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Interactive comment on "Model constraints on the anthropogenic carbon budget of the Arctic Ocean" *by* Jens Terhaar et al.

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Response to Referee #2

We thank both referees for their comments and suggestions. The manuscript will be revised to thoroughly address each point. Generally the plan is as follows:

(1) The description of the simulations analyzed in our study will be improved by adding more details concerning (a) how the different simulations (ORCA2, ORCA05, and ORCA025) were made, making it clear that each of these has its own control simulation, (b) how CFC-12 was simulated, and (c) how carbon transport was estimated at

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the boundaries of the Arctic Ocean.

(2) The discussion section will be expanded to provide more detail about (a) the effect of increasing resolution in ocean models found by other modeling studies and (b) the mechanisms influencing changes in air-sea CO_2 fluxes in the Arctic between the different resolutions, including a discussion of the role of sea-ice. Additional analysis will also be included to show how the different configurations compare to each other in terms of the global-ocean inventory of anthropogenic carbon.

During the review period, we also discovered an issue with our CFC-12 simulations (initialization to non-zero concentrations). Hence we have rerun all CFC-12 simulations (as will be detailed in the revised manuscript). Furthermore, we have used the opportunity to complement the ORCA05 C_{ant} perturbation simulations with analogous simulations for the ORCA2 and ORCA025 configurations (each initialized in the beginning of 1958 with output from the last time step in 1957 of the ORCA05 C_{ant} perturbation simulation and run until 2012). With these updated simulations, the model-data CFC-12 comparison has been improved (to be discussed in Sections 3.2 and 4.1) as have been the corrections for the estimated C_{ant} fluxes at the boundaries (both lateral and at the air-sea interface). The figures and tables of the revised manuscript will be updated accordingly. Despite these improvements, the Conclusions of our study remain the same.

In the following we address their concerns point by point.

Referee # 2

In this study the authors examine the anthropogenic CO_2 budget of the Arctic Ocean and how this inventory depends on model resolution. In that purpose they take advantage of the NEMO-v3.2 OGCM coupled to the biogeochemical model PISCES-v1. They perform experiments with three different horizontal resolution of the OGCM, namely 2°, 0.5°, and 0.25°. Inventories of anthropogenic carbon in the Arctic appear to increase with increasing resolution (from 2.0 to 2.6 Pg C). The role of air-sea fluxes and lateral transport in building these inventories is examined. In this model lateral transport accounts for 75% of the Arctic Ocean anthropogenic CO_2 inventory. A comparative study of the outputs of other modeling studies (CMIP5) allow concluding that models with larger lateral transport appear to better fit data-based estimates of the anthropogenic carbon in the Arctic Ocean. This partitioning does not depend on the model resolution. Resolution appears important in shaping the tracer distribution and improving data-model agreement.

The paper is well written and very well structured. However I have several concerns about the method and the way data-model comparison is performed. Before the method is thoroughly assessed this paper is not fit for publication.

Major comments

Reviewer Question 1 — I have serious concerns about the applied method for estimating the C_{ant}. Conclusions about the impact of model resolution might not be robust due to shortcoming in the method.

C_{ant} is rightly defined as the difference between the simulated historical and control ocean dissolved carbon contents. However, there is only one control experiment performed (page 5), that for ORCA05. As far as I understand the C_{ant} for ORCA2

and ORCA025 is evaluated as the difference between the respective 1958→2012 experiments and the ORCA05 control for the same period. Therefore I strongly suspect that the differences in CFC and C_{ant} among the different models may be explained by model drift. In order to lift that concern the following actions should be taken:

a. Perform control experiments over the period 1958→2012 for each model resolution.

b .While it is defensible to reduce the computation length with the high-resolution model (ORCA025) there is no such need for ORCA2, which runs even faster than ORCA05. The authors should also present results of historical and control experiments performed with ORCA2. The perturbation experiments should also be repeated with ORCA2.

The results of these additional experiments should then be compared to those presented in the present paper. This would provide a means of validating their method and assessing potential drifts

Reply: a. We realize now that our original manuscript is woefully unclear about this point. We did not properly convey what was done. Indeed, each of our three resolutions already has its own control experiment over 1958-2012. For each resolution, C_{ant} was computed from two simulations (historical and control), both made at the same resolution. Therefore, there is no resolution-related drift issue. This point will be clarified in section 2.2 of the revised manuscript.

b. As pointed out, we had already made a control simulation in ORCA2-PISCES from 1958 to 2012 although that was not clear in the submitted manuscript. For the same period, we have in addition added a C_{ant} perturbation simulation in ORCA2. Our strategy to consistently use the same ORCA05 output from the end of 1957 to initialize

ORCA2 as well as ORCA025 in 1958 was by design. It produces a consistent set of results whereby the effect of resolution can be compared rigorously. Had we started all ORCA-PISCES simulations in 1870, which was not computationally feasible, there would have been larger differences due to resolution, e.g., based on the divergence shown in Figure 1. Hence the differences due to resolution discussed in the submitted manuscript are probably a lower limit, something that will be clarified in the revised manuscript. In addition, for the revised manuscript, we are considering making an additional C_{ant} perturbation simulation in ORCA2 initialized in 1765 (and 1870), if allowed by our limited computational resources available at the end of this fiscal year. That new simulation would provide a more complete comparison with the analogous reference ORCA05 simulation as suggested by Referee #2.

Reviewer Question 2 — The experiments, which are presented here are global. What would be the global figures for anthropogenic CO_2 uptake in the 5 cases? How do these figures compare to other assessments? Answering this request would allow evaluating whether the OGCMs as a whole would need serious refinements or should the effort concentrate on less-well resolved areas such as the Arctic Ocean.

Reply: The global corrected C_{ant} inventories for 1765-2009 for the three resolutions are given below as are the uncorrected inventories for 1870-2009 (in parentheses):

- ORCA2: 155 (130) Pg C
- ORCA05: 149 (124) Pg C
- ORCA025: 151 (127) Pg C

These corrections were made by adding the difference between the two tracers in the C_{ant} perturbation simulations, one initialized to zero in 1765 and the other in 1870, in each of the three resolutions. Those global perturbation results are as follows:

- ORCA2: 130 Pg C/155 Pg C
- ORCA05: 127 Pg C/152 Pg C
- ORCA025: 120 Pg C/144 Pg C

In the revised manuscript then, we will present the C_{ant} inventory for 9 cases rather than 5 because of the new perturbation simulations in ORCA2 and ORCA025 (4 additional cases since each has 2 perturbation tracers: one initialized in 1765 and the other in 1870) that were not provided in the previously submitted version of the manuscript.

Regarding other assessments, Khatiwala et al. (2009) report a data-based estimate for the global C_{ant} inventory for the period from 1765-2009 to be 140 ± 24 Pg C. For that same period, our results all lie within that range, falling near the upper boundary. Given the agreement of model results and data-based estimates for the global ocean C_{ant} budget, it does not appear that further enhancements to resolution are needed to improve the global carbon budget. However, we would expect that improving resolution will have a large impact on some regional budgets, e.g., in zones where the ratio between the areas shelf seas vs. open ocean is relatively large, such as in the Arctic Ocean. Inventories of anthropogenic carbon in regions with small areal extent will have little impact on the global inventory, but they do provide some indicator of the potential enhanced effect of ocean acidification in those regions. These results and concerns will be brought up in section 4.2 of the revised manuscript.

Reviewer Question 3 — The other main concern deals with the correction of data-based reconstructions of C_{ant} (Abstract, Sections 4.2 and 4.5, Fig. 9). The authors assume that reconstructed deep values of C_{ant} should be corrected downwards since observed CFC-12 concentrations at those depths are negligible. Doing so means overlooking the important fact that CFCs started to be emitted in the atmosphere much later

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than CO_2 .. Data-based estimates relying on the TTD method take into account the different tracer histories in the atmosphere. Clearly, the TTD method has limitations. The end-product displays rather large uncertainties. However, there are no sound arguments for setting the C_{ant} in the deep Arctic to zero.

Reply: Actually, there is some evidence that the GLODAPv2 estimate using the TTD method may overestimate C_{ant} concentrations in the deep Arctic Ocean. First, the water mass mean ages of deep and bottom waters are estimated to be about 250 to 300 years in the Eurasian basins (Nansen and Amundsen basins) and around 450 years in the Canadian basin (Makarov and Canada basins) (Tanhua et al., [2009]; Schlosser et al., [1994]). Thus one would expect very little if any C_{ant} would have reached those old deep waters. Second, the TTD method is known to estimate C_{ant} concentrations around 5 $\mu molkg^{-1}$ even when the CFC-12 concentrations approach zero (Waugh et al. [2006]), which demonstrates the lack of sensitivity and large uncertainty associated with the TTD estimates for older water masses. Given that data-based C_{ant} concentrations in the old, deep Arctic Ocean water masses are 18% of surface concentrations while CFC-12 concentrations in the same deep water masses are only 3% of surface concentrations, it is plausible that the TTD method overestimates C_{ant}.

In the revised manuscript, we will rephrase the text so as to indicate that to calculate the maximum potential error in the TTD-based estimate for the C_{ant} inventory in the Arctic, we also set the deep TTD estimates to zero.

Reviewer Question 4 — Modeled CFC-12 inventories in the Arctic (Fig. 5 and page 12, lines 24 and 25) appear to be much lower than the observed ones, even with ORCA25. Would it be possible to provide total (integrated over depth and distance) inventories along the AOS94 and Beringia 2005 expedition pathways and compare the 3 model results to the data inventories? The low CFC inventory provides an indication that low C_{ant} would be expected too.

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Reply: Excellent suggestion. Thank you. We have now calculated these total inventories integrated over depth and distance along sections:

- Beringia 2005
  - Observations: 9.4 μmolm^{-1}
  - ORCA025: 7.7 μmolm^{-1}
  - ORCA05: 5.8 μmolm^{-1}
  - ORCA2: 3.7 μmolm^{-1}
- AOS94
  - Observations: 5.5 μmolm^{-1}
  - ORCA025 : 4.8 μmolm^{-1}
  - ORCA05 : 3.5 μmolm^{-1}
  - ORCA2 : 2.0 μmolm^{-1}

For both expeditions, the observed CFC-12 section inventories are underestimated by 13-18% in ORCA05, 31-38% in ORCA05, and 61-64% in ORCA2. This tendency with resolution for these section inventories is consistent with that seen for the Arctic Ocean's basin-wide inventory where the data-based estimate is underestimated by 13-15% in ORCA025, 22-24% in ORCA05, and 34-36% in ORCA2.

A table of these integrated values and corresponding text will be included in Section 3.2 of the revised manuscript.

Reviewer Question 5 — In addition, a description of how CFC-12 is modeled is lacking.

Reply: Details about how CFC-12 was simulated will be included in the Methods section of the revised manuscript.

Minor comments

Reviewer Question 6 — Abstract, line 10: C_{ant} is not defined yet.

Reply: C_{ant} is defined in line 5.

Reviewer Question 7 — Page 3, line 2: a reference to the figure displaying the map of the Arctic should be made here; the reader does not necessarily know about the area characteristics. In this sense Fig. 2 should become Fig. 1.

Reply: The revised manuscript will include these suggested changes.

Reviewer Question 8 — Page 3, line 2: "The bathymetry of the Arctic Ocean differs from that of the in other other oceans..."

Reply: In the revised manuscript we will change "of the in other other" to "in other".

Reviewer Question 9 — Page 3, line 25: is 'laminar' right?

Reply: Yes, "laminar" is a common term used to describe coarse-resolution ocean models. See for example Penduff et al (2011).

Reviewer Question 10 — Page 4, line 2: table 3 does not come into order.

Reply: In the revised manuscript, that table will be put in order.

Reviewer Question 11 — Page 4, lines 11 and 12: "NEMO uses partial steps so that the model better matches the observed topography. Thus the depth of the deepest cell can be smaller than the original grid cell." Could you develop or reformulate? It is hard to understand what it is meant here.

Reply: We propose to rephrase the sentence as follows: "NEMO uses the partial-step approach for the model to better match the observed topography. In this approach, the bathymetry of the model is not tied directly to the bottom edge of the deepest ocean grid level, which varies with latitude and longitude; rather, the deepest ocean grid level for each column of grid cells is partially filled in to better match the observed ocean bathymetry."

Reviewer Question 12 — Page 4, line 22: there is no mention of the Si:P and Fe:P ratios.

Reply: The Fe:C and Chl:C ratios of both phytoplankton groups as well as the Si:C ratio of diatoms are predicted prognostically by PISCES. These model details will be mentioned in the revised manuscript.

Reviewer Question 13 — Page 4, line 29: does sediment mobilization only intervene in the Fe cycle? Or does it also affect the other nutrients?

Reply: Yes, sediment mobilization only intervenes in the Fe cycle, a point that will be clarified in the revised manuscript.

Reviewer Question 14 — Page 4, line 30: "... following the lead of Moore et al. (2004)."

Reply: That phrase will be changed to “following Moore et al. (2004).”.

Reviewer Question 15 — Page 5, line 33: “... simulations made in with the same circulation model...”

Reply: In that sentence, ”in with” will be changed to “with”.

Reviewer Question 16 — Pages 5 and 6: the many occurrences of ‘xCO_2 ’ should be changed into ‘CO_2 ’.

Reply: To avoid any confusion, we prefer to explicitly refer to ‘xCO_2 ’, namely the atmospheric mixing ratio or mole fraction of CO_2 , i.e., the number of moles of CO_2 per mole of air. That ratio is typically multiplied by 106 and given in ppmv (or simply ppm) because CO_2 is a trace gas in the atmosphere. In the text, we need to distinguish between xCO_2 and the partial pressure of CO_2 (pCO_2), which always has pressure units (μatm). Although these two quantities are often confused, they are not the same and our method depends on keeping them straight.

Reviewer Question 17 — Page 6, equation (1): what are the units of pCO_2 and T?

Reply: The units of for pCO_2 [μatm] and T [°C] will be indicated in the revised manuscript

Reviewer Question 18 — Page 6, line 25: “given that it is based on results from ORCA05.”

Reply: This above-mentioned phrase was part of a paragraph that will be removed in the revised version of manuscript. That paragraph explains how the biogeochemical

simulations (all three resolutions) were corrected using the ORCA05 C_{ant} perturbation runs. Now with our complete set of C_{ant} perturbation simulations, two C_{ant} perturbation tracers in each of the three resolutions as detailed earlier, this explanation is not necessary.

Reviewer Question 19 — Page 6, line 28: reference to Table 4 should appear here.

Reply: This line is also part of the paragraph that will be removed in the revised manuscript.

Reviewer Question 20 — Page 10, line 24: "apparent in ORCA025 6)."

Reply: The "6)" will be changed to "(Figure 6)".

Reviewer Question 21 — Page 12, line 22: "that that excess simulated CFC-12 between 1000 and 2000 m..."

Reply: The double that will be changed to that.

Reviewer Question 22 — Tables do not come into order. Table 3 should become Table 1, Table 1→2, and Table 2→3.

Reply: Tables will be ordered correctly in the revised manuscript.

Reviewer Question 23 — Table 1: the 'b' subscript does not appear anywhere in the table

Reply: The 'b' subscript will be added to Table 1 in the revised manuscript.

Reviewer Question 24 — Table 2, caption: "Fitted parameters for the perturbation approach for the tracers starting in 1765 (P1765) and in 1870 (P1870)."

Reply: In the revised manuscript, this sentence will be changed to "Fitted parameters for the perturbation simulations P1765 and P1870."

Reviewer Question 25 — Table 4: what do exactly represent the lines "Total transport" and "Summed lateral flux"?

Reply: Both will be changed to 'Sum' in the revised manuscript. Both terms represent the sum of the lateral fluxes: in one case it is the lateral water flux and in the other the lateral C_{ant} flux.

Reviewer Question 26 — Table 4, caption: "Simulated values are calculated for the same time period as observations."

Reply: This text will be revised as proposed by the Referee.

Reviewer Question 27 — Fig. 1 and Fig. 2 should be inverted

Reply: These figures will be inverted in the revised manuscript.

Reviewer Question 28 — Fig. 10, caption: The first sentence "Profiles of Ω_A after the early industrial period period simulated only in ORCA05 (1870–1957), after intializing

the other models in 1958." is confusing. I suggest to remove most of it; it is not needed.

Reviewer Question 29 — "Results are shown for ORCA05 in 1960 (black solid) as well as for ORCA2 (green dot- dash), ORCA05 (red dashes), and ORCA025 (blue dots) in 2012."

Reply: In the revised manuscript, both sentences will be simplified and combined: "Profiles of Ω_{arag} for ORCA05 in 1960 (black solid) as well as ORCA2 (green dot-dash), ORCA05 (red dashes), and ORCA025 (blue dots) in 2012."

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