

***Interactive comment on* “The influence of the ocean circulation state on ocean carbon storage and CO₂ drawdown potential in an Earth system model” by Malin Ödalen et al.**

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We are thankful to the referee for providing useful insights about necessary clarifications in the manuscript. Below, we respond to the referee’s specific comments/questions, which are presented in paragraphs 2 to 4.

Paragraph 2: The conclusion that the oceanic storage of DIC and the drawdown of atmospheric CO₂ in response to nutrient depletion all depend on the ocean circulation patterns (including the overturning strengths of NADW and AABW, and the volume fraction of the ocean last ventilated from the North Atlantic vs. Southern Ocean) is not new. The circulation effects on the ocean carbon pumps have been extensively stud-

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ied using models and theoretical frameworks: the solubility pump (e.g., DeVries and Primeau, Atmospheric pCO₂ sensitivity to the solubility pump: Role of the low-latitude ocean, GBC 2009), the biological pump (references already cited in the manuscript) and the disequilibrium pump (e.g. Ito and Follows, Air-sea disequilibrium of carbon dioxide enhances the biological carbon sequestration in the Southern Ocean, GBC 2013). One of the new points in this study is that the authors discussed the relative role of the three pumps in the net change of the simulated carbon cycle. But, because the forcing used to generate the different circulation patterns is arbitrary, their discussion of the relative roles does not seem very interesting. Overall, I feel that the authors need to highlight what new findings or insights this study can provide.

Response: In the study by Ito and Follows (GBC, 2013), mentioned by the referee, it was pointed out that more studies like these, where we analyse model output 'in terms of different carbon pump components' and their behaviour could aid in understanding the behaviour of climate models and carbon cycle models. The reviewer is correct that the circulation effect on the ocean carbon pump has been extensively investigated. There are two main advances here that are of importance to the community, and we will place greater emphasis on these in the revised manuscript:

1) The initial state, not only in terms of ocean circulation, but in terms of ocean carbon inventory and the origin of the already stored carbon (e.g. biological or solubility pump), is crucial for the outcome of a study investigating increased efficiency of the biological pump, and the effect of the initial state is quantifiable;

2) the relative contributions by C_{sat} , C_{soft} , C_{carb} , and C_{dis} to the ocean carbon inventory in the initial state depends on the model tuning strategy, yet these combine in a manner to give a response in total carbon that is more straightforwardly related to the circulation than any of the four components (Figs. 3-4).

Regarding 1) We show that differences in the initial states can explain discrepancies in e.g. a CO₂ drawdown scenario where the efficiency of the biological pump increases.

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As the referee points out, this will help to explain why previous model intercomparison studies have seen a large spread in response among models to the same perturbation. Therefore, it can also be important for future model intercomparison studies, in explanations of results, but also in planning for common tuning strategies and experimental design. This will be important in model studies of both glacial and modern climate scenarios. Here, the findings in described in 2) make it easier to understand the outcome of the drawdown experiment and therefore provide a useful basis for explaining the role of the initial state. The findings presented in e.g. Fig. 6 are new, and have not been covered by the studies mentioned by the referee. Of most value here is that we show that it is possible to quantify, from theory, the effect of any bias in the model's initial state on its sensitivity to changes in the biological pump. We have chosen a variety of changes to the forcing in order to demonstrate that the result is robust. This result should be of value in understanding the biases individual models, in inter-comparison studies, and potentially for choosing tuning criteria.

Regarding 2) Firstly, the changes to the forcing that we make are certainly idealised, but they are not arbitrary. We are changing common circulation parameters, mainly within the limits of common tuning ranges, to produce circulation states that are relevant in the context of the amplitude of (palaeo-)climate change (see response to Referee #1, point 3). (The change used to produce a 100%-efficient biological pump is, of course, an intentional exception.) As we explore both an increase and a decrease of the same parameter, the equilibrium states resulting from the increase and decrease, together with the control equilibrium state, provide a range of states that can be expected from tuning of this parameter. By testing several parameters in the same manner, we show that different tuning strategies are expected to affect different reservoirs of the initial state carbon inventory. This leads us to the important conclusion that, depending on the strategy used in model tuning, the relative sizes of the reservoirs of DIC species will differ between models. As the referee points out, this has not been done in previous studies. As a consequence, such differences have not been taken into account in e.g. model intercomparison studies. We hope that our study can bring some attention to

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this problem.

Secondly, when comparing the different changes in forcing, our main intent is for the reader to compare the relative sizes of change of the different pumps when one parameter is changed (e.g. when the wind stress is changed, the most important contribution to the change in carbon inventory comes from the soft tissue pump, whereas if horizontal (isopycnal) diffusivity is changed, the contribution from the solubility pump and disequilibrium carbon dominate). Hence, the magnitude is not what is most important, but rather which carbon pump(s) is(are) affected by which tuning strategy. By doing this, we emphasize that the tuning strategy is important for determining which processes contribute to a model's ocean carbon inventory, and therefore its behaviour. Even if this conclusion is not entirely new, all the previous studies focus on one or two processes at a time. We therefore believe that the structured approach of the ensemble study, and the fact that all DIC species are investigated in the same manuscript, can provide a comparability that can be useful for the community.

We do, however, acknowledge the reviewer's point that these insights need to be better highlighted, and will make appropriate changes to the Abstract, Introduction and Discussion sections.

Paragraph 3: Fig. 10 showing the CO₂ drawdown potential as a function of a change in the mean ocean temperature does not convey any messages. There seems no relationship between the two. Plus, if my reading is correct, water temperature does not control the strength/efficiency of the biological pump in the model. Therefore, there is no reason that the CO₂ drawdown potential should be correlated with ocean temperature. Why don't the authors use other metrics such as the initial preformed PO₄ as an X-axis instead, as was done in Marinov et al., 2008?

Response: The referee is correct in saying there is no direct relationship between mean ocean temperature and the efficiency of the biological pump. Any relationship between the two is indirect and due to the fact that the ocean circulation affects both variables.

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This is what we were trying to show with this figure. However, we agree with the referee that the figure is not crucially important for the story and it will therefore be removed. The figure described by the referee, which presents CO₂ drawdown potential and initial preformed PO₄, is present in the manuscript, but in a slightly different format. Panel c in Figure 6 shows CO₂ drawdown potential on the Y-axis and global average P* on the X-axis. $P^* = P_{\text{reg}} / P$ and $P_{\text{pre}} = P - P_{\text{reg}}$ (see Eqs. 2 and 8), so the quantities of P* and P_{pre} are closely related. We therefore think an additional figure showing CO₂ drawdown potential vs. P_{pre} would be redundant.

Paragraph 4: The way the biological pump is simulated in the model is unclear. The authors included the carbonate pump in their models and analyses (expressed as C_{carb}), but there is no description on how the carbonate pump is represented in the model. For example, are the sedimentation processes included in the model? How are the production and dissolution of calcifying organisms represented in the model? How is the strength/efficiency of the carbonate pump affected by the drawdown experiment? In the drawdown experiment (specified in lines #31-33 of page #9), the remineralization length scale of sinking organic particles is made very deep (10,000m), so that “any carbon that is taken up in organic material to be highly efficiently trapped in the deep ocean and not undergo any significant remineralization”. Does it mean that most of inorganic nutrient is converted to organic form and stored in the abyss without being remineralized back to inorganic form? If this is the case, then the amount of organic matter would increase substantially in the drawdown experiment, and the carbon fixed in organic material should be an important component in the mass balance equations and can't be ignored in the theoretical derivations presented in the manuscript. Likewise, the equation “ $P_{\text{pre}}=P-P_{\text{reg}}$ ” would be incorrect. This needs to be clarified.

Response: The way the biological pump is simulated in the model is thoroughly described by Ridgwell et al. (2007). Since the description is lengthy and the paper is already long, we decided not to include the full description in the manuscript, but instead describe the most relevant part describing remineralisation (section 3.1.2) and

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reference the paper for the in-depth model description. We agree with the referee that it should be clarified that the explicit information on the biogeochemical cycling can be found in this paper (see manuscript changes below).

We also agree that we need to make some clarifications regarding the drawdown experiment. The referee asks ‘Does it mean that most of inorganic nutrient is converted to organic form and stored in the abyss without being remineralized back to inorganic form?’ Yes, that is exactly the case, and the referee correctly states that this means the amount of organic matter in the deep ocean significantly increases. The referee is also correct in saying that the theoretical derivation that neglects the organic matter no longer holds. Therefore, we do not use this derivation for the drawdown experiment (we only use it in the first step, where we compare the initial equilibrium states). For the drawdown experiment, we do not explicitly calculate the change in strength of the soft tissue pump, the carbonate pump, or the changes in disequilibrium. The theoretical derivation could be made valid for this case by assuming that 106 times the amount of particulate P in the deep ocean should be added to C_{soft} . However, we have chosen not to explore the changes in strength of the different pumps in the drawdown experiment, since it is a highly hypothetical case. We are mainly interested in the differences in CO₂ drawdown between different ensemble members in the limit of a highly efficient biological pump. That the biological pump is made highly efficient is clear from the two orders of magnitude decrease in dissolved P in the ocean, which will also be clarified in the manuscript.

We will add a clarification in section 3.1, at the end of the second paragraph. This clarification reads “The model description of export flux of organic matter is based on available surface nutrients (see Ridgwell et al., 2007, Eq. 1-4), and instead of having a “standing plankton biomass” in the model, the export of particulate organic matter is derived directly from uptake of P. The carbonate precipitation rate is thermodynamically-based and relates export flux of CaCO₃ to the flux of POC (see Ridgwell et al., 2007, Eq. 8). As investigation of carbonate system feedbacks are not the purpose

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of this study, interactive sediments are not used, and in terms of carbon cycling, the atmosphere-ocean is studied as a closed system.”

We will also add to section 3.1.2, before the final sentence of the second paragraph, the sentence ‘The remineralisation of CaCO₃ in the water column is treated in a similar manner to particulate organic carbon.’

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