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Climate of the Past Discussions

Interactive comment on “Glacial CO₂ cycle as a succession of key physical and biogeochemical processes” *by* V. Brovkin et al.

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The author’s response (in *Italics*) to comments by Andy Ridgwell

Victor Brovkin and colleagues present a series of analyses using an Earth system model, that not only represent a new (mostly) internally consistent and importantly, non steady-state potential explanation for the observed glacial-interglacial variability in atmospheric CO2 but also illustrate how understanding is advancing towards the ultimate goal of accounting/simulating the glacial-interglacial cycles in their entirety (both climate can carbon cycling) as a response to orbital forcing alone. This is a useful addition to the literature and provides an interesting counterpoint to a series of recent papers using a similar (carbon cycle) model but coming to what on face value is a quite different interpretation of the causes of low glacial CO2. There are some

interesting findings on how a non-steady state analysis of glacial CO2 is important. Overall: although some important information and analysis is missing and needs to be provided for the paper to be of maximum value, there are no fundamental issues with the paper (subject to a couple of clarification) which would prevent publication (following suitable revision).

We are grateful to Andy Ridgwell for his very constructive comments and we will try to improve the analysis as much as we can do with the coupled model.

Primary criticisms/suggestions

The sedimentary (weathering) response is central to the authors’ simulation of CO2 variability. It is hence important that the model projections are rather more and critically exposed to the data. For instance, a useful time-series of variability in mean sedimentary wt% CaCO3 is provided in Figure 3 as a function of depth, hence illustrating what the CCD and lysocline are doing in the model. But despite analogous data-based reconstructions for this interval in time and for the Equatorial Pacific existing (e.g. Farrell and Prell [1989], albeit subject to arguments about how co-variation between depth and latitude might have distorted the original analysis) and modern [Archer, 1996] and LGM [Catubig et al., 1998] reconstructions of wt% CaCO3 for the global ocean (from which the average vs. depth for the 30°S-30°N latitude band in the Pacific could be extracted), no observations are provided here as a point of (essential) comparison.

Reconstruction of carbonate preservation in the Equatorial Pacific by Farrell and Prell (1989) reflects equatorial productivity / focusing effects (Archer, 1991) and it also has only low temporal resolution for the last glacial cycle. We do not think that it is insightful to use this reconstruction for quantitative comparison with the model. Catubig’s (1998) comparison of %CaCO3 between coretop and LGM didn’t find much if any systematic difference, except for the effects of dilution in the Atlantic. As suggested, we will do model-data comparison for wt% CaCO3 during LGM as in the transient model simulations. Let us emphasize that for the modern state, it was done for the equilibrium *simulations (Brovkin et al., 2007), but we will do it for the transient simulations as well.*

At the very least, we need to see the equivalent modern and LGM data plotted on top (filled circles, taking the same scale as for the modern, and plotted at 0 and 21 ka say every 500 m would be fine). Overlaying the wt% contours from e.g. Farrell and Prell [1989] could also be done. Figure 3 exhibits other important features that can be contrasted with data-based estimates. For instance: there is an apparent 1.6 km deepening of the wt% CaCO3 contours between Stage 5e and 2. Assuming that the CCD follows a similar pattern: is such a deepening 'realistic' (consistent with observations)? Associated with this – it is interesting to note that by 0 ka, only partial (if any) 'recovery' of the wt% CaCO3 contours has occurred compared to the LGM. As a consequence of the post LGM reorganisation of Atlantic circulation, driving higher CaCO3 deposition in the Atlantic and lower in the Pacific (to balance), increasing CaCO3 dissolution in the Pacific and hence presumably shoaling of the CCD and lysocline – there would be expected to be adjustment still occurring today which is consistent with both model and observations.

As responded above, we will plot wt% CaCO3 for the modern and the LGM times slices between the model and data and discuss how consistent with observation a deepening of CCD is.

The authors could say more on this and non steady-state issues in general. It is also interesting to note that in the model Equatorial Pacific, there is very little apparent difference between LGM and modern wt% CaCO3. This (surprising) lack of significant differences also comes out in the data. It would be helpful and enlightening if the authors could describe a little more about what the model predicts and why and how it fits (or not) with observations.

We will discuss the non steady-state effects in the revised manuscript. However, it is difficult to answer why the model fits or not with observations in fully coupled simulations where several mechanisms operating on different time scales are involved.

Similar to my comments on Figure 3 – here is another example of model projections that could and should be challenged with the data. For instance – there are (deglacial) time-series reconstructions for Δ[CO3] (e.g. from Zn/Ca) that could be overlain. One of the co-authors of this paper (David) has also previously worked on glacial vs. interglacial reconstructions of Δ[CO3] – reconstructed depth profiles for Atlantic and Pacific basins data could also be helpfully overlain. (And there are other, more recent, data examples that might be considered as well or instead.) It would then help in the model-data comparison to plot both panels as Δ[CO3] rather than [CO3]. This figure as great potential combined with the data, but as it stands, fails to convey a sufficiently useful message or insight.

In our view, it does not make sense to compare the coarse-resolution model with particular site records because of the resolution issue. We can compare model results only with large-scale data syntheses. Besides, each proxy for Δ[CO3] has its own complications in interpretation. There is a work in progress by Fehrenbacher et al. on multi-proxy reconstructions of [CO3] for the Last Glacial Maximum, but it is not yet finalized. We will enlarge discussion of comparison of simulated and reconstructed Δ[CO3], but we are unsure about a really useful figure on the model-data comparison.

With regards to how the model is configured and forced, there are a couple of points that need airing: 1. First – even if Fe is not explicitly included in the model and hence the relationship between changes in dust flux and marine productivity is highly parameterized, the forcing should still be dust flux not ice-core concentration, as applied here.

We agree that the forcing should be dust flux or, more precisely even, the iron flux over the Southern Ocean and it is not obvious how good a proxy for this characteristics the dust flux over Antarctica is. Since we use here a rather crude approach for taking iron fertilization into account, we ignore the difference between dust concentration and dust flux (not available for EPICA ice core) because significant dust concentration (flux) only occurred during MIS4 and MIS2 when temperatures and, therefore, accumulation *rates were comparable. For the future experiments, we are planning to get rid of using Antarctic proxies for the Southern Ocean dust flux and we will instead use an interactive dust cycle model (Bauer and Ganopolski, 2010) which directly provides the dust flux at each model grid cell.*

2. I have some concerns about: “only important difference is that the background vertical diffusivity in the ice free Southern Ocean south of 50°S was enhanced by an order of magnitude, i.e. to ca. 10−3 m2 s−1 (under the sea ice, the standard values of 10−4 m2 s−1 was retained).” Firstly – it needs clarification that the total area subject to enhanced diffusivity changes over the glacial-interglacial cycle (at least this is what I assume). Hence with more extensive sea-ice cover at the last glacial, the total area of ocean with enhanced diffusivity and exposed to the atmosphere (not sea-ice covered) would be reduced. If so, and to be provocative – have the authors not simply made themselves a version of the sea-ice lid mechanism (e.g. Stephens and Keeling [2000])? Should the lower latitude boundary of enhanced diffusivity not in fact shift to latitudes lower than 50°S as sea-ice extent expands? What is the physical justification for pinning northerly limit?

Firstly, the main motivation to enhance vertical diffusivity in the Southern Ocean was to get a realistic present-day net carbon flux in this area. As we stated in the paper, in the standard version of CLIMBER-2 (and similarly in other models), the net carbon flux south of 50°S is close to zero while in reality (according to estimates) it is about 0.4 Gt/yr. This discrepancy is most likely related to the use of a horizontal diffusion scheme while in this region the vertical component of isopycnal diffusion is important. The motivation for including the sea ice area in this parameterization is to account for a strong stratification below the sea ice which results from the melting of the sea ice and from the lack of vertical mixing as a result of wind waves braking. However, since the reviewer is suspicious about this approach, we will perform a detailed analysis of the importance of this parameterization for the results presented in the paper. As far as the Stephens and Keeling (2000) mechanism is concerned, please notice that it is already

included in the standard version of CLIMBER-2, since atmosphere-ocean carbon flux is proportional to the ice-free ocean surface. However, as shown by Archer et al. (2007), in the standard CLIMBER-2 version the “sea-ice lid” mechanism does not work.

3. Associated with (2), we need 3 additional pieces of information associated with the vertical diffusivity parameterization – firstly, we need to see the time-series of sea-ice extent projected in the model (there are other points in the text (see below) where this information would be helpful to have included in the main paper as a figure). Given the apparent importance of this change to the model, I would suggest the addition of a figure containing a panel showing the spatial patterns of sea-ice extent for modern and last glacial to give the readers a much better feel for what is going on, and one panel of time-series of wintertime and summertime limits and/or areas.

The second piece of information that ideally should have been provided is: given the change in parameterization to better match “recent empirical estimates” of CO2 outgassing – how does the model now perform w.r.t. standard model evaluation metrics such as CFC and anthropogenic CO2 uptake, deep ocean radiocarbon ages? Models, including CLIMBER, have rightly been previously carefully assessed against modern observations and inter-compared with other models. But whenever significant (really, for truly transparent science: any) changes to the physics or biogeochemistry are made – these evaluations needs to be repeated and the results presented (or summarized) in the literature. As it stands (and again to be provocative) – are you are now in effect using a model with no established credibility for the modern carbon cycle? I am not recommending that a full (re-)evaluation needs be included in the paper, just highlighting the issue. (The authors might note that the EGU journal GMD exists to facilitate open and transparent model descriptions and evaluations and is designed for the model to be ‘alive’ and further developed and (re-)evaluated as parameterizations are changes, bugs fixed, etc.)

Thirdly and lastly: the authors need to clarify whether the ocean diffusivity change is included in the physical climate simulation or is just restricted to the ocean carbon cycle. Obviously there is a potential issue if different physics were used in the two offline climate and carbon cycle simulations. (If so: what is the effect on the physical climate simulation of changing the ocean physics?)

Firstly, the answer to the last question. Enhanced diffusivity was only applied to the passive tracers. Although the effect of enhanced vertical diffusivity on active tracers (temperature and salinity) is not very large, we prefer to keep the physical component of the model unchanged to avoid inconsistencies with our previous results. In other words, the climate component of the CLIMBER-2 is the same as that used in our previous studies and the sea ice area evolution at present and at the LGM has already been shown in Brovkin et al. (2007). At the same time, we fully agree that because changes were introduced in the carbon cycle component, it is a good idea to present benchmark tests for the present day conditions. This will be done in the revised version of the manuscript.

Finally, ideally I would liked to have seen one additional main experiment – with no prescribed radiative forcing, i.e. with the model forced solely by orbital variations (and dust changes). This would illustrate how sensitive the climate simulation is to CO2 and in turn how sensitive CO2 is to climate changes – i.e., it would give us information about the feedback between CO2 and climate over a glacial-interglacial cycle (in the model), Plotting: e.g. the projected CO2 variability as an anomaly or better, normalized to the CO2 change projected in the baseline case, would further illustrate if and how the strength of the feedback varies with time (and hence in climate (and carbon cycle) space). (Obviously the time-dependence changes in N2O and CH4 radiative forcing would still have to be prescribed as tracers absent from the model.) Is there any series technical barrier to even an asynchronous coupling between climate and carbon cycle simulations?

We have discussed this suggestion with Andy Ridgwell offline (it was too late for the open discussion). Suggested experimental setup is in development phase. In this simulation, radiative forcing of atmospheric CO2 calculated by interactive carbon cy-

cle model drives the physical climate system. This experimental setup requires more model development, including improved land carbon model, and detailed discussion of climate-carbon cycle feedback in the model. This does not fit into the current manuscript and will be presented in a future paper.

Actually, I have one final question. On page 1773; Lines 1-3: I am stumped here – surely atmospheric pCO2 can be simulated in CLIMBER? What exactly do the authors mean by: “CO2 calculated from Eq. (1) (using a conversion factor of 0.47 ppm/GtC) was analysed in a diagnostic mode” (my emphasis)? Are all the pCO2 results presented in the paper not actually simulated directly, but ‘diagnosed’? Alarm bells are ringing loudly from this wording, but I assume it is a false alarm – atmospheric pCO2 is being calculated interactively with ocean, sediment, and terrestrial carbon cycling, primarily by solving air-sea gas exchange every time-step, augmented by carbon exchanged with the terrestrial biosphere and minus weathering (and plus CO2 out-gassing?). Yes?

Again, we clarified this point offline: Yes. The carbon is exchanged interactively among all carbon pools. The clumsy term "diagnostic" was for the fact that the physical system was driven by reconstructed CO2, while carbon cycle was fully interactive. We’ll explain this better in the revised paper.

Minor comments thoughts

Page 1772; Lines 18-20: Why did you change the land carbon parameterization? Is there a justification independent of the ‘results’ you might like to see (i.e., an improved glacial-interglacial simulation)? What effect does this change have on the modern carbon cycle – is the simulation quality of soil carbon stocks better or worse compared to observations (or whatever sparse data passes for an observational constraint at high Northern latitudes)?

We’ll discuss the effect of land parameterization in the revised manuscript. The accumulation flux of carbon at high latitudes is too small at present to modify present-day *budget significantly. We are working on extra module for peat accumulation in high land resolution CLIMBER-LPJ model, see Kleinen et al. (2011)*

Page 1776; Lines 6-12: This is interesting and important stuff – please could you make a little more of it.

We will extend the discussion appropriately.

Page 1779; Lines 10-12: 'Brine rejection' may conjure up interpretations that the authors do not intend – i.e., this is not 'brine rejection' as per Bouttes et al. [various papers]? Please clarify what happens to the salt rejected during sea-ice formation (I assume it is simply added to the surface box, and enhanced convection may or may not occur as a result.)

Brine rejection was treated as surface salinity flux and did not contribute to carbon fluxes (unlike done by Bouttes et al., 2011a,b).

Page 1779; Lines 12-13: Please quote numbers for model-projects and data-based salinity changes.

We will consider this in the revised manuscript.

Page 1779; Lines 24-28: Note analysis on AMOC changes and the cascade of difference carbon cycle and CO2 uptake changes that this induces, by Chikamoto et al. [2008] (Response of deep-sea CaCO3 sedimentation to Atlantic meridional overturning circulation shutdown, JGR 113, G03017, doi:10.1029/2007JG000669). Also relevant to the discussion on: Page 1782; Lines 3-10.

We'll take it into account, thanks.

Page 1780; Lines 1-9: The difference between CLIMBER used here and as modified by Bouttes et al. [2011a,b] is critical as to how low glacial CO2 is obtained in each case. This comparison and discussion needs to be made much more of. Perhaps central to the alternative explanations and distinguishing between them and form reality is what

you project for deep ocean dissolved oxygen changes. My understanding of application of the 'brine rejection' mechanism is that substantial areas of the ocean floor tend to go dysoxic or even anoxic, contrary to what we think we understand (however imperfects and qualitatively at best) about conditions during the last glacial. What is the situation in CLIMBER as used here? A Figure, analogous to 3 and 4 could usefully be added showing how oxygenation evolves as a function of time (and depth).

The oxygen plot will be done.

Page 1781; Lines 17-23: Here is an example of where the addition of a figure illustrating sea ice extends (modern vs. LGM) and how the sea-ice limits vary with time would be extremely useful.

See our response above.

Page 1781; Lines 24-29: What are the authors' views on the potential for (transient) deepwater formation in the N Pacific? This seems to be rapidly becoming a topical point of contention. Does CLIMBER make intermediate water at any time? It is outside the scope of this current paper: but what would it take to make N Pacific deep-water in CLIMBER?

In our simulations, there is no deepwater formation In the North Pacific over the whole glacial cycle. Moderate deep water formation with the associated PMOC can be achieved, for example, by a reduction of the freshwater flux in this area. We made such an experiment and found a rather modest impact on atmospheric CO2.

Figure 2: It might help summarize what the model is doing, to delineate the intervals during which particular mechanisms dominate (in addition to including the time-series for different combinations of mechanisms in panel d).

We'll try to consider it in the revised paper, although we prefer to avoid making that figure too complicated.

Figure 3: [See comments earlier re. data comparison]

Figure 4: [See comments earlier re. data comparison]

Figure 5: ‘purple” line? Maybe my toner is running out . . .

Pink line, sorry.

Figure 6: This figure would be improved by overlying contours for the glacial and interglacial overturning stream-functions as this will be much the more familiar metric for understanding circulation matters as compared to ‘dye’ concentrations.

This is a good idea. Will be done.

References

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