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Biogeosciences Discussions

Interactive comment on "A model-based assessment of the TrOCA approach for estimating oceanic anthropogenic carbon" *by* A. Yool et al.

Anonymous Referee #2

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Review of a manuscript submitted to Biogeosciences MS-NR: bg-2009-154 Version: 1 Title: A model-based assessment of the TrOCA approach for estimating oceanic anthropogenic carbon Author(s): A Yool et al.

Decision: This manuscript is acceptable for publication after major revisions.

General comment:

This study aims at testing an indirect approach, the so-called TrOCA method, that has been developped to derive distributions and inventories of anthropogenic CO2 in the ocean. This method has been applied in various regions, North Indian and Atlantic oceans (Touratier et al., 2007 and reference herein), and compared with different diagnostic approaches and/or ocean model simulations in the Southern Ocean (Lo

Monaco et al, JGR 2005b), in the subtropical Indian Ocean (Alvarez et al., Biogeosciences, 2009) and from Arctic to Antarctica in the Atlantic (Vasquez-Rodriguez et al, Biogeosciences 2009). In most of these intercomparisons analysis, TrOCA presents similarities and differences, but why the TrOCA method leads to coherent or different results is not clear. Many colleagues who used this method, including Touratier and Goyet, suspect that pre-anthropogenic TrOCA, TrOCA0, should be optimised at regional scale. This is also the conclusion of the analysis presented by Yool et al.. In order to quantitatively demonstrate this, the authors used a biogeochemical ocean model (OCCAM) and depending the regions selected to optimize the TrOCA parameters (TrOCA0), they obtain very different results for Cant distributions and inventories. The conclusion is that TrOCA approach could not be applied at global scale, i.e. using an universal parameterisation and should then be applied at regional scale. This is not suprising and I'm not certain that the use of an OBGCM (here OCCAM) could better resolved this question. When one looks at TrOCA results obtained for different regions, it is clear that this approach (as many others) is not valid if one uses what authors called the -default- TrOCA parameters. Very clear examples were recently published in Biogeosciences: see for example the very strange results obtained near or in the strait of Gibraltar (Ait-Ameur and Goyet 2006; Huertas et al., 2009): there, the TrOCA approach leads to the world record of Cant, > 100 μmol/kg, i.e. more than 1 μmol/kg/yr since a century. This clearly calls for regional parameterisations, like in any other model (i.e. biological parameters used in OBGCM like the one used by the authors). To my knowledge, the TrOCA method tested in this study, has never been applied at global scale. Maybe this has been attempted by many scientists and students (TrOCA is a function included in ODV, Schiltzer, 2006) but a map of global Cant inventory based on TrOCA has never been published (I suspect those who developped the method know very well that it is not correct to apply the default-TrOCA at global scale); the present manuscript shows this intriguing result (Figure 6b) based on Glodap/NODC climatologies. This leads to a large Cant inventory, >150 PgC/yr taken up by the ocean since about 200 years. This is an interesting result, but is it true ? (This is more than 50% higher than

previous estimates, Key et al, 2004; Sabine et al. 2005 using C*; Waugh et al 2007 using TTD). I guess Yool et al tried to anwser this question using OCCAM simulations, and found that when they apply TrOCA with the model, they obtain a large range of Cant inventory, between 90 Pg and 460 PgC depending the model-data used to derive TrOCA0 parameters (a,b,c,d,f...). An extreme case is > 1300 PgC when fitting the parameters using subtropical north pacific model-data. The results are thus very sensitive to the choice of regions, basin etc... I guess, all calculations based on OCCAM-Cant are mathematically correct (numerous tables), but back to the introduction, and the aim of reducing the uncertainty on the ocean Cant inventories, I did not really learn new results in this paper. Authors seem to demonstrate that TrOCA approach is not valid but they used simulations fields from an OBGCM which is are not clearly evaluated. Would it be better to use the tracer TrOCA to understand why and where the OCCAM model fails to investigate natural variability ? The authors should first demonstrate that the most important simulated fields used in TrOCA (i.e. DIC, TA and O2 and TrOCA itself) are well reproduced in OCCAM. Instead, authors suggest the model is relatively good to reproduce primary productivity, air-sea CO2 fluxes, CFC concentrations. When evaluating DIC, TA, O2 fields, authors compare the OCCAM results not at global scale, but in a specific region, the North Indian section WOCE/I1, which is probably one of the most difficult ocean sector to simulate: it is a location of complex water masses transformations (Red Sea, Persian Gulf, Rivers in the Bengal Bay....) and nothing is indicated concerning the boundary conditions used in OCCAM for these specific frontiers. I understand that authors have selected this North Indian section because this is where the new TrOCA parameters have been first used (Touratier et al, 2007) but I would prefer a discussion for other regions, such as a North-South Atlantic section (Vasquez et al, Biogeosciences, 2009), a South Indian section around 30S (Alvarez et al. Biogeosciences, 2009) and South Pacific sector (Goyet et al. Biogeosciences, 2009) where TrOCA as been applied and discussed. Although such work would offer an interesting analysis to better investigate errors associated to Cant inventories and consequently levels of acidification (important and timely questions), the present

manuscript does not offer interesting conclusions. Below I also list several questions and comments to address before this manuscript is acceptable for publication:

Specific comments: (follow the pages numbers)

C1: page 7238: ppm is not correct unit for pCO2 (use uatm for pCO2 or change xCO2)

C2: Page 7239: recall units (umol/kg) for CT, Troca, O2...

C3: Page 7241: authors indicate that Touratier et al (2007) used data collected along the WOCE I1 sections in the North Indian Ocean to optimize TrOCA parameters. I think this is wrong. Touratier et al (2007) first used global ocean data from WOCE at large scale to first derive TrOCA0 parameters and then applied the method to the WOCE-I1 sections. This mis-interpretation is recalled in several sections: page 7244, lines 23-25; page 7245, lines 9-10; page 7247 lines 12-14; page 7248, lines 10-11.

C4: Pages 7243-7344: OCCAM simulation. Authors evaluate the performance of OCCAM and they indicate that they would particularly evaluate the carbon cycle, Cant and CFC11. With regard to the present analysis, I think it would be much more interesting to compare DIC, TA and O2 fields (distributions and inventories) in OCCAM, as well as TrOCA distribution itself. The comparisons with air-sea CO2 fluxes and primary productivity are not very demonstrative that OCCAM is able to capture the main processes (carbon cycle) that lead to measure the performance of OCCAM for deriving Cant using TrOCA method.

C5: Page 7243: comparison with surface CFC11 is interesting but figure 1 maybe not very useful. Refering to OCMIP results and CFC inventory is sufficient.

C6: Page 7243: comparison with Cant from Glodap is interesting (figure 2), but it is also presented in Figure 6 with another color scale. Figure 2 could be deleted. The Cant from Glodap is derived from the C* approach and I guess for global distribution it corresponds to the addition of 3 basin scale results (Atlantic, Indian and Pacific), i.e. the global C* inventory was obtained using different C* methods. In a way this

was, as it is recommended for TrOCA, applied at regional scale. Except the TTD method, I don't know any back calculation technic that was applied at global scale. This should be specified when presenting global Cant distribution and introducing the back-calculations technics.

C7: Page 7244: comparison of air-sea CO2 fluxes should be attempted with the most recent climatology from Takahashi et al (2009) that is quite different in the southern ocean sector as well as in the equatorial source. I suspect that OCCAM fluxes were estimated using a different gas transfer coefficient and winds; therefore it would be much more interesting to compare OCCAM pCO2 distribution than fluxes. Because pCO2 experienced large seasonal variabilities, I suggest to present a comparison of pCO2 for two, maybe for seasons.

C8 Authors examine the OCCAM results along the WOCE/I1 sections where TrOCA was applied by Touratier et al (2007). Figure 4 and 5 clearly demonstrate the limitation of this analysis. The distribution of TA and O2 are particularly far from the observations (how the Red Sea and Persian Gulf waters are introduce in OCCAM ?). This certainly creates suspicious results when appying TrOCA in the model (Figure 5b), such as the maximum of Cant in the western section. From these plots, I would conclude that the OCCAM actual Cant (DIC- preindustrial DIC) is relatively correct when compared to observational TrOCA (i.e; this is a validation for OCCAM rather than for TrOCA). It would be interesting to add the C* results from Glodap in this figure. For such observation versus model comparison I strongly suggest to use other sections where default TrOCA has been applied in the Atlantic (Vasquez et al, Biogeosciences, 2009), South Indian (Alvarez et al, Biogeosciences, 2009) and South Pacific (Goyet et al, 2009). The later would be particularly interesting regarding the very high Cant inventories obtained in the eastern pacific using OCCAM-TrOCA (figure 6d). . C9: Page 7246: After the comparison along WOCE/I1 (which again is certainly not the best place to investigate the performance of OCCAM to simulate biogeochemical) authors apply the method at global scale (Figure 6). This is the first time a global map of Cant inventories is

presented using TrOCA method based on observations. The authors derive a very high inventory, up to 155 PgC, which is 50% higher than Glodap. The reason for this is not straitforward. This depends on the method but also on the constraints. Authors use Glodap DIC and TA (for the nineties) and T, S, O2 from climatology. Why not using T, S, O2 from WOCE data, as was done for Glodap estimates ? When comparing ocean models, the same forcing are used (e.g. OCMIP).... When comparing different back-calculation methods it is important to use exactly the same constraints (here the observations, as done in previous comparisons analysis, e.g. Lo Monaco et al, 2005b; Vasquez et al., 2009; Alvarez et al, 2009).

C10 page 7247: to explain why there are such large differences between OCCAM-Cant and default TrOCA, authors identify three points. First the TrOCA method was developped for a regional data set (North Indian): this is wrong, see comment abaove. Second, they consider that OCCAM is a simple representation of biogeochemistry: this is a wrong reason, because simulated OCCAM-Cant (DIC-preindustrial DIC) could be relatively correct with bad representation of TA, O2, even DIC etc.... Third authors indicate that in the context of global results the default TrOCA optimised at regional scale (again wrong interpretation) is not appropriate: this is wrong, and authors could ask the following: why a global TrOCA Cant has not been yet published ? I think Touratier et al know very well that for applying TrOCA at global scale one needs to better optimise coefficient, maybe at local/regional scale. An indication of this could be found in Vasquez et al, 2009, where TrOCA Cant values in the Arctic are different compared to other methods; this is certainly because default TrOCA0 parameters were not constructed using Arctic data....Same conclusions could be derived when looking at results obtained in the Mediterraneen sea waters (Huertas et al, 2009, Biogeosciences).

C11: page 7247: Authors present a side note where they estimate the sensitivity of TrOCA-Cant depending the parameters (theta versus T) , density value, units etc...I suspect the same sensitivity would be obtained for any other method, using both diagnostic or ocean model simulated fields. I don't see what is the main conclusion in the

sensitivity analysis.

C12 Page 7248: the title for the section - Optimising TrOCA - should be, - Optimising Cant(TrOCA) or Optimising TrOCA0 to derive Cant.

C13: Page 7266: two references should be included for LoMonaco et al, JGR 2005a and JGR 2005b

C14: page 7268: in reference for Touratier et al 2007, check the page numbers.

C15: Page 7279: Figure 3 (if used in revision), please specify the direction positive (in) /negative(out) of the CO2 flux.

C16: Page 7282, Figure 6: change the title of Glodap TrOCA estimated Cant as it mixed DIC/TA from Glodap and T,S,O2 from other sources. Title could be: Observational based TrOCA estimated Cant.

C17: Page 7283: Figure 7. Given that Cant derived from TrOCA is not correct in surface layers (<200m or so), my feeling is that all profiles are relatively close expected OCCAM-TrOCA estimate in mid-waters. This should be explained, e.g. regarding the DIC, TA and O2 distributions. For this it would be very interesting to plot average DIC,TA,O2 based on observations and OCCAM. I would also appreciate to see TrOCA profiles from observations and OCCAM, that may also help to distinguish if the problem in mid-waters is related to actual TrOCA or TrOCA0.

C18: Page 7293: Figure 7 (and text in several place). Authors specify that negative Cant values are set to zero before integration. Do they also need to apply this correction when using Glodap Cant ?

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