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Climate of the Past Discussions

CPD

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Interactive Comment

Interactive comment on "Quantifying the roles of ocean circulation and biogeochemistry in governing ocean carbon-13 and atmospheric carbon dioxide at the last glacial maximum" *by* A. Tagliabue et al.

Anonymous Referee #2

Received and published: 11 August 2009

Alessandro Tagliabue and co-workers preset a well-rounded model-data synthesis of glacial-interglacial changes in ocean d13C, as a means of helping constrain the mechanisms involved in the observed variability in atmospheric CO2. I have a few moderate-sized points, and some minor ones, in keeping with the overall quality of the study.

I am somewhat nervous that CO2 changes are discussed and plotted in 'carbonate compensated space' in Figure 7. The reference for carbonate compensation contributing 30-40 ppm is Brovkin et al. [2007], yet this presumably assumes the same glacial ocean state that the authors dismiss in their discussions(?) You cannot have you cake

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Interactive Discussion

Discussion Paper

and eat it! ;) Since the glacial CO2 problem is meant to still remain unsolved, and indeed, apparently even less well understood than we thought according to the abstract, it seems rather unsafe to assume that the contribute of carbonate compensation (30-40 ppm) is accurately known. Please re-plot Figure 7 in normal delta-pCO2 space. I believe that the paper will be all the more clearer and less subject to confusion and hence have greater impact this way. By all means then speculate how much of the CO2 change not explained to the model might be attributed to carbonate compensation (with references). Similarly, in the abstract, I do not think that you can state: 'we can attribute over 90% of the change in atmospheric CO2' on the basis of the work presented here, i.e., lacking an explicit estimate of carbonate compensation.

Another area that might be much better clarified concerns the initial state of the model. Most of the plots (Figures 1, 2, 5, and 6) are given as anomalies (deviations from a baseline state). It would be really helpful to better interpret the impacts presented if the initial states were shown alongside (I recognise that these states may well be fully described elsewhere, but it is a real hassle to have to go dig them up simply to fully interpret the results presented here). Associated with this I have some queries regarding some of the more prominent features in the model behaviour. For instance, the 'plume' of highly oxygenated water emanating from the Ross Sea and filling the SE Pacific basin at depth (Figure 5) is highly unexpected. It is also picked out in d13C (Figure 2). The feature spills across the Drake Passage and into the SW Atlantic Ocean, yet I thought that the glacial radiocarbon ages in the Drake Passage area were meant to be pretty old (do not have ref immediately to have . . .). Do the authors believe that this feature could be a genuine facet of the glacial ocean or might it be a model artefact? What role does an apparently elevated rate of ventilation of (at least part of) the deep glacial Southern Ocean play in the atmospheric CO2 predictions – could the CO2 drawdown be underestimated here because of this? Also, how can CircA represent 'decreased ventilation' w.r.t. d13C (Figure 2 caption) while at the same time causing substantially elevated dissolved oxygen concentrations (Figure 5)? Or is the 'decreased ventilation' an overall (mean) assessment, which is actually highly heterogeneous in practice, with regions of both decreased and increased ventilation? Further explanation/discussion is required on this point.

Model integration time – quoted as 3000 years for the pre-industrial baseline simulation, and 500 years for the glacial experiments – is this sufficient? 3000 years seems surprisingly short to establish the D14C distribution in the global ocean (especially compared to the lifetime of 14C!), and 500 years relatively short for establishing a new (perturbed) steady-state in deep ocean d13C. Do the authors know how far off of steady state they might be? Could some of the 'unusual' features, such as the high O2 anomaly in the SE Pacific (Figure 5), potentially be a transitory flushing event induced by the change in glacial boundary conditions? 500 years is well within the range of lifetimes of transitory states induced in models assessing Heinrich and D-O events (via freshwater perturbations) for instance. Would benthic properties have differed at all significantly if the model had been run for a further 500 (or even 5000) years? I appreciate the likely numerical expense ('heaviness') of ORCA2-PISCES, but running even a single glacial state rather further would give a good indication of how quickly the system re-equilibrates and hence how reliable all the experiments are.

On the subject of figures – some of them tend to the small side and it is difficult to make out some of the features, especially Figures 1 and 2 (whose dark background colours make the observations difficult to make out – perhaps plot the observations with a white border rather than black?).

other comments

o Abstract: The statement: 'we can attribute over 90% of the change in atmospheric CO2 to such factors' does not seem consistent with the following: 'over half of the necessary CO2 change remains to be explained'. Either you 'know' what the 'answer' is or you do not . . . ?

o Page 1464 / line 23: Please check the year of 'Sigman and Boyle' (and in the reference list and cited elsewhere) – I thought it was 2000, not 2002.

CPD
5, C563–C567, 2009

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

o Page 1464 lines 22-24: There are better references for glacial-interglacial changes in terrestrial biosphere carbon storage, and it would be helpful to add the estimated amounts of carbon.

o Page 1465 / lines 2-4: Clarify whether the increase in d13C was global or restricted to a particular basin(s), e.g. Atlantic (it matters to model-data inter-comparison).

o Page 1465 / lines 15-16: I know what you mean, but perhaps qualify that you are talking about greater carbon production per mol of (macro) nutrients.

o Page 1467 / lines 7-9: You mention 'LGM' pCO2 and salinity and alkalinity, but I thought that the 3000 year spin-up was for the pre-industrial (Page 1490 / lines 14-15) (?) If pre-industrial – please correct. If LGM is correct, then please state what the assumed salinity changes was, and more importantly, what the ALK change was, as surely we do not know LGM ALK, 'else we would have already solved the glacial CO2 problem . . . ?

o Page 1469 / line 11: Please add reference for the prescribed -0.4‰ change in mean ocean d13C. (And adding how much carbon this is would be helpful.)

o Page 1471 / lines 4-7: You can only be talking about Atlantic + Indian sector ventilation changes in the SO? Please clarify.

o Page 1472 / line 11 and Figure 5: Please clarify that the O2 anomaly is relative to the pre-industrial (or equivalent) simulation in Figure 5. Ideally add a second panel showing the pre-industrial simulation. We cannot possible verify or evaluate the statement in the main body of the text that the sluggish glacial circulation did not 'drive total anoxia at depth' (Page 1472, line 11) without seeing the baseline.

o Page 1472 / lines 21-26: I do not understand this. If you have less nutrient supply to the surface, then you presumably have lower nutrient concentrations at the surface. If the nutrients are not at the surface, they are at depth, along with CO2 . . . (?), and hence reducing atmospheric CO2? Looking at it another way – if you have glacial O2

CPD
5, C563–C567, 2009

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Interactive Discussion
Discussion Paper

depletion at depth compared to the interglacial baseline, normally one would expect more CO_2 at depth associated with the O_2 consumption . . . ? One might find ways of de-coupling CO_2 and nutrients, but it is much harder to (anti) decouple CO_2 and O_2? You may be right, but this seems the wrong way around. Please check very carefully that this is really what is going on. Or did you include an addition of CO_2 due to a reduced terrestrial biosphere carbon reservoir?

o Page 1476 / lines 6-11: Can you really invoke an increase in C:N on top of all of this? You have already admitted to having widespread dysoxia, which is not observed . . . ? And further involved biological processes increasing export efficiency (Page 1477 / lines 6-10) would surely exacerbate your deep O_2 problem?

o Figure 2: Missing colour bar/scale.

o Figure 3: Why are the CLIMBER 'change in NA ventilation' not broken down into depth and strength changes as per ORCA2-PISCES? (It is not, in fact, entirely clear how the AMOC changes in CLIMBER are calculated.)

o Figure 4: Is this a zonally-averaged Atlantic profile or a specific line of longitude in the mode (please specify)? It would be helpful to see the GLODAP (estimated pre-indsutrial) D14C for comparison with panel (a). Also – please label the 3 panels ('a', 'b', 'c') if you are going to refer to them by panel letter identifier in the caption text.

Interactive comment on Clim. Past Discuss., 5, 1463, 2009.