Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-159-RC1, 2017

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Integrating uncertainty propagation in GNSS radio occultation retrieval: from excess phase to atmospheric bending angle profiles" *by* Jakob Schwarz et al.

Anonymous Referee #1

Received and published: 11 July 2017

General comments

The paper details the algorithms, the underlying assumptions and the results of the uncertainties estimation (random and systematic) in the GNSS radio occultation (RO) retrieval, from the excess-phases to bending angle profiles. The paper, together with other similar papers which address the same topic once applied to the other portion of the entire RO processing chain (thus describing error propagation through the POD solution and through the retrieval steps of atmospheric profiles starting from bending angles), provided for the first time a rigorous mathematical overview of how errors are propagated through the entire RO processing chain. From the scientific point of view

the paper is almost complete; involved data, algorithms, results, discussion and conclusions are presented in a well structured and clear way, even if the paper is excessively long.

In what follows some general comments are provided.

1. One of the most critical step of the RO processing chain is the retrieval of bending angles in the lower troposphere, which involves more "advanced" methods (the so called Wave Optic retrieval). Unfortunately error propagation through this step is not detailed (even if a paper is already available since 2015, it is written a couple of times in the manuscript that the implementation of WO uncertainty propagation in the rOPS is not yet finished... so it is not fully clear whether the results are already available or not). In order to have a complete and more coherent overview, this part is somehow necessary. My suggestion is, if results are not available yet, to remove from this paper all the discussions provided on the WO retrieval and error propagation through it (Sect 3.2.2, Sect 3.2.3, Annex A2.2, Annex A2.3), changing a bit also the title reflecting that only GO retrieval is applied.

2. Another general issue I want to highlight is that the entire discussion and the main results shown from Fig 3 to Fig 8 are based considering an excess-phases "time series" and associated random/systematic uncertainties observed during one real occultation event from the COSMIC mission, taken as representative for the entire day. Not sure that the heuristic associated to the choice of that particular profile is correct, since the ensemble mean profiles of the uncertainties associated to the various variables involved in the retrieval (Fig 10 and Fig 11) show quite different behavior (see for example the increasing of uncertainties with height). It would be nice to have different examples (maybe the best and the wrong together with this most representative one).

3. Regarding the same input profile I found a bit inconsistent the choice of extrapolating downward 30 km the representative excess-phase random uncertainty profile (defined on the above mentioned COSMIC observation) with a linear behavior (I guess linear in

order to provide a "worst" boundary, could you confirm this in the paper?), whose gradient follows some estimate defined for the GRAS instrument on board METOP satellites (and I don't think that in the provided reference [ESA/EUMETSAT] such information can be easily found). The systematic uncertainty on the same input variable is also defined following error estimates characterizing the GRAS receiver. The big question is why you mixed the random uncertainty "rigorously" derived considering a COSMIC observations with some heuristic based on the GRAS instrument? Was not better to derive the random error profile from GRAS real observations? And why for the GRAS case you demonstrated the uncertainty propagation using simulated data only? Please provide a clear motivation on this or repeat the entire analysis using some representative examples taken by GRAS excess-phases.

4. And, always on the definition of the input random uncertainty example profile, does it takes into account the merging of open loop and closed loop data somewhere in the lower part of the profile? I guess that the uncertainties related to the open loop and closed loop tracking should be different (because of different noise levels added by the different tracking behavior, because of different sampling rates between OL and CL, which is different from COSMIC and GRAS for example). Maybe the "boundary" set with the linear extrapolation downward the 30 km includes already these different uncertainty levels. But I expect that a discussion on these aspect will be added in the paper.

5. All the analysis is based on the covariance matrix propagation through linear (or linearized) operators. This is fine. But what are the residuals effects due to the linearization? I guess that the most "critical" linearization is the one applied to impact parameter retrieval (in GO). In that case I guess you can quantify the second/higher order effects by considering more terms in the Taylor's expansion of the operator.

6. Finally, the most important references related the main topic of the paper (the description of rOPS and of the algorithms involved) are presentations to conferences or not accessible technical reports. This is a pity. But at least, for the presentations it

would be nice to have the full web-link specified.

The paper is absolutely worth to be published, but some review/clarifications should be provided. That's why I suggest for a major review.

Specific and technical comments are provided in the next two sections.

Specific comments

Page 2

#6: what does it mean that "the accuracy is also ensured on-orbit"? please specify

#21-22: remove references to unpublished papers or to papers in preparation. It is already mentioned that work is on-going.

Page 3

31: first mention of the use of simulated data for GRAS.

Page 4

2.1 Methods: all the analysis is based on evaluating random/systematic uncertainties as complete independent processes. Could you please explain why the two are completely decoupled? This is probably trivial under the assumption that noise is normally distributed. But is this really the case starting from excess-phases?

#9-10: not clear why the effects of temporal/spatial variations (which kind of variations have you in mind?) in repeated observations can be estimated based on an individual RO profile. This is one of the main issue. Describe/motivate this better. And, by the way, why this should be true based to oversampling in the RO raw data profile?

#29: what do you mean with ionospheric noise? Here you are describing the noise in excess-phase measurements, so on both L1 and L2. Are you referring to scintillations? I don't think these can be treated as "normally distributed noise". Define this better.

#29: Why random errors on positions/velocity are not considered here (later on, page

5

#30 you introduce such effects within the systematic uncertainties)?

Page 5

#24-25: not clear what "is aligned to a joint resolution..." mean. Please rephrase/clarify.

Page 6

#7: It would be nice to motivate clearly here why you are using simulated data for propagating the uncertainties for the GRAS case.

#13-14: clarify why the "baseband" approach allows to avoid biases from numerical operations on near exponentially varying profiles (also repeated at page 17, #11-12).

Page 7

#4-7: here is where the downward extrapolation based on a GRAS "model" is described. See point 3 of General Comments.

#4-7: why linearly extrapolating downward and constantly extending upward a "random" uncertainty profile does not introduce a systematic effect?

#8: Please clarify further why all the noise components responsible for random uncertainty at excess-phase level are uncorrelated (also referring to the ionospheric noise). I believe that at carrier phase level noise is uncorrelated (we have basically phase noise). But when geometry is removed as well as clock biases, and when the result is interpolated to match 50 Hz sampling rate (maybe between open and closed loop observations), I'm not sure that the various components are still uncorrelated.

#14: Being he entire analysis based on a COSMIC profile, why for the MC validation you started from a simulated CHAMP excess-phase profile and not from a realistic COSMIC profile? Motivate it.

#24: in defining the input random uncertainty profile, you are computing the excess

phase mean value between 60-70 km. Is the constant part of the systematic uncertainty consistent with such a mean?

#Eq 6 and 8: Explain why the "gradient" (1/3exp(xx)) for the random and the systematic uncertainties below zGrad is different.

Page 9

#20-21: not clear the sentence. By the way, it would be nice to show the relative error (to the state profile) instead the absolute one.

Page 10

#25: What is written there is true, but this is not clear whether these results (and the merging with bending angles uncertainties derived by GO processing) are available or not (#3-5 p 11 it is clearly stated that their integration in rOPS is not yet finished). I suggest to remove any reference to uncertainty propagation through WO/merging unless the results can be shown. See general comment 1).

Page 11

#6-8: this is definitely not clear. How you can "artificially" substitute WO with GO uncertainty propagation, being the vertical domain of their applicability different?

#17 and 20: Not clear why the low-pass filtering of alphaM1 provides so different random uncertainty levels and broaden the correlation functions if the cut-off frequency is the same of the one already used to filter the corresponding excess-phases. I expected negligible or a smaller effect.

Page 12

#23: I found inconsistent defining the input data for the MC simulations by using a "background" error-free excess-phase profile taken by a simulated CHAMP event (mentioned not here but at page 7, #14) plus error realizations taken by a COSMIC dataset. The description of this input dataset should be better motivated and clarified.

Moreover the input for the covariance matrix (for the MC simulation) seems to be some standard profile uSTD,Lr not well identified.

#28: Here you are saying that the comparisons between MC simulations and covariance matrix propagation (CP) are carried out in terms of their decomposition (error profiles and correlation functions/lengths). This is fine but it is not clear where the CP-propagated covariance matrix comes from. What is the input? The same COSMIC profile taken as example in the previous discussion? Another excess-phase profile (more likely yes, since the results shown in Fig 9 [red, blue plots] are bit different from the same results shown in the previous figures. In #19, p7, you mentioned that "the same standard profile was used as input for the CP"). Rephrase and clarify the entire introduction to Set. 4.

Page 13

#7: the information provided in Fig 9 are not so clear. First of all are the random error profiles taken to lye between the boundaries provided by blue/orange lines used to plot Lr (thus varying up to 20 mm)? Could you use other styles to plot such information (dotted lines or different colors). Why the area within the LF random uncertainty is colored?

#13-14: why it is "obvious" that the CP delivers the correct off-peak results? Provide a clarification.

#17-21: rephrase a bit the sentence, since it is not clear.

Page 14

#9: Are outliers defined in term of individual random uncertainty profiles? Or input Excess-phases profiles?

Page 16

#7: this reference is a technical report, not accessible. Since it is quite important for

the main topic of the paper, please provide an open reference if available (or avoid to cite e technical report which is not public).

Page 17

Eq. A2, #14: The baseband approach allows the application of a certain model (in this case the excess-phase filter) to a delta variable, which is the original variable minus a zero-order model of that variable. In this particular case the excess-phase model is obtained by forward propagating ECMWF atmospheric field. Totally fine. My issue is that the filter is then applied to the delta profile only, and the filtered "delta" excess-phase is then added back to the excess-phase model. High frequency components inherently contained in the model are not filtered out. Do I mislead something? Could you better clarify, since the reference points to a presentation given to a congress?

Page 19

Eq A16, #13: the same as before.

Eq A17, #15: The different coefficients identified in the first two and last two lines of AL2D matrix takes care of boundary effects in applying the 5 points derivative scheme. Could you please provide a further clarification on these values or a proper reference? Also, the denominator (12 Delta t) is not so straightforward.

Page 21

#5: about the "mean" tangent point location. How it is computed? Is it the lat/long of the intersection between the GPS-LEO line with WGS84 when the line is tangent to the ellipsoid (Straight Line Tangent Height = 0). Is there any effect on the uncertainty propagation in the bending angle profile due to an error in locating the center of symmetry? Could you provide a clarification on this?

#7: what does it mean "is accurately valid geometrically"?

#9: the impact parameter retrieval is "mildly non-linear". Fine. But what are the effects of the non linear part in the uncertainty propagation? (see also point 5) in general comments).

#31 - 3 p.22: This sentence is crucial but not clear. Is 2% the residual error on the bending angle or impact parameter left to the non-linearity (see above comment)? You are saying that the assumption is reasonable given the high quality of the forward modeled profiles? Why the high quality related to this error?

Page 22

A29, #4: G(za) also in the first member? I'd say that is the linearization error of 2% applied to all the levels?

#7: in Eq A29, the random uncertainty profile ualphaG is already interpolated to common monotonic impact height grid. Why it is repeated here?

#18: "zero-order contributions and no terms higher than...". Is this the Taylor expansion used to linearize A22-A24 (zero-order is already used to identify the model applied to the baseband approach for the state vector retrieval)? Referring to point 5) in the general comment, you can estimate the effects of non-linearity on uncertainty propagation evaluating ualphaG also including higher order terms.

Page 23

A33, #20: why the derivative of alpha wrt theta is not considered?

Page 24 and Page 25

I'd remove both A2.2 and A2.3. See issue 1) in the general comment section And, by the way, the effect of the apparent systematic uncertainty in the lower troposphere seems "crucial" but not still fully accepted (the paper is under review). It is worth to have the WO/merging part properly described elsewhere in the future, as soon as the results associated to the uncertainty propagation through WO algorithms will be available.

Page 27:

#3: here you are talking about a "more advanced form" of alphaL2 extrapolation which is described in a technical report not publicly available. Provide other references if possible or avoid to cite it.

#13: see above. Provide other references if possible, or avoid to cite it.

Page 28:

Eq A48, #10: why the coefficient are squared? Is it because the CP foreseen to multiply the model and its transpose?

#15-17: Clarify this sentence.

#29: the Healy/Culverwell cited paper refers basically to a new ionospheric correction schema. Are there plans to introduce this in rOPS and to evaluate also how uncertainties are propagated through it?

Page 29 # 19: see previous comments on this.

Technical comments

Page 3

#28: typo: validation

Page 5

#28: introduce what the subscript "r" stands for, even if there is a table in support of all the definitions.

Page 7

#24: make reference to Fig 3b)

Page 8

#14: alphaG,k(za) is repeated. Moreover alphaM,k(za) is never shown. Remove it

Page 10

#16: add "making negligible the systematic uncertainty integrated from the phase/Doppler"

Page 13

#10: is this recombined MC covariance matrix the one defined through Eq. 10? If yes, put a reference to the equation.

#28-32: here you provided a mention another possible method for estimate uncertainties along the processing chain (variance propagation). Totally fine. But it would be nice that it is properly introduced somehow in Sect 2.1.

Page 14: "minor channel". Please reword this. In GNSS we do not have minor/major channels. We have channels associated to the lower and higher carrier frequencies or, simply channel associated to the L1 or L2 carrier frequencies. Also Page 26, #4 and #8, Page 27 #8. Page 45, Figure 10 caption (leading ? channel)

Page 19

#9: typo: derivative scheme

Page 22

#7: I'd add: "In the GO approximation, the bending angle values at each grid point only depend on..."

Figures 3-11: It would be nice to have also the uncertainty (random and systematic) plotted as relative values (relative to the state variable profile)

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