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Interactive comment on "Small-scale gravity waves in ER-2 MMS/MTP wind and temperature measurements during CRYSTAL-FACE" *by* L. Wang et al.

Anonymous Referee #1

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Overall this in an interesting paper for two reasons: it represents a thorough analysis through statistical techniques of the spectrum of gravity wave motions, as measured from aircraft during the CRYSTAL-FACE mission, and second, it employs some rather nice, sophisticated techniques to do so. As remarked in the paper, analysis of the spectrum of GW properties is of particular interest for those attempting to model convectively induced cirrus near the tropopause. The paper should be acceptable for publication following the revisions outlined below.

Major comments

1. Much of the analysis is centered around identifying statistics for λ_h', an *apparent* wavelength for gravity waves as determined from aircraft. However, as noted on p 11384 l. 5, this is necessarily a rather arbitrary quantity because it is a strong function of the flight angle of the aircraft relative to the GW horizontal propagation direction. To a reader, it would seem to be far more physically meaningful for the results in this paper to be uniformly described in terms of the true GW wavelength λ_h, not λ_h'.

2. While the S-transform is obviously a powerful technique for identifying gravity waves and their properties, it is far from clear that statistically robust techniques have been employed in identifying GW's within aircraft flight transects. For example, in Fig. 4, I can be prepared to be convinced that the longer wavelength signal is real, but it is far less clear that the shorter wavelength signal is present, and not an artifact of the statistical analysis. 138 GW features in the data set were identified. How? There are statistically robust ways of identifying peaks in transform analyses. These should be employed and clearly described.

3. A related issue is the possible presence of sampling bias in the aircraft data. While this is a significant concern in any aircraft campaign, it is perhaps particular so for one that attempts to study gravity waves, as they are dispersive, and their properties will vary with distance from the source. In particular, amplitude decays, so will $\tilde{T}$, and hence the pdf of $\hat{\omega}\tilde{T}$ is necessarily quite dependent on where relative to sources the ER-2 is sampling. The ER-2 is particularly finicky about where it chooses to fly, and doubtless avoids areas where the most vigorous (and high frequency?) GW motions may be present. The authors do include in the title the words CRYSTAL-FACE and ER-2, which implicitly acknowledges this issue, however, because the approach to the data set is statistical, these considerations should be described in greater detail.

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Minor comments

1. p. 11383: The physical meaning of λ'_h needs to be described better. What is meant by "apparent"
2. p. 11383, l. 18. Why is the lower cutoff 5 km. Does this have to do with the choice of wavelet transform?
3. p. 13385 Because it is rather central to the analysis, a specific equation describing how θ is derived, however trivial, would clarify the the description.
4. p. 11386, l. 12 These results could be of rather general interest, including to those not intimately familiar with GW analysis. Accessibility would be favored by including the analytic form of the atmospheric GW dispersion relation, particularly considering the statement on p. 11388, l. 1 "$\hat{\omega}$ was already estimated from the GW dispersion relation as described in the previous subsection."
5. p. 11388, l 16 Perhaps a better word can be chosen than "agreed". While good, 30 still seems a substantial difference.
6. Fig. 8 seems to have three solid red lines not 1. Perhaps the two to the left are supposed to be dotted? In any case, it is not particularly convincing that the back-trajectories point to gravity waves originating from convectively active regions. The whole area looks conectively active, and the red line encompasses a good portion of the region. It's hard to imagine a trajectory pointing to anything but a convectively active region. For this argument to be convincing, the analysis would need to be executed within a region with more isolated convective events.
7. p. 11390, l 25 and elsewhere. Do the authors mean "vertical" momentum flux? What about horizontal momentum?

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8. p. 11392, l. 18. Where the ER-2 was sampling these gravity waves, was the atmospheric humidity anything close to the level where the measured temperature perturbations could be expected to induce cirrus formation? In general, the air would either need to be supersatured with respect to ice, or $\tilde{T}$ particularly large (many degrees).

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