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Biogeosciences Discussions

Interactive comment on "Global ocean storage of anthropogenic carbon" *by* S. Khatiwala et al.

Anonymous Referee #2

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General comments

The manuscript "Global ocean storage of anthropogenic carbon" by Khatiwala et al. presents a review of recent estimates of anthropogenic CO_2 (Cant) uptake, storage and transport by the ocean. The review covers both data-based and model-based methods for inferring Cant uptake and storage. A review of Cant uptake rates based on direct measurements is also provided.

In all, I found the review to be well-written and to well cover the important advances in estimating oceanic Cant uptake. The description of the various data-based methods for inferring Cant uptake and storage (Section 2) is well done, although I do still find that some things require clarification (see below), and I also did find some overlap in this section (and the introduction) with Wang et al (2012, Biogeosciences), including

some wording. The description of model-based inversions for Cant uptake, storage and transport (Section 3) is also clear and thorough, although some things still deserve a bit more explanation (see below). I am not qualified to assess the quality of the review of anthropogenic CO_2 uptake rates inferred from direct measurements (Section 4), but I found it mostly well-written and clear. The conclusions (Section 6) could benefit from a better discussion of how to address the remaining uncertainties in oceanic Cant uptake, storage, and transport.

Overall, I believe that the manuscript should be publishable in Biogeosciences after revisions and clarifications to address the points below.

Specific comments

1. The major issue that should be clarified in a revised manuscript is the relationship between the anthropogenic CO_2 uptake inferred by the GF approach and that simulated by the ECCO model (as shown in Figures 2, 3, 4, and 6). The estimates show a remarkable similarity, although as I currently understand it the GF estimate and the ECCO estimate are totally independent. Are the two estimates independent? Or was the ECCO circulation somehow used as a prior for the GF estimate? If they are totally independent, why is there such excellent agreement between the two estimates? What is the RMS difference between the ECCO and GF estimates?

   I would also like to see GF-ECCO differences for the column inventory (Figure 2), the basin profiles (Figure 3) and the depth profiles (Figure 4) for only the GLODAP region (I assume that the ECCO points in the North Atlantic plot in Figure 4 include non-GLODAP regions and that is the reason why the ECCO estimate is uniformly higher than the GF estimate there).

2. Page 8938, lines 11-12: Only the mean of the TTD was estimated from CFC-12

data. The mean/width ratio was specified.

3. Page 8941, first paragraph: Should also note that negative values have been removed from the ΔC* method, resulting in $\sim$10% higher inventory than if they are left in.

4. Page 8944, lines 11-13: Minor point, but how does one know whether the errors are due to errors in the $CaCO_3$ cycling model or in the circulation? It seems likely that the circulation (too weak ventilation) is the main problem, given the discussion in Graven et al (JGR, 2012).

5. Page 8946-7, last paragraph (and Table A3): The degree to which mapping errors have been quantified deserves some comment. As I understand it, the errors in Table A3 do not include mapping error, except perhaps in some very ad-hoc way. For the GF estimate, a Monte Carlo approach was used to determine the uncertainty. Was the mapping error taken into account in this estimate (e.g. by producing new maps of T, S, CFCs etc. using an objective mapping procedure for every member of the suite of MC simulations)? Or were the observational errors assumed independent (in which case their effect would largely cancel out upon globally integrating).

6. Table A3: It would be nice to see the uncertainties given as percentages as well, to allow for easier comparison

7. Page 8952, line 8-10: This deserves more comment - why do some of the OIP and EnKF models have much stronger storage and transport in the Southern Ocean?

8. Figure 6, northward transport for GF: How are the northward transports for the Indo-Pac and Atlantic separated? One knows the flux in from the atmosphere and the storage rate, but what about the flux in from the Southern Ocean? And what about the northern boundaries?

9. Page 8961, last sentence: I don't see any "important differences" between the ECCO Cant estimate and the GF Cant estimate. See #1 above.

10. The conclusion (particularly the last sentence) is rather weak. The "multiple approaches" discussed in the manuscript have not reduced the uncertainty on Cant uptake, so it seems that "multiple approaches" is not necessarily the answer. It would be nicer to discuss some ways in which the uncertainty and biases of the model and data-based methods could be reduced so as to arrive at better Cant estimates.

Technical corrections

- Equation (1): should define symbols x' and t'
- Page 8939, line 6: define RECCAP
- Page 8939: State the resolution of the CCSM
- Page 8940: State the resolution of ECCO
- Page 8940, lines 10-11: Estimating the Circulation and Climate of the Ocean
- Page 8948, line 10: The assumption of the ΔC* method is more properly stated as "no mixing"
- Page 8953, line 16: "Remarkably robust" seems to be overstating it - maybe just "robust"
- Page 8954, lines 24-25: The text refers to four red lines, but there are only two red lines in Figure 6.

- Page 8957, line 15: “evidenced” (typo)
- Page 8957, lines 23-27: This is confusing. The “earlier” study is that of Sabine et al. (2008) while the study of Waters et al. (2007) is referred to as more recent. Furthermore, somehow the 2007 study is based on data from 2010. Typo?

Interactive comment on Biogeosciences Discuss., 9, 8931, 2012.