Response to the comment of H.Brenot

Interactive comment on "Unconstrained, robust Kalman filtering for GNSS troposphere tomography" by W. Rohm et al.

General remarks:

1. This manuscript presents clearly the tomographic technique as well as the robust Kalman filter and the truncation technique applied. A set of sensitivity tests is described. The objective is to show the significant improvement that the use of a combined technique with a robust Kalman filter and a truncation (effective removal of linearly dependent observations and parameters), can bring to an unconstrained approach for GNSS tropospheric tomography. The mathematical problem and the different technical aspects to improve the tomographic adjustment of this ill-conditioned inversion are well presented.

[WR] Thank you very much for this remark.

Nevertheless, I found an error in the consistency between this study and the GNSS technique (see general remarks 2.). This error is in the formulation of the wet refractivity. For this reason it is difficult to have a definitive opinion of the results shown.

[WR] The error has been corrected and as shown in many responses below the new corrected formula for wet refractivity was used.

In addition, I suggest some tests to the authors to show more precisely the different step of this improvement, that the reader can properly see the advantage of this approach (see remarks about section 5.5, Tables and Figures).

[WR] Most of the suggested tests were taken into consideration.

The choice of the reference for these tests is also critical. To validate their method and use the term of validation with real data, the reader needs to have some external/independent clues to justify that this model can be considered as a reference of these tomographic retrievals, specially because the authors uses real GNSS data. Another reference solution (that use tomographic retrievals with real GNSS data considering a priori condition from NWP for all epochs) is also suggested to the authors. If radiosoundings are available during this case study, I suggest to compare the retrievals of this new reference with radiosonde profile to justify this choice.

[WR] As this study is focused on the numerical properties of tomography and new filtering methods we would like to emphasis these achievements. Therefore we choose reference observations to be NWP models analysis step outputs as we do believe the model was tested against ZTDs from GNSS (Rohm et al., 2014) and shows good performance. Only one radiosonde launching site coincide with tomography domain and the time resolution is quite poor, as a result the measures derived from the comparison would be statistically insignificant.

The reader needs also to have information about the geometrical resolution (i.e. 41 percentage of voxel resolved per altitude). A precise description of the different statistical parameters show Table 2 (e.g. bias, standard deviation, a posteriori RMS of SWD, Processing uncertainty) is also required.

[WR] The statistical description has been appended to the Table 2. The number of resolved parameters is constant (as the number and direction of SWD observation is constant) for all types of

tests. The number of resolved voxels varies between 33% in the bottom layer to the more than 80% in the 5th from the ground (~4000m).

2. To choose NWP estimation of the wet refractivity as a reference for their sensitivity tests, is the choice of the authors. Without any external observations, a validation of the improvement of the GNSS tomography with real data can appear a bit ambiguous with this reference. The authors need to explain why their choice of reference is fine to validate their method.

[WR] The reason behind using NWP are twofold. Firstly, the number of available radiosonde profiles in Victoria is very limited, there is only one launching site - in Melbourne, moreover the time resolution of data is low (i.e. 12 hours) which makes deriving any statistics based on the radiosonde profile comparison irrelevant for that short time period. Secondly, the NWP model quality has been verified with the ZTD estimates from multiple ground-based stations (see Rohm et al., 2014 for details), across Victoria. The agreement between GNSS and NWP data was found to be on the level of 10 mm (RMS). In the view of good temporal and excellent spatial coverage we decided to use NWP as a reference.

Nevertheless, there is a problem in the tomographic comparison shown in this manuscript. The reference chosen is not well estimated. This is not consistent with the GNSS technique. The authors compare a tomography from real SWDGNSS GNSS data with simulated SWDNWP data that are not equivalent:

$SWDGNSS = STDGNSS - SHD(Pground) \sim \int Nw \, ds$

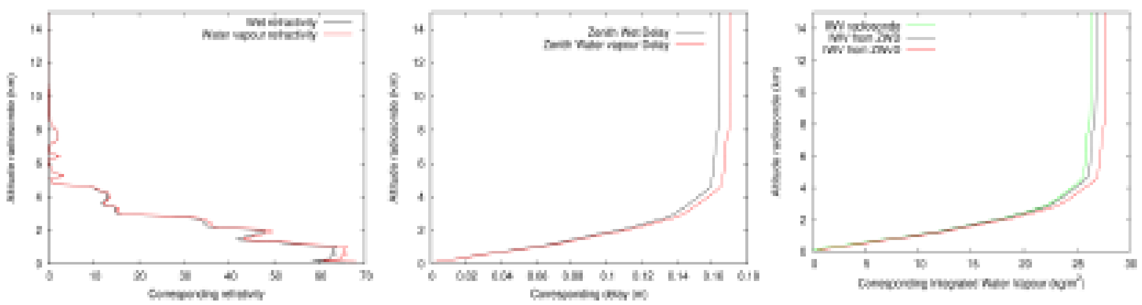
where $Nw = k'2\ e/T + k3\ e/T^2$, with $k'2 = k2 - k1\ Rd/Rw$

The good formulation of simulated data is: $SWDNWP = \int Nw \, ds$

But the authors use this formulation in this study: $SWvDNWP = \int Nwv \, ds$ with $Nwv = k2\ e/T + k3\ e/T^2$

[WR] We are very grateful that you've picked that up. We have to agree with your derivation and therefore we reprocessed simulation data and tomography retrieval based on these data.

To be consistent with the GNSS technique, I recommend to the authors to reprocess the tomographic results from simulated SWDNWP with the good formulation, to update the equation. The authors need also to recalculate all the statistical results.

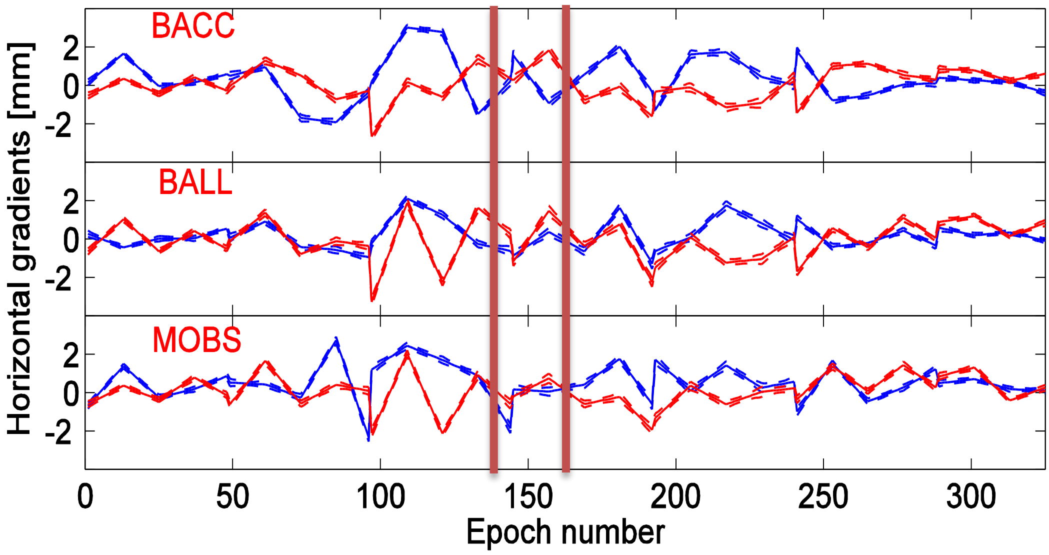


To convince the authors, I show here an example the equivalent wet refractivity, the zenith wet delay (ZWD) and the integrated water vapour (IWV) established from a radiosonde profile (rainfall event of 2005/06/29, 12H UTC Brussels). The wet and water vapour refractivity are shown on the left. The corresponding wet and water vapour delays are presented in the middle plot. I stop voluntary the radiosonde profile at 15 km because there is no significant water vapour contribution to delays over. The authors can see on the right plot the IWV estimated from water vapour density (IWV radiosonde in green line). Using the conversion factor □(Tground) from Bevis et al. (1992) that depends on the ground temperature (Tground), I show the equivalent IWV estimated from the ZWD (as recommended by the GNSS technique), and the equivalent IWV estimated from the zenith water vapour delay (ZWvD) that the authors consider by error in this study.

[WR] Thank you for that comprehensive and convincing example as to why we need to use consistent Nw data. We've reprocessed all tomographic solution. However, inspecting provided figures we've come to conclusion that the difference in terms of wet refractivity should not be larger than 3mm/km in the troposphere boundary layer and the discrepancy reduces to almost 0mm/km in the first 4 kilometers. This imply that the difference introduced by this discrepancy and measured as a mean bias for all 325 epochs and all vertical levels (only one third affected) will not exceed 1mm/km and should not reflect on solution scatter.

3. This study used synthetic and real data. Because the final goal is to apply this technique to real data (meteorological and positioning applications), it is important that the reader can have an idea of the quality of the data used. For example, horizontal delay gradients are relevant observations of the anisotropy of the troposphere induced by the water vapour around a station. Gradients should be included in this study only if these show a relevant information of this anisotropy. For this reason, the reader needs to know the quality of these GNSS delay gradients. If the measurements of gradients do not contain any significant information, this should be specified and/or these should not be used in this study.

[WR] The horizontal gradients were estimated together with ZTDs, the estimation time resolution for gradients were consistent with the ZTDs estimation time (30 minutes). As shown on the figures below there is a strong connection with storm passage and the gradients increase magnitude in time, and shifts direction. The red vertical lines coincide with the beginning and the end of a major storm. As it can be seen in Rohm et al., (2014) the increase of ZTD happens a day before a storm. This is also reproduced in the gradients that are larger before storm. The quality of gradients estimation, i.e. formal estimation error as shown on the figure (dashed line) is relatively small compared to the gradients values. In conclusion gradients contain important information, however the scale of this



information might not coincide with scale of the voxel, therefore we might not be able to exploit its full potential. This requires further studies.

Major issues:
Abstract

The authors mention “that unconstrained solutions are feasible by using a robust Kalman filtering technique and effective removal of linearly dependent observations and parameters“. If I well understand approach: tomographic retrievals are compared to reference refractivities (established from NWP). The reader needs to have some external/independent clues to justify that this model can be considered as a reference of these tomographic retrievals, specially because it uses real data. Maybe the authors can compare their retrievals with radiosonde profile. See comment of section 5.5.

[WR] As mentioned above we decided to compare our data to NWP outputs as it provides good time (6h) resolution and excellent horizontal resolution, conversely the radiosonde is launched only in one spot across Victoria and time resolution (12h) is lower by a factor of two. The assimilation system that is running in ACCESS model uses radiosonde profiles as a data input so in a sense this information is already ingested in the model fields. Statistics, derived as a TOMO2 retrievals departures from model states, are based on 6 x 12 x 10 = 720 values at every 6h epoch, so in total approximately 20 thousands. We believe that this is the best possible reference we can get for the study area.

p. 9134, l. 18-20: in the phrase “Therefore the accuracy.. 10 mm.” there is no new information. The fact that 0.06 m of ZWD is equivalent to 10 mm of precipitable water (PW) or 10 kg/m² of IWV is something shown by Askne and Nordius (1987, Radio Science): ZWD = 6.277 * PW. The authors have to specify what they mean exactly with the fact that the accuracy of the integrated values (ZWD or IWV) should not be worse than 60 mm (for ZWD) or 10 mm (for PW). Hopefully this is the case. If the ZWD accuracy is of 30 mm, this is already a non-accurate observations for which no one of us can expect to bring a good tomographic result. This phrase “Therefore the accuracy.. 10 mm.” should be deleted. What the authors should mention in the case of high non-accurate GNSS observations (ex: ZWD of 30 mm) this is the impact on their tomographic retrievals.

[WR] We agree that this section of abstract is too vague and has no support in data, so it has been removed from the text.

1 Introduction

2 GNSS signal delay

p. 9137, l. 18: The Eq. 2 brings a confusion, it looks that Ndry = Nhydro and this is not true.
N = Nd + Nwv 112
with Nd = k1 Pd/T, so called the dry refractivity, and, Nwv = k2 e/T + k3 e/T², so called the non-dry or water vapour refractivity.
N = Nd + Nwv = Nh + Nw
with Nh=k1 P/Tv , so called the hydrostatic refractivity, and, Nw=k’2 e/T + k3 e/T², so called the non-hydrostatic or wet refractivity.

$$\mathrm{N} = \underbrace{k_1 \frac{P}{T_v}}_{N_h} + \underbrace{k_2' \frac{e}{T} + k_3 \frac{e}{T^2}}_{N_w}$$

where Tv is the virtual temperature and k’2=k2 - k1 Rd/Rw
Then the authors obtain the following formula for STD:

$$\mathrm{STD} = \underbrace{10^{-6}\int_0^\infty \left(\mathrm{k}_1\frac{\mathrm{P}}{\mathrm{T}_v}\right)\mathrm{ds}}_{SHD} + \underbrace{10^{-6}\int_0^\infty \left(\mathrm{k'}_2\frac{\mathrm{e}}{\mathrm{T}} + \mathrm{k}_3\frac{\mathrm{e}}{\mathrm{T}^2}\right)\mathrm{ds}}_{SWD}$$

p. 9137, l. 19-20: SHD is the Slant Hydrostatic Delay and not the unused Slant Dry Delay (SDD) that the authors mention. “Usually tomography models utilise the SWD to reconstruct the water vapour distribution” is a true affirmation, because the SWD (from Nw) is used to obtain slant Integrated Water Vapour and to invert water vapour density, and not the unused Slant Water vapour Delay (SWvD = STD - SDD).
The authors have to clarify Eq. 2, and eventually to write: STD = SDD + SWvD
Even Nw is often used for the earlier defined Nwv (see Chap. 4, Kleijer, 2004), the author should avoid wet refractivity and use water vapour refractivity in this manuscript. If the author want to used the term wet refractivity rather than water vapour refractivity, this approximation must be specified. Eventually this could avoid the reprocess of all the calculations of Nw rather than Nwv.
The approximation of SWvD with SWD must be specified or SWD change to SWvD in all the manuscript (section 3, 4, 5, 6, Tables, Figures and captions).

[WR] It is a mistake that has not being found before, so it kind of get rolled over from previous studies. Even as we said above the anticipated impact should not exceed 1 mm/km in wet refractivity it has to be corrected for. All the calculations were re-run. Thank you very much for spotting it. The manuscript reflect your comments and we renamed all the variables and rewritten the Eq. 2 to match the correct formulation of hydrostatic and wet delay (non-hydrostatic) in both processed real data and simulations. We’ve also update the k coefficients to match recent findings in the carbon dioxide concentration (Rueger, 2002a,b).

3 Model structure
4 Kalman filter application
5 Case study
5.1 NWP model
5.2 Common data
p. 9145, l. 8-17: There is error in this paragraph. The whole paragraph need to be reformulate.

[WR] Following suggestions of the first reviewer now this section is called “5.2 GNSS stations network”. As we changed water vapour delay to wet delay and water vapour refractivity to wet refractivity the text is corresponding with the content and do not need to be changed.

p. 9145, l. 8-9: This is wrong because it shows that the authors will compare simulated apple (SWvDNWP) with observed pear (SWDGNSS); see general remarks 2. The authors just need to explain that to simulate wet delay they need to integrated (along the ray satellite-receiver) the wet refractivity estimated from NWP using pressure (P), temperature (T) and partial pressure of water vapour (e) from ACCESS-R. There is no reason to separate dry and wet delay. The NWP model allows a direct calculation of SWD and of Nw, there is no need to separate anything. Only for the GNSS technique the hydrostatic delay needs to be separated from the wet delay. For this reason the authors need to know only the pressure at the location of the GNSS station. The authors need to explain that the P from ACCESS-R is an estimation of the P at the GNSS station (Pground). This is not the best way. The best way is to use collocated measurements from synoptic PTU station with the GNSS station. The authors Just need to explain that they make an assumption/approximation that the (Pground) from ACCESS-R are good enough to be used in the GNSS conversion (of the total delay of the neutral atmosphere to the wet delay of the neutral atmosphere).

[WR] *We’ve corrected the problem with dry and wet delay in the section 2, here we only explained that the ACCESS-A (ACCESS-R is the same model working in different scale) was used as there was no ground based pressure sensor at the GNSS antenna location. The “separation” of dry and wet delay actually means – “calculating ZHD (Saastamoinen, 1972) and substracting this part from ZTD estimates”.*

p. 9145, l. 9-10: To choose the wet refractivity from ACCESS-R as a reference is the choice of the authors, nevertheless as mentioned in the general remarks (2.) the wet refractivity retrieved from GNSS (Nw) is not exactly the water vapour refractivity (Nwv) chosen as a reference. Calculations of SWDNWP and Nw retrievals need to be reprocessed to be comparable with the GNSS ones.

[WR] The section 2 was corrected and data were reprocessed.

5.3 Simulation data

p. 9145, l. 19-21: Wet refractivity Nw is not water vapour refractivity Nwv. The authors have to clarify what they use. Note the same error has been done for the estimations of the wet refractivity in the previous publications of the authors.

[WR] This part has been clarified.

p. 9145, l. 23: Clarify in all the manuscript if this is wet or water vapour refractivity, and if this is SWD or SWvD that the authors use. I recommend to the authors to reprocess SWV from ACCESS-R and Nw from their tomography.

[WR] This part has been clarified.

5.4 Real data

p. 9146, l. 17: I am not convinced that the gradient should be include in this study (see general remark 4).

[WR] I've enclosed the plot showing gradients (solid): well before, just before, during and after storms for 3 stations located close to the storm, plotted with double standard deviation lines (dashed). It clearly shows variability that is linked to the building up of the water vapour.

p. 9146, l. 22-24: Again this is not the dry part, this is the hydrostatic part that is subtracted thanks to the formula of Saastamoinen (1972).

[WR] Corrected

p. 9146, l. 25-28: If the authors want to use gradients in their study, we need to have more details about them (what is the range of their values at 10° of elevation or in the zenith direction). It misses also bibliographic references. How the authors obtain SWD from ZWD, with or without delay gradients. This is needed to clarify. The delay gradients do not seem to bring additional information in this tomography. My feeling is that the authors should not considered them in this study, because that brings nothing to the technique/study presented (delete l. 25-26).

[WR] The gradients show large variability and are significant (statistically speaking). The North component of the gradient is varying between -2.2 and 2.5 mm, whereas the East component is oscillating between -2.4 and 3.5 mm. The estimation formal errors are relatively small, the average value is 0.1 mm. The text in this section has been updated.

p. 9147, l. 5: Confusion between Ak and AG (Eq. 7). Explain that for each epoch A=AG is calculated.

[WR] Corrected

p. 9147, l. 5: Specify to which Eq. RG corresponds.

[WR] Corrected

5.5 Tomography processing results discussion
p. 9147, l. 10-17: To resume the "A priori type" as mention Table 1:
N - NWP outer (all epochs) and inner (first epoch)
G1 - GPT + UNB3m outer (all epochs) and inner (first epoch)
G2 - NWP outer (all epochs) and GPT + UNB3m inner (all epochs)
In my opinion, the authors should add three others "A priori type".
W - NWP outer and inner (all epochs)
G0 - GPT + UNB3m outer (all epochs) and NWP inner (first epoch)
G1' - GPT + UNB3m outer and inner (all epochs)

[WR] The choice of a priori to test was not subjective. Authors had previous experience with outer and inner model formulation, therefore we choose solutions that are the most likely to succeed. Current results shows that the outer model should be populated with precise wet refractivities to effectively model the signal propagation out of the inner tomography domain.

With a comparison between N with G0, the authors can show the impact of the outer a priori with inner a priori only for the first epoch, as well as with a comparison between G'1 with G2.

[WR] The solution has been run and

With a comparison between N with W and between G1 and G'1, the authors can see the impact of the inner a priori (first epoch considered against all epochs)

[WR] The solution has been run and

The authors can just calculate RWASD, RG0ASD, RG1'ASD to obtain these results. In this manuscript, the authors consider Nw from NWP (ACCESS-R) as a reference, that means they think this reference represents a good estimation of the reality. The authors can certainly have access to radiosonde profiles taken during this event. If they compare RWASD and NWP estimations (interpolated at the location of the profile) with the radiosonde profile, they might find that RWASD is closer to the radiosonde profile. If this is the case, the authors can show this result and can consider RWASD as the reference for all their sensitivity tests!

[WR] As mentioned before we think that NWP is sufficiently accurate and provides a large number of reference observations. While there is only one radiosonde site and the number of observations are limited.

I recommend to give the exact definition of the bias μ (bias between which parameters), and the same for the discrepancy (standard deviation of bias of which parameters) shown. The reader needs to know what are the A posteriori RMS of SWD, and the Processing uncertainty of Table 2.

[WR] Table 2 description extended

p. 9147, l. 19-25: As suggested in the general remarks (1.), it could be nice to see more precisely the different sensitivity tests the authors have done and the different step of the improvement they suggest with the use of a robust Kalman filter and a truncation. The authors mix to many different tests on Fig. 6 and 7. Each graph should be dedicated for a specific test. To make it clear, the authors just need to show a test after the other, step by step. I can see 3 categories of tests (with two figures of the bias and the standard deviation):

a) the improvement suggested by the authors step by step (A, S and D); b) the test of the a priori condition (G2, G1 and N); c) the test of the data input (R, Z, M1, M2).

[WR] Additional tests run, figures are replotted to show new results.

Note that to be consistent with Table 1, in the figures and in the text the authors should write the nomenclature in the same order (RNASD rather than RNSAD). Here is the detailed suggestion that the authors could do for these 3 categories of sensitivity tests:

a) Fig. 4 and 5 show this improvement but why RNAOO is not shown.
Like the authors have done for the same real observations with a priori observations from NWP in outer and inner model (N), it could be nice to see on an independent figure the following tests: RNOOO, RNSOO, RNOAO, RNOOD (test of the different contribution A, S and D) and/or eventually RG2OOO, RG2SOO, RG2OAO, RG2OOD. Then also it could be nice to see (c.f. Fig 4. And 5) the tests: RNOOO, RNAOO, RNASO, RNASD (to see gradually the improvement) and/or eventually RG2OOO, RG2AOO, RG2ASO, RG2ASD. See remarks of Table 1.

*[WR] as shown in the equations 17, 18, 19, 20 the Reconditioning step is tightly linked with SWD removal procedure (as well as R rows removal), therefore the step **A00 is not shown and in the proposed strategy in fact does not exist.*

b) About the impact of the a priori type (like the authors have done in Fig. 6 and 7; but it needs to be clearer for the same input data), I suggest to the authors to use real observations for that and to show: RG2ASD, RG1ASD, RNASD and/or eventually RG2OOO, RG1OOO, RNOOO. If the authors choose RWASD as a reference, the reader can expect that RN tests will be close to the reference (at least for the first epoch). Then it will be interesting to see how RG2 and RG1 tests react.

[WR] New figures were plotted.

c) Then, it could be nice to see how the different observations can have an impact on this tomography. If the authors think there is information in the gradients, they can show Z test here (or mention that there is no real additional information; so maybe the authors should just define R as real observations without gradients). I suggest to the authors to show: RNASD, (eventually ZNASD), M1ASD, M2ASD, and/or eventually RNOOO, (eventually ZNOOO) , M1OOO, M2OOO, and RNOOO. It will be also nice to see on this figure the bias (and the standard deviation on another figure, like the authors have done in Fig 4 and 5 and Fig. 6 and 7) of the Nw from ACCESS-R (the one considered as a reference by the authors) and the Nw from UNB3MGPT in comparison with the Nw from RWASD (if the authors agree to use it as a reference).

[WR] New figures were plotted.

p. 9147, l. 26-27: All the statistical results from all these tests should be on Table 2.

[WR] Table was updated.

p. 9148, l. 4-25: This paragraph need to be rewritten with the new results (with the good formula) of Nw biases and standard deviations, and according to the different sensitivity tests. Hopefully the authors might find the same conclusions. A brief description/result of each sensitivity test will help the reader and make the conclusions more precise.

[WR] Conclusion rewritten.

p. 9148, l. 4-6: c.f. test category b).

[WR] Corrected

p. 9148, l. 6-8: The authors write "whenever the a priori value for all epochs and all voxels are set… the quality of the reconstruction is much higher than those in all other cases". How this conclusion can be obtained looking at the standard deviation Fig. 5 and 7? The reader needs an explanation.

[WR] suggested comparison added (compare RG2ASD and ZG2ASD to RNASD and ZNASD), to point out reader to the important figures.

p. 9148, l. 9: The reader needs a description of the content of Table 2 (A posteriori RMS of SWD, and the Processing uncertainty)

[WR] Table description appended.

p. 9148, l. 21-22: Looking at Fig. 4, I do not really understand how the authors can write that the robust filtering helps to reduce noise in outputs. What I see in Fig. 4, this is that RG2ASD is closer to the reference than RG2SAO and RG2OOO. The reader needs more explanations to understand what the authors mean. The authors should avoid the scatter plot in their figure because the reader can not really see the results on the graph (see comments about Figures).

[WR] Figures were replotted, it wasn't logical expression, the Fig.4 does not show noise it shows systematic errors. We've corrected the sentence.

p. 9148, l. 26 – p. 9149, l. 5: This paragraph need to be rewritten with the new results (with the good formula) of Nw bias and standard deviation, and according to the different sensitivity tests. Again, it will be good to have all the results in Table 2.

[WR] Paragraph rewritten.

p. 9148, l. 26-28: With the choice of reference (Nw from ACCESS-R) and considering the test the authors show Table 2, the fact that M1G2SAD (synthetic SWD and a priori considered all epochs) is the best achievable, is what I personally expect. As mention at the beginning of this section (remark p. 9147, l. 10-17), the authors could certainly find that retrievals from M1WSAD will be closer to the ACCCESS-R than M1G2SAD. An interesting conclusion shown be results is that GNSS tomography with UNB3m and GPT looks feasible.

[WR] That was really expected and comparison to this retrieval seem to be good way assessing degree of observation modelling problems and effectiveness of the robust filtering.

p. 9148, l. 28 – p. 9149, l. 1: the authors compare M1G2ASD with M2NASD, but it looks that the tests M2G2ASD or/and M1NASD are missing to conclude anything about M1, M2 and the introduction of the noise in simulated SWD. Note that M1G1ASD is not cited in Table 1. An update of Table 1 is required.

[WR] The comparison of M1NASD (zero noise) to M2NASD (realistic noise) has been appended to the Table 1. It confirms that the conclusions drawn from M1G2ASD and M2NASD were equally valid, i.e. the a priori data works as an additional constraint limiting noise in the output.

p. 9149, l. 4-6: The authors compare RG2ASD with M2NASD. I agree with the fact that for both configuration, the outliers are effectively filtered. But rather than to write that these are equally accurate, maybe the authors can write that after 40 epochs these looks correlate, and that the mean bias and std are equivalent.

[WR] Corrected.

p. 9149, l. 6-11: In the set of GNSS observations the authors use, delay gradients seem not to show any significant observation of the local anisotropy around GNSS station. Can the authors confirm that? Did the authors retrieve these observations them self? If the authors want to use gradients in their study and give conclusion about their use in tomography, the reader needs to have more information and details about this product (setting of gradient calculations, exact format of output). It could be nice to see in this manuscript one of the time-series of ZTD and NS- and EW-component of the gradients to have an opinion about the delay gradients used. If there is no significant information in the delay gradients, rather than to write “using gradients in the signal delay modelling does not improve solution”, the authors should just simply mention that the signal in gradients is weak and that no conclusion can really be given about their use in this tomography. In that case, an updated of Table 1, Table 2, and p. 9147 l. 10-12 is required. Of course, if the authors are convinced that there is a significant signal in the gradients observations, this should be mentioned with the new conclusion (about the results with the new formula of Nw). What is the interpretation of the authors of the discrepancy between RNASD and ZNASD (Fig. 7)?

[WR] Gradients in revised version of the manuscripts are described in greater detail. As we mention before we strongly believe that there is an useful information in the gradients and it is coinciding with the storm passage. However, the scale of the phenomenon captured by gradients could be smaller than the scale of the tomography model, hence the RNASD performs slightly worse than ZNASD. In that case the gradients are actually introducing noise into the system.

p. 9149, l. 13-24: This paragraph need to be rewritten with the new results (with the good formula) of Nw bias and standard deviation. Again, it will be good the authors show all the results of their sensitivity tests in Table 2, and that they summarize these

[WR] Corrected

p. 9149, l. 22: Like I suggest the authors might reprocess all the sensitivity tests (to be consistent with GNSS technique). If the authors decide to keep this study focussing on water vapour refractivity and not on wet refractivity, they should replace -3.6 by -3.5 mm km-1 in this line. I really think the authors present a relevant technique and improvement in GNSS tomography. That could be really nice to see new results in consistency with GNSS technique (good formulation of the wet refractivity).

[WR] New formulation present in the manuscript.

6 Conclusion

Good conclusion but check if the new results are in agreement with this conclusion (update 6.5 mm km-1, p. 9150, l. 10).

[WR] Values updated

References

Tables

p. 9154, Table 1: As mention in the remarks of section 5.5 (p.9147, l. 19-25), in my opinion, if it is possible to dissociate the Kalman filter improvements in 3 categories, the test RNAOO, RNOSO and

RNOOD are missing to see independently the contributions. Note that an error appears in the description of the Kalman filter improvements: this is O – for No and not “N – No”.

[WR] Table corrected.

p. 9155, Table 2: As mention in the remark of section 5.5 about tomographic results

[WR] Table corrected.

p. 9149, l. 10-11, the reader needs to know what are the A posteriori RMS of SWD, and the Processing uncertainty. All the sensitivity tests should be shown on this Table.

[WR] Corrected

Like I mention already, to be consistent with the GNSS technique, the author should reprocess all the tests anyway because the wrong formula of the wet refractivity is used (for the simulated SWD and the a priori Nw). To be consistent with Table 1, the authors should write the nomenclature in the same order (RNASD rather than RNSAD).

[WR] Corrected

In my opinion, the term “Validation by NWP” is not really appropriated. If the authors want to consider the NWP as a reference, maybe they should write in Table 2 “Comparison to the reference” or “Comparison to NWP”

[WR] Table description altered

Figures
Don’t forget to use the same order (see the combinations), for example RNASD every where and not RNSAD.

[WR] Corrected.

Minor issues:

[WR] Authors are very grateful for that comprehensive list of suggested changes in the text. All comments will be taken into account.

Abstract
p. 9134, l. 2: “Limited a priori” should be change to “a systematic a priori information”

[WR] We would like to leave the limited a priori term as it was pointed out by other reviewer that we actually use some constraints (except from the a priori observations).

p. 9134, l. 8-9: Explain more precisely this 1 hPa. Do the authors mean that: the accuracy of the partial pressure of WV profile estimated from the tomographic 3D field of WV density is 1 hPa?

[WR] No, this is not an accuracy it is just formal error (uncertainty) that is achievable under perfect simulated conditions as shown in Rohm (2012).

1 Introduction

p. 9135, l. 20-21: "increasing the number of pseudo observations" is a bit confusing if it concerns the boundary conditions. Specify the meaning of "(and decreasing the condition number)".

[WR] Short explanation given.

p. 9135, l. 27: "even though not all are intercepted by GNSS signals", the authors should then specify what is the role of the a priori condition.

[WR] Important comment I've clarified that this sentence is to do with first point.

2 GNSS signal delay

p. 9137, l. 9: Is that the refractivity coefficient from Thayer (1974) that the authors use? Specify the value of the refractivity coefficient if this is the ones from Kleijer (2004).

[WR] We have used as mentioned in new version of manuscript the values that are listed in the Boehm and Schuh, (2013) after Rüeger (2002a, b).

3 Model structure

p. 9138, l. 11: Replace "The design matrix" by "The design matrix A"

[WR] Corrected

4 Kalman filter application

p. 9139, l. 17-19: Clarify this phrase and the fact that R ￼￼RR; what is the implication of the covariance E(vKvKT)= RR and a robust Kalman filter observations that assumed to be of a normal distribution contaminated with outliers? "Therefore" l.19 is not clear for me.

[WR] Corrected

p. 9140, l. 1: Wet refractivity should be change to water vapour refractivity (in all the manuscript).

[WR] We've reprocessed the data set so we won't need to change wet refractivity to water vapour refractivity.

p. 9140, l. 5: Looking at Yang (2010), maybe the notation RkR could simply be replaced by Rk in all the manuscript?

[WR] We would like to retain the superscript "R" to emphasise the interactive filtering of observations.

p. 9140, l. 5: Why the authors define RkR(-) l.8 that is not properly written l.5 Eq. 10 (only RkR is written). That brings a bit of confusion.

[WR] Corrected

p. 9140, l. 10: p need to be defined. Note that it brings also confusion with p define Eq. 1.

[WR] Corrected

p. 9140, l. 11-13: e and ei bring confusion with e defined Eq. 1.

[WR] Corrected

p. 9140, l. 14: Clarify the vector e in Eq. 14 and/or properly formulate ei

[WR] Name changed to 'r'.

p. 9140, l. 14: Define (again) A and Ñv(+) of Eq. 14.
Looking at Yang (2010), the residual vector is written: ek = SWDk . Ñvk(+) - SWDk
I will be grateful if that can be clarified.

[WR] Corrected

p. 9141, l. 1: After "is not used." a new paragraph should start to separate the robust Kalman filter (applied to minimise and remove observations contaminated with outliers) and the process of effective removal (of linearly dependent observations and parameters).

[WR] Corrected

p. 9141, l. 13: The k index is used several time before Eq. (6) to (10) to describe the epoch. To help the understand of the mathematical formulation of this technique, I suggest to use a different index to describe the rank of the matrix A (then change also (k) l. 18) or change the letter/index to describe the epoch.

[WR] Corrected

p. 9142, l. 4: Maybe write "in the design matrix A" or "in the model matrix A" rather than "in matrix A"

[WR] Corrected

p. 9142, l. 7: Maybe a reference is missing at the end of this line.

[WR] This is simple method to assess wherever the decomposed A matrix row is significantly different from original matrix A.

p. 9142, l. 15-16: Same comment than for p. 9140, l. 5, RkR(-) is not properly written Eq. (23).

[WR] Corrected

p. 9142, l. 17: Eq. (25) has not be introduced before.

[WR] P explained.

p. 9143, l. 5: Eq. (25) should be more precisely described before or at this time in the summary of the process. Eq. (26) should also be described here.

[WR] Description added

p. 9143, l. 16-20: More precision about the uncertainty parameters rescaled to the tomography models voxels from ACCESS NWP is required. This will help to understand how to obtain uncertainty parameters from climatological data that can be used systematically in GNSS tomography without NWP outputs.

[WR] It should be “interpolated” rather than “rescaled”, more description added

5 Case study
An introduction of the meteorological event studied could be nice.

[WR] We would rather stick to the technical description of this new model as this is the core of this research effort.

p. 9143, l. 22: A reference to the TOMO2 model can be added.

[WR] Well, this manuscript introduces TOMO2 for the first time so this is the reference.

5.1 NWP model
p. 9144, l. 4: A reference to the UK Meteorological Office Unified Model can be added.

[WR] Reference in this case is not required as we use only ACCESS and all the links between UM and ACCESS are explained in Le Marshall et al., (2010).

p. 9144, l. 5-6: This is “AIRS” not “AIRIS”, and “Le Marshall et al., 2010” not “Le Marshall et al., 2012”, see http://www.bom.gov.au/amm/docs/2010/lemarshall_hres.pdf.

[WR] Corrected

p. 9144, l. 5-6: If the authors want to specify the dimension of the grid spacing of the NWP, latitude and longitude dimensions are required.

[WR] In this extended regional model the latitudinal and longitudinal dimensions are equal (0.375 deg)

p. 9144, l. 8: Precision about these 50 levels is required (geopotential? Ref.?, what is the range of altitude).

[WR] These are geopotential heights as in previous study (Rohm et al., 2014) we have had a problem to find the acceleration force at the mean sea level used by the model, therefore we decided to assume that geopotential height is equal to geoid and to convert between these two we only need to use geoid undulation.

p. 9144, l. 10: Fig. 2 is not cited in the manuscript.

[WR] Corrected

5.2 Common data
p. 9144, l. 24-25: Explain more precisely why the large high differences is in favour of GNSS tomography (i.e. vortex close to the ground are crossed by slant delays from other stations…)

[WR] Corrected

p. 9145, l. 1-7: Fig. 2 can be cited in this paragraph (then to follow the chronology of the citation of the number of Fig. 2 and 3 should be exchanged). Additional (lat,lon) values could be nice on Fig. 2a. Also, a modification of Fig. 2b can precisely describe/illustrate the range of altitude of the voxel.

[WR] The purpose of Figure 2. is to introduce the general concept of the model space (inner and outer) without really link to the particular application (here in Victoria). Therefore we will refrain from adding labels to the figures, as the horizontal and vertical spacing and extend is a function of GNSS receivers network density and terrain undulation.

p. 9145, l. 10-12: No separation of dry and wet delay are required for the GNSS technique. See previous comment for l. 8-9. This is the hydrostatic delay that is needed to be separated from the total delay of the neutral atmosphere. For this separation, an estimation of Pground is required, and also an estimation of the mean gravity. This estimation of the mean gravity is not mentioned in the manuscript. Note that a reference to Saastamoinen (1972) can be added in this paragraph.

[WR] Corrected.

p. 9145, l. 12-14: Eventually the use of (P,T,e) has already be mentioned, see comment l. 8-9. The authors just need to be consistent with the different use of the NWP that they will reformulate in this paragraph. For example, the authors can mention that they used ACCESS-R to estimate the 3D field of wet refractivity at the location of the centre of the voxel (interpolating…).

[WR] Corrected

p. 9145, l. 15-17: Explain briefly why the authors use GPT combined with UNB3m, that allows self-consistent estimation of Pground, ZHD, Tground, e, and Nw.

[WR] Corrected

5.3 Simulation data

5.4 Real data

p. 9146, l. 10: The authors provide the horizontal and the vertical repeatability. I know that in GAMIT the horizontal repeatability is expressed with North-South and East-West estimations. I suppose that the horizontal repeatability is also different in the North-South and in the East-West direction with BERNESE. Is that right?

[WR] Actually in Bernese there is only horizontal and vertical repeatability, without separtation into E-W and N-S direction

p. 9146, l. 11-12: This passage is not clear for me, it could be nice to have more explanation "the next step by fixing the translation parameters of the network and pre-eliminating the velocities as well as the coordinates from weekly solution"

[WR] Corrected

p. 9146, l. 16: "mapping function is used to map the observed delays to the vertical direction", this is not precisely what happens. Slant observation can not be directly observed by GNSS receiver because of the ambiguity of the signal emitted by satellites and recorded by receivers. The authors can simply write that: "Considering the variation of the phase of the GNSS signal recorded by ground based receiver, the mapping function of Niell (2006) has been used to adjust a mean ZTD".

[WR] Corrected

p. 9146, l. 26: “in either case the DD residuals are not considered”, DD has not been defined in the manuscript, the authors should delete this phrase.

[WR] The phrase has been redefined.

p. 9147, l. 1-2: Clarify that the SWDG are not uncorrelated because the same isotropic contribution (ZWD) is mapped with the mapping function in direction of GNSS satellites to obtain SWDG. What do the authors mean by “and the mapping function used to map the delay from zenith to slant direction contains implicit information on the vertical distribution of WV”?

[WR] That was perfectly the meaning you were asking for, we reformulate this sentence.

5.5 Tomography processing results discussion

p. 9147, l. 11-12: If the authors do not consider the gradients, just write “(2) real observations (R)“

[WR] We do consider gradients so we would like to keep the Z and R observations.

p. 9147, l. 13-18: For all the different test of the a priori model the authors use the NWP derived outer model values for all the epoch. The details of this paragraph is totally in disagreement with the description of the “A priori type” of Tables 2 (correct it?). If this is the case, the authors can simplify this paragraph and write that they use the NWP derived outer model values for all the epoch for all the tests. If this is the case, I would like to know why the authors consider the same outer model for all the tests and if they have tested something different.

[WR] The description was wrong, it has been corrected. We have processed the data with additional settings to improve our analysis. Three new solutions were obtained: RWASD, where W stands for inner and outer model apriori data from NWP (all epochs); RG0ASD where G0 stands for outer (all epochs) and inner model (first epoch) from UNB3m + GPT data and RG1PASD where G1P stands for outer and inner (all epochs) apriori model data from UNB3m + GPT.

p. 9149, l. 2: Table 2, the std of RG2ASD is 6.5 not 6.7 mm km-1.

[WR] Corrected

6 Conclusion

References

P. 9151, l. 17: Replace “Malderen, R. V.” by “Van Malderen, R.”

[WR] Corrected

P. 9152, l. 9: Replace “2012.” by “2010.”

[WR] Corrected

P. 9152, l. 10: Miss UNB3M_pack in the title of this reference.

[WR] Corrected

Tables

p.9154, Tables 1: for G2, the authors write on the second line “UNB3m outer inner”, delete outer.

[WR] Corrected

Figures

I suggest to the authors not to put too much test in one plot. Don't use the scatter plot. For example If the authors plot RG2OOO, RG2AOO, RG2ASO and RG2ASD, they can use dash black line for RG2OOO, thin black line for RG2AOO, medium grey line for RG2ASO, and thick black line for RG2ASD. If the authors want to plot RNOOO, RNAOO, RNASO and RNASD on the same plot, they can use dash red line for RNOOO, thin red line for RNAOO, medium orange line for RNASO, and thick red line for RNASD. Try to keep the same type of line (colour and thickness) for the same configuration shown in a different figure.

[WR] Figures replotted

Note that the authors write 325 epochs. I think an epoch is 30 minutes. Check that the time in hour is OK. Maybe the authors consider 650 epochs?

[WR] One epoch is 30 minutes.

Hope this can help to improve this study and obtain consistent results according the GNSS technique. I was really happy to review this manuscript that helps me to better understand how to improve GNSS tomography. I am very grateful for that and thank the authors for this nice methodological work which needs just a correction of the formula of Nw and certainly more precision and details about the sensitivity tests. Hopefully, the reader will clearly see how the robust Kalman filtering associated with truncation can improve the tomographic monitoring retrievals, and bring a bonus to the meteorological and geodesic GNSS applications.

[WR] Thank you very much for your time and effort you spend to put together this very comprehensive review.

Yours faithfully,
Hugues Brenot