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Interactive comment on "Is dark carbon fixation relevant for oceanic primary production estimates?" *by* Federico Baltar and Gerhard J. Herndl

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Reviewer #2

1. Reviewer: This is an interesting small paper that reviews data on dark 14C incorporation in the ocean and that postulates that it amounts to a relevant % of total primary production and that should be considered in evaluations of global primary production. I'm sympa- thetic with the author's effort as I had somehow surprisingly been puzzled by the lack of reference to dark C fixation (which it was a classic in the 80s, considered as "errors" of the Steeman-Nielsen method) I like the paper, I find the issue sensitive, and the anal- ysis is certainly worthwhile.

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Comment: We honestly appreciate the positive words and support of the reviewer.

2. Reviewer: There are only a couple of points that could be discussed and that would benefit the ms. First point is stated at line 85: “dark C fixation had been attributed to the inaccuracy of the 14C method. . .” Could you expand on that? Could you tell the reader why the authors at the time thought this was an error? Why dark fixation was never considered primary production? Maybe this was due to the authors considering dark fixation as, at least in part, abiotic fixation? How do you deal with abiotic fixation in your estimates?

Comment: We had already provided an explanation of what it was meant by that in the first paragraphs of the introduction. As we explained in those paragraphs, the 14C method was developed with the aim to quantify the “photosynthetic” carbon production, so that is why they were mostly focused on what happened in the “light” incubations. That is why, is understandable, that from that point of view, the fixation that took place in the dark would be more like an error, since in general they were not considering processes that would fix DIC in the dark to be of importance. However, during the last decades we have learnt a lot about potential metabolic processes that can and do perform DIC fixation in the light. Concerning the abiotic DIC fixation, that was an issue until 1979, when Lean & Burnison (doi: 10.4319/lo.1979.24.5.0917) introduced the HCl treatment correction. They showed that when this step is performed (removal of inorganic 14C by acidification) adsorption becomes negligible. In our case, the data we used was generated by BATS and HOTS, in which this step was routinely performed.

3. Reviewer: A second point concerns to the night extrapolation of the daytime dark incorporation rates. The authors correctly identify mechanisms by which one should not assume nighttime fixation to be equal to daytime fixation (lines >160). However, I wonder how diel changes in organism activity or in water chemistry warrant that the daytime dark fixation should be above or below the night time value. Did anyone ever measure nighttime dark fixation?.

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Comment: That is an interesting point, but to our knowledge no one has done that. Probably the reason for that could be that scientists in the field measuring 14C fixation were mostly interested in photosynthesis, and therefore they would do the incubations during daytime.

4. Reviewer: A third issue that could be expanded is the Table 1 increase in % dark incorporation in the 70-150 m layer. I think it was a good idea splitting the calculations by layer, but you should maybe make very clear whether this layer contains the DCM in all cases and then speculate as to why the DCM or the layer below the DCM should have a larger proportion of chemoautotrophs or anaplerotic reactions. Also, maybe the layer split could be made more clearly separating above-DCM, in-DCM and below-DCM depths.

Comment: The main reason why we decided to not only provide the integrated value for the whole layer but to also split it into two layers was because we realized that there was a clear depth-related pattern (increase) in the dark/light DIC fixation (Figure 1). We also thought about the DCM, and its potential influence. We realized that for both stations, most of the times (excluding when surface spring blooms) the DCM was at 65-75 or deeper (Fig. 2D). Based on the available sampling depths for BATS (i.e., 0,50,75,100,150 m) and ALOHA (i.e., 5, 20, 40, 65, 100, 140 and 175 m), the dark/light DIC fixation plots/dynamics (Figure 1) and on the position of the DCM (Fig. 2D) we decided to split into two layers at the sampling depth of 75 for BATS and 65 for ALOHA.

Action: We have now mentioned in the text the relative position of the DCM and how it related to the depth layers we defined (p.4, l.140-141); it reads: “The deep chlorophyll maximum (DCM) was located, most of the times (except during spring blooms), in the deeper layer (Fig. 2D).”

5. Reviewer: Finally, I’m uneasy about the 4x difference in estimations between ALOHA and BATS. I can’t find any hint of the reasons for the differences, other than

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different people doing the estimations. You should recognize this difference and suggest an explanation if at all possible. Can the differential oceanography of both sites play a role?.

Comment and Action: It is difficult to know exactly the reason why, since for that we would required a much more extended and deep knowledge of the physiology of autotrophic, chemotrophic and anaplerotic organisms/processes than what is nowadays available. Nevertheless we have recognized the difference as suggested by the reviewer and suggested a potential argument in that respect (p.4, l.146-151); it reads: "The reasons for these differences found between BATS and ALOHA are unknown but could be related to the contrasting nature of primary production found in these regions. In BATS, a negligible contribution from N2 fixation to N budget has been found from δ15N budget exercises (Altabet, 1988) and inversion models (Wang et al., 2019). In contrast, in ALOHA, δ15N budgets and inversion models estimate that 30% to 50% of export production is sustained by N2 fixation (Karl et al., 1997; Wang et al., 2019)."

6. Reviewer: Also, and about the shift of dark C fixation (or at least the proportion) occurring at BATS after 2013, I would appreciate a little bit of hypothesis-building providing a mechanistic linkage between the deepening of the mixed layer and the beneficial? effect on anaplerotic fixation (why should it be bene- fited?) or chemoautotrophy.

Action: We have explained this now in the text (p.4, l.132-135); it reads: "Thus, this relative decrease in chlorophyll-a (and PP) relative to the dark DIC fixation might explain the increase in the dark to light DIC fixation ratio in recent years, while also suggesting that autotrophic DIC fixation seems more sensitive to a deepening of the mixed layer than dark DIC fixation."

7. Reviewer: And just a tiny other comment: l. 59. Citation missing here!

Comment: Done

8. Reviewer: Good paper that should be published. My comments point to clarifications

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and further insight that would, I believe, make the authors' point even stronger.

Comment: We appreciate again the contribution and support of the reviewer.

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