Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-89-AC2, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Chemistry and Physics Discussions

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

Dirk Offermann et al.

offerm@uni-wuppertal.de

Received and published: 27 July 2020

DO 8.7.20

Response to Anonymous Referee #2

We thank the Referee for his careful and detailed review. He states that the paper "...should eventually be published to stimulate further research...". This was exactly our intention, disregarding that the analysis was in part somewhat rough.

General Comments

1) "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

Printer-friendly version

Discussion paper

adding sections with brief descriptions of these data sets to Section 2."

The Hohenpeißenberg and GLOTI data are now introduced early in the paper (Section 1, text in red).

" Also, Section 2.1 (Self-sustained oscillations and their vertical structures) appears misplaced in Section 2 (Model data and their analysis)."

This Section first sketches the results of earlier model analyses for shorter periods, in order to guide the reader to the present, longer period results. Secondly, it gives a basic picture of the self-excited oscillations (Fig.1) to allow the reader envisage the oscillations mentioned in the following text.

2) "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.".

This problem was aware to us, as mentioned in Lines 449pp and 823. In Lines 423pp we stated that we show first approximations, only. To emphasize the point, we have now complemented the sentence in Line 424 as follows: "Furthermore, the 10% grid may be sometimes too coarse, and also small amplitude oscillations may be overlooked." In the present paper we want to give an overall picture of this type of oscillations, only, and therefore restrict ourselves to the larger and hence more important ones in Tab.2a.

3) "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."

The discussion of possible displacements has been much longer in the original version

CC BY

of the manuscript, including several pictures. As this appeared to unduely increase the volume of the paper, we have limited this section to a short summay in Lines 642 – 656. We hope to give a more comprehensive presentation in a future paper.

Specific comments:

1) Line 51: “Is it obvious that “self-generated” and “self-sustained” can be used as synonyms?”

No!, you are right!, the two expressions are not synonymous. As our Section 3.6 (“Oscillation persistence”) shows, the persistence of the oscillations may be limited. My dictionary tells me, however, that an “sustained oscillation” is one without damping. Thank you! We have now replaced “self-sustained” by “self-excited” in the text throughout.

2) Line 139 (Introduction): “They are not linked to the ocean.” “How can you know? If there is a reason, please state it.”

The self-excited oscillations are seen in the model results, even if the model boundary values concerning the ocean are kept constant. This is discussed in detail later in the paper. We have included a corresponding reference here: “They are not linked to the ocean (see below).”

3) Line 159: “as mentioned above, Section 2.1 does not really fit here”

See above, General comment #1.

4) Line 182: “At some altitudes the periods could not be determined. Why not?”

One reason may be too small an amplitude (see General comment 2 above). Other reasons are listed in Lines 548pp (for instance insufficient spectral resolution). We have added now a corresponding reference: (“. . . periods could not be termined (see Section 3.3).)

5) 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 some-

CC BY

thing wrong?"

The procedure is described in Section 2.1, Line 183pp: "In these cases the periods were prescribed by the mean. . .". The Levenberg-Marquardt algorithm allows this , as is described in Line 447pp. We have added a more detailed reference in Line 185: "Details are given in Section 3.2, Lines 447pp." In Line 449 we have replaced the "harmonic analysis algorithm" by "Levenberg-Marquardt algorithm".

6) Line 222: "Here, the experiment with monthly varying constant", "Unclear, what monthly varying constant means. Please rephrase."

It is meant that there is a seasonal variation, but it is the same in all years. A corresponding sentence has been added now.

7) Line 226: "Solar cycle variability, however was not kept constant". I don't really understand what this means. The variability was not kept constant? Or do you simply mean that the solar activity was not kept constant, i.e. varied?"

Yes, solar activity was varied, but in a special way: In the time interval 1955-2004 the measured data of Lean et al. were used. Thereafter the measured data from 1962-2004 were used as a proxy data block of 42 years length, to start at year 2005. At the end of this the block was repeated again and again until the total length of the time interval of 150 years had been reached. We have rephrased the paragraph accordingly.

8) Lines 290-294: "Are the dominant spectral signatures the same for both models?"

Yes! Please see Tab.2a!

9) 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 smaller than 4 years."

The effect is unimportant. The altitude levels of maxima, minima, and zero crossings

CC BY

are about unchanged. This is shown in Picture 1 attached to this “Response” for the HAMMONIA data. A corresponding sentence has been added to the text.

10) 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.”

a) Our analysis shows that the vertical correlation structure is due to the vertical phase structure of the self-excited oscillations (Section 3.2, Lines 458-459). These phase structures are similar for all oscillations. The periods are very similar for all models and even measured data (Tab.2a). It is therefore unlikely that the similarity of the HAMMONIA and ECHAM results is an artifact. b) Oscillation #10 in Tab.2a is the same in the HAMMONIA, WACCM, and ECHAM models. The WACCM model is quite different from the other two. c) Basically, we cannot answer the question of the referee from the data alone. We would have to go into the structure of the models, themselves, which is not possible to us. We, therefore, suggest to take the ECHAM data as an extension of HAMMONIA. We have added a corresponding remark in Section 2.4 (Lines 253pp).

11) Figure 6: “I suggest adding an x-axis with periods in years”.

Was done as suggested.

12) 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 informationon the phases”,

The Lomb-Scargle algorithm we used (“original form”) did not give phases.

13) Line 419-424: “This doesn’t seem like a good technique. You will potentially miss many features (see also general comment above).”

See above reply to the General Comment #2.

CC BY

14) 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 diffenence, whether altitude or pressure is used as a vertical coordinate.”

X-axis was complemented as suggested. Y-axis was left as it was given to us by the model provider because the vertical displacement was only sketched here (see above reply to General Comment #3).

15) 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.”

The Levenberg-Marquard algorithm works as follows: An initial period is specified. The algorithm searches in the vicinity of it for a major period. It determines this period , its amplitude, and phase, including error bars. We have now added two corresponding sentences in Line 423pp: “The algorithm starts from a given initial period and looks for a major oscillation in its vicinity. For this it determines period, amplitude, and phase, including error bars. If in this paper the term “harmonic analysis “ is used, this algorithm is always meant.

16) 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.”

This is a misunderstanding. I completely agree with you, and have therefore rephrased the sentence: “. . .the layer anti-correlation. . .corresponds . . .to the phase structure. . .”.

CC BY

17) 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.”

Results for ECHAM6 are now shown in Fig.11b. The text has been modified accordingly.

18) Line 541/542: “Here the Hohenpeißenberg and the GLOTI data appear suddenly and I suggest introducing them earlier , preferably in Section 2.”

See above reply to General Comment #1.

19) Line 546: “. . .the zero level data. . .” “please explain what this means.”

We have now omitted these words as they are unnecessary.

20) Line 569: “to 20 yr. . .to 120 yr”

Has been corrected, thank you!

21) 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.”

The explanation was given in Lines 696pp (last two sentences of Section 3.5 (instead of the Figure legend of Fig.13).

22) Legend Figure 13: “Perhaps the minus sign in “-delta T” can be omitted? It is always the problem with the meteorological and physics definition of the temperature gradient.”

The insert in Fig.13 has now been omitted as it contains redundant information, only.

23) Lines 643-646: “I suggest also showing the identified periods of the variations in CH4 in Table 2.”

CH4 periods have now been added to Table 3 as suggested. The text has been com-

CC BY

plemented accordingly in Section 3.4.

24) Line 651: “This means that the displacement mechanism is the same for all oscillations.” a) “I am 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.”

This was a presumption, only. As mentioned, a detailed discussion is beyond the scope of this paper. We have modified the sentence accordingly: “This makes us presume that the displacement mechanism may be the same for all oscillations.”

b) “Same sentence/paragraph: Please show the vertical profile of D (with scatter from the individual oscillations). Also, if the mechanism is related to vertical displacements (shrinking or 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.”

Again, these detailed analyses are planned for a future paper. They would unduely increase the volume of the present paper.

25) 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.”

The discussion in Section 4.3a (Tab.2a) uses the periods obtained from the Levenberg-Marquardt algorithm with the corresponding error bars. These are 1-sigma errors.

26) Figure 18: Thank you for the comment.

27) Line 815: “by means of harmonic analyses” “Please mention explicitly, what this means. FFT, Lomb-Scargle, wavelet transform etc. are all harmonic analysis methods in a general sense.”

We have complemented Line 815 accordingly: “by means of harmonic analyses (Levenberg-Marquardt algorithm)”. We have also complemented the addendum to Line

CC BY

446 (Comment #15) accordingly.

28) 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”.

Done as suggested!

29) 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?)”

A list of periods and their accuracies/significances has now been added as Table 2b. The text has been complemented accordingly. Some of the analysis methods and their accuracies are too complicated to discuss in this paper. The reader is referred to the original publication in these cases.

30) 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 significacant at the 1 – sigma level, and which are significant at the 2-sigma level.”

All periods derived from the Levenberg-Marquardt algorithm are significant at the 1-sigma level. This is now mentioned in the text (Section 3.3, 3rd paragraph). Significance levels are now explicitely shown in Tab. 2b.

Typos etc.:

Line 16: Please add a space in “Atmospären-und”

was added

Line 73: corrected to “They are also seen in computer models (GC) of the atmospheric.” “Sentence is incomplete.”

CC BY

was corrected

Line 168: corrected

Line 264: corrected

Line 268/269: corrected

Line 283: “Fig. 3” “This occurs many times throughout the manuscript, please check the entire manuscript.”

Manuscript was check as required.

Lines 290, 355, 489, 540, 705, 821, 848: corrected

Thank you for your efforts!

Picture 1 for #9

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

Printer-friendly version

Discussion paper

CC BY

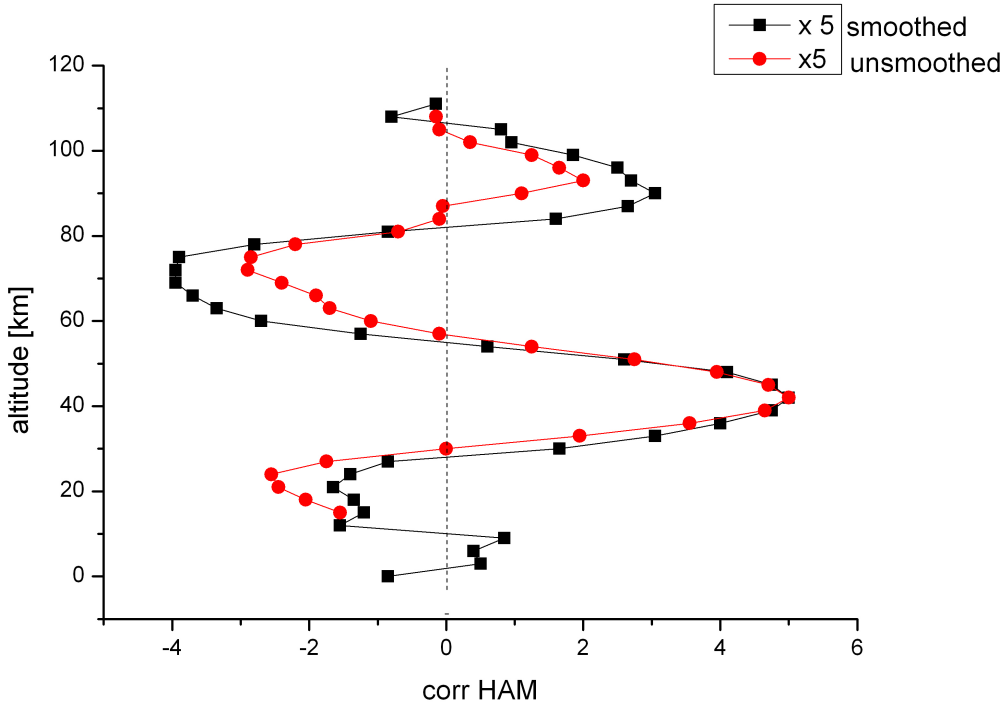

Fig. 1. picture 1for #9

Printer-friendly version

Discussion paper

CC BY