

Interactive comment on “Individual and combined effects of ice sheets and precession on MIS-13 climate” by Q. Z. Yin et al.

Anonymous Referee #1

Received and published: 18 April 2009

Formal review for paper “Individual and combined effects of ice sheets and precession on MIS-13 climate”,

By Q. Z. Yin, A. Berger, and M. Crucifix

Overall comments:

This paper investigates an interesting interaction between ice sheets and summer monsoon under different insolation forcings. The paper is built on the foundation of previous studies by the leading author. Overall, it is an interesting paper to read and focuses on MIS-13 climate. I recommend this paper for publication in *Climate of the Past*, however not before a major revision. The paper is not well written in a few ways: 1) lacking critical references to Asian monsoon studies (e.g., Drs. Z. An, X. Liu, CAS; Y.J.

C64

Wang, Nanjing Normal University), 2) unrelated references cited in (see details below), 3) lacking scientific mechanisms to link ice sheets with African and Asian monsoon systems, 4) the role of land-sea temperature contrast on monsoon precipitation is not discussed at all, and 5) English improvement is urgently needed. Based on my evaluation of the paper, I would suggest the authors to revise their paper to accommodate my comments.

Major concerns:

1) Lacking references to Asian monsoon studies. Over the past decade, significant advancement has been achieved in reconstructing and/or modeling Asian winter and/or summer monsoon systems in the past. However, I could hardly find any of these critical references being cited in the paper. For example, Drs. Z. An and X. Liu of CAS have studied Asian monsoon systems in many aspects. Dr. Y.J. Wang of Nanjing Normal University has published quite significantly in the field of Asian monsoon.

2) Unrelated references were cited in the paper. For example, de Menocal (1995) is a study of Africa climate, but was cited for Asian temperature and hydrology (page 559, line 10); Ding et al. (1994) was cited for low latitude monsoonal climate, while it dealt mainly with Asian winter monsoon (page 559, line 10); and Felzer et al. (1996) talked about the warmer temperature downstream of an elevated ice sheet (adverse effects from cooling), while was cited to favor a positive interaction (enhanced cooling). Notice Felzer et al. used a slab-ocean model.

3) Results presented in the Abstract lack scientific insights to explain why high latitude ice sheets could influence the climates (temperature and precipitation) over Africa and Asia (low latitude). What are the mechanisms to connect the ice sheets with monsoonal climate? The authors just describe their results from the model simulations. There is a need to spend less time on describing the figures and more time on trying to understand how these figures can bring new information.

4) The uncertainty to model simulations and experiment designs is not discussed. In

C65

addition, the role of land-sea temperature contrast on monsoon circulations is not discussed at all.

5) English needs enhancement. In particular, concise (scientific) written English is expected for peer-reviewed journal publications, while at current stage verbose and/or obscure sentences can be found in the paper.

Addition comments:

1) Abbreviations were substantially used in the paper and Abstract. Sometimes it is overwhelming. I strongly recommend reducing the usage of abbreviation unless needed. For example, if you choose to use NHS for Northern Hemisphere Summer, please use it consistently throughout the paper. However, I think that it may be clearer to use NH for Northern Hemisphere, and use SH for Southern Hemisphere. East Asian Summer Monsoon (EASM) was defined twice in Abstract and text (page 560, line 22). You should choose define it once. Again, NHS was defined twice. ITCA (LOVECLIM) was not defined. S6 (OAGCMs) was defined, but not used in the paper. Please define abbreviations only when needed and at the first occurrences in the paper. For example, SH should be defined in the first occurrence. Please do not define P and A for perihelion and aphelion. They are quite confusing.

2) I am not convinced if the study of Felzer et al. (1998) is a good reference to be cited to support your argument (page 559 the end to page 560) because they used a slab-ocean model to simulate the impact of ice sheet on climate.

3) Please change “much heavier” (page 560, line 28) to “much larger”. Right? You do not mean extreme events in precipitation. You mean larger amounts of precipitation.

4) I think that the scientific questions are not well presented near the end of the Introduction. You also need an outline to direct reader through your paper at the end of Introduction.

5) Page 562, line 5, “authors” should be “studies (reconstructions)”.

C66

6) Page 562, line 6, “they are” should be “there is”. “with the assumption” should be “using an assumption”. Line 27, “for” should be “based on”. Line 17, “According to different authors” might be “Based on previous studies”. There are many trivial English problems in the paper.

7) Delete “it means within a factor of 2.5”. It is redundant.

8) Figure 1 is from Berger’s calculation. Right? It is not the model simulation of MIS-13 (page 564, Line 21). Please state it out.

9) Page 565, line 10-11. Please delete all statement beginning with “it means.”. Please state the results directly by changing the previous sentence. This part reads very confusing and repeating. You want to be clear and concise.

10) It is debatable if the climate response (section 3) should be focused on precipitation changes. The results on temperature changes are not new. You have already cited the study of Felzer et al. (1996). You might want to downplay the discussion of temperature changes in the paper. I suggest that you focus on precipitation changes and the pressing mechanisms of those changes. This will substantially reduce those discussions on temperature.

11) Why did you choose four ice sheet volumes for 506 ka BP, while two ice sheet volumes for 495 ka BP? It is not clear to readers.

12) You described your results for precipitation without any discussions of scientific mechanisms. I suggest plotting the wave train for standard and sensitive runs.

13) Please move “or topography” after albedo and delete “alone” (page 573, line 7). The new sentence reads as “albedo or topography as the only forcing”.

14) It is assumed that summer monsoon circulation is caused by the strong temperature gradient between land and ocean. I cannot find any discussions on this pivotal point in the paper. It seems that the wave train becomes the only factor to control the monsoon.

C67

15) Regarding the results for 495 ka BP, the discussion should focus on the difference in solar forcing and its impact on monsoon. Overall, the whole paper reads not very focused.

16) Color contours were used in the paper. However, I think color shading might be easier to readers. This is true even for those plots with wind vectors.

Interactive comment on Clim. Past Discuss., 5, 557, 2009.