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***Interactive comment on* “The seasonal cycle of $\delta^{13}\text{C}_{DIC}$ in the North Atlantic Subpolar Gyre” by V. Racapé et al.**

Anonymous Referee #2

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This paper presents new ^{13}C data set in subpolar Atlantic region, bringing new information about recent d^{13}C temporal variation in this region with both seasonal and interannual scales. I think the final conclusions deduced from the observed data are almost true, but method of data analysis, at least description of that, contains many inaccuracy and sloppiness. I recommend the Author to make substantial revision for this manuscript, paying more attention to input enough information so that the reader can understand what the authors actually did.

specific comments:

[1]about formulation of $\text{Dd}^{13}\text{C}_{\text{bio}}(\text{W-S})$

First of all, I believe that the equation (2) appeared in this manuscript has mistype. This

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equation should, at least, be as follows:

$$\text{Dd13Cbio}(W-S) = r(c:n) \times \text{dC13org}(S) \times [\text{NO}_3(S)-\text{NO}_3(W)]/\text{DIC}(s) \times \text{d13Ccarb} [\text{Alk}(s)-\text{Alk}(w) + \text{NO}_3(S)-\text{NO}_3(W)]/2\text{DIC}(s) \text{ [eq.A]}$$

Second, when based on the original equation of Dd13Cbio in Gruber [1999], the above equation should be as follows:

$$\text{Dd13Cbio}(W-S) = \text{d13C} \times [\text{C}^*(s)-\text{DIC}(s)]/\text{C}^*(s) + r(c:n) \times \text{dC13org}(S) \times [\text{NO}_3(S)-\text{NO}_3(W)]/\text{DIC}(s) \times \text{d13Ccarb} [\text{Alk}(s)-\text{Alk}(w) + \text{NO}_3(S)-\text{NO}_3(W)]/2\text{DIC}(s) \text{ [eq.B]}$$

Eq.[B] can be approximated to Eq.[A] only when $\text{C}^*(s)$ nearly equals to $\text{DIC}(s)$. The author should specify that they made this assumption, if they actually adopt Eq. [A]. However, I doubt validity of this assumption. As $\text{C}^*(s)$ nearly equals to $\text{DIC}(w)$ in this case, the value of the first term in the right side of Eq.[b] becomes around 0.02 (i.e., $1.0 \times [2150-2100]/2150$) when using the data read from Fig. 2. This value is roughly same order of $\text{d13Cphy}(W-S)$ for "climatological seasonal cycle" in Table 2. In another words, the author's estimation of $\text{d13Cphy}(W-S)$ in Table 2 is biased, to a greater or lesser, by the author's assumption in choosing Eq.[A] instead of Eq.[B].

[2]Section 3.1

The authors claim in this section that d13C decreases from 2005-2006 to 2010-2012 while DIC increases during this period. However, I find difficulty to detect this signal from Fig.2, especially in the case of DIC . In addition, I cannot decide this DIC increase signal to the anthropogenic effect, because DIC can also increase only if SSS has been increased during this period. I thus recommend the authors to change Fig.2 to show temporal variation of NDIC instead of DIC , or add graphs of salinity temporal variation.

[3]Section 3.2

Here the authors show that the d13C-DIC relationship has same slope in both period of 2005-2006 and 2010-2012, introducing the results of "t-test". However, t-test can only say that "we cannot say that two slopes are different." Indeed, the similarity of two

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slopes have important information. It means that there is little physical effect to both the DIC and d13C temporal variation, and that there is little change in $r(c:n)$ during this relatively short period. I thus want to distinguish whether the obtained slopes are truly same with high precision, or simply we cannot say about the difference due to the large error bar. We can distinguish these two if you directly show the error bar of each slope estimation in Eqs. (4) and (5).

#p14522, l23: "higher D113C" =>"lower D113C" ?

[4]Section 3.3

I feel that many passages of this section is ambiguous. Please polish the sentence to make the authors' argument clear.

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