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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Influence of gravity wave temperature anomaly and its vertical gradient on cirrus clouds in the tropical tropopause layer – a satellite-based view" *by* Kai-Wei Chang and Tristan L'Ecuyer

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In this paper, remote sensing satellite observations of cirrus clouds, temperature and water vapor are combined to analyze the relationship between ice clouds in the tropical tropopause layer and gravity waves. Using the much larger statistical sample enabled by satellite measurements, the authors confirm previous findings obtained from in situ observations. Furthermore, they expand on previous work by evaluating the qualitative consistency between observations and theoretical predictions linking ice cloud properties to different wave phases. In particular, they examine the influence of background

relative humidity on the gravity wave-cirrus relation and the ice crystal size in different wave phases.

Overall, I find this study thorough and clearly written. The authors designed enlightening composite diagnostics and present rigorous significance tests of their results. The article is definitely worth publication in ACP. I only have a number of minor comments and suggestions for the authors' consideration, which are detailed below. Since my review comes at a late stage in the ACP discussion process (apologies) and since the authors have already answered referee 1, my comments concern the manuscript as well as the reply to reviewer 1. I mostly agree with the other reviewer's suggestions but have spotted a misunderstanding regarding the P18 paper.

Minor comment:

Contrary to Kim et al (2016), the authors show the distribution of clouds between the different wave phases instead of the mean cloud fraction in each phase. The former can be deduced from the latter (which the authors do on page 6 line 148), while the reverse is not true. I am a bit puzzled by this choice which appears to result in a reduction of the information conveyed by the figures.

Is it motivated by the sensitivity of the mean cloud fraction to the CAD threshold used to distinguish clouds from aerosols? By the fact that the cloud fraction depends on CALIOP optical depth detection threshold? Or is it just meant to facilitate the visual and quantitative comparison between regions/altitudes with different cloud fractions? Some explanation would be welcome.

Specific comments

- page 2, line 44-46: A clarification here related to Referee 1's comment: "while P18 agrees that ice is suspended in phase 1, I would mention their finding that upward vertical motions permitted the presence of TTL ice clouds within phase 3 as well" and the authors reply. Actually, the interpretation in the submitted manuscript was correct: P18 indeed claim that phase 1 is more favorable for cirrus than phase 3. Referee 1's confusion lies in the choice of words in the quoted sentence. The exact P18 sentence reads: "The precise location where the confinement occurs ... is always characterized by a relative humidity near saturation and a positive vertical wind anomaly" . The effect of relative humidity cannot be put aside to correctly interpret that sentence. Indeed, relative humidity near or above saturation is mostly found in negative temperature anomalies (Phase 1 or 2) while the vertical wind is positive in phases 1 and 3. Taken together, the two conditions mean that clouds are found in phase 1. This is illustrated by figure 2 of P18. Note that the sketch in their figure 3 was meant for purely pedagogic purposes, showing possible cases.

- P3 line 83-85 : Are the results affected by the choice of the CAD threshold ? By how much does the average TTL cloud fraction estimate vary depending on this threshold?

- P3 lines 88-92 and fig. 10 : I believe that in most thin TTL cirrus there are only Lidar measurements available (their reflectivity is below the radar detection threshold), so that re cannot be unequivocally determined. This might explain why the 2D ice data are so uniform.

- P5 line 128-130: This is more of a sanity check, but are all 4 phases equiprobable ?

- P6 line 147: you should specify that K16 measurements were made in the Western Pacific

- p7 lines 160: The contrast remains, but I would say that K16 found "slightly" more clouds in phase 2 (6 % vs 5 % in phase 1).

- p7 line 172 and Figure 5: Instead of the count (number of layers?), would it be

clearer to show some "phase fraction" (i.e. normalized by cumulated altitude)? I am unsure myself.

- p8 line 177-182: I like the composite approach, but it might be worth mentioning that, by essence, it is stationary in space, so that neither the cloud (moved by the wind) nor the wave anomaly (moving at the wave phase speed) are followed. For instance, if the structure moves at 10 m/s, 30 hours corresponds to a displacement more than 1,000 km (10 times the collocation radius) away from the location where the composite is made.

- P10 lines 202-203: I don't think the roles of the wave through its impact on stability and onvertical motion can be easily disentangled. They necessarily hold at the same time, as a consequence of the polarization relations.

- P10 lines 205-209: I am wondering whether the typical vertical wavelength of 3km which is emphasized by this composite means something.

- p13 line 266: The confinement hypothesized in P18 can only occur in Phase 1 (see their Fig 2.), not in Phase 2

- p13 line 270: Note that P18 derivation is also valid for a constant background wind (the exact same set of equations are obtained considering a frame moving at the constant background wind speed). However, it is true that the background wind shear cannot be as easily included in their approach and is neglected.

- P13 line 280 : A prediction of P18 is that the fall speed is comparable to the GW vertical phase speed. To get an idea, could you compare the fall speed of a crystal (assumed spherical for simplicity) of that radius to to the typical GW vertical phase speed in Fig. 6?

- P15 line 285: Again, P 18-type confinement always occurs in phase 1 for a monochromatic wave. With lower Rhic, the location gets closer to the boundary between phase 1 and 2, so that with a superposition of waves both phases might show similar cloud fraction.

- P 15 lines 290-300: It is an interesting approach that the authors attempt here, and I like the new Fig 11 in the reply to referee 1. However, I see small issues with the method employed by the authors.

  First, the non-linearity of the Goff-Gratch equation means that the coarse-resolution MLS water vapor divided by the saturation pressure of the mean temperature will lead to a larger relative humidity than the average relative humidity (average ratio of the two). This might explain why the authors estimates seem slightly high-biased compared with in situ observations of TTL humidity (see for instance Jensen et al., 2017 for a survey of TTL relative humidity from in situ measurements). This might result in a systematic bias in the authors' estimate. However, I imagine that the trend found by the authors will not be not sensitive to this.

  Second, as far as I understand, the temperature is estimated from 7-day averages but the water vapor from instantaneous values. For consistency, the water vapor should be taken from averages as well.

- p16 line 343: 4-5 km seems larger than the typical wavelength which comes out of your composites Fig. 6

- p16 lines 345-348: "Since negative dT 0 /dz corresponds to a positive cooling rate (due to downward phase propagation as explained by K16), weakened stability, as well as upward vertical motion wave anomalies (according to the gravity wave polarization relationships),": the two points " positive cooling rate" and "upward vertical motion" are equivalent under the usual adiabatic approximation. One should be removed from the sentence.

Wording:

- P1, line 20 :Maybe replace "favor" by "is favorable to" or use a passive.

- p7 line 167: 'vertical cloud fraction': I would remove vertical.

- P7 line 168: 'cloud boundaries' → 'cloud layer' ?

- P7 line 175: Again, I would put a passive form.

- P10 line 213 : "a similar compositing technique ..."→ "a compositing technique similar to the one employed above "

- P16 l340 : large wavelengths → large horizontal wavelengths

Reference

Jensen, E. J., et al. (2017), Physical processes controlling the spatial distributions of relative humidity in the tropical tropopause layer over the Pacific, J. Geophys. Res. Atmos., 122, 6094– 6107, doi:10.1002/2017JD026632.

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