REPLY TO THE COMMENTS BY THE REVIEWERS

Color coding:
Black – comments by reviewers
Blue – reply by authors
Red – textual changes in the manuscript

General

Our manuscript has been reviewed by two referees, who have very different opinions. Reviewer #1 is very positive and suggests some minor modifications. Reviewer #2, however, is uncomfortable with a number of points. Most importantly, he/she feels our current model setup lacks important geological processes that might influence the relation between CO_2 and climate over time, mainly tectonics and erosion. This is certainly a valid argument and in the revised manuscript we shall express better from the start what the value of our study is in terms of the simulation of CO_2 over the past 38 Myr. As requested by this reviewer, we will also improve the introduction by comparing our study to more sophisticated models which are used for particular time slices. We feel that by focusing on the larger picture of transient climate change our current model setup represents a complementary approach to these models, which generally also do not take into account the geological processes mentioned by the reviewer. Furthermore, we will make clearer that our study has a two-fold aim: being a first step in the direction of transient coupled simulations of the climate and cryosphere on long time scales, and quantifying the influence of ice sheets on climate variability. It is an improvement of our earlier research group's earlier work that has been published in several papers which are mentioned in the manuscript. As such, it provides valuable information for the research community. In our opinion, this justifies publication of the current results. Nonetheless, we will take into account as much of the suggestions by both reviewers as possible, which in our opinion will significantly improve the quality of the manuscript. Below, we will answer to the comments of the reviewers.

Structure of the paper

A point where both reviewers agree upon is that the current structure of the paper could be improved. We have therefore decided to follow their suggestions, and structure the revised manuscript in the following manner:

Introduction

As requested by reviewer #2, the introduction will be expanded with a discussion of studies using more sophisticated climate and ice sheet models on shorter time scales. This will provide a better perspective of the research field and our contribution to it. We will more clearly state the strengths and weaknesses of our model setup, as well as the purpose of our current study.

Model

This section will include a more thorough description of the coupled climate-ice sheet model we use, as well as the inverse routine to simulate CO_2. The equations we use to calculate $\delta^{18}O$ and CO_2 will be provided as suggested by reviewer #1. The setup of the different model runs we perform will be moved to the Results and Discussion sections to improve readability of the paper.

Results and Discussion I: Long-term transient simulations
This section will demonstrate the different CO_2 concentrations we obtain over the past 800 kyr when we integrate the model over the past 5 Myr or 38 Myr. We will introduce the hysteresis runs to explore this difference further. Thereafter, we will more clearly describe how and why we re-tune the model. The CO_2 will be compared to the proxy data compilation presented in Beerling and Royer (2011), complemented by records published after that study, as requested by reviewer #1.

Results and Discussion II: Ice sheet-climate interaction
This section will remain largely the same as Section 4, but including more discussion. As requested by reviewer #1, we will begin this section by presenting the main results of our new 38 Myr reference run: $\delta^{18}O$, CO_2, ice-volume-equivalent sea level (total, as well split into contributions from NH and Antarctica) and global mean temperature.

Summary and Conclusions
The discussion of the results will be moved to the Results and Discussion sections. This section will only contain a brief summary of our experiments, and the conclusions we derive from them.

Reviewer #1

General comments:

The authors present results of model simulations of the past 38 million years using a simple zonally averaged energy balance model coupled to a 1D ice sheet model. The presented results contribute to our understanding of climate - ice sheet interactions on very long timescales and therefore the paper represents a valuable contribution to this research field. The use of a relatively simple model is justified by the very long transient simulations which would be too computationally expensive to perform with more complex models. However, in order for the paper to be suitable for publication in Climate of the Past, some minor issues listed below should be addressed.

We thank the reviewer for considering our work, and we are pleased that he/she agrees with our general approach of using a simplified model to simulate the long-term evolution of climate. We will explain below how we will take the comments into consideration. In our opinion this will improve the quality of the paper, hopefully to the satisfaction of the reviewer.

The model is described only very briefly in the Methodology section. I'm aware that the model is described in more detail in previous publications, but it would be useful to the reader who is not familiar with the model if some more details would be given (e.g. the resolution of the ice sheet model is not even mentioned in the text). How is the model initialized? Additionally, the way the CO2 concentration is derived in the model and applied as forcing is crucial for the simulations performed and should be described in the paper. I would suggest to at least include the equations for δ18O and CO2.

The reviewer suggests to expand the Methodology section, a point to which reviewer #2 agrees. Therefore, in our new Model section we will include a more thorough explanation of our modelling strategy, including the equations used to calculate $\delta^{18}O$ and CO_2.

The description of the experiments used to show the hysteresis behavior of the model is spread over several sections of the paper, which is very confusing to the reader. First it is mentioned in the Methodology section that using different δ18O stacks gives very different results but no reason for that is given until section 3. Then at the end of Section 2 (Page 4, lines 7-15) the hysteresis experiments are described, but it is difficult to understand why these experiments are needed before knowing what the problem is (which is only outlined in Section 3). I would suggest collecting all of this in one section describing the difference between 5Myr and 38Myr simulations, the experiment setup for diagnosing the reason for the differences, the hysteresis behavior and the retuning procedure.

We will follow the suggestion of the reviewer, and move the description of the model runs to the new Results and Discussion sections. In the new section Results and Discussion I, we will describe the difference between 5 Myr and 38 Myr simulations, the experiment setup for diagnosing the reason for the differences, the hysteresis behavior and the retuning procedure, as the reviewer suggests. This section will end with a comparison of our simulated CO_2 to the proxy data compilation presented in Beerling and Royer (2011) and newer records (see also our reply on a further comment by reviewer #1). We will include proper headings marking subsections.

I'm not aware of any other modeling study showing a hysteresis behavior that is caused by the atmosphere model or ocean model when excluding overturning, so it would be interesting to know what is causing this. Because of the relatively short time scale of atmospheric processes, it seems difficult to imagine that the climate model keeps memory of the initial conditions over multimillenial time scales. Could the authors elaborate on this? Are the different hysteresis branches really stable equilibria of the model? Also, does this hysteresis behavior depend on the forcing rate (50 ppm/50 kyr)? What are the initial conditions for these experiments?

The hysteresis runs will be more thoroughly described in the revised manuscript. The questions raised by the reviewer will be addressed: indeed, the different hysteresis branches are stable equilibria of the model. As long as the model is indeed in equilibrium at every time step, the hysteresis behaviour does not depend on the forcing rate: using 50 ppm/100 kyr and 100 ppm/100 kyr leads to the same results. The initial conditions are: 450 ppm CO_2, no land ice, glacio-isostatically relaxed present-day topography and present-day insolation.

The model-derived atmospheric CO2 could be compared with available proxy data (e.g. Beerling and Royer, 2011).

The new Results and Discussion I section will contain a comparison of our model results to the proxy data compilation presented in Beerling and Royer (2011) and newer records. Based on this comparison, we will address the caveats and shortcomings of our model. This will clarify the significance of our current model results, as well as indicate a route to go forward from here.

It would be interesting to see also the sea level evolution (maybe also the ice volume evolution separately for NH and Antarctica) and possibly global temperature evolution, also to make it easier for the reader to interpret Figures 4 and 5.

The new Results and Discussion II section will start with a figure showing the main results of our new reference run (after re-tuning): $\delta^{18}O$, CO_2, ice-volume-equivalent sea level (total, as well split into contributions from NH and Antarctica) and global mean temperature.

Figure 3 is very hard to read, especially Figure 3b. Maybe Figure 3b could be split in 3 different plots?

To improve readability, Figure 3b will be split as suggested by the reviewer, such that the revised Figure 3 will be composed of 4 subplots (A-D).

The following sentence in the abstract (Lines 8-9) is not clear, at least not until one has read the rest of the paper: 'Firstly, we investigate the relation between global temperature and $_{CO2}$, which changes once the model run has experienced high CO2 concentrations.'

In the revised abstract, we will be clearer on the implication of the analyses of the hysteresis runs:

Firstly, we find that the CO_2 simulation over the past 5 Myr is dependent on whether the model run is started at 5 or 38 Myr ago. This is because the relation between CO_2 and temperature is subject to hysteresis. When the climate cools from very high CO_2 levels, as in the longer 38 Myr run, temperatures in the lower CO_2 range of the past 5 Myr are higher than when the climate is initialized at low temperatures. Consequently, the modeled CO_2 concentrations are different depending on the initial state.

Reviewer #2

This paper deals with an important issue: the role of ice sheets on the climate evolution since the late Eocene (38 Ma). To achieve this goal, they use simplified climate energy balanced models and also a simplified ice sheet model. Using these tools enables them to simulate very long time spans.

General comment:
Whereas this is an important issue for which there are many unsolved problems as the evolution of Antarctica ice-sheets during Oligocene and Miocene and its implication on climate, I feel very uncomfortable with the target, the methodology used and the analysis provided in this paper.

We thank the reviewer for a careful consideration of our work. Unfortunately, he/she is very critical towards our modelling approach. Although the reviewer certainly has some valid points, we still believe our results represent a step forward in our understanding of the influence of ice sheets on long-term climate variability. Below, we will describe why, and which revisions we make to hopefully ease the objections of the reviewer as much as possible.

These authors had first used this tool to investigate
the relationship between cryosphere and climate for 1 million year (Lennert, B Stap, 2014) and extend afterwards to 8 million years (Lennert B Stap, 2016, A). In this new paper, they enlarge the period to 38 million years. But for many reasons I will explain below, this extension is not convincing with respect to many features: a first obvious one is the role of tectonics on CO2 that the authors perfectly know because they also recently published a paper concerning this issue (Lennert, B Stap, 2016 B). The tectonics, through many different processes, will affect atmospheric pCO2 (see Godderis for a review). For instance opening and closing sea ways may change climate and CO2, orogenesis (E.G Tibetan Plateau Uplift) and plate motion that will impact silicate weathering. Therefore, the extension to 38 Ma they provide in this paper is not really reliable. They reconstruct the pCO2 as a prognostic variable from their model which is indeed important but as they online derive it from radiative perturbation there are missing many fundamental processes. Consequently, their reconstructions of pCO2 over the 38 million years is not in good agreement with data as the authors recognize but instead of accounting for causes of such a disagreement on geological time scale they tuned the model with different parametrization of the clouds physics. This caveat makes the paper not appropriate for publication. Nevertheless, there are potential interesting sensitivity experiments that are possible with such a tool.

The reviewer mentions a number of geological processes that are not taken into account in our model setup, but could influence CO_2. However, our study does not concern which processes govern the CO_2 concentration in the atmosphere – to address this issue, one would need a carbon cycle model – but what influence CO_2 has on the climate, and how ice sheet variability changes this influence. Nevertheless, changing topography could lead to a different relation between CO_2 and the coupled climate-ice sheet system, e.g. via changing ocean overturning strength and surface elevation. Indeed, in a previous publication (Stap et al., 2016B) we have explored the effect of the latter process. Our model is unable to simulate some of the aspects shown by proxy data, as we will show in a comparison of our results to the proxy data compilation of Beerling and Royer (2011) and newer records in the revised manuscript. We therefore do not wish to claim that we provide the definitive evolution of CO_2 over the past 38 Myr. However, our modelling results clearly represent a step forward from previous studies using a stand-alone ice-sheet model (De Boer et al., 2010), and provide valuable insights into the influence of ice sheet variability on climate. In the revised manuscript, we will be very clear from the start on the purpose of our current study, as well as the caveats that can be addressed in further research.

Another drawback
is the fact that they avoid in the introduction to give a context of the state of the art of climate cryosphere interaction using sophisticated GCM as De Conto and Pollard (for instance De Conto and Pollard in Nature 2003, Geoscientific Model Development 2012 and Earth and Planetary Science Letters 2015) developed since many years. One of the major results of De Conto et al. study is to be able to reproduce the evolution of ice sheets since Eocene. They pointed out the importance of cryospheric processes (Pollard and De Conto, EPSL, 2015) that are not discussed at all in this manuscript.

The second major concern of the reviewer regards the lack of discussion of previous results, in particular the work of Pollard and DeConto in many much-cited publications. This point will be addressed by expanding the introduction of our study to include this discussion. Here, as well as in the new section Results and Discussion II, we will discuss how our results relate to their work, which generally concerns shorter time scales (mostly snap-shot simulations) but using a more sophisticated model setup. We refrain from quantitative comparisons on short time scales, however, since our intention is not to capture any event in great detail, but to provide the larger picture of the long-term influence of ice sheets on the climate. Our results are also not completely independent of the work of Pollard and DeConto, since the inception CO_2 level of the Antarctic ice sheet is highly dependent on the parametrisation of the mass balance in our model, and is matched to the one found by Pollard and DeConto (~780 ppm). This will be better explained in the revised manuscript.

Due to these two major problems I don't believe that at this stage such a paper may be published. Nevertheless I will give more details and comments because there is a large room for improvement if the authors want to resubmit their manuscript.

Detailed comments:

1. Abstract First, the relationships between CO2 temperature and ice sheets are consistent within the framework of the modeling study but completely inconsistent with available data concerning CO2 evolution since 38 million years. This is clearly shown in the paper but not in the abstract itself.

We would argue that our results are actually not as bad as the reviewer states here, as we will show in a more rigorous comparison to proxy data in the revised manuscript. However, we will mention the shortcomings of our model and the purpose of our work also in the abstract:

In this study, we use a zonally averaged energy balance climate model bi-directionally coupled to a one-dimensional ice sheet model, capturing the ice-albedo and surface-height-temperature feedbacks. Potentially important transient changes in topographic boundary conditions by tectonics and erosion are not taken into account, but briefly discussed.

Second, the authors insist on very obvious results as for instance it is colder when you get an ice sheet but the most interesting part of the work is to provide many sensitivity experiments. Indeed, this approach, conversely to GCM, as for example De Conto and Pollard (Palaeogeography, Palaeoclimatology, Palaeoecology 2003), allows them to quantify specifically the role of albedo on one side and elevation on the other side. This is not clearly stated in the abstract.

We agree with the reviewer that a main merit of our setup is that it lets us attribute the effect of ice sheets on the climate to two important feedbacks: the ice-albedo feedback and the surface-height-temperature feedback. As this is a main result of this work, it will be mentioned in the revised abstract:

By passing only albedo or surface height changes to the climate model, we can distinguish the separate effects of the ice-albedo and surface-height-temperature feedbacks.

Introduction: This section is a bit short. Some references are missing which may be important. For instance, concerning the Pliocene and Greenland onset, recent publications of Contoux et al (EPSL, 2015) and for MMCO a publication of Hamon (Geology, 2012) constrains on Antarctica ice sheet at MMCO and also Hamon (Climate of the Past, 2013) which depict the role of East Tethys seaway on Antarctica ice sheet 40 million years ago. More importantly, the authors should discuss the interest of their approach compared to the development of GCM studies as those published by De Conto and Pollard (EPSL, 2015) which pinpointed the importance to parametrize the ice sheet with sophisticated models to capture correctly the ice sheet dynamics and therefore to reproduce the ice sheet evolution through Eocene.

We thank the reviewer very much for pointing out these studies. We will expand the Introduction section with a discussion of these papers, which will give the reader a better perspective of the field and our contribution to it. See also our reply to an earlier comment by the reviewer.

Methodology section: First, the authors claimed they used benthic δ18O isotope records to infer the temperature of the Ocean, but it is absolutely unclear to me how they really disentangle the part corresponding to ice-sheet melting and the part due to bottom sea surface temperature. This first step has to be clarified, since it is used then to derive through radiative calculation the atmospheric CO2. I strongly believe than in a first step, the authors should have used the different proxy reconstruction used for CO2 as published in the literature, which provides different CO2 evolution (Boron isotopes, Alkenon, leaf stomates,. . .) to validate their simplified coupled model. Such a strategy based on CO2 reconstruction from data allows to test the response of their tool in terms of cryosphere and climate evolution. Instead, they choose to compute the CO2 from the reconstructed SST, derived from their radiative model.

The reviewer is unclear as to how our inverse CO_2 calculation from benthic $\delta^{18}O$ data works, a concern shared by reviewer #1. Therefore, in our new Model section we will include a thorough explanation of our modelling strategy, including the equations used to calculate $\delta^{18}O$ and CO_2. We would like to stress that CO_2 is not obtained from SST data, but from benthic $\delta^{18}O$ data which is disentangled into contributions from deep-sea temperature and land ice volume in our model.

In a previous publication (Stap et al., 2014), we have validated our coupled model, using CO_2 data from the EPICA Dome C record as input. A reason to refrain from using proxy CO_2 data from earlier times as input is that it is currently to scarce and intermittent. Moreover, there is large inter- as well as intra-proxy disagreement. Instead, we opt to use an inverse routine, and compare the results to available proxy data. In the revised manuscript, an explanation of this choice and a comparison to the proxy data compilation of Beerling and Royer (2011) and newer records shall be included.

As you know,
there are many reasons and causes that may affect atmospheric CO2, that cannot be

accounted for in this very simple modeling tool, especially when dealing with geological time span (38 million years). For instance, seaway changes - and there are many seaway changes in that period (see Zhang et al. Climate of the Past. 2011) - or the impact of mountain uplift and associated weathering (see Raymo et al. Nature 1992 and C France-Lanord, Nature, 1997). Therefore, the only processes they captured here, attributing Ocean temperature changes to CO2, is obviously missing a lot of important processes that will change the atmospheric CO2 during that period.

We agree with the reviewer that we are missing certain processes (tectonics, erosion) that may affect the relation between CO_2 and the coupled climate/cryosphere on the long time scales we investigate, by changing the topographical boundary conditions of the climate model. We therefore do not want to present our CO_2 record as the definite simulation of CO_2. Rather, as we express in the Discussion/Conclusion section, we pave the way for long time scale simulations, identifying interesting phenomena and potential obstacles. One of these is precisely that these missing processes are important. As this contradicts certain earlier studies (e.g. Foster and Rohling, 2013), we shall make this clearer in our new section Results and Discussion I.

Moreover, they
use a fixed contribution for the methane in this radiative calculation, (factor 1.3, which is supposed to include the methane radiative perturbation). This value is certainly valid for the last million years, for which data are available, but which is also a very cold period compared to the last 37 million years period they are investigating.

We would like to argue that we do not see a better alternative here. The factor 1.3 is indeed derived over the past 800 kyr, the only period over which we have reliable CH_4 and N_2O data, as is explained in our publication Stap et al. (2014). We will mention the implication of this modelling choice in the new section Results and Discussion I.

Finally, they
consider the lapse rate also constant through time whereas, this has been also shown as oversimplified (Svetlana Botsyun et al., Climate of the Past. 2016).

Here again, this is the best we can do at this moment. This point will also be included in the discussion.

These important
caveats in the methodology used here, which are absolutely not discussed, imply, as the authors themselves pinpoint, very large underestimation of their computed CO2 when compared to different proxies: the CO2 computed from the temperature record of Zacchos or Raymo, but also those much more accurate and directly obtained from Antarctica ice core (EPICA).

We are afraid that we have not been able to convey our findings well enough to the reviewer. We do not underestimate a proxy computed from the temperature record of Lisiecki and Raymo (2005) and Zachos et al. (2008). The point is that initially we tuned the model to simulate CO_2 over the past 800 kyr in agreement with the EPICA Dome C record. Using the same exact model, however, we lose this agreement if we start our model integration further back in time (38 Myr instead of 5 Myr ago). The disagreement can therefore not be caused by omitted processes, since we in fact use the same model. Hence, we explore the cause of it, by analysing separate hysteresis runs. We will explain this more clearly in the revised manuscript.

The authors claimed that such a mismatch may be overcome by changing the optical properties of the clouds. This is not really serious for me, because it is a kind of tuning without really understanding what is the physics of the problem, but more importantly, they do this tuning for all the time period, whereas there is a strong bias using only EPICA data, which is associated to a very cold period compared to the whole period they are studying. Indeed, most of these 38 million years were much warmer than LGM or present day climate. Therefore, there is no reason for a constant tuning.

We would like to remind the reviewer that all models used to simulate climate and/or ice sheets are to a certain degree tuned in some way. We want to be very clear that the cloud optical thickness is such a tuning factor used in our climate model. Indeed, there is no physical preference for its value before or after re-tuning. Precisely therefore we choose this factor to regain agreement with the EPICA Dome C core. Although we agree that physically cloud properties may change in different climates, we think using a variable parameter setting is cumbersome and does not lead to increased understanding of the studied system. However, we will explain the implications of this choice more precisely in the revised manuscript.

This also explains why the underestimation is so large for deep time (larger for Zacchos than for Raymo). This methodology by itself induces many problems and leads the authors to explore methodological induced problems, as hysteresis, rather than to really try to capture the dynamics of the cryosphere, or the evolution of the climate in their result section.

We are not clear on which underestimation the reviewer refers to here. Also, we are confused by the argument that we do not try to capture the dynamics of the cryosphere, or the evolution of the climate. In our opinion, this is thoroughly dealt with in the Section 4, which will be transformed into a new section Results and Discussion II.

Part 3 results: The part concerning hysteresis is not relevant and convincing for me. Hysteresis has been shown to be an important factor to account for instance in glacial/interglacial cycles (see for instance papers from Paillard Nature 2001, Calov, GRL, 2005. Alvarez-solas Nature Geosci, 2010, De Conto and Pollard Nature 2008. . .). Here the analyses of the results which depict a strong correlation with the initial climate is not really explained in terms of physics and for me belongs much more to model caveats and development than to the analyses of results interesting enough to be published.

We are aware that ice sheet variability, as well as other feedbacks, may cause hysteresis in models and possibly also in the real world. This hysteresis, which is also to some degree contained in our coupled model, however, is inherently different from the hysteresis we explore in this study. We aim to find out why the simulated CO_2 over the past 5 Myr is so different if we start our simulations further back in time. This is in fact not caused by a feedback, but by the core of the model we use, since it shows up even when all feedback processes are shut off. The hysteresis shown by our model may therefore very well be a model caveat. We, however, think this is all the more reason to be honest about its implications, particularly because results of this model have already been published before. Moreover, more sophisticated models have, as far as we know, not been tested for this behaviour. We therefore advise to check for this, before using such models on long-time scales (when computer power permits to do so). This is also in line with our studies objective of identifying interesting phenomena and potential obstacles for transient long-term simulations.

Part 4 discussion: In the discussion section, the summary of the paper is too exhaustive, we really expect a discussion of the results and comparison with the results of other models. For example, these last years, many studies provided by De Conto and Pollard depicted very new results on climate and ice sheets evolution, since the last 40 million years. In this part, we should expect a serious comparison between these results and those provided by the others including the fact that the tools used are different. Therefore, it would be interesting to discuss the result of these two complementary approaches (GCM versus simplified models). Such a discussion will allow the authors to clarify the potential and weaknesses of their method. For instance, simplified tools as used here do not capture important processes that are necessary to simulate ice sheet evolution in GCM. The authors show comment on this point in the discussion section and also highlight on the fact that their tools allow to quantify different forcing factors through the sensitivity experiments.

We realise now that the discussion of our results in relation to previous studies from our own and other research groups is a little bit obscured by the combination with the summary of our current study in this section. We will therefore move the discussion to the two new Results and Discussion sections in the revised manuscript. In the Introduction, and in these Results and Discussion sections, we will also include a discussion of our results with respect to the work of Pollard and DeConto. In the new Summary and Conclusion section we will present our main conclusions.

Conclusion: I strongly believe that there is much room for improvement in this paper. The sections that are devoted to sensitivity experiments (albedo vs topography of the ice sheets) could be a valuable contribution, but at this stage and, accounting for the weaknesses in methodology and construction design of the paper, I think the paper should be rejected. Nevertheless, there are some parts of paper, that, if completely rebuilt could be used and might be a valuable contribution, but in a framework of a completely new and rethought paper.

It is unfortunate that the reviewer thinks the results of our current model setup are not suitable for publication. In our opinion, we provide a complementary approach to snap-shot and short timescale results of more sophisticated models, by focussing on the larger picture of the long-term influence of ice sheets on the climate. We make a step forward from just using stand-alone ice sheet models (e.g. De Boer et al., 2010). Even though this does not lead to any definite answer – there is always a way forward in science – it represents a marked improvement with important results and implications, one of them indeed being the attribution of the effect of ice sheets on the climate to albedo and topographic changes. We think it is therefore a meaningful contribution to the research field. Nevertheless, as we pointed out, we will make an effort to make the merit of our approach, and the purpose of our study clearer, mainly by improving the structure of the paper and by adding more discussion in order to show the embedding in the existing literature which has more focus on detailed time slice simulations.

References:

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