Response to Referee #3 DO 2.10.20

We thank again the Referee for his detailed comments.
Again, changes in our manuscript are marked in red, and replies to the Referee's comments are given below *in italics*.

General Comments:

1.

"*The land component of the models has been kept constant, too. A corresponding remark was added in Section 2.2.*"

I am sorry, but I could not find any sufficient explanation on the land component in section 2.2. Your remark that land parameters are fixed. But Schmidt et al. (2010), who describe the Hhi-max simulation, do not mention that the land component has been kept constant in this experiment. As they do not give any specific details on the land model configuration, it must be assumed that the land model is normally included and thus interacts with the atmosphere as in the underlying base model ECHAM5, for which Roeckner et al. (2003, 2006) are cited.
I think this is an important detail for the interpretation of the results and the conclusions which can be drawn. As long as this is not sorted out, you cannot claim that the oscillations observed in the atmosphere are self-sustained by the atmosphere basic dynamics. Still it is interesting to find such oscillation in simulations where no interactive ocean model is included.

a) There is still a misunderstanding: I do not believe that the oscillations are caused by the atmospheric dynamics . We do not yet know the origin! The misunderstanding obviously stems from the word "link" which in my mind means "connection, relation". I learn, however, that you understand it as "origin, cause". I asked the colleagues around me and found that the understanding is divided. To avoid confusion, I now replaced "linked" by "related" throughout the paper.

b) As concerns the land component, my model informant tells me: vegetation parameters (as leaf area, wood coverage) and ground albedo are kept constant. Other parameters are not (e.g.snow or ice on lakes). It is believed that their influence on the oscillations is small, but it cannot be excluded. We have therefore included a corresponding paragraph to Section2.2:

> *As concerns the land parameters part of them were also kept constant (vegetation parameters as leaf area, wood coverage) and ground albedo. Others were not (e.g.snow and ice on lakes). Hence, some corresponding small (?) influence on our oscillations cannot be excluded.*

The text has been searched throughout to improve corresponding formulations.

2.

"*Agreed! The longest periods in Tab.2a are shown just for completeness.They are not really used in the paper. The corresponding error bars in Tab.2a are large and thus are a warning. Nevertheless it is interesting to see that the longest periods of HAMMONIA and of WACCM find approximate counterparts within combined errors in ECHAM6."*

The explicit mentioning of the 341 year period in the key-points and abstract gives the message that you consider them as important enough to be highlighted. If you avoid this, the reader would not become disappointed when understanding later that the error bars are so large. It is certainly interesting enough to point out the multi-decadal time scales, which you diagnose in a system without an ocean component.

The period of 341 yr has been omitted now from the key words, abstract, Tab.2a and the text throughout.

Specific Comments

1.

"*This is a misunderstanding: We did not claim an atmospheric origin of the oscillations, but we said that the oscillations are atmospheric properties. We do not know yet the origin of the oscillations, as was stated several times in the paper. We certainly agree that clarification will presumably need a number of steps."*

If no claim is intended in an atmospheric origin of the oscillations, the wording needs to be adjusted in several places across the whole manuscript. I find it very irritating to read for instance in the Key Points: "self-sustained oscillations linked to the atmosphere basic dynamics" although you respond that you do not claim an atmospheric origin of the oscillations. "linked to atmosphere basic dynamics" in my understanding implies that the atmosphere is the cause.

Please see above General Comment #1a.

Response to Referee #2, (von Savigny) DO 28.9.20

We thank again the Referee for his detailed and careful comments.
We follow the line numbering used by the Referee. Again, changes in our manuscript are marked in red, and replies to the Referee's comments are given below *in italics*.

General Comments

1 There is, however, one major point that questions part of the results in my opinion. The periods listed in Table 2a are based on the harmonic fit approach as described in the text. The authors write in line 477: „The clusters are separated by major gaps, as is indicated by vertical dashed lines (black)."
Looking at the right parts of Fig. 9 and also Fig. 10 it's not really obvious what qualifies as a gap and what doesn't. Based on Figs. 9 and 10, the choice of gaps appears quite arbitrary. In turn, the derived „mean periods" within these clusters are also arbitrary. I argue that some of the periods determined are not really robust or may not be robust, because they depend on the choice of cluster bounds, which was done by you and not based on an objective approach. This does not appear to affect all identified periods, but probably a substantial part of them.

We have modified the wording of Paragraph Lines 474 pp and added the subsequent Paragraph:

In determining the mean oscillation periods we have avoided subjective influences as follows: Periods obtained at various altitudes were plotted versus altitude as shown in Fig. 1 (middle column, red).When covering the period range 5 to 30 years nine vertical columns appeared. The definition criterion of the columns was that there should not be any overlap between adjacent columns. It turned out that such an attribution was possible. To make this visible we have plotted the histograms in Fig. 9 and 10. The pictures show that the column values form the clusters mentioned which are separated by gaps. The gaps that are the largest ones in the neighbourhood of a peak are used as boundaries (except at 7.15 yr). It turns out that if an oscillation value near to a boundary is tentatively shifted from one cluster to the neighbouring one the mean cluster values experiences only minor changes. Figure 10 shows that our procedure comes to its limits, however, for periods longer than 20 years (for HAMMONIA). This is seen in Tab.2a from the large error bars. We still include these values for illustration and completeness.

It is important to note that all HAMMONIA values in Tab.2a (except 28.5 yr) agree with the Hohenpeißenberg values within the combined error bars. The Hohenpeißenberg data are ground values and hence not subject to our clustering procedure. Furthermore also all other model periods in Tab.2a have been derived by the same cluster procedure. The close agreement discussed in the text suggests that this technique is reliable.

Specific Comments

1 Lines 278 and 282: the standard deviations are given with +-. Standard deviations cannot, however, be negative by definition.

Agreed! Text was corrected.

2 Line 305: ´the significance is much better for ECHAM6).´

Please show the 95% lines for ECHAM as well.

Done as required. Text was modified accordingly.

3 Line 351: ´multiplied by 2)´

I suggest mentioning briefly, why the values were multiplied by 2 (to improve clarity)

Multiplication by 2 was done for easier comparison to other curves. Text was complemented accordingly.

4 Line 390 – 394: Did you scale the Gaussian noise in any way, e.g. to match the standard deviation of the temperature data?

As each Lomb-Scargle Periodogram is normalized with the variance of the noise in the same way as for the data (see Lines 385-387), the noise needs not to be scaled before. A scaling of the noise to match the standard deviation or variance will not change the normalized power.

5 Fig. 6: Period labels at top ´34 YR´ and ´4 YR´ not well placed.

Period scales have been improved in Fig. 6 and 7.

6 Line 425: ´This was done by stepping through the period domain in steps 10% apart.´

I wonder, how this assumptions affects the identified periods. How would the periods look like, if you had used 15% or 20% steps. Would you identify different periods?

We would not identify different periods, if the steps were chosen larger. However, in such a case certain period might be overlooked. This is the reason why we chose steps this narrow.

7 Fig. 7: Period labels at top ´400 YR´ and ´10 YR´ not well placed.

See above #5.

8 Fig. 8: Please mention briefly in Fig. caption, what the red line shows.

The straight red line is too complicated to explain in the caption. Reference to the text is made, instead.

9 Line 477: ´The clusters are separated by major gaps, as is indicated by vertical dashed lines (black).´

As mentioned above, this seems to be a very arbitrary approach and the resulting main or mean periods will directly depend on you subjective choice of gaps.

See reply to General Comment #1 above.

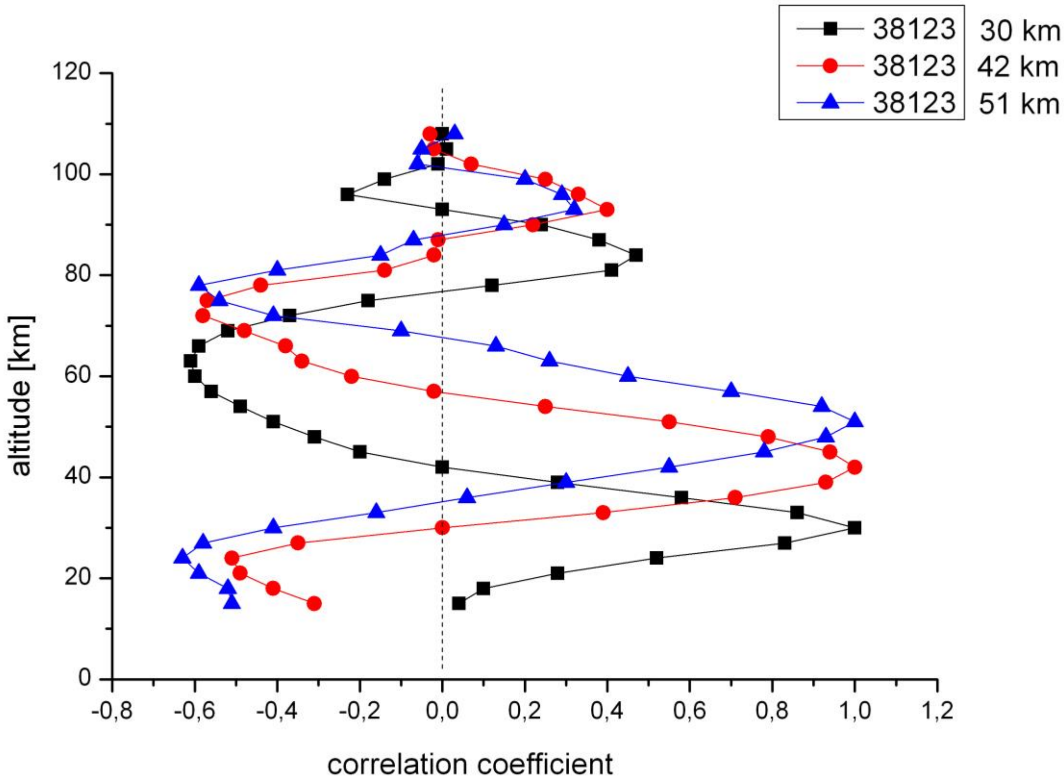
10 Fig. 12: Please explain, what ´W´ means.

Figure 12 was taken from the textbook of Schönwiese (1992, their Fig.57). The meaning of "w" is not explicitely explained in that text. However, from the context I conclude that "white noise" is meant.

11 Fig. 13: The occurrence of the lower maxima (near 40 km) in the correlations is certainly not surprising, because at 42 km the correlation is an autocorrelation and the coefficient is 1. This means that you can directly choose the altitude, where a value of 1 occurs, but adjusting the reference altitude. It would be interesting to see how the Fig. changes if a different height is used as reference. Perhaps the 2 curves agree much better, if z = 35 km is used as a reference? I suggest showing 2 or 3 curves with different reference altitudes.

A similar question has been raised by Referee #3 during the previous round. The corresponding answer should apply here , as well (o.k.?):

" "a) reference height: No quantitative conclusions are drawn from the correlation profile. Text has been rephrased (Line 317).
The layered structures in question are also seen if a different reference altitude is chosen. This is shown for the altitudes suggested (30 km, 51 km) in Picture A below.""



Picture A Vertical correlation with reference altitudes 30 km, 42 km, 51 km. HAMMONIA 38123,annual data (unsmoothed).

12 Line 667: ´ This is a relative change, only.´

I don't really understand this statement. This is an absolute change in mixing ratio, not a relative change (in %), right?

This statement is a reply to a question of Referee #3 in the last round. He had wondered whether an absolute photochemical change was meant, which is not the case.To hopefully clarify the point we have rephrased pp 667as follows:

"*If an air column is displaced vertically by some distance D* ("displacement height") a *seeming change in mixing ratio is observed at a given altitude. This is a "relative" change, only, not a photochemical one. It can be estimated by the product {D times mixing ratio gradient}."*

13 ´ Line 788: ´The periods are robust, i.e. they are found with similar values in different models.´

As pointed out above, I don't think all the periods are robust.

Text has been modified as follows: Many of the periods appear to be robust, i.e. they are found with similar values….

14 Line 798: ´Maxima of oscillation amplitudes appear to be associated with westerly (eastward) winds together with large temperature gradients (positive or negative). Amplitude minima are associated with either easterly (westward) winds or with near zero temperature gradients.´

This could be directly related to the propagation of planetary waves in a westerly wind regime (Charney-Drazin-criterion). I suggest discussing this briefly. PW are also associated with vertical displacments, which could explain some of the observed effects.

We hesitate to believe that our oscillations are some kind of waves . This is because waves propagating in the atmosphere should show an exponential amplitude increase in the vertical direction. This is not the case here, as is shown in Fig.1, and is similar for all other oscillations. We therefore avoided the word "wave" in this paper.

15 Line 849: ´ (b) The periods given in Table 2a were all calculated by means of harmonic analyses´

There's a logical error here. The use of the LM-algorithm and the potential occurrence of a common-mode failure cannot be used as an argument for non-spurious results, right!

There appears to be a misunderstanding here. The Referee appears to read this text in connection to the previous paragraph (Lines 844-847). This was not our intention, but Lines 849 pp are a new paragraph "b" that does not discuss non-spurious results but leads to supplementary analyses.

16 Typos etc.:

Have been corrected. Thank you for the list!