

Interactive comment on “Self-sustained Oscillations in the Atmosphere (0–110 km) at Long Periods” by Dirk Offermann et al.

Anonymous Referee #2

Received and published: 8 July 2020

General comments:

This manuscript deals with temporal oscillations in atmospheric temperature profiles obtained from simulations with three different global models (HAMMONIA, ECHAM6 and WACCM). The authors identify spectral signatures of “self-excited” oscillations in the model results and also a few other data sets. The paper is generally well written. I’m not really convinced all the findings and implications presented in this paper are robust or will turn out to be correct. For example, many of the spectral signatures listed in Table 2 are not significant, even at the 1-sigma level, if I interpret the table correctly. But several results are indeed interesting and somewhat stunning (e.g. the vertical structures in the correlations shown in Figure 3) and I think the paper should eventually be published to stimulate further research on this topic. In my opinion a

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major revision is required before the manuscript should be accepted for publication in ACP.

Apart from the specific concerns listed below, I have the following general comments:

- Parts of the manuscript are not well organized. The Hohenpeißenberg and GLOTI data are not really introduced, but appear suddenly in the middle of the paper. I suggest adding sections with brief descriptions of these data sets to section 2. Also, section 2.1 (Self-sustained oscillations and their vertical structures) appears misplaced in section 2 (Model data and their analysis).
- The spectral analysis by subsequent fitting of different components is not a good technique, because it generally misses components with small amplitudes. This is a known problem with such an approach and I ask the authors to consider alternative methods not affected by this problem. This is probably also the reason, why signatures cannot be identified at certain altitudes in Fig. 1.
- If the observed effects are associated with vertical displacements (e.g. a shrinking and expanding atmosphere), then the results should differ depending on whether pressure or altitude is used as a vertical coordinate when analyzing the data. I suggest plotting the results as a function of both pressure and altitude to check for any systematic differences.

Specific comments:

Line 51: “Self-generated (self-sustained) oscillations”

Is it obvious that “self-generated” and “self-sustained” can be used as synonyms?

Line 139: “They are not linked to the ocean.”

How can one know? If there is a reason, please state it.

Line 159: as mentioned above, section 2.1 does not really fit here.

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Line 182: "At some altitudes the periods could not be determined."

Why not?

Figure 1 (related to the previous point): How can an amplitude and phase be provided, if the period is unknown? I don't understand how this is possible. Is there something wrong?

Line 222: "Here, the experiment with monthly varying constant"

Unclear, what "monthly varying constant" means. Please rephrase.

Line 226: "Solar cycle variability, however, was not kept constant"

I'm don't really understand what this means. The variability was not kept constant? Or do you simply mean that solar activity was not kept constant, i.e. varied?

Lines 290 – 294: Are the dominant spectral signatures the same for both models?

Line 311: "The HAMMONIA data used for Fig.4 were annual data that have been smoothed by a four point running mean"

What is the effect of the 4-point smoothing on the correlations? Some of the pronounced signatures have periods < 4 years.

Line 337 – 340: HAMMONIA and ECHAM are based on the same underlying model, right? How can the possibility be excluded, that the similarity in results is related to that? This aspect is discussed below and I suggest adding a brief reference to the following discussions.

Figure 6: I suggest adding an x-axis with periods in yr.

Line 418: "The Lomb-Scargle spectra (in their original form) do not reveal the phases of the oscillations."

Well, a Lomb-Scargle analysis will/can certainly also provide information on the phase.

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Line 419 – 424: This doesn't seem like a good technique. You will potentially miss many features (see also general comment above).

Figure 7: I suggest adding a x-axis with periods and also using altitude rather than pressure as a vertical coordinate. If the main mechanism is related to vertical displacements (or shrinking & expansion of the atmosphere) it may make a difference, whether altitude or pressure is used as a vertical coordinate.

Line 446: "This procedure allows to obtain estimated amplitude and phase values for instance in the vicinity of the amplitude minima."

Well, one doesn't really know, how robust these estimates are, right? If I understand correctly, you assume a period, and then obtain an estimate of the amplitude and phase by fitting this period? Perhaps this can be mentioned more explicitly.

Line 458: "They suggest that the layer anti-correlation discussed above is at least in part due to the phase structure of the self-sustained oscillations in the atmosphere."

I don't think the logic behind this sentence is correct. The layer anti-correlation and the vertical phase structure are two manifestations of the same underlying phenomenon, right? I don't think it makes sense to state that one follows from the other.

Line 505: "Accumulated amplitudes have also been calculated for the ECHAM6 periods, and very similar results are obtained as for HAMMONIA."

I suggest also showing the results for ECHAM6.

Line 541/542: Here the Hohenpeißenberg and the GLOT1 data appear suddenly and I suggest introducing them earlier, preferably in section 2.

Line 546: "the zero level data"

Please explain, what this means.

Line 569: "to 20 yr" -> "to 120 yr" ?

C4

Line 608: "Two gradients are given for monthly mean temperature curves in addition:"

I don't quite understand this statement and why the two symbols appear at their specific altitudes. Please explain. Perhaps I'm missing a point here.

Legend Figure 13: perhaps the minus sign in " $-\Delta T$ " can be omitted? It always the problem with the meteorological and the physics definition of the temperature gradient.

Lines 643 – 646: I suggest also showing the identified periods of the variations in CH4 in Table 2.

Line 651: "This means that the displacement mechanism is the same for all oscillations."

I'm not sure this conclusion is justified. Why should the displacement mechanism be the same, if the vertical displacement is essentially independent of the period? I don't necessarily see a direct connection between the two aspects.

Same sentence/paragraph: Please show a vertical profile of D (with scatter from the individual oscillations).

Also, if the mechanism is related to vertical displacements (shrinking and expansion of the atmosphere) then the results should look very different if analyzed on an altitude grid rather than a pressure grid, right? I suggest plotting the results both as a function of pressure and altitude and check, how they differ.

Line 785: Most of the signatures are not significant at the 1-sigma level, if I interpret the Table correctly. This questions your arguments a bit.

Figure 18: This is just a general comment, but I'm not asking for additional analysis: Wavelet transform would also be a good technique to investigate both time and spectral information. It should provide more robust results compared to the windowed FT performed here.

Line 815: "by means of harmonic analyses"

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Please mention explicitly, what this means. FFT, Lomb-Scargle, wavelet transform etc. are all harmonic analysis methods in a general sense.

Line 850: "It needs to be emphasized that the oscillations discussed in the present paper are not influenced by the ocean as they occur even if the ocean boundaries are kept constant."

Well, they could still be influenced by the oceans to a certain extent, right? I would replace "influenced" by "caused".

Line 1287: "or are confirmed in the literature"

Please indicate, which ones are significant in your analysis and which ones appear in the literature (are they significant there?)

Table 2: If I interpret it correctly, then most spectral components are not significant at the 1 sigma level, right? This should be mentioned in the main text.

Also, please indicate – perhaps using color – which components are significant at the 1-sigma level, and which are significant at the 2-sigma level.

Typos etc.:

Line 16: Please add a space in "Atmosphären-und"

Line 73: "They are also seen in computer models (GCM) of the atmospheric."

Sentence is incomplete

Line 168: space missing in "et al.,2003)."

Line 264: "Figure1" -> "Figure 1"

Line 268/269: Please delete space before comma.

Line 283: "Fig.3" -> "Fig. 3" (this occurs many times throughout the manuscript, please check the entire manuscript)

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Line 290: “2.)” -> “2)”

Line 355: “picrure” -> “picture”

Same line: “structur” -> “structure”

Line 489: “intermittend” -> “intermittent”

Line 540: “rows” -> “columns”

Line 705: “self- excitation“ -> "self-excitation“

Line 821: “Table2” -> “Table 2”

Line 848: add space in “et al.(2010) with”

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-89>, 2020.