General response

We thank both referees for useful and constructive comments and suggestions. We performed significant revision of the manuscript following most of reviewers suggestions. In particular:

1) We add one new figure (fig. 5 which show oxygen concentration in the Southern Ocean) and modified seven other figures. In particular, we added data for the atmospheric ^{13}C and oceanic ventilation age. Now, following reviewer recommendation, we consistently use radiocarbon ventilation age instead of $\Delta\Delta^{14}C$.
2) We now described the carbon cycle model in more detail and provided all critical references.
3) We described (in the Appendix) parameterization of iron fertilization effect.
4) We discussed the mechanism of CO_2 overshoot and its relation to the AMOC.
5) We compare our results with the results from Skinner et al. (2016)
6) We discussed our approach to the carbon "stew" and the limitations of our model.
7) We added more than 15 new references.

Below is our response (plane) to the referees comments (*blue italic*) and what has been done (purple). Hereafter we will refer to our manuscript as GB17.

Both referees raised a number of questions primarily related to our choice of the "carbon stew". Reviewe#1 explicitly stated that he/she believe that influence of physical processes is underestimated in our model while iron fertilization effect is overestimated. Referee#2 questioned our stew more implicitly by asking question *"how do we know it [recipe] is the right one?"* and also suspected that in our model the iron fertilization is *"a sort of 'magic bullet' for drawing down carbon into the ocean"*. Indeed, the choice of the "carbon stew" is important for successful simulations of glacial cycles but only one among many other critical "modeling choices". The aim of our paper is not to present the ultimate solution for the "carbon stew" problem since at present this is simply impossible. The aim of our paper is to demonstrate that with a reasonable representation of physical, geochemical and biological processes in the model, it is possible to reproduce main features of Earth system dynamics over the past 400 kyr, including the magnitude and timing of climate, ice volume and CO_2 variations. The key world in the previous sentence is "reasonable". In a number of previous publications we have demonstrated that, in spite of its relative simplicity and coarse spatial resolution, CLIMBER-2 has a reasonable climate sensitivity (3^{o}C) and its spatial and temporal patterns of response to CO_2 and orbital forcing are in good agreement with the state-of-the-art climate models. Since both referees are concerned primarily about the "carbon stew", below we argue that our "carbon stew" is also reasonable and consistent with numerous studies published over the recent years.

Action: We added discussion of the uncertainties in carbon stew and justifications of our choice in the Section 4.

<u>The role of physical effects in glacial CO_2 drawdown</u>. CLIMBER-2 is a rather simple and coarse-resolution model compared to the state-of-the-art Earth system models. This however, does not imply that it should necessarily underestimate (or overestimate)

something. Unfortunately, Referee#1 did not explain why he/she believes that CLIMBER-2 underestimates contribution of physical processes to CO_2 drawdown and what is the correct value for this contribution. In Brovkin et al. (2007) we have shown that the net effect of the physical processes (solubility, ocean circulation, stratification, sea ice, but not changes in the global ocean volume and salinity) at LGM is about 45 ppm of CO_2 drawdown. This is not a small effect and we are not aware about results of 3-D ocean carbon cycle models which have much more. The last IPCC AR5 report summarized effect of different factors on glacial CO_2 and gave the median values of 25 ppm both for temperature and circulation effects. More recently, Buchanam et al. (2016) reported the total effect of temperature and circulation to be 40 ppm while Menviel et al. (2012) attributed only 20 ppm to physical processes. Kobayashi et al. (2015) found 45 ppm LGM drawdown, primarily through the physical processes. Thus we see no reason to assume that CLIMBER-2 underestimates effect of physical processes on glacial CO_2 drawdown. It has to be noted that the P-experiment, described in Brovkin et al. (2012), includes together with other physical processes also effect of the ocean volume change which counteracts other physical effects by ca 15 ppm. When 15 ppm are added at LGM to the results of P-experiment, the CO_2 drop at LGM becomes very close to 45 ppm reported in Brovkin et al. (2007).

Action: We clearly stated the magnitude of physical effect on atmospheric CO2 and compare it with recent publications of other authors.

Iron fertilization effect. We are surprised by the fact that both referees are so skeptical about importance of this mechanism. Since Martin's paper published 1990 (the paper was cited more 1000 times), the iron fertilization as one of plausible mechanisms of glacial CO_2 drawdown has been supported both by numerous modelling and paleoceanographic papers (e.g. Jaccard et al., 2016). As seen in Fig. 8c, at LGM the iron fertilization mechanism is responsible for ca. 25 ppm of CO_2 drawdown in our model. Note, that this number includes also effect of carbonate compensation. This value is well within the range of recent modelling estimates. For example, recent study by Lambert et al. (2015) attributed ca. 20 ppm to iron fertilization. Buchanan et al. (2016) attributed 55 ppm to the total change in biological pump. For comparison, if we sum up effects of iron fertilization and temperature-dependent remineralization depth, we arrive to less than 40 ppm. Schmittner and Somes (2016) used ^{13}C and ^{15}N isotopes to better constrain contribution of different factors to the LGM CO_2 drawdown. They came to the following conclusion: "Our results support Martin's [1990] hypothesis that increased iron input enhanced glacial ocean carbon storage by accelerating phytoplankton growth rates, consistent with previous studies [Bopp et al., 2003; Brovkin et al., 2007; Tabliabue et al., 2009]" and Galbraith and Jaccard (2016) arrived to a similar conclusion. Based on reasonable assumptions that are consistent with qualitative proxy evidence for the carbonate ion and oxygen concentrations, Anderson et al. (2015) concluded that about half of the DIC increase in the deep ocean during LGM had a respiratory origin.

Action: We compared our results for iron fertilization effect with other studies.

LGM time slice versus transient simulation of glacial cycles. Most of previous studies concentrated on explaining 80 ppm CO_2 drop at LGM. Although the LGM "carbon stew" still remain a hot topic, it became also clear that very different combinations of numerous factors can explain 80 ppm drawdown. This is why recent development, first of EMICs and now of complex Earth system models, offers a new opportunity to better constrain "carbon stew" by performing transient simulations over the entire glacial cycle or, as in our case, even several glacial cycles. As we have shown in Brovkin et al (2012), at different phases of glacial cycle, the relative role of different factors differs significantly. This is why a good match between simulated and observed CO_2, not only during LGM but during entire 400 kyr of simulation, gives higher confidence that our 'modeling choices' are reasonable. Referee#2 suggested that our '*success*' is almost solely explained by arbitrary tuning of iron fertilization effect which play the role of '*magic bullet*' in our model. This is obviously not true. Fig. 8d clearly shows that iron fertilization explains less than 10 ppm during 80% of the last 400 kyr. At the same time, the agreement between observed and simulated CO_2 during these 80% is at least as good as during 20% when iron fertilization plays more significant role.

The use of paleoclimate data to constrain the carbon stew. Needless to say that paleoclimate proxies are essential component of evaluating of results of paleoclimate modeling. However, we do not share optimism of Referee#2 concerning possibility to constrain tightly the "carbon stew" by available paleoclimate data. Numerous attempts to achieve that (including the most recent by Schmittner and Somes (2016) and Heinze et al. (2016)), show a rather limited success. One of the reasons is that the proxy data syntheses are in the state far from being perfect, with proxies telling contradicting stories, such as Mg/Ca and organic proxies (eg. alkenons) for SST reconstructions. In spite of that we always tried to use available paleoclimate information to compare with modeling results. In the manuscript as well as in Brovkin et al (2007 and 2012) we showed and analysed a large amount of oceanic and atmospheric characteristic such as atmospheric and oceanic ^{13}C and ^{14}C, oceanic oxygen, $CaCO_3$, etc.

Action: Follow the recommendations of the Referee#2 we made direct comparison of simulated and reconstructed oceanic ^{14}C and atmospheric ^{13}C

Success or "success"? In his most general comment Referee#2 put the word *success* in quotes. We believe the quotes are unnecessary since our work indeed represents an important step forward in modeling and understudying of Quaternary climate dynamics. This is the first ever simulation of the past glacial cycle with the fully interactive ice sheet and carbon cycle models forced only by the orbital forcing. One should realize that dealing with long-term carbon cycle dynamics (volcanism, weathering, sedimentation) with geographically explicit Earth system is a very novel and challenging task, so one should not expect perfect agreement between modeling results and data. In the manuscript we thoroughly discussed all significant mismatches between data and model. Still the agreement between model and CO_2 and global ice volume is reasonably good. For example, the correlation coefficients between simulated and modeling CO_2 is 0.86 in the one-way coupled experiment and 0.66 in the fully interactive. Root mean square errors (RMSE) are 13 ppm and 21 ppm respectively. For the last glacial cycle, for which model was calibrated, the agreement is even more impressive: correlation coefficients are 0.92 and 0.88 respectively; RMSE are only 11 and 13 ppm. Since the magnitude of stochastic millennial scale variability of CO_2 is about 10 ppm, such agreement is close to the upper theoretical limit.

Importance of our previous publications for understanding of GB17. Both referees complain that some important details of our modeling approach and analysis of mechanisms are not described in GB17. We will try our best to clarify as many issues as possible or to give proper references. However, it is important to realize that the manuscript presents results of the 20-years-long project and is based on a number of previous publications and it is both impossible and unnecessary to repeat things that we have published already. Fortunately, three of four the most relevant papers needed for understanding of GB17, namely Brovkin et al. (2012), Ganopolski et al. (2010) and Ganopolski and Calov (2010), are published in Climate of the Past and readily available for any potential reader. Only Brovkin et al. (2007) was published in Paleoceanography to which not everybody has free access.

Response to Referee #1

We thank the Referee #1 for useful and constructive comments. Please find our replies below.

Ganopolski and Brovkin simulate four Glacial/interglacial (G/IG) cycles with the model of intermediate complexity CLIMBER2 in both a fully interactive and 1 way coupled mode. In both set ups, the model is able to reproduce the major features of G/IG cycles: i.e. changes in sea-level, ice-sheet extent (and volume), atmospheric CO2. . . It is an interesting study, certainly worth publishing in Climate of the Past. My main comment would be that I don't find the goals or conclusions of the study very clear.

For a rather narrow community of fellow scientists striving to understand glacial cycles, the importance of successful simulation of glacial cycles with an Earth system model driven by orbital forcing alone is quite obvious. However, we agree that for a broader audience it is worth explaining why this problem is considered by some as the "holy grail" of paleoclimatology.

Action: We modified the conclusions to make our main messages and importance of the work more clear.

The manuscript tries to tackle various aspects of G/IG cycles but without going deeply in any of them.

We have commented on that in the general response.

The authors are rightly very careful in not over-interpreting or making hasted conclusions from their results because the model used is quite simple.

We fully agree that we used a rather simple model, although arguably the only one which is available at present for this sort of studies. However, it has to be noticed that complexity high resolution do not automatically resolve all problems because many processes in the Earth system are not yet properly understood.

1. The first part of the introduction suggests that the radiative role of CO2 in driving 100kyrs G/IG cycles is controversial. To explore this, a simulation with constant pCO2 (240ppm) is performed. It leads to G/IG variations with 50% full G/IG amplitude and with dominant periodicity of 40ka. To me, this would tend to highlight the dominant role of CO2 in driving 100ka cycles, but this result or its implications are not really discussed.

This is a misunderstanding. We do not downplay the role of CO_2 in amplifying of 100 kyr cycles. In Ganopolski and Calov (2011), the paper which is devoted to the nature of 100 kyr cyclicity, we wrote: "*the CO_2 concentration not only determines the dominant regime of glacial variability, but also strongly amplifies 100 kyr cycles*". What we stated in the introduction is that we do not consider CO_2 as the **driver** of 100 kyr cycles, as some other workers proposed. According to our theory, 100 kyr cyclicity originates from the nonlinear response of the climate-cryosphere system to the orbital forcing through phase locking of long glacial cycles to 100 kyr eccentricity cycle (Ganopolski and Calov, 2011). In turn, 100 kyr cycles are strongly amplified by CO_2. This result is consistent with the earlier findings of Andre Berger and colleagues.

2. CO2 changes: The study simulates full G/IG changes in pCO2 due to a combination of processes and in global agreement with previous studies. However, due to the relative simplicity of the model and its configuration (zonally-averaged basin), I would think that the impact on pCO2 of oceanic circulation changes, sea-ice and wind related changes are underestimated, while iron fertilization changes are overestimated.

This part of the comment we discussed in the general response.

3. In addition, I am a bit surprised not to see any mention of the impact of changes in the carbonate system (e.g. shallow water carbonate deposition). A few studies (see A. Ridgwell or F. Joos studies) have shown that this has a significant impact on pCO2 particularly at the end of the deglaciations (early interglacial) and thus also glacial inceptions.

The shallow water carbonate deposition is included in the CLIMBER-2 model which is described in Brovkin et al (2007) where we attributed to them 12 ppm of glacial CO_2 drawdown. We put more attention on this mechanism in our papers on interglacial simulations with CLIMBER such as Kleinen et al. (2016), Brovkin et al. (2016).

Action: We extended the model description part were we mentioned that changes in carbonate system, including shallow carbonate deposition, are accounted for in our model.

4. The authors highlight the impact of deglacial AMOC changes on the shape of the pCO2 trajectory at the end of the deglacial phase. This is an interesting aspect but:

i) Its reasons are not discussed in details

It is true that we did not discuss this mechanism in GB17 but in Brovkin et al (2012) we devoted the entire section 3.3 to the discussion of millennial-scale variability in atmospheric CO_2 during the AMOC shutdowns. Now we introduced additional mechanism – temperature-dependent remineralization depth – which also contribute to CO_2 to the AMOC changes but to a smaller degree than the mechanism described in Brovkin et al. (2012).

Action: We discussed the mechanism of CO_2 overshoot in more details in the Section 3.

ii) Can we really believe it given that the shape and timing of the deglacial CO2 changes are not represented correctly and some processes are likely missing or misrepresented (e.g. shallow water carbonate deposition, oceanic circulation changes).

We see no reasons why our results are not plausible. In fact, the shape and timing of glacial termination are not so bad, the shallow water carbonate deposition is accounted for and, as far as the ocean circulation changes are concerned, we know that at least for the LGM, CLIMBER-2 does a better job than many complex models (e.g. Weber et al., 2007; Muglia and Schmittner, 2015; Marzocchi and Jansenis, 2017). Indeed CLIMBER-2 correctly simulates shoaling of the glacial AMOC, decrease of deep Atlantic water ventilation and significant (above 1 psu due to continental ice sheets buildup) increase in salinity of the deep Southern

Ocean water masses, while most of PMIP3 models show the opposite response. Marzocchi and Jansenis (2017) attributed this problem, at least partly, to the fact that most GCMs significantly underestimate sea ice extent in the Southern Ocean at LGM. At the same time, CLIMBER-2 (see Fig. 3 in Brovkin et al. 2007) simulates both modern and LGM sea ice extent in good agreement with modern and paleo data.

As far as 10-20 ppm CO_2 rise due to shutdowns of the AMOC are concerned, similar CO_2 rise simulated also in other models (e.g. Schmittner and Galbraith 2008; Matsumoto and Yokoyama 2013; Menviel et al. 2014, etc.). And if some other models are unable to simulate such rise – this is their problem because 10-20 ppm CO_2 rise occurred in reality during most of Heinrich stadials and some non-Heinrich stadials.

Another important argument in favor of credibility of our finding is that it allows to understand why CO_2 overshoots coincide with strong overshoots in Antarctic temperature during MIS 5, 7 and 9, while during MIS 1 and 11 overshoots are absent both in CO_2 and Antarctic temperature records.

Minor. Changes in weathering and its impact on pCO2 are not very clear. I realize it is mentioned in Brovkin et al., 2012, but maybe a brief description might be useful.

Action: We mentioned that simulated changes in silicate weathering are small as have been shown already in Brovkin et al. (2012).

Figure legends: Please make sure all appropriate references for the proxy are included in figure legends. For example Antarctic dust in Figure 2, Figure 4c. . . Proxy for atm. d13CO2 could be included in Figure 4b, even if they only cover part of the last G/IG cycle. Figure 8a: purple line.

Action: We added the references for proxy records to the respective figure captions as suggested.

Response to comments by Luke Skinner (Referee#2)

We thank again Dr. Skinner for very detailed and useful review. Please find our replies below.

*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?*

We responded to this comment and question in the General response.

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.

This is, of course, a correct view on the model-data comparison, however, it might go beyond current state-of-the-art in both modelling and data. As we see it, few "robust" proxy-based features of the glacial states compared to interglacial ones are: (i) deep ocean (at least in Atlantic) was slower and colder; (ii) a biological productivity in the Southern Ocean was higher, however the deep ocean was not anoxic, and (iii) land had smaller or comparable amount of stored organic carbon. These 3 features are captured by our model. It is unclear for us whether regional details of proxy reconstructions are coherent enough to go beyond these three main features. To show regional details, we provide additional plots on ^{13}C, radiocarbon age and oxygen distributions in the ocean simulated by the model. For land, few data left beyond the last glacial maximum are coming from the pollen records, which is more qualitative than quantitative evidence for the land carbon storage.

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 longstanding 'mystery of the ice ages'? It would be helpful if the abstract summarised the authors proposal succinctly.*

To learn more about the solution of 'mystery of the ice ages' the reader should read several our previous papers plus the paper on which one of the authors (AG) is currently working. Obviously, a comprehensive theory of glacial cycles cannot be presented in an abstract but we did our best to accommodate this referee's suggestion.

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 CO2 to ice volume (using parameterizations for iron fertilisation and volcanic CO2 outgassing), and by further implementing additional climate-carbon cycle feedbacks that operate primarily through temperature-dependent respiration rates in the ocean, marine CO2 solubility effects and ocean circulation changes.

Although this is rather a statement than a question or comment, we feel that we have to respond because this statement grossly underestimates amount of work we made over the past 20 years. Successful simulation of climate, ice sheets and CO_2 concentration is achieved not only (and mostly not) by scaling of something to ice volume but rather by the development, calibration and coupling of numerous models of individual components of the Earth system. Although carbon cycle is important for simulating of glacial cycles, climate and ice sheets are more important because glacial cycles can be simulated without carbon cycle model (with constant CO_2), while without climate and ice sheet components glacial cycles cannot be simulated.

As we already explained in the General response, half of glacial CO_2 drop in our model is explained by physical processes (solubility, stratification, sea ice, etc.) and this is why it is very important that CLIMBER-2 simulate changes in glacial circulation and deep water ventilation realistically (see response to Referee#2). All related climate-carbon cycle feedbacks are not "implemented" but are intrinsic part of our model, and they operate in our model the same way as in the most advanced ESMs. Some sort of scaling to ice volume is only applied to volcanic outgassing and iron fertilization and these two effects never give together more than 35 ppm.

The extent to which the phenomena have been implemented as modelling choices, and the extent to which the magnitude of their impacts (e.g. on CO2) depends on parameter choices, should be made clear.

Parametrizations for iron fertilization and volcanic outgassing do affect the magnitude of the atmospheric CO_2 changes, but even without them – and with constant CO_2 - the system will go through the glacial cycles, albeit with smaller amplitude. Of course, any modeling result depends on the choice of modeling parameters.

Action: We discussed uncertainties in the choice of "carbon stew" in the section 4.

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?*

"Robust" here means that qualitative evolution of the system - such as direction of changes and occurrence of events - is not dependent on the choice of parameters, of course, within their plausible range. The CO_2 response to the AMOC shutdown is also robust in the model, however, the longer the shutdown, the stronger is an overshoot and the CO_2 recovery afterwards. In the CO_2 record, it looks like an overshoot and stabilization, like in the Eemian, or as small jump continued by increasing CO_2, as in the Holocene (Fig.2 , TI). As the timing of AMOC changes is very sensitive to the freshwater flux, these two types of responses could occur by chance, and therefore are not "robust".

3. The issue of CO2 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?

We described the mechanism of CO_2 response to AMOC shutdown in Brovkin et al (2012), section 3.3 and it is indeed related to fundamental aspect of physics and carbon cycle in the model. Incorporation of the temperature-dependent remineralization depth additionally Contribute to CO_2 response to AMOC changes but the mechanism described in Brovkin et al (2012) remains the dominant one.

Action: We made this point clear in the revised manuscript (page 7).

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?

This is absolutely correct observation. Indeed, during periods of strong dust flux, response of CO_2 to the same AMOC changes is smaller compare to the experiment without iron fertilization. This is consistent with the fact that during Heinrich event 1 no significant changes in CO_2 occurred while during previous Heinrich events CO_2 rose by 10-20 ppm. In any case, the influence of enhanced biological pump on CO_2 sensitivity to AMOC is not relevant for CO_2 overshoot at the end of glacial termination because iron fertilization ceased to influence CO_2 well before the end of glacial terminations.

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?

This argument is not clear since the AMOC shutdown and iron fertilization cause opposite effect on CO_2.

More explanation is needed for this phenomenon, especially if it is highlighted as being particularly noteworthy.

Action: we now discussed the mechanism of CO_2 overshoot in more detail on page

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.

The nutrient utilization is linearly proportional to the amount of "Antarctic dust" which is prescribed from Antarctic ice cores in the case of one-way and computed from sea level in the case of fully coupled experiment. Nutrient utilization has upper limit corresponding to the complete utilization of phosphates at the surface. The iron fertilization plays a role only during the 2nd part of the glacial cycles when global ice volume is large than 50m.

Action: We described parameterization of iron fertilization mechanism in the Appendix.

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.

Indeed, we apologize for not citing original ^{14}C production model used by Hain et al: Kovaltsov, G.A., Mishev, A., Usoskin, I.G., 2012. A new model of cosmogenic production of radiocarbon 14C in the atmosphere. Earth and Planetary Science Letters 337, 114-120. We used these data as provided by Hain et al. and scale them for pre-industrial state assuming that the system is in equilibrium, i.e. that production is equal to decay in the model. As a result of the scaling, the atmospheric $\delta^{13}C$ at 0 ka is around 0 permil, as in the IntCal data.

6. Page 5, Line 5: notably this way of doing greenhouse gases will produce incorrect results for millennial timescales, since methane and CO2 are not in phase during DO/ Heinrich events. Does this matter; can it be shown that it does not matter?

The magnitude of CH_4 changes during DO events is typically less than 150 ppb that represents only 5% of the radiative forcing of all GHGs during glacial cycles. Since periodicity of DO event is much shorter than the orbital time scales, DO event represent nothing more than a red noise of a small magnitude and it cannot produce any measurable effect on glacial cycles. On the other hand, even if one would have a model which incorporates methane cycle and is able to simulate DO events rather realistically, the right timing of DO events cannot be simulated anyhow because they are random. Thus 5% errors in instantaneous GHG forcing on millennial time scale is both unavoidable and insignificant.

7. Page 5, Line 22: can this careful calibration of volcanic outgassing be tested against e.g. atmospheric d13CO2 for example (note that these data are available for the last glacial cycle from Eggleston et al. 2016)? If the volcanic control on atmospheric CO2 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.

The volcanic ^{13}C is assumed to be constant (2 permil); its isotopic footprint is similar to the carbonate footprint that goes out of the system. Therefore, the effect of volcanic outgassing on the atmospheric ^{13}C is negligible on the timescale of simulations. The carbon budget was balanced for the preindustrial simulations as in Brovkin et al. (2007, 2012) – the silicate weathering of 12 Tmol was balanced by ½ of it with 6 Tmol of volcanic outgassing. For glacial cycles, silicate weathering is changing depending on the runoff, so the average volcanic outgassing used in glacial simulation should be slightly less than in the pre-industrial state.

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)?

The long-term carbon cycle (outgassing, weathering, sedimentation) requires a fine balance. With smaller or higher carbon input, the system will drift either up or down, but after some time will find a new cycling state with higher or smaller CO_2 level. Therefore the model parameters should be properly tuned and initial conditions are selected in a way to prevent such drift. The main quantity which has to equilibrate is the total ocean carbon content.

9. Page 6, Line 30: the lag of CO2 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).

We fully agree that any mismatch between model and data indicate that something is not perfectly right in the model. In this specific case, this is definitely not related to the parameterization of iron fertilization. Since both in the one-way coupled experiment and in the fully interactive experiments the dust flux drops nearly to zero already during the initial phase of glacial terminations, the rapid decline of the biological pump facilitate rapid CO_2 rise during the initial phase of deglaciaton. In fact one is potential candidate for creating this problem is the land carbon which starts to grow rapidly in parallel with ice sheets retreat. This is confirmed by the experiment in which land carbon is not accounted for and in which the lag between simulated and observed CO_2 is somewhat smaller. However because the lag is still present even in this experiment, it must be other problems. For obvious reason we cannot say what is wrong because if we would know, we would fix the problem and obtain perfect results.

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.

See our response to the comment N3.

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.

Increase of the lag between simulated and observed CO_2 in the fully coupled experiment does not provide additional information about the problems with the carbon cycle model. In the fully coupled simulations, where strong positive feedbacks between CO_2, climate and ice sheet are activated, any errors already present in the one-way coupled experiment will be strongly amplified. This is why accurate simulation of climate, ice sheet and CO_2 in the fully interactive simulations is much more challenging task comparing to simulations with prescribed CO_2.

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 cycles in the model, but where does the 100kyr timescale for ice sheet growth/decay come from in this model;

This is described in Ganopolski and Calov (2011) paper which is entirely devoted to the nature of 100 kyr cyclicity. In this paper we demonstrated that the 100 kyr cyclicity originates from the nonlinear response of the climate-cryosphere system to the orbital forcing through the phase locking of long glacial cycles to the 100 kyr eccentricity cycle.

is it simply the timescale at which the ice sheets get big enough for the dirty-ice albedo instability to kick in?

It is not simple. The time scale of ice sheets is about 30 kyr (Calov and Ganopolski, 2005), which is much shorter than 100 kyr but much longer than half of precessional cycle and it takes several precessional cycles for ice sheets to reach their "critical" size after which termination becomes possible. Most favorable conditions for reaching of this critical size is the periods of low eccentricity when one of positive precessional cycle coincide with a negative obliquity cycle. This is why long glacial cycles are phase locked to 100 kyr eccentricity cycle (Ganopolski and Calov, 2011). In turn, critical size is related not only to positive dust feedback but also to several other processes and feedbacks. This issue will be further discussed in a forthcoming paper.

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...);*

The time scale of ice sheet response to the orbital forcing is not prescribed and therefore it is not "a model choice". The time scale of ice sheets response to orbital and other climate forcings is determined by surface mass balance and ice sheet dynamics. We simulate surface mass balance using a physically based energy balance approach which has been successfully validated against present day observations and other models. Our ice sheet model SICOPOLIS is the standard 3-D thermomechanical model. This model also has been extensively tested for present day and paleo ice sheets. The basal sliding is parameterized in SICOPOLIS the same way as in other similar models. As any parametrization, it is a simplification and there are uncertainties but a good agreement between simulated and reconstructed ice sheets during the last glacial cycles gives as confidence in our model.

how do we know it should happen on that timescale?

It is not clear what is meant under "it". If this means 100-kyr time scale, then as explained above, this time scale is not directly related to the time scale of ice sheets.

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?

This is somewhat strange interpretation of the fact that the experiment with 10% enhanced freshwater flux has a "better" performance. First, it is only marginally "better". The RMSE of CO_2 in ONE_1.1 is 13.4 ppm, while in ONE_1.0 RSME is 14.9 ppm (see also Fig. 2e). Second, in reality the Northern Hemisphere ice sheet volume at LGM is not even known with accuracy of 10%. Therefore both experiments can be considered as equally plausible.

Is it possible that it could also be that this enhanced forcing is needed to compensate for other biases, e.g. from iron fertilization or volcanic CO2 parameterizations? How would we know, what do we learn from this?

First, we do not agree that model biases originate from iron fertilization or volcanic outgassing parameterizations. To the contrary, these two processes are introduced into the model to reduce model biases. Second, we cannot see any relationship between sensitivity of the AMOC to freshwater flux and iron fertilization because differences between these two experiments are only seen at the end of glacial terminations (Fig. 2e) when iron fertilization does not play any role. The only thing which one can learn from comparison of these two experiments is that the timing of AMOC resumptions at the end of glacial termination is very sensitive to the magnitude of freshwater flux. However, this is not surprising – it has been shown already in Ganopolski and Roche (2009).

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.

Action: We include additional Table 2 with contribution of each mechanism for the LGM and the averaged value over the entire 400 kyr time interval.

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.

Action: the original reference has been added.

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

We do not distinguish between these two sources because at the orbital time scales their effect on atmospheric CO_2 is nearly identical

… how is this treated and on what basis is a particular magnitude of volcanic CO2 flux chosen?

The values of the parameters in the equation for volcanic gas outgassing (p. 17) is chosen to produce glacial-interglacial variations in volcanic outgassing of about 30% of its average value which adds additional 10 ppm to glacial CO_2 drawdown.

More justification/testing of the volcanic CO2 implementation is also needed;

Again, we should repeat that the aim of the paper is not to justify or test individual components of the carbon stew - this has been done already in numerous papers, including our own. As far as volcanic outgassing is concerned, there is a number of papers which argues in favor of this mechanism such as Huybers and Langmuir (2009), Lund et al. (2016), Huybers and Langmuir (2017) and many others. This mechanism has been tested already with a carbon cycle model by Roth and Joos (2012) who concluded that a large change in volcanic outgassing during termination I cannot be ruled out but it occurs too late to be the main cause of deglacial CO_2 rise . Of course, we do not consider volcanism to be the main cause of deglacial CO_2 rise. Note that Roth and Joos considered much more drastic scenarios, where volcanic outgassing increased by factor 2 and more (in GB17 volcanic outgassing change by only 30%) during glacial termination. As the results, additional CO_2 rise due to including of time-dependent volcanism in Roth and Joos (2012) ranges between 13 and 142 ppm while it is less than 10 ppm in our case.

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)?

The impact is essentially nil. The magnitude of present volcanic outgassing is about 0.1 GtC. Our parameterization introduces anomaly of about ±0.015 GtC while the total ocean carbon content is about 40,000 Gt. By comparing these two numbers, it is obvious that at the orbital time scales 10^4-10^5 yrs the impact of variable volcanic outgassing cannot affect marine carbonate chemistry.

Is there a longer-term feedback via carbonate preservation

This feedback always operates in our model irrespectively of the source of carbon

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?

Averaged over long period of time (> 100 kyr) volcanic outgassing must be balanced by weathering and sedimentation, otherwise the model will drift away from the realistic state. This is why we tune the value of average volcanic outgassing to prevent such drift.

Also, as noted above, please state what the impacts of the changing volcanic carbon fluxes on atmospheric carbon isotopes are: are they essentially nil?

Yes, as has been shown already by Roth and Joos (2012), it is 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?

Indeed, both variable volcanic outgassing and brine rejections are hypothetical mechanism with some support from paleodata. Temperature-dependence of organic matter decomposition is well established process, so we have a higher confidence in this process than in brine rejection or volcanism. However, as it is explained on page 10 in GB17, the fact that it is unknown whether efficiency of brine rejection can be close to 100% during glacial time as postulated in Boutess et al. (2012). Even more serious problem is that the temporal evolution of this key parameter is unknown. Boutess et al. (2012) and Mariotti et al. (2017) assumed rapid drop in this value from maximum to zero at the beginning of glacial termination which is hard to justify in a view that the Antarctic ice sheet did not start to retreat at that time. Interestingly, Menviel et al. (2012) who also tested the role of brine rejection, assumed a totally different temporal scenario for the brine rejection, with the maximum of brine rejection efficiency reached in the middle of glacial cycle and essentially zero at LGM (their Fig.2 and Table 2). It is important to stress that the main strength of our modeling approach is that we do not use any explicitly time-dependent model parameters. Only orbital forcing is prescribed in the fully interactive run and the rest our model does on its own. Therefore we cannot use the approach by Boutess et al. (2012) and Mariotti et al. (2017). Until a clear idea of how to relate brine rejection efficiency with the simulated state of the Earth system will emerge, we simply cannot introduce brines in our "carbon stew".

*A critical analysis of **all key modelling choices** should be provided; not just for brine rejection.*

Unfortunately, this is impossible. Earth system models are based on numerous modeing choices which are crucial for successful simulation of glacial cycles. After all although CLIMBER-2 is an EMIC, it is still incomparably much more complex model than for example box-models and its program codes consist of more than 30,000 FORTRAN lines.

As far as the composition of "carbon stew" is concerned, individual processes have been already analysed in our previous papers and numerous papers of other authors. Even a rather exotic, volcanic mechanism, has been tested already by Roth and Joos (2012). As far as the iron fertilization and temperature-dependent remineralization depth are concerned, they are now routinely implemented in ocean carbon cycle models.

There are two reasons why we specifically addressed the brine rejection mechanism in GB17. First, we implemented this parameterization in CLIMBER a while ago but never described and tested it. Second, we were particularly interested in whether this parameterization helps to resolve the problem with atmospheric ^{14}C and we believe that the results presented in the section 5.2 of GB17 are worth discussing.

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.

We disagree that "*The text gives the impression that the deglacial d13Catm tends are quite accurately reproduced".* On page 11 we wrote that "the magnitude of the $\delta^{13}C$ drop is in a good agreement with empirical data" which is correct. Then we wrote "The model is also able to simulate W-shaped $\delta^{13}C$ evolution" which is also true. The problem is that modeled W-shape is shifted compare to the real one because model analog of Bolling-Allerod occurs ca. 1500 yrs earlier than in reality. This is absolutely natural because this event occurs in the model internally without any prescribed external forcing and therefore one cannot expect that it should occur at the same time as in reality. If we would shift the red curve in Fig. 9c, such a way that the timing of simulated warm event would be in agreement with the real Bolling—Allerod (which we will do in the revised manuscript), then the visual agreement is much better. Then we wrote on the same page that "At the same time, simulated present-day atmospheric $\delta^{13}C$ is underestimated compare to ice-core data by ca 0.2‰" which is not perfectly correct because in reality this difference is even smaller. The reason for this data-model mismatch during Holocene is not clear. The total land carbon uptake in the model during deglaciation is larger than 3000 GtC (see Fig. 13a), which is in an agreement with current estimates. However, in our model, atmospheric $\delta^{13}C$ is much stronger controlled by the marine processes than terrestrial one. We therefore assume that $\delta^{13}C$ mismatch reflects mismatches in the ocean C cycle. There are few reconstructions of the carbonate ions available; as in Brovkin et al. (2012), the model simulations of CO_3^- are qualitatively in line with observations. We rely more on $CaCO_3$ sedimentation records which show enhanced preservation during deglaciations, when deep ocean carbon was released to the atmosphere and deep waters became less acidic. This spike in preservation is reproduced by the model.

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).

We are grateful to the Referee for pointing on his recent paper. We agree that ^{14}C data are useful for testing the model and we are going to use $\delta^{14}R$ vertical profiles from fig 3 in Skinner et al. (2017) for comparison with our profiles in Fig. 12.

Action: We added basin averaged values for radiocarbon age for Atlatic, Pacific and Southern Ocean in Fig. 13 (former 12).

As far as the Referee's suggestion to plot individual data points in our fig. 10c is concerned, we have doubts. The data are too noisy. For example in the deep Atlantic between 20N and 40N radiocarbon ages are scattered between 1000 and 3000 years (Fig. 4a in Skiner et al., 2017) which suggests that either the uncertainties of radiocarbon age as high as 50% or that the data contain strong regional signal which anyhow cannot be reproduced by the zonally averaged ocean model.

Action: We made a qualitative comparison between our new Fig. 11 and fig. 4 from Skinner et al. (2017) (page 13).

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).

By comparing our fig. 12 with fig. 3 from Skinner et al. (2017) we cannot understand why the Referees arrived to such conclusion. At LGM our standard model (without brines, blue) gives 2000-2500 yrs in the deep Atlantic and 2500-3000 yr in the deep Pacific which is in very good agreement with Skinner et al. (2017). At the same time, the model with brines (green) gives age more than 3000 yrs in the deep Atlantic and 3500-4000 yr in deep Pacific which is much older than in Skinner et al. (2017).

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.

Action: The reference was corrected

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.

Of course, there is no guarantee at all that the magnitude and timing of mechanisms proposed in the paper are quantitatively accurate. On the other hand it is unclear whether there is a direct link between the composition of the "carbon stew" and LGM radiocarbon problem. We can only discuss qualitative fit of the model to the ^{14}C and other proxy data, and suggest possible reasons for disagreement.

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 reservoirs 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 _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.

Action: We are grateful to the Referee for this suggestion and now showed the radiocarbon age instead of $\Delta\Delta^{14}C$ in all our figures.

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)?

We investigated the permafrost carbon hypothesis and found that its impact on CO_2 is not significant enough to explain trends in ^{14}C and ^{13}C. The land surface model operates with large grid cells which smooth out possible abrupt changes in the land C storage.

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.

Why "not CO2"? Glacial terminations occur roughly every 100 kyr. The lag of simulated CO_2 relative to the observed one (measured by timing of termination midpoints) is ca. 3 kyr. Even if the ice core CO_2 age is perfectly correct, our model computes terminations with the accuracy 3% which is a very high accuracy by the standard of climate modeling.

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)?

Action: we specified that the match is for stable carbon isotopes.

26. Page 14, Line 3: should this read "the magnitude of atmospheric 14C change is underestimated"?

Action: this sentence has been corrected

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.*

Action: such comparison is now presented

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

See reply on our understanding of robustness above.

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)?

First, parameterization of the dust flux is only applied in the fully interactive experiments. In the one-way coupled experiment, the dust flux is taken from the ice core data and it drops rapidly at the beginning of each termination. Second, in the parameterization described on page 16, the dust is not just scaled with sea level, it has a much more complex dependence on sea level, and time derivative of sea level dS/dt plays crucial role. As the result, after the LGM the term dS/dt turns negative and dust flux starts to decline almost immediately after the LGM and not after crossing of 50m threshold. This formula was chosen by tuning simulated dust to the measured in ice cores. As one can see from Fig. 5c, the agreement between simulated and measured dust is not bad. In any case, there is no tendency for simulated dust to stay too high too long during glacial terminations. In fact, the effect of iron fertilization on CO_2 in the second half of glacial terminations is always small. Therefore Referee's concern that our parameterization keeps "*iron fertilization strong for too late*" is not justified.

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.

This is done already. See Fig. 5c in GB17.

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.

The effect of iron fertilization in our model is highly parameterized.

Action: This parameterization is now described 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).

Action: we added comparing simulated $\delta^{13}C$ with the Eggleston et al. (2016) reconstruction.

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

Action: done

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.

Action: done

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.

Marine radiocarbon ventilation age from individual locations cannot explain changes in atmospheric ^{14}C. This only can be done by proper global averaging but paleodata are too sparse and uncertain to produce such global averaging.

33. Figure 10: why do the plots only go to 40°S? 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).

See our response to the comment N19.

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).

Action: done

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.

The experiment with brine rejection has almost the same freshwater forcing as the standard run. However, intensive brine rejection in combination with density-dependent vertical mixing strongly affects density fields, ocean circulation and its sensitivity to freshwater flux. As the result, the model with brines does not simulate millennial scale variability during termination.

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.*

Action: We added aggregated observational data from Skinner et al (2017) to this figure and discussed agreement/disagreement.

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

Action: this figure has been removed

We also took into account all minor points in the revised manuscript.

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Simulation of climate, ice sheets and CO_2 evolution during the last four glacial cycles with an Earth system model of intermediate complexity

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In spite of significant progress in paleoclimate reconstructions and modeling of different aspects of the past glacial cycles, the mechanisms which transform regional and seasonal variations in solar insolation into long-term and global-scale glacial-interglacial cycles are still not fully understood, in particular, for CO_2 variability. Here using the Earth system model of intermediate complexity CLIMBER-2 we performed simulations of co-evolution of climate, ice sheets and carbon cycle over the last 400,000 years using the orbital forcing as the only external forcing. The model simulates temporal dynamics of CO_2, global ice volume and other climate system characteristics in good agreement with paleoclimate reconstructions. These results provide strong support to the idea that long and strongly asymmetric glacial cycles of the late Quaternary represent a direct but strongly nonlinear response of the Northern Hemisphere ice sheets to the orbital forcing. This direct response is strongly amplified and globalized by the carbon cycle feedbacks. Using simulations performed with the model in different configurations, we also analyze the role of individual processes and sensitivity to the choice of model parameters. While many features of simulated glacial cycles are rather robust, some details of CO_2 evolution, especially during glacial terminations, are sensitive to the choice of model parameters. Specifically, we found two major regimes of CO_2 changes during terminations: in the first one, when the recovery of the Atlantic meridional overturning circulation (AMOC) occurs only at the end of the termination, a pronounced overshoot in CO_2 concentration occurs at the beginning of the interglacial and CO_2 remains almost constant during interglacial or even decline towards the end, resembling Eemian CO_2 dynamics. However, if the recovery of the AMOC occurs in the middle of the glacial termination, CO_2 concentration continues to rise during interglacial, similar to Holocene. We also discuss potential contribution of the brine rejection mechanism for the CO_2 and carbon isotopes in the atmosphere and the ocean during the past glacial termination.

1. Introduction

Antarctic ice cores reveal that during the past 800 kyr, the atmospheric CO_2 concentration (Petit et al., 1999; Jouzel et al., 2007) varied synchronously with the global ice volume (Waelbroeck et al., 2002; Spratt and Lisiecki, 2016). The most straightforward explanation for this fact is that CO_2 drives glacial cycles together with orbital variations, and the longest, 100-kyr component of the late Quaternary glacial cycles, which is absent in the orbital forcing, is the direct response to CO_2 forcing where 100-kyr component is the dominant one. However, simulations with climate-ice sheet models of different complexity (e.g. Berger et al., 1999; Crowley and Hyde, 2008; Ganopolski and Calov, 2011; Abe-Ouchi et al., 2013) show that long glacial cycles (i.e. cycles with typical periodicity of ca. 100 ka) can be simulated with constant CO_2 concentration if the latter is sufficiently low. Moreover, these model simulations show that not only the dominant periodicity, but also the timing of glacial cycles, can be correctly simulated without CO_2 forcing. This fact strongly suggests an opposite interpretation of close correlation between global ice volume and CO_2 during Quaternary glacial cycles – namely that glacial cycles represent a strongly nonlinear response of the Earth system to orbital forcing (Paillard, 1998) while variations in CO_2 concentration are directly driven by ice sheets fluctuations. In turn, CO_2 variations additionally strongly amplify and globalize the direct response of ~~the Earth system~~ice sheets to the orbital forcing.

In spite of significant number of studies aimed to explain low glacial CO_2 concentrations (e.g. Archer et al., 2000; Sigman and Boyle, 2000; Watson et al., 2000), the influence of ice sheets on carbon cycle remains poorly understood. It is also unclear how much of CO_2 variations represent the direct response to ice sheets forcing and how much is the results of additional amplification of CO_2 variations through the climate-carbon cycle feedback. Indeed, although radiative forcing of ice sheets contributes about a half to glacial–interglacial variations in global temperature (Brady et al., 2013), most of cooling associated with ice sheets is restricted to the area covered by ice sheets and their close proximity. Thus the direct contribution of ice sheets to glacial ocean cooling is rather limited and therefore the effect of ice sheets on CO_2 drawdown through the solubility effect can explain only a fraction of reduction in glacial CO_2. At the same time, the direct effect of ice sheets on atmospheric CO_2 concentration through ca. 3% changes in the ocean volume and global salinity is rather well understood but works in the opposite direction and leads to glacial CO_2 rise of about 10-20 ppm (Sigman et al., 2000; Brovkin et al., 2007). Another direct effect of ice sheet growth on the carbon cycle through reducing area covered by forest (e.g. Prentice et al., 2011) also operates in the opposite direction. However, several other processes could potentially contribute to glacial CO_2 drawdown through ice sheets growth and related lowering of sea level. One such mechanism is enhanced biological productivity in the Southern Ocean due to the iron fertilization effect (Martin, 1990; Watson et al., 2000). The latter is attributed to enhanced dust deposition over the Southern Ocean seen in the paleoclimate records (Martinez-Garcia et al., 2014; Wolff et al., 2006). At least part of this enhanced deposition is associated with the dust mobilization from exposed Patagonian shelf and glaciogenic dust production related to Patagonian ice cap (Mahowald et al., 1999; Sugden et al., 2009). A number of studies on the effect of iron fertilization suggested a contribution of 10 to 30 ppm to the glacial CO_2 decrease (e.g. Watson et al., 2000; Brovkin et al., 2007). Another effect is related to the brines rejection mechanism, more specifically, to a much deeper penetration of brines produced during sea ice formation in the Southern Ocean during glacial time. The latter is explained by shallowing and significant reduction of the Antarctic shelf area. According to Bouttes et al. (2010) this mechanism, in combination with enhanced stratification of the deep ocean, can contribute up to 40 ppm to the glacial CO_2 lowering.

Apart from the mechanisms mentioned above, many other processes have been proposed to explain low glacial CO_2 concentration. Among them are changes in the ocean circulation (Watson et al., 2015) and an increase in the South Ocean stratification (e.g. Kobayashi et al., 2015), increase in sea ice area in Southern Ocean (Stephens and Keeling, 2000) and a shift in the westerlies (Toggweiler et al., 2006), increase in nutrients inventory or change in the marine biota stoichiometry (Sigman et al., 2000; Wallmann et al., 2016), changes in coral reefs accumulation and dissolution (Opdyke and Walker, 1992), accumulation of carbon in the permafrost regions (Ciais et al., 2012; Brovkin et al., 2016), variable volcanic outgassing (Huybers and Langmuir, 2009) and several other mechanisms. Most of these processes are not directly related to the ice sheets area or volume, and thus should be considered as amplifiers or modifiers of the direct response of CO_2 to ice sheets operating through the climate-carbon cycle feedbacks. Although paleoclimate records provide some useful constraints, the relative role of particular mechanisms at different stages of glacial cycles remains poorly understood.

Most studies of glacial-interglacial CO_2 variations performed up to date were aimed at explanation of low CO_2 concentration at the Last Glacial Maximum (LGM, ca. 21 ka). In these studies, both continental ice sheets and the radiative forcing of low glacial CO_2 concentration were prescribed from paleoclimate reconstructions. Only few attempted to explain CO_2 dynamics during part (usually glacial termination) or the entire last glacial cycle with models of varying complexity from simple box-type models (e.g. Köhler et al., 2010; Wallmann et al., 2016), models of intermediate complexity (Brovkin et al., 2012; Menviel et al., 2012), or stand-alone complex ocean carbon cycle models (Heinze et al., 2016). In all these studies, radiative forcing of CO_2 (or total GHGs) was prescribed based on paleoclimate reconstructions. Similarly, ice sheets distribution and elevation were prescribed from paleoclimate reconstructions or model simulations where ~~ed but, again, using prescribed~~ radiative forcing of GHGs has been prescribed. Thus in all these studies, CO_2 was treated as an external forcing rather than an internal feedback. Here we for the first time performed simulations of the Earth system dynamics during the past four glacial cycles using fully interactive ice sheet and carbon cycle modelling components, and therefore the only prescribed forcing in this experiment is the orbital forcing.

2. The model and experimental setup

2.1 CLIMBER-2 model description

In this study we used the Earth system model of intermediate complexity CLIMBER-2 (Petoukhov et al., 2000; Ganopolski et al., 2001). CLIMBER-2 includes a 2.5-dimensional statistical-dynamical atmosphere model, a 3-basin zonally averaged ocean model coupled to a thermodynamic sea ice model, the 3-dimensional thermomechanical ice sheet model SICOPOLIS (Greve, 1997), the dynamic model of the terrestrial vegetation VECODE (Brovkin et al., 1997) and the global carbon cycle model (Brovkin et al., ~~2012~~2002, 2007). Atmosphere and ice sheets are coupled bi-directionally using a physically based energy balance approach (Calov et al., 2005). ~~Ice~~ The ice sheet model is only applied to the Northern

Hemisphere. The contribution of the Antarctic ice sheet to global ice volume change is assumed to be constant during glacial cycles and equal to 10%. The model also includes parameterization of the impact of aeolian dust deposition on snow albedo (Calov et al., 2005; Ganopolski et al., 2010). The CLIMBER-2 model in different configurations has been used for numerous studies of past and future climates, in particular, simulations of glacial cycles (Ganopolski et al., 2010; Ganopolski and Calov, 2011; Willeit et al., 2015; Ganopolski et al., 2016) and carbon cycle operation during the last glacial cycle (Brovkin et al., 2012).

As it has been shown in Ganopolski and Roche (2009), temporal dynamics of the Atlantic meridional overturning circulation (AMOC) during glacial terminations in CLIMBER-2 ~~is~~ are very sensitive to the magnitude of freshwater flux to the North Atlantic. To explore different possible deglaciation evolutions, together with the standard model version, we performed an additional suit of simulations where the component of freshwater flux into the ocean originated from melting of ice sheets was uniformly scaled up or down by up to 10%. This rather small change in the freshwater forcing (typically smaller than 0.02 Sv) does not affect AMOC dynamic appreciably during most of time but does induce a strong impact during deglaciations (see below). Other modifications of the climate-ice sheet component of the model are described in the Appendix.

The ocean carbon cycle model includes modules for marine biota, oceanic biogeochemistry, and deep ocean sediments. Biological processes in the euphotic zone (the upper 100 m in the model) are explicitly resolved using the model for plankton dynamics by Six and Maier-Reimer (1996). The sediment diagenesis model (Archer, 1996; Brovkin et al., 2007) calculates burial of $CaCO_3$ in the deep sea, while shallow-water $CaCO_3$ sedimentation is simulated based on the coral reef model (Kleypas, 1997) driven by sea level change. Silicate and carbonate weathering rates are scaled to the runoff from the land surface; they are also affected by sea level change (Munhoven, 2002). Compare to Brovkin et al (2012) the carbon cycle model has been modified in several aspects. Similar to Brovkin et al. (2012), the efficiency of nutrients utilization in the Southern Ocean is set to be proportional to the dust deposition rate (see Appendix), which in the case of one-way coupling is prescribed to be proportional to the dust deposition in EPICA ice core. However, in the fully coupled experiment, the dust deposition rate over the Southern Ocean has been computed from simulated sea level (see Appendix). This means that in the fully interactive run (see below) we did not use explicitly any paleoclimate data to drive the model and the orbital forcing was the only driver of the Earth system dynamics. In the marine carbon cycle component, we also account for a dependence of the remineralization depth on ocean temperature following Segschneider and Bendtsen (2013) (see Appendix). In the previous studies, remineralization depth was kept constant.

The CLIMBER-2 model used in earlier studies of glacial carbon cycle did not include long-term terrestrial carbon pools such as permafrost carbon, peat and carbon buried beneath the ice sheets. In the present version of the model these pools are included. The model also accounts for peat accumulation. Modification of the terrestrial carbon cycle components is described in detail in the Appendix. For simulation of atmospheric radiocarbon during the last glacial termination we used the rate of ^{14}C production following scenario by Hain et al. (2014) which is based on the production model by Kovaltsov et al. (2012).

2.2 One-way coupled and fully interactive experiments

In our previous experiments performed with the CLIMBER-2 model (Brovkin et al., 2012; Ganopolski and Brovkin, 2015) we not only prescribed temporal variations in the Earth's astronomical parameters (eccentricity, precession and obliquity) but also the radiative effect of GHGs (CO_2, CH_4 and N_2O) computed using their concentrations from the ice cores records (Luthi et al., 2008; Petit et al., 1999). In these experiments, which we will denote hereafter as "one-way coupled" (Fig. 1a, Table 1), atmospheric CO_2 was computed by the carbon cycle module but not used as the radiative forcing for the climate component. Similarly, in these experiments CO_2 fertilization effect on vegetation was computed using reconstructed CO_2 concentration. Therefore in one-way coupled experiments there were no feedbacks of the simulated atmospheric CO_2 concentration to climate. In the present study, we performed a suit of one-way coupled experiments for the last four glacial cycles but we also performed fully interactive simulations in which the orbital forcing was the only prescribed external forcing. Since CLIMBER-2 does not include methane and N_2O cycles and does not account for these GHGs in its radiative scheme, we made use of the fact that CO_2 is the dominant GHG and that temporal variations of other two follow rather closely CO_2. To account for the effect of methane and N_2O forcings, we computed the effective CO_2 concentration used in the radiative scheme of the model in such a way that radiative forcing of equivalent CO_2 exceeds radiative forcing of simulated CO_2 by 30% at any time. This type of experiments we will refer to as "fully interactive" (Fig. 1b). In the fully interactive experiment we use computed CO_2 concentration also in terrestrial component to account for CO_2 fertilization effect. As was stated above, dust deposition over the Southern Ocean used in the parameterization of iron fertilization effect computed from the global sea level. The radiative forcing of aeolian dust and dust deposition on ice sheets (apart from the glaciogenic dust sources) in both types of experiments were obtained identically to Calov et al. (2005) and Ganopolski and Calov (2011) by scaling the field computed with GCMs, where scaling parameter was proportional to global ice volume.

2.3 Model spin-up

The model spin-up and proper choice of model parameters for simulation of multiple glacial cycles represents a challenge when using the models with very long-term components of the carbon cycle because inconsistent initial conditions or even a small disbalance in carbon fluxes could lead to a large drift in simulated atmospheric CO_2 concentration (in the case of one-way coupling) or the state of the entire Earth system (climate, ice sheets, CO_2) in the case of fully interactive experiments. Note that in the latter case, the negative climate-weathering feedback will eventually stabilize the system but this occurs at the time scale of several glacial cycles and over this time climate could drift far away from its realistic state. To avoid such drift, volcanic outgassing should be carefully calibrated. Based on a set of sensitivity experiments, we found that the value of 5.3 Tmol C/yr allows us to simulate quasiperiodic cycles without long-term trend in atmospheric CO_2. Note, that even ±10%

change in volcanic outgassing leads to significant (order of 100 ppm) drift in CO_2 concentration simulated over the last four glacial cycles.

When the carbon cycle model incorporates such long-term processes as terrestrial weathering, marine sediment accumulation and permafrost carbon burial, the assumption that the system is close to equilibrium at preindustrial period or at any other moment of time is not valid even if CO_2 concentration was relatively stable during a certain time interval. To produce proper initial conditions at 410 ka we performed a sequence of 410 ky-long one-way coupled runs with the identical forcings. We first used as the initial conditions the final state obtained in simulation of the last glacial cycles (Brovkin et al., 2012). Then we launched each 410 ky experiment from the final state obtained in the previous model run. The results of such sequence of experiments reveal a clear tendency to converge to the solution with similar initial and final states of the Earth system. We then used the state of climate and carbon cycle obtained at the end of the last run as the initial conditions for all experiments presented in this paper. In the analysis of all experiments described below we exclude the first 10,000 years when adaptation of different fields to each other occurs.

3. Simulations of the last four glacial cycles

Realistic simulation of climate and carbon cycle evolution during the last four glacial cycles is more challenging in the case of fully interactive configuration, because in this case a number of additional positive feedbacks tend to amplify initial model biases. Therefore we begin our analysis with the one-way coupled simulations similar to that performed in Brovkin et al. (2012). This configuration was also used for calibration of new parameterizations (see section 4) and sensitivity experiments for the last glacial termination (section 5).

3.1 Experiments with one-way coupled climate-carbon cycle model

Simulated climate and ice sheets evolution in the one-way coupled experiments are rather similar to the ones in Ganopolski and Calov (2011), which is not surprising since the only difference between model versions used in these studies is related to the coupling between ice sheet and climate components (see Appendix). Simulated glacial cycles are characterized by global surface air temperature variations of about 5^{o}C (not shown) and maximum sea level drops by more than 100 meters during several glacial maxima. Simulated global ice sheets volume during most of time is close to the reconstructed one (Spratt and Lisiecki, 2016) (Fig. 2d). In general, differences between simulated and reconstructed global sea level are comparable or smaller than uncertainties in sea level reconstructions obtained using different methods.

Simulated CO_2 concentration (Fig. 2e) is also in a good agreement with reconstructions based on several Antarctica ice cores (Barnola et al., 1987; Monnin et al., 2004; Petit et al., 1999; Luthi et al., 2008). The model correctly reproduces the magnitude of glacial-interglacial CO_2 variability of about 80 ppm. Results of simulations with the standard model version (ONE_1.0) and model with 10% enhanced meltwater flux (ONE_1.1) are essentially identical during most of time except for glacial terminations. During glacial terminations even rather small differences in the freshwater forcing cause pronounced differences in the temporal evolution of the AMOC, and as a result, of CO_2 concentration. As seen in Fig. 2d, in the experiment ONE_1.0, CO_2 concentration grows monotonously during the last glacial termination (TI, midpoint at ca. 15 ka) and TIV (ca. 330 ka) while it rises faster and overshoots the interglacial level during TII (ca. 135 ka) and TIII (ca. 240 ka). To the contrary, in the experiments ONE_1.1, similar overshoots occur during TI and III but not TIV. In all cases, simulated CO_2 lags behind the reconstructed one but this lag is smaller in the case when overshoot is simulated. Experiments with CO_2 overshoots are clearly in better agreement with empirical data for MIS7 and MIS9. Analysis of model results shows that pronounced CO_2 overshoot occurs in the case when the AMOC is suppressed during the entire glacial termination and recovers only after the cessation of the meltwater flux (Fig. 3). To the contrary, if the AMOC recovers well before the end of deglaciation, simulated CO_2 experiences only local overshoot and continues to rise during most of the interglacial. The latter behaviour is similar to that was observed during MIS 11 and the Holocene, while the former is typical for MIS5, 7 and 9. Thus our model is able to reproduce both types of CO_2 dynamics during the interglacials.

The rise of CO_2 by 10-20 ppm on millennial time scale during the AMOC shutdowns is the persistent feature of CLIMBER-2 and the cause of this rise has been explained in Brovkin et al. (2012) by a weakening of the reverse cell of the Indo-Pacific overturning circulation during periods of reduced AMOC. A similar rise in atmospheric CO_2 concentration during periods of AMOC shutdown has been simulated in some other (but not all) similar modeling experiments. Incorporation of the temperature-dependent remineralization depth additionally contributes to the CO_2 overshoots at the beginning of several interglacials (see below) but the mechanism described in Brovkin et al. (2012) remains the dominant one.

Comparison of simulated deep ocean $\delta^{13}C$ with paleoclimate reconstructions (Fig. 4) show that the model correctly simulates larger $\delta^{13}C$ variability in the deep Atlantic in comparison to the deep Pacific but underestimates the amplitude of glacial-interglacial $\delta^{13}C$ variability. Simulated atmospheric $\delta^{13}CO_2$ shows a rather complex behaviour and amplitude of variability up to 0.6‰. The agreement between the simulated and reconstructed (Eggleston et al., 2016) atmospheric $\delta^{13}CO_2$ is rather poor. Both model and data show a drop in atmospheric $\delta^{13}CO_2$ during the last and penultimate deglaciations but the data suggest also the strong drop at the end of Eemian interglacial while the model simulated continuous rise of $\delta^{13}CO_2$ at that interval. In addition, temporal variability of the reconstructed $\delta^{13}CO_2$ is significantly larger than the simulated one. More detailed comparison with empirical data during the last deglaciation is presented in the Section 5.

Changes in the ocean oxygenation is considered to be an important indicator of respired carbon storage in the deep ocean, and therefore the proxy for the strength of ocean biological pump. Jaccard et al. (2016) inferred a significant decline in the deep South Ocean oxygenation and interpreted it as the result of combine effect of iron fertilization by dust and decreased deep ocean ventilation. Our results (Fig. 5) are fully consistent with such interpretation. The model simulates significant reduction of the dissolved oxygen in the deep South Ocean during glacial period. Roughly 2/3 of this reduction is simulated already in the experiment without iron fertilisation and can be solely attributed to the reduced deep ocean ventilation. It is noteworthy that changes in the oxygen concentration in this experiment are strongly anticorrelated with the area of sea ice in the South Hemisphere (Fig. 5c). The late is explained by the fact that sea ice directly and indirectly (through stratification of the upper ocean layer) affects gas exchanges between the ocean and the atmosphere. Oxygen concentration is additionally reduced in the experiment which accounts for the iron fertilization effect during period with high dust deposition rate (Fig. 5d).

3.2. Experiments with the fully interactive model

In two-way coupling experiments (fully interactive runs), orbital forcing is the only prescribed forcing and the model does not use any time-dependent paleoclimatological information (such as the Antarctic dust deposition rate used in the one-way coupled experiment). Results of fully interactive experiment INTER_1.0 are shown in Fig. ~~5~~6. For the first experiment of this type ever, the agreement between model simulations and empirical reconstructions is reasonably good. The model simulates correct magnitude and timing of the last four glacial cycles both in respect of sea level and CO_2 concentration. It also reproduces strong asymmetry of glacial cycles. Naturally, the mismatch between simulated and reconstructed characteristic in fully interactive experiments is larger than in the one-way coupled experiment. In particular, in the fully interactive experiment, simulated ice volume is underestimated by 10-20 meters compared to reconstructed one. Although the magnitude of glacial-interglacial CO_2 variability in the fully interactive experiment INTER_1.0 is similar to that in one-way coupled experiment ONE_1.0 and in reconstructions, the lag between simulated and reconstructed CO_2 during glacial terminations increases additionally in comparison to one-way coupled experiment. Interestingly, the last glacial cycle and the first 150 ky of the INTER_1.0 and ONE_1.0 runs are in very good agreement while during time interval between 300 ka and 150 ka BP discrepancies are larger. This period corresponds to higher eccentricity and therefore larger magnitude of the orbital forcing. Similarly to the results of one-way coupled experiments, fully interactive runs also show strong sensitivity to magnitude of freshwater flux during glacial terminations.

Comparison of simulated ice sheets spatial distribution and elevation (Fig. ~~6~~7) shows that the results of one-way coupled (ONE_1.0, Fig 6a) and fully interactive run (INTER_1.0, Fig. ~~6b~~7b) are almost identical during the LGM (the same is true for the previous glacial maxima, not shown) and in a reasonable agreement with the paleoclimate reconstructions. During glacial terminations, the difference between two runs increases since in the fully interactive run the radiative forcing of GHGs lags considerably behind the reconstructed one used in the one-way coupled experiment. As the result at 7 ka continental ice sheets melted completely in the one-way coupled experiment (Fig. ~~6c~~7c) while in the fully interactive run a relatively large ice sheet is still present in the northern-eastern Canada (Fig. ~~6d~~7d).

It is instructive to compare frequency spectra of simulated and reconstructed global ice volume in one-way and fully coupled experiments (Fig. ~~7)~~8. In addition, we show here results from the experiment ONE_240 performed with constant radiative forcing of GHGs corresponding to equivalent CO_2 concentration of 240 ppm. As already shown by Ganopolski et al. (2011), even with constant CO_2, the model computes pronounced glacial cycle with 100-kyr periodicity, although it has much weaker amplitude than the reconstructed sea level. Both model experiments with varying CO_2 radiative forcing (ONE_1.0 and INTER_1.0) reveal much stronger 100-kyr periodicity, which has only slightly weaker amplitude than the spectrum of reconstructed sea level. Interestingly, frequency spectra of sea level simulated in one-way and fully interactive runs have rather similar power in 100 ka and obliquity (40 ka) bands, but in the precessional band (ca. 20 ky) one-way coupled experiment reveals much higher spectral power. This cannot be explained by the prescribed radiative forcing of GHGs because the latter contains very little precessional variability. The explanation of stronger precessional component in the ONE_1.0 run is related to the fact that one-way coupled model simulates slightly faster ice sheet growth during the initial part of each glacial cycle and the modeled sea level variability at the precessional frequency is very sensitive to ~~the~~ ice volume.

4. The composition of “the carbon stew” and factor analysis

In this section, we discuss the contribution of different factors to simulated variations in CO_2 concentration. Because neither of mechanisms could explain the CO_2 dynamics in isolation from the other factors (e.g. Sigman and Boyle, 2000; Archer et al., 2000), we call the composition and timing of the mechanisms leading to the glacial CO_2 cycle “the carbon stew”. As has been shown in Brovkin (2012), the role of different mechanism controlling CO_2 concentration at different phases of glacial cycles is different. However, even if we consider only the LGM (as most of previous work did), the composition of “carbon stew” remains highly uncertain even although there is a growing awareness that both physical and bilological processes must have played a comparably important role in glacial CO_2 drawdown (e.g. Schmittner and Somes 2016;~~–~~ Galbraith and Jaccard, 2015). Obviously, the choice of the “carbon stew” is crucially important for successful simulations of glacial cycles. The aim of our paper is not to present the ultimate solution for the “carbon stew” problem since at present this is impossible. Rather we want to demonstrate that with a reasonable representation of physical, geochemical and biological processes in the model, it is possible to reproduce the main features of Earth system dynamics over the past 400 kyr, including the magnitude and timing of climate, ice volume and CO_2 variations.

Similar to the study by Brovkin et al. (2012), we performed a set of experiments using one-way coupling (see Table 1 for detail). We use this approach instead of fully interactive coupling to exclude complex and strongly nonlinear interactions associated with ice sheet dynamics which significantly complicate factor analysis. In the case of one-way coupled experiments climate, ice sheets and other external factors are identical and experiments only differ by parameters of the carbon cycle model. Since CO_2 simulated in the one-way coupled experiment with 10% enhanced meltwater flux (ONE_1.1) is in a slightly better agreement with observational data than the standard one (ONE_1.0), for the factor analysis we used experiment ONE_1.1 as the reference one and performed all sensitivity experiments with 10% enhanced meltwater flux.

4.1 The standard carbon cycle model setup

We begin our analysis from the experiment that incorporates only standard ocean biogeochemistry as described in Brovkin et al. (2007) (Fig. ~~8~~9). This experiment does not include effect of terrestrial carbon cycle. In this configuration, the model is able to explain only about ~~40~~ 45 ppm of CO_2 reduction during glacial cycles. Note that this experiment accounts for changes in the ocean volume by ca. 3% and corresponding changes in the total biogeochemical inventories including salinity. These volume changes are often neglected in simulations with 3-dimensional ocean models (e.g. Heinze et al. 2016), although in our simulations they counteract to glacial CO_2 drawdown by ca. 12 ppm. Without the effect of ocean volume reduction, the combination of physical processes and carbonate chemistry can explain of up to 57 ppm at the LGM and 38 ppm during the entire 400 kyr time interval (see Table 2). This is consistent with the resent results by Buchanam et al. (2016) and Kobayashi et al. (2015) . Note that simulated changes in silicate weathering and its impact on atmospheric CO_2 are small as have been shown already in Brovkin et al. (2012).

Accounting for the land carbon changes does not help to explain the CO_2 concentration changes, since terrestrial carbon contains by ca. 350 Gt less carbon at the LGM compared to the pre-industrial state. This reduces the glacial-interglacial CO_2 difference by 10-~~20~~ 15 ppm comparing to the ocean-only experiment (Fig. ~~8b~~9b). Enabling of parameterization for the iron fertilization effect in the Southern Ocean results in additional glacial CO_2 drawdown of up to 30 ppm (22 ppm at the LGM), mostly towards the end of each glacial cycle which is related to the chosen parameterization for the dust deposition rate (Fig. ~~8c~~9c). This value is rather close to that reported by Lambert et al. (2015). With all these processes considered in our previous study by Brovkin et al. (2012), we are still short of ca. ~~20~~ 25 ppm to explain the full magnitude of glacial-interglacial variability.

4.2 Additional processes included in the carbon cycle model

There is a number of other proposed mechanisms which can explain several tens ppm of glacial CO_2 decline. Our choice of two processes to obtain the observed magnitude of glacial-interglacial CO_2 variations is somewhat subjective. Chosen mechanisms are explained below, while an alternative one (brine rejection) is discussed in the section 4.3.

The first additional to Brovkin et al. (2012) mechanism is temperature-dependent remineralization depth. In the standard CLIMBER-2 version, remineralization depth is spatially and temporally constant. Since in the colder ocean remineralization depth increases, this enhances the efficiency of carbon pump and contributes to a decrease of atmospheric CO_2 concentration (e.g. Heinze et al., 2016; Menviel et al., 2012, Matsumoto, 2007). Details of the mechanism implementation are described in Appendix. As seen from Fig. ~~8e~~9e, making remineralization depth temperature-dependent introduces additional glacial-interglacial variability with the magnitude of about 20 ppm. Roughly half of this value is clearly attributed to the CO_2 overshoots which are seen at the beginning of some interglacials. The reason is that the AMOC shutdowns due to melt water flux that happened during glacial terminations lead not only to surface cooling in the North Atlantic, but also to significant thermocline warming that occurs over the entire Atlantic ocean (e.g. Mignot et al., 2007). This subsurface warming causes significant shoaling of the remineralization depth and the release of carbon from the ocean into the atmosphere. This process reverses after the recovery of the AMOC at the beginning of interglacials.

Burley and Katz (2015) and Huybers and Langmuir (2009) proposed that the rate of volcanic outgassing varies during glacial cycle due to variable load of the ice sheet and ocean on the Earth crust. Therefore we assume that volcanic outgassing has a variable component (about 30% of its averaged value of 5.3 Tmol/yr) which represent the delayed response to the change in ice volume. This simple parameterization explained in Appendix does not affect cumulative volcanic outgassing over glacial cycle, but contributes to glacial-interglacial variability by additional 10 ppm (Fig. ~~8d~~9d). With varying volcanic outgassing and temperature-dependent mineralization depth, CLIMBER-2 model reproduces glacial-interglacial CO_2 cycles in a good agreement with paleoclimate records (Fig. ~~8a~~9a~~, blue line~~).

4.3 Brine rejection mechanism

Using a different version of CLIMBER-2, Bouttes et al. (2010) proposed that a significant fraction of glacial-interglacial CO_2 variations can be explained by the mechanism of brine rejections, more specifically, by a large increase in the depth to which brines can penetrate under glacial conditions without significant mixing with ambient water masses. Such increase in brine efficiency under glacial conditions would result in large transport of salinity, carbon and other tracers from the upper ocean layer into the deep ocean. By choosing the efficiency coefficient close to one, Bouttes et al. (2010) demonstrated that brines are able to explain up to 40 ppm CO_2 decrease. We have implemented this mechanism in combination with stratification-dependent vertical diffusivity in our version of the CLIMBER-2 model and got results qualitatively similar to Bouttes et al. (2010).

While we think that the brine rejection mechanism belongs to a class of plausible mechanisms contributing to glacial CO_2 drawdown, we did not use brine parameterization in our simulations for several reasons. Firstly, the parameterization for brine rejection cannot be tested against observational data. For present day climate conditions, brine rejections efficiency should be below 0.1, otherwise modern Antarctic bottom water becomes saltier than the North Atlantic deep water which is ~~in~~ at odds with reality. This means that to be an efficient mechanisms for glacial CO_2 drawdown, the brine efficiency should increase under glacial conditions at least by an order of magnitude. Whether this is physically plausible is not clear. The only paleoclimate constraint on the brine efficiency is reconstruction of paleosalinity based on the pore water (Adkins, et al. 2002) which suggests increase of deep water salinity in the Southern Ocean by more than 2 psu during the LGM. Such increase in salinity is indeed difficult to reproduce without contribution of brines. ~~–~~However the accuracy of salinity reconstruction based on such method remains uncertain (Wunsch, 2016). Second, there is a problem with temporal dynamics of brine rejection efficiency. Mariotti et al. (2016) assumed abrupt decrease of brine rejection efficiency from 0.7 to 0 in a very short interval between 18 and 16 ka. However, both sea level and the size of Antarctic ice sheets were essentially constant during this period and therefore there is no obvious reason for such large variations in the brine rejection efficiency. According to the interpretation of Roberts et al. (2016), brines rejection remained efficient during most of glacial termination and ceased only after 11 ka when most of glacial-interglacial CO_2 rise has been already accomplished. In the view of these uncertainties, we decided not to include parameterizations of brine rejection mechanism in simulations of glacial cycles. However, for simulations of the last glacial termination discussed below, we analysed potential effect of brine rejection on radiocarbon and other paleoclimate proxies.

5. Simulations of the Termination I

5.1 Simulation of climate, CO_2, and carbon isotopes during the last termination

The last glacial termination provides a wealth of paleoclimate records with a potential to better constrain the mechanisms of glacial CO_2 variability. In this section, we discuss the last glacial terminations in more detail. Similarly to the previous section, to exclude nonlinear interaction with ice sheets, we discuss here only one-way coupled experiments. To reduce computational time, we performed experiments only for the last 130,000 years starting from the Eemian interglacial and using the same initial conditions as in the experiments discussed above.

In the standard ONE_1.0_130K experiment, the model simulates climate variability across the Termination I rather realistically. In particular, it reproduces temporal resumption of the AMOC in the middle of the termination resembling Bølling-Allerød ~~Bolling-Allerod~~ warm event (Fig. ~~9a~~10a). The timing of this event in our model is shifted by ca 1000 years compared to the paleoclimate records. Results of our experiments reveal a high sensitivity of the timing of the AMOC resumption to the magnitude of freshwater flux. A change of the flux by just 2% in the ONE_0.98_130K experiment significantly alters millennial scale variability during the last glacial termination (Fig. ~~9~~10). This result suggests that simulated millennial scale variability during the Termination I is not robust, i.e. it is unlikely that a single model run through the glacial termination would reproduce the right timing or even the right sequence of millennial-scale events.

Although simulated CO_2 concentration at LGM and pre-industrial state are close to observations, simulated CO_2 appreciably lags behind reconstructed CO_2 during the termination (Fig. ~~9b~~10b). This is primarily related to the fact that simulated CO_2 does not start to grow at ca. 18 ka BP as reconstructed, but only after the end of simulated analogue of Bølling-Allerød ~~Bolling-Allerod~~ event. At the same time, in agreement with paleoclimate reconstructions, CO_2 concentration reaches a local maximum at the end of the North Atlantic cold event, which resembles the Younger Dryas. Simulated CO_2 concentration also reveals continuous CO_2 rise during Holocene towards preindustrial value of 280 ppm. This result confirm that such CO_2 dynamics could be explained by only natural mechanisms and does not require early anthropogenic CO_2 emissions until ca. 2 ka (Kleinen et al., 2016). This result also demonstrates that temporal dynamics of CO_2 during interglacials critically depends on the timing of final AMOC recovering. Late recovery during glacial termination causes strong overshoot of CO_2 at the beginning of interglacial following by some decrease or stable CO_2 concentration. ~~At the~~

~~same time earlier~~However, if the complete AMOC recovery occurs well before the end of termination, ~~causes~~ only temporal CO_2 overshoot occurs and ~~leads to continuous~~ CO_2 continues to rise during the entire interglacial.

It is assumed that atmospheric $\delta^{13}C$ provides useful constraint on the mechanisms of deglacial CO_2 rise (Schmitt et al., 2012; Joos et al., 2004; Fischer et al., 2010). Simulated atmospheric $\delta^{13}C$ drops from the LGM level of about -6.4‰ to the minimum value of -6.7‰ between 16 and 14 ka (Fig. ~~9c~~10c). This is primarily related to the reduction of marine biological productivity which, in turn, is explained by the decrease of iron fertilization effect over the Southern Ocean during the first part of Termination I. The magnitude of the $\delta^{13}C$ drop is in a good agreement with empirical data (Fig. ~~9c~~10c). The model is also able to simulate W-shaped $\delta^{13}C$ evolution associated with reorganization of the AMOC, however, this W-shape is shifted in time comparing to the reconstructed one by ca. 1000 years because model analogue of Bølling-Allerød event occurs earlier than the real one by the same amount of time. Note that this local maximum in $\delta^{13}C$ is completely absent in the experiment ONE_1.1_130K where temporal resumption of the AMOC during glacial termination does not occur. $\delta^{13}C$ rise after 12 ka is primary attributed to the accumulation of carbon in terrestrial carbon pools (forest regrowth and peat accumulation). At the same time, simulated present-day atmospheric $\delta^{13}C$ is underestimated compared to ice-core data by ca. 0.~~2~~15‰.

The model simulates almost monotonous decrease of atmospheric $\Delta^{14}C$ from the LGM to present. Most of this decrease (ca. 200‰) is caused by prescribed production rate which was about 20% higher during LGM. Only about 80‰ of $\Delta^{14}C$ is attributed to difference in climate state between LGM and present, primarily, due to less ventilated deep ocean. As shown in Fig. ~~9d~~10d, simulated atmospheric $\Delta^{14}C$ is significantly underestimated before 12 ka compared to reconstruction by Reimer et al. (2013) and at LGM this difference reaches more than 100‰. It is possible but unlikely that such big differences can be attributed to uncertainties in reconstructed production rate. An alternative hypothesis for explaining this mismatch is discussed below.

Fig. ~~10~~11 shows the LGM time slice anomalies and temporal evolution of $\delta^{13}C$ and ~~^{14}C~~ radiocarbon ventilation age during the Termination I in the Atlantic ocean simulated in the experiment ONE_1.0_130K. Spatial distribution of glacial anomalies and temporal dynamics of $\delta^{13}C$ and radiocarbon ventilation age ~~both carbon isotopes~~ during termination are qualitatively very similar. ~~Both show pronounced response at all depth to the millennial scale reorganization of the AMOC. Glacial $\delta^{13}C$ in the deep Atlantic at the LGM is by 0.6-1‰ lower than at present; that is primarily related to shoaling of the AMOC and reduced ventilation in the Southern Ocean.~~ Glacial $\delta^{13}C$ in the deep Atlantic at the LGM is by 0.6-1‰ lower than at present; that is primarily related to a shoaling of the AMOC and reduced ventilation in the Southern Ocean. The vertical distribution of $\delta^{13}C$ anomalies at the LGM is consistent with the paleoclimate reconstructions (e.g. Hesse et al., 2011).

Simulated ventilation age at the LGM can be directly compared with Skinner et al. (2017) (their Fig. 4a, c). Both models and data show significant increase of radiocarbon ventilation age in the deep Atlantic. However, the spatial patterns of ventilation age changes are rather different. In the model, the largest increase in the ventilation age occurs in the deep

Northern Atlantic which is explained by a shoaling of the AMOC cell and an increased presence of the poorly ventilated Southern Ocean water masses. At the same time, the data for the deep North Atlantic are characterised by very large scattering (from 1000 to 3000 ^{14}C years) and it is unclear whether their average values can be directly compared to the results of the zonally averaged ocean model. ~~At the LGM, a difference between ^{14}C is the upper ocean layer and at given depth, $\Delta\Delta^{14}$C, reaches ca 200‰ that corresponds to relative (to the upper layer) reservoir age of about 2000 years. This is in good agreement with Roberts et al.(2016). Both carbon isotopes show little changes during Holocene.~~

During glacial termination, both δ^{13}C and radiocarbon ventilation age show pronounced response at all depth to the millennial scale reorganizations of the AMOC (Fig. 12,b,d). The ventilation age in the deep Atlantic, which is about 2000 years prior to the model analogue of warm Bølling-Allerød event rapidly reaches nearly modern level after the AMOC resumption and drops again to glacial level during model analogue of the cold Younger Dryas event. Such evolution of ventilation age in the North Atlantic is in a good agreement with paleoclimate reconstructions (Robinson et al., 2005; Skinner et al., 2014).

5.2 Brine rejection mechanisms and radiocarbon in the ocean and atmosphere

As discussed above, our version of the CLIMBER-2 model is not able to reproduce accurately atmospheric ^{14}C decline during the first part of glacial termination. At the same time, Mariotti et al. (2016) demonstrated that their version of CLIMBER-2, which incorporates mechanism of brine rejection, is able to simulate larger atmospheric ^{14}C decrease from LGM till present, consistently with observational data (Reimer et al., 2013). By introducing similar parameterization for brine rejection and stratification-dependent vertical diffusivity in our model, we are able to reproduce results similar to Mariotti et al. (2016) (Fig. ~~11~~12). It is noteworthy that we use different temporal dynamics of the efficiency of brine rejections during termination. Instead of abrupt and non-monotonous changes in the brine efficiency prescribed in Mariotti et al. (2016), in the ONE_BRINE_130K experiment we assume that this efficiency is 0.75 at the LGM, 0 at present, and in between it follows global ice volume. We do not claim that this scenario is more realistic, but at least it is more consistent with the findings of Roberts et al. (2016). Fig. ~~10~~11 shows that the model with brine rejection and stratification-dependent vertical diffusivity simulates atmospheric Δ^{14}C in better agreement with empirical data then the standard version. This is explained by the fact that brine rejection in combination with stratification-dependent vertical mixing produces very salty and dense deep water masses which are almost completely isolated from the surface. Comparison of the vertical profiles of ventilation age (Fig. 13) with the basin averaged data from Skinner et al. (2017) shows that in the Atlantic and Pacific oceans, even the standard model version overestimates the radiocarbon ventilation age of glacial water masses. In turn, the model version with the brine rejection parameterization simulates water masses which are by 500 to 1000 years older than in the standard version. Only in the Southern Ocean the reconstructed ventilation age is consistent with both models version. As the result, the standard model version simulates ca 800 ^{14}C yr increase in glacial ocean ventilation age at the LGM which is in a good agreement with 689 ± 53 ^{14}C yr reported in Skinner et al. (2017). At the same time, the model with brine rejection simulates the increase in the ventilation age by more than 1300 ^{14}C yr.

~~In response, $\Delta\Delta^{14}$C in the deep Atlantic drops below -400‰ (in Mariotti et al. (2016) it drops even below -500‰). Such low $\Delta\Delta^{14}$C corresponds to relative (to the surface) ventilation age of 3000 to 4000 years both in Atlantic and Pacific. Similar ventilation age values simulated for South Ocean and deep Pacific (Fig. 12b) are much older than suggested by Freeman et al. (2016).~~

~~-~~Interestingly, the two model versions do not differ much in respect of simulated deep ocean δ^{13}C. At last, the two model versions differ significantly in respect of the deep South Ocean salinity. Change in salinity in the standard model version is only about 1 psu which is close to the global mean salinity change due to ice sheets growth. The model version with the brine rejection parameterization simulates glacial deep South Ocean salinity of above 37 psu which is in a good agreement with the reconstruction by Adkins et al. (2002). Thus we found that including additional effects (brines and stratification-dependent diffusion) helps to bring atmospheric Δ^{14}C and the deep South Ocean salinity in better agreement with available reconstructions but in expense of very old (likely to be ~~in odd~~at odds with paleoclimate data) water masses in the deep ocean. Of course, these results are obtained with a very simplistic ocean component and it is possible that more realistic ocean models would be able to resolve this apparent contradiction.

5.3 Changes in the terrestrial carbon cycle

The evolution of the carbon cycle in the "offline" simulation is presented on Figure 13. The "conventional" components of the land carbon cycle (vegetation biomass, soil carbon stored in non-frozen and non-flooded environment) change between 1400 GtC during glacial maxima and 2000 GtC during interglacial peaks. Such an amplitude of 600 GtC of glacial-interglacial changes is typical for the models of the land carbon cycle without long term-components (Kaplan et al., 2002; Joos et al., 2004; Brovkin et al., 2002). However, when we account for permafrost, peat, and buried carbon, the magnitude is decreasing to 300-400 GtC. This is due to counteracting effect of the permafrost and buried carbon pools relative to the conventional components. Both these pools vary between 0 and 350 GtC and reach their maxima during glacials. The peat storage also reaches about 350 GtC, but it grows only during interglacials or warm stadials. Let us note that during glacial inceptions, while biomass and mineral soil carbon decrease, terrestrial carbon storage increases due to an increase in buried and permafrost carbon. As a result, total land carbon did not change much during the period of large ice sheet initiation.

During the last deglaciation (Fig. ~~13~~14, right), the peat storages increase monotonically reaching ca. 350 GtC at pre-industrial. The conventional carbon pools increase from 1400 to 1800 GtC at the peak of interglacial (ca. 9 kyr BP), and then start to decline due to orbital forcing effect on climate in northern hemisphere. The permafrost and buried carbon pools show opposite behaviour, experiencing minimum at 10 and 5 ka, respectively, and grow afterwards. The combined effect on the total land carbon is a monotonic increase during interglacials, mostly because of peat accumulation.

6. ~~Conclusions~~ Discussion and conclusions

We present here the first simulations of the last four glacial cycles with one-way and two-way coupled carbon cycle model. The model is able to reproduce the major aspects of glacial-interglacial variability of climate, ice sheets and of atmospheric CO_2 concentration even when driven by orbital forcing alone. These results provide strong support to the idea that long and strongly asymmetric glacial cycles of the late Quaternary represent a direct but strongly nonlinear response of the Northern Hemisphere ice sheets to the orbital forcing which, in turn, is amplified and globalized by the carbon cycle feedback. ~~In particular, t~~

The model simulates correct timing of the past glacial terminations in terms of ice volume while the ~~and dominant 100-kyr cyclicity of the past glacial cycles~~simulated CO_2 concentration lags behind the reconstructed one by several thousand years. The model is also able to simulate temporal evolution of the stable carbon isotope in the ocean. At the same time, the agreement between simulated and reconstructed atmospheric $\delta^{13}C$ is rather poor. Similarly,

~~The model correctly simulates climate and carbon cycle evolution across the last glacial terminations. In particular, ocean carbon isotopes evolution is simulated in agreement with empirical data. At the same time, simulated CO_2 lags behind reconstructed one during glacial termination and~~ the magnitude of simulated atmospheric ^{14}C decline during the last glacial termination is underestimated. Introducing the brine rejection parameterization and stratification-dependent diapycnal diffusivity allows us to improve the agreement for ~~the~~ atmospheric ^{14}C but leads to unrealistically "old" glacial deep ocean water masses.

Temporal dynamics of CO_2 during interglacial depends strongly on the timing of the AMOC recovering during glacial termination. If the AMOC recovers only at the end of glacial termination, CO_2 concentration experiences the overshoot at the beginning of interglacial and then CO_2 declines. To the contrary, early recovery of the AMOC~~,~~ leads to monotonous rise of CO_2 during interglacials.

~~Glacial~~ In our simulations, millennial scale variability during the last glacial termination ~~similar to the Termination I~~ is very sensitive to magnitude of meltwater flux,~~-~~ and the sequence and timing of simulated millennial scale events are not robust even when the model is forced by prescribed radiative forcing of GHGs. ~~are very sensitive to magnitude of meltwater flux.~~

Adding new long-term carbon pools (peat, buried and permafrost carbon) decreases ~~an~~ the amplitude of glacial-interglacial changes in the total land carbon storage. It helps to reduce an effect of terrestrial biosphere on the CO_2 change during glacial inception and to lesser extent during glacial terminations.

This work demonstrates that simulation of glacial cycles with Earth system models driven by orbital forcing alone is possible. This does not mean that we presented here the ultimate solution for ~~-~~the accurate recipes for all processes and feedbacks and, in particular, for "the carbon stew". The understanding of how global carbon cycle operates on orbital and suborbital time scales still remains incomplete and large uncertainties remain in the choice of individual processes and their parameterisations. Paleoclimate data provide some useful constrains but the proxy data syntheses are in the state far from being perfect, with some proxies telling contradicting stories and others are difficult to interpret.

The CLIMBER-2 model is rather simple and coarse resolution Erath system model. This allows us to perform a large ensemble of model simulations on orbital and even longer time scales. Obviously, such fast model has significant limitations, in particular, it employs the zonally averaged ocean model. Many essential processes, such as iron fertilization effect, are parameterized. The development of a high-resolution state-of-the-art Earth system model suitable for simulation of the interaction between climate, ice sheets and carbon cycle at the orbital time scales is absolutely crucial to make the next step forward in understanding of the Earth system dynamics during Quaternary.

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Appendixes

A1. Modifications of terrestrial carbon cycle model

The old version of the CLIMBER-2 carbon cycle module described in Brovkin et al (2002) considers two vegetation types – trees and grass. Each of two vegetation types has four carbon pools – leaves, stems, fast and slow soil carbon. Each of these four pools occupies the same fraction of grid cell as the respective vegetation type. Crichton et al. (2014) in their version of permafrost carbon implementation into CLIMBER-2 have not changed the pool structure but modified turnover time, assuming that it is increasing under permafrost conditions. In the new version of the carbon cycle module which we use in present work, we introduced three new carbon pools: boreal peat, permafrost, and carbon buried under ice sheets (Fig. A1). The fractions of land covered by grass and trees are computed in the vegetation model following (Brovkin et al., 1997), the fraction of land covered by ice sheets is computed by the ice sheet model and the fraction of permafrost f_{pm} for the temperature range -5°C < T_{ts} < 5°C is computed in the land surface module as

$$f_{pm} = 0.5 - 0.1T_{ts},$$

where T_{ts} is annual mean top soil layer temperature. It is assumed that grass (in boreal latitudes this mean tundra) is located north of forest and therefore freezes first. Only if permafrost exceeds grass fraction, the permafrost can expand over the area covered with trees. During the ice sheet growth, all carbon under ice sheets apart from the living biomass is re-allocated into the buried carbon pool. Buried carbon remains intact till it is covered by ice sheets. During deglaciation, this buried carbon is transformed into the permafrost pool. Fraction of land covered by peat is define as

$$f_{pt} = f_{pt}^{*}(1 - f_{gc} - f_{pm}),$$

where f_{pt}^{*} is the potential fraction of peat for each grid cell prescribed from modern observational data and f_{gc} id the faction of land covered by ice sheets. Note that we do not consider peatlands in low latitudes. Although peat and permafrost have certain areal fractions, they are considered to be parts of grid cell covered by vegetation. Net primary production and fluxes between the fast carbon pools (leaves, stems and fast soil pool) are computed the same way as in Brovkin et al. (2012). The downward flux of carbon from the fast soil is partitioned between slow soil pool and permafrost proportionally to their relative factions. The rate of peat accumulation is equal to a fixed fraction of net primary production in the respective vegetation type. Evolution of carbon content p_i in slow carbon pools is described by the equation

$$\frac{dp_i}{dt} = q_i f_i + \frac{df_i}{dt} b_i' ,$$

where $p_i = b_i f_i$, and b_i is the concentration of carbon in the i-th carbon pool (in kgC/m^2), and f_i is the fraction of the i-th pool. Value q_i represents the difference between local accumulation and decay of carbon in the pool, and b_i' is carbon concentration in the pool by which i-th pool is expanded and $b_i' = b_i$ if i-th pool is shrinking. For peat $b_i' = 0$. For the permafrost, the situation is more complex, because it can gain/lost carbon from/to slow soil, peat and buried carbon pools. The source terms for the permafrost pool q_{pm} consists of the sum of fluxes from the fast grassland and tree fast soil pools into the respective slow pools (see Brovkin et al., 2002 for detail) minus the decay term, where decay time scale is set to 20,000 years. Apart from carbon, terrestrial carbon model also computes carbon isotopes (^{13}C and ^{14}C) contents in all carbon pools. Since carbon isotopes are also computed in the oceanic carbon cycle model, we can compute δ^{13}C and δ^{14}C in the atmosphere and compare modeling results with available paleoclimate data.

A2 Modifications of the ocean carbon cycle module

A2.1. Dust deposition in the Southern Ocean

In the one-way coupled experiments, similar to Brovkin et al. (2012), we used the concentration of eolian dust in the Antarctic ice cores as the proxy for iron deposition over the Southern Ocean. Such choice is supported by recent measurements of iron content in the Southern Ocean sediments core (Lamy et al., 2014). In the fully interactive run, the iron flux over the Southern Ocean (D) in arbitrary units is parameterized through the global sea level change as

$$D = (100\frac{dS}{dt} + 10)\max(S - 50;\, 0) + 1.5S \ ,$$

where S is the ice volume expressed in meters of sea level equivalent and time t is in years. This formula gives significant increase in iron flux for the case when sea level drops below 50 m, that is likely related to the fact that Patagonian dust source is very sensitive to the area of exposed shelf and glacial erosion processes. Numerical parameters in this formula were obtained by fitting simulated D to the dust concentration in Antarctic record. This allows us to use the same parameterization for the iron fertilization effect in one-way and fully interactive experiments. To prevent large fluctuations in the iron flux related to fluctuations of time derivative of S, the dust deposition D computed by this equation has been smoothed by applying relaxation procedure. Namely, at each time step n, the dust deposition D_n is computed as

$$D_n = (1-\varepsilon)D + \varepsilon D_{n-1}$$

Where ε=0.001 which is approximately equivalent to introducing of 1000 years filter.

A2.2 Dependence of remineralization depth on temperature

In CLIMBER-2 the vertical profile of carbon below the euphotic zone is given by the formula

$$f(z) = \left(\frac{z}{z_r}\right)^{0.858}$$

where remineralization depth z_r is hold constant equal to 100 m. To take into account dependence of remineralization rate on ambient temperature, following (Segschneider and Bendtsen, 2013) we now use dependence of z_r on the thermocline temperature (300m) T:

$$z_r = z_{ro}\ 2^{\frac{T-T_0}{10}},$$

where T_0= 9^{o}C and z_{ro}=100 m. The value of T_0 was selected such that introducing of temperature-dependent remineralization depth does not affect atmospheric CO_2 concentration under preindustrial climate conditions. During glacial times temperature in the thermocline decreases by 2-3^{o}C which causes increase of z_r by 20-30%. This results in additional CO_2 drawdown by ca. 15 ppm.

A2.3 Parameterization of iron fertilization effect

The rate of dust deposition which is prescribed from the ice cores in one-way coupled experiment or computed from global ice volume in fully interactive experiments is considered to be a proxy for iron flux and is used in parameterization of iron fertilization mechanism. This parameterization is only applied to the Southern Ocean (south of 40^{o}S). As described in Brovkin et al. (2002), net primary production of phytoplankton Π in the model is described as

$$\Pi = c(D)\, r(T,R) \frac{P}{P+P_0} C_p (1-f),$$

where C_p is the phytoplankton concentration, P is the phosphorus concentration in euphotic zone, r is a function of temperature T and photosynthetic active insolation R, f is the fraction of grid cell covered by sea ice, P_o is a constant and c is a function of normalized dust deposition rate D. Note that in the case of prescribed dust deposition rate, D was obtained by multiplying observed dust concentration in mg/g units by factor 10^{-3} . North of 40°S, parameter c is set to 1 and south of 40°S

$$c = \min(1, 0.1 + c_d D) .$$

With c_d=2, during glacial maxima the value c reaches one thta implies that at these periods there is no iron limitation in the Southern Ocean. During interglacials, when D is much smaller than 100, c is close 0.1. Parameters of this parameterization were selected to reproduce present-day nutrients concentration in the Southern Ocean, and to obtain about 20-30 ppm additional CO_2 drop during glacial maxima due to the iron fertilization effect.

A3. Variable volcanic outgassing

Following the idea by Huybers and Langmuir (2009) which has been tested already in Roth and Joos (2012), we introduced a dependence of volcanic CO_2 outgassing O on the rate of sea level change. Namely, we assume that volcanic outgassing linearly depends on sea level derivative with the time delay of about 5000 years:

$$O(t) = O_1\left(1 - O_2 \frac{dS(t-5000)}{dt}\right).$$

Here O_1=5.3 Tmol C yr^{-1}, and O_2=50 Tmol C m^{-1}. With these parameters volcanic outgassing does not change by more than 30% during all glacial cycles. Note, that over one glacial cycle the average value of O is very close to O_1.

A4. Modifications of the energy and surface mass balance interface

In our previous simulations with CLIMBER-2 we found that if maximum ice sheet volume in the Northern Hemisphere exceeds 100 m, the AMOC remains in the off mode during the entire deglaciation. Although it may be realistic for some recent deglaciations, such long AMOC shutdown prevents simulation of complete deglaciation of North America. This is related to the fact that due to a very coarse spatial resolution of CLIMBER-2 linear interpolation of surface temperature between neighbouring sectors (American and Atlantic) cause a strong cooling over eastern Part of Laurentide ice sheet doe to the AMOC shutdown (see for example Arz et al., 2007). In the high resolution climate models, the effect of AMOC shutdown on North America is rather limited compare to Europe (e.g. Zhang et al., 2014; Swingedouw et al., 2009). This is explained predominantly eastward direction of air masses transport. To compensate this resolution-related problem we made magnitude of temperature anomaly correction over eastern North America (see Fig. 2 in Ganopolski et al., 2010) dependent on the strength of the AMOC. Namely, for the AMOC strength below 10Sv, the amplitude of temperature correction is scaled down by factor $\Psi_{max}/10$, where Ψ_{max} is the maximum of the meridional overturning stream function (in Sv, 1 Sv=10^6 m^3/s) in the Atlantic Ocean. With this parameterization, during complete shutdown of the AMOC cooling over eastern North America is compensated by reducing of temperature correction. Introducing of this procedure minimizes the impact of the AMOC on Laurentide ice sheets mass balance. As the result, even prolonged AMOC shutdown does not prevent complete melting of the Laurentide ice sheet during glacial terminations.

Table 1. Model experiments performed in this study. P denotes prescribed characteristic, I - interactive, STD - standard model configuration, RD - variable remineralization depth, VO – variable volcanic outgassing, IF - iron fertilization in the South Ocean, TC – terrestrial carbon cycle, BR - brine rejection mechanism. Minus sign means that the process is excluded and plus sign means that process is included. Ice sheets are interactive in all simulations.

Experiment	Radiative forcing of GHGs	Southern Ocean dust	Atlantic freshwater factor	Modell configuration
		400,000 yr experiments		
ONE_1.0	P	P	1	STD
ONE_1.1	P	P	1.1	STD
ONE_S1	P	P	1.1	STD-RD
ONE_S2	P	P	1.1	STD-RD-VO
ONE_S3	P	P	1.1	STD-RD-VO-IF
ONE_S4	P	P	1.1	STD-RD-VO-IF-TC
ONE_240	240 ppm	P	1	STD
INTER_1.0	I	I	1	STD
INTER_1.1	I	I	1.1	STD
		130,000 yr experiments		
ONE_1.0_130K	P	P	1	STD
ONE_~~0.98~~1.1__130K	P	P	~~0.98~~1.1	STD
ONE_BRINE_130K	P	P	1	STD+BR

Table 2. "The carbon stew" at the LGM and the entire 400 kyr period

	LGM (22-19 ka) ppm	400-0 ka ppm
Physical process + carbonate chemistry*)	57	38
Ocean volume change	-12	-6
Terrestrial carbon storage	-13	-8
Iron fertilization	22	6
Remineralization depth	15	10
Volcanic outgassing	8	3
Total	77	43

*) deep ocean and shallow water carbonate sediments, carbonate and silicate weathering

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Figures

a) **b)**

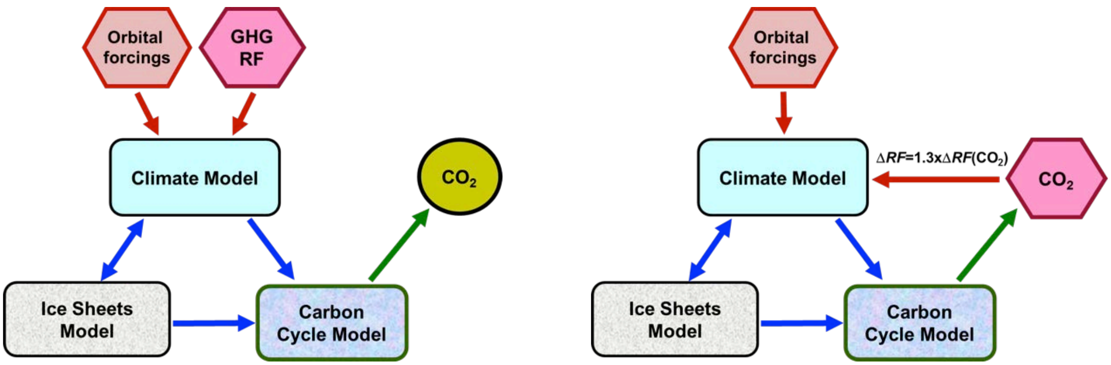


Figure 1. Coupling strategy. a) one-way coupled experiment; b) fully interactive experiment.

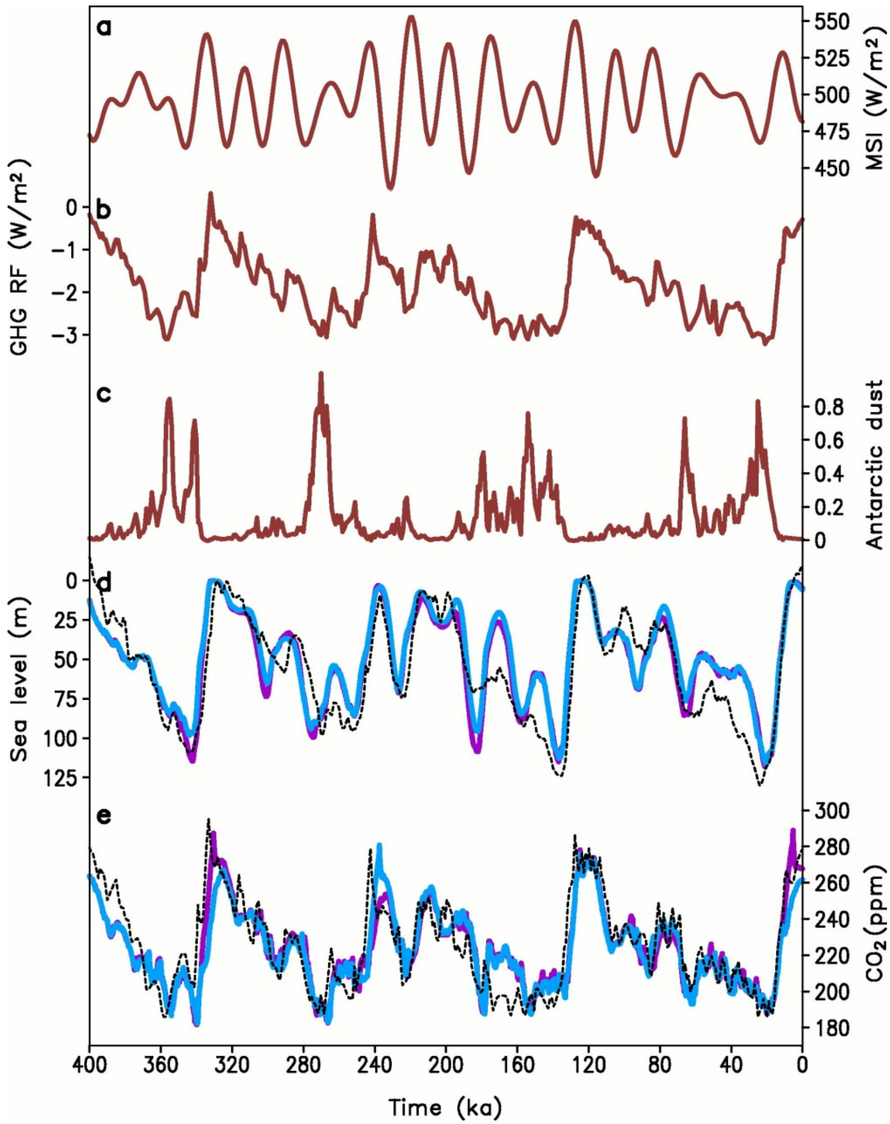


Figure 2. Transient simulations of the last four glacial cycles forced by orbital variations, observed concentration of well-mixed GHGs and dust deposition rate (one-way coupled experiments). a) Maximum summer insolation at 65°N, W/m^2; b) radiative forcing (relative to preindustrial) of well-mixed GHGs, W/m^2; c) Antarctic dust deposition rate in relative units; d) global ice volume expressed in sea level equivalent (m); e) atmospheric CO_2 concentration (ppm). Dark red colour in (a-c) represents prescribed forcings. Black dashed lines in (d) is sea level stack from Spratt and Lisiecki (2016), in (e) compiled Antarctic CO_2 record from Lüthi et al. (2008). Radiative forcing of GHGs in (b) is from Ganopolski and Calov (2011). Antarctic dust is from Augustin et al. (2004). Blue lines in (d, e) correspond to the baseline experiment ONE_1.0 and pink lines to the experiment ONE_1.1 where meltwater flux into Atlantic was scaled up by factor 1.1.

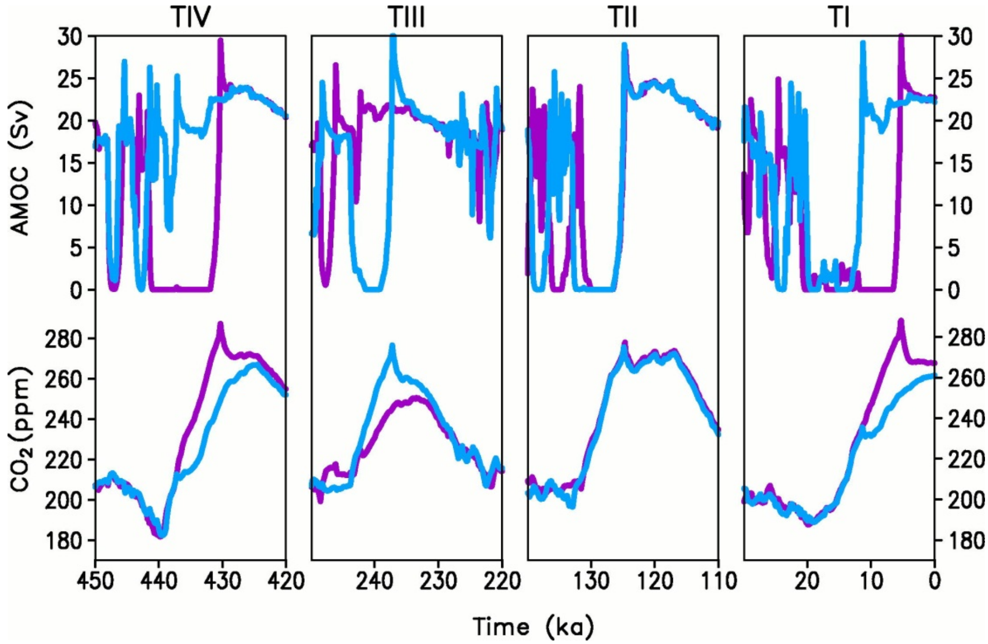


Figure 3. Temporal evolution of the AMOC, Sv (a), and atmospheric CO_2 concentration, ppm (b) during the last four glacial terminations. Blue lines correspond to the experiment ONE_1.0 and pink lines to the experiment ONE_1.1 where meltwater flux into Atlantic was scaled up by factor 1.1.

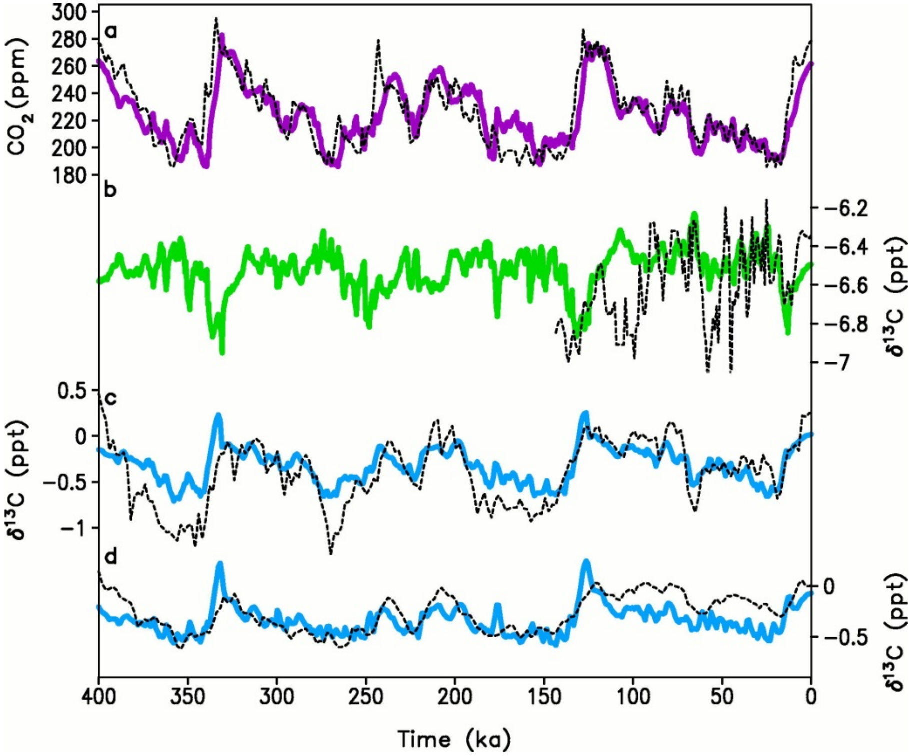
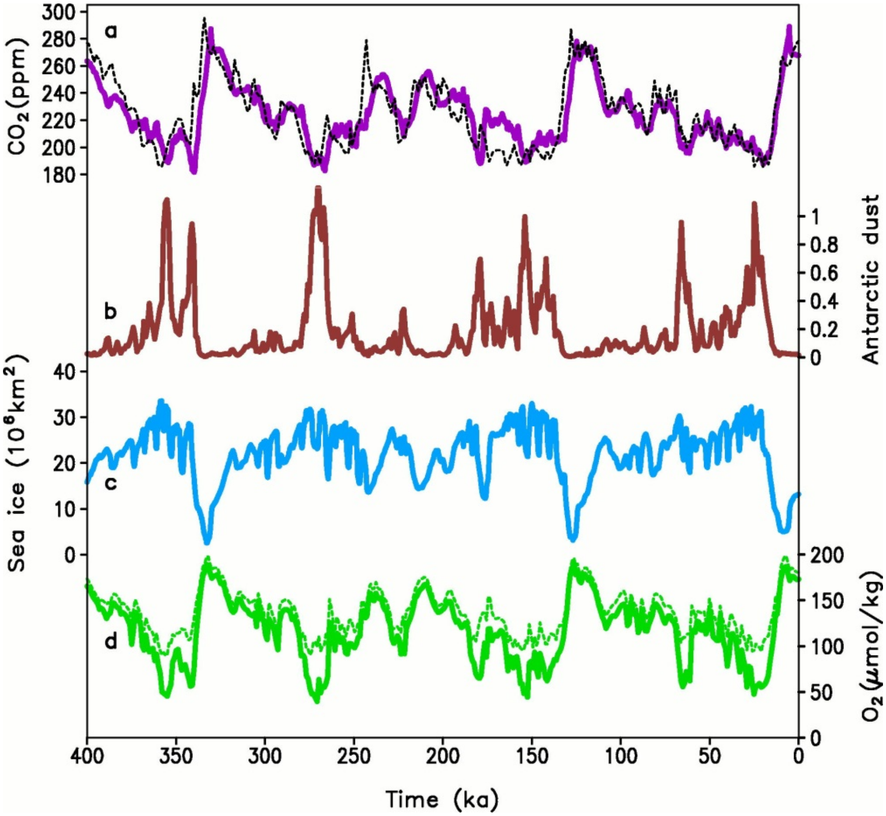


Figure 4. Simulated CO_2 and $\delta^{13}C$ with the one-way coupled model (ONE_1.0). a) CO_2 concentration (ppm) ref, b) atmospheric $\delta^{13}CO_2$ (‰), c) deep South Atlantic $\delta^{13}C$ (‰); d) deep North Pacific $\delta^{13}C$ (‰). Color lines – model results. Empirical data (black dashed lines): a) Lüthi et al. (2008); b) Egglestone et al. (2016); c) and d) Lisiecki et al. (2008).



5 Figure 5 (new) a) Simulated CO_2 concentration (ppm); b) prescribed Antarctic dust deposition rate in relative units; c) simulate annual mean sea ice area in the Southern Hemisphere (10^6 km^2);~~,~~ d) simulated oxygen concentration in the deep South Ocean in (μmol/kg) in the ONE_1.1 experiment (solid line) and the identical experiment but without iron fertilization effect (dashed line).

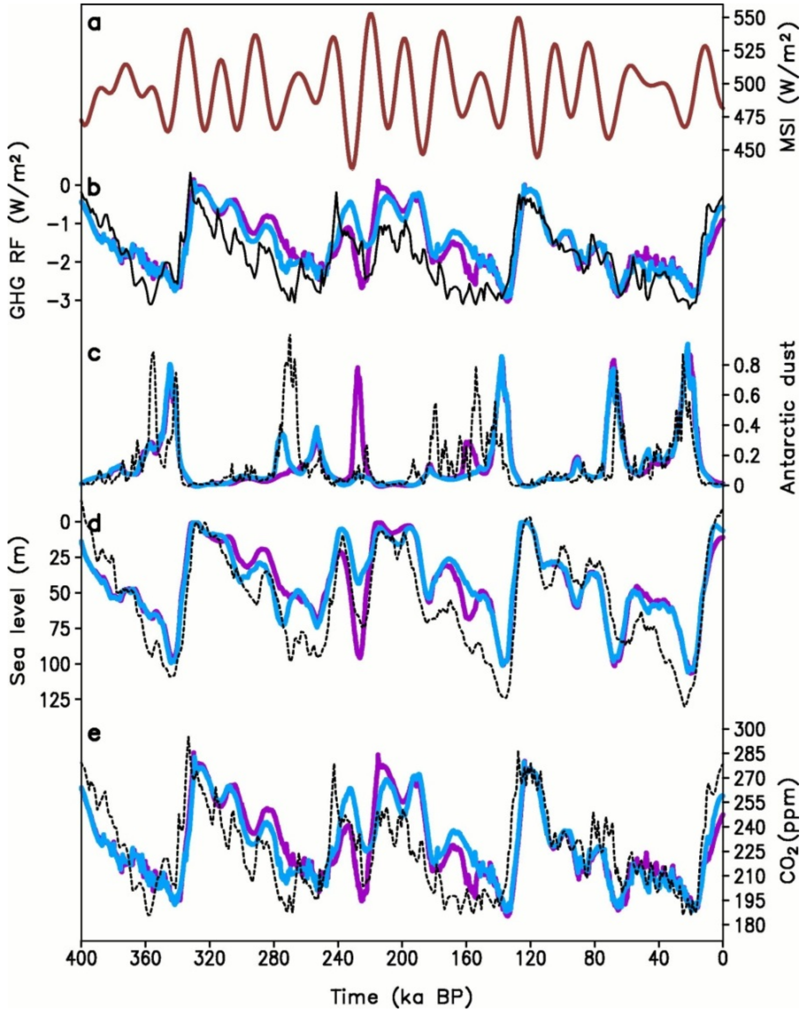


Figure ~~5~~6. Transient simulations of the last four glacial cycles forced by orbital variations only (fully interactive experiments) a) Maximum summer insolation at 65°N, W/m^2; b) radiative forcing (relative to preindustrial) of well-mixed GHGs, W/m^2; c) Antarctic dust deposition rate in relative units; d) global ice volume expressed in sea level equivalent, m; e) atmospheric CO_2 concentration, ppm. Black line in (b) is radiative forcing of GHGs from Ganopolski and Calov (2011). Black dashed lines in (c) is Antarctic dust is from (Augustin et al., 2004), in (d) is sea level stack from Spratt and Lisiecki (2016), in (e) compiled Antarctic CO_2 record from Lüthi et al. (2008). ~~Black lines in (b) and (c) are as forcings in Fig.2; (d) and (e) as data in Fig. 2.~~ Blue lines in (d, e) correspond to the fully interactive experiment INTER_1. and pink lines to the experiment INTER_1.1 where meltwater flux into Atlantic was scaled up by factor 1.1.

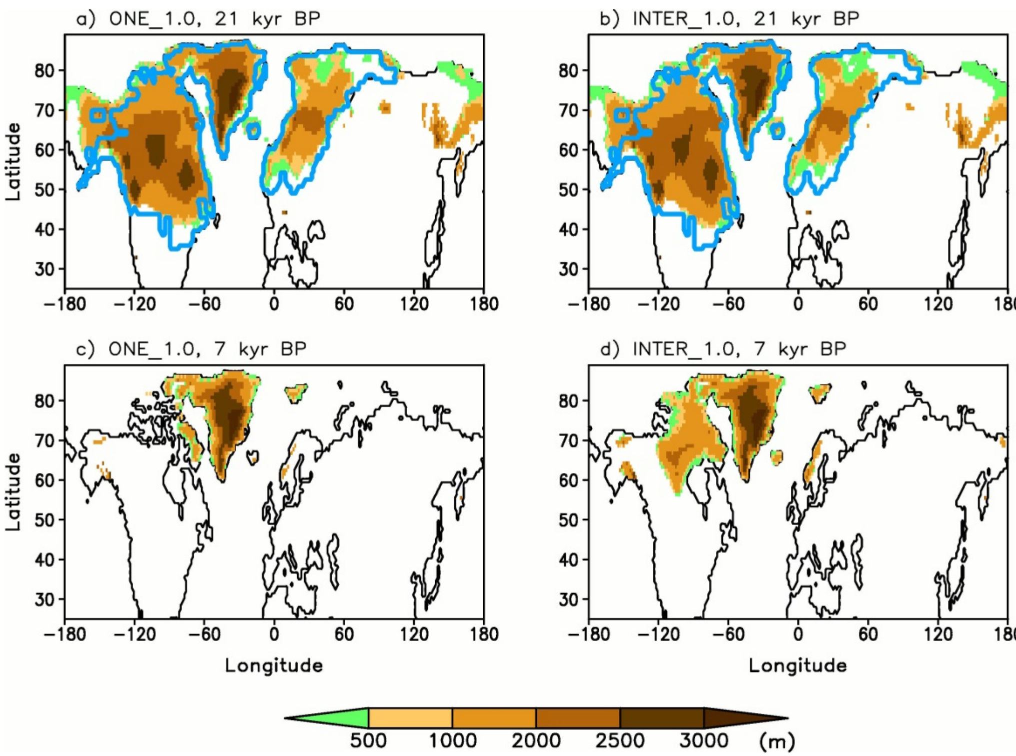


Figure ~~6~~7. Simulated ice sheets elevation, m, at 21 ka (a, b) and 7 ka (c,d) in the one-way coupled experiment ONE-1.0 (a, c) and fully interactive experiment INTER-1.0 (b, d). Blue lines represent Ice-5g reconstruction at the LGM (Peltier, 2004).

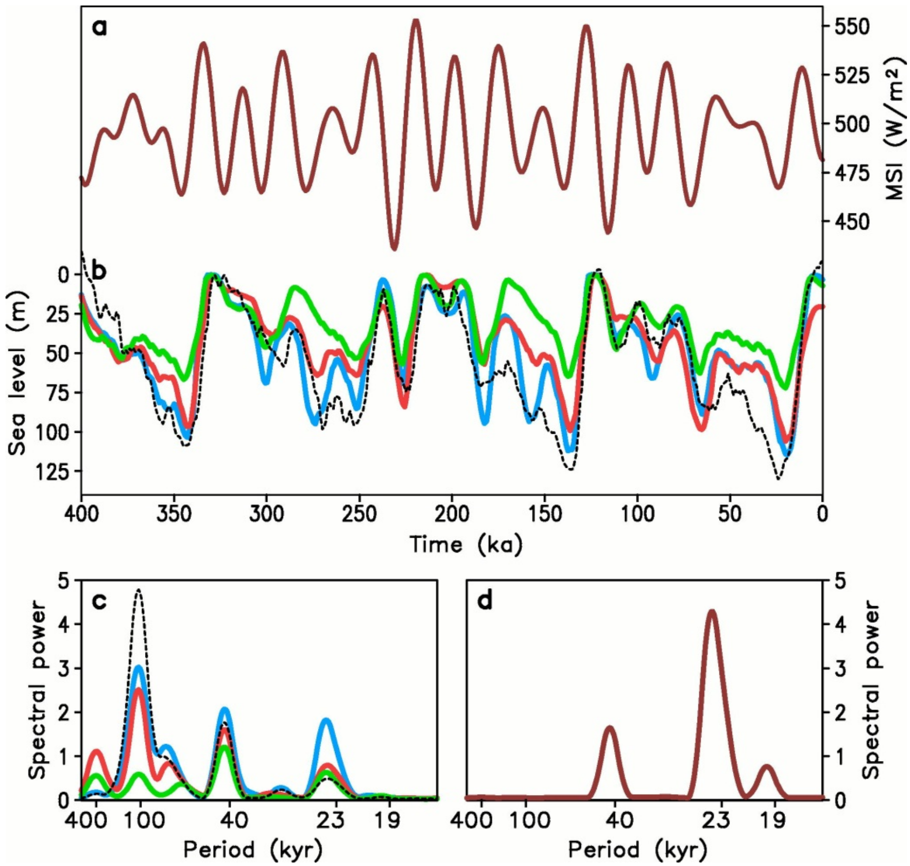


Figure ~~7~~8. Transient simulations of the last four glacial cycles forced by orbital variations, with prescribed, interactive and fixed concentrations of well-mixed GHGs. a) Maximum summer insolation at 65°N, W/m^2; (b) Temporal evolution of reconstructed and simulated sea level, m; (c~~b~~) ~~their~~ frequency spectra of the global ice volume; (d) frequency spectra of boreal summer insolation~~, kyr~~. Black line is for the data (Spratt and Lisiecki , 2016), blue line corresponds to the one-way coupled experiment ONE_1.0, red line to the fully interactive experiment INTER_1.0, and green line to the ONE_240 experiment with constant (240 ppm) CO_2 concentration; d) frequency spectra of orbital forcing

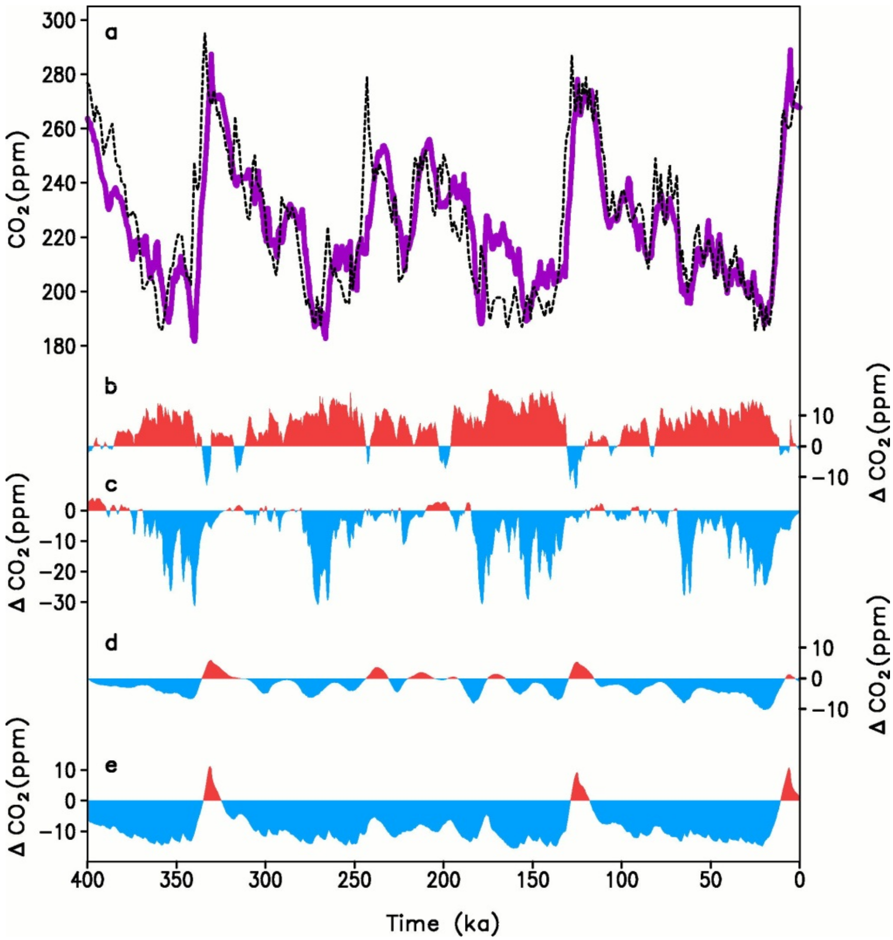


Figure 89. Results of factor separation analysis. a) Simulated CO_2 (ppm) in one-way coupled ONE_1.1 experiment (purple line) and reconstructed CO_2 concentrations (black dashed line, Lüthi et al., 2008). b-d): contributions to simulated atmospheric CO_2 (ppm) of terrestrial carbon cycle (b), ONE_S4 – ONE_S3; iron fertilization (c), ONE_S3 – ONE_S2; variable volcanic outgassing (d), ONE_S2 – ONE_S1; temperature-dependent remineralization depth (e), ONE_S1 – ONE_1.1.

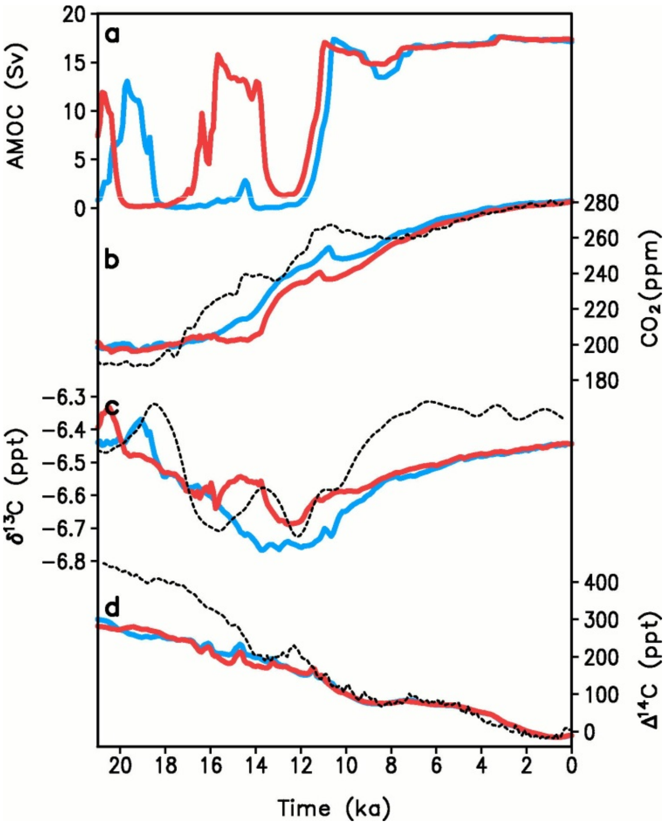


Figure ~~9~~10. Simulation of Termination I with the set of one-way coupled models which differs only by scaling of freshwater flux. Blue line corresponds to the ONE_1.1_130K experiment with scaling factor 1.1, red line – the ONE_1.0_130K experiment with scaling factor 1.0. a) AMOC strength, Sv; b) atmospheric CO_2, ppm; c) atmospheric $\delta^{13}CO_2$, ‰; d) atmospheric $\Delta^{14}CO_2$, ‰. Dashed lines: b-c) ice-core data (Lüthi et al., 2008; Schmitt et al, 2012); d) IntCal13 radiocarbon calibration curve (Reimer et al. 2013).

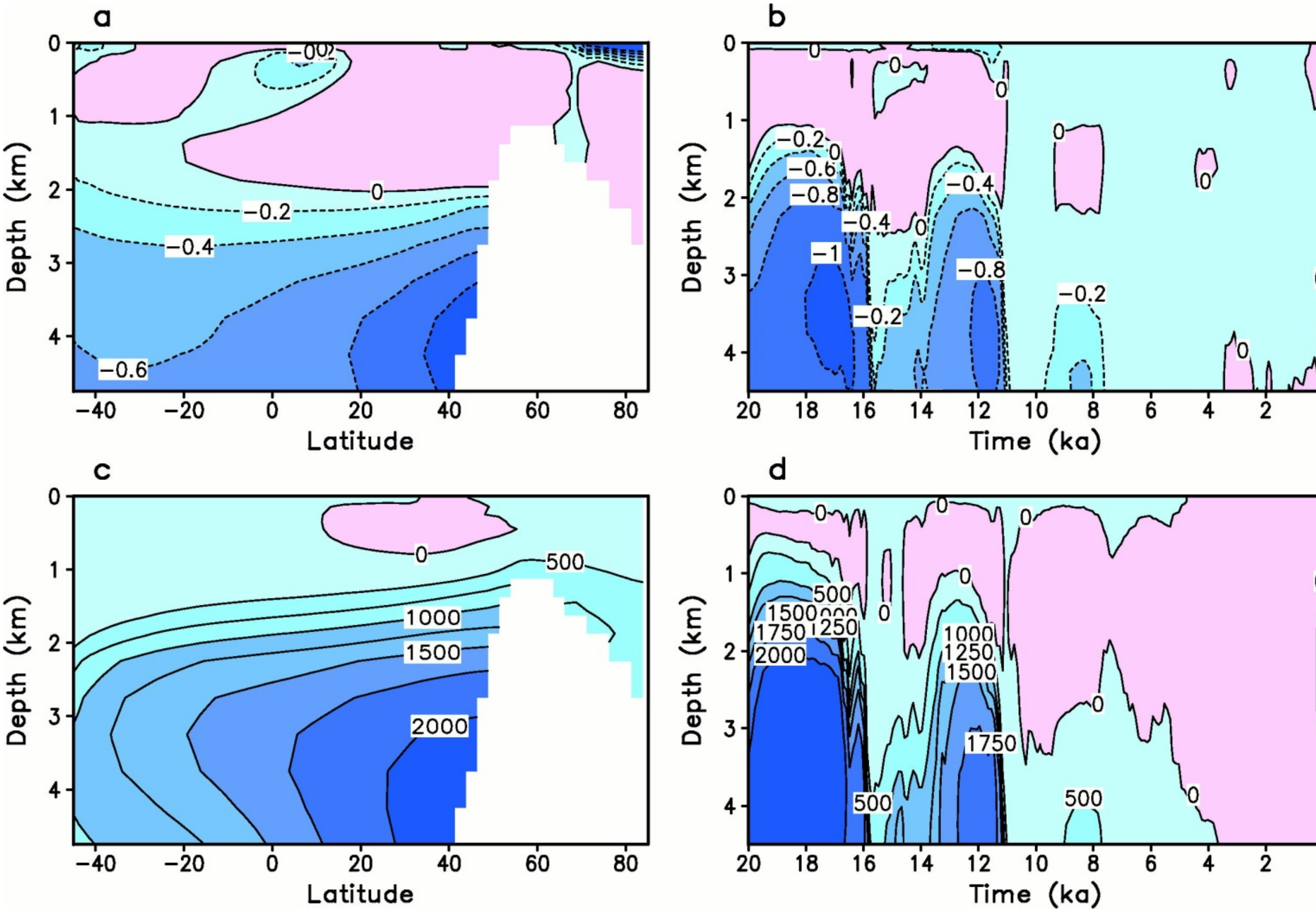


Figure ~~10~~11. δ^{13}C and radiocarbon ventilation age ~~Carbon isotopes~~ distribution in the Atlantic ocean in the ONE_1.0_130K simulation of Termination I. (a, b) δ^{13}C, ‰, (c, d) radiocarbon ventilation age in yr ^{14}C. (a, c) differences between LGM (21 ka) and pre-industrial in the Atlantic ocean. (b, d) temporal evolution of anomalies during the past 20 ka at 20°N in Atlantic.

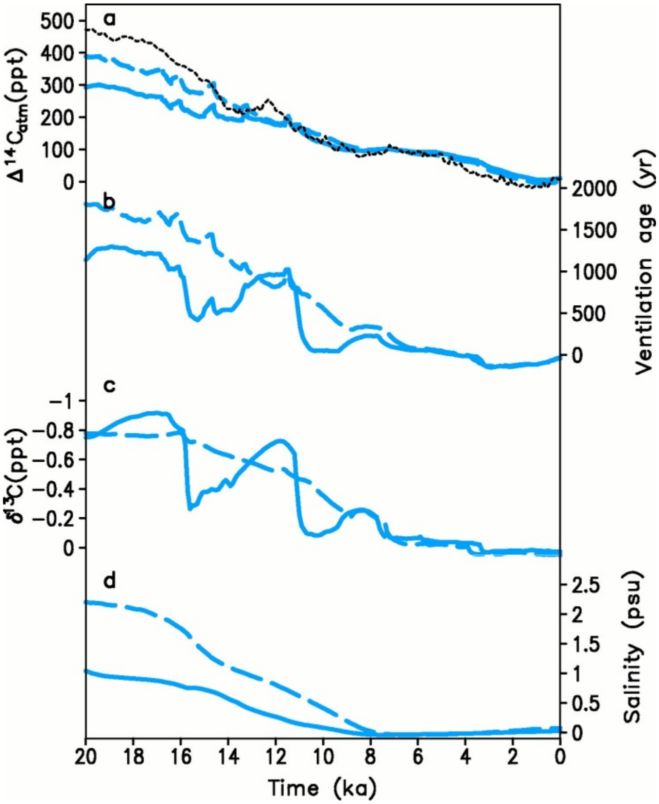


Figure ~~11~~12. Simulation of Termination I in the standard ONE_1.0_130K experiment (solid blue) and the ONE_BRINE_130K (dashed blue) experiment which includes brine parameterization and stratification-dependent vertical mixing (g). (a) atmospheric $\Delta^{14}C$ (in ‰). (b) Deep tropical Atlantic $\Delta\Delta^{14}C$, ‰. (c) Deep tropical Atlantic $\delta^{13}C$, ‰. (d) Deep Southern Ocean salinity, psu. (c-d) are for the depth 4 km. Black dashed line is IntCal13 radiocarbon calibration curve (Reimer et al. 2013).

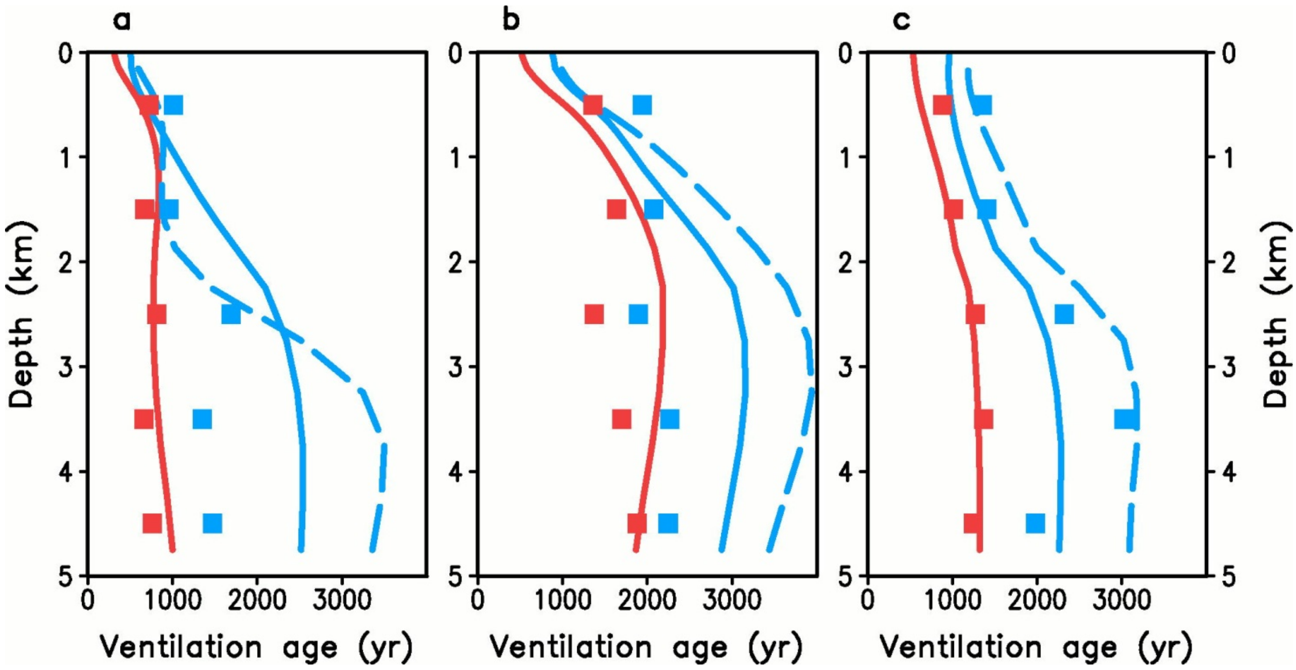


Figure ~~12~~13. Vertical profile of ventilation age in ^{14}C years for Atlantic (a), Pacific (b) and Southern Ocean (c). Red line represents modern conditions, solid blue – LGM in ONE_1.0_130K experiment using the standard version of the model, dashed blue – LGM in ONE_BRINE_130K experiment with the model version which includes brine parameterization and stratification-dependent vertical mixing. Red (blue) squares represent basin averaged radiocarbon age for modern (LGM) state based on the data from Skinner et al. (2017).

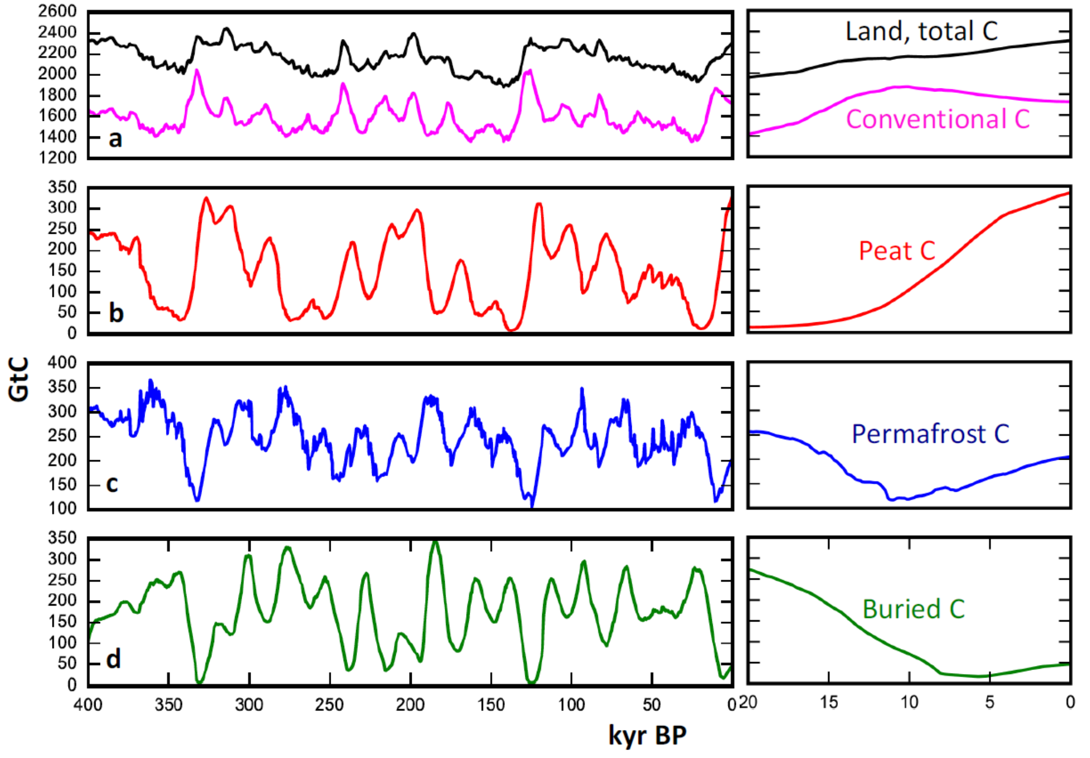


Figure ~~13~~14. Dynamics of terrestrial carbon pools (Gt C) in the one-way coupled ONE_1.0 simulation. Left, the whole 400 kyr period; right, the Termination I period. a) Black line – total carbon storage; magenta line - conventional carbon pools (biomass and mineral soils), (b-d): peat, permafrost, and buried carbon storages, respectively.