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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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1 Summary

Dr. Yool and co-authors use model-based synthetic-data in order to assess the accuracy of the TrOCA method to determine the amount and distribution of anthropogenic CO_2 in the ocean. The authors find the potential for substantial biases in the method, even when they refitted the parameters using model results.

2 EVALUATION

The accurate quantification of the oceanic uptake of anthropogenic CO_2 is one of the primary goals of ocean carbon cycle research. While much progress has been made recently, the fundamental challenge remains: The anthropogenic CO_2 that the ocean has taken up from the atmosphere since the beginning of the anthropocene cannot be measured directly, it must be inferred from the observations. A variety of methods have been developed in the last two decades to address this challenge, but only the ΔC^* method has been thoroughly evaluated with synthetic data so far(Matsumoto and Gruber, 2005), i.e. data from an ocean biogeochemical model for which the "true" anthropogenic CO_2 distribution is known. This represents a critical gap, since this is the only way the potential biases inherent in all methods can be quantified.

Yool and coauthors now extend this list by re-assessing the TrOCA method of Touratier and Goyet (2004) as implemented by Touratier et al. (2007) using synthetic data from their global ocean biogeochemistry model. This is an urgently needed study, and one that should be undertaken for each method that has been proposed so far. This study is particularly timely, since several intercomparisons of the results of different methods have been published recently, leaving the community in an unsatisfactory state of uncertainty. This is because such intercomparisons lack a calibration point and are therefore fundamentally unable to conclude which methods perform better than others.

By filling this gap, and thereby addressing an issue of great interest beyond the oceanographic community, Yool et al.'s manuscript fits very well into the scope of Biogeosciences and will attract significant readership.

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

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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.

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 CO_2 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.

3 Recommendation

I support publication of this manuscript with minor changes. I strongly recommend that the authors streamline the paper by focusing on the assessment of the TrOCA method using the best comparison method possible.

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4 Comments on the TrOCA method

Before making detailed statements, I would like to point out a key reason why the TrOCA method is problematic for determining anthropogenic CO_2 in the ocean.

I make two statements: First, and this may sound counter-intuitive, I will show that the TrOCA tracer is not a new tracer, but just a rescaled version of the C* tracer that was introduced by Gruber et al. (1996) (their equation (13)). Therefore, by giving it a new name, Touratier and Goyet (2004) have tended to obscure the fact that their method is a back-calculation method as well. Second, I will show from a process perspective that the chosen parameterization for preformed TrOCA is problematic.

i) TrOCA is C*

First, it is of note that the "new" tracer TrOCA is actually a scaled version of C* as defined by Gruber et al. (1996) (their equation 11), i.e.

(1) $C^* = TCO_2 - r_{C:O2}\ O_2 - 0.5 \cdot (Alk + r_{N:O2}\ O_2)$

re-arrange to group the O_2 terms

(2) $C^* = TCO_2 - (r_{C:O2} + 0.5\ r_{N:O2}) \cdot O_2 - 0.5 \cdot TAlk$

divide by $(r_{C:O2} + 0.5\ r_{N:O2})$

(3) $C^*/(r_{C:O2} + 0.5\ r_{N:O2}) = - O_2 + (TCO_2 - 0.5 \cdot TAlk)/(r_{C:O2} + 0.5\ r_{N:O2})$

Touratier and Goyet defined their stoichiometric ratios, e.g. $R_{C:O2}$, as being positive, (i.e. 123:165) whereas I defined them as negative (117:-170), so $R_{C:O2} = - r_{C:O2}$ and $R_{N:O2} = - r_{N:O2}$. Inserting these (positive) stoichiometric ratios gives:

(4) $C^*/(R_{C:O2} + 0.5\ R_{N:O2}) = O_2 + (TCO_2 - 0.5 \cdot TAlk)/(R_{C:O2} + 0.5\ R_{N:O2})$

thus:

TrOCA = $C^*/(R_{C:O2} + 0.5\ R_{N:O2})$, or using Touratier and Goyet's definition of

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a = 1/($R_{C:O2}$ + 0.5 $R_{N:O2}$)

(5) TrOCA = a · C*

Thus, there is nothing new about this tracer. This becomes particularly evident when C^{ant}_{TrOCA} is computed (eq 7 in manuscript by Yool et al.), as TrOCA is divided again by a.

(6) C^{ant}_{TrOCA} = (TrOCA - $TrOCA^0$)/a = C* - C^*_{pi}

which directly corresponds to equation (13) in Gruber et al. (1996). Therefore, the TrOCA approach is a back-calculation technique, i.e. it corrects first the observed TCO_2 for the influences of biology and then it subtracts a pre-industrial preformed value.

(ii) The parameterization for $TrOCA^0$ is not justifiable

The real problem with the TrOCA method is the way the pre-industrial pre-formed TrOCA (or actually C^*_{pi}) is estimated. This is not a detail, but the crucial difference among all methods to estimate anthropogenic CO_2 on the basis of TCO_2 observations and some form of a "back-calculation" method. Touratier and Goyet essentially follow the strategy of the Chen method and use data from the deep ocean and thought to be free of anthropogenic CO_2 to determine a parameterization for the relationship between C^*_{pi} and temperature (with some correction by TAlk). In their latest parameterization, they additionally use near surface estimates of anthropogenic CO_2, inferred from CFCs. They then interpolate to waters from all depths, using an a priori functional form based primarily on temperature and to a limited degree on alkalinity. There are process-based arguments that strongly speak against such a relationship.

Let's consider first what controls the distribution of the TrOCA or C* in the pre-industrial ocean. Without going into the details, C^*_{pi} can actually be deconvolved linearly into a C*(P) part and into an O_2* part (see Gruber et al., 2001; Gruber et al., 2002 for definitions). Variations in C*(P) primarily reflect the air-sea exchange of CO_2 (to better reflect this, I later referred to C*(P) as ΔC_{gasex}), while variations in O_2* primarily reflect

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the air-sea exchange of O_2. Therefore, variations in C* (and TrOCA) reflect the combined effect of the air-sea exchange of CO_2 and O_2, i.e. any uptake of CO_2 and/or O_2 from the atmosphere increases C^*_{pi}, while any outgassing of CO_2 and/or O_2 leads to a decrease in the concentration of C^*_{pi}.

This means that Touratier and Goyet's interpolation of C^*_{pi} between cold deep waters and warm surface waters is equivalent to assuming that the air-sea exchange signal of both CO_2 and O_2 is the same across all temperatures (since the contribution of the alkalinity term is relatively small). Or expressed yet in another words, their assumption requires that the air-sea exchange flux of both CO_2 and O_2 scales with the air-sea flux of heat in a way across all temperatures and regions that is prescribed by their mathematical function. Is this justifiable?

Fortunately for Touratier and Goyet, the gas exchange signal of O_2* has a strong (exponential) relationship with temperature (see e.g. Figure 1 in Gruber et al., 2001). But there is much additional structure in the O_2* to temperature relationship reflecting the role of biology in modifying air-sea fluxes of O_2 as well as the fact that gases do not exchange as quickly across the air-sea interface as heat does. The relationship of C*(P) with temperature is much more complex, primarily because the effect of biology and that of heat fluxes on the air-sea exchange flux of CO_2 tend to act in opposition. Therefore, for both CO_2 and O_2, biology tends to decouple the actual air-sea fluxes from those related to heat fluxes, and this decoupling is different in different regions of the oceanographic. As preformed TrOCA (C^*_{pi}) is parameterized from the relationship of the air-sea fluxes of CO_2 and O_2 with those of heat in a few places only, it is highly unlikely that this relationship can hold across all regions. Thus it is not surprising that Yool et al. find substantial biases in the TrOCA reconstructed anthropogenic CO_2 distribution. In essence, these biases are all driven by errors in estimating pre-industrial preformed TrOCA or C^*_{pi}.

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5 Detailed comments

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.

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?

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 CO_2 perturbation was relatively small, but persistent. This anthropogenic CO_2 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.

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 quasi-conservative tracers, and (ii) the reasons for why it is so difficult to parameterize pre-formed 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 CO_2.

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.

section 3.1, page 7243-44, line CFC versus Cant inventory: Most models that overestimate the CFC inventories also overestimate the inventory of anthropogenic CO_2 (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.

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section 3.1, page 7244, CO_2 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).

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.

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.

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.

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

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.

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

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Nicolas Gruber, September 1, 2009.

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