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## Comment on acp-2022-491

Anonymous Referee #2

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Referee comment on "Aircraft observations of gravity wave activity and turbulence in the tropical tropopause layer: prevalence, influence on cirrus clouds, and comparison with global storm-resolving models" by Rachel Atlas and Christopher S. Bretherton, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-491-RC2>, 2022

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Review of

Aircraft observations of gravity wave activity and turbulence in the tropical tropopause layer: prevalence, influence on cirrus and comparison with global-storm resolving models

by Atlas and Bretherton

### Summary and general comment

In this study wind measurements are used for investigations of gravity waves and turbulence in ice clouds in the tropical tropopause layer. The data from aircraft measurements at different campaigns are separated by several criteria (high/low ice water content, distance to convective activity) in order to determine the impact or relation to ice clouds.

Generally, this is a well written manuscript that constitutes a valuable contribution to ACP. However, I have some concerns on the data evaluation of the aircraft measurements and the use of model data. Therefore I recommend major revision for the manuscript, before it can be accepted. In the following I will explain my concerns in detail.

## **Major points**

### (1) Discrimination between low/high ice water content

In the evaluation of the ice water content a fixed threshold of  $1e-3 \text{ g/m}^3$  is used. The choice for this constant value is not clearly motivated. Since the water vapor mixing ratio as well as the resulting ice water content (IWC) of ice clouds is highly depending on temperature, it is more than questionable if a fixed threshold is meaningful. The authors mention the investigations by Krämer et al. (2016, 2020) for their interpretation. However, in these publications (e.g. Krämer et al. 2020, fig. 6) the high variation is clearly shown together with a formerly derived parameterization for the median of IWC. It can be seen, that the median value varies over the relevant temperature range  $185\text{K} < T < 200\text{K}$ . I suggest to revise the investigation using a variable threshold (e.g. using the median, formulated in Schiller et al., 2008) or even to investigate the sensitivity of the results due to changes in the threshold. It remains still questionable, if the discrimination is meaningful, since the detection limit of some hygrometers is close to these values (as stated in the text).

### (2) Interpretation of low values of ice water content and saturation ratios

In section 2.2 an interpretation of values of IWC and ice crystal concentration (NI) together with the saturation ratio (or relative humidity over ice) is given, mostly arguing that ice clouds with small IWC cannot obtain water vapor sufficiently enough. The argumentation is mostly hand-waving and not really convincing. For a more precise argumentation one should use the growth equation for ice mass concentration. The growth rate is proportional to the number concentration (NI) and to the mean radius of the crystals (i.e.  $\sim m^{(1/3)}$ ). Thus, for small ice crystals and/or low number concentrations the rates remain low and the relaxation to equilibrium (i.e.  $S_i=1$ ) is slow. For such estimations, I would suggest to plot the measurements in a different way, i.e. NI as x-axis and ICW as y-axis. Straight lines through the origin would represent different mean masses. A simple back of the envelope calculations from the reported values gave me ice crystal mean masses of order  $m \sim 4.5e-11 \text{ kg}$ , leading to a mean equivalent sphere radius of  $r \sim 24 \mu\text{m}$ . However, the spread is quite large, so plotting different mean masses would help for interpretation.

The right plot in figure 2 is not convincing for corroborating the interpretation of slow relaxation to saturation for low IWC values, since (a) the median is only slightly enhanced (110% vs. 100%) and (b) for lower values of IWC the median is lower than saturation.

I suggest to carefully reconsider this interpretation also in terms of the investigations of the related vertical velocities. It is well known that a persistent vertical updraft will lead to enhanced steady state values of RHi, so this might also play a role for the interpretation, not just the pure microphysical particle properties together with the thermodynamics. Overall, it is not clear to me if such an interpretation is really relevant for the further investigations of turbulence in ice clouds.

### (3) Threshold for turbulence

As the authors stated in their manuscript, the choice of the threshold is somewhat arbitrary (and not explained well in the text) and the threshold is lower than previously defined ones (as e.g. in Podglajen et al., 2017). Since the whole evaluation relies on this threshold, I would suggest to investigate the sensitivity of the results with respect to variations of this threshold. Otherwise, the robustness of this investigation is not really convincing.

### (4) Use of model data

In section 3.4 4 different models are used for comparison of simulated vertical winds with the measurements. Since the models have very different treatments of convection (parameterisations vs. resolving convective events) and the comparison is really difficult and not convincing, the purpose of this section is not clear to me. Either the authors should explain more in details the models/simulations and what they can learn from this comparison or they should delete this section completely (and adjust the title), since at the moment it does not provide additional insights in the relation between Gw/turbulence and ice clouds. For a better comparison and a meaningful evaluation, ice clouds should also be taken into account. E.g. one could investigate if there is a similar relation between ice clouds close vs. far away from convection in the models.

### **Minor points**

## (1) Units of ice water content

Since the values of IWC in the upper troposphere are quite small, it is more common to use units like ppm, ppmv or mg per m<sup>3</sup>, see also Krämer et al. (2020). I would recommend to change the units for improving the readability.

## (2) Turbulence in ice clouds due to buoyancy effects

There are some studies indicating that latent heat release and/or radiative heating inside cirrus clouds might lead to buoyant instabilities (see, e.g., Dobbie and Jonas, 2001; Marsham and Dobbie, 2005; Spichtinger, 2014). It is not clear if such effects also might play a role in the TTL, if the stratification leads to potentially unstable layers. At least, such a possibility should be mentioned.

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