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Interactive comment on "The influence of the ocean circulation state on ocean carbon storage and CO_2 drawdown potential in an Earth system model" *by* Malin Ödalen et al.

Malin Ödalen et al.

malin.odalen@misu.su.se

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We thank the referee for an overall positive review, and for helpful comments that will allow us to improve the manuscript. In this comment we respond to the main review points listed by the referee. There is also a list of minor points made by the referee. Those will not be addressed here, but we are happy to include the requested corrections and clarifications in the updated manuscript.

Point 1: One major concern is its length and level of detail, which dilutes its most important points. On the one hand, I appreciate that the authors are attempting to describe a complex system clearly and completely. The background and methods

sections do provide thorough definitions of the different carbon pumps, controls on alkalinity, and nutrient utilization efficiency, as well as a very detailed description of how the different carbon components are estimated in the model. At the same, all these components have been defined previously, so the paper might be shortened by placing large chunks into an appendix. The results section is similarly very wordy; it mixes methods, results, and discussion together; many points that take multiple paragraphs to make could be simplified to 1-2 sentences. I suggest a thorough attempt to go through this paper and streamline the writing. As one example, the entire top of page 13 describes how figure 2 will be put together, with only two half-sentences (regarding ensemble range of pH and pCO2atm) that describe results. Some additional examples (there are more): LN 12-17 on pg 14 – this section does not describe any results presented. Pg 15 LN 18-31 – the average global temperature of simulations has a range of 2.3-4.9_C, which results in a range in delta pCO2atm from Csat of -16 – +17 ppm or -13 to +12ppm (depending upon how the calculation is made). Glacial-interglacial implications belong in discussion.

Response: We find the referee's criticism valid and will address the problem according to the suggestions. For example, as much as possible of Section 2, Section 3.1.1 and Sections 3.2-3.4 will be put into an appendix. After re-running the model with pre-formed tracers (see Author's Response to Referees #2 and #3), the parts of the text that describe back-calculations to pre-formed nutrient from apparent oxygen utilization (see e.g. section 3.2, p. 10) and the regression model for preformed alkalinity (see section 3.3) can be removed entirely. This will be replaced by a short subsection (3.1.3), which describes the use of pre-formed tracers in the model. Note: Using pre-formed tracers rather than back-calculations causes only minor changes to the results, which indicates that the back-calculation method is robust in this case. However, for the sake of clarity and for shortening the manuscript, we change to using the pre-formed tracers. We thank the reviewer for the specific examples of how to improve the results section. We will make the suggested changes and work through the manuscript to find sentences and paragraphs that can be improved in a similar way. The results section will be compacted and clarified. Text that concerns glacial-interglacial implications will be moved to Section 5.3.

Point 2: Figure 2 summarizes all results, but its current presentation makes it very difficult to distill anything more than the general magnitudes. I suggest (1) providing the labels of the sensitivity experiments and sorting them somehow, perhaps by the anticipated magnitude of total effects, from largest to smallest; (2) separating this figure in to 3-4 panels: biological, residual, solubility, and total (indicating on the total plot the largest contributor to the total change).

Response: We have attempted to make the figure clearer by (1) providing the labels of the sensitivity experiment (note: the acronyms in the labels have changed from the submitted manuscript, to acronyms that should be easier to remember); (2) separating the figure in to the suggested panels; and 3) by re-sorting the simulations. We have chosen to keep the results sorted in pairs of high/low adjustments of circulation parameters (denoted x2 for doubled and /2 for halved, of which the doubled are always listed on top), and made this separation into pairs clearer, in order to make it easy to see the expected range of carbon storage differences within the span of the chosen parameter values. We have changed the order so that all the SEs with changes to atmospheric parameters come first, and put wind stress (WS) on top of atmospheric heat diffusivity (AD) because the wind effect is stronger. Then come the changes to the ocean diapycnal (DD, "vertical") and isopycnal (ID, "horizontal") diffusivities. Finally come the combined simulations, also re-ordered to have the simulations with larger DeltaTC come first. Sorting them by anticipated magnitude of total effects appears to be less useful, since the values of the explored parameters do not cover the full range of extreme values that could potentially be used in climate simulations (see response to point 3). The new version of the figure is attached to this response.

Point 3: Pg 9 Methods: Are the ranges for vertical diffusivity, wind stress, horizontal diffusivity, etc that are used in the sensitivity experiments comparable to the range of values that are normally used to tune models? Would be useful to provide this

information here, so that the reader can assess whether your sensitivity experiments represent values that might normally be used.

Response: For this study, our intention is for the ensemble to be representative of a wide range of plausible ocean circulation states. The chosen parameter ranges correspond to a halving and doubling of the values used in the control simulation. Our chosen values are within the parameter space explored for a predecessor to the GENIE-model by Edwards and Marsh (2005), except the low wind stress simulation (see below). Similar parameter ranges are also explored for GENIE by e.g. Marsh et al. (2013). For the most part, our selected values are within the parameter ranges that generate the subset Edwards and Marsh (2005) refer to as low-error simulations. In the Bern3D model, with physics based on Edwards and Marsh (2005) and thus similar to GENIE, Müller et al. (2006) doubled the observed wind stress (W = 2) to get a more realistic gyre circulation. Marinov et al. (2008 a,b) used the Geophysical Fluid Dynamics Laboratory Modular Ocean Model version 3, which has the same default value for isopycnal diffusivity (1500 m2 s-1) as our model. Marinov et al. (2008 a,b) explore a range of 1000-2000 m2 s-1 (c.f. our range of 750-3000 m2 s-1). When comparing with models that have different available tuning parameters, diagnostic variables such as temperature, salinity and AMOC volume transport can indicate whether our achieved states are within the common tuning range for ocean circulation. We compare the temperature and salinity ranges in two selected grid points of the ensemble of pre-industrial control states (PIC) of PMPI2 and CMIP5/PMIP3 (IPCC; see Table B1) to the corresponding grid cell ranges of our equilibrium states SE1-SE12. In these selected grid cells, we cover a similar span in salinity and an equally broad range in temperatures as the PMIP-ensemble, though the temperatures in our ensemble are higher (range shifted by $\sim$1.5°C). According to Muglia and Schmittner (2015), the PMIP3 PIC AMOC range is 12.6-23.0 Sv (Table B1). If we exclude the combined simulation with halved wind stress and halved diapycnal (vertical) diffusivity (SE12, now denoted WS/2_DD/2), which has a very weak AMOC (2.0 Sv), the AMOC range for our equilibrium states is 8.3-18.0 Sv (Table B1). Thus, there is a difference of $\sim$8-9

Sv between highest and lowest value, which is also the case for the PMIP3 PIC:s, but our ensemble does not cover the two highest PMIP3 AMOC values. This validation of the chosen parameter changes and a correctly formatted version of Table B1 will be included in the updated manuscript.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-166/bg-2017-166-AC1-supplement.pdf

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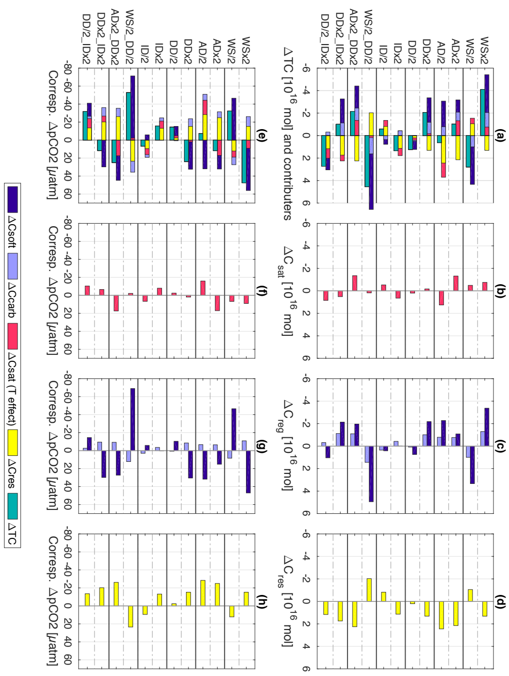

Fig. 1. Updated version of fig. 2