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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Long-live High Frequency Gravity Wavesin Atmospheric Boundary Layer: Observations and Simulations" *by* Mingjiao Jia et al.

**Anonymous Referee #1**

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**Summary:**

This manuscript presents observations of persistent gravity wave activity in a stable atmospheric boundary taken as part of a field experiment over Anqinq, China. Alongside the observations, a series of CFD simulations are conducted in order to understand the likely mechanisms for generation of these gravity waves. The results are not particularly novel – gravity waves have been observed in previous studies of the ABL and shear is known to be an important mechanism for generation of gravity waves. Having said that, the observations make a nice case study, bringing together Doppler lidar observations with some radiosonde data to give an unusually detailed set of observations



of such waves.

**Major comments:**

1) The general motivation for the work is perhaps a little misleading. The introduction talks about the breaking of gravity waves at critical layers at altitude and the role of high frequency waves in momentum transport into the upper troposphere. The observations presented are of vertically coherent waves which look much more like horizontally propagating trapped wave modes to me, and hence it is not clear the relevance of either of these? Such horizontal modes might well be important and linked to low level turbulence (e.g. the role of trapped waves in generation of rotors as in the T-Rex experiment, Grubisic et al, 2008) and they have also been linked to the initiation of convection (e.g. Lac et al, 2002; Marsham and Parker, 2006; Birch et al, 2012). You might also mention low level wave drag (e.g. Lapworth and Osborne, 2016; Tsiringakis et al, 2017). The introduction also suggests (page 1, line 29) that waves are only generated in the troposphere, which is not true.

It is also not clear what the specific motivation for this work is. How might this work help to i) deepen our understanding of the mechanism leading to the generation and propagation of gravity waves in the boundary layer and/or ii) improve our capability to model these waves or parametrise their effects? How might the results be useful to researchers in other regions of the world? You probably want to return to these questions in the conclusions and discussion.

2) It would be useful to the reader, and increase the impact of your work, if you can highlight the novelty of this work given that various other studies have observed and modelled gravity waves in the boundary layer, including many of the papers referenced in the introduction.

3) Table 1 gives the spatial resolution. I assume this is the along-beam range gate size? It would be useful to clarify this. There are two additional factors to consider. Firstly, the beam spread angle will alter the effective horizontal resolution of each sample

(increasing with height). Perhaps more importantly the Doppler wind retrieval requires multiple scans (1 vertical and at least 2 at an angle – here 30°). This means the wind retrieval is over a cone with a much larger width than the 60m resolution given. At 2km the radius of this cone would be 1150m. This might be a significant fraction of the wavelength of a short, high frequency wave. These issues should at least be discussed.

4) Bottom of page 4. These jets are important as the shear associated with them is hypothesised to lead to the gravity waves. It is not clear what the cause of these jets is though. The big changes in wind direction are unusual. Can you offer any explanation? It is possible that the upper jet is some sort of nocturnal low level jet as it seems to strengthen during the night and weaken a little around dawn, but there is not much evidence of wind turning with time as one might expect to see if this was an inertial oscillation. Have you tried looking at hodographs at a specific height from the lidar winds to see evidence of this? Are there other explanations, e.g. the height of the surrounding topography leading to channelling in the valley?

5) Page 6, line 2. It seems likely that this aerosol concentration profile is associated with a stably stratified boundary layer, but I'm not sure that you can definitively say this from the lidar observations since you have no direct measure of temperature or stability.

6) Page 7, figure 5. It would be useful to include a plot of the Scorer parameter here in addition to $N^2$ since it is hypothesised that wind shear is important.

7) Page 7, lines 17-18. I am not sure you can say anything about the presence of absence of waves above 1800-2000m. The lidar barely extends above 2000m between 0400-0900 and the CNR is quite low, with noisy $w$ fields. I would reword this conclusion.

8) Page 8, Figure 6. If you are plotting W1 and W2 in the text, then you need to at least briefly explain them here. The main text should be understandable without reading the appendix. The appendix is for additional details. At the moment the figure



is meaningless without knowing what it means having only W1 or only W2. (Actually this is still not very clear, even after reading the appendix – see below).

9) Page 8, Figure 6. Also, are these plots are for the whole field campaign or just the case study on 4th - 5th Sept. What height did you go up to? How was this chosen?

10) Page 9, lines 15-16. This is a very bold claim! Both the terms "mesoscale models" and "CFD models" can cover a wide range of different things. Ultimately both are solving the same equations. Which is better will depend on the details of individual models and their numerics, and the setup of individual simulations (e.g. resolution, turbulence scheme etc. . ..). CFD models traditionally do not include many atmospheric processes, although the distinction is increasingly blurred. All your advantages could equally apply to a high resolution atmospheric model. I would just remove this sentence altogether.

11) Page 9, section 4.1. So which CFD model are you actually using? Is this a commercial code? Include a reference to the actual model and its validation if at all possible.

12) Page 10, lines 13-14. With the first cell height at 5m then you are not resolving the roughness sub-layer at all, and so you need to apply some sort of wall function / Monin-Obukov similarity function rather than just the no-slip boundary condition. Are you doing this? If so, what?

13) Presumably there is a prognostic equation for potential temperature or similar, in order to include stability effects? No mention of this. What boundary conditions are used for this variable?

14) Page 11, line 9. What do you mean by “symmetric condition” at the upper interface? Which variables does this apply to?

15) Page 11, lines 17-18. I don't understand the comment that the flow solution is initiated as a steady state in all cases except case 7 and 8. How can you initiate at a steady state? Do you mean you initialise with the merged wind profile everywhere? In

general this will not be an exact solution to the model equations, even in 1-d, so this isn't a steady state. Even if it was a solution in 1-d, the inclusion of topography means there will be variations across the domain which you cannot know without solving first. I assume the additional complication for cases 7 and 8 is that the addition of a constant velocity everywhere would break the no-slip boundary condition. How do you deal with that? Is the velocity near the surface reduced to produce a consistent initialisation / inlet profile? If so, how?

16) Page 11, table 2. You don't actually define $u_0$. I assume this is the merged wind profile (lines 4-6), in which case define it there. It would be useful to have a plot of this model wind profile, and also the potential temperature profile.

17) The simulations presented are all 2-D, however the analysis elsewhere suggests that topography might be constraining the low level flow. Why choose to conduct 2-D simulations? What is the impact of this? This choice needs to be justified, and the limitations discussed.

18) Page 13, lines 15-16. If these waves were topographically generated, one would expect stationary waves rather than the propagating waves seen here. You have only shown time-height plots. If you look at vertical cross sections is there evidence of stationary topographically generated gravity waves at all?

19) While there are a relatively comprehensive set of simulations presented here, there is relatively little attempt to explain physically why the differences between the simulations occur. It might be fruitful to look at the Scorer parameter for different wind profiles. Does this explain the differences in wave trapping observed for example? One factor which is not investigated at all is the role of stability. Without stability there would be no waves at all, and the presence of these trapped horizontal wave modes must be at least partly due to the low level inversion. How would altering this affect the results?

20) Page 13, line 22. It is rather unusual to see negative values of $N$ at 2km. This implies an unstable atmosphere at this height. Do you have any idea what is driving



this? I note that these negative values are fairly small, and only over a narrow height range (Figure 5b). It is interesting to note that the waves appear to reach well above the stable boundary layer in this case (perhaps 200-300m depth), even though there are several near-neutral levels below 2km. Can you explain this? Plotting temperature profiles (rather than potential temperature) and the choice of scales for the $N^2$ plot makes this difficult to judge though.

21) Appendix B. From what is written I cannot see why you need to choose and compare two separate wave frequencies W1 and W2. From figure B1 I am guessing that W1 is the most significant peak with LS and W2 the most significant peak with the Morlet wavelet? The text does not explicitly say this. Similar, I am assuming from the figure that the wave is identified as the most significant peak, although this is not explicitly stated. Only when similar waves are identified by both methods is the case treat as a GW. Why do you use this criterion? This whole section could be better explained.

22) Page 22, line 1. Do you have any evidence of false detection? Are any of the wave signals unrealistic? There doesn't seem much evidence to confirm or deny this at the moment so it is rather speculative.

**References**

Birch et al (2013) Impact of soil moisture and convectively generated waves on the initiation of a West African mesoscale convective system. QJRMS 139 1712-1730. Grubisic et al (2008) THE TERRAIN-INDUCED ROTOR EXPERIMENT: A Field Campaign Overview Including Observational Highlights. BAMS 89 1513–1534. Lac, Lafore and Redelsperger (2002) Role of Gravity Waves in Triggering Deep Convection during TOGA COARE. JAS 59 1293–1316. Lapworth and Osborne (2016) Evidence for gravity wave drag in the boundary layer of a numerical forecast model: A comparison with observations. QJRMS 142 3257–3264. Marsham and Parker (2006) Secondary initiation of multiple bands of cumulonimbus over southern Britain. II: Dynamics of secondary initiation. QJRMS 132 1053-1072. Tsiringakis, Steenveld, Holtsag (2017)

Small-scale orographic gravity wave drag in stable boundary layers and its impact on synoptic systems and near-surface meteorology. QJRMS 143 1504–1516.

**Minor comments:**

Title. "Long-lived High Frequency Gravity Waves in the Atmospheric Boundary Layer" would be better English.

Page 4, line 4. "Wind field"

Page 4, line 20. "4-dimensions"

Page 6, Fig 3 caption. Should this be "Cone-of-Influence"?

Page 7, line 15. Units should not be in italics.

Page 9, line 2. The word "quadrature" is not really appropriate here. I would say "is perpendicular to the corresponding wind rose". This occurs at other places in the text too.

Page 9, line 25. "simulate the wind field".

Page 12, figure 9 caption. "wave motions are not only exist in the vertical wind". "no wave motions are generated". "cases 2-4"

Page 15, line 18. "... perturbations of the GW were 90° out of phase with vertical perturbations..."

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