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

Anonymous Referee #2

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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 sympathetic 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 analysis is certainly worthwhile. 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

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Discussion paper

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? 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? 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. 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 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? 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 benefited?) or chemoautotrophy... And just a tiny other comment: l. 59. Citation missing here!

Good paper that should be published. My comments point to clarifications and further insight that would, I believe, make the authors' point even stronger.

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