Biogeosciences
Discussions

Interactive comment on "Transport and storage of anthropogenic C in the Subpolar North Atlantic: Model–Data comparison" *by* Virginie Racapé et al.

Anonymous Referee #1

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1 Summary

Racapé and co-authors use a combination of model results and observations to assess the relationship between variations in transport and storage of anthropogenic CO_2 in the subpolar North Atlantic. They find that while transport dominates the year to year variability in the rate of change of storage, the long-term increase is primarily driven the uptake of anthropogenic CO_2 from the atmosphere.

2 Evaluation

While recent research has demonstrated that the oceanic sink for atmospheric CO_2 is much more variable than previously thought, the exact mechanisms underlying this variability, and in particular the contribution of transport remains unclear. Thus by assessing the relationship between the transport and air-sea fluxes of anthropogenic CO_2 in the subpolar North Atlantic, Racapé and co-authors are addressing an issue of relevance and one that is certainly of interest to the readers of *Biogeosciences*. With its overproportional contribution to the global ocean carbon sink, the Atlantic is certainly also a good place to study the role of climate variability.

The approach taken by these authors is sensible and the methods generally sound and well explained. I very much like the approach of combining models and observations as this permits to take advantage of the complimentary strengths of both approaches. The results are relatively clear and the conclusions are well based on the presented results. Thus, in many respects, this paper deserves to be published. But, I am afraid to write, the authors need to put in quite some additional effort in order to bring this manuscript over the (high) bar of *Biogeosciences*. My reservation is based on three major concerns.

- (i) *Evaluation*:

  On the one hand, the authors spend a lot of text and effort on the evaluation of the model results. But on the other hand, this evaluation is biased in that they nearly exclusively rely on the data from the Ovide section. In order to assess the relationship between air-sea fluxes, transport and storage of anthropogenic CO_2 in the model, it is necessary, in my opinion, to assess all of these elements and not just the data along the southern boundary. A particularly glaring gap is the lack of assessment of the air-sea CO_2 fluxes. Of course, the data provide constraints on the total air-sea CO_2 flux and not only on that of anthropogenic

CO_2, but a demonstration that the model is capturing the observed variability in the total flux would substantially strengthen the analysis. Further elements to assess include also the transport across the northern boundary, which is as large as that through the well assessed southern boundary.

- (ii) *Depth of analysis*:

  This is my most important concern. As it stands, the paper is imbalanced in that too much effort is spent on evaluating the model results (also for aspects that are not so relevant for the question at hand), while the main objective of the paper is not covered in sufficient depth. As it stands, the manuscript remains essentially descriptive in its discussion of the variability in the different terms making up the budget, but does not really identify and discuss the underlying mechanisms. For example, it would be important to know and understand what drives the variability in transport, fluxes and storage. A correlation with an index is not really insightful enough here. What is needed is a disentanglement of the relative contribution of mass transport, changes in concentrations, and residence times within different water masses (density classes). The approach taken by Daniele Iudicone could serve as an excellent template here, i.e., Iudicone et al. (2016).

- (iii) *Discussion*:

  The article does not really contain a discussion, i.e., a place where the results from this study are put back into the context of other people's work. I also miss a thorough assessment of the robustness of the conclusions given the uncertainties and biases in the model and the data. Finally, there is also no discussion right now about what this all means and what we should conclude from this regarding the future uptake of CO_2.

3 Recommendation

I cannot recommend acceptance of this manuscript as is. It will require a major revision in order to address my major concerns. In particular, it would behoove the authors well if they re-focused this paper from a model evaluation paper to one that analyzes and discusses the anthropogenic CO_2 budget, its variability, and the processes driving these variations.

4 Minor comments

Abstract: The abstract is a good example to illustrate my most important concern. In essence not much more than 4 lines (end of line 30 to the beginning of line 35) is devoted to the results and the underlying drivers, while the remaining 13 lines are devoted to motivation, method, and outlook. This is not a good balance, in my opinion. Concretely, I suggest to shorten the introduction part (lines 20 through 26) and the method and evaluation parts (lines 26 to 30), in order to generate the necessary space for a more in depth discussion of the results and the key governing processes.

Introduction, line 44: Add uncertainty to uptake fraction.

Introduction: The introduction provides a nice summary and concludes with a clear objective. However, I am wondering about a missed opportunity here. By focusing exclusively on the role of anthropogenic CO_2, the authors forgo the opportunity to truly link air-sea CO_2 fluxes, transport and storage of inorganic carbon. This is merely a thought, and by no means a request to substantially alter the orientation and scope of this paper. But it may serve as a motivation for taking the next step.

Methods: p5, line 166ff: φCT method: Each of the different methods to separate Cant from the background comes with its uncertainties and biases. It would be actually quite

insightful to investigate the robustness of the conclusions with regard to the choice of separation technique. Would it be possible to use estimates from the TTD or the ΔC^* methods?

Methods: p6, line 190ff: Offline approach: It is unclear why some of the calculations were done offline. Wouldn't it have been easier to do all analyses online? This would have avoided the need to neglect the contributions from diffusion and eddy transport.

Model evaluation, p7ff: This section is overly long, and in many respects also not that relevant. Further, as mentioned in my first major comment, it is a bit biased in the sense that the focus is almost exclusively on the data from the Ovide section, thereby omitting important other constraints. For example, there is no need to evaluate the modeled nutrient and oxygen distributions as they are not relevant for this paper. I thus recommend to substantially shorten this section, so that more space is available for the really novel aspect of the paper, i.e. section 4

Model evaluation, p9, line 281: "accumulated arrangement". This is unclear. What is meant here?

Model evaluation, p9, line 307ff: This bias is clearly important, but its implications are only partially discussed later on. This should be improved.

p10, line 336: change "processes" to "process".

p11, line 367: $^{m}T_{Cant}^{adv}$: This is very cryptic. Is it really necessary to use a symbol that is not intuitive and that one needs to look up 2 pages above instead of simply writing the advective transport of Cant?

p11, line 376ff: "overestimation": This is a reasonable interpretation, but key here is really the surface ocean concentration of Cant, and not really the concentrations at depth. Thus, I recommend to discuss this more specifically.

Model evaluation, p12, lines 390ff: This statement is hard to understand and follow without a more thorough discussion of the underlying mechanisms. This is a key find-

ing, and thus needs to be given the emphasis it needs.

Long-term change, p12ff: This is where the paper starts to become really interesting. Unfortunately, only a little bit more than 2 pages are devoted to this most novel aspect of this study. This is clearly unbalanced when considering that section 3 was given more than 5 pages.

Long-term change, p13, section 4.1: This section needs to be improved. As it stands it is very difficult to understand. For starters, I would re-evaluate whether the symbols are really a good strategy to provide clarity (in my opinion, they don't). Also, the authors are providing too many details (also too many numbers), so that the important message gets drowned. Further, the writing is complex and lacks a good storyline.

Long-term change, p13-14, section 4.2: A good fraction of the analysis here builds on correlations. While this provides a good starting point, it does not provide a fruitful avenue to develop a good understanding of the mechanisms and processes driving the responses. As suggested in my major comment above, I think a more process oriented framework would be very helpful here. On top of this, also section 4.2 is not that well written, and like 4.1 could be much improved to increase its readability.

p15, line 492. Wouldn't it make sense to add here a Conclusion section? Otherwise, this last paragraph makes little sense.

p15, line 497 "preconditioning". As far as I was able to discern, this preconditioning has not been shown.

Figure 4: I strongly recommend to add an estimate of the uncertainty to the observation-based estimates of transport.

Figure 5: Caption. Replace "Shadows" with "shaded band".

Figure 5: Caption. Please specify location of the two estimates more explicitly.

Figure 6: Unclear what the standard deviations is based upon and what its meaning is.

Figure 9: Grey bands are not visible in my printed version.