Review of revised version of acp-2019-256. "Long-live High Frequency Gravity Waves in Atmospheric Boundary Layer: Observations and Simulations" by Jia et al.

Summary: This revised version of the manuscript is a definite improvement and has addressed the majority of the reviewers' comments from last time. I think removing the wavelet analysis is probably wise and leads to a more focussed paper. I have a couple of minor comments still remaining.

Minor comments:

1) The written English throughout would benefit from some thorough proof reading by a native English speaker. Although I think the meaning is generally clear, I had to re-read a number of sentences to make sure I had the meaning right since they were oddly phrased. I haven't commented on these in detail since this is an editorial matter.

2) Reviewer 1, major comment 13. The amended text here is definitely clearer, however I am still not completely sure what is done. For instance, the comment that the thermal field is assumed to be uniform in a horizontal plane cannot be true. I think you mean that the reference temperature field is uniform in a horizontal plane. It is also not entirely clear whether T_{ref} and ρ_{ref} are constant, or functions just of height? As it stands the formulation does not seem entirely consistent. Normally in the Boussinesq approximation it is assumed that is ρ_{ref} constant and that fluctuations in ρ are neglected except in the buoyancy term. T_{ref} (or more usually potential temperature θ_{ref} need not be constant, but is a function of height, with the buoyancy term written in terms of $T-T_{ref}$ or $\theta-\theta_{ref}$. If variations of ρ_{ref} with height are included then this is typically the anelastic approximation.

3) Reviewer 1, major comment 13. The response does not discuss boundary conditions for T at all. Is it a fixed surface temperature or are there any imposed fluxes?

4) Reviewer 1, major comment 15. Maybe this is a misunderstanding due to language, but I don't see how you can have a steady state solution at t = 0 if you are initialising the model with zero velocity everywhere. There must be some spin up time for the mean flow and turbulence surely?

5) Reviewer 1, major comment 17. I don't think this is a very satisfactory response. Sure, 2-d simulations can be useful as an idealisation to study processes. Whether they are a good model for the real world depends a lot on the particular topography and how two-dimensional it is. As such, the references may be misleading since they are for different locations. There are plenty of idealised 3-d studies which show differences from idealised 2-d studies. My point is that if you are suggesting elsewhere that topography is constraining the flow in this case (a 3-d effect), then you should at least discuss the possible impact of only modelling 2-d flow for this site. How 3-d is it? Why did you choose the transect you did? How representative is this?

6) Fig 5, caption " between 22:00"