

## Comments on “Model constraints on the anthropogenic carbon budget of the Arctic Ocean”

by Terhaar et al., revised version, submitted to Biogeosciences, doi: 10.5194/bg-2018-283

First of all I appreciate that the authors carefully considered most of previous comments. As I already mentioned the paper is clear and provides interesting results.

However, some concerns remain in addition to some errors in the revised version. Were these concerns lifted, the paper would then be fit for publication.

1. In my first review I had questioned the adequacy of the initialization with ORCA05 results in 1958 and suggested that this method be compared for ORCA2 with an experiment identical to that performed with ORCA05 over the entire period. I expected results of this sensitivity experiment to be presented in Figures 2, 7, and 9 and discussed in the text. However, the authors only performed a simplified perturbation simulation with ORCA2. This does not allow assessing the method consisting in the initialization of ORCA2 and ORCA025 with ORCA05 results in 1958. In addition, except for the mention in Section 4.2, there is no discussion of that additional experiment.
2. The discussion of model data comparison (Section 4.2) is mostly speculative and relies on a misunderstanding of the TTD method.

The fact that models predict lower values than data-based  $C_{\text{ant}}$  reconstructions is no proof that reconstructions overestimate  $C_{\text{ant}}$  as sentence on lines 2-3 page 14 suggests. The different model versions clearly underestimate CFC-12 invasion (Sections 3.2 and 4.1). In consequences one would also expect  $C_{\text{ant}}$  to be underestimated. In that respect why do the authors insist on lowering data-based estimates?

While the TTD method as applied by Tanhua et al. (2009) or in GLODAP-v2 is less well constrained for large transit times (since this method relies on tracers with a short atmospheric history) it does not follow that  $C_{\text{ant}}$  should be set to zero whenever CFC concentrations are very low. Any water parcel in the ocean is characterized by a distribution of transit times (TTD) which differs from a delta-function due to the presence of mixing (e.g., Waugh et al., 2006). The mean of that distribution corresponds to the mean water age and its width depends on mixing strength and pathways. The assumption that the TTD width is equal to its mean seems to be adequate enough for most ocean areas (Waugh et al., 2006; Tanhua et al., 2009). Hence the water body under consideration is characterized by ages ranging from zero to the mean age and beyond. Taking that into account, and acknowledging that CFCs and CO<sub>2</sub> do not have the same atmospheric history (CFC-12 concentrations in the atmosphere started to rise significantly after 1950 while the anthropogenic carbon perturbation started 2 centuries earlier) a mean age of 300 to 400 years does not preclude any  $C_{\text{ant}}$  contribution at depth.

Additionally there is no rationale for assuming that “there is a symmetry during 1765-2005 about the ORCA05 result with ORCA2 being lower and ORCA025 being higher” even if it happens to be the case after 1958. There are many processes at stake (air-sea exchange, lateral transport, mixing, atmospheric increase...). Therefore the response of the system is

expected to be non-linear. In consequences there is no justification for assuming that the  $C_{\text{ant}}$  inventory with ORCA025 would be larger by 0.4 PgC had the experiment started in 1765.

Further, the ORCA2 experiment which result in a lower inventory is not a biogeochemical experiment but a perturbation one which relies on a simplified carbon cycle. Inventories should not be corrected on that basis. Results of a complete biogeochemical experiment with ORCA2 starting in 1765 would be needed for such assessment. I am rather surprised that no such experiment seems to be available?

Rather than aiming at reconciling modeled  $C_{\text{ant}}$  and data-based reconstruction this section should be devoted to discussing  $C_{\text{ant}}$  in view of the CFC-12 results.

3. Conclusions need to be revised along the preceding lines (Page 17 lines 29-32 and page 18, line 1-4). The conclusions should also mention that all model versions underestimate CFC inventories in the Arctic, hence underestimated  $C_{\text{ant}}$  inventories.
4. Model vertical resolution and mixing schemes.
  - a. In the model description on page 4 the authors state  
“Vertically, all three model configurations have the same discretization, where the full-depth water column is divided into 46 depths levels, whose thicknesses increase from 6 m at the surface to 500 m in the deepest grid box”  
  
If I am well informed the 500 m box thickness for deep boxes is typical of ORCA2 with 31 levels while a thickness of 6 m at the surface is typical of the 46 levels versions.  
  
Is the vertical grid spacing actually the same for all 3 model versions?
  - b. Additionally, one may wonder if the vertical diffusivity and viscosity are represented the same way in all 3 versions? Could the authors add information on that aspect?

## Miscellaneous

- The CFC model-data misfits quoted on page 11 (lines 13-15) do not agree with the values in Table 6. In the later ORCA2 displays the best agreement with data!
- Figure 4: the bottom right panel displays CFC-12 results and not temperature.
- Page 7, line 2: “...using ORCA05 until 1957 and then all three configurations from **1858** **1958** to 2012...”
- Page 11, line 13: Table 6 does not come into order; should be Table 5
- Page 11, line 29: “In 2002, the upper limit **of the** of the modeled  $C_{\text{ant}}$  inventory range...”
- Page 11, line 32: “**These This** correction is 0.4 Pg C in 2005 for each resolution...”
- Page 12, line 25: “... flow fields is 0.05 Pg C (~3%) **is** smaller than...”
- Page 14, line 15: “Although we cannot assess this **affect effect** directly,”