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Comment on bg-2021-315

Emmanuel Boss (Referee)

Referee comment on "Physical mechanisms for biological carbon uptake during the onset of the spring phytoplankton bloom in the northwestern Mediterranean Sea (BOUSSOLE site)" by Liliane Merlivat et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-315-RC2, 2021

Reviewer: Emmanuel Boss, UMaine.

This paper focuses on the dynamics of DIC, light and chlorophyll in March and April at two sites in the Ligurian Sea, linking those dynamics to atmospheric forcing and stratification. The measurements from two buoys are also enhanced with measurements with a glider. The claim in the paper is that 'These analysis support the hypothesis that decreases in the depth of active mixing, a result of the transition from buoyancy-driven to wind-driven mixing, control the timing of the spring bloom.'

Since what is considered a bloom is not defined in this paper, it is impossible to judge whether the result suport this hypothesis (see below).

The paper is short, clear and of interest to the readers of Biogeosciences. I have, however, several comments, that if addressed will make this paper of much more interest. Since these comments are significant I suggest a major revision is necessary.

- The concept of a 'bloom' is never defined as is that of the 'onset of the bloom'. The two competing theories you relate two (Sverdrup's and Behrenfeld's) are focused on when the ***depth integrated phytoplankton biomass*** starts accumulating. This, I believe, occurs much earlier than at March in the region in question.
- For surface concentration to accumulate, mixing with phytoplankton deplete waters needs to cease, which requires a change in heat flux. This indeed happens around March-April as described here, though it is not, typically, a smooth process but rather involves passages of storms. It is also a period of very rapid phytoplankton accumulation as stratification drives higher phytoplankton growth rates. For this to be the bloom initiation, one needs to define the bloom based on ***accumulation rate of***

surface concentrations being above a certain threshold.

- In todays ocean DIC dynamics are driven primarily by the solubility pump (which keeps increasing as anthropogenic CO2 is put in the atmosphere) and to a significantly lesser degree by ocean biology. Be good to provide the relative strength of each and hence the sensitivity of the DIC measuremnts to NPP.
- The neglect of advective effect is justified on longer time scales rather than short scales (as claimed here) as spatio-temporal scales tend to correlate in the ocean. While ML deepening is often well described as a 1-D process, restratification is most often a 2-3D process driven by horizontal gradients (e.g. papers from the MLML experiment in the N. Atlantic, and many papers trying to use PWP model to study upper ocean dynamics). To convince one that indeed here 1D dynamics control restratification locally, such an exercise needs to be shown (e.g. PWP modeling showing that the density structure is consistent with local forcing only).
- The abstract ends with 'We estimate net daily community production in the mixing layer over periods of 3 days between 2016 and 2019 as between 38 mmol C m−2 and 191 mmol C m−2. These results have important implications on the oceanic carbon cycle and biological productivity estimates in the Mediterranean Sea in a scenario of climate-driven changes of the wind regimes.' – there no discussion of climate-driven changes of the wind regimes or the importance of the specific values reported anywhere else in the paper.

Given given the above major issues, I am not providing minor comments (e.g. significant digits in DIC values, etc'). Those could be dealt with in future itterations.