

Review of the manuscript amt-2015-131 "Preliminary validation of refractivity from a new radio occultation sounder GNOS/FY-3C" by M. Liao et al., manuscript version as of 01 September 2015.

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The manuscript discusses results from the validation of a newly developed, multi-GNSS (GPS/BeiDou) capable sounding instrument that is flying on an operational weather satellite. Important central results are the comparison of refractivity profiles derived from the data to ECMWF's ERA-Interim reanalysis, and comparisons with other RO missions. The results confirm the outstanding features of radio occultations to provide essentially calibration-free, bias-free, and mission-independent observations of the atmosphere.

General remarks

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The quality of the manuscript has improved considerably since the initial version. Essential comments and questions of the quick review have been answered. Still, the paper lacks coherence in the presentation. The authors should consider to seek additional support from proficient English speakers for checking and improving the language.

There are several periods being used for the selection of data. When looking at the figures, it is not always clear which period applies. It is recommended to add the relevant period where it is missing.

Specific remarks

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Abstract:

P.9010, lines 7ff: the comparison is actually performed against ECMWF's ERA-Interim analysis, which is a reanalysis and differs in several respects from ECMWF's operational analysis. The authors should try to coherently refer to the ECMWF reanalysis over the entire manuscript.

Line 9: the mean bias is quoted here as a positive quantity, while I find negative values in tables 1 and 2, and also differing height ranges. I don't think that the authors suddenly quote a mean absolute bias. Can they please clarify?

Introduction:

P.9010, line 20: "though" should read "through".

Line 25: "technology" should read "technique".

P.9011, line 6: "nuclear" should read "numerical".

The use of RO in operational NWP actually started after the papers of Hajj and Kuo. The authors should consult e.g. the papers by Healy and Thepaut (2006), Cucurull et al. (2007), Aparicio and Deblonde (2008), etc. for more information.

Lines 13ff: A refractivity profile could be considered a "primary" product, but I would never call it "elementary", since it is generated after several processing steps. For the NWP community, bending angle is nowadays considered the primary RO product, and refractivity a primary or secondary product.

Line 15ff: Most NWP centers assimilate bending angles into their global models; only a minority uses refractivity. See e.g. the overview at:

<http://irowg.org/projects/ro-data-use-in-operational-nwp-systems/>

Line 19ff: Performance of the "current" radiosonde? The paper of Kuo et al. rather states that they are able to (statistically) differentiate the performance of various types of radiosondes, which is something different. Please rephrase.

P.9012, lines 3-4: It is my impression that GPS is an "operational" system, and that development did not stop since satellites need to be replaced when they age. It is also my impression that BDS, even being developed and improved over time, is a system being "deployed", with currently roughly half of the number of planned satellites in place.

## Section 2

P.9012, line 16: "consistently": should it read "continuously"?

Line 20: "The more information for GNOS..." should read "More information on the GNOS..."

P.9013, line 3: "affiliated" -> "ancillary" or "auxiliary"?

Line 13f: citation/reference for the Bernese software?

Line 14: What does "At length" mean in that context?

P.9014, line 5: The GRAS SAF changed its name to ROM SAF some time ago.

Line 8: (Doppler) "drift" should read "shift"?

Line 9ff: The authors claim that "below 25 km there are ... complicated multipath effects." This number sounds unusually high. Multipath is usually expected to occur more likely in moist tropospheric conditions, less so in the stratosphere. Can the authors be more specific on the reasons for their choice?

Lines 12ff: There's no reason to talk about refractivity here. The first step is the derivation of the ionosphere-free bending angle.

Lines 23ff: Question: is  $n(r)$  calculated on a fixed grid, i.e. same grid for all profiles, or is the grid different for each profile?

P.9015, line 4: There is no "moisture ambiguity", it is the decomposition of refractivity profiles into temperature and humidity that is ambiguous.

Line 7: Which "T639 forecast model" is being referred to?

Line 13ff: Is the full profile rejected if any ray has a bending angle larger than 60 mrad, or only part of the profile?

P.9016, line 5ff: There is some confusion here. I think that the authors wanted to express that GNOS does support open-loop tracking for GPS L1, but not for BDS B1. I am not aware of a problem with the GNSS signal design. Please check and reformulate.

Line 25: "...comparable to the GPS occultation". Did the authors try to compare to other RO missions?

## Section 3

P.9017, lines 10f: "...while GNOS BDS is obtained from the experiment system as it does not become operational." Does this imply that there will never be an operational GNOS BDS product stream?

Lines 14-16: To the best of my knowledge, COSMIC and GRAS are actually quite different types of GPS receivers.

Lines 19-20: The ERA-Interim website and the ERA-Interim publication clearly state that the model top of the atmosphere is at 0.1 hPa, not at 1 hPa. See e.g.

[http://old.ecmwf.int/products/data/technical/model\\_levels/model\\_def\\_60.html](http://old.ecmwf.int/products/data/technical/model_levels/model_def_60.html)

That is quite important if you want to use the reanalysis for impact heights above 45 km.

Lines 21-22: Is there a reason not to compare to COSMIC, which has more total occultations than a single Metop/GRAS?

P.9018, lines 10ff: Instead of "forwarded", use "modeled" or something similar. How shall I understand "vertically logarithm interpolated"? Does it mean that the logarithm of refractivity is vertically interpolated in some way, or something else?

Lines 18ff: There is excessive and unconventional use of the word "statistics": "through statistics", "during the process of statistics". Please reformulate.

How many profiles are rejected by the "extra quality controls"? Looking at figures 1 and 3, one expects approx. 450 profiles/d for GNOS GPS before quality control. The procedures described in section 2.1.5 reduce this by roughly 20%, leaving roughly 360/d or 21600 for 2 months of data. Line 23 has 17509 profiles (290/d) before extra QC, roughly 20% less than the naive estimate. So where did this 20% go? And yet there is another 27% rejected by the extra QC?

There are several occurrences of the misspelling "outliners" instead of "outliers" in the text.

#### Section 4

P.9019, lines 12-16: As explained above, the ERA-Interim analysis is available up to 0.1 hPa, which is more like 65 km rather than 46 km (for 1 hPa), and no extrapolation seems necessary. I therefore do not understand the discussion here. Maybe the authors made some mistake?

P.9020, lines 5-10: I think that part of the apparent differences in the performance of GNOS GPS versus GNOS BDS is a genuine sampling issue. This can already be seen from figures 2 and 11: the more difficult tropical lower troposphere is less covered by GNOS BDS. In my opinion this has little to do with "representativeness error".

Line 18: Which "fluctuant features" are the authors talking about? Assuming that the ECMWF model used in the reanalysis is not perfect, there will be biases in the analysis. Maybe the authors can clarify.

Lines 24-26: "As seen in previous studies, the radio-occultation-data spreads from the middle troposphere to the lower stratosphere play a key role in numeric weather prediction (Kuo et al., 2000)". Sorry, but I do not understand this. Clarify or remove.

P.9021, lines 27-28: What is "non-optimal statistics"? Why only GNOS vs. GRAS, not vs. COSMIC?

P.9022, lines 17-18: "It should not be excluded that the systematic representative error due to time and space gaps." Sorry, but I do not understand this.

P.9023, lines 3-4: "As the analyses are based on the non-optimal statistical bending angles...". Do the authors refer to the ionosphere-free bending angles without/before statistical optimization, or something else? Please clarify.

Line 20: "Figure 9 is shows..." Remove "is".

P.9024, line 3: "below the lower troposphere." Where exactly is that?

Line 13: "for the next generations." Should this rather read "for the next generation of GNOS."?

Line 15: "different latitudes" -> "different latitude bands".  
Same for the caption of table 2.

Line 16-17: Southern Hemisphere (30°S-90°S) (not "N").  
Same typo in the caption to figure 11.

Line 21: Regarding "representativeness error" see also my comments above.

Is it possible to redo figure 10 in the same way as figure 3, with the same color scale for the penetration depth?

Lines 26-28: "The Northern Hemisphere and Southern Hemisphere can be considered as data-rich and data-sparse regions, respectively." This is maybe relevant for the discussion of the quality of ERA-Interim for those periods when there were fewer satellite data available, but I fail to see why it is relevant for the present discussion. Clarify or remove.

P.9025, line 7: "zones" -> "latitudes"?

Lines 6-7: "These comparisons give evidences to say that radio occultations perform better at middle and high zones." As it is written, I think this is a too general and misleading statement. The conditions in the tropics are more challenging for both NWP models and RO than in the extra-tropics. Rephrase or remove.

Lines 8-9: "showing that the wavy structures are real." This is not really clear and obvious. What do the authors mean by "real"? Are they talking about structures in the bias and/or the stdv? Are they sure that there is no potential problem in their calculation of the profiles and/or statistics?

The ROM SAF provides monitoring of observations minus background statistics on their website:

<http://www.romsaf.org/monitoring/index.php>

The monitoring of refractivity is against the MetOffice model, which has different quality than the IFS from ECMWF. Even historic data can be selected for displaying. Looking e.g. at data for November 2013, I find it difficult to reproduce the findings of the authors. Could they please verify that e.g. the differences in the bias between COSMIC and GRAS agree with the differences between COSMIC and GRAS as shown in the ROM SAF monitoring?

Lines 11-12: The reference to the Alexander et al. paper here is not really clear to me. Do the authors want to express the potential for measuring gravity waves or something different? Please clarify or remove.

## Section 5

P.9026, lines 4-5: "As to different zones, GNOS GPS can reflect the superiority of middle and high latitude zones over the tropics, due to less multipath propagation in the moist atmosphere especially in the lower troposphere." As I explained above, I consider this judgment misleading, and recommend to remove it.

I would rather recommend to add a summarizing sentence about the comparison of the raw bending angle profiles in section 4.2, as it is more relevant to the NWP community.

Tables 1 and 2: Units [%] are apparently missing.

Figures 4,5,9,11: x-axis label " $\sigma N$ " should read " $\Delta N$ " instead, to be consistent with eq.(5).

Figure 11, caption: Southern Hemisphere ( $30^{\circ}\text{S}$ - $90^{\circ}\text{S}$ ) (not "N").