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

**T. Kleinen et al.**

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**Author reply to comments by anonymous reviewer #2**

We very much thank the reviewer for taking the time to review our manuscript. We aim to incorporate all of the reviewer's comments in the final manuscript, this will lead to a substantial improvement over the original submission. For reader convenience we have included the reviewer's comments in full in this reply, marking them by bold font.

**The manuscript describes how the carbon cycle within the EMIC CLIMBER is improved by two slow processes ((a) shallow water CaCO3 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 CO2 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 CO2 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.**

We have the impression that the reviewer may have misunderstood our intentions. It was neither our intent to fully investigate and discuss the "Early anthropogenic hypothesis" by Ruddiman (2003) including later modifications (e.g., Ruddiman 2013), nor to fully explain the last glacial-interglacial cycles. Instead our intent was much more modest: we aim at understanding *trends* in the carbon cycle during three recent interglacials. We will clarify this in the revised submission.

With regard to the original early anthropogenic hypothesis, it is addressed in detail in many publications (e.g. Claussen et al., 2005). Later modifications, e.g., the approach to account for peat carbon accumulation (Ruddiman 2013), which partly compensates the land carbon emissions from anthropogenic land use, still cannot fully explain the observed record of an increase in CO2 and simultaneously relatively stable d13CO2 because both peat and landuse carbon have similar 13C signatures. This implies that any compensation of d13C changes from land use changes through peat uptake would require that the entire carbon emitted is taken up by peatlands. Here, we disentangle an oceanic CO2 source which does not affect 13CO2 from terrestrial sinks (peat) and sources (land use in the Holocene) with significant d13C fractionation.

In addition, some components of the system that are crucial to address the original early anthropogenic hypothesis in full depth are missing in our model. For example, methane emissions from agriculture are not something we can determine – and that would be very difficult to quantify in any meaningful way since we lack data on historical rice agricultural practices.

With regard to the setup of initial conditions, we indeed have a limitation of our equilibrium approach since the carbon cycle is never in equilibrium, neither at the early Holocene nor during the Last Glacial Maximum. Performing transient runs through several glacial cycles would be the most appropriate way to address interglacials, but this is very challenging, both computationally and scientifically. While we have made some progress in simulating the full glacial CO2 cycle with the CLIMBER-2 model



(Brovkin et al., 2012), the processes that govern the interglacial carbon cycle dynamics are different from those that play a dominant role in glacial periods. During glacial periods, atmospheric CO2 is mainly driven by changes in ocean volume, SSTs, circulation, and marine productivity, i.e., oceanic processes play a much more important role in the carbon cycle than terrestrial ones. During interglacials, land carbon also plays a significant role, as climate and oceanic circulation are relatively stable and memory effects from the previous glacial period/deglaciation are operating through relatively slow changes in the marine carbonate chemistry. Our approach is to start simulations several thousand years after stabilization of CO2 in the atmosphere at the beginning of interglacials to reduce the memory effects, but we cannot completely exclude them.

**2. One of the most interesting aspects of interglacial differences in the carbon cycle is the 0.2‰ offset in atmospheric $\delta$ 13 CO2 observed from ice cores between Holocene and Eemian (Schneider et al., 2013), while CO2 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 $\delta$ 13CO2 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 CO2 changed slowly over time, these results might only be of limited values, and might follow the $\delta$13 CO2 (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.**

Indeed, our approach is limited because we cannot yet model the full glacial cycle. Since the difference between Eemian and Holocene is apparent through the entire interglacial, the reason for this difference must lie somewhere in the glacial period, which we cannot yet model sufficiently well. To simulate drifts in atmospheric d13C from one to another interglacial, we would need (1) to simulate the carbon cycle dynamics through several glacial cycles, and (2) to account for mechanisms which could lead to an imbalance in d13C. Such an imbalance could, for example, result from unbalanced sinks or sources of organic material, such as burial of organic material in the marine sediments, or mineralization of carbon stored in permafrost soils during interglacials. The sediment model we use in this study accounts only for carbonate, but not for organic sedimentation. Since neither deep-sea sedimentary organic burial nor permafrost burial are accounted for in our model, we cannot test the "organic burial" hypothesis and have to use observed d13C data as initial conditions for the carbon cycle. Our goal is then to simulate trends in d13CO2, and not to explain the difference in the initial conditions.

**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.**

While it is in principle possible to model any particular interglacial, doing so becomes less and less fruitful as one goes back further in time, due to the lack of data of sufficiently high resolution and precision. Therefore we chose MIS 1 and 5 as a much better test for a model, since sufficient data are available for a meaningful test of model



results. We furthermore chose MIS 11 because of its unusual length (e.g., Tzedakis et al., 2012). We will discuss the choice of the analysed interglacials in the revised manuscript.

**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 CO2 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 CO2 of 25 ppmv is explained by the uptake of 320 PgC by peatland growth. However, 25 ppmv in CO2 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 CaCO3 accumulation also changes ocean alkalinity, which then changes in the marine carbonate system and thus the ability of the ocean to absorb CO2 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 CO2 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 CO2 (due to land carbon uptake) led to outgassing?**

On the time scales of interest (i.e., several thousands to tens of thousands of years), it will be unavoidable for the ocean carbon cycle to feed back onto atmospheric perturbations arising from $CO_2$ exchanges with the terrestrial biosphere and permafrost. In this respect, although we have not looked into the finer details, the 25 ppm (or about 50 PgC) decrease together with the 320 PgC uptake by peatland growth mentioned by the reviewer fits quite well with the usual ocean buffering and carbonate compensation framework. If peatlands take up 320 PgC from the atmosphere, 85%, or about 270 PgC will be replenished from the oceans on time scales of several hundreds to a few thousands of years as a result of ocean buffering (which would already fit the balance fluxes in our case). On longer time-scales of several thousands to a few tens of thousands of years, increased carbonate accumulation in the deep-sea due to the decreased ocean DIC would decrease global ocean alkalinity, thus contributing in turn to reduce the remaining 50 PgC deficit in the atmosphere (compared to the pre-peatland-uptake situation) by an extra one third to one half. However, these latter time scales are possibly already somewhat too long to play a significant role in our case.

Weathering is dependent on climate (via runoff, as stated on p. 1949, ll. 21-23). Therefore it changes with time and is different between the interglacials. During the early Eemian, temperatures, as well as precipitation and runoff, are higher than during the Holocene, leading to stronger weathering.

We plan to extend the discussion of marine C cycle changes in the revised submission of our manuscript (with extra figures and attribution analysis of C cycle changes as appropriate).

**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.**

With regard to the anthropogenic carbon emission scenarios, uncertainties are certainly very large. While we admit that our approach of rescaling the Kaplan et al. scenario is to some extent arbitrary, we do, however, disagree with it being unscientific. First of all, a 25% difference certainly falls within the uncertainty range of the Kaplan et al. (2011) scenario. Secondly, a C emission scenario similar to our rescaled version of the scenario of Kaplan et al. (2011) has been derived from Kaplan's land use change data using a different carbon cycle model (B. Stocker, personal communication). Unfortunately this latter scenario has not yet been published and we therefore cannot use it in this study. Nonetheless we will extend the discussion of the forcing data and include model runs with Kaplan's original scenario, as well as other scenarios, in the revised submission, as recommended by the reviewer.

**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 CO2 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 $\delta$ 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.**

Sea level change contributions from Antarctica are actually included in our model sea level forcing. It is assumed that they are 10% of the NH ice sheet changes, which is a decent approximation for glacial-interglacial changes, but which might underestimate Antarctic contributions to strong sea level high stands during interglacials.

The sea level forcing we used in our experiments comes from a forward model simulation of the last eight glacial cycles performed with CLIMBER-SICOPOLIS. Since the model does not include any a priori information about the final ice sheet mass, the sea level at the end of the experiment may differ from zero. In fact the present-day Greenland ice sheet mass is slightly overestimated by the ice sheet model. We did not correct for this mismatch when plotting the results, but we will do so for the submission of the revised paper.

The Holocene is the only interglacial where sea level reconstruction are reliable, and



here our model is in very good agreement with observations.

Although the CLIMBER-SICOPOLIS results for the Eemian are clearly different from Rohling et al (2008), they are very similar to IPCC AR5, Chapter 5, Figs. 5.15 a and b. We therefore believe that our results are reasonable for the Eemian.

For MIS 11 we estimate that uncertainties on the reconstructed sea-level stands are probably +/- 20 meters at the very best. Rohling et al. (2010) and also Grant et al. (2014) indeed find a sea level substantially lower than in our model at 390 ka BP, but they also find sea levels 5-10m below present during the entire MIS 11, while other studies (Raymo et al., 2012; Bowen, 2010) show sea levels 5-10m above present. De Boer et al. (2013) indeed find a decrease after 400 ka, Rohling et al. (2010) and Grant et al. (2014) document a plateau around 395-380 ka BP, and Elderfield et al. (2012) a rise in sea level during this period of time.

Our model sea-level therefore fits well into the available reconstructions. We will discuss these issues in more detail in the revised manuscript.

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

We agree.

**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".**

The present title might indeed raise reader expectations that the paper would not fulfil. Unfortunately the title suggested by the reviewer does not quite fit our paper either, since we do include a full carbon cycle in our model. We will reconsider the title, though, and aim to make it fit better to the paper.

**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 CO2 and $\delta$ 13CO2 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).**

We will try to add a figure of all interglacials on the same axes.

**3. Although no atmospheric $\delta$ 13CO2 data from ice cores yet exist for MIS 11 it would of course be of interest to see the educated guess (simulation results) of $\delta$ 13CO2 from this study, which might illustrate, what dynamics in that variable might be expected.**

We will include it in the revised submission.

**4. What is called "shallow-water CaCO3 sedimentation" throughout the test is for my understanding "shallow-water CaCO3 accumulation", please change.**

We will clarify the text.

**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 CO2 (and CH4) 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.**

We will clarify the text.

**6. page 1949, line 5: "DGVM" was already explained on page 1948.**

Thank you for pointing this out.

**7. page 1950, line 14: Please state briefly name and reference of the DGVM embedded within CLIMBER, probably VECODE.**

Thank you for pointing this out, we will clarify the text.

**8. page 1950, line 27: ..."corals as the main" SHALLOW WATER "carbonate producers"**

We will clarify the text.

**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 Gmax and Ik are yet given. Please extend on parameter values and motivation (reference) for your choice.**

Parameters (incl. SST growth limits – reviewer's point 9 above) were taken from Kleypas (1997). We will revise the text to make this clearer. Since a figure of G over Iz is already included in original Kleypas (1997), we prefer not to print this again but instead refer to the original paper.

**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.**

Thank you for pointing this out.

**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).**

It was the Yu et al. '2010) paper that was meant. Thank you for pointing out this source of potential misunderstanding, we will correct the text accordingly.

**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.**

We will correct the citation.

**14. Ice core CO2 data: The authors might refer to the most recent compilation of ice core CO2 data on the most recent ice core age model as published in (and available in the supplement to) Bereiter et al (2015) in GRL.**

Unfortunately the original submission was written before the compilation by Bereiter et al. was available. We will refer to it in the revised version of the paper.



**15. Ice core $\delta$13 CO2 data: I suggest to show the Monte-Carlo-based spline through all available $\delta$ 13CO2 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.**

When writing the original submission, we decided to use the raw data in order to also show the uncertainties in the measurements. We will reconsider that choice and also show the MC spline.

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

We will clarify the text.

**17. page 1958, line 20: Please include SHALLOW WATER before "CaCO3 accumulation rate".**

We will clarify the text.

**18. page 1959, lines 1-5: Modelled CO2 and $\delta$13 CO2 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 $\delta$ 13CO2 data.**

We will extend the discussion of model results and data.

**19. Discussion: As explanation (a) of the misfit to the Holocene $\delta$13 CO2 data it is suggested that Elsig underestimates the true uncertainty. By using the spline in $\delta$13 CO2 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.**

When writing the original submission we had underestimated the significance of the MC spline fit. We will reconsider that choice for the revised submission. We will also extend the discussion to marine C cycle changes, although these are less relevant for d13CO2 in our model. Nonetheless we will check for this when we analyse marine C changes, as written in our reply to the reviewer's major point #4.

**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.**

We will clarify this in the figure caption.

**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 CO2 concentration is used by the CLIMBER model to calculate also any temperature changes via the greenhouse effect. This would imply, that any change in CO2 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.**



Results for HOL_PEAT and HOL_NAT are indeed different since climate and CO2 are different. We decided not to show them to avoid overloading the Figure. We will reconsider this choice for the revised submission.

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