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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.

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[In the following text, all referee comments are given *in italics*, while our replies appear in normal font]

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.

Our experience with both global and more locally optimised variants of TrOCA is that while it is possible to deconvolute anthropogenic CO2 relatively well in some regions (for instance, the North Atlantic), the errors are much greater in many other regions (for instance, the equatorial Pacific). Through input from another referee, we have expanded our introduction to TrOCA to analyse the issues that complicate its wider application.

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 mol / kg, i.e. more than 1 mol / 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).

Contrary to the suggestion of the referee, our model does not have any regional variability in its ecological/biological parameters. While it is undoubtedly an oversimplification, our plankton ecosystem model is parameterised identically throughout the model domain, and this is a standard procedure in such modelling. Local variability in the concentrations of model components affects the magnitudes of different processes, but the underlying parameters are constant.

Regarding the referee's point concerning regional variants of TrOCA, the use of global-scale observations by Touratier et al. (2007) to optimise TrOCA parameters implies that it should be applicable everywhere. If not, what data remain to optimise it for particular local instances? Furthermore, parameter a in TrOCA is explicitly tied to biogeochemical stoichiometry, which is relatively invariant across the World Ocean. In finding wide (and often non-overlapping) ranges for such parameters, our localised optimisations suggest otherwise. Our results are in full agreement with the reviewer's call for regional parameterisations.

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).*

In our original manuscript, we assumed that TrOCA was optimised locally, and so were not surprised when it estimated much greater anthropogenic CO2 when applied at the global scale. Thanks to comments from our reviewers, we now understand that the optimisation uses global-scale observations. Given that this is the case, the most obvious scale at which to apply it is the global one, not least since no further data remain to optimise it for regional use. To this end, while we have removed material concerning local application of TrOCA, we have retained the global-scale analysis because of these comments. By using observationally-derived fields, this also has the advantage of avoiding the errors inherent in our model's circulation and biogeochemical fields.

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.

We agree with the referee that we could have validated our model more comprehensively. We have introduced new plots comparing model fields to those from climatology

sources to provide the reader with a fuller picture of model performance. Following the recommendation of referee 1, we have moved all validation material to a new appendix.

The new material added to the introduction to TrOCA (please see other replies) provides a theoretical angle that examines TrOCA assumptions and limitations.

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.

As pointed out by referee 1, our use of default TrOCA in our original manuscript is not without problems. To this end, we have removed some of this material and now mention our work with default TrOCA more briefly in the main text. For this reason, we have not expanded our consideration of default TrOCA to other regions as suggested by the referee here, although we would certainly agree that other areas of the World Ocean are potentially of much greater interest than the northern Indian Ocean.

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.

We have significantly expanded our introduction to TrOCA to include the analysis of TrOCA provided by referee 1. This explores the assumptions which underpin the method and complements the manuscript's main analysis of OCCAM output.

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

We have amended the manuscript as directed by the referee.

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

We are uncertain what the referee is indicating here. Should this portion of the text remind readers of the units used in TrOCA?

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 WOCEI1 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.

The referee is entirely correct here. We erroneously interpreted Touratier et al. (2007) to have derived TrOCA parameters from this one region. We have corrected the

manuscript to remove this.

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.

We agree with the referee that our overview of model performance is restricted. To this end we have introduced further figures showing the zonal concentrations of the tracers involved in TrOCA for both OCCAM and from observations (Figures 12 and 13). However, in keeping with comments from referee 1, we have moved this section to a new appendix.

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.

We agree with the referee and have deleted this figure as suggested.

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.

We agree with the referee and have deleted this figure as suggested.

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.*

The referee is correct that the GLODAP global estimate of anthropogenic CO2 is composed of three basin scale estimates. However, while the three basins were estimated separately, the same ΔC* technique was used in each, although slightly different approaches were used in each basin for determining the mixing ratios of the different water masses. This is different from the situation described by the referee.

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.

As suggested by the referee, we have replotted this figure (now Figure 14) with the most up to date (October 2009) version of the Takahashi et al. CO2 fluxes, and have amended the text appropriately.

Regarding comparison between OCCAM and Takahashi et al., there are a number of steps in the calculation of air-sea fluxes and, as suspected by the referee, our OCCAM simulation uses different functional forms to those favoured by Takahashi et al. Given this, the referee is correct to suggest a further comparison, and we have prepared a further figure that shows ocean delta pCO2 for both our model and the Takahashi et al. climatology.

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). .*

As pointed out by referee 1, the use of default TrOCA in our original manuscript is not without problems. To this end, we have removed the material concerning the local application of TrOCA (but have retained the global application). For this reason, we have not expanded our consideration of default TrOCA as suggested by the referee here, although we would certainly agree with the referee that our simulation has deficiencies in the Indian Ocean that are deleterious to our application of default

TrOCA.

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).

Ideally, we would perform our calculations on the raw profiles of observations from the WOCE and other programmes. Since default TrOCA's performance at the global scale is secondary to our aims in this manuscript, we decided to limit our efforts to using the derived climatology datasets instead.

C10 page 7247: to explain why there are such large differences between OCCAMCant 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).

We are grateful to the referee for these points. It is true that our statements here are erroneous or incomplete, and we have amended the text accordingly. On the first point, we have altered the text to note that Touratier et al. (2007) utilise global rather than regional data for their optimisation. On the second point, the referee is essentially correct: since anthropogenic CO2 enters the ocean via physico-chemical proceses, even carbon cycle models devoid of biological processes can represent it well (cf. OCMIP-2). The referee's third point is related to the first, and our explanation at this point is incorrect. We are grateful to the pointers to currently uncited papers in which limitations to TrOCA are considered, and have amended the text to include these published results.

We are also grateful for the referee's observation concerning a global TrOCA estimate. We are not aware of such an estimate, although given that TrOCA's parameters are optimised with global observations, this would be an obvious application of the method. To this end, although we have removed much of the material concerning default TrOCA, we have retained its application to the global scale.

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.

We added this side note purely as a practical aside for users of the TrOCA method. We ourselves got caught out a couple of times when we accidentally used data in the wrong units with default TrOCA. We have altered the text slightly so that it is clearer that these are practical notes rather than a formal parameter sensitivity study.

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

We agree with the referee. The section title implies that we are optimising the TrOCA method rather than its parameters. We have changed the title to "Optimising TrOCA variants".

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

We have amended the references as suggested by the referee.

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

We have checked the page numbers against both the journal website and a PDF version of the paper and believe that we are using the correct ones.

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

We have modified the Figure (now Figure 14) so that it is clearer which direction represents the flux of CO2 into the ocean.

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.

We have modified the title to "Observational, TrOCA estimated Cant" (note: now Figure 2).

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.

This question lies beyond the scope of this study. The main reason is that the distribution of TrOCA is a reflection of the air-sea exchange of CO2 and oxygen, and that a detailed investigation of model-data misfit for TrOCA would quickly develop into a detailed investigation of the full set of complexities associated with the cycles of natural CO2 and oxygen. Our focus here is on anthropogenic CO2, and how well the TrOCA method is able to recover this signal.

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 ?

The referee is correct: Figure 7's (now Figure 3) caption is incorrect. While other plots show results where negative values are set to zero, this plot leaves the negative values unchanged (hence the slightly negative anthropogenic CO2 in the ΔC^* GLODAP profile). We have amended this in the manuscript, and have ensured that other plots and captions are accurate.

Interactive comment on Biogeosciences Discuss., 6, 7231, 2009.