Response to Referee 2's comments

Thank you for the feedback on the submitted manuscript and Referee 1's comments. We have adopted most of your suggestions, as detailed below. Referee comments are in black, author responses are in green, and manuscript changes are in blue.

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.

Author Response: With aircraft observations as in Kim et al. (2016), they can obtain the cloud fraction in each phase, presumably by dividing the number of observations with clouds by the total number of obs. With CALIPSO observations, the 'total' number of observations is somewhat arbitrary. For example, if we define the total number of obs to be all CALIPSO bins within 14.5 to 20 km, then the cloud fraction in all phases will be quite low, since above ~17 km most CALIPSO bins will have no clouds. As you can see the 'cloud fraction' derived this way can vary a lot depending on the TTL height bounds, so we decided to look at the partitioning among phases, which is straightforward to interpret.

Manuscript Changes: No changes made for this comment.

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.

Author Response: We will leave this sentence as-is, since the interpretation is consistent with P18

Manuscript Changes: No changes made for this comment.

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?
Author Response: The choice of CAD fraction doesn't affect the cloud fraction much. In terms of the percentage of all TTL clouds in each phase, a threshold of CAD >= 80 results in 53.4%, 25.9, 12.2, and 8.5 for Phases 1,4, while CAD>=90 yields 53.4%, 26.1%, 12.1%, and 8.5.
Manuscript Changes: No changes made for this comment.

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
Author Response: Yes, it is true that radars miss most of the thin TTL cirrus. Aside from this, their being very optically thin also poses challenges in constraining the retrieval.
Manuscript Changes: No changes made for this comment.

P5 line 128-130: This is more of a sanity check, but are all 4 phases equiprobable?
Author Response: Yes, we've done some checks and found that the number of samples across the four phases are more or less evenly distributed.
Manuscript Changes: No changes made for this comment.

P6 line 147: you should specify that K16 measurements were made in the Western Pacific
Manuscript Changes: Added the clarification starting at line 152 of the revised manuscript.

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).
Manuscript Changes: Added "slightly" in the at line 170 of the revised manuscript.

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.
Author Response: You are correct that the count is the number of layers. We added an example of how the cloud fraction is calculated, in hopes of making this part clearer. We don't' quite understand what is meant by showing phase fraction normalized by cumulated altitude, and decided to leave the figure as-is.
Manuscript Changes: Added an example of the cloud fraction calculation at line 180 of the revised manuscript.

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

Manuscript Changes: We added a sentence in the conclusion (line 351) to note the stationary nature of this technique.

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

Manuscript Changes: We modified this sentence here to be : "Since negative dT'/dz also corresponds to upward vertical motion anomalies (assuming that these anomalies are from gravity waves), it is difficult to separate the effects of each on cloud formation."

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

Author Response: We are not quite sure. One thing comes to mind, which is that Alexander et al. (200) found that temperature fields at dominated by waves of vertical wavelengths ~2 km, which is not so different than 3 km. However their result is for the tropical lower stratosphere 18 – 25 km so their findings may not have a direct connection to the ~3 km structure we found.

Manuscript Changes: No changes made.

P13 line 266: The confinement hypothesized in P18 can only occur in Phase 1 (see their Fig 2.), not in Phase 2

Author Response: We mentioned that ice can be found in Phase 2 based on P18's Figure 2 right panel, where for RHic=0.63 you can see a small portion of the ice within Phase 2. We reworded this sentence to clarify this.

Manuscript Changes: At line 271 of the new manuscript, the sentenence is now "P18 suggests that (1) ice crystals within a confined range ofr_e are suspended in Phase 1, and (2) for low background relative humidity with respect to ice (RH_ib}), the confinement in Phase 1 may be positioned closer to 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.

Manuscript changes: This sentence (now at line 274 of the revised manuscript) is changed to note that only wind shear is neglected in P18.

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?

Author Response: Assuming a radius of 15 um and temperature of 200 K, P18's equation (20) (valid for radii of 5-100 um) yields a sedimentation velocity of ~ 2cm/s. This corresponds to a displacement of ~1.3 km over 18 hours. In Figure 6, using the cold anomaly in -12 to +6 hours, the descent rate is about 1 km, which is comparable to the ice sedimentation velocity.

Manuscript Changes: A paragraph is added at line 221 to describe P18's prediction regarding the fall speed, and how the descent rate of the cold anomaly rates to the estimated ice fall speed.

P15 line 285: Again, P18-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.

Manuscript Changes: Sentence at line 290 is modified to say that in low RHic ice gets closer to Phase 2 (instead of being inside Phase 2).

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.

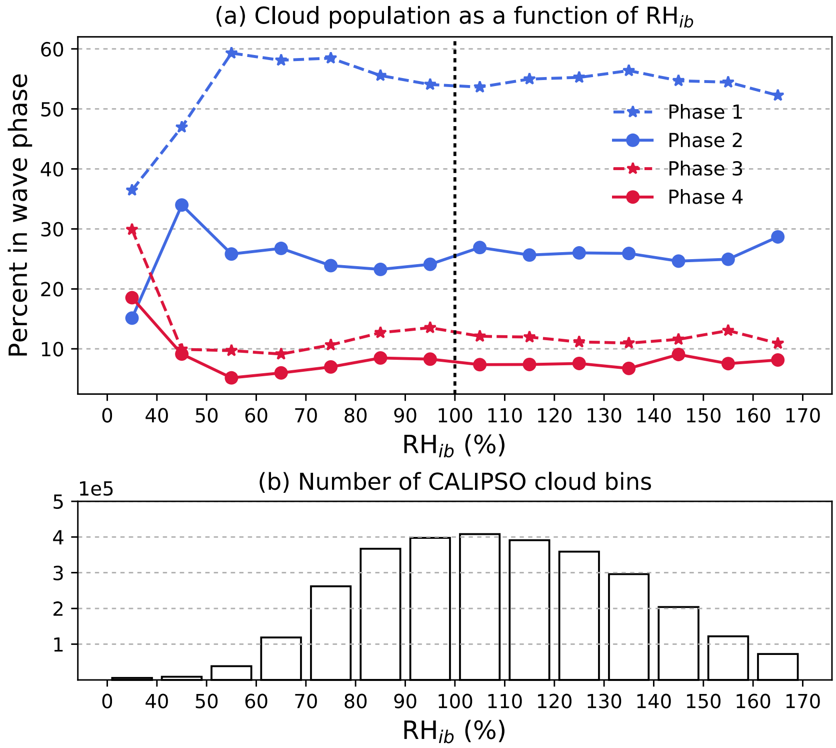
First, the non-linearity of the Goff-Gratch equation means that the coarse resolution MLS water vapor divided by the saturation pressure of the mean temperature will lead to a larger relative humidity than the average relative humidity (average ratio of the two). This might explain why the authors estimates 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.

Author Response:

We will note in the text that the coarse resolution of the MLS and the nature of the Goff-Gratch equation may have contributed to the high-biased RH estimates. Regarding your second point, we have tried to calculate Figure 11 with 7-day averages of moisture, as shown below. In this Figure, the Phase 1 fraction increases up to RH = 60%, and then there isn't much dependence on RH after that. However, the number of samples below 60% is very small.

We tend to think that the previous approach (using 7-day temperature but instantaneous mixing ratio) makes more sense physically. The goal is to use a "background" temperature that is unperturbed by the wave, and it is assumed that the 7-day temperature represent this temperature. Aside from wave anomalies, the basic state of temperature wouldn't vary much over 7 days (in the tropics), so this assumption is probably adequate. For "background" moisture, ideally we'd be able to get the mixing ratio just before the wave passes through. However, unlike temperature, moisture can vary in short timescales (i.e. due to processes such as convection), so the 7-day average moisture is likely to be biased low compared to the actual background moisture. Because of this, we think that using the time-collocated MLS mixing ratio is a better way to find the relationship between RHib and clouds, although it is by no means ideal. For this reason, we are inclined to keep the previous approach.



Manuscript Changes: At line 305, we added a short discussion on why our RH estimates are high-biased.

P16 line 343: 4-5 km seems larger than the typical wavelength which comes out of your composites Fig. 6

Manuscript changes: We have rewritten this part to be clearer about what we wanted to convey. The new addition is: "The vertical wavelength inferred from the anomalies in our composites is about 3 km. Dzambo et al. (2019) showed that the power spectrum of TTL gravity waves tend to peak at wavelengths of around 4--5 km, though at 3 km there is still considerable power (their Figure 1). These wavelengths are all resolvable by RO, so it can be assumed that this analysis has included a large part of the TTL gravity wave spectrum."

P16 lines 345-348: "Since negative dT0/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

Manuscript Changes: “positive cooling rate” has been removed here.

Wording

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

Manuscript Changes: “favor” changed to “is favorable to”

p7 line 167: ‘vertical cloud fraction’: I would remove vertical.

Manuscript Changes: “vertical” has been removed

P7 line 168: ‘cloud boundaries’ → ‘cloud layer’ ?

Manuscript Changes: “boundaries” changed to “layer”

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

Manuscript Changes: This paragraph has been rewritten in response to Referee 1’s comments, so the word “favor” is no longer there.

P10 line 213 : “a similar compositing technique ...” → “a compositing technique similar to the one employed above “

Manuscript Changes: Changed as suggested

P16 l340 : large wavelengths→large horizontal wavelengths

Manuscript Changes: Changed as suggested

References

Alexander et al., (2001): Gravity waves in the tropical lower stratosphere: A model study of seasonal and interannual variability, JGR-atmospheres.