Response to Referee #2

"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 2.

Summary: This paper analyzed the collocated GPS radio occultation profiles near the convective systems identified from ISCCP over two orographic regions of Alps and Andes. Out of a total of 10 collocated RO profiles, one RO sounding (bending angle and temperature profiles) from each region was analyzed. The convective cloud top height was identified. Gravity wave (GW) analysis over both selected regions were also carried out. The gravity wave signature from the two case studies were derived base on the WRF simulation and the RO vertical profiles. The GWs with horizontal wavelengths of 20 km and 40 km (two mode) and vertical wavelengths (15 km or 20-25 km) were identified from the WRF simulation over the two regions. The vertical propagating GWs with "apparent" vertical wavelength of ~4 km over Alps, or ~4.5 km over Andes were also identified from GPS RO soundings. The so-called distortion factor was also investigated, which measures the discrepancy between the "apparent" (or measured) and the "real" vertical wavelength affected by the RO observation geometry reference to the GWs propagation direction. Overall, the paper use the case study to demonstrate the GPS RO capable of detecting the vertically transporting GW over the orographic regions, where mountain waves persistent as seen in the WRF simulations. The high vertical resolution GPS RO sounding has the advantage of detecting the fine vertical scale GWs but less capable of identify the GW with fine horizontal wavelength (e.g., Wu et al., 2006). The paper seems trying to synergize the GPS RO observation with the WRF simulation to present a more complete picture of the GW morphology, which has the merit for publication. However, it is not clearly stated in both the introduction and conclusion of the paper. I would recommend publication of the paper after "major revision" with my comments below:

Major comments:

(1) The paper writing need some significant improvement to better describe the research work, including the grammar and sentence structures.

a. Many super long sentences should be split into shorter sentences.
b. The author(s) intend to use the "first" person throughout the paper. Generally the scientific paper should be written with third person.

Our revised manuscript, as it may be appreciated in the attached, lengthy new version "with track changes", has been completely rewritten. Along these track changes, the general reduction of sentences as well as the use of third person may be appreciated. The grammar was revised by an expert. 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 (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, reanalyses data and RO measurements are included.

(2) The motivation and key contribution of this paper need to clearly stated in the introduction. The authors add a short paragraph (L102-108) in the introduction, which was the details of work of the paper, but not "Why" to carry out such work. The motivation should be the science or technical questions that haven't been addressed, or what is new in this paper that advances the field of study.

We now feel that all of these comments and suggestions have been fulfilled in our new version (please see Sec 1).

(3) Only 5 collocated cases from each regions (Alps and Andes) were identified between GPS RO and ISCCP. And only one RO case for each region were used for the case studies. It is hard to believe that one single RO case is representative of such a large region, especially hard to believe the single cloud-top-height identified from the bending/temperature anomaly method will be representative over such a large study domain.
a. The author's response shows 294 collocations over Alps but only 50 collocation over Andes within 3-hr and 100 km of the ISCCP convective systems, which should be included in the manuscript. The selection of the 10 case need to be justified.
b. Should consider demonstrate these RO soundings in statistical sense that they can detect consistently the gravity waves with vertical wavelength of 4-4.5 km, instead of randomly pick one out of 5 cases.

As we now explain in the text (line 186-7): "In the present study, a GW climatology from the available limited number of collocated cases is not intended". The response to these two comments (a. and b.), as well as the pre-selection and later selection of case studies are detailed and discussed in lines 187-197.

c. Also ISCCP website shows the DX (B3) data are available 07/01/1983 - 12/31/2009.

This point is now explained in several segments of text between lines 140-171

d. More robust analysis of the GW from more collocated GPS RO soundings near convective systems will strengthen the selection of the "representative" case for further WRF simulation study and improve the paper quality.

Our final objective in this paper was only a detailed analysis of two cases study, however we fully agree with this comment.

(4) Some technical details are missing and need to be added,
a. What exact parameters from ISCCP were used?

The parameters extracted from ISCCP data, are: time of occurrence, center (mass center) and radius of the storm. The COSMIC mission started in June 2006.

b. How ISCCP identify the convective system? Any uncertainty related to the usage of infrared CTT to identify the convective system, especially over cold surface?

A discussion detailing all the information available for us regarding the used cloud data is provided in Sec. 2. We used time of occurrence, the center of the storm (mass center) and radius of the storm. We don't have access to ISCCP methodology so we just used their database.

c. How to detect the CTH from the anomaly method (Biondi et al, 2012)? What threshold etc. used to identify the CTH in Fig. 1a,b? Can't just cite the paper. The basic details are still needed to be included in the paper.

Here we are just able to answer in a similar way as before, regarding ISCCP. We don't have access to the methodology used in Biondi et al. (2012). On the other hand, in the mentioned paper, details of the procedure used by the authors to detect CTH, are available.

d. Discussion on the result out of CWT is missing for both Fig. 5 and Fig. 9. What exactly plotted (e.g., parameters) need to be detailed described in the manuscript, and should be after the discussion of Fig. 4. The author(s) sometimes jump the discussion without following the orders of the figures.

We agree with this observation. The original text has been fully rewritten and we feel that a new order is given to the description of parameters, figures and hypotheses. Previous (some of them modified) and new results may be now appreciated. In particular, the CWT analysis is now limited only to both GPS RO δT profiles.

e. Section 2: missing information of the duration of RO data used in this paper, e.g., what years data were used, are they same for different RO missions?

This information is now included in the text.

f. L156: need description of the reference climatology profile, e.g., how many year average, horizontal, vertical sampling interval, how to do average etc.

g. L159: WRF model description needed, e.g., version, horizontal, vertical resolution, citation.

Please see lines 172-186

(5) Figures need to be improved:
a. Fig. 1: The four cases in smaller inlet are hardly legible.
i. Might consider combine all five (or more cases) into one statistical plot plus the one individual plot in the middle panel.
ii. RO profiles from which RO mission should be indicated in plot or the manuscript.
b. Fig. 8&9 should be consistent with Fig. 4&5, respectively
i. Why Fig. 8 only has three UTC time but Fig. 4 shows four UTC time?
ii. Why adding temperature wavelet analysis result in Fig. 9d but not in Fig. 5?

As mentioned above, old figures 4, 5, 8 and 9 were eliminated and replaced by the new figures 2, 5, 7, 8, 9. The plates in the new figure 3 are enlarged. We feel that the results are more clear and consistent now.

c. Fig. 10:
i. The distortion factor could explain the discrepancy of the GW vertical wavelength seen in GPS RO (4 – 4.5 km, L322,) as compared to the WRF simulation (~15 km, L288, ~20-25km, L421). But why the two "green circle" at those certain locations were not discussed. Will the plot different for other region, e.g., over Alps?

Now, part of the distortion discussion was moved to an appendix. Other reviewer suggested to not distract the reader with these considerations too much. Nevertheless, the two geen circles, suggesting possible under- and/or overestimations for a given set of parameters is included for illustration only.

(6) Most of the figure captions did not describe what is plotted and need to be updated.
a. The reader should be able to understand the figure without the need to consult the text. It might be worth consulting the paper by de la Torre A. et al. (2006) or others on caption writing.
b. All the color bars do not have "UNITs" either on plot or in the caption.

All of these points have been carefully taken into account in the preparation of the new version. We hope that the descriptions are more clear now.

c. Fig. 3: There are no description on what is plotted in Figure 3, variance??. You can't simply say it is "GW" structure.

The GW perturbation to the vertical velocity is explicitly written in the caption and in the text.

d. Same for Fig. 5, 6c, 7,8, 9c. The details in each panel need to be clearly described in caption succinctly.

Here we apply the same comment as the two before.

Technical comments:
L40: "Vertical profiles … (Kursinski and Gebhardt, 2014). Need to be rewritten to be parallel statement.
Done.

L42: "0.1-0.3 g kg-1 "
Done.

L48: "troposphere and lower stratosphere
Done.

L72: "storms"

Done.

L74: "strongly affects"
Done.

L83: "Fovel et al., …, generating high-frequency???" Sentence is not complete.
Done.

L102 -108: Not a motivation, need to be rewritten.
Done.

L109: "Section 2…"
Done.

L125: RO data duration should be added
Done.

L130: Figure 1 caption should include what is plotted. E.g., the elevation map, Alps over Europe and Andes in South America…
Done.

L143: According
Done.

L146: "interval" à "difference"
Done.

L156: Missing description of the reference climatology profile, e.g., how many year average, horizontal, vertical sampling interval, how to do average etc.
We used the data from the climatology, as provided by the Wegener Center. Nevertheless, some details regarding how it was obtained were not available for us.

L172: The total number of collocations (e.g, 294 over Alps, 50 over Andes) should be mentioned, and better to show the statistical results of the analysis instead of the handpick of the 5 cases. Or justification of the representativeness of the selected cases are required.
An detailed explanation about this comment is included in the text now.

L180: Fig.1 need to be updated. Hardly legible.
Done.

L183: Caption should describe/mention each panel. Very hard to understand and need to be rewritten.
This was considered in all the new and old figures.

L195: 3.1. Case study over the Alps region
Done.

L205: What is plotted in Fig. 3 need to be clearly stated.
Done.

L240: They reach again… after a partial… à They reach large amplitudes again in stratosphere after … tropopause at around 11km.
New figures, corresponding captions and discussions in the text are now included.

L247: in red for altitude reference. L250: (CWT) corresponding to Figs. 4a, b, and d, …, Why missing Fig. 4c??
This figure is now removed.

L311: "perfect" band-pass, What does that mean?
A comment is included at lines 284-285.

L297: Fig. 6c caption should describe what is plotted, basically what is the results after CWT, plus the unit of the color bar.
A text is now included in the figure caption.

L313: "This method has two steps: …, …, and to force a zero mean." Super long sentence and need to be broken up into smaller pieces.
Done.

L316: We keep in mind that à Note that.
Done.

L328: prevent us to (from) observing
Done.

L371: 3.2. Case study over the Andes region
Done.

L377: What does "[7;-3]" mean? It was shown in many places.
The horizontal mean wind components, now explained in the text.

L390: caption need to describe what is plotted, plus the unit.
Figure now removed.

L414: Missing detailed description and discussion of Fig. 9.
This discussion was moved to an Appendix and partially referred to a very recent accepted paper, following a suggestion of another reviewer.

L415: Fig. 8, Zonal variation of w in WRF simulation at ??altitude in three UTC times (13, 17, and 21 UTC) over the Andes region. The GPS RO took place at 16:56 UTC with mean LTP at ….
This figure is now removed**.**

L440: Similar to Fig. 6, but for the RO case study from Fig. 1b over the Andes region.
Done.

L455: Equation (1): should cot(a) be cos(a) instead?
It is correct as it is written (please see A18).

L465: please describe why the two green circles at the specific "propagation angle".
Because the α value represent the inclination of the LTP respect to the horizontal plane.

L474: were → where
Done.