

## ***Interactive comment on “Modelling ice sheet evolution and atmospheric CO<sub>2</sub> during the Late Pliocene” by Constantijn J. Berends et al.***

### **Anonymous Referee #2**

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The Pliocene is the focus of numerous modeling studies due to its warmer climate compared to present day. By understanding the climate variation of this period, we could tentatively understand the climatic consequences of our present-day warming climate. This paper uses a global coupled climate model with forcings including benthic oxygen observations along with inverse modeling method to model the evolutions of pCO<sub>2</sub> and ice sheets. The authors have a particular focus on the MIS M2. This work shows promising results and contributes greatly to the field of paleoclimate modeling and could lead to a better understanding of our past climate and I would really like to see this work published. While the methodology is well thought of, other aspect of the paper requires consideration before being published.

General comments:

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1. The paper is well written with good language and grammar.
2. The paper focuses a lot on the MIS M2 while the title mentions the modeling of the Pliocene.
3. Throughout the paper, acronyms and notations failed to be defined consistently as they appear in the text. I should note that the abstract should be considered somewhat independent from the main core of the paper and definitions and acronyms should be (re)defined starting from the introduction. Once they are defined they do not need to be redefined thereafter. Also, while some notation might be very common to some people specialized in the field, not everybody is an expert as they first read a paper.
4. Throughout the paper, please try to define/explain concepts, ideas or results directly when you mention them, not several paragraphs later. If a concept is explained later, simply indicate the section.
5. In general, the paper relies a lot on studies or modeling setup prior to this work. I am one these persons who does not like to have to read 10 papers before being able to know what is happening in one paper. I am not talking about adding in depth details but brief descriptions and summaries of the major model configuration or idea that is being used in the current study.
6. The introduction is relatively short for all the previous work it is trying to highlight. I would recommend briefly stating the findings of previous studies and contrast them with the novelty of the work presented here with obvious differences and the advantage(s) of your new approach. In particular, Berends et al 2018 is heavily cited throughout the paper and sometimes I felt like it was a prerequisite to this paper. A brief summary of this paper in the introduction would be appropriate.
7. Please provide more details regarding equations 1 and 2, their origins, citations, the link between the 2, and the definitions of the symbols. Also, are these equations valid for both the LGC and the Pliocene?

8. Many results should be emphasized quantitatively rather than using generic words such as “results agree well”, . . .

9. The climate matrix is well suited for the validation of the LGC which presents colder climate compared to the Pliocene. I would say this is a major caveat of this study and ideally more Pliocene-like climates should be included in the matrix to validate the Pliocene.

10. The paper does not include a discussion of the importance of the perturbed parameters. Are any of them more sensitive than others? Along these lines, while parameters might have a big influence on model output, so does the resolution of the model particularly for ice sheets. A grid resolution of 40 km for Antarctica without proper grounding line dynamics sub-parameterization included in the dynamical solver will lead to large model differences (the same will hold true for Greenland but likely less so). I understand the computational expense of running paleo climate simulations, but the study would benefit from a run (even if it is for Antarctica only) at a resolution higher (20 km for Antarctica) of the one used in the current presentation and using the benchmark parameters. This run could be done for the Pliocene and stopped after MIS M2 and compare this portion of results only (to the 40 km with benchmark) if computing time is an issue.

11. The modeling section should include more details about the process of the modeling and clearly laying out the strengths and drawbacks. More importantly some parameters that are varied are not defined anywhere in the text (e.g. enhancement factor, . . .) They should be tied in with the modeling section with their impact on the model. Also, a better description (even brief) of the coupling should be included. Does the ice sheet model feeds back into the climate model in terms of ice sheet topography, ice extent and fresh water fluxes? Also, your model does include ocean forcing under floating ice which I discovered only at the end of the paper. This detail should be included in the modeling section when you are talking about the ice sheet model. Feel free to discuss the caveat of your method in the discussion.

Specific comments (I will highlight them as they appear in the paper). Some of the Rewordings are merely suggestions and you should feel free to consider them as you see fit.

1. Page 2 line 9: why focusing only on the first 20kyr and not the 60kyr that see the increase? Also, is the benthic oxygen time series by Lisiecky & Raymo the only one available? If not why use this one?
2. Page 2 line 8: define MIS M2 (you did in the abstract but please do it once in the main text).
3. Page 2 Figure 1: I am aware of the tendency in geological study to have the time series displayed with decreasing time but I still find it confusing, especially for modeling studies.
4. Page 3 line 4: add reference(s) for HadCM3.
5. Page 3 line 14: the matrix method is mentioned here for the first time but not explained. You will do so in section 2 so add something like “(see Sect. 2.3)”.
6. Page 3 line 15: add reference(s) for ANICE.
7. Page 3 line 16: be more quantitative to define the word “accurately” (especially for paleo study).
8. Page 3 line 18: why is the matrix method applied specifically to MIS M2 while the title of the paper aims at modeling the Pliocene?
9. Page 3 line 25: define LR04 stack and add reference(s).
10. Page 3 line 27: replace “ice-climate” with “ice sheet-climate”.
11. Page 4 line 7-8: “has resolution ...”. Maybe you could say a few words on the choice and practicality of this resolution.
12. Page 4 line 13: add reference(s) for SSA.

13. Page 4 line 14: add reference(s) for SIA.

14. Page 4 line 15: “with basal stress included in the SSA.” On page 4 line 13 you mention that SSA is used only for floating ice and one can wonder why you include it in SSA. Instead add some precision in your text stating that there is a length scale over which the model transitions from SIA to SSA upward of ice shelves. This is what the PISM-PIK model does and your ice sheet model sounds to be very similar so I would add reference(s) to it.

15. Page 4 line 19: “for this application is 20 km for Greenland and 40 km for Antarctica. . .”. Say a few words on the reason why you chose this coarse resolution for these ice sheets and why do you think they are adequate for the kind of modeling you are doing. (A recent paper (Goeltzer et al. 2016, Parameterization of basal friction near grounding lines in a one-dimensional ice sheet model) uses a resolution of 10 and 20 km for Greenland and Antarctica respectively.) Especially when modeling marine ice sheets, numerous studies have shown that a resolution of 100 m or higher is necessary to accurately model grounding line transition (Gladstone et al. 2012, Leguy et al. 2014, Parameterization of basal friction near grounding lines in a one-dimensional ice sheet model) which in turns impact sea level change prediction (which you are looking at in your paper). This requirement can be relaxed somewhat if using a grounding line parameterization which you are not. The lack of horizontal resolution for Greenland and Antarctica will likely lead to the greatest uncertainty in your study (even more so when applying basal melt rate under ice shelves). In the discussion, please clearly indicate what ongoing development you are considering for future modeling of the Pliocene.

16. Page 5 line 14: in a few words, describe what PRISM3 is, the configuration you are using, and reference(s).

17. Page 6 line 3: “200 ppmv pCO<sub>2</sub>”, do you mean instead “220 ppmv”?

18. Page 6 line 9: “because the ICE-5G. . .” It is the first time you mention ICE-5G and you mention it later on (page 9) again. What is the difference between ICE-5G

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ice sheets used in Pelletier 2004 and the ICE-5G LGM? Maybe a short paragraph about ICE-5G might be adequate, highlighting what is used for the purposed of your simulation and why.

19. Page 6 line 15: replace “Northern Hemisphere” by “Northern Hemisphere (NH)”. The acronym “NH” is used two lines below without being defined.

20. Page 6 eq. 1: define the terms used in the equations and comment on the validity of the equation. Also, add reference(s).

21. Page 6 line 21: a simple suggestion, simply define the meaning of the upper bar notation in your equation this way you would not need to redefine both entities in each equation.

22. Page 6 line 22: replace “PD” by “present day (PD)”.

23. Page 7 Figure 3: this figure is really nice, clear, and worth many words. Maybe it is worth mentioning earlier on, towards the beginning of Sect. 2.3.

24. Page 9 line 4: reword “last four glacial cycles” as “four last glacial cycles (LGC)”. You mention the acronym LGC in table 1 without ever defining it.

25. Page 9 line 11: “The values that were used . . . Table 1.” The text does no mention anything about the motivation of varying model parameters by 10%, nor in the table, please do so.

26. Page 10 line 8: please define the subscript “sw” somewhere.

27. Page 10 line 9: please define the subscript “dw” somewhere.

28. Page 10 line 15: replace “Surface temperature anomalies” by “Surface temperature anomalies ( $\Delta T_s$ )”.

29. Page 10 line 15-16: divide the ice core records citation per ice sheet similarly to the way it is done in Figure 7 caption.

30. Page 10 Figure 6: the figures show results from Shakun et al. (2015) but these results are never referenced in the text. Please do so and describe their importance for your model comparison or do not display it. In the figure caption, be consistent with your label and add the publication year for Shakun et al. Also, explain what panels B-D are, not only A. Finally, similarly to what is done in figure 5, provide some metric (like the  $R^2$  value) measuring the difference in fitting between this study and Berends et al. 2018.

31. Page 10 line 17: replace “temperature records is comparable. . .” by “temperature records are comparable”. Also, be more quantitative rather than simply using “comparable”. You can provide similar comparison as what you did for Figure 5 (using  $R^2$  and RMSE). For Antarctica one could argue that your results are not comparable especially between -80 kyr and -20 kyr. For Greenland, both models fail to capture the strong minimum around -70 kyr, is there an explanation for that?

32. Page 10 Figure 7: similar remark as for figure 6, provide quantitative metric.

33. Page 11 line 8: “is much better in the simulation here”. Provide quantitative comparison.

34. Page 11 line 11: “possibly somewhat”, This wording is really vague, try to omit it.

35. Page 11 line 9-13: “Whereas . . .” this sentence carries a strong statement without being shown (you have not shown any sea-level variation for this study). Please provide a figure supporting your explanation. Also, this sentence is really long, try to split it in 2.

36. Page 11 line 16: “ice geometry”. Nothing in the paper supports this claim. Either you have the results and you can show them or you could add something like “(not shown)”.

37. Page 11 line 23: spell out GIA since used for the first time.

38. Page 11 section 3.2: add a few words in this section indicating why the climate

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matrix you used to run your Pliocene simulation is appropriate.

39. Page 12 line 7: replace “sea-level drop” by “sea-level change”. The values for KM5c are not dropping.

40. Page 12 line 7: “MIS M2 (3.295 My ago) . . .”. No need to repeat “(3.295 My ago)”, it should be mentioned earlier on.

41. Page 12-16: see general comment 9.

42. Page 12 line 18: replace “ice volume” by “sea-level contribution”.

43. Page 12 line 20: “. . . when pCO<sub>2</sub> rises again”. A similar observation can be made with the strong peak at -2.8 My ago. Why is that time less important than MIS M2?

44. Page 13 Figure 9: mention in the caption what baseline you are using for your comparison (current sea level rise or from the beginning of the simulation?). On the figure, there is no need to repeat the y-axis titles for every subfigure after mentioning the one for North America. In your figure caption, please add something like “note the different y-axis limits”. Also, replace “Volumes” (the first word in your caption) by “sea level contribution” to be consistent with your y-axis label.

45. Page 13 Figure 10: spell out SLE since never used before.

46. Page 14 line 6-8: which reconstruction should be used as a benchmark?

47. Page 14 line 6-7: “our model results agree well . . .”. Similar to previous remarks, be more quantitative. Also, describe the way in which the results are in good agreement; is it because of the trend or else?

48. Page 15 Figure 12: similar to Figure 9, indicate your base or reference for the global mean sea level.

49. Page 15 line 14-16: “However, since . . .”. I am puzzled with this sentence. It makes me believe that the AND-1B sediment core results cannot be trusted. I then wonder



why Figure 13 is shown at all in the paper and what value it adds. Maybe you could add this comparison (or lack of) to the discussion section or simply remove it from your paper.

50. Page 16 line 14-15: “it performed at least equally well”. None of figures 5-7 show quantitative results supporting this statement. Figure 5 does not show results from Berends et al. 2018, and figure 6 A shows that this study performs better compared to Berends et al. 2018. Please reword.

51. Page 16 line 18: no need to repeat “(3.295 My ago)”.

52. Page 16 line 23: “(1-3 m SLE)”. Do you mean “(1.5-3.5 m SLE)” as stated in Fig. 10?

53. Page 17 line 1: no need to repeat “(3.205 My ago)”.

54. Page 17 line 4-7: “The larger uncertainty ...”. Again, this is a strong drawback of this study. Please, discuss a bit further the impact on the Pliocene results or why the study is legitimate.

55. Page 17 line 19-24: some of the pros and cons of the matrix method mentioned here should be added in Sect. 2.3.

56. Page 17 line 27-28: “sub-shelf melt ...”. It would be good to add in Sect. 2.2 that sub-shelf melt rates are applied based on ocean forcing.

57. Page 17 line 27-28: “temperature-based formulation by Martin et al. (2011) ...”. Here or in the methodology section, be more specific on the relationship between melt rate and thermal forcing (i.e, linear like equation 5 in Martin et al., or quadratic as in Polard & DeConto (2012), or else). The impact could be important. Note that the relation suggested by Polard and DeConto is considered more accurate.

58. Page 17 line 32-33: “... and possibly ...”. I am struggling with this sentence as it sounds like it contradicts your sentence on page 15 line 16-17. Either the AND-1B

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ice core has discrepancies and is not to be trusted or it should be and a more reliable comparison needs to be done in the result section.

59. Page 18 line 8: “East Antarctic . . .”. This is also true for West Antarctic ice sheet where grounding line dynamics plays an important role.

60. Page 18 line 10: “grounding line retreat (Schoof 2007,...)”. Please, also cite Leguy et al. 2014 (Parameterization of basal friction near grounding lines in a one-dimensional ice sheet model) who went further in the grounding line influence as it investigated grounding line representation for different sliding laws, one being closer to the Mohr-Coulomb you are using in your model.

61. Page 18 line 26: Maybe replace “we can not . . .” by “we did not”.

62. Page 24 Table 1: In column 4 (and sub-column 3) and row  $e_{\{SIA\}}$ ,  $e_{\{SSA\}}$ , do you mean “5.6, 0.5”? (to be consistent with sub-column 1). If not why this difference in increment? Also, according to subsection 3.2, you not only used these model parameters for LGC but also for the Pliocene simulation.

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-34>, 2019.