

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

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Khatiwala and co-authors provide an excellent overview of total anthropogenic carbon storage in the ocean and changes in storage. They provide estimates based on empirical observational approaches of carbon measurements and transient tracer distribution, and inverse models, and forward models. The authors present the discussion according to: -Observation-based estimates of total anthropogenic CO<sub>2</sub> inventory using the carbon based methods and transient tracers -Ocean inversions and transport of total anthropogenic CO<sub>2</sub> - Decadal changes in anthropogenic carbon determined from repeat measurements - A reconciliation of different approaches to derive at a best estimate of global ocean anthropogenic CO<sub>2</sub> inventory in 2010.

Each section provides a comprehensive discussion of methodologies, its shortcoming and uncertainty, along with ample references to pertinent literature. The manuscript is

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well documented with illustrative figures and tables.

There are no issues with the fine work. It is well written and comprehensive. It will serve as a key attribute for those seeking a comprehensive assessment of storage and transport of anthropogenic carbon in the ocean.

General comments: -There did not appear to be consistent use of total inventory change (Pg C), concentration change (umol/kg) and specific inventory change (mol/m<sup>2</sup>). Moreover, the changes were not always provided in context of total inventory of the basin making it difficult to put the values in context. -The discussion of the different empirical back calculation methods is limited. The problems with the TrOCA method are well described and referenced but the some of the others methods are not. In specific the “Phi CT method” is mentioned in the tables in the Appendices but not discussed in text. - The use of terminology differs for the different RECCAP papers. For instance, Ocean forward models mentioned are also listed as Ocean Global Circulation Models; column inventory is referred to as specific inventory in Wanninkhof et al 2012. -It is unclear why the Tables are in an Appendix rather than part of the main text. There is no preface to the Appendix. - At the end of the references there are the page # listed where they appear in the text. It's an interesting idea, but I've never seen this before.

Specific comments and technical corrections: Line

Page 8933, line 24: state absolute amount of uncertainty: 155 +- 31 Pg C (20 %)  
Page 8934, line 5: “currently sequestering” perhaps change to “which to date have sequestered”  
Page 8934, line 15: delete “(unknown)”  
Page 8935, line 19: Is “forward integrations of OGCMs” the same as “forward models”?  
Page 8937, line 6-14: perhaps mention other approaches (and their issues)  
Page 8938, line 3: “avoids the need for complex and uncertain biological corrections “. This is a bit misleading perhaps rephrase that it a priori assumes that the biology is in steady state  
Page 8939, line 7: “forward model” = “forward ocean model”  
Page 8942, line 13: “preindustrial” = “prein-

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dustrial era” Page 8942, line 23: check the concentrations. I recall that the NATl had up to 70  $\mu\text{mol/kg}$  based on the  $C^*$  method(see Lee et al). Page 8943, line 24: As I recall Graven et al 2012 only compare two models and while they state that the models are a lower and upper bound, I would be cautious stating it as such as it implies that these are the actual upper and lower bounds of uncertainty. You in essence state this concern in the first paragraph of page 8945. Page 8946, line 14-18: switching from  $\mu\text{mol/kg}$  to  $\text{mol/m}^2$  makes comparison difficult Page 8949, line 24: “uses observations” uses “transient tracer observations”. It is important to emphasize that the TTD and GF approaches do not use inorganic carbon parameters, or use them as a weak constraint Page 8951, line 18: “assuming . . . . increases proportionally to perturbation of atm  $\text{CO}_2$ ”. It must be stressed that this is a fundamental assumption Page 8952, line 29: perhaps mention papers by Brewer et al., and Keeling and Peng who were some of the first to assess carbon transport across 26 N Brewer, P.G., Goyet, C., Dyrssen, D., 1989. Carbon dioxide transport by ocean currents at 25 oN latitude in the Atlantic Ocean. *Nature* 246, 477-479. Keeling, R.F., Peng, T.-H., 1995. Transport of heat,  $\text{CO}_2$  and  $\text{O}_2$  by the Atlantic’s thermohaline circulation. *Phil. Trans. R. Soc. Lond.* 348, 133-142. Page 8955, line 5: “transport” = “inorganic carbon transport” Page 8959, line 12: it would be helpful to put these numbers in context, also note change of units in storage rate from page 8958 line 12. Page 8960, line 25: It might be worth a few words how these estimates for the marginal seas were determined. Are these more or less “wild guesses” based on area? Page 8961, line 17: “variability” = “variability and trends” Table 1: Why is the difference between the GLODAP region and model domain not constant for the CCSM models (the difference between number and number in brackets ranges from 9 to 11. Table A2. Why isn’t Green function uncertainty listed here? Table A4: include the average transport and location Table A5: include average storage rate

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