Orbital Insolation Variations, Intrinsic Climate Variability, and Quaternary Glaciations

First of all, we would like to thank all three referees for their very thorough and careful reading of our paper. Following their constructive criticism and valuable feedback, we would like to propose several changes to the manuscript. We are convinced that these changes will substantially improve the quality and clarity of our manuscript and that they will address the referees' objections, questions and suggestions. Whenever we prefer to leave the current version of the manuscript unchanged, where a referee has proposed a change, we have made an effort to justify our view thoroughly. Finally, there is some overlap between the remarks of the referees. We have taken the freedom to answer some comments by more than one referee simultaneously. Whenever a point raised by a certain referee has already been addressed in our reply to another referee, we simply refer to this answer.

In order to improve the readability of our replies we applied a color coding to discriminate our replies from the referees comments. Please understand that we have attached our replies as a pdf document since color coding is not available in this browser based text editor.

Color coding:

Comment by the referee.

Reply from the authors.

Text from the original version of the manuscript.

Suggested improved text.

Referee 1:

I wonder actually who first-authored the paper. If Prof Ghil "conceived and designed the study", why would he not write the paper? Or, if he did, why is he not the first author?

We have chosen the order of authors as usual according to how much they contributed to the final manuscript. We agree that the 'authors contribution' statement leaves room for interpretation and therefore will change the current wording

'MG conceived and designed the study. KR and TM carried out the numerical computations. All authors interpreted and discussed the results and wrote the manuscript.'

to

'MG conceived and designed the study. KR and TM carried out the major part of the article's new research. All authors interpreted and discussed the results and wrote the manuscript.'.

I do sympathise with Dr Daruka Istvan (whom i don't know) if he has misgivings about any misrepresentation of his work, even if inadvertantly, especially regarding novelty. Although i should say that being completely ignored would be worse. I would like to kindly request from the authors that they do their utmost to be fair. Probably i didn't even need to say this any more.

In our reply to CC1 (posted on 26.11.2021) we already indicated that in a revised version of the manuscript we will be more specific about the differences between the original and the modified version of the model that we use.

In particular, following the sentence (line 450)

'The model's variables are a global temperature anomaly y that is proportional to minus the global ice volume and an effective climatic memory term x that represents the internal degrees of freedom.'

we will add the sentences

'In the original model formulation of DD16, MPT-like behavior was produced by slowly varying the parameter $\kappa(t) = \kappa 1 + 0.5(\kappa 0 - \kappa 1) \{1.0 - \tanh((t - t0)/ts)\}$. We deviate from DD16 by introducing instead a slow change in the parameters $\alpha(t)$ and $\beta(t)$ of Eq. (29b), as follows: $\alpha(t) = 2.1 - 1.4 \tanh((t + 1100)/500)$, (30a) $\beta(t) = 2.5 + 1.4 \tanh((t + 1100)/500)$. (30b). The functions $\alpha(t)$ and $\beta(t)$ so defined are plotted in Fig. 10(b) and they induce, as we shall see forthwith, a change in model behavior that not only resembles the MPT but also shows correct timings for most of the terminations.'

Furthermore, we will explain better why we actually selected the DD16 model out of the large number of available conceptual glacial cycle models that do reproduce the MPT. To do so, we would replace

(line 447)

'In this section, we illustrate how the PBA concept can help shed more light upon the dynamics of ice age models. For this purpose, we apply the Daruka and Ditlevsen (2016) model of glacial-interglacial cycles with slight modifications. We show first that this model approximates rather well the glacial cycles inferred from the benthic δ18 O proxy reconstruction of Lisiecki and Raymo (2005) and then compute the model's PBA to investigate the dynamical stability of its glacial cycles.'

by

'In this section, we illustrate how the PBA concept can help shed more light upon the dynamics of ice age models. Among glacial-cycle models, the Daruka and Ditlevsen (2016; hereafter DD16) model belongs to the more abstract type, as it is not derived from detailed physical laws. However, its concise form, its interesting nonlinear dynamics and its ability to simulate the glacial cycles, including the MPT, make the DD16 model well suited for our purposes. Nevertheless, in what follows, we first slightly modify this model, so as to better approximate the benthic δ18O proxy reconstruction of glacial–interglacial cycles in Lisiecki and Raymo (2005), especially insofar as its timing of glacial terminations; please compare our Fig. 10 with Fig. 1 in DD16. Then we compute the PBAs of the DD16 model so modified to investigate the dynamical stability of its glacial cycles.'

Finally, in a revised version of the manuscript, we would include a table that comprises conceptual glacial cycle models of low dimensionality that consistently reproduce an MPT-like behavior. Doing so, we aim to stress that many plausible mechanisms for the MPT have been proposed, and that other models merit investigation along the lines of the present approach.

1. The term "interaction" is used in this paper as often as we encounter it in general. However, i don't really understand what is meant by this so often, including in this paper. Please clarify, or, if it is not possible, avoid using this language. As i understand, interaction is about two-way causality, which only makes sense in terms of couplings in governing equations.

We thank the referee for highlighting this linguistic inaccuracy. We agree that the term 'interaction' should be reserved for situations in which two dynamical variables influence each other.

There are several occasions in the manuscript, where we aim to say that only the combination of external forcing and internal variability of a system can explain the observed variability of the forced system. Wherever we used formulations like '*interaction between external forcing and internal variability*' we will replace the current wording by a different formulation, like '*the combination of external forcing and internal variability*' or the '*the external forcing modifies the system's variability*'. The individual changes we propose with respect to this comment are comprised in our answers to the list of the referee's minor comments further below.

2. I also don't understand the paper's distinction between stochastic and deterministically chaotic sources of (internal) variability.

3. In contrast, i think we should distinguish between external forcings versus influencesunder internal variability. Stochastic terms in equations are not meant to represent external forcing.

We believe this comment mostly refers to the statement in line (194)

'*In returning to the "fundamental question #2" in Box 1, one must recall that — apart from deterministic chaos à la Lorenz (1963), as obtained by H. Le Treut and colleagues (Le Treut and Ghil, 1983; Le Treut et al., 1988) and shown here in Fig. 4(a) — stochastic contributions à la Hasselmann (1976) to the continuous part of the paleoclimatic spectrum must also play an important role.*'

which was also commented upon by the referee as follows:

I thought Hasselmann uses the stochastic framework only to model time scale separation. Otherwise, what appears as noise is clearly deterministic irregularity due to chaos. Perhaps this distinction should be avoided, or, made precise if possible. The distinction should have to do something objective -- not just subjective, whether we bother to resolve time or not.

We beg to differ: it is well known that (i) "one person's signal is another person's noise"; and that (ii) there is no real spectral gap in atmospheric, oceanic and climatic variability; see, for instance, Nastrom & Gage (*J. Atmos. Sci*., 1985). The use of deterministic vs. stochastic description of certain processes depends on the availability of data, the need for certain levels of detail and accuracy, and other modeling considerations; see, for instance, Palmer & Williams (eds.), *Stochastic Physics and Climate Modelling*, Cambridge U. P., 2009.

On the slow time scale of glacial–interglacial cycles, influences coming from fast processes such as weather, volcanic eruptions, and decadal fluctuations of solar irradiation can be treated as stochastic forcing. Many of these, as the referee correctly noted, are in fact deterministically chaotic processes and need to be resolved as such in numerical weather prediction, for instance. However, on the large time scales of Pleistocene climate, fast processes can be treated as stochastic to the degree of detail describable by the available data. On the other hand, as we show in the manuscript, deterministic chaos can govern the dynamics on the large time scales relevant for Pleistocene climate; see Sections 3 and 4 of the paper. Both the fast forcing, modeled as stochastic, and the relevant chaotic dynamics can contribute to the continuous spectrum of the record.

The wording 'deterministic chaos à la Lorenz' could direct the readers' intuition to fast unresolved processes which in fact should be regarded as 'stochastic contribution à la Hasselmann'. To avoid this potential confusion and in order to make it clear that deterministic chaos can occur on any time scale, we would slightly rephrase the statement in line (194) as follows:

'*In returning to the "fundamental question #2" in Box 1, one must recall that — apart from deterministic chaos à la Lorenz (1963) on the intrinsic time scale of the system under study, as obtained by H. Le Treut and colleagues (Le Treut and Ghil, 1983; Le Treut et al., 1988) and shown here in Fig. 4(a) — stochastic contributions à la Hasselmann (1976) to the continuous part of the paleoclimatic spectrum - originating from the unresolved time scales - must also play an important role.*'

4. Point (ii) in the definition of a pullback attractor might be redundant. This might be the correct conclusion to be drawn from Fig. 11. The situation might be parallel with a time-dependent version of the system that is the normal form for a pitchfork bifurcation. I suppose that the PBA of that system — that goes back to negative infinity in time — is a pullback fixed point that is associated with the single stable solution and unstable solution before and after the bifurcation point, respectively. I suppose that after the bifurcation, we have PBAs that do not go back to negative infinity. This should have serious implications for climate projections.

The point (ii) – or Equation (15) – is in fact required in the definition of a pullback attractor. The first equation guarantees the invariance of the family A_t with respect to the dynamics of the system, while the second equation guarantees pullback attraction – note that X_0 in (ii) is not required to be an element of A_s, as it was the case in (i).

This definition might differ in terms of notation from other definitions; it is, however, the standard definition used by many textbooks, e.g., Caraballo and Han (2017).

In fact, in (ii) the $\rightarrow$ must be replaced by an equal sign. Thank you for inadvertently helping us detect this typo.

Minor comments

l.6 ‘*We introduce herein a unified framework for the understanding of the interplay between internal mechanisms and orbital forcing on time scales from thousands to millions of years.*’

The forcing is not influenced by the climate by definition. I thought interplay imples that causal influence goes both ways.

See our reply to Comment #1. We will change the sentence to:

‘*We introduce herein a unified framework for the understanding of the orbital forcing’s effects on the climate system’s internal variability on time scales from thousands to millions of years.*’

l.23 ‘*Specifically, Hays et al. (1976) were able to create a composite record — back to over 400 kyr b2k, i.e., over 400 000 yr before the year 2000 A.D. — from two relatively long marine-sediment records of the best quality available in the early 1970s.*’

I only found the boyband b2k on the web.

We were not aware of the boy band, however, b2k is the currently standard way to denote ages, especially in the ice core community. For example, see the papers by Sune Rasmussen or Anders Svensson from the University of Copenhagen.

l.23 We will replace 2000 A.D. by 2000 CE.

l.25 *‘The authors demonstrated therewith that precessional and obliquity peaks near 20 kyr and 40 kyr were present in this record’s spectral analysis; see Fig. 1.’*

Power spectra are quite notorious for their noisiness. Did Hays et al. or anyone later perform some stat test for these freq components?

Hays et al. (1976) did actually show that the spectral peaks corresponding to the orbital frequencies are significant. Such a significance test must be done avoiding the effect of orbital tuning. Huybers and Wunsch (*Nature*, 2005) as well as Huybers (*Nature*, 2011) did show evidence for orbital forcing in records that were not orbitally tuned; the latter papers were not based on power spectra but on the timing of terminations.

l.50 *‘On the other hand, it also became clear that a model whose only stable solutions were stationary, could not reproduce very well the wealth of variability that the proxy records were describing.’*

A stationary solution in the sense of a fixed point attractor means that no variability is present at all, let alone DO events.

We thank the referee for pointing out this inaccuracy. We will change the sentence accordingly:

‘On the other hand, it also became clear that a model whose only stable solutions were stationary could not reproduce very well the wealth of variability that the proxy records were describing, not even in the presence of stochastic forcing’

l.54 ‘*For instance, the models of Ghil and associates (Källén et al., 1979; Ghil and Le Treut, 1981) captured the phase differences between peak ice sheet extent and minimum temperatures suggested by Ruddiman and McIntyre (1981) in the North Atlantic, while the work of Saltzman and associates (e.g., Saltzman and Maasch, 1988) captured the asymmetry of the glaciation cycles with their more rapid “terminations” (Broecker and Van Donk, 1970).’*

Is it not uncommon to refer to the self in third person?

Since we are a team of four authors, neither ‘we’ nor ‘I’ would correctly refer to Michael Ghil exclusively, so we do not see any alternative to the current formulation.

l.60 ‘*Hence, they could not capture the wealth of spectral features, with their orbital and other peaks, of the paleorecords available by the 1980s.’*

What would be the difficulty with adding in these simple models the variability in solar forcing? If none, then would they still not capture some of the associated features of the paleo record?

Adding the orbital insolation forcing to simple climate models was the next evolutionary step in the history of paleoclimate modeling. To clarify further this point, we will slightly modify the sentence (line 64):

‘In this paper, we try to show a path toward resolving the four fundamental questions listed in the box below. In the next section, we summarize existing results on how the climate system’s intrinsic variability arises at Quaternary time scales and on how this variability interacts with the time-dependent orbital forcing.’

and instead write

‘*In this paper, we try to show a path toward resolving the four fundamental questions listed in the box below. In the next section, we summarize existing results on how the climate system’s intrinsic variability arises on Quaternary time scales and on how this variability is modified by the time-dependent orbital forcing, which was added to the previously autonomous climate models as the next step in paleoclimate modeling evolution.’*

Note that this modification also takes into account the referee’s comment on the term ‘interaction’.

Fig.3 ‘*Courtesy of N. Boers’*

He is an author of this paper. Or, has this same diagram appeared in a specific past publication? In that case i thought the precise reference should be given.

The phrase was added at an early stage in the development of the ms. and it will be deleted. Thank you for noticing.

Box1. *'How does the dominant peak of the observed variability near 100 kyr arise, given the rather diffuse orbital forcing at this periodicity?'*

In what sense is it dominant? Does this peak exceed a hypothetical background level more than the other peaks. At the higher freq peaks, this background is at a lower level. Exceedance is meant not in a log but lin scale.

In fact, the 100-kyr periodicity can be seen by the unaided eye to dominate the late Pleistocene's benthic d18O records (e.g., LR04). Also, it is widely agreed upon in the literature that the 100-kyr peak is the dominant one after the MPT, as already stated by Hayes et al. (Science, 1976).

We agree with the referee that this point is not sufficiently explained until the point where Box 1 is shown in the manuscript. We will add, therefore, to the sentence in line 30

'The work of Hays et al. (1976) and of the subsequent CLIMAP and SPECMAP projects resulted in a much more detailed spatio-temporal mapping of the Quaternary and extended the belief in the pacemaking role of orbital variations into the more remote past.'

the following statement:

'The spectral peaks near 20 and 40 kyr were interpreted as evidence for a linear response of the climate system to the orbital forcing. A third spectral peak at 100 kyr, though, was actually dominant and much more difficult to reconcile with the orbital theory of Quarternary glaciations. Since no sufficiently pronounced counterpart can be found in the spectra of the earth's orbital parameters, Hayes and coauthors hypothesized a nonlinear response of the climate system in order to explain this dominant periodicity of the late Pleistocene glacial–interglacial cycle.'

Box 1 *'What are the contributions of the orbital forcing and of the climate system's intrinsic variability to items (1)–(3) and how exactly do the two interact?'*

What two? Can we say that variance at different frequencies, or different Fourier modes, intercat? Isn't it variables that can interact?

The following reformulation should resolve the issues rightfully raised by the referee:

'What are the contributions of the orbital forcing and of the climate system's intrinsic variability to items (1)–(3) and how does the former one modify the latter?'

l.80 Eq. (1b)

Why would a positive temperature anomaly build up ice volume?

The positive influence of T on $\dot{V}$ is due to the precipitation-temperature feedback, which states that the higher the temperature, the more moisture can transported by the atmosphere and as a consequence, the higher will be the amount of precipitation over land masses leading to net growth of the ice sheets.

This is explained in detail in the following, so we would refrain from changing the manuscript with respect to this comment.

l. 84 ‘*The EBM represents the well-known ice-albedo feedback used by both Budyko (1969) and Sellers (1969), while the ISM relies on the precipitation-temperature feedback postulated by KCG and used also by Ghil and Le Treut (1981), who coined the term.*’

If we have only +ve feedback, the system would be unstable. In the EBM we indeed have -ve feedback too.

We are not sure, what exactly is meant by the referee, here. Note that the term EBM only refers to Equation (1a), while the term ISM refers to (1b). Together the two equations constitute an oscillatory climate model.

l.104 Why does this i not have a dot on top?

This is a pretty standard character for the imaginary unit; its LaTeX representation is $\imath$.

l.115 Perhaps you don't want to start a new para here because that would leave you with a one sentence para.

Yes, we agree and would remove the paragraph in a revised manuscript.

l.170 ‘*We start this section by describing some fairly simple ways in which the orbital forcing might have interacted with intrinsic climate variability, thus helping to solve the mismatch between Figs. 3(a) and 3(b) in Section 1.*’

interacted

In line with our answer to Comment #1, we will replace the above sentence by:

‘*We start this section by describing some fairly simple ways in which the orbital forcing might have modified intrinsic climate variability, thus helping to solve the mismatch between Figs. 3(a) and 3(b) in Section 1.*’

l.174 ‘*These authors found that, as expected for a nonlinear oscillator, its internal frequency f_0 interacts with the forcing ones, $\{f_1, \ldots, f_5\}$, to produce both nonlinear resonance and combination tones (Landau and Lifshitz, 1960).*’

See comment 1.

Here, we would use the term ‘affected by’ instead of ‘interact.’

‘*These authors found that, as expected for a nonlinear oscillator, its internal frequency $f\ 0$ is affected strongly by the forcing ones, $\{f\ 1, \ldots, f\ 5\}$, resulting in both nonlinear resonance and combination tones (Landau and Lifshitz, 1960).*’

l.195 ‘*In returning to the “fundamental question #2” in Box 1, one must recall that — apart from deterministic chaos à la Lorenz (1963), as obtained by H. Le Treut and colleagues (Le Treut and Ghil, 1983; Le Treut et al., 1988) and shown here in Fig. 4(a) — stochastic contributions à la Hasselmann (1976) to the continuous part of the paleoclimatic spectrum must also play an important role. In fact, the theory of random dynamical systems touched upon in the next subsection provides an excellent framework for a “grand unification” of these two complementary points of view (Ghil, 2014, 2019).*’

I thought Hasselmann uses the stochastic framework only to model time scale separation. Otherwise, what appears as noise is clearly deterministic irregularity due to chaos. Perhaps this distinction should be avoided, or, made precise if possible. The distinction should have to do something objective -- not just subjective, whether we bother to resolve time or not.

Please see our response to the Comments #2 and #3.

l.201 ‘*The highly preliminary results on interaction between external forcing and internal variability summarized in Sec. 3.1 encourage us to pursue in a more systematic way the interaction between orbital forcing and intrinsic climatic variability that may have contributed to generate the rich paleoclimate spectrum on Quaternary and longer time scales (e.g., Westerhold et al., 2020).*’

somewhat redundant language, beside the problem with the concept of interaction.

In a revised manuscript we would reformulate as follows in order to address both issues:

‘*The highly preliminary results summarized in Sec. 3.1 encourage us to pursue in a more systematic way the action of the orbital forcing on the intrinsic climatic variability that may have contributed to generate the rich paleoclimate spectrum on Quaternary and longer time scales (e.g., Westerhold et al., 2020).*’

l.209 ‘*On the road to including deterministically time-dependent, as well as random effects, one needs to realize first that the climate system — as well as any of its subsystems, and on any time scale — is not closed: it exchanges energy, mass and momentum with its surroundings, whether other subsystems or the interplanetary space and the solid earth.*’

Does Earth climate affect the sun? I wouldn't have thought. Surely, on certain time scales, we can treat some effects as external forcing with a very good approximation. Volcanic eruptions are surely external to the climate system.

We are not sure whether we understand this comment correctly. Of course, the Earth does exchange energy and momentum with the sun, however, the size of the sun makes the influence of the earth on the sun negligible, while the contrary is true for the moon.

However, the aim of the above sentence is to emphasize, that neither energy nor momentum in the climate system are preserved quantities. We believe that the original formulation is suited to convey this message and would stick to it in a revised version of the manuscript.

l. 215 ‘*Alternatively, the external forcing or the parameters were assumed to change either much more slowly than a model's internal variability, so that the changes could be assumed to be quasi-adiabatic, or much faster, so that they could be approximated by stochastic processes. Some of these issues are covered in much greater detail by Ghil and Lucarini (2020, Sec. III.G).*’

I think it's important to make a distinction between the very slow and fast processes other than their time scale. A slowly evolving ice sheet should be possible to model as an external forcing; but a fast atmosphere regarding the upper ocean "of interest" is considered part of the internal variability. In fact, I can't easily think of a fast process that is not considered as part of the internal variability but rather as an external forcing.

We have a relevant criticism of the OCCIPUT project in Sec. 4.4 of

https://link.springer.com/article/10.1007/s10955-019-02445-7

Thank you for reminding us to cite the Tél et al. (*JSP*, 2020) paper in the revision, which we definitely plan to do. In the broader perspective hinted at in this comment, it might be true that, in the case of paleoclimate, the fast processes which can be modeled as noise are mostly internal to the climate system itself – though for volcanic eruptions at least this is rather debatable. However, such an assertion is far from true in general and there are a number of several cases in which fast external forces act on internally slow systems. Thus, we will refrain from classifying fast processes that can be modeled as noise as being ‘internal’ processes opposed to forcings that are necessarily ‘external and slow,’ as proposed by the referee, to the extent that we understand this comment.

Also, the waxing and waning of ice sheets constitutes the dynamics of interest here, and thus it would not make much sense to consider global ice volume as a slow changing parameter. However, it might be, that we did not interpret this comment correctly.

l.217 ‘*The key concepts and tools of NDSs and RDSs go beyond such approaches that rely in an essential way on a scale separation between the characteristic times of the forcing and the internal variability of a given system; such a separation is rarely, if ever, actually present in the climate sciences.*’

I think the Hasselmann view hinges on this.

Yes, Hasselmann's approach does hinge on such a separation, but he is wrong, Nobel prize notwithstanding. In particular, as far as modeling the Quaternary's glacial–interglacial cycles, the time scales of climatic oscillations, of about 40–100 kyr, cannot be separated from the time scale of the external forcing, of about 20–40 kyr. We agree that the Hasselmann Brownian-motion model of climate that relies on such a time scale separation produces the widely observed continuous red-noise spectrum. This point, though, does not necessarily prove that there exists a clear time scale separation in the weather and climate phenomenology; see, for instance, Huybers & Curry (*Nature*, 2006) or Lovejoy & Schertzer (CUP, 2013).

l.224 *'Readers who are less interested in this mathematical framework — which allows a truly thorough understanding of the way that orbital forcing interacts with intrinsic climate variability on Quaternary time scales — may skip at a first reading the remainder of this section and continue with Section 4.'*

perhaps some editing issue here?

We must admit that we do not understand this comment. However, the term 'interacts with' will be replaced here too by 'acts on'.

l.257: We have realized that there is a typo in the 'family of snapshots' in the subscript $t \in R$.
The subscript R will be replaced by a $\mathbb{R}$.

l.320 *'The noise processes may include "weather" and volcanic eruptions when X(t) is "climate," thus generalizing the linear model of Hasselmann (1976), or cloud processes when we are dealing with the weather itself: one person's signal is another person's noise, as the saying goes.'*

The "weather is not independent of the climate" so there is no point in considering the pullback attractor for the SAME random noise realisation, as mentioned above.

We really do not know how to interpret this comment. It is not stated anywhere that weather was independent from climate, in contrast, by introducing $G(X)$ in Equation (22), we explicitly allow for multiplicative noise. Certainly, there are still more complex forms of noise, which are not covered by Equation (22), but we don't think that is the referee's point here?

The pullback attractor $A(\omega)$ in fact assumes a fixed noise realization. However, in this picture $A(\omega)$ itself is random and its distribution is determined by the distribution of the noise and the internal system dynamics.

l. 407 *'Figure 9 shows a trajectory of the FHN model for which the sinusoidal forcing used in Fig. 8 was replaced by a rescaled time series of atmospheric CO_2 concentrations retrieved from Antarctic ice cores (Bereiter et al., 2015):'*

Why that? There has been no physical meaning given to (24) yet so that we could guess why CO_2 is the driver.

The aim here is to provide an illustrative application of the NDS theory to a concrete paleoclimate modeling example, where we have prioritized concise math over physical consistency. However, we agree, that the introduction of the CO_2 forcing is a little bit ad hoc and therefore will add explanation. We will also make an effort here to better connect the section with Sections 2.1 and 2.2, by emphasizing that the external forcing truly acts as a bifurcation parameter switching on and of internal oscillations. Since this section might be subject to quite some changes, we refrain from presenting a possible additional or alternative formulation at this stage.

l.448 ‘*For this purpose, we apply the Daruka and Ditlevsen (2016) model of glacial-interglacial cycles with slight modifications.*’

We hope that this point has been sufficiently addressed in our answer above.

l.451 ‘*The model’s variables are a global temperature anomaly y that is proportional to minus the global ice volume and an effective climatic memory term x that represents the internal degrees of freedom.*’

This sounds a bit like eq. (1). However there the proportionality is between the tendency of temperature and minus the ice volume. Why?

The proportionality is not causality as in Eq. (1) but a simple (anti-)correlation such that the glacial has low temperature and large ice volume.

I don't really know what to think of by this. Some more details would be helpful.

As mentioned above and as was the case for the FHN model: We have set out to provide the reader with mathematically easy to understand non-autonomous dynamical systems with some relevance to paleoclimate modeling. In fact, we provide little physical interpretation. Nevertheless, we hope that the introductory text on the DD16 model (see beginning of this answer) we aim to incorporate in the revised manuscript provides a better guidance for the reader.

l.476 ‘*The PBA in this case is simply a moving fixed point, as plotted in Fig. 11(a), since the model dynamics is predominantly stable in the long time interval prior to the MPT that is situated around 1.2–0.8 Myr b2k.*’

This means that the PBA has no useful application here. Also, as we pointed it out in https://link.springer.com/article/10.1007/s10955-019-02445-7 a "pullback fixed point" is known in classical ODE theory as a "particular solution".

Please note, though, that in classical ODE theory, a "particular solution" does not have the stability properties that a "pullback fixed point" has, nor does it lag the forcing necessarily in the same way. We agree that the result up to this point is not very impressive; this being so, however, could not be known in advance. More importantly, our investigation shows that the situation changes across the MPT, a point that is central to our paper. We believe that the modified DD16 model’s change in PBA behavior across the MPT regime — given an external forcing whose characteristics do not change at or near the MPT —is a nice illustration of the point that the combination of internal dynamics and external forcing is crucial for the resulting variability.

fig. 11(a) Is this really the very same integration whose result we see in Fig. 10c? In there, after the MPT, the wiggles seem to indicate irregularity, i.e., instability of the trajectory, i.e., chaos.

That's true. The dynamics becomes unstable after the MPT. This instability, however, is not very strong. As a result, we can observe a fairly stable solution that is consistent with the proxy record.

l.480 '*However, when keeping the parameters α and β fixed at their post-MPT values α = 0.7 and β = 3.9 throughout the simulation interval and repeating the computation of the PBA, a more complex picture arises. In the latter case, Fig. 11(b) shows a bunching of trajectories into separate clusters, subject to the quasi-periodic orbital forcing of Fig. 10(a).*'

Are these really separate clusters? One question is if there is one attractor, or, there are coexisting attractors.

Thank you very much for this comment! Given this comment, we looked at the chaotic PBA closely and observed that there were no separate clusters, as in the work of Pierini & Ghil on a simple model of the wind-driven ocean circulation or in the coupled ocean–atmosphere VDDG model (Vannitsem et al., *Physica D*, 2015). In the revised manuscript, we will rephrase the term "separate clusters" as "separate fuzzy clusters"; see Ghil & Robertson (*PNAS*, 2002).

If the latter, only one of these can be accessed with time-dependent parameters given that the PBA is a moving fixed point. It is a kind of "intransitivity".

Prior to the MPT the PBA is a moving fixed point and yes, this means that also after the MPT the PBA as defined in equations (14) and (15) certainly remains a single moving fixed points, since the pre-MPT dynamics would cause trajectories starting from different initial conditions to converge if the the starting time is taken to minus infinity.

'in case of the latter' means that there are coexisting attractors – this contradicts 'given that the PBA is a moving fixed point'.

This raises the issue that, out of the two, the chaotic PBA originates not in the infinite past but at a particular point in time. That is, your point (ii) given by eq. (15) to define a PBA seems redundant.

The first statement is true, but the second isn't (see our reply to the comment #4).

The chaotic PBA bursts into existence from a single point in phase space, i suppose.

Regardless whether this is the case here or not, clearly, it is a possibility, and so point (ii) needs to be revised. One only needs to consider a system for which for some fixed values of a parameter there is only one attractor, and for other fixed values of the parameter there are more. Then the autonomous system can be defined in a way that the parameter is made time-dependent, taking values in the single attractor regime earlier and progressing in time to values where there are co-existing attractors of the autonomous system.

Perhaps the simplest example can be given by

x' = c + mu*x-delta*x^3, mu = atan(t)*2/pi

With fixed mu and c = 0, this is the normal form for a pitchfork bifurcation. With c>0, we have an imperfect pitchfork.

Alternatively, you have in (29) more like a perfect pitchfork scenario, c = 0, with a continuity of the nonchaotic and chaotic attractors across the regimes, the bifurcation point being the point of continuity. You can simulate this in Matlab by

```
[t,x] = ode45(@(t,x)(atan(t)*2/pi * x - x.^3), [-50 30], linspace(-2,0,11));
```

In Fig. 11a, you continue to have the 40 trajectories clumped "completely" together perhaps because the trajectory data is represented by the SAME number of finite machine precision, as a consequence of starting your simulation sufficiently far back in time. Perhaps, continuing the simulation longer, you would start to have chaotic trajectories and so a broadening of the ensemble. However, that might just be because of imprecise number representation on the computer, as i suppose your PBA that is initiated in the infinite past, is really just a moving fixed point. (That would be the unstable middle pin of the perfect pitchfork.)

The chaotic "part of the" (?) PBA does not go back to the infinite past.

We like to try to answer to this comment, with the proviso that we might not have understood it in all it’s details.

The PBA of the DD16 with the parameter shift is certainly just a single moving fixed point. As the referee correctly mentioned – other possible trajectories, that burst into existence across the MPT, cannot be accessed, because the pre MPT dynamics force trajectories that start from different initial conditions to converge arbitrarily strong.

The DD16 model with fixed post-MPT parameters has a more complex PBA comprised of fuzzy clusters. This is also what we explain in the manuscript and this does not conflict with our definition of the of the PBA which is correct as is.

In fact, the presence of the more complex yet in the deterministic setting inaccessible structure of potential trajectories after the MPT in the DD16 model with shifted parameters could be investigated in future work. The ‘inaccessible’ trajectories could become highly relevant, once noise is added to the system.

l.483 *‘First, post-MPT dynamics is much more irregular and unstable than the stable, quasi-periodic dynamics prior to the MPT.’*

You said earlier it was a moving fixed point; that's not the same as quasi-periodic.

We will remove “quasi-periodic" because, strictly speaking, the motion is not quasiperiodic, as you point out, i.e., it is not a sum of periodic motions with irrationally related frequencies. We will use instead “nearly, but not exactly, periodic.”

l.498 ‘*In Sec. 3, we presented first results on the interaction between the orbital insolation forcing of Sec. 1 and the intrinsic variability of Sec. 2, and proceeded to introduce the novel concepts and tools of the theory of nonautonomous and random dynamical systems (NDSs and RDSs) that can help to better model and understand this interaction.’*

In line with our response to comment 1 we replace the sentence by

‘*In Sec. 3, we presented first results on the action of the orbital insolation forcing of Sec. 1 on the intrinsic variability of Sec. 2 and proceeded to introduce the novel concepts and tools of the theory of nonautonomous and random dynamical systems (NDSs and RDSs) that can help to better model and understand this action.’*

l.499 novel – novel in what sense?

In the sense that this theory was only introduced into the climate sciences by Ghil et al. (*Physica D*, 2008) and by Bódai et al. (*NPG*, 2011), as stated in the first paragraph of Sec. 3.2 herein.