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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Evaluation of atmospheric profiles derived from single- and zero-difference excess phase processing of BeiDou System radio occultation data of the FY-3C GNOS mission" *by* Weihua Bai et al.

Anonymous Referee #2

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This paper presents results from the "GNOS" radio occultation (RO) measurements aboard the Chinese FY-3C satellite. It is shown that BeiDou GNSS observations, analyzed in single-differencing (SD) and zero-differencing (ZD) mode, produce bending angle and refractivity profiles of equivalent quality, when compared to ECMWF and co-located radiosonde data. In addition, due to the non-uniform global coverage of the current BeiDou space segment the ZD data set includes about 20% more events compared to the SD set because occasionally suitable BeiDou satellites providing the reference link were not available within the receiver's antenna field of-view. Further-

more, a unique feature of the BDS system is that the signal transmitters are placed into three diverse orbits (MEO, IGSO, GEO). The present study convincingly shows that these orbit differences significantly modify the zonal and meridional distribution of RO events, but have no appreciable impact on the quality of the derived atmospheric profiles.

This well-written paper is a valuable contribution to the present knowledge on single- versus zero-differencing RO analysis and I definitely recommend publication with some minor modifications described below.

General comments:

As emphasized by the authors the successful application of zero-differencing is made possible by the presence of an ultra-stable oscillator driving the GNOS instrument. It would be instructive to illustrate the performance of this clock by providing clock offset statistics. These could be extracted from the results of the FY-3C precise orbit determination.

The comparisons of SD and ZD with ECMWF and radiosonde data are instructive and illuminating. In addition, the direct comparison between SD and ZD bending angle profiles would be worthwhile to consider, in order to substantiate the hypothesis that no biases between the SD and ZD results exist. If possible, I would encourage the authors to add a corresponding figure in the revised paper.

Specific remarks and questions:

Page 3, lines 21ff:

> "One of these LEO missions is China's GNss Occultation Sounder (GNOS) onboard first time on the FengYun 3 series C satellite (FY-3C), [...]."

For completeness I suggest to add the reference

> Bai, W. H., Sun, Y. Q., Du, Q. F., Yang, G. L., Yang, Z. D., Zhang, P., Bi, Y. M., Wang, X. Y., Cheng, C., and Han, Y.: An introduction to the FY3 GNOS instrument and mountain-top tests, Atmos. Meas. Tech., 7, 1817–1823, 10.5194/amt-7-1817-2014, 2014.

Page 4, lines 6–7:

> "So far, a large dataset of FY-3 GNOS RO observations has been obtained."

If I understand correctly, GNOS measurements aboard FY-3C started in September 2013. Thus, as of now the available data set should cover more than 3.5 years. I suggest to add a comment clarifying the decision to restrict the data analysis to the time period of three months between October and December 2013.

Page 6, lines 9ff:

> "Specifically, in this study, we use the BDS satellite data as orbital data inputs and outputs, while time-wise also using GPS time for the processing of the BDS data."

I'm not sure I understand this sentence. Is GPS time used for time-tagging of GPS as well as BDS observations? Please explain.

Page 6, eqn. (1), page 7, eqn. (2), and elsewhere:
To avoid a potential misunderstanding, I suggest to define δt_a as the LEO clock error (offset) *at the time of signal reception* and similarly δt^b as the GNSS clock error (offset)

at the time of signal transmission. With this change there is no need to regard δt_a and δt^b as functions and the function arguments in brackets (which might be confused with brackets marking an algebraic expression) could be dropped.

Page 7, lines 13ff:

> "The GNSS satellite orbits (positions and velocities) and the GNSS clock offset estimates [...] are provided by the International GNSS Service [...]."

IGS orbits are provided in a terrestrial reference frame. Here, a (quasi-)inertial true-of-date frame (page 5, section 2.1 "Basic algorithm of the excess phase processing") is used. For clarity, I suggest to add a remark indicating that a corresponding frame transformation has been applied.

Page 8, eqn. (7) and (8):
Which one of the two equations is used in the actual processing?

Page 9, lines 19ff:

> "In order to use that specific reference satellite that most likely has the best signal quality and lowest ionospheric influence, our FY-3C GNOS processing chooses the GNSS satellite with highest elevation angle as the reference satellite."

From Bai et al. (2014) (see reference above) I had assumed that the decision which satellite to track as reference is already taken at the receiver level and not during data processing. Second, it would be interesting to note if the reference satellite is tracked by the occultation or zenith antenna. In the latter case SNR at high elevation angles is expected to be higher at the expense of an additional attitude dependence which must be corrected for. Please clarify.

Page 9, lines 19ff:

> "In practice, less than 0 deg means that there is in fact no reference satellite in view and [...]"

At a (sun-synchronous) orbit height of about 840 km (reference) satellites at elevation angles down to -27° could indeed be visible. Please clarify and/or rephrase the sentence.

Page 10, line 24:

> "In our data processing, a quality control algorithm has been used."

I suggest to quote the fraction of RO events removed by quality control.

Page 12, lines 15ff:

> "The target domain for the comparative statistical analysis is from 5 km to 35 km height [...], since commonly the data quality above 35 km and below 5 km is less good, due to the ionospheric effects and tropospheric multipath effects, respectively [...]"

I assume that the data retrieval is based on geometric optics and wave optical methods (CT, FSI) have not applied. Please clarify.

Pages 25 & 26, Figs. 6 & 7:
From the figure inserts it appears that the analysis is based on the intersection of the SD and ZD data sets and that the intersection contains less events than both, the SD and ZD data set. Why are there 192 (if I counted correctly) events found in the (quality-controlled) SD data set, which did not make it into the ZD set? I suggest to add a clarifying remark.

Page 26 & 27, Figs. 7 & 8:
Why is geopotential height instead of geometric height used as vertical coordinate? Please clarify.

Technical corrections:

Page 7, eqn. (2):
Typo: $\delta\rho^{b}_{a,ion,i} \quad \rightarrow \quad \delta\rho^{c}_{a,ion,i}$

Page 7, eqn. (3):
Typos: $\rho^{a,c}_{a} \quad \rightarrow \quad \rho^{b,c}_{a}$
and the three bracketed expressions need to be squared.

Page 7, eqn. (4):
I suggest to replace the horizontal bars in eqn. (4) ($\overline{r^{b,c}}$ and $\overline{v^{b,c}}$) by a more conventional notation indicating vectors, e.g., $\vec{r}^{\,b,c}$ and $\vec{v}^{\,b,c}$ or $\mathbf{r}^{\,b,c}$ and $\mathbf{v}^{\,b,c}$ (bold-faced font).

Page 8, eqn. (6):
Here, in contrast to eqn. (4), the horizontal bar seems to differentiate between

transmitter and receiver dipole vector. I suggest to clarify the notation.

Page 13, line 10:
There appears to be a reference missing (empty bracket).