

Interactive comment on “Systematic study of the fresh water fluxes impact on the carbon cycle” by N. Bouttes et al.

Anonymous Referee #1

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This paper simulates impacts of changes in ocean circulation on the global carbon cycle in order to understand observed variability in atmospheric CO₂. The attempt of the authors seemed to be to repeat and review two previous papers on the same topic in order to clarify existing disagreements and therefore they attempt to give an overall view on the topic. While I think their own model simulations are worth publishing (although some more in-depth discussions are necessary) I think they fail on their overall target on giving the overall overview and in the clarification of the disagreements. This later point is based on various facts: (1) They miss out two important study on the same topic (Bozbiyik et al., 2011, Clim. Past, 7, 319–338; Obata, A. Climate–Cycle Model Response to Freshwater Discharge into the North Atlantic Journal of Climate, 2007, 20, 5962–5976), which for the first time used full GCMs, and not an EMIC as used here and in the two other previous studies. (2) Their analysis of the detailed changes in the

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carbon cycle is performed on a very aggregated level, but does not go to the details. For example, the studies which investigate changes in the terrestrial carbon cycle in most details were a study using the dynamical global vegetation model LPJ-DGVM (Köhler et al., 2005, Climate Dynamics, cited in the Bouttes paper, which includes dynamic vegetation, but no feedback from vegetation changes to climate physics) and the Bozbiyik et al. paper, with prescribed vegetation, but with feedbacks to the climate physics, see discussion in that paper). Both papers give very detailed analysis, why terrestrial carbon storage changed. In the Köhler paper the response was an interplay of a southwards shift in the northern treeline with the temperature-dependent change in the soil respiration fluxes. Depending on the background climate (amount of land ice sheets) the combination of both processes led to either a CO₂ peak (for present-day) or drop (LGM). The Bozbiyik paper finds more dynamics in the tropics connected with the ITCZ. This depth of analysis is missing in the Bouttes paper. It argues more on the phenomenal level, but is not going to the details.

Concerning the differences to Schmittner and Menviel (the two other papers on the same topic) it needs to be said, that Menviel uses the same vegetation model as Bouttes (VECODE) and this should be mentioned and discussed. In the description of the different scenarios the reader is sometimes confused if it is talked about “Menviel” or “Schmittner”, if the authors means the other papers or the own scenarios which are labelled similarly.

From my understanding the paper needs to improve to fulfil its targets. It needs to go beyond showing time series of changes in typical variables (such as temperature, precipitation, CO₂, terrestrial and marine C inventories) to a deeper understanding, what is really happening in the model. VECODE is much simpler than LPJ, (contains only three classes (trees, grass, nothing)) so maybe the changes in the terrestrial part are based on different issues (precipitation as mentioned?). For the ocean my understanding of the text so far is, that the argument of changes in the C cycle is mainly based on the solubility effect (colder ocean stores more C). Is this really explaining all? What

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about regional changes in NPP (biological pump). Furthermore, the authors need to tackle the results of the Bozbiyik paper. Bozbiyik for example finds, that the ocean C cycle comes to a new equilibrium, thus the ocean is not acting as passive sink to the atmospheric CO₂ anomalies. What is new there and is it in (dis)agreement with the own results?

Throughout the text I think the wording could be more specific. Sometimes we read things like “In Menviel et al. (2008) the ocean first takes up more carbon then loses it, while the vegetation loses carbon then takes it up.” When do the changes occur with respect to the freshwater fluxes? This is a little bit too sloppy.

In the abstract it reads “atmospheric CO₂ concentration rapidly increases and decreases by around 15 ppm at the same time as climate experiments an abrupt cooling in the North Hemisphere and warming in the South Hemisphere.” I think the ice core data show things a little bit different. CO₂ changes as gradually as Antarctic temperatures, so not rapidly as suggested here (how rapid is rapid?) and CO₂ normally switches from increasing trends to decreasing trends when both Antarctic temperature switches from warming to cooling and Greenland temperature happens to rapidly rise. You might also note (and maybe comment on) the different behaviour of some Dansgaard/Oeschger events in MIS 4 and for the Bolling/Allerod as discussed in Ahn and Brook (2008) and in a recent paper in *Climate of the Past* (Köhler, P.; Knorr, G.; Buiron, D.; Lourantou, A. Chappellaz, J. Abrupt rise in atmospheric CO₂ at the onset of the Bølling/Allerød: in-situ ice core data versus true atmospheric signals, *Climate of the Past*, 2011, 7, 473-486). It is furthermore not correct to speak of temperature changes in the northern and southern hemisphere, because the bipolar seesaw leads to different (more gradual) changes in Antarctica than in the South Atlantic because of the heat capacity of the Southern Ocean, see Barker et al 2009, NG and Stocker and Johnsen 2003, PO for details, see also next comment.

The introduction needs also some more clarification. The description of the bipolar seesaw is not precise. It reads “... are characterised by cool conditions in the north

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and simultaneous gradual warming in the south, followed by a return close to initial values (EPICA community members, 2006; Ahn and Brook, 2008; Barker et al., 2009). During these events, ice core records indicate that atmospheric CO₂ increases rapidly by around 15 ppm, and then decreases back to its initial level (Ahn and Brook, 2008).”

This is in detail not correct: The bipolar seesaw describes a gradual warming in the south during cool conditions in the north, which flips to gradual cooling conditions in the south during a abrupt warming in the north. Where here “south” means Antarctica, while in the South Atlantic the changes in temperature should be as fast as in Greenland (see Barker et al., 2009). Details on that were elaborated first by Stocker, T. F. Johnsen, S. J. A minimum thermodynamic model for the bipolar seesaw Paleoclimatology, 2003, 18, 1087, doi: 10.1029/2003PA000920, which was not cited. Furthermore the changes in CO₂ are NOT rapid, they are as fast (or slow) as the gradual southern warming. These details should also put right in the abstract.

The review of Kageyama et al., 2010 on simulated changes in the AMOC is mentioned in the discussion, but it might also be mentioned right at the beginning (intro), or/and the discussion section needs to be expanded largely. Please also note, that in Kageyama new model experiments of LOVECLIME and UVic are investigated, the two models used in Schmittner and Menviel and thus the model discrepancies concerning the physics of the climate system (not the C cycle) between these two studies might have been analysed there already. What are the data saying? Can you reproduce the rapid temperature shift in the South Atlantic as illustrated by Barker et al., 2009? For example, there was a whole special issue in QSR (Volume 29, Issues 21-22, Pages 2823-2980, October 2010) on "Vegetation Response to Millennial-scale Variability during the Last Glacial", which also included the Kageyama paper. I do not think that every details found in paleo data should be compared with the model, but some main features might need to be discussed. For example, the Bozbiyik paper find large changes in terrestrial C in South America and therefore discusses proxy evidences for that during the Younger Dryas cold period.

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For the design of the experiments (sec 2.3) you might not only discuss, what Schmittner and Menviel were doing, but also what freshwater fluxes others were using (see again Kageyama, e.g. Knutti using ECBILT-CLIO (same physics as LOVECLIM = Menviel)) used 0.3-0.5 Sv (used in Köhler et al. 2005), Bozbiyik used up to 1 Sv put in either the North Atlantic or two different areas in the Southern Ocean.

Furthermore, the comparison of your freshwater experiments with that of Schmittner is incomplete. Schmittner in UVic needs to have a negative freshwater flux to get the AMOC back on again (Fig 2b top in their paper).

Concerning the physics of your results, the temperature anomalies shown on Fig 3, 4, 7 is not the typical behaviour of the bipolar seesaw. The northern hemispheric temperature rise at the end of the freshwater flux is in your results very gradual and not abrupt. Maybe one needs to show time series of specific regions. The temperature anomaly in Schmittner covers the behaviour more closely to the data than here (but as said above, Schmittner needs a negative freshwater flux at the end, maybe you should investigate this also). This model results / disagreement to the data needs to be discussed more widely. For my understanding there are only a few model application available, which are able to generate the right speed and magnitude in the temperature change as seen in the Greenland ice cores, e.g. see Smith, R. S. Gregory, J. M. A study of the sensitivity of ocean overturning circulation and climate to freshwater input in different regions of the North Atlantic, Geophysical Research Letters, 2009, 36, L15701, doi: 10.1029/2009GL038607. From the NGRIP ice core it was shown that temperature / climate changes within about 50 yr, while the change in the $\delta^{18}\text{O}$ is even happening in less than 10 yr, see Steffensen, J. P.; Andersen, K. K.; Bigler, M.; Clausen, H. B.; Dahl-Jensen, D.; Fischer, H.; Goto-Azuma, K.; Hansson, M.; Johnsen, S. J.; Jouzel, J.; Masson-Delmotte, V.; Popp, T.; Rasmussen, S. O.; Rothlisberger, R.; Ruth, U.; Stauffer, B.; Siggaard-Andersen, M.-L.; Sveinbjörnsdóttir, A. E.; Svensson, A. White, J. W. C. High-resolution Greenland ice core data show abrupt climate change happens in few years, Science, 2008, 321, 680-684, doi: 10.1126/science.1157707.

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In the LGM (and LGM+brine) experiments only CO₂ radiative forcing is changed. What about changes in CH₄ and N₂O? For LGM they contribute together about -0.6 W m^{-2} to the radiative forcing, while CO₂ is responsible for -2.1 W m^{-2} .

As said already, the results section needs a good revision to get more precise description of the results.

Page 1369: “When the additional fresh water flux is too small to change the AMOC (below the threshold value of 0.2 Sv) the resulting change of CO₂ is small (less than 10 ppm, Fig. 2c and f).” This is not correct. AMOC changes in all experiments, it needs to say “STOPPING AMOC” or “switching to off mode” or so. Furthermore, I see in Fig 2 changes of less than 5 ppmv, not less than 10 ppmv, but maybe the details are difficult to see here.

The section 3.4 (discussion and data) is very short. Is your experiment comparable to all Heinrich stadials or to all Dansgaard/Oeschger events? Concerning the idea of an impact of the freshwater fluxes in the Southern Ocean, and the Bolling Allerod, is this connected to meltwater pulse 1A? Please also see a recent paper on a different interpretation of this CO₂ (Köhler et al. 2001 Clim Past, full citation was already given above).

In the caption to Fig 10 you should say right at the beginning (not the end) that here freshwater was put into the Southern Ocean, not North Atlantic, to give the reader a chance to see quickly what is plotted here.

Interactive comment on Clim. Past Discuss., 7, 1363, 2011.

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