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Climate of the Past Discussions

# *Interactive comment on* "Simulation of climate, ice sheets and $CO_2$ evolution during the last four glacial cycles with an Earth system model of intermediate complexity" *by* Andrei Ganopolski and Victor Brovkin

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This modelling study is truly fascinating and contains a wealth of hypotheses and information to explore. This represents a major strength of what is a very interesting paper of course, but it also presents something of a challenge, not least with respect to maintaining complete clarity on all possible points that readers might wish to see explored and explained. I believe this to be a valuable and informative contribution that should eventually be published in CP; however, I provide some comments below, which I hope may prove helpful in revising the manuscript prior to publication.



My most general comment is that that study appears to focus overly on the 'success' of the numerical model simulations (and therefore the apparent success of the many model *choices* that have been implemented), rather than the justification or otherwise of the choices that have been made, for example as attested to by proxy data. In other words, it may well be that a viable 'recipe' for glacial-interglacial CO2 has been devised, but how do we know it is the right one? Arguably the only way to explore the latter question is to compare the biogeochemical/physical 'fingerprints' of that recipe with proxy data. My feeling is that more could (and probably should) be done in this regard, in particular with respect to carbonate chemistry, radiocarbon, oxygen and nutrient distributions/trajectories. Indeed, I would suggest that even if proxy data are too sparse to comprehensively test the particular 'CO2 recipe' that is adopted in this study (or if it is too much work to compile the data needed for this, since arguably this could be beyond the scope of this initial study), it should still be possible to identify its 'biogeochemical fingerprints' so that eventually the recipe we are being offered can be tested by others. Without this we are left without the means of assessing whether or not the CO2 recipe in this study is not only viable, but also possibly correct. I would propose that three specific parameters to possibly consider in more detail are: radiocarbon, carbonate chemistry and oxygenation/respired carbon. Of these, radiocarbon and carbonate chemistry offer the best opportunities for data-model comparisons. I return to these suggestions below.

Specific comments:

1. The abstract states that the co-evolution of climate, ice-sheets, and carbon cycle have been simulated over 400,000 years using insolation as the only external forcing. This is an impressive feat, and the reader wonders how this has been achieved of course; what are the key processes and feedback loops at the heart of the long-standing 'mystery of the ice ages'? It would be helpful if the abstract summarised the authors proposal succinctly. More specifically, it seems that a successful simulation of climate, ice volume and atmospheric CO2 has been achieved by appropriately

scaling the rate of change of atmospheric $CO_2$ to ice volume (using parameterizations for iron fertilisation and volcanic $CO_2$ outgassing), and by further implementing additional climate-carbon cycle feedbacks that operate primarily through temperature-dependent respiration rates in the ocean, marine $CO_2$ solubility effects and ocean circulation changes. The extent to which the phenomena have a been implemented as modelling choices, and the extent to which the magnitude of their impacts (e.g. on $CO_2$) depends on parameter choices, should be made clear.

2. The abstract focuses on the deglaciation as being particularly sensitive to parameter choices, apparently in contrast to the rest of the glacial cycle (for which many features are argued to be 'rather robust'). I feel that this might be a little misleading; can the meaning of this statement be clarified? In what sense exactly can modelled features be said to be robust?

3. The issue of $CO_2$ overshoot: this is highlighted in the abstract as a key finding, but it needs to be explained more fully I think. Why exactly does this phenomenon occur? Does it depend on model choices and if so which ones, or is it a fundamental aspect of the physics in the model? It would appear that the AMOC is sensitive to freshwater forcing throughout the deglaciation, but that AMOC anomalies early in the deglaciation (and during the glacial?) have no appreciable carbon cycle impact; why is this? Is marine soft tissue pump efficiency 'maxed out' (exhausted) and therefore insensitive to further enhancement until the parameterizations for increased Fe-fertilisation and nutrient respiration rate are released? More explanation is needed for this phenomenon, especially if it is highlighted as being particularly noteworthy.

4. Page 4, Line 11: the way in which iron fertilisation is implemented needs to be clarified. How is exactly is nutrient utilisation scaled with dust and on what basis? How do we know that the right scaling has been applied, or is it essentially arbitrary? Can the scaling be justified on the basis of nitrogen isotopes (simulated) or anything else? Without such details the iron fertilisation mechanism will always seem like a sort of 'magic bullet' for drawing down carbon into the ocean.



5. Page 4, Line 23: the implementation of radiocarbon in the model should be explained a little more clearly too (e.g. is it simulated as an isotope tracer that undergoes gas exchange, fractionation etc... or is it a pseudo-tracer with a decay timescale that is restored to a particular value at the ocean surface?). Note that Hain et al. (2014) did not produce a radiocarbon production scenario; please check this reference (ultimately the production scenario will be based on Be-10 or geomagnetic field strength and the original references should be cited). In general I think that more should be made of the radiocarbon outputs, e.g. in comparison with existing data. Such data should be added to figure 12 for example, and any agreement/disagreement discussed. I return to this later.

6. Page 5, Line 5: notably this way of doing greenhouse gases will produce incorrect results for millennial timescales, since methane and $CO_2$ are not in phase during D-O/Heinrich events. Does this matter; can it be shown that it does not matter?

7. Page 5, Line 22: can this careful calibration of volcanic outgassing be tested against e.g. atmospheric $d13CO_2$ for example (note that these data are available for the last glacial cycle from Eggleston et al. 2016)? If the volcanic control on atmospheric $CO_2$ is so strong, it might also be expected to affect the isotopic composition of the atmosphere quite strongly (as well as the deep ocean carbonate system - more on this later). Is the surface (i.e. non-solid Earth) carbon cycle balanced; i.e. is 5.3TmolC/yr going back into the solid earth in the model? All of these are important questions that jump out at the reader, but are not dealt with at all in the current manuscript.

8. Page 5, Line 30: this procedure for 'initial condition conditioning' is very interesting, but it is not so obvious why the system should converge on the same initial and final states, regardless of the history of evolving boundary conditions over 410kyrs; is it possible to clarify? What component is drifting that depends on the state of the system (and that eventually reaches an equilibrium through this iterative process)?

9. Page 6, Line 30: the lag of $CO_2$ is an important clue as to what is (perhaps) not

right in the model parameterizations that have been selected. One wonders if this has something to do with the choice to scale iron fertilisation with ice volume: dust does not track sea level very closely in reality, and more specifically it drops off rapidly before sea level has risen much in the deglaciation. Or is the lag due to something else? More analysis of the source of this mismatch would be illuminating (more illuminating than if the model happened, perhaps accidentally, to match observations perfectly).

10. Page 6, Line 32: as noted above, a more detailed explanation is necessary for the mechanisms underlying the overshoot in CO2, and for CO2 release as a function of AMOC variability in general. There is not universal agreement amongst models for millennial scale controls on atmospheric CO2 and the role of the AMOC, so it will be useful to know what is going on in this particular model experiment, and why the carbon cycle response to AMOC changes is so context dependent. On page 9 it is suggested that the CO2 overshoots depend primarily on remineralisation depth changes that in turn stem from subsurface heat anomalies, but this is not clearly stated or explored anywhere else.

11. Page 7, Line 20: again, this lag, and it's increase in the fully coupled runs is important, and should be diagnosed more clearly, as it is telling us something important about the model choices that have been implemented.

12. Page 8, Line 4: it is very interesting and important that CO2 changes on a dominantly 100ka timescale are not needed to produce glacial cycle sin the model, but where does the 100kyr timescale for ice sheet growth/decay come from in this model; is it simply the timescale at which the ice sheets get big enough for the dirty-ice albedo instability to kick in? If so, how is that feedback constrained (is the time scale a model choice once again or is it due to a fundamental limitation on ice growth rates and basal sliding etc...); how do we know it should happen on that timescale?

13. Page 8, Line 24: can it be stated that the 'better' performance of the enhanced freshwater flux experiments indicates an under-representation (or misrepresentation)



of the role of ocean circulation perturbations, at glacial transitions in particular? Is it possible that it could also be that this enhanced forcing is needed to compensate for other biases, e.g. from iron fertilisation or volcanic CO2 parameterizations? How would we know, what do we learn from this?

14. Page 9, Line 1-7: it would be helpful to include a table that clarifies the 'carbon stew' and the contribution of each mechanism that is implemented, e.g. based on average glacial and interglacial values.

15. Page 9, Line 14: the original references of Matsumoto (2007) and Matsumoto et al. (2007) are missing here, and where the notion of temperature dependent respiration rates is introduced.

16. Page 9, Line 25: some more detail on the volcanic CO2 implementation is needed; what about the balance of marine versus sub-aerial volcanism, and their different responses to ice vs water loading; how is this treated and on what basis is a particular magnitude of volcanic CO2 flux chosen? More justification/testing of the volcanic CO2 implementation is also needed; what is the impact on marine carbonate chemistry and does this tally with proxy evidence (it should cause marine carbonate ion concentrations to go up in the glacial, at odds with data from the Atlantic where it goes down, and the Pacific where it stays pretty constant)? Is there a longer-term feedback via carbonate preservation; are changes in volcanism perfectly balanced by weathering and sedimentary carbon outputs in the model, and if not what is compensating for the drift in global 'surface' carbon inventories that would result from this? Also, as noted above, please state what the impacts of the changing volcanic carbon fluxes on atmospheric carbon isotopes are: are they essentially nil?

17. Page 10, Line 11: it is stated that the brine rejection parameterization cannot be tested with observational data, but is this entirely true/fair, especially given the lack of testing offered in this study for the volcanism and temperature dependent respiration rate mechanisms? A critical analysis of all key modelling choices should be provided;

not just for brine rejection.

18. Page 11, on deglacial d13Catm: The text gives the impression that the deglacial d13Catm tends are quite accurately reproduced, but the match is not great. The 'W' in deglacial d13Catm is not particularly clear; what is this mismatch attributable to? Does it mean that the model is not simulating the correct marine carbon cycle response to AMOC change? Also, why is the more substantial early Holocene d13Catm rise seen in available data not reproduced; does this mean that terrestrial carbon uptake is too small in the model? Do marine carbonate ion values confirm this latter possibility or not (or at least demonstrate that marine carbonate ion reconstructions could be used to test the model)? I think a great deal more should be made of the isotope simulations and their comparison with proxy data.

19. Page 12, on deglacial 14C: Even more so than for the stable carbon isotopes, I think that a great deal more should be done with the radiocarbon simulations and their comparison with observations. Figure 10 should really include data, as should Figure 12 (this could be made substantially easier to include by a recent compilation by Skinner et al., Nature Communications, 2017). Radiocarbon data provide very strong constraints on the ocean state; if the simulation does not fit the available data, some discussion is warranted. This relates to the following section, where it emerges that the model simulation not preferred by authors, using brine rejection as a stratification mechanism, produces radiocarbon data that better fit the data (though again, no direct comparison with data is shown).

20. Page 12 , Line 14: it is stated that the radiocarbon data are in good agreement with Roberts et al. (2016); however that publication did not present radiocarbon data. Please correct the reference and/or clarify.

21. Page 12, Line 28: if the preferred model simulation does not fit the radiocarbon observations, does this not mean that the "CO2 stew" proposed in the manuscript must not be completely accurate? Please clarify.



22. Page 12, Line 30: in the manuscript DD14C is used as the preferred ventilation metric; however, this metric does not scale with the isotopic disequilibrium between two reservoir in a constant manner. In other words, a given DD14C value will reflect a different degree of isotopic disequilibrium (or ventilation age) depending on the absolute D14C. This not only makes DD14C a particularly confusing metric, but it also means that simulated DD14C values can match observed values without being correct if the absolute atmospheric/marine D14C values are too high/low. This indeed seems to be the case here, as the simulated D14Catm at the LGM is $\sim$150 permil lower than observed. For these reasons I would urge the authors to use marine vs atmosphere radiocarbon age offsets (B-Atm), which can also be converted to a ratio of isotopic ratios (or F14b-atm, Soulet et al., 2016) if a semblance of 'geochemicalness' is required.

23. Page 13, Section 5.3: can the authors state clearly what the implications are, if there are any, for marine and atmospheric carbon isotopes (13C, 14C) of the terrestrial carbon shifts, e.g. at the last deglaciation? It has been proposed that parts of the observed deglacial 14Catm record might be explained by permafrost changes; do the model results support a significant impact on deglacial atmospheric radiocarbon (or d13C)?

24. Page 13, Line 33: "..the model simulates the correct timing of glacial terminations..." I would suggest to be more precise (e.g. ice volume, but not CO2?), and perhaps to quantify this as being within a certain (millennial?) margin of error.

25. Page 14, Line 2: "...ocean carbon isotopes evolution is in agreement with empirical data." Should stable carbon isotopes be specified; should the statement be qualified somewhat (e.g. global spatial patterns have not been matched.. and the fit is assessed only in very general terms)?

26. Page 14, Line 3: should this read "the magnitude of atmospheric 14C change is underestimated"? And on Line 5, I would say that the statement regarding disagreement with data has not really been backed up very strongly as there is no illustration of

a comparison with data in the manuscript.

27. Page 14, Line 10: I think that some more explanation is required for what is meant by 'robust' in this context.

28. Page 16, Line 25: as noted above, the scaling of iron flux with sea-level is arguably questionable, since although dust fluxes in Antarctica increase relatively late, when sea level has fallen and CO2 has already dropped somewhat, it is also true that dust fluxes drop off very quickly on the deglaciation, before sea level as risen appreciably. Does this not mean that the 50m RSL threshold for dust changes is somewhat incorrect (i.e. it has the effect of keeping iron fertilisation strong for too late in the deglaciation)? A plot of how the timing of dust/iron fluxes in the model compare with the timing of dust fluxes in Antarctic ice cores might provide a test of this. I would suggestion including such a figure as a justification of the chosen parameterization. Again, I think that a clear description is needed for how export production is scaled to dust fluxes in the model, and on what basis the chosen scaling is justified (it would be nice to know what the Southern Ocean and global export productivity is in the model on average for glacial and interglacial states). How is iron release from dust simulated, how is biological activity as a function of iron availability simulated etc..? I think that a clear description of how biological carbon fixation/export is linked to dust fluxes should be included in the appendix.

29. Figure 4: atmospheric d13C data for the last glacial cycle and deglaciation should be added, including e.g. Eggleston et al. (Palaeoceanography, 2016).

30. Figure 7b: perhaps add the power spectrum for a appropriate insolation record, as a dashed line?

31. Figure 8: I personally would find it useful if the plots b-e were drawn as filled curves, either side of the zero line, so that it was clear when each process was acting as a source or sink for CO2.



32. Figure 9: I think this figure would benefit from adding a comparison between simulated and observed marine radiocarbon ventilation ages some key locations/regions. It may provide insights into why the atmospheric simulations do not match the observations.

33. Figure 10: why do the plots only go to 40oS? I think this figure would greatly benefit from added data comparison. For this it would be essential to convert the radiocarbon activities to radiocarbon age offsets or radiocarbon ratios (i.e. not relative deviation offsets).

34. Figure 11: probably it would be good to add an indication of what the green line is (even though it is obvious by process of elimination). Does the brine rejection experiment not include freshwater pulses during deglaciation; why does it not exhibit any deglacial anomalies at all? Again, data might usefully be added to the figure for comparison.

35. Figure 12: this figure is the most obvious one in which to include a comparison with observations, along with an addition to the text of a discussion of any mismatches between the various experiment outputs and the observations. It seems to me that if the simulation does not fit the data, then something is amiss, which we might learn from if it was identified.

36. Figure A1: What are the different coloured substrates? Perhaps more can be done with this figure?

I hope the above comments are useful to the authors; their quantity is testimony to how interesting this study is. I add below some purely editorial/grammatical suggestions that I hope the authors may also find useful:

1. Page 3, line 30: The ice sheet model is only applied...

2. Page 4, line 3: As shown in Ganopolski and Roche (2009), temporal dynamics.... in CLIMBER-2 are very sensitive... of freshwater flux to the North Atlantic.

3. Page 5, line 16: ...multiple glacial cycles represents a challenge..

4. Page 7, line 16: In particular, in the fully...

5. Page 8, line 11: ..faster ice growth during the initial part... very sensitive to ice volume.

6. Page 9, line 1: ...in our simulations they counteract glacial $CO_2$ drawdown... since terrestrial carbon contains ca. 350 Gt less carbon at the LGM...

7. Page 9, line 16: I would cite Matsumoto (2007) here...

8. Page 10, line 15: ..which is at odds with reality. This means that to be an efficient mechanism for ...at least by an order of magnitude.

9. Page 10, line 20: is there a paper by Miller et al. that would be appropriate here?

10. Page 11, line 8: note the need for Danish letters in Bolling-Allerod.

11. Page 11, line 18: ...maximum at the end of the North Atlantic cold event, which... ?

12. Page 11, line 22: ...at the beginning of the interglacial followed by... At the same time an earlier AMOC recovery causes only a temporary/brief... ?

13. Page 12, line 6: ...An alternative hypothesis..

14. Page 12, line 33: I would propose to cite more than just Freeman et al. (2016), since these authors only presented new data from the low-latitude and Northeast Atlantic.

15. Page 13, line 7: ..very old (likely to be at odds with palaeoclimate data)...

16. Page 14, line 2: ...ocean stable carbon isotopes...

17. Page 14, line 12: ..decreases the amplitude of glacial-interglacial...

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