

Review of “Impact of Increased ROMEX GNSS Radio Occultation Data on Forecast Performance in the Korean Integrated Model”

By Eun-Hee Kim et al.

This manuscript presents a study on the impact of increased GNSS-RO data provided by the ROMEX project on the KMA's NWP system. Specifically, it examines the contribution of GNSS-RO data at the current operational level, the impact of the increased data volume, and the sensitivity of the forecasts to changes in assimilation parameters.

The study makes a valuable contribution to the broader RO community by improving our understanding of the value of increased GNSS-RO observations. The manuscript is generally well presented. However, there are a few aspects that I believe the authors could further improve in terms of presentation and supporting results, which would enhance the overall quality and readability of the manuscript.

Major comments,

1) L176 and following discussion. The description of observation error is unclear and misleading.

(a) The authors mentioned that the error is “1% at altitude above 10km”. I do not think 1% is the value for all levels above 10km. Please check and clarify.

(b) They should provide the observation error value at surface level. This is important because it is related to the inflation in the later sections.

(c) “The GNSS RO observation error used in KIM has an altitude-dependent structure (Kwon et al., 2015).” If this study uses the same baseline structure, the observation error at surface is 10%.

(d) “Larger observation errors are assigned in the lower troposphere to account for super-refraction and signal-tracking uncertainty.” I do not think observation error can directly account for super-refraction. Please considering revising.

2) L181–189, observation error inflation

(a) If my comment 1(c) is valid, an inflation factor of 5.3 would increase the observation error to 53% at the surface, which is very large. Even if this is an experimental setup, the authors need to clearly discuss the theoretical and practical rationale for using such a large inflation factor.

(b) If my comment 1(a) is valid % at altitude above 10km, how is the inflation done for observations above 10km? 5.3% at 10km?

I suggest the authors provide a figure to visualize the baseline observation error model and the values used in the error inflation experiments. That would be very helpful.

(b) Usually, observation error specification is related not only to the weighting of RO observations in the cost function but also to the background departure (O–B) quality control check. Many NWP centers reject an observation if its background departure is greater than a certain multiple (e.g., 3 times) of the observation error. If this also applies

to KIM, a larger observation error could allow more observations with large O–B departures to pass this quality control check.

(c) The implications of both the de-weighting of the observations and the potential increase in the number of observations passing quality control need to be thoroughly discussed. In addition, statistical figures showing the O–B mean and RMSE, as well as the number of observations used for each experiment, would be helpful and necessary. At a minimum, these statistics should be provided for EXP_35, EXP_Errinfl43 and EXP_Errinfl53.

3) The assimilation of RO data impacts temperature and humidity directly in the model analysis. However, the authors do not provide any verification results for humidity. Including that verification would make this manuscript more complete. In addition, verification on wind components or speed is preferred.

4) L165, how and why is the thinning done?

(a) Presumably, each RO profile contains about 247 observations, with variable intervals from 50m to 200m vertically. Did the authors use a set of fix vertical altitudes and apply interpolation?

(b) What is the major purpose of vertical thinning? Computational cost? If so, can the authors elaborate how much more computational cost the 35K daily profiles add to the existing system? And how much the thinning reduces compare to the full 35K data?

(c) I guess the vertical model interval in the near surface layers is less than 400m. Can the authors elaborate the potential impact of using fewer observation in the lower troposphere?

(d) In my understand, the thinning is only applied to EXP_35K. Is this true?

5) Section 4.3, analysis increment

I do not quite understand why the authors show the increment of geopotential height. RO observations are more directly related to temperature and humidity rather than height. I understand geopotential height can change indirectly with the assimilation of RO data, but what do the authors expect in the analysis increment?

6) The presentation structures can be adjusted to improve the flow and readability. For example,

(a) Fig. 3 does not only talk about general thinning strategy but also discuss other QC results. I suggestion the authors move this part to the result section after the introduction of experimental design. They should only keep the introductory part before the presentation of any results.

(b) Fig. 2 also needs to be presented after the introduction of experiments. In addition, this figure needs to be considered with my earlier comment 2(c).

(c) “Figure 6 shows changes in the forecast performance of EXP_20k and EXP_35k relative to CTL as lead-time–altitude cross-sections, using the IFS analysis as the reference.”

~~“Figure 7 and Table 2 summarize the effects of increased data volume in terms of global and regional means. Figure 7 shows how the RMSE and STDV reduction rates for the 100 hPa temperature, relative to NoRO, changed with the data volume.”~~

Table 2 should be introduced right after “Figure 6 shows” because it provides the similar metrics using CTL as a reference, and it was cited in the same paragraph.

The use of effect is vague. The authors may consider remove “Figure 7 and Table 2 summarize the effects of increased data volume in terms of global and regional means.” The following sentence should be enough.

7) Description of figures can be improved. For example, in the discussion of Figure 6

(a) L297, “some variable”. Since there are only two variables in this content, using specific variable name will be clearer.

(b) Since the figures show results for different regions, lead times, pressure levels, and variables. The authors need to clearly describe these different aspects and discuss the potential reasons for the observed differences.

(c) L301, “....suggesting a structural response related to forecast error growth rather than a simple initial-condition fluctuation.” How is this from? any further evidence or discussion?

Specific comments,

L37, I do not think “surface-based” is needed here

L40, I do not agree that GNSS-RO data only provide information in the UTLS. This sentence needs revising.

L44, since it was mentioned and Aparicio and Deblonde (2008) demonstrated the usefulness of refractivity data, it would be better to mention what observables Healy and Thépaut (2006) used.

L44, “could improve”

L77-83, “however, there studies focused.....”,

and L87-92, “previous studies have shown that ...”

The content of these two places is largely duplicated. Please consider revising and simplifying them.

L 89, “However, whether large-volume RO data, ..., translate linearly into forecast improvement ..”

What do translate and linearly mean here? It needs to be revised or explained.

L94, please spell out JEDI here.

L97, “Simultaneously, the European Centre for Medium- Range Weather Forecasts (ECMWF) (Lonitz, 2024) and the Met Office (Bowler and Lewis, 2026), among other ROMEX participants, reported increasing biases in lower-stratospheric temperature and geopotential height forecasts in 35 000 profiles d-1....”

I feel these sentences are misleading. They emphasize the bias issue without mentioning the positive impacts seen in their systems. In addition, are the biases only seen in the experiments with 35,000 daily profiles? Do they imply that the experiments

with 20,000 daily profiles performed better than those with 35,000 daily profiles in terms of bias and/or other metrics?

L102, I suggest remove “by assessing the impact of large-volume RO data within KIM’s assimilation configuration.” That would make the sentence shorter and clearer.

L126 and Figure 1, “06 UTC”, it should be mentioned the time window here. Are the observation distributions showing over 6h period?

L126, “CTL and EXP”, EXP may be EXP_35k to be specific for Figure 1b?

L130, “notably reduced over oceans,...”

I do not see the gaps over oceans.

Fig.2, please provide a complete caption.

L164, please spell ROPP and provide reference(s).

L198, “observation-operator coefficient” -> refractivity coefficient

L228, 100-> 100%

L255–257, “upper-level”, “lower-level temperature”, and “mid-level geopotential height”, are too ambiguous. The authors should be more specific.

L289, “...while reducing the scatter at some levels”. It can be “...while reducing the scatter above 700hPa”?

L290, I do not see “a different bias structure from CTL and EXP_20K in the lower stratosphere”

L438, “RO bending-angle observations are most stable around 15–30 km.” any reference about this range? What does stable mean? What is not stable below 15km? RO observations or their impact?

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