Responses to the Comments of Referee 1

General comments

This paper assesses the impact of a large number of GNSS-RO real observations made available in the context of ROMEX study on the mesoscale forecast over the Indian Region.

In addition to some classical thermodynamic variables, the article focuses on rainfall forecasting in the Indian Region with verification against ERA-5 reanalyses and IMERG satellite observations. This latter point together with the mesoscale domain contribute to the interest and originality of the study.

Clearly, the study required significant technical work to implement a 2D bending angle observation operator within the WRF data assimilation system and run WRF coupled to ERA5.

The well-written paper presents accessible and understandable results and illustrated with very clear figures. It is mostly very clear and almost ready for publication.

Nevertheless, the manuscript could still be improved by addressing the points discussed below in order of importance from highest to lowest:

Reply: We sincerely thank the reviewer for the constructive comments and encouraging remarks.

Major comments:

Comment 1: The study covers a period of one month, which I think is the minimum length of time required to draw meaningful and robust conclusions. I guess the main reason is that the study focuses on the summer monsoon which ends in September (the monsoon is mentioned line 82) but this is not explicit written. Is the length of the period really determined by the monsoon forecast, or is it due to the heaviness of the experimentation? If the main objective is to evaluate the impact of ROMEX GNSS-RO data on summer monsoon forecasting, this must be clearly stated (for instance in the first sentence of the abstract).

Reply 1: We agree with the reviewer that including the full monsoon period would have been useful. The Indian summer monsoon extends from June to September; however, the ROMEX data are available only from September to December 2022. Therefore, only September overlaps with the monsoon season, and this month was used to assess the impact of ROMEX data. In the revised manuscript, we have clearly specified the study period in the abstract as well as in the last paragraph of the introduction

Comment 2: Figure 11/Figure 15 (or in a more general way): are the results statistically significant? This information is important for the robustness of the results. I suggest you add an indication of a confidence interval (for instance, error bars on the curves and profiles).

Reply 2: We thank the reviewer for the suggestion. In the revised manuscript, these two figures have been regenerated, and the corresponding significance levels are now indicated. For most variables and pressure levels, the differences are significant at the 95%

confidence level. Similarly, the differences in rainfall skill scores are also significant, except for the bias differences during the first 24 h forecast.

For most other variables, although we did not perform a formal statistical analysis, the differences between the control and the GNSS-RO observation experiment are substantial (15–20%) and can be considered significant.

Minor comments:

Comment -1: line 8 « Conventional observations »: as there is no official definition for conventional observations, this needs to be explicitly defined here, even if described further in part 2.3. Here, you mean in situ together with satellite wind-observations

Reply 1: Thank you for the comment. The conventional meteorological observations used in the data assimilation include surface observations from synoptic and METAR stations, upper-air observations from radiosondes, pilot balloons, wind profilers, aircraft measurements, winds derived from geostationary satellites and scatterometers, and marine observations from ships and buoys

In the revised manuscript, the conventional observations have been clarified in the abstract..

Comment 2: - line 22: It seems that the term 'conventional' is used to mean 'in situ' observations here. For more clarity, I suggest you use the expression 'in situ' instead here.

Reply 2: Yes, it was meant to indicate in situ observations. In the revised manuscript, the term "in situ observations" has been used instead of "conventional observations" for clarity and consistency.

Comment 3: - line 38: COSMIC-2 constellation provides observation between ±40 degrees latitude. This could be mentioned.

Reply 3: Thanks you for the suggestion. The suggestion has been incorporated. The region covered by COSMIC-2 has been mentioned in the revised manuscript.

Comment 4: - line 95: the NMC sigle should be spelled out.

Reply 4: The abbreviation NMC has been expanded as National Meteorological Center in the revised manuscript.

Comment 5: lines 112 to 115 : it is first written that the RO observation errors used in the study are those applied in GFS in operation. But line 115, it seems that GFS has « larger observation error assumptions » in operation. The two sentences seem to contradict each other. I probably misunderstood or this needs to be clarified.

Reply 5: What we have mentioned is that we used the same error specification and quality-control approach as adopted in the NCEP GFS operational system. However, we note that the NCEP GFS assigns slightly larger errors (larger compared to the errors provided in the BUFR data) to GNSS-RO observations, probably because an operational system requires robust and smooth cycling without adverse impacts from poor-quality observations. To clarify this, we have added additional references and discussion in the revised manuscript. We hope this addresses the reviewer's concern.

Comment 6: Use of ERA5 for the boundary conditions and also for verification. Perhaps it could be specified that most of the RMX-BA RO observations were not assimilated in ERA5 reanalysis.

Reply 6: Thank you for this suggestion. We agree that the verification dataset can be considered largely independent of the ROMEX data. This is because the full ROMEX dataset itself is not directly assimilated into ERA5; however, a portion of it might be, as ROMEX contains data from multiple missions—such as COSMIC-2—whose data may already be integrated into ERA5. To avoid any ambiguity, we have added the following statement to the revised manuscript:

"Although ERA5 assimilates a limited subset of the ROMEX observations, most of the available ROMEX data were not assimilated; ERA5 can therefore be regarded as a largely, though not fully, independent reference for verification."

Comment 7: IMERG estimates are chosen as references for rainfall validation. In addition to Huffman et al., 2020 the reference to an article on IMERG performances on estimating heavy rainfall might be of interest (for instance *https://doi.org/10.1016/j.advwatres.2015.11.008*)

Reply 7*:* Thank you for the suggestion. We have included the suggested reference in the revised manuscript.

Comment 8*:* line 195: « TE is a vertically integrated index » as TE is (homogeneous to) a norm, perhaps it would be better to change index (which has generally no unit) into norm or metrics.

Reply 8: The term "index" has been replaced with "metric" in the revised manuscript.

Comment 9: line 202 : « as expected TE exhibits a systematic increase with forecast lead-time » Obviously, this is the general trend but we can see decrease of TE between 0 and 6-h of lead-time for both experiments, also between 12 and 18h, 24 and 30h, 36 and 42h. Just out of curiosity, what's your interpretation of this?

Reply 9: Thank you for pointing this out. It is difficult to provide a single definitive explanation for this behaviour. It may be partly associated with diurnal variability or the variation in numbers and location of the observations in each assimilation cycle. However, we prefer not to speculate in the paper without further study.

Comment 10: line 205 : introduction and definition of the « Impact Parameter » IP. In the field of GNSS-RO, « impact parameter » refers to « nr sin Φ » or *a* on Figure 3 of the paper (the distance of closest approach for the straight line path).

So, I suggest using another term for the quantities defined here, for instance Impact Index or something else.

Reply 10: Thank you for this observation. In the revised manuscript, we have changed “impact parameter” to “impact metric (IM)” in the results section.

Comment 11: - 3.2.2 : « only minor degradation observed near the surface (…) mainly in specific humidity » : near the surface, the degradation in specific humidity is 10-20 % during the first 6 hours (6-8 % for temperature), getting smaller after. But that’s where the forecast error in CNTL is maximum. So, adding ROMEX RO observations does not fix this problem in the initial hours; on the contrary, it makes it worse.

Reply 11: The higher error during the first 6 hours may be attributed to model spin-up issues, which are commonly observed in regional models. Another possible explanation is that near-surface bending angle observations are associated with more complex observation errors due to sharp refractivity gradients and frequent super-refraction, particularly in regions with high atmospheric moisture, such as the Indian Summer Monsoon (ISM) region. Consequently, the observation errors assigned to GNSS-RO bending angle observations may not accurately represent the true uncertainties in this region, which could result in a suboptimal analysis.

This has been mentioned in the revised manuscript.

Comment 12: - line 285 : the new IP could be introduced by a sentence like « the impact of RO data assimilation on the rainfall forecast is quantified by... »

Reply 12: Thank you for pointing this out. The following statement has been added to the revised manuscript: “The impact of RO data assimilation on the rainfall forecast is quantified by computing the Impact Metric (IM), as given below.”

Comment 13: - line 301 (and following), figure 15: ETS (Equitable Threat Score) should be defined or a reference to a paper should be given.

Reply 13: We have cited the following references for the use of bias score and ETS in precipitation forecast verification in the revised manuscript.

“The rainfall forecast skill was evaluated using categorical verification metrics, including the bias score and equitable threat score (ETS), following Colle et al. (2000).”

Colle, B. A., Mass, C. F., and Westrick, K. J. (2000). MM5 precipitation verification over the Pacific Northwest during the 1997–99 cool seasons. *Weather and Forecasting*, 15, 730–744.

Please note that in the original manuscript, although the score was mentioned as ETS in the figure and text, it was actually the threat score (TS). In the revised manuscript, we have corrected this mistake and provided the actual ETS.

Comment 14: Figure 15 : the caption should indicate which plot is for 24h accumulated rainfall, which one is for 48h.

Reply 14: Thank you for the suggestion. The 24-h and 48-h forecast lead times are indicated at the top of the figure.

Comment 15: Conclusion line 326 : the biases for specific humidity are often corrected, but not all of them. So, I would nuance the text « The assimilation corrects both moist and dry biases »

Reply 15: Thank you for the suggestion. We have revised the statement to: "GNSS-RO assimilation generally reduces both moist and dry biases in specific humidity."

Typos:

Comment 1: line 159 : the name of the variable that represents the refractive-index radius product is missing here or there is an extra comma.

Reply 1: Thank you for pointing this out. This has been taken care of in the revised manuscript.

Comment 2: I suggest you choose one of both writings GNSS-RO (more usual) or GNSS RO, as both are used in the paper.

Reply 2: Thank you for the suggestion. In the revised manuscript, "GNSS-RO" has been used consistently throughout.

Comment 3: caption for Figure 5 : the mathematical symbol for multiplication appears in letters in the size of the grid (5◦ *imes*5◦ for grid 5◦ x 5◦ grid)

Reply 3: Thank you for pointing this out. We have corrected this in the revised manuscript.

Comment 4: Figure 6 : the unit for O-B and O-A statistics is missing.

Reply 4: Thank you for pointing this out. The unit for the bending angle is degrees, and this has been included in the figure in the revised manuscript."

Comment 5: line 223 : the greek mathematic symbol σ appears as « Std.Dev »

Reply 5: Thank you for pointing this out. "Sigma" has been replaced with "Std. Dev." in the revised manuscript.

Comment 6: line 244 : missing « . » at the end of the sentence

Reply 6: Thank you for pointing this out. This has been fixed in the revised manuscript.

Responses to the Comments of Referee 2

General Comments

The manuscript presents the results of Observing System Experiments (OSEs) that assimilate massive amounts of GNSSRO observations to assess their impact on forecasts over the Indian region. OSEs are carried out by employing the Weather Research and Forecasting (WRF) model with 2D bending-angle observation operator implemented in this study. The manuscript provides a detailed evaluation of the forecast impact by verifying model performance against both reanalysis fields and satellite-derived observations. These results demonstrate the potential of GNSS-RO observations to improved forecast skill of rainfall associated with mesoscale system. While most previous studies assimilating GNSS-RO to limited area model employ refractivity, this study assimilates bending angle using 2D operator. I feel that the results add valuable knowledge to the community and suggest publication after revision to address the following points.

Reply: We sincerely thank the reviewer for the positive and constructive comments, which have helped us improve the manuscript.

Major comments

Comment 1: Fig5/6. The quality and impact of GNSSRO observations on the NWP system vary with altitude. To help readers better interpret the results, it would be helpful to include the data distribution and OmB/OmA statistics as a function of altitude and relate these to the presented findings.

Reply 1: We thank the reviewer for this valuable suggestion. In the revised manuscript, we have included a figure showing the O-B and O-A statistics as a function of altitude.

Comment 2: Line 205. The term "Impact Parameter" has a well defined meaning in GNSSRO. Using the same term for a forecast verification metric may confuse readers. Please revise the wording so as to make readers easy to follow.

Reply 2: We fully agree with the reviewer that the use of the term “impact parameter” created confusion. Therefore, in the revised manuscript, this term has been replaced with “Impact Metric (IM)” in the results section.

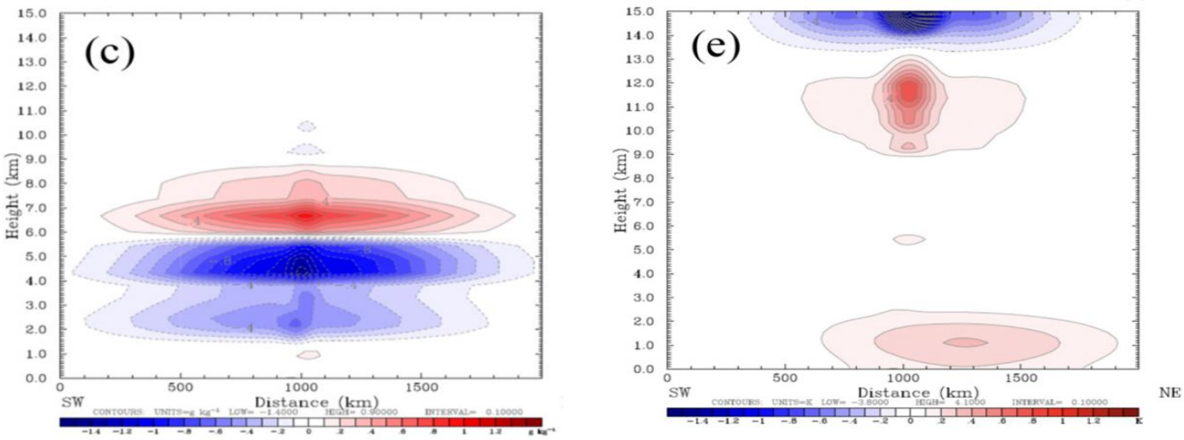
Comment 3: Line 324. Figure 10 shows that the improvement of temperature forecast remains longer than that of specific humidity. It is reasonable since lifetime of convection is shorter than that of large-scale flow. However, the current wording appears to imply that the improvement in specific humidity is primarily due to the water vapor information directly contained in the RO observations. If this is the authors’ intention, please provide clearer physical justification for this interpretation. If not, I suggest rephrasing the text to clarify that the longer-lived temperature improvement may be the main driver of the humidity improvement.

Reply 3: We agree that the lifetime of the specific humidity increments is shorter than that of temperature, as indicated by the reviewer and shown in Fig. 10. RO observations directly impact both the water vapor and the temperature analysis in the moist tropical tropopause. Fig. 10 shows that the impact of RO on specific humidity is immediate (affects the analysis and short-term forecasts) and has no relationship to the impact on temperature. This impact decreases with time during the first day, which is different from what would be expected if the temperature changes were the main driver of the specific humidity because the temperature changes increase over the first day and persist over the entire three days of the forecast.

Many studies have shown that the impact of RO data on the water vapor analysis is direct and immediate because bending angles and refractivity are sensitive to water vapor in the moist tropical troposphere. Increments in bending angle produce changes in model refractivity and increments in both water vapor and temperature. In moist tropical air, the largest changes often occur in the water vapor.

Fig. 3 in Chen et al. (2020) show an example of the increments in a model water vapor mixing ratio (c, in g/kg) and temperatures (d, in K) at 700 hPa due to the assimilation of RO observations. This illustrates that most of the impact of RO information at this level occurs in the water vapor rather than temperature.

Fig. 3 in Chen et al. (2020)



We have revised the paragraph beginning with line 324 to more specifically describe what is shown in Fig. 10:

"RO assimilation improves the specific humidity analysis and forecasts in the middle troposphere out to 3 days, with the largest improvement (10-15%) occurring in the first day. Specific humidity biases are reduced, especially over the oceans where conventional observations are sparse. The assimilation corrects both moist and dry biases, resulting in a more realistic representation of the water vapor distribution. Temperature and wind fields also show improvements (10 to 12%) over the three day forecasts, with the most noticeable impact on winds occurring in the upper troposphere."

Reference

Chen et al. 2020. The Impact of GPS RO Data on the Prediction of Tropical Cyclogenesis Using a Nonlocal Observation Operator: An Initial Assessment. Mon. Wea. Rev.
https://doi.org/10.1175/MWR-D-19-0286.1

Minor comments:

Comment 1: line 22. Please specify 'conventional observation' for the reader's understanding.

Reply 1: Thank you for the comment. The conventional meteorological observations used in the data assimilation include surface observations from synoptic and METAR stations, upper-air observations from radiosondes, pilot balloons, wind profilers, aircraft measurements, winds derived from geostationary satellites and scatterometers, and marine observations from ships and buoys.

This has been added to the abstract of the revised manuscript

Comment 2: line 53. Adding a reference here would help readers follow the discussion more smoothly.

Reply 2: We thank the reviewer for this suggestion. In the revised manuscript, the references (Healy and Thépaut, 2006; Cucurull et al., 2007) have been cited

Comment 3: line114. Please specify the dataset from which the 'observation errors' were obtained. Since raw conventional data usually does not include error estimates for assimilation, it would be helpful to clarify how these errors were prepared.

Reply 3: We thank the reviewer for pointing out this important aspect of the data assimilation system. The following statement has been included (in Section 2.3) in the revised manuscript:

"For conventional observations, the observation error information is included in the BUFR data itself. Therefore, the observation errors were taken directly from the BUFR data. However, for GNSS-RO observations, the bending-angle error is estimated using an approach similar to that adopted in the NCEP GFS (GSI data assimilation) operational system (Cucurull et al. 2007, Cucurull et al., 2013; Kleist et al., 2009).

Comment 4: line 116. Adding a reference here would help readers follow the manuscript more smoothly.

Reply 4: We thank the reviewer for this suggestion. In the revised manuscript, the references (Cucurull et al., 2007, 2013; Kleist et al., 2009) have been added.

Comment 5: line 245.The statement "with only minor degradation" does not seem match with the figure which show the magnitude of degradations is comparable to those of improvements. Please revise the wording so as to better reflect what is shown in figure.

Reply 5: We thank the reviewer for pointing this out. We agree with the reviewer that, near the surface during the initial hours, the degradation is comparable to the improvement. The discussion has been revised accordingly, and the word "minor" has been removed from the revised manuscript.