

Interactive comment on “Trends of anthropogenic CO₂ storage in North Atlantic water masses” by F. F. Pérez et al.

Anonymous Referee #2

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Review of “Trends of anthropogenic CO₂ storage in North Atlantic water masses” by F. F. Pérez et al

This contribution discusses the development of anthropogenic CO₂ storage in the North Atlantic over the last three decades by considering both the trends in anthropogenic CO₂ concentrations as well as the changes in water mass distribution that have taken place over the years. It is an exiting approach, which provides important information, however, there are many issues that must be dealt with before it can be published. These are given in the following.

1. Unfortunately I found the paper extremely frustrating to read, because of three issues: (a) The language is far from adequate. There are numerous examples in the text of wrong use of words, odd formulations, strange sentences structures, wrong tense

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etc. The language must be significantly revised. (b) Section 3 was tedious to read. It contains 10 equations, many of which are essentially repeats of each other, but with minor modifications (e.g. (1), (6) and (10)), they do not appear in order (Eq. (10) is mentioned before Eq. (7)) and there is simply an excessive amount of explanatory text. This section should be shortened, it should be simplified, any adjective-rich subjective evaluation should be removed (for example page 171, line 19, “to produce high performance parameterizations”). Please, briefly outline your line of thought, and then briefly provide the computational framework with the key equations. (c), the results are also confusing, and I think the paper would benefit if figure 3 was introduced at the very start, the main results on Cant storage rates and their variability was summarized, and then discussed in terms of causes and effects. Table 2 can be replaced with a figure that shows the trends of layer thickness, T, S, AOU and silicate. At least a figure with layer thickness should be included.

2. Abstract and elsewhere. The term “storage capacity” is used. I think of capacity as unused potential, what can be absorbed, not has been absorbed. The authors seem to use the latter. Another word should be used, in many cases “capacity” can just be deleted.

3. Abstract, line 22 “Detrimental” is not the right word, use “reduced”.

4. Page 166, line 25. When referring to Sabine, it is “anthropogenic CO₂ sink” not just “CO₂ sink”

5. Page 167, line 5, replace “bring forth” with “have”

6. Page 167, line 7. The extent to which as slowdown of the MOC would reduce ocean CO₂ uptake is a matter of debate, Swingedouw et al, GRL, 2007. Consider to use more than the Sarmiento and LeQuéré reference.

7. page 167. During high NAO, when LSW formation is reduced, Nordic Seas convection is intensified, and we may expect ISOW with higher loads of Cant into the North

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Atlantic. Perhaps the authors may quantify the extent to which this cancels the effect of reduced LSW formation on Cant column inventories?

8. Page 168, line 22. I'd expect Oaces in full caps (i.e. OACES)

9. Page 169, line 17, replace "exceptionally" with "on some occasions"

10. Page 169. Corrections to the TTO data were suggested by Tanhua and Wallace, 2005. Use these.

11. Page 170, line 6, I do not understand, some words must be missing.

12. Page 170, line 13. Replace "3-D grid nodes" with sampling depths

13. page 171, lines 11-24. I question the validity of using an approach that has not yet been published in a peer-reviewed journal, to calculate the anthropogenic CO₂ concentrations. I also question the clearly highly subjective review that is given here. In order to give credibility to their results I think that the authors must (1) use an additional approach, which has been reviewed positively, and evaluate whether this gives the same trends, and (2) they must tone down their review of their own approach. Please state only accuracy.

14. Page 171, line 22-23. The method does not bring the estimates from the other methods closer together, it estimates Cant concentrations which falls between the estimates of the other methods. Please revise section.

15. The following 7 pages (to 178) must be revised for clarity and brevity, as I have required in part (b) of my first comment.

16. Eq (1) has density in it, but not eq 6 and 10.

17. Page 173-174 The approach assumes that the ratio of change in layer thickness (F from Eq. (2)) is constant over the whole basin. How valid is this assumption? This must be evaluated. An initial trial can be carried out by looking at the $F_{b,l,C}$ term. It should be same at every station on a given cruise. If not, then I question the validity of

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the approach.

18. Page 176, line 10 to page 178, line 10 These two pages describes the approach that is used to correct for any bias that may arise following sparse spatial sampling coverage in the ENA basin, i.e that the cruise data may not be representative of the average conditions in the ENA basin at the time the cruise was carried out. It uses the cruise data to determine the MLR fit between Cant and (AOU, theta, and S). This MLR fit is then applied WOA and cruise data at the “same locations of the considered cruise track” to calculate the corrections, eq. (8). I do not understand how this can correct for biases in average values that arise from too poor sampling coverage, I mean – the equation is applied to data from the cruise track only, and regardless whether it is WOA or cruise data, spatial biasing may occur. This needs to be fixed. If I misunderstand, then the section must be revised for meaning and clarity.

19. Page 180, line 7-20. This section discusses the extent to which different cruise tracks has had an influence on T, S, AOU and silicate. Why is this effect not discussed for anthropogenic CO₂ at all? It might be important. It must be dealt with.

20. Page 181, line 9, the statement on reduced convection must be referenced. In fact, when I think of it, I do not think that anybody has seen reduced overflow from the Nordic Seas in the recent years (and so this is relevant for my comment 7), see Steffen et al, Nature, 2008

22. Page 182, line 9-15. Is the concentration of Cant in the uNADW the same as in the INADW? I would expect otherwise, since uNADW is younger (as reflected in AOU). If it is the same, please explain why.

23. Page 182, lines 15-18. Any method would give lowest concentrations of Cant in these watermasses, therefore it is not valid to use this as a support for the Vázquez-Rodríguez method.

24. Page 183, line 1-2 Neither Corbiere nor Schuster attributed the reduced air-sea

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flux to increased stratification.

25, page 183, line 6. Le Quéré does not quantify how much the changes in the NA has contributed to the increase of atmospheric CO₂, we have no idea whether it is significant or not.

26. page 186, line 21-24. Corbiere, as well as Schuster et al, evaluated changed in air-sea CO₂ fluxes. This paper evaluates anthropogenic CO₂ storage. Since air-sea fluxes have a natural component as well, and since water moves around, transporting CO₂ as well, these are not the same things. It is therefore not correct to state that these results support each other or are in good agreement, since they are not comparable.

27. Table 2. Include basin name in each table header.

28. Include a figure with at least the layer thicknesses over the years

29. Quantity the relative contribution of layer thickness and Cant changes on the inventory trends.

30. My final comment is on the uncertainties, which are not dealt with at all. There are many sources, for example (1) uncertainty of anthropogenic CO₂ estimates (2) from spatial biasing of average values (see comment 19), and (3) the assumption of a constant $F_{b,l,C}$ (comment 17), and measurement errors. How significant are the trends after the effect of these uncertainties have been taken into account? Please identify, quantify and propagate all errors you can think of, and evaluate the significance of the trends in light of these.

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