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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multivariate analysis of Kelvin wave seasonal variability in ECMWF L91 analyses" *by* Marten Blaauw and Nedjeljka Žagar

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Review of "Multivariate analysis of Kelvin wave seasonal variability in ECMWF L91 analyses", by Marten Blaauw and Nedjeljka Zagar, submitted to Atmos. Chem. Phys.

Minor revisions

General comments

This is a fine paper on Kelvin activity that utilizes the normal mode function decomposition method pioneered by Kasahara and Puri and further refined by Zagar and others. The paper essentially represents a "proof of concept" of the technique as applied to Kelvin waves, although a lot of interesting information is included. The paper succeeds in demonstrating the utility of NMF decomposition and should provide a good start-

ing point for those interesting in pursuing this approach, especially given the fact that software has been conveniently set up for others to apply as described in Zagar et al. 2015.

Specific comments

The paper appears to be in good shape overall, although there are some lingering questions in this reviewer's mind about interpretation, as discussed below. This mainly has to do with lumping all of the vertical modes together, which does not necessarily seem physical to me for all the cases considered. Perhaps not for this study, but it would be very instructive and add a lot of value to come up with some associated relationships between the subseasonal variability in Kelvin energy discussed here and some indices of tropical convection. The authors have made an initial attempt of this for the seasonal cycle and interannual timescale and their interpretation seems reasonable there. As far as higher frequencies go, for instance, cross spectra between the timeseries shown in Fig. 5 and geographically distributed OLR or brightness temperature could be very revealing. Ultimately, this could also be done for other modes isolated by this technique too. Figure captions could be improved overall, especially at the locations noted below. Also the text is not as clear as it could be in places.

Technical corrections

Comments by line number:

32: do you mean "modulate the TTL"?

49: nomenclature here is slightly confusing: hkw has not been defined. If you are indeed following Holton then this is a perturbation term, otherwise it might be mistaken for the equivalent depth.

58: this statement is misleading, actually the tropical "cloud activity" really refers to the mean cloudiness (Tindall et al. were citing Zhang 1993) which is a maximum in January and minimum in July near the equator.

79: the horizontal and temporal resolution from line 106 should also be included here.

93: not sure what “denotedm” means here, should this be in parentheses (denoted m)?

126: I am confused by one aspect of this procedure: As nicely discussed in detail by Zagar et al. (2015) and references therein, each vertical mode is characterized by an equivalent depth and associated horizontal structure function. Here it appears that all of the vertical modes are summed. For the sake of discussion, suppose we have a stratospheric “free” Kelvin mode which is present at the same time as an independent “convectively coupled” Kelvin mode that has maximum amplitude in the troposphere. These could be either collocated in lat-lon space or present at the same time in different regions of the globe. I think it would be profitable to make clear that it would still be possible to separate these modes by this procedure if one had enough additional information on the associated equivalent depths of each mode, which could be much different from each other. Stratospheric Kelvin waves at 50hPa follow dispersion related to a 120 m equivalent, whereas this is more like 25 m for convectively coupled waves. Acknowledging this fact seems appropriate, along with perhaps some words on how it could be dealt with in practice. I wonder, for instance, if investigating time series of KW energy for individual vertical modes could be done in a systematic way, using an extension of the approach in Zagar and Franzke (2015)? I think it would add considerable value to add a short discussion on these points.

137: As in the previous comment, you are including the projection onto the vertical mode that corresponds to, say, the 10 km equivalent depth, which I would assume be more representative of an external Kelvin mode. Perhaps one way to look at this is to assume that there would be a “spectrum” of vertical modes for each situation depending on how much the data projects onto each individual mode. I think it would it be worth elaborating on this point here, especially for those who may be less familiar with the idea that you are discretizing the vertical and associated horizontal structures for a reason, but that in reality a given atmospheric disturbance will be composed of a potentially different combination of these from case to case.

138: probably should describe the figure in more detail first, such as the tilted structures of what fields are shown, etc., before launching into the implications.

145: “strong KW activity was present” I guess you are referring to the case discussed in Fig. 2? Are you saying that the entire pattern shown represents a free Kelvin mode? This is where some information on the associated convective activity might be very useful.

154: Another very useful bit of information would relate the activity of KWs (such as measured by the energy spectrum) to the QBO, perhaps in a future study.

161: “climatological zonal structure” at first, I thought this was only for the period of Fig. 2 and the figure caption does not help.

191: “resonates” => “fluctuates” might be a better choice of words.

244: I’m unsure what the tidal effect would look like, but could the tide itself be projecting onto the Kelvin structure in some way? It doesn’t seem that a Kelvin wave structure should be impacted by the tide, especially if you consider that these are both orthogonal “normal modes”. This may deserve a few more words and could emphasize one potential drawback of the approach.

259: Probably should use a bit more care when discussing the impact of the QBO since this is highly dependent on the level you are referring to. One way to put it might be to point out that vertical penetration of Kelvin wave energy into the stratosphere depends on the state of the QBO in the lower layers of the stratosphere.

269: It turns out the the QBO was easterly at 50 hPa in July 2007, but only just beginning the easterly phase at that level.

291: What is the advantage of using “absolute amplitude” over say, variances? This should be discussed and justified.

287: I wouldn’t insist on it, but it may also be of interest to examine whether there is

significant skewness in the distributions of the raw filtered data on the subseasonal timescales, and whether they are approximately normal. In other words, does it matter what the phase of the Kelvin component is at a given level, or are negative perturbations approximately the opposite of positive ones?

292: caption and discussion of Fig. 7 is confusing. When describing 7a the caption says “mean” when you really mean “(semi)annual” as discussed above.

Isn’t 7c for the high frequency? If so the caption should say so.

297: “quadrupole” is more commonly used, but perhaps not in Europe.

310: There is decent agreement between the results here and those of Flannaghan and Fueglistaler as to where the Kelvin activity is maximized and I think this should be discussed at this point as well as below (e.g. Fig. 6 of Flannaghan and Fueglistaler 2013).

333: This is a very interesting analysis and the link between the low frequency Kelvin component to the “Gill-type response” is very insightful. However, the pure Gill response is only Kelvin-like to the east and includes Rossby gyres to the west of the heating, so at least part of the easterlies would not necessarily be due to a projection on Kelvin waves. This should at least be mentioned, if not discussed in more detail.

356: This is a clever way of getting at the impact of the quasi-stationary Kelvin wave forcing.

384: It would indeed be interesting to see what the relationship between intraseasonal Kelvin activity isolated here might be to the convective activity on the same timescale.

416: It seems that the right panels refer to a specific longitude only but this is never identified in the text or caption.

431: Now it seems that these numbers are somehow zonal averages? Rather confusing, and once again the figure caption does not help either.

437: I guess this is not shown? That should be noted.

Signed,

George Kiladis

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