Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-177-AC4, 2017

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.

Weihua Bai et al.

liucongliang1985@gmail.com

Received and published: 13 November 2017

We thank the referees very much for the constructive comments and recommendations and for the overall positive rating that this is a significant scientific paper. We thoroughly considered all comments and carefully revised the manuscript accounting for most of them. In addition, we carefully complemented these revisions with a range of further improvements throughout the manuscript text in the spirit of the comments.

(Please read the amt-2017-177-supplement.pdf by the link at end of this document, in

Printer-friendly version

Discussion paper

which you can find the response to all the referees and the revised manuscript)

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 Chinese 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.

Thank you.

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 geo-location 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. Thank you for these frank yet constructive comments. We agree that this paper is just an initial study to evaluate and show what we found to be a quite good quality already of the new GNOS BDS RO data. But of course, it clearly has further improvement potential from more rigorous future analysis, and we plan to do this by follow-on work. We therefore took your suggestions serious, related to the scope and limitations of results of this initial paper, and carefully considered to tone down some statements in the paper; and we did so for several statements. For example, in the abstract we now say “are reasonably consistent” (instead of “are consistent”) and “the validation results indicate” (instead of “the validation results establish”), etc.

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 _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)? We agree it is basically a very good suggestion to look into such ionosphere-corrected excess phases at high altitudes, complementary to the current upper troposphere/lower stratosphere (UTLS) validation and with more rigor. In this initial study on GNOS BDS RO evaluation we preferred to focus on UTLS validation against GNOS-independent reference data, however, as we started to do in previous papers (e.g., Liao et al. AMT 2016) for GNOS GPS RO evaluation. As noted in the first response above, we agree that this paper is an initial step only and that more rigorous inspection of small residual errors, of different possible sources within the orbit determination and excess phase processing, are needed and will be done by us in follow-on work. For the retrieval of bending angle and refractivity profiles from the excess phase data, we used the ROPP software (available from the European ROM SAF consortium); we added a clarifying sentence to this end in section 3.2.

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)? Response: "Are there both setting and rising occultations in the statistics, and how many of each?" Yes, the numbers of rising and setting are around half of the total number in Fig.1b, i.e., the contribution of setting and rising events to the total number is about the same. "How far down is the B2 signal typically tracked in rising and setting? How far up are the signals typically tracked?" the B2 signal typically could track down to about 5 km, near half of them could track down to about 3 km, few of them could track down to 2 km or more. “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)?” We did not use the B1-B2 extrapolation in the BDS processing so far, but we consider to do it in future and will evaluate this further.

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" Ok, done.

1/22: "[The] GNOS ..." Ok, done.

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

2/12: Skip "as small as". Ok, done.

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. Ok, done.

2/14-15: "as may be expected from its lower vulnerability to noise." could also be skipped here. Thank you for the suggestion, we carefully considered it but we then preferred to keep this sentence here, to explain the potential reason.

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

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

3/9: "LEO" is not previously defined. Ok, done.

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." Thank you, done.

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 ..." Thank you, done.

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)? Yes, all these antennas are used by the instrument.

4/4: "... in [the] GNOS design.". Ok, done.

4/5: "... from Earth's surface ..." Ok, done.

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

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

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

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

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

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

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

Section 1: Perhaps you could mention the B1 and B2 frequencies somewhere in the introduction. Section 1: Perhaps you could mention the different semi major axes and inclination of the GEO, IGSO, and MEO sub-systems somewhere in the introduction. Ok, done.

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

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

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

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

Printer-friendly version

Discussion paper

for the processing of the BDS data." Please clarify. It means, the GPS time is used for time-tagging also of the BDS observations, while the transmitter orbit data are of course the BDS data. We have clarified the sentence in the text.

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

6/16: Is there a reference for eq. (1)? Ok, done; we included Schreiner et al. (2010) now: Schreiner, W., Rocken, C., Sokolovskiy, S., and Hunt, D.: Quality assessment of COSMIC/FORMOSAT-3 GPS radio occultation data derived from single- and double-difference atmospheric excess phase processing. GPS Solut., 14, 13-22, doi:10.1007/s10291-009-0132-5, 2010.

6/17: Skip "(m/s)" Ok, done.

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

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

7/12: Shouldn't it be capital letters "B or C" here? Thank you, we chose to revise the labels 'A B C' to 'a b c' in Figure 1, to be more easily consistent everywhere.

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). Ok, done as good as we could for now. The reference for Eq. (4) is as well: Schreiner, W., Rocken, C., Sokolovskiy, S., and Hunt, D.: Quality assessment of COSMIC/FORMOSAT-3 GPS radio occultation data derived from single- and double-difference atmospheric excess phase processing. GPS Solut., 14, 13-22, doi:10.1007/s10291-009-0132-5, 2010. And yes, the clocks of BDS are similarly adjusted prior to launch as for GPS. So the same equations as for GPS can be used for BDS data processing. In terms of the values of frequency adjustments, they depend on the orbit altitudes, BDS MEO satellite are set closely similar as GPS satellite; and the BDS GEO and IGSO satellite clocks are set to slightly different values. Currently in our data processing, we did not consider the LEO satellite relativistic effects but investigate in this direction for future updates. We have included this type of explanations in the text below Eq. (4) now.

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

7/18: Shouldn't it be "GNSS" instead of "GPS"? Ok, done.

7/20: Please provide a reference for eq. (5). Ok done. It is again Schreiner et al (2010): Schreiner, W., Rocken, C., Sokolovskiy, S., and Hunt, D.: Quality assessment of COSMIC/FORMOSAT-3 GPS radio occultation data derived from single- and double-difference atmospheric excess phase processing. GPS Solut., 14, 13-22, doi:10.1007/s10291-009-0132-5, 2010.

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

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). Ok, done.

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. Ok, done. We have used the bold italic letters as vectors.

Section 2.1: Generally, It would make good sense to mention the order of magnitude of different corrections and their relative importance. Now that we have the references included we considered it makes the text less concise to read if we include also this. Also for the purpose of this paper it is an introductory description to the excess phase equations; in the follow-on study looking in detail into the improvement of remaining small residual errors in the excess phase processing we would of course intend to describe these aspects in more detail.

8/6: I suggest skipping "adopt". Ok, done.

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

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. Ok, done.

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." Ok, done.

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

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

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

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? Yes, in the processing a high-pass filter has been used and the band-width chosen was seven seconds. Please refer to: Oliver Montenbruck, Yago Andres, Heike Bock, et al. (2008); Tracking and orbit determination performance of the GRAS instrument on MetOp-A. GPS Solut., 289-299.

11/24 (and other places): You could use the word "difference" instead of "error". We considered this, but given the rest of the relevant notation as used in this paper, we preferred to keep the current terminology also here.

11/25: Use a mathematical symbol for bending angle in eq. (11) (BA is an abbreviation, not a symbol). Ok, done, we use the greek symbol Alpha now, which is often used for bending angle in the RO community.

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 ...". Ok, done.

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. Ok, clarified.

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

13/3: "Scherllin" instead of "Scherrlin". Ok, done.

13/10: Empty parenthesis. Perhaps a reference is missing. Ok, a typo was left, corrected.

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. Ok, revised.

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. We agree that comparing with the GNOS GPS RO retrievals to validate BDS RO retrievals is a good idea and a potential complementary way to do the FY-3C GNOS RO data evaluation. However, the radiosonde observations and the ECMWF analysis data are reliable GNOS-independent data, which have previously been used as reference data to also validate GPS RO retrievals. Therefore, for this initial GNOS BDS evaluation we selected the radiosonde and ECMWF data as preferred source to use as reference to validate the BDS RO. Nevertheless, since we could achieve a limited collocation ensemble of BDS RO and GPS RO, we included one BDS vs. GPS intercomparison figure now (in section 3.2.), which shows reasonably high consistency. Of course, further improvements and a detailed intercomparison analysis of the GPS and BDS RO data is a very worthwhile next study as well, and we plan to do it by an extra paper.

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

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. Thank you, done.

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? After the LEO clock correction in zero-difference, the LEO clock left a residual errors, which could be estimated by stability of the LEO clock. We can use the Allan deviation to describe it; for more information you can refer to Figure 10 in "Cai Y, Bai W, Wang X, et al. In-Orbit Performance of GNOS on-board FY3-C and the Enhancements for FY3-D Satellite. Advances in Space Research, 2017."

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

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. Ok, done.

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

Results shown in Figure 6: 1) StdDev: I would have expected visible differences between single- and zerodifferencing 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. For the StdDev profile differences below about 10 km and above about 20-25 km, the reasons are likely somewhat complicated (several possible sources in the excess phase processing, also BDS ephemeris, esp. for GEO) and we are analyzing this type of differences in follow-on work to this initial study. We have included a clarifying sentence explicitly pointing to this. On the “same number of occultations”, yes, both ZD and SD involve the same RO events.

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. See the previous response above, which in general also holds here; more detailed follow-on work will clarify the more subtle differences. We have include an additional sentence related to the specifics of the GEO RO events now as well, pointing to the fact that the GEO orbit determination is the most challenging from all BDS and that the GEO RO events are restricted to high latitudes only (as visible in Fig. 5), i.e., a potential regional selection effect.

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. On top of what is said above related to the bending angle Figure, we also included a sentence here for refractivity, pointing to the follow-on work for detailed error analysis and to the specifics of the GEO results.

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"). Ok, done.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2017-177/amt-2017-177-AC4-supplement.pdf

---

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-177, 2017.

Printer-friendly version

Discussion paper