

Interactive comment on “Retrieval of temperature and water vapor profiles from radio occultation refractivity and bending angle measurements using an optimal estimation approach: a simulation study” by A. von Engel and G. Nedoluha

Anonymous Referee #1

Received and published: 12 April 2005

GENERAL COMMENTS

The main purpose of this study is to assess the possible assimilation impact from radio occultation data and to assess the advantages and disadvantage of assimilating bending angle and refractivity. Unfortunately, neither of these goals are fulfilled and the manuscript need substantial revisions before publication. First, the impact of a

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

new data type in a NWP model can only be assessed if the new data are assimilated together with other data types, which is not done in this study. Second, the error covariances used in this study for bending angle assimilations and for refractivity assimilations are not consistent, which means that the differences which are found in this study between the two retrieval technique mainly reflect the somewhat arbitrary choice of error covariances rather than revealing genuine advantages or disadvantages of any of two methods. Furthermore, the authors claim, based on their simulations that the assimilation impact from radio occultation data in the troposphere is limited by the inability to effectively separate water vapor and temperature. This statement is true for the current study as the applied error covariances are not very accurate, but it is not a general statement as it appears in the current manuscript. Generally, the results and conclusion presented in this manuscript suffer from a poor choice of error covariances; the authors must redo or supplement their simulations using more accurate error covariance estimates before this manuscript should be published. Also, there are a number of wrong or unclear statements which must be corrected. I will go more into details with these issues in the following.

SPECIFIC COMMENTS:

p1586 1 lines 1-3:

'The Optimal Estimation Method is used to retrieve temperature and water vapor profiles from simulated radio occultation measurements in order to assess possible assimilation impacts of this data.'

It is not clear to me what the meaning of this sentence is - are the authors referring to assimilation into NWP models? If this is the case, I cannot see how the authors can assess the impact from their study. To assess the impact on a NWP, new data should be assimilated together with other observations. Please clarify this.

p1588 lines 9-11:

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

'In this study we will attempt to quantify, using a simulated atmosphere based upon a high resolution ECMWF dataset, the statistical effect of various choices in forward model and retrieval.'

The phrase 'various choices' is too general and does not really convey any information to the reader. I would suggest that the authors state explicitly which forward models and retrieval schemes are used in this study.

p1588 lines 24-26:

'Nevertheless, the inclusion of a ray tracing forward model requires large computer resources. A 1-dimensional processing would therefore be more desirable.'

Here the authors ignore recent works on fast non-local operators, see e.g.[Syndergaard et al., 2003; Poli, 2004; Syndergaard et al., 2004]. Please include relevant references.

p1589 lines 2-3:

'By using the same retrieval and forward model for bending angles and refractivity assimilation we quantify the impacts of different effects in a uniform set of simulations.'

Though the authors use the same retrieval and forward model for bending angles and refractivity, bending angle errors and refractivity measurement errors are not estimated consistently. The authors simply assume that the SNR is the same for bending angles and refractivities, an approach which the authors themselves refer to as crude (p 1592). It would be better to propagate the assumed bending angle errors to refractivity, which is fairly easy, see e.g. [Syndergaard, 1999; Rieder and Kirchengast, 2001]. Have the authors considered how sensitive the relative performance of their two retrieval schemes are to the applied error estimates?

p1591 lines 9-11:

'Within the radio occultation processing, the transform of this equation is used to calculate the refractivity profile from the bending angle measurements.'

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

I assume that it should be 'the inverse of this equation' rather than 'the transform of this equation'?

P1591 lines 14-17:

'The canonical transform, as discussed by Gorbunov (2002) can theoretical improve the resolution of radio occultation data to within 30 m, but errors of the measurement will limit the resolution practically to about 100m to 200m (Ao et al., 2003).'

First, it is not only the Canonical transform which has sub-fresnel resolution, this is also true for other Fourier Integral Operator based methods like FSI [Jensen et al., 2003], phase matching [Jensen et al., 2004], CT2[Gorbunov and Lauritsen, 2004] and for back-propagation, e.g. [Karayel and Hinson, 1997]. Second, the authors should refer to the more recent estimates for the theoretical resolution given by [Gorbunov et al., 2004]. Third, in the paper by (Ao et al., 2003), I could not find justification for the authors statement that for real RO measurements the resolution is limited to 100-200 m. I do not think that this statement is correct - please clarify. Also do the authors actually use CT in their processing?

p1593 Section 3 Retrieval model

It would be helpful if the authors included an explicit expression for the cost function, also is the approach described here different from 1DVAR?

p1594 Equation 6

It looks like as if the authors do not include the representativeness errors which arise from horizontal variations. If this is the case, the measurements errors in this study are severely underestimated. For RO observations with realistic horizontal gradients these errors can be order of magnitudes larger than any other error source in the lower troposphere see e. g. [Kursinski et al., 1997; Palmer et al., 2000]. This is a crucial point and these errors must be included.

p1595 line 15:

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

'Contributions to the error budget arise from the limb sounding geometry.'

I do not understand this sentence, please rephrase'

p1595 lines 25-29

'These settings are consistent with the capabilities of a NWP model short range forecast calculation (Palmer et al., 2000), except for the 1% a priori uncertainty for the reference pressure retrieval, but sensitivity of the retrieval to this uncertainty is very low.'

Why are the authors using a 1% pressure error if it is such bad estimate? Also, the authors are not using a short range forecast as a priori but a 24 hours old analysis, how well do the authors believe that these error estimates apply to their study? I will suggest that the authors consult [Kuo et al., 2004] as that study assess the 24 hours variability of the ECMWF analysis.

p1596 lines 8-9:

'This processing introduces correlations in refractivity measurements at different altitudes.'

I know other authors have made similar (unfounded) statements, but error propagation analyses [Syndergaard, 1999; Rieder and Kirchengast, 2001] clearly show that the observation errors for refractivity are not significantly more vertically correlated than the observation errors for bending angle. This is because the bending angles are obtained via the derivative of the phase observations, which results in negative error correlations. The corresponding refractivity errors, when the errors are propagated through the Abel transform, also show these negative correlations, but they are generally reduced as compared to the correlations of the bending angle errors. In both cases the vertical correlations are narrow, depending on the degree of smoothing applied to the data. It is correct that the Abel transform would broaden the vertical correlation of the errors if bending angle errors were uncorrelated, but the important point is that the bending

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

angle errors are not uncorrelated. It is somewhat similar to saying that the operator corresponding to a simple integration will broaden the error correlations, but if the data you integrate are a result of a derivative operation, you will get back to your original data (apart from a constant offset), and if the errors were uncorrelated to start with, then they will still be uncorrelated after the combined derivative-integration operation. As pointed out by [Gorbunov et al., 1996], the Abel transform is somewhat equivalent to a half integration (applying it twice is somewhat equivalent to a full integration), which explains why the negative correlations in the bending angle errors are basically just reduced by a factor of about two when the errors are propagated through the Abel transform.

p1597 lines 3-5:

'the result is more general, since vertical correlation schemes will vary depending upon who is doing the assimilation.'

I do not understand this sentence; please explain why assimilations with vertical correlations should be more depend on who is doing the assimilations than assimilations without vertical correlations.

p1597 lines 6-8:

'Generally, the a priori profiles and their error covariance matrices have to be chosen very carefully for a retrieval from real data, in order to avoid the introduction of a bias.'

Which bias is the authors referring too; the bias in the a priori or the bias in the observations, or do the authors believe that a poor choice of error covariances can lead to a biased retrieval by itself?

p1597 line 14:

'These results are based on ray tracer simulations, wave optics results are very similar'

One of the major differences between ray tracer simulations and wave optic simulations

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

is that the latter cannot simulate multipath propagation. As the authors get similar results when they apply the two simulators it must indicate that the ECMWF field used in the simulations is fairly smooth in the vertical so that no multipath is generated. However, I would expect that the ECMWF field does contain sufficiently small structures to produce multipath. If this is the case, this could be the explanation for the problems related to separating temperature and humidity which is found in this study. Please explain how multipath propagation is dealt with in the simulations?

p1597 lines 19-22:

'Within the humid areas of the troposphere, either water vapor or temperature has to be estimated in order to determine the other quantity.'

In Figure 1 the authors show results for direct inversion all the way down to the surface, which a priori water vapor and temperature profiles are used to do that?

p 1597 lines 25-27:

'The improvement plots show that the retrieval from refractivity measurement works slightly better for temperature in the altitude range of 15 km to 35 km, yielding improvements over the a priori data of about 85% to 45%'

I am not convinced that these results demonstrate that refractivity retrievals generally work better than bending angle retrievals, I would rather think that this just reflects that the authors' choice of error covariances works better for refractivity than it does for bending angles, another choice of error covariances could lead to the opposite result. In the height range from 15-35 km the relation between bending angles and refractivity is virtually linear. That is, if the two error covariances are chosen consistently (i.e. by propagating errors from bending angles to refractivity or vice versa) assimilation in that height range should give the same result for both bending angle and refractivity. To repeat myself, I will urge the authors to change their error covariances by propagating bending angle errors to refractivity errors.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p1598 lines 1-4:

'Above, retrieval from bending angle measurements are slightly better, since the integration of bending angles through all atmospheric layers above the tangent point provides additional information in an altitude region where the sensitivity for temperature retrieval decreases.'

Yes this must be true near the top of the occultation (60 km in this study), but I am not convinced that this effect can be felt in the height range from 35 km to approx. 50 km. I still think that the two retrieval schemes should give the same results at those heights when the error covariances are chosen consistently. Also the authors should explain how they handle the integration to infinity when applying the Abel transform to the simulated bending angles, as this will also introduce differences between the two retrieval schemes at high altitudes.

p1998 lines 8-12:

'Although the Optimal Estimation Method succeeds in minimizing the cost function, there are occultations where adjustments are made to water vapor to compensate for temperature differences and vice versa. This also occurs in idealized retrievals. A more rigorous quality processing and further research is necessary to filter out retrievals that lead to these erroneous results in the lower troposphere.'

Again, I am not convinced that this is not w result of the poor choice of error covariances, and that this problem would disappear for more accurate error covariances. For the quasi realistic simulations the authors do not include errors caused by horizontal variations, which makes their measurement error estimates way too small and constant with height which is highly unrealistic in the lower troposphere. For that reason I am very skeptical about these simulations. For the idealistic setup the applied measurement error covariances should be reasonable, but the authors assume that the a priori water vapor error is 40%, whereas the 'true' a priori water vapor error is between 50-200% according to the authors. Have the authors tried to vary the assumed water

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

vapor a priori error also for the idealistic setup? And if yes did they find that the problem of separating water vapor and temperature remained? Moreover, I assume that the 'true' a priori error varies with height, have the authors tried to include these variations? To conclude, do the authors believe that separating water vapor and temperature is a generic problem or do they believe that the problem is related to the lack of knowledge of the 'true' error covariances?

p1598 lines 18-19:

'Dry temperature retrievals are unaffected by the quality of the a priori data.'

I would think that the dry temperature retrievals were independent of the a priori?

p1598 lines 25-26:

'The a priori data has a small temperature bias generally below 0.5K which is effectively removed by the optimal estimation method above 10 km.'

It would be good if the authors also showed plots of bias improvements like the std-dev plots. What do the authors mean by 'removed by the optimal estimation method' I would think the bias disappears because most weight is given to the un-biased measurements?

p1599 lines 7-9:

'Ray tracer calculations give slightly better results in the lower troposphere, since wave optics calculations will introduce a vertical smoothing of the highly variable water vapor field.'

The authors should rephrase this sentence as it could give the wrong impression that wave optic simulations are less accurate than simulations with ray-tracing when the opposite is true. Ray tracing, in contrast to wave optics, neglects diffraction and that is why more smoothing is introduced in the wave optics simulations. However, if the data is processed with CT the diffraction limited resolution is approximately 50 m [Gorbunov

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

et al., 2004], which is smaller than the resolution of the analysis field used in the simulations. If the data is processed using geometrical optics the resolution is limited to size of the size of the Fresnel zone in this case the above statement is correct.

p1599 lines 27-29:

'Low latitude retrievals start to produce negative improvements at altitude below about 10 km, while mid latitudes and high latitudes yield positive improvements further down.'

My previous comments about the choice of error covariances also apply here.

p1600 lines 3-4:

'Hardly any difference is found in the lower troposphere, the three dimensional structure of the atmosphere is not responsible for the degraded improvement here.'

That is true the degradation is likely to be due to the poor choice of error covariances.

p1600 9-13:

'These could be caused by the sharp water vapor gradient at the planetary boundary layer, as for example discussed in von Engel et al. 10 (2003b) and by the horizontal inhomogeneities in water vapor. The idealized simulations do not show these negative improvements, indicating that the horizontal variability is mainly responsible.'

My understanding of the idealistic set-up is that the measured bending angle is computed from the Abel integral (is this correct?). The Abel integral works for atmospheric conditions with multipath, whereas the ray-tracer does not, could this be another explanation for the differences between quasi-realistic and idealistic simulations? For the same reason I would expect that the idealistic occultations penetrate to lower altitudes than the quasi realistic occultations is this so? Have the authors tried to run the ray-tracer for a spherical symmetric tropical atmosphere and compared the retrieved bending angle profile to the corresponding bending angle profile derived from the Abel transform?

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

p1600 lines 13-14:

'A ray tracer assimilation scheme should thus improve the water vapor retrieval at low latitudes, since it includes the horizontal inhomogeneities.'

Please also include references to fast non-local operators e.g. [Syndergaard et al., 2003; Poli, 2004; Syndergaard et al., 2004].

p1600 lines 15-17:

'But it also shows that the capabilities of radio occultation observations are mainly limited by the inability to separate water vapor and temperature effectively in the lower troposphere, and not by horizontal inhomogeneities.'

The results in this study do not justify this general statement, however, the results in this study do show that the above statement is true if the error covariances are not chosen carefully. In order to verify whether the above statement is correct or not, I will suggest that the authors perform simulations where the applied errors are in agreement with the error covariances used in the assimilations.

p1601 Section 5 impact of errors:

All statements w.r.t. the error covariances made above also apply to this section. Furthermore, I think that the results in Figure 3 seem to confirm my points. Do the authors also find occultations where the separation of water vapor and temperature fails when they multiply the temperature a priori error with 0.5?

p1603 lines 14-15:

'Hence, these correlations essentially reduce the weight of the a priori.'

This is not a general statement, I would rather prefer something like: 'Hence, in this case, these correlations essentially reduce the weight of the a priori. Introducing correlations in the a priori can both reduce or increase the weight of the a priori depending on the true error covariances and what is assumed for the measurement error covariance.'

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p1604 Section 7 Conclusions:

All the statements made above also apply to the conclusion.

p1605 lines 4-5:

'Refractivity profiles are generated from bending angle profiles assuming a spherical symmetric atmosphere in the vicinity of the occultation location. Thus bending angle profiles are more complex to calculate but contain more information on the non-symmetric atmosphere.'

This statement is not correct, first bending angle profiles are also computed by assuming spherical symmetry see e.g. [Kursinski et al., 1997]. Second, there is a one-to-one relation between bending angle and refractivity (ignoring super-refraction), so that no information about the horizontal structures of the atmosphere is lost when a bending angle profile is transformed into a refractivity profile. In fact the non-local operator suggested by [Syndergaard et al., 2003; Syndergaard et al., 2004] is based on refractivity.

p1605 lines 3-4:

'A priori data is required above and below this altitude range for the retrieval.'

I do not understand this statement please rephrase.

p1607 line 18:

'do not terminate when the a priori data shows critical refraction at one level'

I cannot see why bending angle retrievals must terminate if the a priori data shows critical refraction at one level. I understand that such layers will require special treatment in the assimilations scheme, but it should be possible to assimilate measured bending angles above and below a super-refraction layer. I think it is more important that the measured refractivity will be biased below a super-refraction layer [Sokolovskiy, 2003].

p1612 Table 1.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

What is the unit of the refractivity errors?

p1614 and 1615 Figures 1 and 2.

These figures are very small I will suggest that the authors use the same size as for Figures 3 and 4.

REFERENCES

Gorbunov, M. E., H.-H. Benzon, A. S. Jensen, M. S. Lohmann, and A. S. Nielsen, Comparative analysis of radio occultation processing approaches based on Fourier integral operators, *Radio Sci.*, 39, doi:10.1029/2003RS002916, 2004.

Gorbunov, M. E., A. S. Gurvich, and L. Bengtsson, *Advanced Algorithms of Inversion of GPS/MET Satellite Data and Their Application to Reconstruction of Temperature and Humidity*, 1996.

Gorbunov, M. E., and K. B. Lauritsen, Analysis of wave fields by Fourier integral operators and their application for radio occultations, *Radio Sci.*, 39 (RS4010), doi:10.1029/2003/RS002971, 2004.

Jensen, A. S., M. S. Lohmann, H.-H. Benzon, and A. S. Nielsen, Full Spectrum Inversion of Radio Occultation Signals, *Radio Sci.*, 38 (3), 1084, doi:10.1029/2002RS002763, 2003.

Jensen, A. S., M. S. Lohmann, A. S. Jensen, A. Nielsen, and H.-H. Benzon, Geometrical Optics Phase Matching of Radio Occultation Signals, *Radio Sci.*, 39, doi:10.1029/2003RS002899, 2004.

Karayel, E. T., and D. P. Hinson, Sub-Fresnel scale vertical resolution in atmosphere profiles from radio occultations, *Radio Sci.*, 32 (2), 411-423, 1997.

Kuo, Y.-H., T.-K. Wee, S. Sokolovskiy, C. Rocken, W. Schreiner, D. Hunt, and R. A. Anthes, Inversion and Error Estimation of GPS Radio Occultation Data, *J. Met. Soc. Jap.*, 82 (No 1B), 507-531, 2004.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

Kursinski, E. R., G. A. Hajj, J. T. Schofield, R. P. Linfield, and K. R. Hardy, Observing Earth's atmosphere with radio occultation measurements using the Global Positioning System, *J. Geophys. Res.*, 102 (D19), 23429-23465, 1997.

Palmer, P., J. Barnett, J. Eyre, and S. Healy, A non-linear optimal estimation inverse method for radio occultation measurements of temperature, humidity, and surface pressure, *J. Geophys. Res.*, 105, 17513-17526, 2000.

Poli, P., Effects of horizontal gradients on GPS radio occultations observation operators. II: A fast atmospheric refractivity gradient operator (FARGO), *Quart. J. Roy Meteorol Soc.*, 130, 2807-2825, 2004.

Rieder, M., and G. Kirchengast, Error analysis and characterization of atmospheric profiles retrieved from GNSS occultation data., *J. Geophys. Res.*, 106, 31755-31770, 2001.

Sokolovskiy, S., Effect of superrefraction on inversions of radio occultation signals in the lower troposphere, *Radio Sci.*, 38 (3), doi:10.1029/2002RS002728, 2003.

Syndergaard, S., Retrieval Analysis and Methodologies in Atmospheric Limb Sounding Using the GNSS Occultation Technique, DMI, Copenhagen, 1999.

Syndergaard, S., D. Flittner, E. R. Kursinski, D. Feng, B. Herman, and D. Ward, Simulating the influence of horizontal gradients on retrieved profiles from ATOMS occultation measurements - a promising approach for data assimilation, in *OPAC-1*, pp. 221-232, Springer Verlag, Graz, 2004.

Syndergaard, S., D. Flittner, E. R. Kursinski, and B. Herman, Simulating the influence of horizontal gradients on refractivity profiles from radio occultations, in *4'th Oersted International Science Team Conference*, pp. 245-250, Narayana press, Copenhagen, 2003.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 5, 1585, 2005.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper