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

Anonymous Referee #3

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General comments

The authors present a statistical analysis of time series of annual temperature at a single place named "Central Europe" in three simulations. They present a number of periods, up to essentially the length of the data series, and claim that these are self-sustained oscillations of the atmosphere.

There are many points to be criticized in this study. First of all the claim of self-sustained atmospheric oscillations cannot be made, as the investigated simulations include also a land component that is coupled to the atmosphere and can provide memory for long time scales (cf. Hasselmann's Stochastic climate models, 1976). In many places in the text it would be best to remove the term self-sustained.

Further, oscillations with periods essentially the same as the length of the time series can certainly be identified in Fourier transforms, but the uncertainty in the spectral estimate for such periods is so large, that no claim on the existence of this period can be made.

A further weakness is the lack of discussion of the presented oscillations with respect to atmospheric dynamics. The vertical coupling of the stratosphere and troposphere or stratosphere and mesosphere in high and middle latitudes, in the winter/spring period, is an active field of research, but non of this is mentioned. Using this knowledge the study could have been focused on seasons from the beginning.

In the end this study is mainly a statistical time series analysis that identifies some periods in simulations made for "fixed climate conditions", including the atmosphere and land components. Some of the shorter periods have been reported by others for observational data.

Overall a major revision would be necessary to bring this manuscript in a publishable form.

Specific comments

L136: ". . . It is emphasized that on the contrary the self-excited multi-annual oscillations described by Offermann et al. (2015) and those discussed in the present paper are properties of the atmosphere, and exist in a large altitude regime between the ground and 110 km altitude. They are not linked to the ocean. . . ."

It is a logical error to conclude from the occurrence of harmonic signals in atmospheric fields that these oscillation are of atmospheric origin. The reason is that models like HAMMONIA or WACCM couple the atmosphere to the land, through the hydrological cycle as well as the heat exchange. Thus more steps are needed to attribute the oscillation to the atmosphere alone. In the case of the QBO this has been clarified by the theory and a range of models from minimal 1-dimensional models to Earth system models.

L142: “. . . (Central Europe) . . . “ Please specify the place. Is it (45°N–55°N; 4°W–16°E) as in Offermann et al. (2015)? Please also explain why this place is chosen, and why only a single place is used. The model data are not limited to the single place. Would you expect the same results for a polar or equatorial place?

L143: “. . . The model boundary conditions (sun, ocean, trace gases) are kept constant. . . . “ Such model require more external data than sun (= spectral solar irradiance), ocean (surface temperature and sea ice concentration, at least), trace gases (CO_2, CH_4, N_2O, CFCs): - Earth orbit parameters - natural and anthropogenic aerosol distributions in troposphere and stratosphere - land cover description and land use changes All of these contribute to low frequency variability. What was done with these sources of variability?

L162-165: “. . . These were found in temperature data of HAMMONIA model runs (see below). They were present in the model even if the model boundary conditions (solar irradiance, sea-surface temperatures and sea ice, boundary values of green-house gases) were kept constant. Therefore they were interpreted as self-sustained (self-excited) oscillations. . . . “

It needs to be clarified if all external data were made constant-in-time. Further the text suggests that the presented oscillations are self-sustained (self-excited) oscillations of the atmosphere. But also this cannot be claimed as commented above. The text needs to be changed.

L167: “. . . Robust periods are typical of self-excited oscillations (Pikovsky et al.,2003) . . . “ But also for externally forced oscillations may be robust, e.g. diurnal and annual periods as a result of Earth rotation and orbit modulating the locally incoming SW flux at the top of the atmosphere. “Robustness” is also a relative term. The variance of ENSO indices has a spectrum of a certain width, such that no single robust period exists, but instead a the robustness of the spectrum could be discussed. Thus this

statement and citation, is problematic, as it is unclear whether this statement is correct for the types of oscillations described in this paper.

L175: “. . . whether such longer periods could also be self-excited in the models. . . . “ This should be broken up in two questions: (1) whether long periods can be identified in the simulations used here, and (2) which is the origin of any such oscillation.

L302-303: “Fig.2 HAMMONIA temperature residues at 0 km and 3 km altitude with fixed boundary conditions (see text).” Needs clarification. Is this at the “Central Europe” position?

L183-15: “. . . In these cases the periods were prescribed by the mean of the derived periods (dash-dotted red vertical line, 17.3 yr) to obtain approximate amplitudes and phases at these altitudes (see Offermann et al., 2015). . . . “ If no period can be determined, there is no value in determining phase and amplitude for a prescribed period. It is strongly recommended to remove these suggestive phase and amplitude data from this figure. The fact that this practice was used in the Offermann et al. (2015) article, does not justify to repeat this practice here.

L221-222: “. . . This 150 year run was analyzed from the ground up to 108 km. The model experiments are described in Hansen et al. (2014) . . . “ It is not clear which experiment is meant. The experiments listed in Hansen et al. (2014) are shorter than 150 years. Do you mean the experiment named “Fixed SSTs” listed there with a length of 56 years?

L254: “. . . A summary of the model properties is given in Table 1. . . . “ For boundary conditions, please add in Table 1 information for the other boundary conditions: orbit parameters, aerosol distributions, where prescribed: ozone, and land properties. For “ocean” boundary conditions, three different ways are used to express that ocean surface conditions are “climatological”: - HAMMONIA: “SST fixed”, - WACCM4: “climatological SST and sea ice”, - ECHAM6: “fixed” Does HAMMONIA fix only SST, but not sea ice? Why is the ocean boundary condition for WACCM4 named “climatological”, while those for HAMMONIA and ECHAM6 are simply “fixed”? Is there a difference, or just different wording for the same?

L264-265: “. . . Figure1 indicates that there are some vertical correlation structures in the atmospheric temperatures. . . . “ This impression is evoked mainly by the phase profile, which includes many filled-in points at levels where not oscillation period could be determined, as shown by the gaps in the profile for the period.

L266: “Ground temperature” Does this mean surface temperature? Figure 2 shows in the legend for the black and red lines: 0 km, and 3 km, respectively. Are these heights above surface? (A land point would normally be higher than 0 km above sea level). Or are these heights above sea level? If so, how was the surface temperature extrapolated to sea level?

L269-270: “. . . The temperature fluctuations thus show the internal atmospheric variability . . . “ The soil model has its own prognostic temperature variable in a number of layers, so that the surface temperature in the end is determined not only by the atmospheric temperature, but also by the soil temperature. Therefore the variability in the surface temperature is not internal atmospheric variability, but rather internal variability of the coupled atmosphere – land system.

L286-27: “. . . with two maxima in the upper stratosphere . . . “ The maximum at 42 km height, where r=1, occurs by construction.

L311-313: “. . . The HAMMONIA data used for Fig.4 were annual data that have been smoothed by a four point running mean. This was done to reduce the influence of high frequency “noise” mentioned above, which is substantial (a factor of 2). . . . “ There are two problems with this figure. First of all, the similarity in heights of correlation and standard deviation peaks depends on the selected reference height for the correlation profile. If this was 30 km or 50 km instead of 42 km, the correlation profile would be different, and also their similarity to the standard deviation profile. Secondly, from the two profiles shown in Figure 4 one is based on annual data and the other on the

smoothed time series. This raises the question if the similarity pointed out is valid only for the smoothed standard deviation profile, but not for the suppressed time scale. To avoid this question, it would be necessary to show the standard deviation profile for annual data, even if more “noise” appears.

L332: “. . . The results are very similar to those of HAMMONIA . . . “ Here the same problems need to be addressed as for Figure 4.

L355: picrure → picture

L360-361: “. . . Obviously, the computer simulations contain periodic temperature oscillations, . . . “ What seems obvious is potentially misleading, where periods are nearly as large as the time series. FFTs of the full time series alone do not give any information on the uncertainty. This can be provided by spectral estimators, which make use of (possibly partially overlapping) time windows. The use of time windows of course reduces the maximum period.

L362-364: “. . . Because the boundary conditions of the computer runs were kept constant, these oscillations cannot be excited from the outside. They are therefore interpreted as self-excited (self-sustained) oscillations, and thus as intrinsic properties of the atmosphere . . . “ The attribution of the variability to the atmosphere is wrong. It can be attributed to the atmosphere – land system, provided the boundary conditions are “fixed”.

L378: “. . . The mean spectrum of all altitudes was determined . . . “ Is this simply the arithmetic average over all levels?

L386-387: “. . . For each representation we took noise from a Gaussian distribution . . . “ If the idea here is that “white noise” can be used as default, the question arises if the internal variability rather resembles white noise or red noise. If any redness is allowed for, the “2-sigma” line would would no longer be a horizontal line I Figure 8, but rather increase towards larger periods (smaller frequencies). Thus the main question here is why white noise is a good choice for estimating the background noise, against which oscillations should be identified. White noise has been assumed for instance by Hasselmann (1976) to represent forcing by weather, and to show that this would explain a red spectrum in the longer climate time scales. In the work presented here, the time scales are rather in the climate than in the weather time scale range, from which one could motivate the usage of red noise.

L397–398: "... A coupling mechanism between the layers has to be present to explain the observed mean Lomb-Scargle Periodogram for the ECHAM6 data. ... " Figure 8 collects by construction spectra from different levels into one graph, whether or not they are vertically coupled in the model. This cannot be derived from this graph alone, even if the coupling exists. A cross-spectral analysis between the levels of interest would be more suitable.

L438-450: " This analysis was performed for all altitude levels available. ... The harmonic analysis algorithm calculates an amplitude and phase if a prescribed (estimated) period is provided." I find it quite unfortunate that the authors use such techniques to derive amplitude and phase information for periods which cannot be determined in the preceding time series analysis. This generates a number of results which are highly questionable. Scientifically it would be much more rewarding to learn about the nature of the few frequencies which are strong, which can be identified in model simulations used here and in observational data.

L508-509: "... This is remarkable because many more oscillations are contained in the ECHAM6 data set than in HAMMONIA ... " This is to be expected because the underlying processes, which drive the dynamics of the models, are of the same nature. Then the shorter time scales existing in the shorter HAMMONIA simulation have to be expected also in the longer ECHAM simulation.

L535-536: "... The maximum period cannot be longer than the length of the computer run. ... " This holds for Fourier transforms. But spectral estimates must be limited to

considerably shorter periods, maybe half of the time series length or less, depending on the uncertainty one wants to allow for. Thus also the upper limit for periods (lower limit for frequencies) needs to be chosen smaller than the time series length.

L551-552: "... For the measured data in Table 2 it needs to be kept in mind that they were under the influence of varying boundary conditions. ... " Should the varying boundary condition - with respect to the atmosphere-land system of the model simulations – eliminate oscillations if these are self-sustained? I find this difficult to accept. Can you describe how this should work? But I would agree that the richer forcing of the real world, compared to the "fixed climate" model simulations, adds variance to the spectrum.

L627-629: "... Also shown in Fig.13 is the correlation profile of HAMMONIA from Fig.3 (black squares here). The two curves are surprisingly similar (correlation coefficient is 0.80. Outside the range shown the correspondence is lost.). .. " Here the correlation profile depends on the choice of the reference levels, and thus the reported similarity to the vertical temperature gradient profile also depends on the choice of the reference level. A change of the reference level from 42 km to 30 or 50 km would strongly modify the similarity and the conclusions drawn from this. Still, one can expect that the vertical expansion of certain oscillator modes will be confined by the general stratification of the atmosphere. But this needs to be shown differently.

L636-638: "... If an air parcel is displaced vertically by some distance D ("displacement height") a relative change in mixing ratio is observed ... " A vertical displacement alone will never change the composition of air. This happens only in the presence of chemistry or photo-chemistry, which is sensitive to for example temperature or radiative fluxes.

L728-730: "... An FFT analysis was performed in 12 equal time intervals (blocks of 32 yr length) in the altitude regime 0.01 – 1000 hPa and the period regime 2 – 40 yr. ... " Blocks of 32 years do not allow to compute amplitudes/phases for 40 year periods.

L878-879: “. . . and even if the boundary conditions of sun, ocean, and greenhouse gases are kept constant. . . . “ It is shown here only for “fixed” boundary conditions. Therefore it is better to write: “. . . where the boundary conditions . . . “

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