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Interactive comment on "Carbon cycle dynamics during recent interglacials" *by* T. Kleinen et al.

Anonymous Referee #2

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The manuscript describes how the carbon cycle within the EMIC CLIMBER is improved by two slow processes ((a) shallow water $CaCO_3$ accumulation (coral reef growth) and (b) peat accumulation) and how the improved model is the performing for parts of three interglacials (Holocene, Eemian, MIS 11).

The content of the paper is certainly of interest for readers of the journal. However, I believe there are some more steps in the analysis and in the presentation of the paper necessary before it should be accepted for publication in Climate of the Past.

My main concerns are the following:

1. One of the objectives to analyse and to compare interglacial carbon cycles was the hypothesis of Ruddiman, who proposed that the rise in CO_2 after 8 kyr BP in the Holocene is due to early anthropogenic contributions (and potential feedbacks). This hypothesis is clearly mentioned in the paper, but most recent idea in that direction are not taken up (e.g. Ruddiman (2013, The Anthropocene, Annual Review of Earth and Planetary Sciences, DOI: 10.1146/annurev-earth-050212-123944) already claimed that a large peat burial in the Holocene would offset a large anthropogenic CO_2 rise). Furthermore, the authors have chosen to simulate only the later parts of the interglacials, while the first some thousand years in all three interglacials are omitted. This might be motivated by the potential influence of the long-term feedbacks from the previous deglaciation, but then also reduces the chances of really investigation the Ruddiman hypothesis and to compare the interglacials. One might also learn from this decision of the authors to focus on the final part of the interglacials, that in transient simulation the deglaciations need to be taken also into account, when understanding interglacial carbon cycle dynamics as widely as possible. This shortcoming of the study (caused by the chosen setup) might need to be discussed (and maybe motivated) more widely as done so far. Please also note, that others (e.g. Joos et al., 2004; Menviel and Joos 2012) include the whole deglaciation in order to understand Holocene carbon cycle dynamics.
2. One of the most interesting aspects of interglacial differences in the carbon cycle is the 0.2‰ offset in atmospheric $\delta^{13}CO_2$ observed from ice cores between Holocene and Eemian (Schneider et al., 2013), while CO_2 itself was comparable between both interglacials. In this data-based study of Schneider it was already suggested, that slow, long-term processes (weathering or volcanism) in the carbon cycle might be responsible for these effects. However, again, the authors have chosen an experimental setup by which this open research question can not be tackled, since they prescribe $\delta^{13}CO_2$ at the beginning of their experiments

from data and only simulate its dynamics over the rest of the interglacials. Since it is evident from the Schneider et al. (2013) data, that the sources and sinks for $\delta^{13}CO_2$ changed slowly over time, these results might only be of limited values, and might follow the $\delta^{13}CO_2$ (for those scenarios which meet the data) for the wrong reasons. Again, this is even more than my comment #1 above an argument for transient simulations which cover longer time periods.

3. I can not remember, that the choice of the investigated interglacials (Holocene, Eemian, MIS 11) was ever motivated. Why have the interglacials between Eemian and MIS 11 (MIS 7, MIS 9) not be chosen? There are various studies published, which compared different aspects of interglacial climate (aligning orbital configuration or greenhouse gas changes or temperature records of different interglacials) in search for the best analogue for the Holocene and to investigate the Ruddiman hypothesis (e.g. Ruddiman 2007, Reviews in Geophysics, doi:10.1029/2006RG000207; Yin and Berger 2010 (NGS, DOI: 10.1038/NGEO771) 2012 (CD, DOI 10.1007/s00382-011-1013-5) 2015 (QSR, http://dx.doi.org/10.1016/j.quascirev.2015.04.008)). From my reading of the literature MIS 19 seems to be the best analogue of the Holocene.

4. The analysis lack some important details on what the marine carbon cycle is doing. So far, one can understand how in the different scenarios carbon is accumulated in terrestrial vegetation, soil or shallow water. However, the changes in biomass+soil (for scenarios investigating the impact of the new peat carbon formation) do not add up to the changes that the anomalies in atmospheric CO_2 produces, implying that the marine carbon cycle is also affected. For example, page 1957, lines 4-10, it is said that the decrease in atmospheric CO_2 of 25 ppmv is explained by the uptake of 320 PgC by peatland growth. However, 25 ppmv in CO_2 correspond only to a change in the atmospheric carbon pool of about 50 PgC, so where are the other (320-50=)270 PgC coming from? Furthermore, shallow water $CaCO_3$ accumulation also changes ocean alkalinity, which then changes in the

marine carbonate system and thus the ability of the ocean to absorb CO_2 from the atmosphere. What is needed here, is either the addition of several new subplots or an overview results table on various additional (mainly marine) carbon pools and fluxes: ocean C content, C content in deep-ocean sediments, shallow-water C content, ocean alkalinity, weathering flux (does weathering change over time and is a function of climate or CO_2 and is it different for different interglacials?). Furthermore, to compare results with earlier studies (e.g. Elsig et al., 2009) the reader would be interested why marine carbon pools changed as they did. Was it because of SST changes or because of carbonate compensation or because a reduced atmospheric CO_2 (due to land carbon uptake) led to outgassing?

5. For the anthropogenic carbon emissions in the Holocene results from Kaplan et al (2011) are taken. However, in order to obtain simulation results which agree with CO_2 data the authors downscaled the Kaplan-based anthropogenic carbon emissions by 25%. I argue that this is an arbitrary non-scientific approach to fit the simulation results to the data. The authors should test different anthropogenic carbon emissions — as they were published — in their model and then discuss how their results meet the data. Please note, that the Kaplan et al. (2011) study contains two different anthropogenic carbon emissions, others are cited within Kaplan et al. (2011) and in Ruddiman (2013). See also Stocker et al (2011) BG, doi:10.5194/bg-8-69-2011.

6. The records of sea level change, that are important for the shallow-water $CaCO_3$ accumulation needs a wider description and discussion. So far, the sea level change (plotted in Figs 5a, 8a 11a) is obtained from CLIMBER-SICOPOLIS coupling. To my knowledge, this setup only considers changes in northern hemisphere land ice, but none from Antarctica. This needs at least to be mentioned or even better discussed. The plotted sea level records which force the coral reef growth should be compared with other sea level records in order to understand if any mismatch here might influence the simulated coral reef growth. In detail: (a)

the Holocene sea level does not reach zero, but the over change over time seems to be reasonable; (b) Eemian sea level only falls, while Rohling et al (2008) NGS, doi:10.1038/ngeo.2007.28, finds rising sea level until about 122-123 kyr BP, then falling, clearly in disagreement with Fig 8a; (c) The pronounced sea level variation of CLIMBER (Fig 11a) with rising sea level around 420 ka BP by 20 m and falling around 400 ka BP by 15 m (which shows clearly a large imprint on simulated CO_2 in scenario MISS11_NAT (Fig 9), is this discussed as such in the text?) needs to be compared with others. For MIS-11 please see Rohling et al (2010) in EPSL, doi: 10.1016/j.epsl.2009.12.054, who find a rise and fall in MIS-11 sea level by about 40 m between 420 and 390 ka BP, thus about twice as much as used here. Also note, that deconvolution of benthic δ^{18}O into temperature and sea level by models (e.g. de Boer et al (2013) CD, DOI:10.1007/s00382-012-1562-2) is different in MIS 11 showing a decreasing sea level from 400 ka BP onward without any plateau around 395-380 ka BP. The paper of de Boer et al (2013) also analyses the contribution of Antarctic ice sheets to sea level, but from my reading it indeed seems to be the case that the Antarctic contribution to sea level change during interglacials is minor, so this is NOT the reason for the disagreement between both studies.

7. After this revision the whole discussion section probably needs a complete rewriting.

Minors:

1. The title should be changed according to what is contained in the paper, e.g. "The importance of peat accumulation and coral reef growth for the carbon cycle dynamics during interglacials in MIS1, 5, 11".
2. It is difficult to compare the dynamics during the different interglacials from the way the results are plotted right now. At best, the changes in CO_2 and $\delta^{13}CO_2$

are given for all 3 interglacials on plots, that have the same scales in x and y direction, see for example Fig 11 of Yin and Berger 2015 (QSR).

3. Although no atmospheric $\delta^{13}CO_2$ data from ice cores yet exist for MIS 11 it would of course be of interest to see the educated guess (simulation results) of $\delta^{13}CO_2$ from this study, which might illustrate, what dynamics in that variable might be expected.
4. What is called “shallow-water $CaCO_3$ sedimentation” throughout the test is for my understanding “shallow-water $CaCO_3$ accumulation”, please change.
5. page 1946, line 23: “While the Holocene CO2 trend has generated considerable interest previously (Ruddiman, 2003), the context of previous interglacials has been neglected.“ This is not correct. The whole idea of the Ruddiman hypothesis is about the trend in CO_2 (and CH_4) in the Holocene in comparison to other interglacials. It might be correct that so far no process-based carbon cycle models addressed other interglacials. Please rephrase.
6. page 1949, line 5: “DGVM” was already explained on page 1948.
7. page 1950, line 14: Please state briefly name and reference of the DGVM embedded within CLIMBER, probably VECODE.
8. page 1950, line 27: ...“corals as the main” SHALLOW WATER “carbonate producers”
9. page 1951, line 9: Please give a reference for the SST growth limit of corals.
10. Please include a figure, in which the vertical coral accumulation rate G is plotted as function of light. No values of the parameters G_{max} and I_k are yet given. Please extend on parameter values and motivation (reference) for your choice.

11. page 1952, line 16: “last glacial maximum” should be written as “Last Glacial Maximum (LGM)”, that would then introduce “LGM” which is used later-on.

12. page 1953, line 3: It is not clear if “this publication” is related to “Yu et al (2010)” or to this manuscript (Kleinen et al 2015).

13. page 1953, line 16: There is no reference “Ganopolski et al (2011)” in the reference list, maybe you mean “Ganopolsi and Calov (2011)”, please check and correct.

14. Ice core CO_2 data: The authors might refer to the most recent compilation of ice core CO_2 data on the most recent ice core age model as published in (and available in the supplement to) Bereiter et al (2015) in GRL.

15. Ice core $\delta^{13}CO_2$ data: I suggest to show the Monte-Carlo-based spline through all available $\delta^{13}CO_2$ data as published in Schmitt et al (2012) in Science, DOI: 10.1126/science.1217161 (here: the Elsig data as taken so far in this manuscript are included) and in Schneider et al (2013) Climate of the Past; doi: 10.5194/cp-9-2507-2013. The Schmitt spline is available as download at Science, and the Schneider spline certainly via email from the Bern ice core group.

16. page 1956, line 24: “terrestrial biomass”, this means vegetation? If so, say so.

17. page 1958, line 20: Please include SHALLOW WATER before “$CaCO_3$ accumulation rate”.

18. page 1959, lines 1-5: Modelled CO_2 and $\delta^{13}CO_2$ are within the range of the data (including errors). Please expand on what the variations in simulation and data are, not just that you meet the data, and briefly mention where there are disagreements, I again suggest to use the spline for $\delta^{13}CO_2$ data.

19. Discussion: As explanation (a) of the misfit to the Holocene $\delta^{13}CO_2$ data it is suggested that Elsig underestimates the true uncertainty. By using the spline in $\delta^{13}CO_2$ such a potential shortcoming should be overcome. Furthermore, another explanation for the misfit might be, that the marine C cycle change (which are not yet described, see my major point #4) are wrong.

20. Figures: In the figures which show ice core data, the ice cores from which the data are, should be mentioned in the caption (at best with reference) and the age model, on which the data are plotted.

21. Figure 4: No results for HOL_PEAT are shown, or are they similar to HOL_NAT? If they are indeed similar, I have probably not fully understood the modelling setup. My understanding is, that the internal simulated atmospheric CO_2 concentration is used by the CLIMBER model to calculate also any temperature changes via the greenhouse effect. This would imply, that any change in CO_2 would change temperature and therefore also peat accumulation. I therefore expect that results for HOL_PEAT and HOL_NAT differ. Please extent the model description in order to clarify this issue. But maybe I missed some details, e.g. a different coupling scheme between climate and carbon cycle.

Interactive comment on Clim. Past Discuss., 11, 1945, 2015.