

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 thank the referee for the supportive comments on choice of topic and methods. We appreciate the detailed and helpful comments, which will improve the manuscript in its revised form. Below we address the general and specific comments of the referee. The technical corrections will be made in the revised paper, if they are still applicable in the new version of the manuscript.

General comment 1) I assume that you don't have preformed nutrients (O₂, PO₄, DIC) written out in the model output? If you do, this eliminates the problem with O₂dis, as you can calculate O₂sat and then calculate the remineralised O₂ and hence Csoft

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explicitly. Additionally, preformed O₂ and PO₄ would be much more useful in the parameterisation for ALKpre.

Response to General comment 1: We did not have preformed tracers in the output used for the first version of the manuscript. However, for the new version we have re-run the simulations to get the output for pre-formed tracers Cpre, O₂pre, PO₄pre and ALKpre. This eliminates the problems mentioned by the referee. It also eliminates the need for the regression model for ALKpre.

General comment 2) Is it possible to change the circulation in the model in the Southern Ocean and not in the Atlantic or vice versa? The bipolar seesaw could theoretically induce changes in Cdis in the Southern Hemisphere but not in the Northern Hemisphere.

Response to General comment 2: It is possible to efficiently change Cdis in only one of the hemispheres e.g. by changing the wind stress manually in selected regions rather than re-scaling it globally. We have done preliminary experiments where we reduce the winds only over the Southern Ocean, and even though this mainly causes changes of Cdis in AABW, the magnitude of DeltaCdis (henceforth we replace Delta by D in this response) is similar to the results for changes in global winds. We have therefore chosen not to include these simulations in the study. Studies of the bipolar seesaw go beyond the scope of the present study.

General comment 3) If it's not too cumbersome, perhaps consider using descriptive abbreviations for the different SEs.

Response to General comment 3: We will include such abbreviations in the updated manuscript.

Specific comments (SC): - SC1) p. 3, lines 4-5: Could you specify the order of magnitude of the change in CO₂?

Response SC1: The order of magnitude of the CO₂ rise in deglacials, as observed in

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ice cores (e.g. Petit et al., 1999), is ~ 100 ppm. It is likely that circulation differences as large as in PMIP LGM states (see e.g. Otto-Bliesner et al., 2007) could cause differences in deglacial CO₂ on the order of tens of ppm, if such experiments were run with equally different circulation states in cGENIE. However, the order of magnitude of the potential discrepancies is not explicitly mentioned by Zhang et al. in the cited paper, and we would not want to speculate about the response of the models they are evaluating in their study.

- SC2) p. 3, line 35: It's also important to note that these studies don't even have C_{res} due to infinitely fast gas exchange.

Response SC2: Marinov et al. (2008a,b) do not have C_{dis} in their main ensemble of simulations, because of the fast gas exchange in these simulations. They do, however, study the disequilibrium component separately for a subset of 3 simulations, where they state that they apply 'regular' gas exchange. In this study, we cover a wider range of simulations that include C_{dis}. In Kwon et al. (2011), they assume the change in C_{dis} to be negligible in the theoretical derivation and therefore run most of the simulations with very fast gas exchange. They do, however, also analyse some simulations with a 'normal' gas exchange coefficient, in order to validate the theoretical model for atmospheric pCO₂, but they do not explicitly study the disequilibrium response. The component of C_{res} that consists of calculation errors should be present in all of these studies, regardless of whether they have artificially fast or normal gas exchange. E.g. in Kwon et al., the calculation errors from the theoretical calculations of the changes in C_{soft} and C_{carb} will be grouped together with C_{sat}, which is not calculated.

- SC3) p. 4, lines 3-5: I don't find this paragraph necessary.

Response SC3: The paragraph will be removed in the updated manuscript.

- SC4) p. 4, lines 28-29: I don't believe that this is correct, as the pumps can have opposing effects. It should be specified that the net effect of all of the pumps must be to redistribute carbon from the surface to the deep ocean.

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Response SC4: We will rephrase the sentence as follows: 'If the net capacity of the carbon pumps to redistribute carbon from the surface to the deep ocean increased, this would act to decrease pCO_{2atm}.'

- SC5) p. 5, line 17: I think what you mean is that alkalinity is not set (or affected) by gas exchange, but referring to an "expected" value of C_{pre} is a little misleading.

Response SC5: Since the regression model for ALK_{pre} is no longer used, the two sentences at the end of this paragraph (starting with 'Unlike CO₂...') will be removed.

- SC6) p. 5, lines 31-32: Please include Martin (1990), *Paleoceanography*, as this is one of the central references for increased soft tissue pump efficiency during glacials.

Response SC6: We will add Martin (1990) in the updated manuscript.

- SC7) p. 7, lines 5-6: "and has a level of detail for the carbon system that made it particularly suitable for this study." This is very general; please specify why it is appropriate for this study (and/or why less complex models are not).

Response SC7: The referee asks particularly about less complex models. A very simple ocean carbon system model such as miniBLING (Galbraith et al., 2015), which only has P, DIC and O₂, would not be suitable for this study, because we are using ALK in our calculations. However, we want to emphasise that it is not only a question of complexity being high enough. Too high complexity could also pose a problem, e.g. a highly complex ocean ecosystem model with several functional types would be more expensive to run in terms of computational cost and therefore less suitable for this study. cGENIE balances having enough complexity in the ocean carbon system with a suitable level of complexity in terms of ocean resolution.

The full sentence on p.7, lines 5-6: "cGENIE is higher in complexity than box models, but is still efficient enough to allow running a large ensemble to equilibrium for the carbon system, and has a level of detail for the carbon system that made it particularly suitable for this study." should be rephrased as follows: "cGENIE is higher in com-

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plexity than box models, but is still efficient enough to allow running a large ensemble to equilibrium for the carbon system. In terms of ocean carbon system tracers, the minimum required for this type of study is P, DIC, O₂, ALK. cGENIE includes many additional tracers, out of which only some are used in this study. For example, particulate (POC) and dissolved organic carbon (DOC) are included in the calculations of model carbon inventory. Of particular importance for this study is the possibility to run with pre-formed tracers (Ppre, Cpre, O₂pre, ALKpre)."

- SC8) p. 8, line 25: When referring to Cdis, it would be useful to cite Ito and Follows (2013), GBC.

Response SC8: The citation will be added in the updated manuscript.

- SC9) p. 10, lines 14-24: Another important reference is Ito et al. (2004), GRL.

Response SC9: The citation will be added in the updated manuscript.

- SC10) Section 3.3: Do you use the same parameterisation for preformed alkalinity in all simulations? Please specify the errors in Cres in the surface field.

Response SC10: Yes, for comparability we were using the same parametrisation. Since we are now running with pre-formed tracers (including ALKpre), this question is no longer relevant for the updated manuscript.

- SC11) p. 12, lines 17-22: Again, please specify the size of the error introduced by making this approximation.

Response SC11: First, we want to emphasise that the dissociation constants used to determine CO₂ solubility in the model (Mehrbach et al., 1973) are only defined for temperatures between 2–35 °C. Since this restriction is applied in the model, we also apply it in the calculation of Csat throughout the manuscript. To make estimates of the error caused by the restriction, we must assume that the dissociation constants are valid for all temperatures and then make a new calculation of Csat without the restriction (henceforth we call this fullCsat). This assumption inevitably makes the error

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estimates approximate. However, the dissociation constants determined by Goyet and Poisson (1989) for artificial seawater between -1–40°C are similar to the constants given by Mehrbach et al. (1973). Thus, the assumption that the dissociation constants are applicable for a wider range of temperatures appears to be partly valid. When we calculate fullCsat, we find that this is larger than Csat in all ensemble members, by between 0.06 and 0.6% (clarification of line 20). When use the inventories of fullCsat to calculate DfullCsat (between the SEs and PIES278), we find that the contribution by temperature changes to DTC is on average underestimated by approximately 33 +/- 36% when the temperature restriction is applied. This hence strengthens our argument in Section 5.1. that the effects of the changes in the solubility pump should not be disregarded.

The paragraph on p. 12, lines 17-22 will be rephrased as follows: "The dissociation constants used in the cGENIE calculations of solubility for CO₂ in sea water follow Mehrbach et al. (1973), which are only defined for waters between 2–35 °C. Hence, the expression for CO₂ solubility in the model is restricted so that all water below 2 °C has the same CO₂ solubility (similarly for all water above 35°C). In the calculations of Csat, we use CO₂SYS with this temperature restriction, to accurately represent the model behaviour. In order to estimate the error introduced by this restriction, we need to assume that the same dissociation constants can be used outside the given temperature interval. The validity of this assumption is supported by the results of Goyet and Poisson (1989), who find similar dissociation constants for the interval -1–40°C in a study on artificial seawater. When CO₂SYS is run using model ocean temperatures without the temperature restriction, we find that the calculated inventory of Csat in the SEs is 0.06-0.6% larger than with the restriction. For PIES278 the inventory of Csat is 0.25% larger. In terms of DCsat, the unrestricted Csat inventories indicate that the contribution by temperature changes to DTC is on average underestimated by approximately 33 +/- 36% when the restriction is active. Since the restriction is used consistently, the error caused by the restriction being present in the model should not constitute a significant problem for our analysis. Nonetheless, the underestimation of

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the effect of temperature changes should be heeded in the discussion of our results.”

On p. 19, line 10, after the sentence ending with “. . .the dominant response”, we add: “Due to the temperature restriction on the CO₂ solubility constants (see Section 3.4), the effect of DC_{sat} is likely to be underestimated by on average 33 +/-36 % in our results, further emphasising its importance.”

- SC12) p. 14, lines 6-7: Isn't this relationship true per definition of C_{soft}?

Response SC12: The text on line 6-7 on p. 14 does not mention C_{soft}. We assume the referee is in fact referring to the sentence starting on p. 15, line 6-7, which mentions the linear relationship between C_{soft} and P*. It is true that this linear relationship results from the definitions of C_{soft} and P*, which is also mentioned in the next sentence, starting with “P* is a direct measure. . .” on p. 15, line 7.

- SC13) p. 15, lines 29-31: Please describe here the importance of the temperature limit on the calculation of C_{sat}, if this is on a comparable order of magnitude.

Response SC13: On line 28, after sentence ending “. . . larger than DC_{soft}.”, we add: “In addition, the temperature restriction on the dissociation constants (see Section 3.4) is likely to cause DC_{sat} to be underestimated by on average 33 +/- 36% in our ensemble.” Lines 29-31 are moved to Section 5.3, implications for glacial studies. After the sentence ending “. . .were likely larger than in our set of experiments.”, we add: “Fig. 7 suggests that DC_{sat} for a change in T_{avg} of -2.6°C c.f. pre-industrial would be approximately 2.6×10^{16} mol (310 GtC), whereas DC_{sat} for the coldest of our simulations is only 1.3×10^{16} mol (160 GtC). If we account for a likely underestimation of DC_{sat} of 30% (see Section 3.4) in Fig. 7, a simulation as cold as the LGM state suggested by Headly and Severinghaus (2007) would have an increase in strength of the solubility pump corresponding to ~400 GtC.

- SC14) p. 17, lines 13-14: Have you done experiments to specifically examined he role of sea ice in determining C_{dis}? Quantitative results would be very interesting!

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Response SC14: We have done experiments to examine the differences in C_{dis} between simulations in a subset of the ensemble (as described in the manuscript). There is some sea ice output available from these simulations, but we have chosen not to include a deeper analysis of this output in this already long manuscript. We have not made separate simulations where we e.g. only vary the extent of the sea ice, but this could potentially be done for a future study.

- SC15) p. 17, line 15: Only 0.01%? This seems to be at odds with Fig. 2.

Response SC15: The number should be 0.1% (the model carbon inventory is approximately 3×10^{18} mol and DeltaC_{dis} is on the order of 10^{16} mol). The calculation giving the number 0.01% used the difference between runs with normal and artificially fast gas exchange, which underestimated the signal of C_{dis}. We thank the referee for finding this error. The estimates of Fig. 2 agree better with the calculation of C_{dis} = C_{pre} – C_{sat}, resulting from the new runs with pre-formed tracers (see new Fig. 9).

- SC16) p. 18, line 15: Changes in the solubility due to ocean temperature changes don't seem “indirect”

Response SC16: By indirect we mean secondary, as in a response that is the result of some other change. In this case, the primary change is to pCO_{2atm} as the result of increased biological efficiency. As a response to the lower CO₂, climate changes in terms of changed ocean circulation and ocean temperature occur. There is then a secondary, or rather additional, response of the ocean carbon system to these changes. We will rephrase the sentence as follows: “There are also additional effects on pCO_{2atm} due to changes in ocean temperature caused by changes in radiative balance, circulation and disequilibrium.

- SC17) p. 19, lines 31-32: Ito and Follows (2013), GBC also uses the same scheme to look specifically at this; please include this.

Response SC17: The citation will be added in the updated manuscript.

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- SC18) p. 20, lines 23-24: Please specify what you mean by “an LGM-like circulation” and add appropriate citations

Response SC18: By an LGM-like circulation we mainly mean that the boundary between North Atlantic Deep Water (NADW) and Antarctic Bottom Water (AABW) was substantially shallower during the LGM than today. This circulation pattern is supported by paleonutrient tracers (reviewed by Marchitto and Broecker, 2006). This will be added to the updated manuscript.

- SC19) p. 22, line 27: Please cite the statement that “there may have been more, not less, preformed nutrients in the deep ocean during the last glacial”

Response SC19: The reference is Homola et al. (2015), but the reference is located in the wrong part of the sentence.

- SC20) p. 24, line 6: please specify if you mean the soft tissue pump and/or the carbonate pump

Response SC20: We mean both pumps. There are alkalinity corrections that are associated with the carbonate system as well as with the formation and destruction of organic matter (related to the nitrogen cycle).

- SC21) Fig. 8: Perhaps difference sections would be more useful?

Response SC21: We wish to also show the structure of the water mass in the different states. Hence, we suggest adding difference sections as supplementary material, showing how SE5 and SE6 deviate from PIES278.

- SC22) Fig. 9: It would be more illustrative to zoom in with the colourbar.

Response SC22: This figure will be updated to show $C_{pre} - C_{sat}$, which improves the estimate of C_{dis} and shows that the previous method (calculating the difference between runs with normal and artificially fast gas exchange) did not fully reveal C_{dis} . See new, updated figure, which is attached to this response. When pre-formed tracers

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are available, the simulations with unrealistically fast gas exchange are no longer used for the analysis. Note that the figure in the updated manuscript will be larger than displayed in this comment.

- SC23) Figures: Please make the font size larger, particularly in Fig. 7-10

Response SC23: This will be corrected in the updated manuscript.

- SC24) Table 3: Please give units for DC; why not include DCcarb?

Response SC24: The table shows correlation coefficients between circulation strength and DC, not values of DC in different ocean basins. The order of magnitude of DC could be specified in the table caption. DCcarb will be included in the updated manuscript. The first sentence of the table caption will be rephrased as follows: “Correlations between the changes in carbon species and the changes in strength of the zonal average overturning streamfunction ($PSI_{max} - PSI_{min}$) below 556 m depth in different geographical regions.”

Technical corrections: All technical corrections that are still relevant for the updated manuscript will be made.

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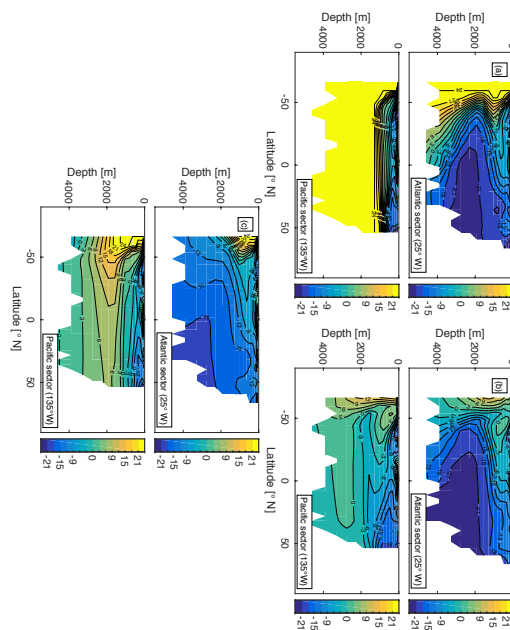


Fig. 1. Updated version of Fig. 9

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