Dear Anonymous Reviewer,

Thank you for reviewing our manuscript and providing insightful feedback. We are definitely flattered by your several mentioning of “multiple interesting papers by the lead author”. Below, we reply to your comments (marked as **bold**) and propose several changes to the manuscript motivated by your suggestions.

1. The authors use a Saltzman-style 3-equation in 3-unknown model of the ice ages. They explore the response to pure obliquity, pure precession, and combined Milankovitch forcing, and examine the dependence on initial conditions. There is no question that the subject is interesting and important. The authors use a relatively simple model, which again is very appropriate in this context.

Response: We are grateful that you find this paper to be interesting and important and the model employed to be appropriate.

2. It seems, however, that the paper largely repeats previously published analyses, which the authors do not discuss. I recommend a major revision that will allow the authors to search and examine the related literature, discuss it, and reformulate their analysis and discussion in a way that focuses on some novel elements to be identified.

The authors may want to take a look at Tziperman et al. 2006, "Consequences of pacing the Pleistocene 100 kyr ice ages by nonlinear phase locking to Milankovitch forcing", as well as papers that are cited and cite this paper. This could be a useful entry to the idea that glacial cycles are phase locked to Milankovitch forcing. Figures 1 and 2 in the current paper, and possibly additional ones, seem to repeat analysis in that paper and in related papers that have been written by multiple authors. Phase locking to Milankovitch implies that different initial conditions converge to the same ice volume trajectory. This seems directly relevant to the goals of this paper, yet this draft does not mention this concept.

Response: To make things really simple: (a) nonlinear phase locking to Milankovitch forcing is about applying orbital forcing to a short-memory terrestrial system; (b) this paper is about applying Milankovitch forcing to a long-memory terrestrial system.

Nevertheless, your suggestion to have seminal Tziperman et al (2006) paper as a reference is a very good one. Definitely, when we are talking about initial values we should not forget about non-linear phase locking mechanism. As Tziperman et al (2006) postulate, phase locking requires some kind of dissipation in the terrestrial climate system that erases the memory of initial values from the time series. From this perspective, Figures 1-3, as you correctly observed, may be interpreted as phase locking.

At this point non-linear phase locking becomes almost irrelevant and beginning with experiments described in Fig.4 and further we are investigating a very different mechanism. Specifically, we are describing in fact *a novel physical phenomenon when orbital forcing changes the terrestrial physics in such way that instead of erasing memory of initial conditions, this memory is extended and initial values become major governing parameters.* Apparently,

under favorable initial values, such long-memory system can also reproduce major events of the past.

To summarize: Up to date, there have been two commonly accepted interpretations of the Milankovitch theory: ice ages either are directly driven by the insolation changes, or they represent the internal property of the terrestrial physics, paced by the orbital forcing. With this paper, we introduce the third reading: As it is evident from the structure of the C-similarity parameter (we illustrate it graphically in Fig. AC2-1), the orbital forcing may change the terrestrial physics (in our case, the timescale of ice vertical advection) thus being the enabler of its sensitivity to the terrestrial initial values.

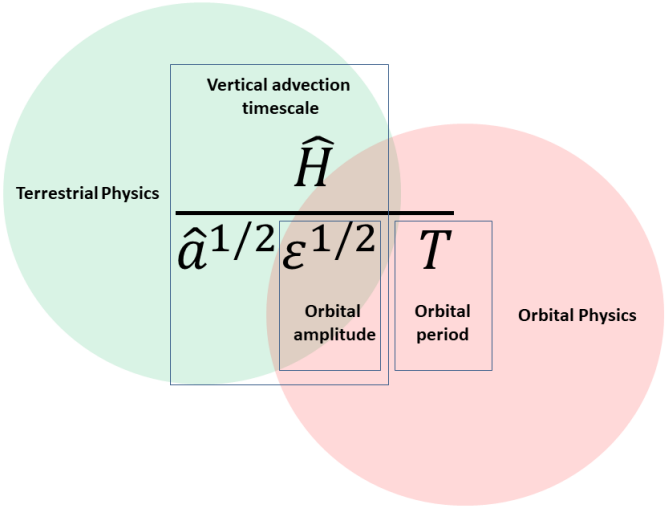

Figure AC2-1. The C-number, i.e., $C = \frac{\hat{H}}{\hat{a}^{1/2}\varepsilon^{1/2}T}$, as the ratio of the orbital-forcing period and *orbitally modified* vertical advection timescale of ice-media.

This interpretation of the Milankovitch theory, where initial values are in control of the dominant-periods trajectories, that is reflected in the title of the paper “as an Initial Value Problem” is just an opposite to phase locking concept that is based on initial-values independence.

Action: We will better articulate the novelty of the paper relative to nonlinear phase locking.

3. The initial couple of paragraphs where simple models are advocated seem unnecessary in this research field where simple models have been used for many decades now, including in multiple interesting papers by the lead author.

Response: Yes, this is true, but as ice-age simulations with 3-D and intermediate complexity models are now more and more affordable, are these simulations more credible than the results obtained with dynamical (but physics-based) models? In his “Dynamical Paleoclimatology” Barry Saltzman (2002) insisted that “The essential slow physics is to be sought in the low-order model”, and the “supermodel” should be tuned to satisfy a “best available” dynamical model constraints. While I am personally convinced that he was right, I am familiar with some scientists

who would consider such views as extreme. Therefore I hope that some additional arguments in the Introduction (like for example, Bahr et al, 2015) that were not available at the time when “Dynamical Paleoclimatology” was written may actually provide support for Barry Saltzman very strong vision and are appropriate in the paper devoted to his memory.

Action: We will add the above discussion into Introduction

4. I am not convinced the analysis of the linear equation (1) and its solution (2) adds a meaningful message. "A model should be as simple as possible, but not simpler". This model may be "simpler", given the sophistication of idealized glacial models in the literature over the past decades, including the work of the lead author over the past decades. This linear model shows no oscillations, does not include Milankovitch forcing, and its relevance to the ice age problem is questionable. I do understand the lesson the authors are trying to derive here, but it seems to me that a sentence or two could make the point without this linear equation. My feeling is that our understanding of ice age dynamics is too sophisticated for this linear equation to provide any new or useful insights.

Response: The goal of bringing the linear equation (1) and its solution (2) was to introduce readers to some simple concepts of memory, memory duration, memory-duration sensitivity to initial values, etc. When the preprint of the paper was published, I asked several colleagues if such introductory part is helpful. The following is the part of the email exchange (my correspondent is mathematician from German major climate research institution):

MV: *Since you have already read the Introduction, a quick question: Do you find this simple linear example to be helpful? Or this is such a common knowledge that it may be unnecessary?*

Correspondent: *For me it was very useful to get started with the language and with the notation. In fact, I will need to go back to the observations following equation (2) and make sure that I understand them fully.*

Action: I hope it will not be considered as confrontational if I keep equations (1) and (2) for such readers but I promise to shorten this part as much as possible.