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Interactive comment on "Long-term Lidar Observations of the Gravity Wave Activity near the Mesopause at Arecibo" *by* Xianchang Yue et al.

Anonymous Referee #2

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Xianchang Yue et al. report about temperature soundings obtained with a potassium resonance lidar at Arecibo (18°N). Overall, 1451 h of data are obtained between December 2003 and April 2017, with good data coverage especially in the first three years. From this data set the seasonal variations of temperatures and their variability are derived, with emphasis mainly on the AO and SAO. There are only few temperature data sets available from the tropical mesopause region, and these data are a worthwhile contribution. The paper is well written and the Figures are of good quality. Unfortunately the whole reasoning is partly incomplete and digs not very deep into the data. Examples are given below. Overall, I recommend revision and extension of the manuscript.

General comments:

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Discussion paper

- The authors describe in the Discussion a relation between the wind field as published by Garcia et al. (1997) and Smith (2012) and the observed variation of GWPED. While there is indeed a pronounced altitudinal and temporal correlation, the paper lacks a description of the mechanism that relates the GW activity and zonal wind velocity. All statements are true, but remain vague and unspecific. The interpretation seems to imply pure zonal propagation of the waves, but the lidar data contain waves of all directions. Is the westerly wind between 60 and 70 km taken into account that may filter a lot of the eastward propagating GW?

- The authors do not show any kind of raw data, i.e. mean temperature profiles of a single night or even examples for temperature variability (T'). The lowest level figures are fitted AO and SAO, and it remains open, how representative they are. The authors state that these are the most important variations, but the large variability of the mean values in 2b/3b/4b and the "random phases" (l. 8/1 on page 8) seem to contradict. Therefore I recommend to provide also examples for T and T', as well as unfitted seasonal variations of all relevant quantities.

- The calculation of T' should be described in much more detail. This is the most crucial point for the interpretation, and a reference to Gardner and Liu 2007 is not sufficient. What are the main points in the retrieval? How does incomplete sampling influence the results? How are tides removed from the fluctuations? How is the increasing uncertainty at the layer edges acknowledged?

Specific comments:

l. 1/14-15: I think it is not a unique feature that N^2 maximizes below/at an inversion. This is just the result of the temperature increase with altitude (dT/dz is large and positive).

l. 1/26: I suggest to replace "usually" by "often" as there are also stratospheric sources, secondary waves etc.

l. 2/9: There is a logical break. I suggest writing "The stratospheric SAO leads to a seasonal variation of filtering of the upward propagating waves, which results in a specific seasonal variation of GW activity in the mesosphere."

l. 2/34-3/1: Sounds odd. Suggestion "Mze (...) observed a nearly undamped propagation of GW ...".

l. 3/10: What is meant by "transforming of the mean zonal wind"?

l. 3/26 and l. 3/31: The reference to Yue at al. 2017 is not appropriate here, because the cited paper mainly deals with K density data.

l. 4/4: The worst data coverage is (by chance?) right in the month of largest wave activity. This should be discussed shortly.

l. 4/11-13: No. The ratio of kinetic and potential energy is a function of the intrinsic period of the wave (and the Coriolis parameter). From temperature soundings only the potential energy can be calculated, but not the total energy, because the intrinsic period is generally unknown.

l. 5/8: Please motivate the choice of this altitude interval. Especially N^2 is strongly varying in this range, and the phases of T and N^2 precess.

l. 5/13: Is the inversion also visible in the raw data or is it a result of the fit? If it is real, it should be discussed in more detail because it may strongly affect the propagation of GW. If it is not discussed here, some reference should be made.

l. 5/15: It remains open from this Figure, whether the mesopause could also be above 100 km in Sep-Dec.

l. 5/20 and Fig. 2: It would be helpful to have contour lines to assess the similarity between the data sets.

l. 5/28: This is essentially expected if N^2 is calculated from the mean temperature data set. See above.

l. 6/1-2: Please explain why it is worth to note this. My impression is that any kind of instability would be eliminated by building the temperature composite and applying an AO/SAO fit. Is the mesopause region more stable above Arecibo than somewhere else? If so, please explain and provide a reference.

l. 6/4-5: Consider plotting the phase of the SAO shifted by 180 d between 96 and 99 km (-80 will be +100). It may look nicer. Is the variability of the AO phase really unexpected (being the derivative of another property)? How does the variable phase affect the conclusions of the paper?

l. 6/8/9: Do I understand correctly that the fitted T and N^2 are used as the mean value to calculate T'? This would be a significant difference to Gardner&Liu. Furthermore, using the fitted time series will affect the seasonal variation of the derived GW activity. Please clarify.

l. 7/9-10: I actually do not understand this sentence. Temperature enhancement from dissipating GW? Secondary waves? How relates a strength in temperature variation to a wind velocity?

l. 7/13: How much of the increase above 97 km is due to the increasing uncertainty at the edge of the K layer or by a (potential) deviation of the fitted data from the original data?

l. 8/1: I do not agree that the phases of T and N^2 are "random", even if they are less constant with altitude. A random phase would imply that AO/SAO are not relevant oscillations in this altitude range.

l. 8/5-6: Please be more specific: Which processes do you expect that relate the GW activity to the seasonal wind variation in your data and what evidence you have?

l. 8/9-10: This statement is rather vague. Please be more precise.

Figure 2-4: Please provide error bars for fit amplitudes and phases.

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Discussion paper

Figure 4: I recommend plotting the GWPED per volume. For linear propagation this should be conserved. The strong increase of GWPED close to 100 km would be less pronounced.

Technical comments / Typos:

l. 3/19: "exhibits" should read "exhibited"

l. 4/8: "evenly" should read "even"

l. 5/2: "is" should read "are"

l. 6/14: It is Fig. 4a