

Interactive comment on “Observations of in-situ generated gravity waves during a stratospheric temperature enhancement (STE) event” by A. J. Gerrard et al.

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We wanted to thank both reviewers for their time and effort spent towards reviewing this paper. They have both made valuable contributions – which we greatly appreciate. In particular, Reviewer #1 found an error in our original manuscript that we have fixed and we are very grateful for his/her contribution!

Original reviewer comments are shown in italics.

Anonymous Referee #1

C10801

While these might really be the first measurements of gravity waves emitted from an STE, I find the paper does not provide strong evidence for this. A simultaneous analysis of wind fields would be informative, and hopefully demonstrate that the polarization relations are fulfilled. It is obvious that the lidar cannot provide more information in this regard, but what about analysis data or a regional simulation? If the authors cannot provide such additional support, they should rather admit the speculative nature of their interpretations.

Most of the analysis data that we have looked at (i.e., NCEP and UKMO) cannot adequately resolve such features. This is now commented on in the text.

Addressing the need to “... admit the speculative nature of their interpretations”, we largely agree that in this draft the text appears to overzealously argue our case without sufficient supporting data. As suggested by the Referee, we have modified the language of the text to reflect this change in tone. For example, in the very first sentence of the abstract “Observations of...” has been changed to “Evidence for...”. The same has been done in the Conclusions.

Also, why should the downward propagating solution be physical? What evidence is there beyond a somewhat indistinct allusion to baroclinic processes above an STE?

This paragraph of the conclusion [and the corresponding sentence in the abstract] has been removed in the revised manuscript.

Nonetheless, we should note that our understanding of the mechanisms of gravity wave generation is still incomplete. It is well known in the literature that downward propagating gravity waves can be generated from a variety of sources, and thus there

is little reason to dismiss such a possibility. Does the reviewer have some specific information showing that downward propagating gravity waves cannot be generated from a baroclinic process? If yes, we'd love to include that material in this paper.

In the analysis of the dispersion relation I am missing some information.

1. What dispersion relation is used (equation)?

This equation is now included in the paper.

2. Has the Coriolis effect been taken into account? Since the ratio between horizontal and vertical wavelengths of the identified waves is large, inertia should be taken into account.

Yes, Coriolis effect has been taken into account.

3. How have the roots of the fourth- order equation been determined?

Numerically using a Laguerre method (included mention in the paper).

4. How have the group velocities been determined, and what results has one obtained in this regard?

This aspect is now discussed in the revised manuscript, and while attempting the revisions; we found an error in our calculation that has now been fixed. We greatly

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thank the reviewer for his/her insightful critique!

5. It is assumed that the waves are aligned with the wind, without strong reasoning. What results would one get if one assumed propagation in the horizontal at angle 30°, 60°, 90°, . . . ?

Justification for alignment is now included in the revised text.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 14221, 2011.

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