on different timescales (from years to days) or different seasons."

Other specific comments:

- Figure 1 presents surface chlorophyll a concentrations on 24 March 2016. I think that the climatology for the period of study or the bloom period would be more useful as context for N estimates.
- Which are the consequences of using a single mean euphotic depth of 46 m for calculating N throughout the study. Large temporal differences in backscatter (Fig.5) and spatial differences in chlorophyll-a (Fig.1) suggest that the actual euphotic depth should have changed substantially during the period of study, particularly associated to

the phytoplankton bloom. Do this have an impact on N estimation under different scenarios? More specifically, is the calculated N an unbiased estimation of euphotic zone net community production both during periods when Z_{euZlim} ? On the other hand, photosynthetic gross production (GP) is limited to the euphotic layer, however the respiration (R) of the organic matter produced is not; beyond entrainment, do the large changes observed in the ratio between the euphotic and mixed depths (Fig.5) have an effect in the interpretation of N?