

Interactive comment on “The devil’s in the disequilibrium: sensitivity of ocean carbon storage to climate state and iron fertilization in a general circulation model” by Sarah Eggleston and Eric D. Galbraith

Anonymous Referee #1

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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.

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 is 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 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 $R = (\Delta CO_2 / CO_2) / (\Delta DIC / DIC)$ which is around 10, but varies 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 issue. Maybe the disequilibrium DIC is not that new at all.

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

Other majors:

- 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 simplification,

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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 therefore 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 ΔT_{CO_2} , 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 separate 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 this 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.

- Anomalies on mean ocean DIC are analysed given in $\mu\text{mol/kg}$. 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 ($\mu\text{mol/kg}$), 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 $\mu\text{mol/kg}$,

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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 stating the change in oceanic DIC in PgC.

- 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.
- 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 oceanic 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 DIC_{soft}." 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.

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

Minors (chronologically):

- The meaning of the four separations of DIC (DIC_{sat} , DIC_{dis} , DIC_{soft} , DIC_{carb}) 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.
- page 2, line 24: " DIC_{soft} depends ... on the flushing rate of the deep ocean which clears out accumulated DIC_{soft} ". This is a bit sloppy and not correct: DIC_{soft} depends on the ocean circulation as a whole, the surface to deep ocean transport also transports it to the deep ocean.
- page 3, line 11: "preformed DIC" is not explained/defined her, onlxy later on page 4.
- page 3, line 25f: "Here, we use a fully-coupled general circulation model (GCM) to investigate the potential importance of DIC_{dis} 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.
- 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.
- page 4, line 5: "static land and ice sheet". Please explain, or should this read "static land ice sheets"?

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- 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.
- page 4, experiments: As already given above, the different CO₂ levels should be transferred 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?
- 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.
- 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 group 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.
- page 4, line 18: "standard deviation of only 4 $\mu\text{mol/kg}$ ". I do not see any error bars in Fig 1, and in DIC_{carb} the values vary by 20 to 35 $\mu\text{mol/kg}$. Something is wrong here.
- page 4, line 23: "under nutrient depletion experiment". Which one would that be? I only see Fe fertilization experiments, which would be the opposite.
- 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.

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- page 6, line 30: Define "ideal age".
- page 6, line 31,32: "quantitative strength of the relationship is striking". It is, what I would expect. The interesting 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?).
- 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.
- page 7, line 2": We know nothing on ocean ventilations rates yet.
- 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.
- page 8, Eq 7: Here, the Southern Ocean contribution is termed f_{AABW} , while in Eq 10 it is termed f_{SO} . Please be consistent.
- 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.
- page 8: I suggest to combine the to subsection 4.2 and 4.3 to one subsection.
- page 8, line 26: "when deep convection is occurring" When does it occur and where, scenario?
- page 8, line 27: "... ventilation rates are high at both the cold and hot extremes". Show plot.

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- page 9, line 9: $O_{2,dis}$ 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).
- page 9, line 11: " $O_{2,dis}$ in the SO is as high as 100 $\mu\text{mol/kg}$ ". I believe this is wrong, should be "as low as -100 $\mu\text{mol/kg}$ ", and the reference to the figure is needed (Fig 9). $O_{2,dis}$ seems to be largely anticorrelated to DIC_{dis} , but this was never mentioned as such.
- page 9, line 21: "reduced sensitivity of export" to what?
- page 9, last line: I do not understand why the two effects of iron fertilization and remineralization rate should be linearly additive.
- page 10ff: Change NO_3 in NO_3^-
- page 10, line 13: What happens to your 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 phytoplankton acclimation to light, nutrients, and temperature, *Limnology and Oceanography*, 1998, 43, 679-694.
- page 10ff: Unified framework: Here it is argued that the $\text{NO}_{3,pre}$ 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 $\text{DIC}_{dis,deep}$ early in the draft (sec 4.2) was only split between NAtl and SO, but not NPac. Please be consistent in both approaches.
- page 11, Eq 14 and 15: I have the feeling the factors V_i/V_{total} which are included in Eq 14 are missing in Eq 15, but should still be included here.

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- page 11, line 17: RMSE: How has this been found? Is this the mean difference to the 1:1 line in Fig 10?
- page 11, Section 4.7: I think this section might be called "General discussion". I do not think the naming of " DIC_{dis} nadir" is helpful here, please consider other wording.
- 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 CO_2 boundary condition hinders in my view that such bold hypothesis are made based on the made analysis.
- page 13, line 13: "ratio of remineralized to UTILIZED O_2 "
- page 13, line 18ff: Preformed alkalinity (alk_{pre}) is defined as total alkalinity at the surface. However, alk_{pre} is then calculated from T, S, NO_3 , PO_4 . 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, CO_2 , CO_3 , HCO_3 are then calculated. Please clarify, and explain.
- page 14: Eq A6 is trivial, its another version of Eq A1.
- 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).
- 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,

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- clarify what you mean in the caption with changed seasonality (probably a change in precession).
- Fig 2: If results are averages from 4 runs (as said here) give mean and error from averaging.
 - Fig 3: X axis: "Global export times age". "Global export" here implies only export of organic C or also CaCO₃ export?
 - Fig 4: Why do you take DIC_{dis} at 100m water, and not the average over the mixed layer?
 - Fig 6,8,9,11: Define scenarios "glacial" and "interglacial". Are these averages over several scenarios? Unit is missing in Fig 6.
 - Fig 7: I find this figure, highly confusing (not clear what lines represent), and not necessary at all.
 - 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.
 - Fig 11: I am not sure, this Fig is necessary.

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