Atmos. Chem. Phys. Discuss., 11, C16260–C16267, 2012
www.atmos-chem-phys-discuss.net/11/C16260/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Numerical modeling of lower stratospheric Doppler ducted gravity waves over Jicamarca, Peru" *by* Z. Li et al.

Z. Li et al.

yajnaval@gmail.com

Received and published: 29 March 2012

Author Replies to Anonymous Referee #1

The authors of this paper are very concerned that neither reviewer mentioned Figure 3 (i.e., the 3-figure plot showing a zoom in of the phase structure and spectral analysis) and also requested for material that was already included in the revised paper accepted in ACPD (e.g., discussion of the thermal structure, use of vertical winds, etc.). Reviewer 1 specifically requested to see this exact information again. Was the old manuscript version, i.e., the original version initially submitted to ACPD, sent to the reviewers for review accidentally, instead of the revised version after the ACPD review?

In addition, both reviewers requested very similar pieces of information, specifically the

desire to see m^2 vertical profiles, in an effort to see the potential for monochromatic gravity wave ducting (which we have now included in the revised manuscript). As we attempted to indicate in Figure 3 and the discussion of such, we do not think we are actually seeing a Doppler ducted gravity wave, as the structure of the "wave-fronts" are very unusual.

Assuming that the reviewers did indeed use the revised manuscript, and refer to Figure 3, we can only conclude that our poor writing has caused both reviewers to not understand the intent of the paper. As such, we have gone back and greatly reworded a number paragraphs in the revised manuscript in an effort to better explain our observations and indicate that we think what we are seeing doesn't appear to be a simple ducted feature.

(Reviewer comments are in italics)

Reviewer #1

This paper reports an attempt to model gravity wave features observed over Jicamarca observatory.

This is not really the intent of the paper. Our intent is to 1) show the observations, 2) note the stationary wave wind fields, and 3) show features that appear to be ducted gravity waves, though we are not certain of it. We have attempted to further clarify this in the revised manuscript.

There do appear to be serious issues with the present manuscript, which I've provided some comment on below. I have tried to think carefully of a path towards revisions, but this may require a significant revision and resubmission, perhaps even leading to a somewhat different paper than the present manuscript.

A major concern is that the waves being modeled are of quite different scales from the waves that were observed, such that it is difficult to justify the comparisons. Specifically, the simulated waves are of short period (~ -22 minutes), and the *observed waves had relatively longer periods ∼minutes. In order for ∼minute waves to experience some reflection and trapping between the ground and a stratospheric wind jet, they would need to have quite small horizontal scales. The present paper does not clearly confirm that these waves are ducted. More analysis, and also some investigation of expected reflection conditions, would be needed to determine whether it is reasonable to conclude that they may be ducted waves.*

We have attempted to be more clear with our intent in the revised manuscript.

A revised manuscript could focus on, for example, 1) trying to clearly explain and model the ∼ minute waves apparent in the data. Or, 2) identifying shorter-period ducted waves in the data that may be trapped in the way that the modeling results currently demonstrate. In other words, it would make sense to bring the modeling efforts in line with the observations, or vice-versa. However, either approach would significantly change the manuscript.

Both of these routes are important, and we attempted to go with suggestion #1 in the revised paper. Suggestion #2 was also examined, and no such shorter period wave structures were seen in the data, as noted in the revised manuscript.

There are also some limitations with the data presentation and analysis, such that it is difficult for the reader to fully appreciate the contents of the data (which otherwise appear to be good).

Suggestions to "focus" in on wave scales of interest are listed below, along with

general and specific comments/concerns.

General Comments: The waves identified in the data have ~ -90 minute periods and, at present, it is not clear that they are ducted (the manuscript only makes note of the shape of phase structures seen in Figure 2). Some analysis could be used to determine whether they may be ducted under these conditions (assuming a certain propagation direction), and whether this may occur for typical horizontal and vertical wavelengths allowable in the stratosphere / upper-troposphere. For example, it may be helpful to examine m^2 profiles with altitude, and calculate estimated Doppler-shifted intrinsic periods, to determine theoretically what range of wave scales and periods are expected to be reflected / ducted under the observed conditions.

Waves likely to be ducted within the stratospheric wind jet (or a stratospheric thermal duct) would need to have short periods, similar to MLT-region ducted waves. Longer period waves can become reflected by the wind jet, and become trapped between the jet (at 15 km) and the ground surface (sometimes weakly, with leakage occurring over time). This appears to be the case for the waves simulated for this manuscript (10-20 minutes period). For winds to Doppler-shift the intrinsic frequency of a 90 minute wave sufficiently to produce reflection, the horizontal wavelength would need to be extremely short; so, it may not be reasonable to conclude that they are ducted waves.

If the aim is to explain the relatively-coherent ~ -90 minute waves seen in this dataset, then it would be helpful to show how the waves behave over time, to illustrate clearly their phase progression, vertical wavelengths, amplitudes, and periods. Focusing in on a shorter time period and altitude range (i.e., in Figure 2, which now shows a full 24 hours), and picking out individual wave events to plot, may help to illustrate these waves more clearly (by showing phase progression, illustrating vertical wavelength, and amplitude). Some calculations, such as those described in the previous paragraph, would be needed to understand how the ambient conditions affect the waves' propagation, and to provide guidance for any model simulations.

This is the very intent of Figure 3 in the original ACPD version of the manuscript.

If the aim is to identify possibly-ducted waves at shorter periods, then it may be helpful to look at even shorter spans of time ($\sim$ hour), filtered to show only waves having $<$ minute periods (or even shorter). Since the time resolution of the data is 2 minutes, it should be possible that some short-period signatures would be present. Using radial or vertical winds (e.g., Fritts and Janches, 2008) would provide more insight into these time-scales, as the shortest-period waves will exhibit stronger vertical wind components.

As commented in the revised paper, no such shorter period waves were found.

It is also important to note that the demonstrated form of Doppler ducting (between the ground and a wind jet) appears rather different from that which occurs for very-short period ducted waves and bore features seen in the MLT region. In the MLT, trapping typically occurs due to wind and temperature profiles alone (within a wind jet along the direction of flow, or within a thermal inversion layer), without the influence of a fixed lower boundary (ground). Both cases are interesting, but some caution is needed when making comparisons (last paragraph, page 19021). Shorter-period stratospheric ducted waves (with few minutes periods) would likely exhibit more-comparable characteristics and behavior to MLT ducted waves.

The interesting possibility that some stationary waves may be present in the data is already noted by the authors (page 19015, and figure 2). It may also be that some wave signatures observed here could be associated with quasi-stationary waves (or

secondary processes of quasi-stationary waves), which may complicate the analysis even further. Analyses of such waves could be quite difficult given the nature of the measurements and uncertainty of exact propagation direction. But maybe a consideration for future study?

The authors agree with the reviewer comments above.

Specific Comments: 1) If possible, it maybe worthwhile to investigate vertical or radial winds. Shorter intrinsic period waves will have larger amplitudes in the vertical velocity component, and contamination from the background winds and larger-scale waves (having stronger horizontal motion) would be reduced dramatically.

This was already commented on in the original ACPD version of the paper.

2) If seeking to study short-period ducted waves, it would be worthwhile to use an MSISE90 temperature background instead, since stratospheric structure tends to produce robust thermal ducts. I understand that the authors' goal is to study waves trapped by winds, however in the stratosphere (and upper-troposphere) the thermal variation can contribute significantly to reflection/trapping. The winds shown in the data and model are of modest strength, so thermal effects should indeed be very important.

The reason for not using a temperature structure was already commented on in the original ACPD version of the paper. We have tried to clear up this discussion in the revisions.

3) I am confused by the description of the model parameters: "The horizontal, vertical, and temporal scales of the disturbance were 10 km, 2 km, and 5.86 min (approximately the Brunt-Vaisala frequency), respectively. The initial amplitude disturbance was equivalent to a 1.2 cm/s vertical wind, and the horizontal wavelength and period were initially set to 20 km and 10 min, respectively." Does this mean that the Gaussian envelope has half-widths of 5.86 minutes, 10 km in the horizontal and 2 km in the vertical? These would lead to an isolated and impulsive source. Were these parameters changed when longer-period runs were performed? The source also appears to be positioned rather close to the ground $(\sim kmabovedomainboundary)$.

*These values of "10 km, 2 km, and 5.86 min" are RMS values (i.e., a 1-sigma standard deviation) values of the Gaussian packet as seen in the equation. Thus the FWHM of the Gaussian envelop is = 2.35*RMS. Because this is only half-max, there can be multiple oscillations of the source feature.*

As implied by the referee, we have used the phrase "RMS" much more prevalently throughout the revised manuscript.

4) As noted by the authors, the direction of wave propagation relative to the winds is not known, so it will remain unclear what exact effect the winds may be having on any observed wave. This significant uncertainty should be noted carefully when

discussing individual wave features of the data, since the effects of winds will be determined by the relative propagation directions of the wave.

We agree with the reviewer and the manuscript has been revised accordingly.

5) It is said that parameters of the source and jet were varied for a number of test runs. I'd caution that the 1km jet test likely was too small relative to vertical grid resolution, and may contribute to non-physical reflections from the wind jet.

As explained earlier in this response, the 1-km vertical resolution was RMS width, not FWHM width.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 19011, 2011.