Response to Referee #1

“Orographic and convective gravity waves above the Alps and Andes mountains during GPS radio occultation events — a case study” by Rodrigo Hierro et al.

We acknowledge very much the detailed comments and suggestions made by Referee 1.

General Comment

In my AMTD access review several major concerns were raised which showed that a major rewriting and refocusing of the manuscript would be required, probably exceeding the time usually allotted for revisions of a paper under review. Therefore my recommendation at this stage was to reject the paper. Since then, only minor changes were made. This is why this recommendation is repeated. Of course, having a collocation database between RO events and convection over orographic regions is a valuable contribution, same as the detailed discussion of two cases of WRF simulations. The comparison of RO events and WRF simulations, however, which is the core part of the paper, is incomplete and based on wrong assumptions. Still, I think that the two selected cases are good examples for discussing several important aspects that have to be considered when comparing high resolution simulations with real observations. Therefore resubmission of a revised manuscript is encouraged. If the manuscript were to be revised and resubmitted, several major issues have to be addressed.

Major Concerns

(I) Firstly, it is obvious that for both detailed comparisons, the Alps and Andes cases, the waves simulated with WRF are not seen in the corresponding RO soundings. Following Alexander et al. (2008), the amplitude attenuation factor E can be calculated.

For the Alps case, E is $<10^{-11}$

For the Andes case, E is <0.03

Amplitudes in the RO soundings are 2K, sometimes more. For explaining these amplitudes with the WRF simulations, simulated amplitudes would have to be >60K. This is physically not reasonable. In the Andes case the tangent points do not even hit the region of simulated wave trains. Further, the simulated wave trains are quite fuzzy, suggesting that amplitude attenuation should be even stronger. Details on the calculation of E are given below in a separate section.

Our revised manuscript, as it may be appreciated in the attached, lengthy new version “with track changes”, has been completely rewritten. Old figures 4, 5, 8 and 9 were eliminated and replaced by the new figures 2, 5, 7, 8, 9 (new version). Old Figure 2 was enlarged in two plates (new figure 3). Many additional paragraphs are now included. The discussion regarding the expected wavelengths distortion was separately explained in an Appendix. New calculations and simulations regarding the 2 case studies, now including simultaneously WRF simulations, reanalysis data and RO measurements are included.

In particular, in base of this last reviewer’s comment, a detailed calculation of the attenuation factor has been made. This factor has been obtained for the two case studies, based on the mathematical details provided in our paper Alexander et al. (2008). We analyzed the possibility of RO detection of 4 clearly different structures, at tropospheric and stratospheric altitudes, during both case studies. These were not completely reported in our previous version. The results are shown in Table 1, now included. As it may be appreciated, 3 structures are expected to be, at least partially, detected. This is not possible for the fourth structure. For example, in the Andes case study, we found that different segments of the same profile exhibit GW belonging to different sources.

Please notice that the new simulations and our new interpretations of the corresponding GW lead to different attenuation values, as aspect ratios and angles between LOS and GW fronts have now changed.

(II) This clearly shows that the WRF simulations alone are not sufficient to explain the observations. In some cases they may be sufficient, but generally they are not. The simplest explanation is that horizontal wavelengths of the observed waves are usually much longer than those of the simulated waves. This possibility and its consequences are not discussed and completely ignored. The wavelet analysis even stops at 120km, which is at the verge of the range of waves that can be seen by GPS RO. One explanation could be mountain waves and convective waves of longer scales that are not captured by the WRF simulations, but co-exist with the simulated short scale wave modes.

The analysis now includes ERA Interim reanalyses data and an extensive discussion is given about the need to consider this information to perform a reliable study of the observed GW structures, mainly for the Alps case study (Sec 3.1.1.to 3.1.3. and 3.2.1.) The wavelet analysis is now applied only to the RO δT profiles at both regions (3.1.2. and 3.2.1.)

Another explanation could be gravity waves emitted from jets and fronts. Surprisingly, this third major source of gravity waves has been completely disregarded in the manuscript. Just as an example, in the beginning of the abstract only orography and convection are listed as major wave sources. Usually gravity waves emitted from jets and fronts are of larger scale and could co-exist with the small scale waves that occur in the WRF simulations. Since the Alps case is in the winter season when gravity waves emitted from jets and fronts are quite abundant, ignoring this wave source is not possible.

We completely agree with this comment. Here, we carefully selected our case studies, taking into account the convenience of considering only scenarios in the absence of jets and fronts. In doing so, we would expectedly filter out GW from at least one or two of these major sources, thus avoiding an extremely intricate analysis, perhaps not possible at all.

(III) Thirdly, since the WRF simulations are insufficient to describe the observed waves, meteorological data sets, such as ECMWF analyses, could be investigated whether larger scale wave patterns are found that can explain the RO observations. GPS RO measures temperatures. Therefore analysis of temperature fluctuations is preferable to the analysis of vertical wind w', also in the WRF simulations. Focusing too much on w' will bias any analysis towards high-frequency waves that are difficult for GPS RO to observe. Moreover, a direct comparison of amplitudes is not possible.

In the new version, in addition to the included reanalyses data, we restricted ourselves to show vertical velocity simulations only in relation with mountain waves. The inclusion of δT from simulations and reanalysis data is now considered and shown in all the new figures and results.

(IV) Fourthly, including a detailed discussion of vertical wavelength biases in the manuscript will distract the readers. For the examples given, observed and simulated waves are obviously different and wavelengths extracted from the simulations cannot be assumed for the observations. Therefore it is not clear whether these sampling biases apply.

Two of our main changes in the manuscript are the inclusion of the expected amplitude attenuation and wavelength distortion during the collocated RO measured profiles. This last discussion is now moved to the Appendix. We feel that now it will not distract from the main objectives of the manuscript.