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Atmospheric Measurement Techniques Discussions
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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 #4

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General comments:

The paper describes the evaluation of zero-difference processing vs. single-difference processing of BeiDou System (BDS) radio occultation (RO) data collected by the Chineese GNOS instrument on board the FY-C3 satellite.

Although this is not the first paper describing GNOS BDS retrievals, it is the first paper describing the zero- and single-differencing methods in that context, and comparing results using either method. For that reason it should be published in AMT. There are,

however, a number of issues that needs to be addressed in a revised version.

The single- and zero-differencing methods are outlined and their application seems sound, although not all relativistic corrections seem to be adequately described. This, together with small unexpected differences in the results, gives me a grain of uncertainty as to whether the relativistic effects and clock offsets are correctly removed. I elaborate on this in one of the specific comments below.

Comparisons of derived bending angle and refractivity to reference profiles from ECMWF analyses are encouraging, although I do not think the results presented show that GNOS BDS RO data are of such high quality as claimed in the text. The authors mention (top of page 11) that part of the bias in their results could be from differences in vertical geolocation of GNOS and reference profiles (which are from ECMWF analyses and radiosondes). I'm not sure that such differences could give rise to the biases that are shown, but if so, such systematic difference should be better understood in the presented data set and possibly corrected. I am not aware of bias problems in the data evaluation of GPS RO data from other sources, e.g., COSMIC/CDAAC or Metop/EUMETSAT. In my comments to the results in Fig. 6 and 7 below, I point to a few other issues that are not mentioned in the text, but which should at least be discussed if improvement of the results in a revised manuscript is not possible.

If improvement is not possible, then some of the statements in the paper should be toned down, e.g., in the abstract where it says that "The statistical evaluation against these reference data shows that the results from single- and zero-difference processing are consistent in both bias and standard deviation, clearly demonstrating the feasibility of zero-differencing for GNOS BDS RO observations.", or at the end of the abstract where it says "The validation results establish that GNOS can provide, on top of GPS RO profiles, accurate and precise BDS RO profiles both from single- and zero-difference processing." Although the GNOS BDS RO data might be of a very high quality comparable to that of GPS RO data, such claims are not fully supported by the results in this paper.

One thing that could give more confidence in the single- and zero-differencing results would be to show cases of ionospheric corrected excess phases at very high altitudes. Although the ionospheric correction in the processing is done at the bending angle level (at common impact parameters of B1 and B2 bending angles), excess phase data corrected at the same times could be shown for cases where the ionospheric residual is small (this could be based on the difference between B1 and B2 excess phases, choosing only cases where such difference/variation is small). If such cases, at altitudes above $\sim$100 km, show virtually no slope (giving confidence that the relativistic effects and clock offsets are correctly removed), and only noise at the level indicated in Fig. 3, then that would give added confidence in the quality of the data. A few examples together with statistical evidence that ionospheric corrected excess phases at high altitudes are virtually flat compared to the random noise, would make a very good case.

Unfortunately, there is no method description of the derivation of bending angle and refractivity. Could such description be added (possibly just with reference to previous works)?

A few additional questions comes to mind: Are there both setting and rising occultations in the statistics, and how many of each? How far down is the B2 signal typically tracked in rising and setting? How far up are the signals typically tracked? Are extrapolation of B1-B2 performed in the troposphere to extend profiles down to where B1 is tracked (if it is tracked lower than B2)?

Below I give specific comments and technical corrections with <page>/<line> referring to the pdf copy of the manuscript. In some places I give suggestions for improved language that could easy the readability, but not in all places where such improvement could be warranted. Suggested words are in square brackets. I kindly urge the authors to run the manuscript by a person with excellent skills in the English language.

Specific comments and Technical corrections:

1/3: Consider a small change to the title: "... data [from] the FY-3C GNOS mission"

1/22: "[The] GNOS ..."

1/26: "... on [the] FY-3C GNOS, [and] thus ..."

2/12: Skip "as small as".

2/13-14: Bad syntax: "including for the GEO, IGSO, and MEO subsets.". Could be skipped here, since you already indicated earlier in the abstract that the data are from these three sub-systems.

2/14-15: "as may be expected from its lower vulnerability to noise." could also be skipped here.

2/17: "... satellites [can] thus provide..."

2/24-26: Move "Earth's" to before "atmospheric parameters...".

3/9: "LEO" is not previously defined.

3/20-23: I suggest reformulation, e.g.: "One of these LEO missions is the FengYun 3 series C satellite (FY-3C), carrying China's first GNSS Occultation Sounder (GNOS) (Liao et al., 2016). FY-3C was successfully launched on 23 September 2013."

3/28: I suggest reformulation, e.g.: "... satellites, the next being FY-3D, scheduled for launch in 2017, will also carry GNOS instruments, similar to ..."

4/1-3: Please reformulate. Are the antennas considered part of the instrument (line 1) or are they used by the instrument (lines 2-3)?

4/4: "... in [the] GNOS design.".

4/5: "... from Earth's surface ..."

4/13: Replace "it" with "the single-difference method".

4/15: "... [the] single-difference ..."

4/15-16: Redundant information (and bad syntax) that could be skipped: "during the GPS clock offset estimation process."

4/17: "... needs no ground station data, [the] processing is simpler".

4/24: "...requires that the LEO receiver [is equipped with] an ...".

4/26: "... is [equipped with] such ...".

4/31: "... received [the signals from five] geostationary ... (MEO) orbit satellites.".

Section 1: Perhaps you could mention the B1 and B2 frequencies somewhere in the introduction.

Section 1: Perhaps you could mention the different semimajor axes and inclination of the GEO, IGSO, and MEO sub-systems somewhere in the introduction.

5/23: "Recently, [because of] its higher complexity ...".

6/1: "The inputs to [the processing] ...".

6/20: "... for [the] receiver clock and [the] GNSS satellite clock ...".

6/9-11: I do not understand this sentence: "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." Please clarify.

6/13: "(in units of [length]) at [carrier signal] i". (also in first line of page 7)

6/16: Is there a reference for eq. (1)?

6/17: Skip "(m/s)".

7/4: Superscript on last term in eq. (2) should be "c".

7/10: Superscript "a" should be "b" on the left-hand side of eq. (3).

7/12: Shouldn't it be capital letters "B or C" here?

7/17: Please provide a reference for eq. (4). You say that this is a "periodic relativistic effect", but does not mention the main part of the relativistic correction, and it is therefore unclear if you make all the necessary corrections. If I understand relativistic effects in the GPS correctly, then eq. (4) is a residual that comes about because the GPS transmitters have their clocks adjusted prior to launch, such that the GPS clocks in orbit beat at the same rate as a clock on the Earth (Ashby, Relativity in the Global Positioning System, Living Rev. Relativity, 6, (2003), 1, http://www.livingreviews.org/lrr-2003-1). However, part of that adjustment of the transmitter clock results in an additional frequency shift in ECI that must be taken into account in the zero-differencing (it cancel in the single-differencing). The shift is proportional to the effective gravitational potential at the surface of the rotating Earth, and is actually larger then the relativistic effect in orbit that would have been without this clock adjustment. See, e.g., eq. (46) in Ashby (2003). You should make clear how you make this additional correction. Ashby describes the relativistic effects in the GPS. Are clocks in the BDS similarly adjusted before launch? If so, it would be interesting if you could give the different values of the frequency adjustments in the BDS subsets (GEO, IGSO, and MEO). Also, eq. (4) is relevant for GNSS clocks (as you write), but what about the relativistic effects of the FY-3C satellite clock? They do not seem to be described? Nor is it mentioned how they are estimated in the zero-differencing. Again, eq. (46) in Ashby (2003) could be of help here. In any case, you should make clear how you estimate all the main relativistic effects and clock offsets (please also make clear whether you consider the correction for the transmitter clock adjustment part of the clock offset or part of the relativistic effects).

7/18: Shouldn't there be bars above r and v here?

7/18: Shouldn't it be "GNSS" instead of "GPS"?

7/20: Please provide a reference for eq. (5).

7/20: Subscript "r" should be "a" five places in eq. (5).

8/2: I think eq. (6) needs to be multiplied by the i'th wavelength to be consistent with the terms in eq. (1) and (2).

8/3: In eq. (4) bars were used to indicate vectors, so it is unfortunate here to distinguish the two effective dipole vectors by a bar on one, but not the other. I suggest to use another distinction and consistently use bars to indicate vectors.

Section 2.1: Generally, It would make good sense to mention the order of magnitude of different corrections and their relative importance.

8/6: I suggest skipping "adopt".

8/10: I suggest to remove "employing Eq. (2),".

8/14-16: Some of the "a" and "c" subscripts and superscripts on the right-hand sides of eq. (7) and (8) should be interchanged.

8/14-16: I suggest the use of different symbols in eq. (7) and (8) (and similar in eq. 9) for the phases on the right-hand side, since these are corrected for the effects mentioned in the first paragraph of this section. Perhaps you could simply use a tilde to indicate that they are not strictly the same as the ones in eq. (1) and (2), and at the end of the first paragraph in section 2.2 (line 9) you could write something like: "In the following we refer to these as <symbol_ab> and <symbol_ac>, respectively."

8/18: I suggest replacing ". c1 and c2 are just" with "are".

9/18: You could say "mentioned" instead of "aforementioned".

9/21: Could the occulting and reference satellites be from two different sub-systems, e.g., a MEO as reference for an occulting GEO?

10/1-2: It is not clear how you calculate the "carrier phase observation error standard deviation" shown in Fig. 3. Are you applying a high-pass filter? With what band-width?

11/24 (and other places): You could use the word "difference" instead of "error".

11/25: Use a mathematical symbol for bending angle in eq. (11) (BA is an abbreviation, not a symbol).

11/28: You could here introduce the use of "Bias" and "StdDev" as they are used later in the text: "... estimates of biases (Bias) and standard deviations (StdDev) are illustrated ...".

12/27-28: I do not understand the sentence in parenthesis: "though more standard deviation suppression might be expected from avoiding the reference link computation". It is not clear what "standard deviation suppression" mean, and I'm not sure if this statement is different from what you just said in the sentence before? Using the word "though" indicates that it is contradicting what you said before. Please clarify.

12/28: Schreiner et al. 2009 is not in the reference list. Should perhaps be 2010.

13/3: "Scherllin" instead of "Scherrlin".

13/10: Empty parenthesis. Perhaps a reference is missing.

13/11-12: Is it really the first time that RO retrievals from other than the BDS MEO is demonstrated? Liao et al. (2016) also describes the GNOS-BDS occultation coverage using BDS GEO and IGSO, and I could not find any indication in their paper that the statistics they show is only from MEO occultations. If it is the first time, then you should here make clear that the results in Liao et al. (2016) did not include GEO and IGSO occultations.

14/3: You say that GNOS BDS retrievals are comparable to GPS retrievals, but you have not really shown that here. Either you should show comparisons to GPS retrievals, or you need to support such statements with citations to previous works.

14/5: "... not [only] on MEO satellites but [also] on GEO and IGSO satellites.".

14/26: You say that "Single-differencing does not need to correct the receiver clock offset". I know what you mean, but it is not strictly correct. The receiver clock offset is removed because it cancel in the single-differencing. Please reformulate.

15/2-4: You say that in the zero-differencing there can be some residual errors after the clock offset correction, but you have not shown that anywhere. Can you give examples of such residuals?

16/4: It should be "Allan", not "Allen".

16/5: Please reformulate the statement on the Allan variance (or deviation) here. It is correctly formulated in the abstract. The unit is not second.

16/10-12: The last paragraph should be reformulated or removed. It is unclear what "in this context of the leading instruments" means.

Results shown in Figure 6:

1) StdDev: I would have expected visible differences between single- and zero-differencing to be only at high altitudes/impact heights. However, even below 10 km it seems that the StdDevs are significantly different, with the zero-differencing results generally having the larger StdDev. How can that be explained? From the legend it appears that it is exactly the same number of occultations involved (and I assume therefore that it is the same occultations). Also, it seems that the StdDev starts increasing already at 20-25 km. I would have expected the increase to start a bit higher when I compare with GPS RO statistics from other sources.

2) Bias: The differences in bias between single- and zero-differencing are similar below 20 km, and the somewhat larger negative bias for GEO can probably be explained by the fact that all the GEO occultations are at very high altitudes (and for some reason that gives a larger negative bias when compared to ECMWF). However, above 20 km, the biases for the three subsystems (MEO, IGSO, GEO) are diverging more for the zero-differencing results, and in particular the bias for the GEO occultations becomes more negative than it is for single-differencing. Why?

These issues needs to be discussed in the text.

Results shown in Figure 7:

The same comments as above applies here, but additionally, it is very strange to see the bias for the GEO occultations for single-diffencing at high altitudes being more positive than the others. This is inconsistent with the biases in the bending angle. It is critically important to understand this, since you are trying to make the point that zero-differencing has lower StdDev than single-differencing, but it is difficult to have confidence in the results if there are such inconsistencies in the biases.

Figure 8 axes labels: I suggest to redo this figure with labels as in Figure 7 ("R%" does not make sense; "geop" should be "Geopotential height").

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