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 2

1. It is not clear to me what level of mathematical knowledge this paper is targeting. The paper begins assuming very little knowledge, by examining Hopf Bifurcations in equations (3--5) which are a topic covered in any dynamical systems course. Yet later in the paper readers are assumed to know what the `Hausdorff semi-distance' is. I think it would be better to assume less mathematical knowledge than more, perhaps the Hausdorff semi-distance could be replaced by a more informal comment about the system approaching $\mathcal{A}_t$. Another place the analysis could be streamlined without loss of understanding is by setting $\beta = \mu/2$ in equation (17) thereby reducing the number of parameters. The paragraph starting on line 304 provides conclusions without justification, which are only obvious to people familiar with dynamical systems. Perhaps a figure would help here?

We thank the referee for this constructive comment. Given that Referee #3 made a closely related comment, we would like to give a combined answer to both comments.

Referee #3

At lines 102 (section 1) the authors bring in the notion of a Hopf bifurcation with one type of simple system (eqn 5). Then in section 2.2 the description of the subcritical and supercritical Hopf bifurcations are described with another system (eqn 6). I would like to see more diagrams in Section 2.2 (some of us can visualise in our head what is happening when parameters are varied (e.g. through a Hopf bifurcation) but I think it is important to try to improve section 2.1 and 2.2 in a more unified way so at make these sections more accessible to a newer audience that is reading this type of material for the first time. I think a clear illustration with both language and an additional set of figures (possibly using the example systems from equation 5 or 6) would be helpful. For example, one might introduce the sections with language such as, “A Hopf bifurcation occurs when a periodic solution or limit cycle that surrounds an equilibrium point appears or disappears when a (control) parameter is varied. When the stable limit cycle surrounds an unstable equilibrium point, the bifurcation is supercritical. In the case that the limit cycle is unstable and surrounds a stable equilibrium point, the bifurcation is subcritical.” And then also illustrated these concepts later on with the simple systems used.

The comments of Referees #2 and #3 convinced us to give a typical illustration of a Hopf bifurcation in our manuscript to supplement the text. Referee #2 correctly noted that this is part of any dynamical systems course; we target, however, a readership where not everybody necessarily attended such a course. In a revised manuscript, we will elaborate a little bit more upon the examples of Eqs. (5) and (7) and, in particular, provide some visualization and emphasize the applicability to climate system examples even more.

Referee #2

Yet later in the paper readers are assumed to know what the `Hausdorff semi-distance' is. I think it would be better to assume less mathematical knowledge than more, perhaps the Hausdorff semi-distance could be replaced by a more informal comment about the system approaching $\mathcal{A}_t$.

Since we omit a specification of the metric already in the definition of the PBA in Eq. (15), it makes sense to do so in Eq. (20) as well. Therefore, we would replace

‘*lim d_h(…)* ‘

by

‘lim | … | ‘

in Eq. (20) knowing that this is not precise but gives the reader the right intuition.

Another place the analysis could be streamlined without loss of understanding is by setting $\beta = \mu/2$ in equation (17) thereby reducing the number of parameters.

With respect to Eq. (17), we do not think that replacing one parameter by a fraction of the other makes things significantly easier, and would keep the manuscript as it is at this point.

The paragraph starting on line 304 provides conclusions without justification, which are only obvious to people familiar with dynamical systems. Perhaps a figure would help here?

With respect to the paragraph starting at line 304, we agree with the referee that, without additional explanations, a reader who is not familiar with NDS theory might not understand the point. In a revised manuscript, we would therefore point out, on the one hand, that this paragraph contains additional information which is not essential to capture the main idea and, on the other, justify the given conclusions more fully.

We would replace

'Note that the structure of the system's trajectories depends on the ratio ω/ν and three different cases must be distinguished. If the radius is modulated with the same frequency as the oscillation itself, i.e. $\omega = \nu$, after one period the system practically repeats its orbit. More precisely, the radius of the oscillation does differ from one "roundtrip" to the next, but this difference (ρ) tends to zero as $\rho(t)$ asymptotically approaches the PBA A_t. If ω and ν are rationally related, $m\,\omega = n\,\nu$ with $n, m \in N$, then the same quasi-repetition of the orbit occurs after n periods of the radial modulation and m periods of the system's oscillation. Such a trajectory will appear as an n-fold quasi-closed loop. Finally, if $\omega/\nu \notin Z$, then the trajectory does not repeat itself but instead covers densely the annular disc $D = \{(\rho, \varphi)\colon \rho \in [\mu - \alpha\beta, \mu + \alpha\beta]$ and $\varphi \in [0, 2\pi)\}$. The trivial evolution of the phase (ρ) is depicted in panel (c), while the trajectories of $\rho(t)$ and their convergence to the PBA A_t are shown in panel (d).'

by

*'Note that the structure of the system's trajectories depends on the ratio ω/ν and three different cases must be distinguished. If the radius is modulated with the same frequency as the oscillation itself, i.e. $\omega = \nu$, the forcing and the system have a fixed phase relation. That is, for a given phase of the system, the radius of the system is always attracted by the same fixed point and hence after one period the system practically repeats its orbit. More precisely, the radius of the oscillation does differ from one "roundtrip" to the next, but this difference (ρ) tends to zero as $\rho(t)$ asymptotically approaches the PBA A_t. If ω and ν are rationally related, $m\,\omega = n\,\nu$ with $n, m \in N$, then for a given phase of the system, the forcing's phase will be the same after n periods of the radial modulation and m periods of the system's oscillation and hence we observe the same quasi-repetition of the orbit occurs after the time n*2pi / ν = m * 2\pi / ω. That is, such a trajectory will appear as an n-fold quasi-closed loop. Finally, if $\omega/\nu \notin Z$, then a selected phase of the system will never coincide with the same phase of the radius modulation more then once. Hence the trajectory does not repeat itself but instead covers densely the annular disc $D = \{(\rho, \varphi)\colon \rho \in [\mu - \alpha\beta, \mu + \alpha\beta]$ and $\varphi \in [0, 2\pi)\}$. The trivial evolution of the phase ρ is depicted in panel (c), while the trajectories of $\rho(t)$ and their convergence to the PBA A_t are shown in panel (d).'*

2. The point of this paper is to demonstrate the advantage of the NDS picture over the autonomous picture, yet much of the analysis could be done in the autonomous regime using standard assumptions about timescale separation. In the analysis of the FHM model, γ is taken to be a slow oscillation which allows for a discussion (lines approximately 385--400) that would be familiar to people who had only worked with autonomous systems. In the Daruka-Ditlevsen model, α and β are again slow parameters, so why shouldn't this be analysed with the classic tools of autonomous dynamical systems? I think it would be useful to emphasise what extra information the nonautonomous picture gives us, and what would go wrong analysing it using the tools from autonomous dynamical systems.

In the case of the DD16 model, α and β are not meant to represent the external forcing, but instead a slow change of the internal dynamics – discussing the reason for this change is not the aim of this manuscript. Instead, the external forcing is the insolation at 65°N at the summer solstice. While the autonomous system has a single stable fixed point for all $\alpha(t)$, \beta(t)$, the response of the system to forcing changes significantly over time. The time scales of the resulting dynamics are the same as the forcing time scales, so for this example we believe, there is no other way but treating this nonautonomous system with methods that are well adapted to such system systems.

Regarding the FHN model, we agree that this is a borderline case. The external forcing acts like a slow parameter and the time scales are pretty much separated, as denoted in Eq. (27). We will try to improve this example by reducing the time scale separation or by adding a second frequency to the forcing. This should cause a higher entanglement of the external forcing with the internal dynamics and would thus help to underpin the need for an NDS approach.

In the paleoclimate application of the FHN model, we have less freedom in our choice of time scales. However, it should be noted that, here too, the forcing changes on time scales that are relevant for the internal dynamics as well and it directly determines the length of stadials and interstadials, as well as the frequency of the oscillation. We will make an effort to explain these points better in a revised version of the manuscript.

3. I find the section on RDS a bit disconnected from the rest of the paper. There are no concrete paleoclimate applications given and the section introduces concepts such as the Random Attractor which is not defined even informally and are not used in the rest of the paper. I would recommend either cutting this section or adding in a simple paleoclimate example.

The paper aims to give the reader an overview over the mathematical concepts used for the study of non-autonomous dynamical system and thus we would prefer not to omit Random Attractors. However, the paper is already pretty long as is, and so we would prefer to not add yet another paleoclimate example. Since the original DD16 model was formulated, however, as a stochastic differential equation (SDE), we will explore the possibility of adding a paragraph in Section 4 to address the uses of Random Attractors for SDEs.

4. Figures 8 and 9 are seriously misprinted, e.g. Fig 8e has times labelled as -20000,-000,-0000,-000,0,000,0000.

We are sorry, this must have happened during the uploading process. In the version we have stored locally and which we uploaded as it is, the figures have the correct labeling. There are other details missing in both figures and we will exercise extra care in uploading the modified version, if encouraged to do so by the handling editor.

I realise this is a matter of personal preference, but might the PBA figures e.g. 5a look clearer if projected onto the x-y plane? I always find 3D figures confusing. Figure 5c doesn't seem useful, perhaps it could be replaced by the trajectories of the system with different ω/ν values.

Originally, we had a 2D projection of panel (a) in Figure 5, but the different trajectory overlapped so strongly that the plot was rather messy and not helpful. We hence prefer to keep the 3D version of Fig. 5a.

The referee is right that panel (c) is kind of trivial. For a revised version of the manuscript, we will, first of all, specify the values for μ and ν used for the computations. Second, we will replace panel (c) by a 2D projection of two trajectories with different μ/ν ratio.

Minor comments:

1. Lines 31--32 `an the' should be `and'.
Thank you, will be corrected.

2. Around lines 455, what do the parameters mean physically?
Please see our reply to the comment by referee #1 that starts with ‘I do sympathize with, …’. There, we present an introductory explanation on the DD16 model that will be added in a revised manuscript.

3. Lines 518--527, why not quote Emiliani and Geiss directly?

The analysis of Emiliani & Geiss in *Geologische Rundschau* (1959) was purely descriptive and is no longer relevant to the level of mathematical discourse in this paper. But the two paragraphs cited from Ghil & Childress (Springer, 1987, Sec. 12) still are very much so.