

## ***Interactive comment on “The influence of ice sheets on the climate during the past 38 million years” by Lennert B. Stap et al.***

### **Anonymous Referee #2**

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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. 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

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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 CO<sub>2</sub> 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 pCO<sub>2</sub> (see Godderis for a review). For instance opening and closing sea ways may change climate and CO<sub>2</sub>, 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 pCO<sub>2</sub> 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 pCO<sub>2</sub> 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. 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. 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 CO<sub>2</sub> temperature and ice sheets are consistent within the framework of the modeling study but completely inconsistent with available data concerning CO<sub>2</sub> evolution since 38 million years. This

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is clearly shown in the paper but not in the abstract itself. 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.

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.

Methodology section: First, the authors claimed they used Penthic  $\delta^{18}O$  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 CO<sub>2</sub>. I strongly believe that in a first step, the authors should have used the different proxy reconstruction than for CO<sub>2</sub> as published in the literature, which provides different CO<sub>2</sub> evolution (Boron isotopes, Alkenon, leaf stomates, . . .) to validate their simplified coupled model. Such a strategy based on CO<sub>2</sub> reconstruction from data allows to test the response of their tool in terms of cryosphere and climate evolution. Instead, they choose to compute the CO<sub>2</sub> from the reconstructed SST, derived from their radiative model. As you know,

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there are many reasons and causes that may affect atmospheric CO<sub>2</sub>, 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 CO<sub>2</sub>, is obviously missing a lot of important processes that will change the atmospheric CO<sub>2</sub> during that period. 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. 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). 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 CO<sub>2</sub> when compared to different proxies: the CO<sub>2</sub> computed from the temperature record of Zaccos or Raymo, but also those much more accurate and directly obtained from Antarctica ice core (EPICA). 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. This also explains why the underestimation is so large for deep time (larger for Zaccos 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

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the climate in their result section.

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.

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.

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

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completely new and rethought paper.

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