The devil's in the disequilibrium: sensitivity of ocean carbon storage to climate state and iron fertilization in a general circulation model
Sarah Eggleston and Eric D. Galbraith
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Response: Reviewer 1

This paper describes some 44 simulation scenarios with the GFDL climate model in order to understand which processes might be responsible for the glacial CO2 drawdown, which is observed in the ice core data.

The paper makes an separation of DIC into the soft issue pump, the carbonate pump, and saturation and disequilibrium, and might in principle be worth publishing. However, the form of presentation needs some fundamental rework for various reasons, which are find below. I find both the representation of the text and of the results in the figure very sloppy and full of not very detailed descriptions.

1. Main issue: As already indicated by the title of the paper the disequilibrium component of DIC is the part of the carbon fluxes which seemed to be of major relevance, but which seemed to have been neglected in previous papers. Disequilibrium DIC ist the difference between saturated DIC (surface ocean DIC in equilibrium with the atmosphere, here defined by constant 270 ppm) and actual DIC. Marine carbon uptake is slowed down by the marine carbonate chemistry, because DIC for present surface ocean conditions is found as 1% CO2, 90% HCO3 and and 9% CO3, but only the 1% CO2 can exchange with the atmosphere. My understanding of the disequilibrium DIC is therefore, that it represents a different way of saying, that oceanic carbon uptake is restricted by marine carbon chemistry. For example, a change in atm CO2 by 10% leads only to a change in DIC of about 1%. This effect of the chemistry is summarized by the Revelle or buffer factor $R = (\Delta CO2/CO2)/(\Delta DIC/DIC)$ which is around 10, but various between 8 and 15 (e.g. Sabine et al., 2004, Science). Since full carbon cycle models all include the relevant carbonate chemistry, this effect is always included, and I am missing a connection of the newly analysed disequilibrium component of DIC with this issues. Maybe the disequilibrium DIC is not that new at all.

Thank you for the comment, which has indicated to us that the terms were not sufficiently described in the initial submission. We will elaborate on the discussion of DIC(dis) in the introduction of the text to clarify what it represents, and we plan to add a new figure to illustrate the concepts more clearly. In short, the disequilibrium component is indeed related to the carbonate chemistry, which slows the air-sea exchange as described, but it is also a function of biological uptake, ocean circulation, and gas exchange. It can be seen as the net result of all non-equilibrium processes on the DIC concentration within the surface layer.
It is also absolutely true that the disequilibrium effect is included in all full carbon cycle models (though, as pointed out on page 3 in lines 18-20, this component is by definition excluded from models when air-sea gas exchange is assumed to be infinitely fast). The purpose in recognizing it as an explicitly defined component of carbon storage is for understanding the underlying mechanisms. For example, in studying the glacial-interglacial change, the focus is generally on the importance of the saturation and soft tissue "pumps"

(e.g., Archer et al., 2000; Sigman and Boyle, 2000). We note that Ödalen et al. (Biogeosciences Discussions, 2017) use a similar approach, with a similar motivation.

2. Therefore, I have the feeling, the paper is lost in details, but misses more strength on a red line.

Thank you for the suggestion to highlight the take-home messages more clearly; we intend to make this a central goal of the revision.

3. I also believe the scenario definition with fixed and constant CO2 at 270 ppm is a major drawback in the value of the paper, since it implies that all quantifications of the fluxes have to be treaded very carefully: They have to be wrong, since the gas exchange of CO2 heavily depends on the pressure gradient in CO2 between atmosphere and ocean.

The choice to hold CO_2 constant at 270 ppm will indeed have an effect on DIC(dis). And of course, as George Box famously noted, all models (including this one) are wrong. However, as discussed on page 11 in lines 22-25, the atmospheric CO_2 acts primarily on the saturation concentration, while the effect on the disequilibrium fraction is expected to be small. The "minor, non-linear effects" here refer to the fact that air-sea gas exchange is itself a function of DIC(dis), as one term in the calculation of the piston velocity is the departure from equilibrium between the atmosphere and surface ocean with respect to CO_2 (using an atmospheric value of 270 ppm), as the reviewer has asserted. We recognize that this is an important point to address in more detail, and we will therefore provide a short quantitative estimate of the magnitude of this effect, along with additional clarification earlier in the paper (page 6, section 4: Discussion).
In addition to the fact that the effect is likely to be small, we note that the fact that the carbon cycle is equilibrated with 270 ppm in all cases helps to provide a cleaner comparison between simulations. The carbon cycle has many moving parts, and by holding the atmospheric CO_2 constant we provide a simpler picture of mechanistic differences. We would note that the value of the disequilibrium component will also depend on the base state of the soft tissue pump strength, ocean circulation, and alkalinity distribution, none of which are perfectly simulated by any model that we are aware of. Our work is not meant to be a definitive quantification of the disequilibrium component, rather, we hope to illustrate its conceptual importance (which has been overlooked) and show the basic controlling mechanisms.

4. The authors have chosen to keep atmospheric CO2 fixed, so they calculate changes in oceanic DIC only as a function of prescribed CO2 (always 270 ppm), which ignores dynamic aspects of the gas exchange, that largely depend on the surface ocean-atmospheric difference in pCO2. This is a significant simplication, which reduces the significance of the quantification of the process separation a lot. It implies, that atmospheric CO2 concentration is not a dynamic part of the carbon cycle analysis anymore. CO2 is nevertheless varied, but only to generate different background climatologies, implying only the radiative forcing of CO2 is used here. I therefind find the description of all scenarios and results highly confusing, they should not be defined by the prescribed atm CO2 value, because this is not considered in the carbon cycle change, but by the resulting global annual mean surface temperature changes, ΔT. I therefore expect, that the authors, (i) calculate ΔT, probably with respect to their control simulation (probably the one with CO2 = 270 ppm), and (ii) use ΔT when describing the scenarios, in the text, in Table 1, and in the Figures (e.g. x axis of Fig 1, 4). Since changes in

obliquity, precession, and land ice sheet might also change ΔT, they might also be more specific and call this ΔTCO2, but they might then also analyse the temperature change related to these other processes. If they then plot results in Fig 1 as function of Delta T, a lot of the various scenarios with similar CO2, but different other boundary conditions might then separte in ΔT, and might be easier to be identified. Right now Fig 1 is an mess, with various symbols plotted on top of each other. If thss step does not improve figure 1, the authors might also consider to plot Fig 1 as bar charts, where different scenarios by definition are plotted NEXT and not ON TOP of each other.

Thank you for the suggestion; we can see that the radiative-CO_2 labeling was a major source of confusion for both reviewers, given that it is independent of the CO_2 used for air-sea exchange. We will relabel the axes on these plots in terms of the associated change in mean surface air temperature (ΔT). We also appreciate the suggestion to alter the plot style, but substituting a bar plot for figure 1 (as in figure 2) would reduce the amount of information that we are able to convey by eliminating the x-axis and thereby the depiction of the average DIC concentration as a function of ΔT, so we would prefer to retain this plot style, which is more easily legible when plotted vs. temperature.

5. Anomalies on mean ocean DIC are analysed given in μmol/kg. However, I would find it much more helpful, if the amount of carbon taken up by the ocean would be given in terms of PgC (= GtC), which should be transferable easily (if the mean density of water and the volume of the ocean are known). Maybe, if the authors insist on their view on the system (μmol/kg), they might simply add a 2nd y-label (right-handside) with PgC. This would help a lot, since it is not clear to me, if the setup of the climate model in the LGM mode (more land ice) would also imply less ocean volume, which would directly affect the concentration of DIC in μmol/kg, but not total amount ocean C in PgC. The discussion how much a change in DIC would change atm CO2 (Discussion, page 6) can be simplified a lot by stading the change in oceanic DIC in PgC.

Thank you for the suggestion. We will add a second y-axis indicating the change in DIC in PgC. The range of the changes in the ocean volume across the different simulations is less than 0.5%, so the changes in DIC concentration and oceanic C inventory are approximately proportional.

6. In the text various times a change in carbon due to a change in ocean circulation is seen (e.g. page 5, line 26, line 29, page 6, line 2), however, the ocean circulation state is never described in the draft. The reader does not know which of the scenarios has a slower ocean overturning. If this relationship should be kept in the text, some further details (ocean circulation analysis) is needed. My impression is, this might be found in the draft Galbraith + de Lauvergne (submitted), but this is not accessible. So, either the resubmission of this paper has to wait for the other paper, or some of these analysis need to be repeated here.

Indeed, this is a key aspect of the manuscript submitted by Galbraith and de Lavergne. Because this is a journal without an open discussion (Climate Dynamics), it is, as the reviewer points out, not yet available. However, we anticipate that it should be accepted before we resubmit this manuscript. We will also add a short summary of the overturning circulation in the different scenarios to the results section.

7. I disagree with the potential usability of the quantification of the soft tissue pump on page 6, implying that the equations might be used as simplification in more complex models.

First. eq 6 should be deleted, since the change in atm CO2 can only be quantified once atm CO2 is calculated dynamically. Second, the ocenic DIC uptake (eq 5) has also as major weakness to deal with the overall setup, that include constant and identical CO2=270 ppm, which implies, that quantification have to be discussed with care. Here, they are taken as given quantification of the soft tissue pump, which might even be used elsewhere. I do not think, this is the case for the reason given above. Furthermore, the sentence, page 7, line 22 "In contrast, the model suggests that greater ocean ventilation rates in the glacial state would have led to reduced global DICsoft." and the following discussion is coming from nowhere and is not supported with any data. We know nothing on the ocean ventilation stage so far. Later-on, in section 4.7 it is argued, that interactive CO2 would only lead to minor effect. I strongly disagree, since the atmosphere-surface ocean gas exchange is a function of the pCO2 difference of both.

We include equation 6 in order to illustrate the approximately linear dependence of changes in CO_2 due to the soft tissue pump and the product of global export and ideal age of the ocean. As discussed above, the major differences in DIC(total) due to setting atmospheric CO_2 to 270 ppm should be in the DIC(sat) term. Our calculations are based on a prescribed atmospheric value of CO2 of 270 ppm; thus, using the equation R = $(\Delta CO_2/CO_2)/(\Delta DIC/DIC)$, we simply substitute equation 5 for $\Delta DIC(soft)$, the global average DIC(total) for DIC, $CO_2 = 270$ ppm and R = 10, we derive equation 6. But we do not mean to use this to ignore the intricacies of this or any other general circulation model; our aim is simply to demonstrate how these three variables appear to be related in these scenarios, in order to improve the conceptual understanding. Because it is common the think about carbon reservoir changes in terms of atmospheric CO_2, we feel it is valuable to leave equation 6 as it is, but we will add a discussion to this section to clarify that this is indeed a simplification. We would also point out that Ödalen et al. (2017) independently made an almost identical simplification.

8. Whenever a statement is made, which variable change how much, which is also find in a figure, please include a reference to this figure.

Thank you for this suggestion; we will add these references.

9. The meaning of the four separations of DIC (DICsat, DICdis, DICsoft, DICcarb) are explained in the abstract, but not again in the introduction. I believe, this should be repeated around line 10 (page 2), to make the main text independent from the abstract.

Thank you for this suggestion; we will add this to the introduction.

10. page 2, line 24: "DICsoft depends ... on the flushing rate of the deep ocean which clears out accumulated DICsoft". This is a bit sloppy and not correct: DICsoft depends on the ocean circulation as a whole, the surface to deep ocean transport also transports it to the deep ocean.

We agree and will alter this phrasing.

11. page 3, line 11: "preformed DIC" is not explained/defined her, onlxy later on page 4.

We will add an explanation of preformed DIC to the introduction along with the description of the DIC decomposition (comment #9).

12. page 3, line 25f: "Here, we use a fully-coupled general circulation model (GCM) to investigate the potential importance of DICdis in altering air-sea CO2 partitioning on long

timescales." This aim of the paper is not given as such in the abstract, but should be contained there.

We will add this to the abstract as suggested.

13. page 3, line 30: "prescribe a constant CO2": It is not only constant in individual experiments, it is also identical in all experiments as 270 ppm. This should be clarified here directly, because it implies, that the atm CO2 is only a driver of climate, but not of carbon cycle changes.

We will clarify this at this point in the text.

14. page 4, line 5: "static land and ice sheet". Please explain, or should this read "static land ice sheets"?

Thank you for catching this; the list should read "…a sea ice module, static land, *and static* ice sheets." This will be changed in the text.

15. page 4, line 10: The paper Galbraith and de Lavergne (submitted) is missing in the reference list, but need to be included there, at best with a link to an accessible version. If not, maybe introduce this information only, once the paper is accessible somewhere, eg in the next iteration of the paper.

We will include this information in the bibliography. As noted (cf. #6), we expect that that paper will be accepted for publication before this manuscript is resubmitted and in any case before publication.

16. page 4, experiments: As already given above, the different CO2 levels should be transfered into ΔT. Also give a brief reasoning for your choices of CO2 here, e.g. why is the reference CO2 270 ppm, and not 278 ppm, as usual, why 607 and 911 ppm?

The reference CO_2 of 270 ppm is used as a simplification for interglacial CO_2. The radiative forcing is proportional to $\ln([CO_2])$; thus, 405, 607 and 911 ppm are chosen as linear increments above 180 and 270 ppm (the former approximately represents glacial CO_2). This rationale will be added to the text on page 4, section 2.2.

17. page 4, experiments: Orbital parameters: Eccentricity seemed to have not changed, but nevertheless, state its values, and for which climate state it is typical. Also state, typical values of obliquity and precession for today and LGM.

This is correct: eccentricity is constant in all of these simulations. This as well as the values for obliquity and precession for modern and LGM conditions will be added to page 4, section 2.2.

18. page 4, line 17: Iron fertilization: Did I get it right, that the glacial dust fields only change iron availability, but not the radiative forcing? Please clarify. Furthermore, the field in the cited paper (Nickelsen and Oschlies 2015) are taken from Mahowald et al 2006, which should be state here. Also be aware (and potentially discuss), that, at least to my knowledge, more recent LGM dust fields of the Mahowald groud differ to the dust field published in 2006 (especially in the high-latitudes), (e.g. Albani et al 2012 (Clim Dyn), Albani et al 2016 (GRL), which might have an impact on Southern Ocean iron fertilization.

This is correct: the radiative forcing is not adjusted based on the dust field. This will be clarified in the text. We will also add the Mahowald et al. (2006) reference to page 4, line 17 and note the disagreement with more modern reconstructions.

19. page 5, line 18: "standard deviation of only 4 µmol/kg". I do not see any error bars in Fig 1, and in DICcarb the values vary by 20 to 35 µmol/kg. Something is wrong here.

As stated in the text (page 5, line 18: "over the entire range of CO_2 values"), this standard deviation is not an error bar for individual simulations, which are simply run to equilibrium, but rather the standard deviation of the values of DIC(carb) among all simulations. This will be clarified in the text.

20. page 5, line 23: "under nutrient depletion experiment". Which one would that be? I only see Fe fertilization expleriments, which would be the opposite.

This refers to the experiments run by Ito and Follows (2013), as indicated in the text. We did not conduct nutrient depletion experiments but, as noted, rather the opposite, in order to investigate the same dependence. We will clarify the phrasing here.

21. page 5ff: I suggest to combine "3 Results" and "4 Discussions" into "3 Results and Discussions" since sec 3 so far is pretty short and sec 4 also includes results.

We plan to restructure these two sections in the revision; thank you for the input.

22. page 6, line 30: Define "ideal age".

The ideal age tracer is a conservative tracer that is set to 0 in the surface ocean and increases by 1 for each model year in the ocean interior. This definition will be added to the first discussion of this tracer in the main text (page 5, line 21).

23. page 6, line 31,32: "quantitative strength of the relationship is striking". It is, what I would expect. The interesing part for me would actually be, to see the individual contribution of the two variables combined in Figure 3, so please also plot "global export" and "age" and define both precisely (are they averaged, if yes, over which results?).

The correlation with DIC(soft) and each of these parameters individually is lower than the correlation of DIC(soft) and the product of global export and age, which is why only the latter is presented here. Indeed, global export and age are individually averaged spatially over the global ocean for each simulation, and then the product of these two averages is taken. This will be clarified in the text (page 6, lines 29-30). We will also add the two additional panels, as suggested.

24. page 7, eq 5,6: These regressions equations are of very limited use, since atm CO2 has been kept constant at 270 ppm, and the gas exchange is a linear function of the atmosphere-surface ocean CO2 difference.

As in comment #7, these equations are given simply to illustrate the apparent linear relationship between global export * ocean age and DIC(soft), which can roughly be translated into a change in atmospheric CO_2 related to the soft tissue pump. Therefore, although coefficients in these equations are only derived empirically, we would argue that they add to the text by showing the relationship depicted in figure 3 mathematically.

25. page 7, line 22: We know nothing on ocean ventilations rates yet.

As in comment #6, we will add a brief summary of the central results of the Galbraith and de Lavergne paper, which will include the results of ocean circulation and ventilation in the different simulations.

  26. page 8, line 2: Here surface and deep ocean are split at 1 km depth, but in Fig 5 at 500 m
      depth, Please be consistent.

Thank you for catching this inconsistency, which reflects a change in the choice of boundary in earlier versions of the manuscript; we will change this to consistently use 1 km for the boundary between the surface and deep ocean.

  27. page 8, Eq 7: Here, the Southern Ocean contributen is termed fAABW, while in Eq 10 it
      is termed fSO. Please be consistent.

We consciously draw a distinction between f_{SO} (the fraction of water originating from the surface south of 30^oS) and f_{AABW}, which is not explicitly traced in the model but would be the true fraction of AABW. Similarly, we use f_{NAtl} and f_{NADW} to refer to the modeled approximation and true fractions of NADW, respectively. We will clarify this in the text (page 8, lines 7-8).

  28. page 8, line 15: "hot and cold climate state": Be more precise what this means in terms of
      scenarios. This sentence probably refers to Fig 4, but no reference to it was given.

"Hot" and "cold" climate states refer to the extreme radiative forcing scenarios (CO_2 at 911 and 180 ppm, respectively). We will clarify this in the text and add a reference to figure 4 on page 8, line 15.

  29. page 8: I suggest to combine the to subsection 4.2 and 4.3 to one subsection.

We will restructure these sections.

  30. page 8, line 26: "when deep convection is occuring" When does it occur and where,
      scenario?

Deep convection in the model is sporadic in the simulations. The simulations with deep convection are those shown in figure 4 b. In the Southern Ocean, this includes all simulations; in the North Atlantic, this is all simulations at $CO_2 = 180$, 220, and 911 ppm, 1 (of 4) at $CO_2 = 270$ and 405 ppm, and 3 (of 4) at $CO_2 = 607$ ppm. We will allude to this in the text (page 8, line 26).

  31. page 8, line 27: "... ventilation rates are high at both the cold and hot extremes". Show
      plot.

We will include a plot showing this.

  32. page 9, line 9: O2;dis is not defined (I believe). You might add why O2 equilibrates an
      order of magnitude faster than CO2 (no bottleneck of the carbonate chemistry during
      oceanic uptake).

We will define $O_2(dis)$ on page 9, line 6 (equivalent to DIC(dis), it is equal to the departure from equilibrium of O_2 in the surface ocean with respect to the atmosphere and advected into the ocean as a conservative tracer). Additionally, we will briefly discuss why it is able to equilibrate approximately an order of magnitude faster than DIC, as stated on page 9, lines 6-7.

33. page 9, line 11: "O2;dis in the SO is as high as 100 µmol/kg". I believe this is wrong, should be "as low as -100 µmol/kg", and the reference to the figure is needed (Fig 9). O2;dis seems to be largely anticorrelated to DICdis, but this was never mentioned as such.

We will change this in the text to read "The magnitude of O_2(dis) in the SO is as high as 100 µmol/kg" and add the reference to figure 9. We will also mention the anticorrelation.

34. page 9, line 21: "reduced sensitivity of export" to what?

We have written "reduced sensitivity *to* export;" this refers to the relationship between DIC(soft) and global export (cf. page 9, line 20). We will clarify this in the text.

35. page 9, last line: I do not undertstand why the two effects of iron fertilization and remineralization rate should be linearly additive.

We agree that this is not obvious from first principles, but it appears to be the case in this model. This is evidenced by the fact that DIC(Fe fert)-DIC(no Fe fert) for each of the DIC components is approximately the same using both remineralization rates under pre-industrial conditions, as shown in the first two panels of figure 2. We will clarify this in the text (page 9, lines 30-32).

36. page 10ff: Change NO_3 in NO_3^-

We will make this change.

37. page 10, line 13: What happens to you framework, if N:C ratios are not constant, as for example in postulated by Geider et al. (1998)?
Geider, R. J.; MacIntyre, H. L. Kana, T. M. A dynamic regulatory model of phytoplanktonic acclimation to light, nutrients, and temperature, Limnology and Oceanography, 1998, 43, 679-694.

This would indeed the possibility of calculating DIC(soft) from NO_3^-; i.e. equation 8 would no longer hold. The RMSE of the DIC(dis)+DIC(soft) is 5.2 µmol/kg; to achieve this error through changes in the N:C ratio only, this ratio would have to vary by more than 6%, which Geider et al. (1998) suggest is possible. However we still feel that this is an improvement on using preformed PO_4^{3-}, given that P:C varies much more dramatically (Geider and La Roche, 2002; Galbraith and Martiny, 2015). We will add a short discussion of this point to the text at the end of this section (page 11, line 18).

38. page 10ff: Unified framework: Here it is argued that the NO3;pre can be split in the contributions from Southern Ocean (SO), North Atlantic (NAtl) and North Pacific (NPac), later-on the argument is made that the North Pacific can be neglected due to missing deep convection. The contributions to DICdis;deep early in the draft (sec 4.2) was only split between NAtl and SO, but not NPac. Please be consistent in both approaches.

We will state earlier on page 10 that the contribution from the North Pacific is negligible and remove it from equation 10.

39. page 11, Eq 14 and 15: I have the feeling the factors Vi=Vtotal which are included in Eq 14 are missing in Eq 15, but should still be included here.

These are absorbed into the individual terms of global NO_3^-, NO_3^-(den), DIC(dis), and NO_3^- (pre). For example, NO_3^-(global) = V(deep)/V(total) * NO_3^-(deep) + V(upper)/V(total) * NO_3^-(upper). Thus, the volume terms drop out of equation 15.

40. page 11, line 17: RMSE: How has this been found? Is this the mean difference to the 1:1 line in Fig 10?

Yes, this is calculated from the modeled values of the different components (global average in each simulation) compared to the parametrized values (calculated from equation 15). We will clarify this in the text (page 11, lines 16-18).

41. page 11, Section 4.7: I think this section might be called "General discussion". I do not think the naming of "DICdis nadir" is helpful here, pleasxe consider other wording.

We will revise and rename this section.

42. page 12, lines 7-13: This paragraph is highly speculative and with the given support not justified. We know nothing on AABW formation for different climate state and the fixed, constant atmospheric CO2 boundary condition hinders in my view that such bold hypothesis are made based on the made analysis.

Indeed, this paragraph is deliberately speculative. This is stated in the text ("We do not claim that this soft upper limit was significant, but simply propose the possibility as a hypothesis that can be tested"). As such, we find it a useful contribution to the paper, as it points to a testable hypothesis for future work.

43. page 13, line 13: "ratio of remineralized to UTILIZED O2"

Thank you; we will add "utilized" to the text (page 13, line 13).

44. page 13, line 18ff: Preformed alkalinity (alkpre) is defined as total alkalinity at the surface. However, alkpre is then calculated from T, S, NO3, PO4. Does this imply, total alkalinity is not followed as an independent tracer in the carbon cycle of the ocean in the model? If not, this need some explanation, since normally the full carbon cycle needs 2 variables to be fully prognostic, typically the conservative tracers DIC and ALK are taken for that (from which pH, CO2, CO3, HCO3 are then calculated. Please clarify, and explain.

Preformed alkalinity was not included in the simulations. Therefore, as described at this point in the text, performed alkalinity has been a posteriori reconstructed as a linear function of temperature, salinity, and nutrients, similar to the approach used by Bernardello et al. (2014). This will be clarified in the text (page 13, line 18).

45. page 14: Eq A6 is trivial, its another version of Eq A1.

We will remove equation A6 from the text.

46. Figures: Make plots larger to have larger font size, include sub-plot names (eg Fig 1a,bc), check if units are always given (missing in Fig 11). In all figures each caption needs to have the full explanation of what is seen here (and not referring to Fig 1 as done so far).

We will increase the font sizes, include subplot labels, and write the marker key (as in figure 1) in each of the plots using this scheme (figures 3, 4, 5, and 10).

47. Fig 1: Plot all 5 subfigures on top of each other, planing, that this will fill 1 column in the final layout text, change x axis to ΔT, consider plotting it as bar charts, clarify what you mean in the caption with changed seaonality (probably a change in pressession).

We will plot the five subplots vertically and change the x-axis to ΔT. As above (cf. #4), we prefer to retain this plot style to demonstrate the relationship between DIC and radiative

forcing in this model. Indeed, "seasonality" refers to precession, which we will clarify in the caption.

48. Fig 2: If results are averages from 4 runs (as said here) give mean and error from averaging.

Indeed, this assertion is correct. As the means are given (these are the values plotted), we will include the standard deviations of the respective four runs as error bars and clarify this in the caption.

49. Fig 3: X axis: "Global export times age". "Global export" here implies only export of organic C or also CaCO3 export?

This refers to export of organic C only; we will clarify this in the figure caption.

50. Fig 4: Why do you take DICdis at 100m water, and not the average over the mixed layer?

We show DIC(dis) at 100 m depth given that we are using annual mean concentrations, and there is a strong seasonal cycle of mixed layer depths at high latitudes. The depth of 100 m was chosen keeping in mind that polar deep-water formation regions tend to have strong haloclines, and 100m tends to be below the summer mixed layer but within the winter mixed layer, and is therefore relatively representative of the mixed layer concentrations when deep waters are forming.

51. Fig 6,8,9,11: Define scenarios "glacial" and "interglacial". Are these averages over several scenarios? Unit is missing in Fig 6.

These are single scenarios where the CO_2, obliquity, precession, and ice sheet configuration are 180 ppm, 22º, 90º, and LGM for the "glacial" scenario and 270 ppm, 24º, 90º, and pre-industrial for the "interglacial" scenario, respectively. We will clarify this in methods and in the figure captions and will add the units of DIC(dis) (μmol/kg) to the caption of figure 6.

52. Fig 7: I find this figure, highly confusing (not clear what lines represent), and not necessary at all.

We agree and will remove this figure.

53. Fig 10: Caption: Please reduce text and refer to where the Eq is found in text, e.g. lower plot y axis follows Eq 15. I am not sure for the other 2 plots there is a complete Eq contained in the theoretical framework, if not, please extend.

We will cite equations 11, 12, and 15 in the caption and reduce the caption length by referring to the appropriate section of the text for details (section 4.6). As we note in the text, we only expand the equations for the deep ocean (page 11, lines 1-2), but the full equations for DIC(dis) and DIC(soft) follow analogously to equation 13.

54. Fig 11: I am not sure, this Fig is necessary.

We find this important to show where the parametrization used for preformed alkalinity induces errors in the DIC(dis) analysis (cf. #44), as this is the main source of uncertainty. Thus, we would prefer to retain this figure in the appendix.

References

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