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Biogeosciences Discussions

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Interactive Comment

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]

As part of his original review, Prof. Gruber included a significant quantity of theoretical material that complemented the analysis in the original manuscript. Because of its strong relevance for our work, we wished to incorporate this material into the final published manuscript, but also wished that Prof. Gruber was properly acknowledged for his efforts and insight. As a result, and with the full knowledge of the Biogeosciences editorial board, we approached Prof. Gruber and asked to join us as a co-author of our manuscript. This offer was accepted by Prof. Gruber, and he now joins the manuscript as a full co-author.

The study is well designed and the conclusions are fully supported by the presented work. Some potential weaknesses stemming from the particularities of the model have been identified and have been addressed. I particularly like that the authors attempted to fit various regional parameterizations for preformed TrOCA to see whether this would improve the accuracy of the method. The paper itself is overall in good shape, but I have several detailed issues and two overarching issues.

The first issue is that the authors loose a lot of the potential clarity of the paper by including material that is not essential. First - and I point this out also to counter an argument that is made in another review - much of the evaluation of the model against observations can go into an appendix since it is only marginally relevant for the assessment of the TrOCA method (or any other method). What matters is the relationship between the different tracers, and those are usually captured much better in the model in comparison to the spatial distribution. Second, having acknowledged the first point, it is imperative that the parameters of the fit are retuned using the model results. Therefore, the section where the authors use the parameters as established by Touratier et al. (2007) can be skipped entirely. If these two sections were (re)moved, the paper could be streamlined substantially, permitting the authors focus directly on the assessment of the TrOCA method. By doing so, the paper will become much more accessible to the average reader.

We agree with the referee, and have amended the manuscript such that the model-observation comparison is removed to an appendix of the manuscript. Since referee #2 has expressed a strong interest in our use of TrOCA at the global scale, and since we have latterly realised that default TrOCA is optimised using global-scale observations, we have retained a shortened form of this section that focuses on the global analysis only.

The second issue is that the paper would benefit if the authors explained in more detail why the method is having problems in retrieving the amount and distribution of anthropogenic CO2 correctly. Below, I provide below some additional background material on the TrOCA method and also give my interpretation for why TrOCA does not work well. I invite the authors to peruse this material if they are interested.

We are extremely grateful to the referee for this analysis of factors underlying the TrOCA methodology that may explain our difficulty in using it to deconvolute our model's anthropogenic CO2. We have expanded the section on the TrOCA method to include this detailed analysis.

section 2.1.3, p7237, line 21, GLODAP DIC. It would be good if the authors specified which DIC field they used from the GLODAP database. I very much hope they used the pre-industrial DIC field.

We have amended the manuscript to note that we initialised with the "pre-industrial" DIC field from the GLODAP climatology.

section 2.1.3, p7237, line 24, This is a relatively short spinup. How large is the drift of the model at the end of this spinup?

At the end of the model's spin-up period (47 years), the globally integrated air-sea CO2 flux is 0.1387 Gt C y-1 (for years 43-47) and 0.1376 Gt C y-1 (for year 47). This is now mentioned in the text.

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section 2.1.3, p7237, line 26, I am worried about the fact that Yool et al run the model for the period of 1864-2004. Yes, the anthropogenic CO2 perturbation was relatively small, but persistent. This anthropogenic CO2 would have had time to invade deep into the ocean, leading to signals there that otherwise would be absent. I think the authors need to address this more up front. My take is that this does not matter much for testing the TrOCA method, but it matters substantially when comparing the model to e.g. DC derived Cant estimates.*

We thank the referee for directing our attention to this erroneous assumption on our part. Because of our simulation's computing cost, we followed the approach of Cox et al. (2000; Nature 408, 184-187) of "shaving off" the first 100 years of the anthropogenic transient (and spinning up under 288 ppm CO2). Our naïve assumption was that this would only have a relatively minor effect on total CO2 uptake by the ocean. However, comparing our results here with those obtained by the NOCS (then SOC) component of the OCMIP-2 project, we find that approximately 14.5

We have amended the main text to note this feature of our simulation, and have added material describing its magnitude to Appendix B (where the OCCAM simulation's performance is assessed).

section 2.2: See major comment above. I think the paper would benefit if the authors described in more detail (i) the relationship between TrOCA and previous quasiconservative tracers, and (ii) the reasons for why it is so difficult to parameterize preformed TrOCA, i.e. because one needs to capture the net effect of a complex set of processes that control the air-sea exchange of oxygen and CO2.

On the first point, we have expanded the introduction section to better put the TrOCA

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method in context with other back-calculation and deconvolution methods. On the second point, we have made use of the material provided by the referee in an expanded section on the TrOCA method.

section 3.1: See major comment above: I don't quite see the need for this section here. It tends to distract from the main topic, which is the evaluation of the TrOCA method. I suggest to move this to an appendix.

We agree that the performance of OCCAM is, to a degree, secondary in this manuscript, and in accordance with referee #1's suggestion, we have moved this material to an appendix and modified the main text appropriately. However, referee #2 has suggested changes to this section including expansion to compare TrOCA-relevant biogeochemical fields to observations. Following these latter remarks, we have deleted Figures 1 and 2 (the content of the latter is repeated elsewhere) and replaced them with more relevant model fields. We have also updated Figure 3 (now Figure 14) to include the most recent information from Takahashi et al.

section 3.1, page 7243-44, line CFC versus Cant inventory: Most models that overestimate the CFC inventories also overestimate the inventory of anthropogenic CO2 (see e.g. Matsumoto et al., 2004). This finding of a 6% lower Cant inventory and a 49% larger CFC inventory is thus surprising. Is this a consequence of the authors starting their model only in 1864? Please explain.

As noted above, our decision to initialise our simulation in 1864 rather than 1765 (that preferred by OCMIP-2) has significantly decreased our simulation's total uptake of anthropogenic CO2. This would not, of course, affect CFC-11, for which the full period of oceanic invasion was simulated.

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Assuming that our current simulation is comparable in relative terms to the SOC OCMIP-2 simulation, the table below shows a "corrected" estimate of anthropogenic CO2 alongside observational and other model estimates.

	"1990s" CFC-11 Gmol	"1990s" aCO2 Pmol
GLODAP	0.540	8.66
SOC OCMIP-2	0.565 (+4.6%)	11.16 (+35%)
OCCAM	0.790 (+46%)	8.11 (-6.4%)
OCCAM ("corrected")	-	9.29 (+7.3%)

As the table shows, correcting for this deficiency in our experiment design leads to OCCAM overestimating anthropogenic CO2 uptake rather than underestimating it (i.e. qualitatively in line with CFC-11 overestimation). However, OCCAM's large overestimate of CFC-11 inventory remains, although we also note that a better CFC-11 fit for the SOC OCMIP-2 model is balanced against a much worse fit for anthropogenic CO2.

Another part of the explanation for OCCAM's overestimation of CFC-11 uptake relative to anthropogenic CO2 lies in its simulation of deep water formation. As shown in new supplementary figures showing OCCAM's performance in terms of the zonal distributions of relevant biogeochemical properties, high latitude regions are excessively ventilated. These regions also coincide with those of greatest CFC-11 solubility, with the result that deep high latitude waters down to around 1000 m contain more CFC-11 than found in the GLODAP climatology (this can also be seen in the corresponding plots for oxygen). While anthropogenic CO2 is similarly enhanced in these regions, since it reaches its highest concentrations in tropical waters, the effect

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of this deficiency in OCCAM is less pronounced in terms of total tracer inventory.

Appendix B includes a summary of these explanations.

section 3.1, page 7244, CO2 fluxes: The authors may want to use the newest climatology of Takahashi et al. (2009). It happens to compare well with an independent set of flux estimates, based on an inversion of ocean interior observations (Gruber et al., 2009).

We have re-plotted this Figure to make use of the latest dataset prepared by Takahashi and colleagues (October 2009). We have amended the text slightly to account for the change in model-observation agreement.

section 3.2, pages 7244-7247: Given the unavoidable inconsistencies between the model and the observations, it is imperative, in my opinion, to refit the parameters. Therefore, I suggest to delete section 3.2 entirely. It is inappropriate to use the original parameterization.

We would agree with the referee that our application of default TrOCA to our model output should not be expected to yield positive results. However, as indicated by the comments of referee #2, the application of TrOCA at the global scale using observational datasets is novel and likely to be of interest to the community using TrOCA. To this end, we have retained the global scale inter-comparison but have deleted the local scale analysis of WOCE cruice I01. For completeness, we have retained the use of the OCCAM global comparison here and have amended the text to reference the inconsistencies noted by the referee.

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section 3.3, page 7249, "using standard equations": The same equation as used in the model need to be employed here. Otherwise, this would cause inconsistencies.

Close examination of our "standard equations" has uncovered some discrepancies between the equations used in OCCAM and those used for the manuscript's analysis, which cause small differences in our estimates of saturation CO2. We have repeated our entire analysis using consistent equations drawn directly from OCCAM's code. As already implied by existing results in which actual, simulated anthropogenic CO2 replaced that estimated, our results are not significantly affected by this change.

section 3.3, page 7249, "optimizing a": In the model, the parameter a is a-priori specified by the stoichiometric relationship employed in the biogeochemical equations, i.e. the value of a is known. Therefore, it would be good to know what would happen if the optimization was done with a specified according to the model's equations. By doing so, the authors can investigate in more detail the errors that come from the parameterization of preformed TrOCA. When a is permitted to float, errors in the retrieval in a project onto errors in the parameters of fit of preformed TrOCA.

We are grateful to the referee for this suggestion. Since parameter a is treated as a largely free parameter in Touratier et al. (2007), we followed this approach in our manuscript. In passing, Touratier et al. (2007) optimise the value of parameter a in a slightly different manner from that which we use. Using a fixed value of parameter a, they optimise the values of the remaining three parameters, but then repeat this process with different values of parameter a to find a global minimum fit.

However, from the original derivation of TrOCA (and more so than the other three parameters), parameter a should have a strong stoichiometric relationship with ocean biogeochemistry, and can be calculated from equations 1 and 5 as 1.205.

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Because of differences in our oxygen stoichiometry (151 mol O2 per 106 mol C, instead of 138 mol O2), in our biogeochemistry, parameter a should take the value 1.319.

To address the referee's point, we have repeated our TrOCA optimisation but held the value of parameter a constant at 1.319. We have amended the text to include the results from this further experiment in a separate section.

section 3.4: Why is this in a separate section? Shouldn't this be done as part of section 3.3 already?

The referee is correct that section 3.4 could logically sit within an earlier section, since it is a standard approach in parameter optimisation. We separated it out here primarily for clarity, and believe that this is still appropriate. Section 3.3 is already sufficiently dense and self-contained that merging section 3.4 into it, while logical, would overcomplicate the resulting super-section. To this end, we have retained the current ordering (note that section 3.4 is now section 4.3).

section 3.5: This is confusing. It would be much more straightforward to directly compare the models pre-industrial TrOCA field with the re-constructed one. Trying to reconstruct the DIC field is putting the carriage before the horse.

This is a fair criticism. We originally selected pre-industrial DIC since it is a field that will be intuitively familiar to readers. We have amended the manuscript to include this alternative comparison, and have changed the text appropriately.

Figures: Many of the multi-panel figures are too small.

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The quality of the figures in the manuscript PDF is noticeably lower than that of our original figures. This may make the small figures more difficult to interpret. We anticipate that they will be clearer in the final version of the manuscript (which will also be formatted more favourably), though we can split multi-panel figures across multiple pages should this prove necessary.

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