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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Measurement report: Immediate impact of the Taal volcanic eruption on atmospheric temperature observed from COSMIC-2 RO measurements" *by* Saginela Ravindra Babu and Yuei-An Liou

Anonymous Referee #1

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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).

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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).

- 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.

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 $\pm 5^\circ$ 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°?

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.

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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.

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

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.

Technical corrections

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

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but it is the vertical sampling - Lines 124-127 . . . NCEP data should be described in a separate sub-section - Line 142 . . . "plume was accumulated"? Why accumulated? - Line 169 . . . please provide a reference about the amount - Line 244 . . . 200 m is not the vertical resolution but it is the vertical sampling

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

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