Response to Anonymous Referee #1

General comments

The paper analyzes the impact of the Taal volcanic eruption 2020 on the atmospheric thermal structure by using the new COSMIC-2 radio occultation dataset. The authors collocated the OMI SO2 with COSMIC-2 RO and NCEP wind, and analyzed the atmospheric structure variations due to the presence of the volcanic cloud. Particular attention is given to the temperature profiles collocated with the cloud. The topic has already been faced in the past by other papers for different volcanic eruptions (e.g. Wang et al., 2009; Okasaki and Heki, 2012; Biondi et al., 2017).

Reply: First of all, we wish to thank the reviewer for going through the manuscript carefully, providing the constructive comments for our paper and offering potential solutions to improve the manuscript content further. We have revised the manuscript while considering both the reviewers' comments/suggestions

My main concerns are: - the aim of the paper is not clear, the introduction of the manuscript completely focuses on the impact of large eruptions on climate while the study of Taal eruption is not (and can't be at the moment) a climatological study. It is too early for doing climatological studies and the impact of relatively small eruption on the atmospheric structure has already been done in the past with a similar approach (e.g. Wang et al., 2009; Okasaki and Heki, 2012; Biondi et al., 2017).

Reply: Yes, we agree with the reviewer that relatively small eruption on the atmospheric structure has already been done in the past with a similar approach. However, the temperature responses were different for different volcanic eruptions. Depending on the location, season, and magnitude of the eruption, the climatic response can be very different. Thus, the present study gives some insights about the temperature response to the recent Taal volcanic eruption.

In the introduction section, first we started with the importance of volcanic eruptions for the climate perspective. Then, we addressed the suitability of GNSS RO data to study the temperature variations during the volcanic eruptions with the existing few studies as a reference. Finally, we described the usefulness and strength of the recently launched COSMIC-2 RO data to characterize the temperature structure at diurnal scale during the volcanic eruption.

Our main aim of the present measurement report is to establish and provide the importance, capability of COSMIC-2 RO data to further study the diurnal scale variability of atmospheric temperature and relative humidity during a volcanic eruption. For this purpose, we have selected the recent January 2020 Taal volcanic eruption as a case study. We have modified the introduction section in the revised manuscript as suggested by the reviewer.

- the "background mean temperature" and the "reference mean bending angle" are computed in a very limited temporal period (one week just before the event) and this does not provide a strong and sufficient reference since the number of profiles is too small and anything could happen on that short period deviating from the real climatology of the area. This background does not correspond to the climatology which could provide a solid information. As shown in past studies, the anomaly (of temperature and bending angle) can strongly depend on the chosen background. For this reasons the results of the paper can't be evaluated since the background assumptions are not strong enough and significant. I'm sorry to suggest the rejection of the paper, more details hereafter.

Reply: We have considered 13 years of COSMIC-1 RO data (2007-2019) as a background climatology in the revised manuscript. The entire results are compared with the new background climatology instead of one week before the eruption as used in the earlier version of the manuscript.

Specific comments

Why the authors did not also use the RO profiles from other missions (e.g. Metop, Grace, TerrasarX . . .)? in this way they could collect a higher number of profiles and make the analysis stronger. Lines 200-202 . . . "within ±5 ◦ latitude and longitude radius" means a box 10◦x10◦ , but in the conclusion the authors report (line 391) that they worked in a 5◦x5◦ box around the volcano. Please clarify, was the box aound the volcano 5◦ or 10◦?

Reply: Our main purpose of the present measurement report is to see the strength and usefulness of recently launched COSMIC-2 RO measurements during a volcanic eruption period. For this we have been using only COSMIC-2 RO data in the present study. For comparing the results with background, we also utilized 13 years of COSMIC-1 RO data in the present study.

Sorry for the confusion. We have considered the $\pm 5^{\circ}$ radius around the Taal volcano. That means in all the directions, the distance from the Taal volcanic center will be 5 degrees. In the revised manuscript we corrected this as suggested.

Lines 248-254 . . . the authors claim that they computed the cloud top altitude by using the bending angle anomaly developed in Biondi et al. 2017 and Cigala et al., 2019. However, according to this two papers, the bending angle anomaly refers to a solid climatology computed with 12 and 17 years of data respectively, over a large area which ensures a robust average that is representative of large-scale background field resolution. In this paper, the anomaly is computed versus a small number of profiles in a specific temporal period: I have personally downloaded the RO profiles in the same period/area (10°x10° box used by the authors as reference in the period 5-11 January 2020) and collected exactly 100 profiles. This is not sufficient for creating a solid background. Since we have at the moment 20 years of RO availability, I suggest to re-compute the climatology making the study stronger. The same applies to the temperature anomalies. Moreover, the cloud top altitude can be computed with this technique for punctual profiles collocated with the clouds and can't be an average value of random profiles. The shape of the anomaly in Figure 5, looks more like a wave than a peak highlighting a cloud top.

Reply: The authors agree with the reviewer comments. It is reported that the discrimination between volcanic ash clouds and convective (water) clouds from RO is not possible, since the cloud top cooling is common to all convective processes (Biondi et al., 2012, 2013, 2015). As our main purpose of the study is to see the atmospheric temperature and relative humidity changes on a diurnal scale during a volcanic eruption period, the cloud top detection somewhat deviates our study foci. To avoid deviating from the main purpose, we totally removed this section from the revised manuscript.

Section 3.4 . . . Please note: Humidity above 10 km is very likely completely coming from the model.

Reply: We have considered the relative humidity up to 10 km region in the revised manuscript.

Section 4 . . . The conclusions are misleading, the authors compare temperature differences in very short time range and particular conditions, with climatological anomalies from other studies. As already reported, the analysis is based on a background (one week before) which can't be assumed as a strong reference, so it is not possible to evaluate the results.

Reply: In the revised manuscript, we have compared the present eruption with 13 years of background climatology. We have modified the conclusions in the revised manuscript.

Technical corrections –

Please write SO2 consistently, sometimes it is SO2 and sometimes with "2" subscript

Reply: Modified in the revised manuscript as suggested.

- Section 2.1 . . . if the authors do not want to describe the Radio Occultation in the paper, they would cite at least a paper describing the radio occultation technique and characteristics (e.g. Kursinski et al 1997)

Reply: We included the Kursinski et al 1997 reference in the revised manuscript.

- Lines 98-99 . . . the authors state that they used wetPrf products but they also used atmPrf (to analyze the bending angle). It should be reported in this section. - Line 99 . . . 100 m is not the vertical resolution but it is the vertical sampling

Reply: Corrected in the revised manuscript.

- Lines 124-127 . . . NCEP data should be described in a separate sub-section

Reply: We have described NCEP data in a separate sub-section as suggested by the reviewer.

- Line 142 . . . "plume was accumulated"? Why accumulated?

Reply: The volcanic plume was transported along with the background anti-cyclonic circulation. We have modified this sentence in the revised manuscript.

- Line 169 . . . please provide a reference about the amount

Reply: We have collected this information from Bulletin of the Global Volcanism Network, vol. 45, no. 6 (June 2020). We have included this reference in the revised manuscript.

- Line 244 . . . 200 m is not the vertical resolution but it is the vertical sampling

Reply: Corrected in the revised manuscript.

- Figure 1 . . . why it is reported the RO spatial distribution in the box 0-35◦ /110-180◦ when the analysis is done and focused in a much smaller area?

Reply: There is no specific reason for this. We want to describe how the COSMIC-2 RO data distributed on a single day over some region. The selected region is also most effected by the volcanic eruption during 12 -20 January, 2020. Our two locations which we considered in the present study are located within this region.

- Figure 2. please explain in the caption what the black arrows represent/show

Reply: Corrected in the revised manuscript.

- Figure 4 . . . some profiles of Figure 4 should not be collocated with the eruptive cloud since those RO are collocated in an area 5◦x5◦ but the cloud just moved north-east.

Reply: Yes. Some of the profiles are not collocated with the cloud, which is shown in the figure. However, unfortunately we don't have OMI satellite overpass over the southern side of the volcano during 13 January.

- Figures 6-7 . . . I would extend the panels to the 1st od January to see what is the difference before and after the eruption (of course, in this case, the reference period must be changed)

Reply: In the revised manuscript, we have performed the analysis based on 13 years of COSMIC-1 RO data. We carried out our analysis for the period of 05-20 January, 2020 as suggested by the reviewer number 2. As the eruption happened on 12 January, we considered that day as an eruption day and 05-11 Jan as one week before and 13-20 Jan as one week after in the revised manuscript. We obtained the temperature anomalies by subtracting the background climatology from daily mean temperature profiles. Finally, the obtained anomalies are discussed with respect to one week before and one week after the eruption.

We once again thank the reviewers for going through the manuscript carefully and offering potential solutions, which made us to improve the manuscript content further.

—END—