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Atmospheric Chemistry and Physics
Discussions

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

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Response to the Referee #1 Interactive comment on "Long-term Lidar Observations of the Gravity Wave Activity near the Mesopause at Arecibo" by Xianchang Yue et al. Anonymous Referee #1 Received and published: 30 August 2018 Response: Thank you for your constructive and kind comments.

General comments: This paper shows the extended climatology of temperature and potential energy density above Arecibo using lidar data. My main comment about the paper is that the work on gravity wave activity is not a major part of the paper despite it's title. I would like to see included at least one comparison with other gw lidar studies in the mesopause region (regardless of latitude) to see how their results

compare in terms of seasonal variation or magnitude of gw activity observed. Perhaps also an expansion of the GW section by also looking at the year to year variation of GW PE if the authors feel it is appropriate and are not planning on doing this for a future paper. Response: there are some reports about the seasonal variations of temperature variances around mesopause in the literatures, we will add some comparisons about temperature variances in the revised manuscript after finishing the reprogramming of the data processing program.

Specific comments: Page 3, line 1 – What do you mean by the conservation of GW potential energy? This needs a clearer explanation. Reponse: Thank you for pointing out this improper sentence, the corresponding sentence has been rephrased by "Mzé et al. (2014) observed a nearly undamped propagation of GW in summer in the low mesosphere". Page 3, lines 2-3 – you need to include more detail into why these studies show that more attention should be paid to the mesosphere in terms of gw parameterizations. What do your results in this paper offer that will help improve these parameterizations? Response: Thank you for this comment, this sentence has been revised as "Since the effects of GW in the numerical climate and weather prediction models are usually represented simply by parameterization (Kim et al., 2003). there are still large discrepancies between model and measurement results (Geller et al., 2013). Therefore, more attention should be paid to the GW parameterization about these kind of observations in the upper mesosphere and mesopause region to improve the model results." Page3, line 9 – "transforming" is not the right word here, I think you mean changes. Also, what is the change in the mean zonal wind above 80 km in the tropical region? This needs to be explained. Response: "transition" is a proper word to replace "transforming". The changes in the mean zonal wind near mesopause in the tropical region has been introduced in the 3rd paragraph of the introduction section. Page 4 – line 1 – Can you please include a reason as to why there is a 5 year gap in the dataset. Is the data from two different K lidars? Was the one lidar broken? Response: In the time from 2011 to 2015 the lidar building was upgraded with an extension for the telescopes which were in a hut until this time. Page4, equations 1 and 2 –why have you used the EP equations for temperature from Vincent et al and not used the one that Mze et al (2014) use in their lidar studies? Response: The difference between equation (1) in this manuscript and the equation (8) in Mze et al (2014) is that (T'/T ÌĚ)ˆ2in this (1) is replaced by atmosphere variance in that (8). The temperature inversion by an K Doppler lidar near the mesopause region uses the resonance scatter signal of K atoms, while the atmosphere variance estimation by a Rayleigh lidar from 30 to 80 km uses the Rayleigh scatter signal of atmosphere molecules which is taken as part of the background noise of K Doppler lidar.

Page 4, line 21 – please include a brief description of the procedure for calculating T' rather than just pointing at a reference. Response: The brief description added is as the following "For each night of observation, data points with photon noise errors larger than 10 K in temperature are discarded first. The linear trend in time is then subtracted from temperature profiles at each altitude to compute the temperature perturbations, perturbations exceeding three standard deviations from the nightly mean are discarded. Finally the vertical mean is subtracted from each temperature perturbation profile." Page 4, line 25 – Doesn't applying this Hamming window alter again the minimum period and wavelength gravity waves that you will be able to detect? This will make the values you state at the start of section 2 invalid. Please address this in the text. Response: Thank you for this question. We made a mistake in the writing. We have not applied this Hamming window on the temperature perturbation Tˆ'. "The weekly composite night data of ÂŕT and Tˆ' are" have been updated to "The weekly composite night data of ÂŕT is" after we checked the data processing MATLAB procedure. Page 4, lines 27-29 – what model is referred to here? I suspect it's the harmonic fit used in the Friedman and Chu paper you reference but it's not clear at all. More detail on what exactly is being done here and why is needed in the text. Response: Yes, it is just a harmonic fit, we have added the following texts and equation in the revised manuscript: "The equation of the model is as following: Ψ(z,t)=Ψ_0 (z)+A_12 (z)cos[2π/(365/7) (t-φ_12 (z))]+A_6 (z)cos[4π/(365/7)(t-φ_6 (z))] (3) where Ψ(z,t) is the value of a weekly mean parameter at altitude z and week t, expressed in week of the

year (1-52), Ψ_0 (z) is the annual mean, A_n (z) and φ_n (z) (n=6,12) are the amplitude and phase of the -month oscillation, respectively." Page 5, line 19 – why is the secondary peak insignificant? Surely it is just not as large, why does that make it statistically insignificant? Response: We have used an improper word here. 'An insignificant secondary' has been updated to "A secondary" in the revised version. Page 5, line 21 – I have compared Figs 6 and 7 in the Friedman and Chu paper (F&C) with your Figure 2 and yes the annual variation is similar but there are also large differences that need to be explained. In your Fig 2a the vertical temperature structure is different to that shown in F&C, with you showing warmer temperatures around March and October/November that are more extensive that those in the F&C paper. Also the semi-annual phase and amplitudes they show are quite different to yours in Fig 2c&d (which is expected as the SAO you show is different). The question needs to be asked as to why your climatology (which includes the data used in the F&C paper) is showing such differences. Are you using the exact same method as F&C? If not, when you perform your analysis on the same section of data as used in F&C do the results agree? Are there one or two years which have this warmer vertical structure and that is influencing the results in your paper? You need to explain why you are seeing a different structure to other results which use part of the same dataset. Response: Thank you for pointing out the difference between these two work. The differences are caused by three reasons. The first and the key point is the quality control to calculate the temperature perturbation in this study. Figure 1 in the appendixes of this response shows the fitted result without quality control. It is quite consistent with the Figure 6 in F&C. The second reason is the much more extensive data set from year 2003 to 2017 covering a whole solar period here. The last reason is the harmonic fit model is in term of week here, while it was in term of month in Friedan and Chu (2007).

Page 6, line 25 – you need to show an example of the seasonal cycle of the zonal winds to which you refer to in the paper Response: Thank you for this suggestion. We have added "(see e.g., Fig. 3 in Garcia et al. 1997; Fig. 3 in Smith 2012), the monthly mean HRDI equatorial zonal wind showed that, the easterly winds were prevailing in equinoxes seasons near 80 km altitude. They then decreased with altitude from 80 km above and turned to increase above $\sim$ 92 km, while the westerly winds prevailed in in the range 80-94 km in solstice seasons, they then turn to be easterly. the reversal is at about 95 km (Smith, 2012). Therefore, the zonal winds are low or zero around 92 km altitude in tropical region. The zero-wind lines will enhance damping or dissipating of zonal propagating gravity wave with low to moderate phase speed.' between "In retrospect to the scientific literature in SAO studies" and "The features of gravity wave potential energy . . .. . .". Figure 2a – can you plot the MIL you refer to on the text on the figure Response: Figure 4a – it might be easier to compare with other sites/lidar gw studies if you plot the lognormal of the GW PE. Response:

Technical corrections: Abstract – the phrase "potential energy of the temperature fluctuations" is not correct. You are using the temperature fluctuations to determine the potential energy density of the gravity wave field, i.e. the gravity wave activity levels. Please change so that it is correct. Response: Thank you for pointing out this error. Phrase 'potential energy of the temperature fluctuation' has been changed to 'potential energy derived from the temperature data' in the revision Page 2, line 21 – change "are" to "have been" Response: It is changed. Page 2, line 34 – eminent is not the right word to use here, do you mean evident? Response: "evident" is proper here, and 'eminent' is replaced with 'evident'. Page 2, line 34 – again I don't think you mean to use "almost", "also" would make more sense. Response: Thank you for this comment, we have rewritten this sentence as "Mzé et al. (2014) observed a nearly undamped propagation of GW in summer in the low mesosphere." Page 3, line 5 – "These researches" should be replaced with something like: "The studies" Response: It is replaced. Page 3, line 18-19 – The sentence "the vertical structures of SAO and AO in these parameters and their relationships are exhibits" does not make sense, please rephrase. Response: since this sentence is not necessary, we omitted it in the revised version. Page 4, equations 1 and 2 – The overbar on the temperature indicates averaging over altitude, please include this in your description of the variables. Response: It is included according to this suggestion. Page 4, line 22

– replace 0.5h with 30 minutes (or replace 30 minutes with 0.5h in the other instance in the paper). Try to be consistent with how you refer a time interval. Response: "0.5h" has been replaced with 30 minutes here.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-731/acp-2018-731-AC1-supplement.pdf

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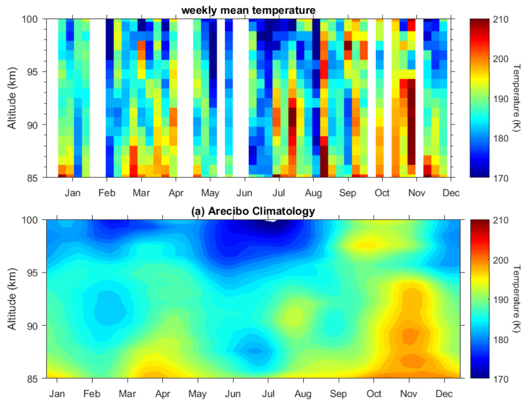
weekly mean temperature
Altitude (km)
Temperature (K)
(a) Arecibo Climatology
Altitude (km)
Temperature (K)
Jan
Feb
Mar
Apr
May
Jun
Jul
Aug
Sep
Oct
Nov
Dec

Fig. 1.