

## ***Interactive comment on “Glacial CO<sub>2</sub> cycle as a succession of key physical and biogeochemical processes” by V. Brovkin et al.***

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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 CO<sub>2</sub> 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 CO<sub>2</sub>. There are some interesting findings on how a non-steady state analysis of glacial CO<sub>2</sub> is important.

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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).

\*\*\* Primary criticisms/suggestions \*\*\*

The sedimentary (/weathering) response is central to the authors' simulation of CO<sub>2</sub> 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% CaCO<sub>3</sub> 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% CaCO<sub>3</sub> for the global ocean (from which the average vs. depth for the 30DEGREES-30EGREEN latitude band in the Pacific could be extracted), no observations are provided here as a point of (essential) comparison. 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% CaCO<sub>3</sub> 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% CaCO<sub>3</sub> contours has occurred compared to the LGM. As a consequence of the post LGM reorganisation of Atlantic circulation, driving higher CaCO<sub>3</sub> deposition in the Atlantic and lower in the Pacific (to balance), increasing CaCO<sub>3</sub> dissolution in the Pacific and hence presumably shoaling of the CCD and

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lysocline – there would be expected to be adjustment still occurring today which is consistent with both model and observations. 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% CaCO<sub>3</sub>. 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. 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 DELTACO<sub>32</sub>- (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 DELTACO<sub>32</sub>- – 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 DELTACO<sub>32</sub>- rather than CO<sub>32</sub>-. This figure has great potential combined with the data, but as it stands, fails to convey a sufficiently useful message or insight.

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.

2. I have some concerns about: “The only important difference is that the background vertical diffusivity in the ice free Southern Ocean south of 50EGREES was enhanced by an order of magnitude, i.e. to ca. 10–3 m<sup>2</sup> s<sup>-1</sup> (under the sea ice, the standard values of 10–4 m<sup>2</sup> s<sup>-1</sup> 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

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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 50EGREES as sea-ice extent expands? What is the physical justification for pinning northerly limit?

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 CO<sub>2</sub> out-gassing – how does the model now perform w.r.t. standard model evaluation metrics such as CFC and anthropogenic CO<sub>2</sub> 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 need 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 dif-

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fusivity 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?)

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 CO<sub>2</sub> and in turn how sensitive CO<sub>2</sub> is to climate changes – i.e., it would give us information about the feedback between CO<sub>2</sub> and climate over a glacial-interglacial cycle (in the model), Plotting: e.g. the projected CO<sub>2</sub> variability as an anomaly or better, normalized to the CO<sub>2</sub> 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 N<sub>2</sub>O and CH<sub>4</sub> 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?

Actually, I have on final question. On page 1773; Lines 1-3: I am stumped here – surely atmospheric pCO<sub>2</sub> can be simulated in CLIMBER? What exactly do the authors mean by: “Atmospheric CO<sub>2</sub> calculated from Eq. (1) (using a conversion factor of 0.47 ppm/GtC) was analysed in a diagnostic mode” (my emphasis)? Are all the pCO<sub>2</sub> 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 pCO<sub>2</sub> 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 CO<sub>2</sub> out-gassing?). Yes?

\*\*\* Minor comments & thoughts \*\*\*

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\* 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)?

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

\* 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.)

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

\* Page 1779; Lines 24-28: Note analysis on AMOC changes and the cascade of difference carbon cycle and CO<sub>2</sub> uptake changes that this induces, by Chikamoto et al. [2008] (Response of deep-sea CaCO<sub>3</sub> 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.

\* Page 1780; Lines 1-9: The difference between CLIMBER used here and as modified by Bouttes et al. [various papers] is critical as to how low glacial CO<sub>2</sub> 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

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imperfections 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).

\* 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.

\* Page 1781; Lines 24-29: What are the authors' views on the potential for (transient) deep-water 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?

\* 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).

\* 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 . . .

\* 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.

SEE SUPPLEMENT PDF FOR A RATHER MORE 'ACCEPTABLY'-FORMATTED VERSION OF THE REVIEW ...

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/7/C1329/2011/cpd-7-C1329-2011-supplement.pdf>

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Interactive comment on Clim. Past Discuss., 7, 1767, 2011.

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