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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Tropospheric profiles of wet refractivity and humidity from the combination of remote sensing datasets and measurements on the ground" *by* F. Hurter and O. Maier

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We thank the two anonymous referees I and II, and Dr. Witold Rohm for their constructive and interesting discussion comments.

##
– Anonymous Referee 1

– Major issues:

– generally: the use of ppm for errors is rather difficult to interpret, thus suggest to add the % in brackets at relevant text/table entries. This makes all the different error

discussions also more harmonic.

We will definitely follow this advice. It creates a smoother transition from the results given in absolute wet refractivities to the results in relative units. Examples include p4911,20; p4913,01; p4926,Table4; p4915,26;

– p4911, l26: are you using rs in the processing and validation? This is no independent validation!

When we validate the model against the radiosonde, we do not use radiosonde data in the processing. This is entirely true for Figures 6, 9, 10, 11, 12, 13, 14 and corresponding results presented in the text.

The authors are aware of the fact that including the radiosonde into the collocation makes it not independent from the validation dataset anymore. The rightmost plots in Fig. 7a and b are not intended as validation but to show the smoothing effect of the collocation (with the here used parameter set) on the reference dataset from the radiosondes. It should demonstrate that collocation inherently acts as a kind of averaging kernel. Together with Fig. 8, it becomes clear that even with 'perfect' data, there will be an error introduced by the algorithm. We will try to make the point clearer in the text and thank you for pointing out the ambiguity.

– Minor issues:

– p4897, l14: please add IR to list and also use radio occultation here, this is commonly used in this way, I have never seen radiooccultation before

Indeed, IR should be added to the list and radiooccultation be changed to radio occultation.

– p4898, l01: missing reference behind are? The link itself in pdf seems to work.

Please note the supplemental comment C1203 in the discussion section.

– p4899, last lines: maybe there is a better write way for K h Pa-1, it looks a bit like the h is for hours

Unfortunately, the authors have not noticed that KhPa-1 was changed to K_h_Pa-1 during the typesetting process. Probably the best way to write this would be K_hPa-1.

– p4900, l20: what about directional data use, not just zenith mapping?

There are two answers to this question:

1) A directional component could be included using zenith path gradients (E-W and N-S components) as suggested in the Conclusions p4918,12. It might add some value to the scale height H, to the humidity content near the boundary layer and obviously to the azimuthal asymmetry. The impact of those gradients is currently investigated, since it is definitely the easiest directional data type to be included in the present implementation of the collocation.

2) The use of slant path delays from GPS could also add directional information. Adding slant paths would boil the problem down to fusing collocation with tomography. This is theorically possible, however, considerably harder to solve than adding gradient parameters to the collocation (and also computationally more expensive since the number of observations determines the size of the covariance matrix to be inverted in the algorithm). Slant paths, consisting of the back-mapped zenith path delays, gradient parameters and GPS processing residuals, might add some value to a better reconstruction of the atmospheric heterogeneity, since residuals from the GPS processing are known to contain some parts of this heterogeneity.

– p4901, l16: is there a reference for Niell?

@ARTICLE{Niell1996, author = {A. E. Niell}, title = {{Global mapping functions for the atmosphere delay at radio wavelengths}}, journal = JGR, year = {1996}, volume = {101}, pages = {3227–3246} }

– p4902, l13: can a radio sonde really reach the tropopause exactly at 00 or 12UTC? I assume you give the launch times, not the tropopause time.

It should be added that the tropopause is 'approximately' reached at 00UTC/12UTC. The launch time is indeed 1 hour in advance. To be more precise: The sonde should reach 20km at 00UTC/12UTC. The sonde therefore arrives at the tropopause roughly 30-45min after launch depending on the tropopause height. Some tests not shown in the paper suggested that the collocated profiles 1 hour before 00/12UTC best represented the lower troposphere compared to the radiosonde. We will add more information to make this point clearer.

– p4092, l23: when you say restricted to the area, is e.g. an occultation just outside not considered? Given that occultations sample along the ray easily 100km or more, this might be a bit too stringent. You could thus get more collocations.

We only require the tangent point of the collocation to be within the study area (p4896,06 and p4903,18). This already implies that in the worst case half of the occulation ray path lies outside the study area. Furthermore, occultations with tangent points outside the area would have no noticable effect on the results in Payerne due to the chosen correlation lengths stated on p4925, Table 3 and the distance of the study area border to the validation site in Payerne. Full use of the ray information would ask for a tomographic approach (posing again the problem where the tomography domain should stop...).

– p4904, l04: is the derived bias correction constant over the whole time? Including the downtime period?

Yes, the derived bias correction is constant over the entire study period from 2009-2011. Why is a bias correction needed for a downtime period? What is exactly meant by a downtime period?

– p4904, l12: how does this least-squares collocation actually compares to optimal estimation/1DVar methods?

Commonality:

a) Also collocation can be shown to build on variational principles, as is shown in Moritz1980.

b) Both methods try to minimize the square error of the residuals.

Variability:

a) Collocation gives an analytical representation of the parameter space we are looking for. Discretization of any problem domain is not needed. Any linear functional of the analytical representation can be easily calculated, e.g. raytracing across a collocated parameter space would make interpolation between grid points needless.

b) Least-squares collocation requires inverting a covariance matrix of the size of m x m, where m is the number of observations. It is symmetric, but by no means diagonal, as is usually assumed for the weighting matrix of the observations R in least-squares prediction. One might truncate the covariance between two points in space from a certain distance on, which would leave us with inverting a band matrix. Nonetheless, the number of observations is a computationally more critical issue in collocation than in least-squares prediction.

c) 1DVar is usually used in meteorology as an assimilation technique, where observations are merged with an existing background field according to their variances. Collocation is not an assimilation technique, but fits functions at observation points, i.e. with a systematic part and a signal, where the systematic part is parametric in the classical least-squares sense and the signal is a function of known 'global' variance and zero mean. Collocation could actually mimick an assimilation by introducing the background field as observations.

Performance:

3DVar and 4DVar assimilations of zenith path delays into weather models are operational at for example the UK Met Office (see e.g. Bennitt2012). Comparing their performance to our results is very difficult since they have a background field at hand.

@ARTICLE{Bennitt2012, author = {Bennitt, Gemma V. and Jupp, Adrian}, title = {Operational Assimilation of GPS Zenith Total Delay Observations into the Met Office Numerical Weather Prediction Models}, journal = {Monthly Weather Review}, year = {2012}, volume = {140}, pages = {2706-2719}, doi = {10.1175/MWR-D-11-00156.1} }

@BOOK{Moritz1980, title = {{Advanced Physical Geodesy}}, publisher = {Herbert Wichmann Verlag, Abacus Press}, year = {1980}, author = {H. Moritz}, series = {Sammlung Wichmann: N.F.: Buchreihe; Band 13}, address = {Karlsruhe: Wichmann, 1980} }

###

– Anonymous Referee 2

– Major issues:

– It is not easy to see what the authors really want to communicate and what is the benefit for the reader, what can the reader take home from these investigations. There is a risk that a reader will stop reading after a few paragraphs. The main reason for this is that the largest part of the paper deals with refractivity, resp. the wet path delay in the atmosphere, a parameter commonly used in geodesy but not in atmospheric investigations.

The paper would like to demonstrate that there is humidity information in GPS path delays that is not available from ground measurements alone (see Fig. 10, p4936) and that these data are continously available without any additional cost. We also demonstrate that the humidity information can be retrieved with a relatively simple method and that the results are comparable to results from more sophisticated methods, such as water vapor tomography. The method has also no need for climatic information in the form of pseudo observations, as is frequently used in many other data retrieval methods (as a side note: most retrieval methods of water vapour and temperature radiometers rely on pseudo observations from NWP models to retrieve meaningful profiles of temperature and specific humidity).

Furthermore, we are not aware of any scientific argument why assimilating refractivities or path delays into numerical weather models is per se less valuable than assimilating any other measurement of the atmospheric state. Since we validate measurements of less frequent use in meteorology than in geodesy, the authors consider it of even more importance to focus on the less known.

Shifting the primary focus from wet refractivity to humidity would mean that we do not care enough about the error components of the derived parameter humidity. Not even GPS path delays are direct measurements of the atmosphere, but are derived from a least-squares adjustement. How could we justify the validation of the derived humidity profiles if we did not thoroughly treat the validation of wet refractivities beforehand?

– Path delay and refractivity that depend on humidity and temperature are frequency dependent parameters.

In this paper, we are concerned with the real part of the complex refractive index. Thus, for the Troposphere neither refractivity nor path delay are frequency dependent in the microwave frequency band. The measurement principle is fundamentally different from the measurements of a microwave radiometer that determines the strength of the reemitted microwaves from vibrating water molecules. Having said this, there is no reason to exclude any of these measurement systems. They can also be easily combined in a collocation approach. With the paper, we would like to foster the combination of measurements already available in real time and NOT excluding or discredit any of them.

– Most part of the paper deals with the refractivity what per se is not a problem. Difficulty arises towards the end of the paper when humidity profiles are retrieved at a single location (Payerne) by using data (temperature) from HATPRO, a microwave radiometer able to retrieve temperature AND humidity profiles. The reader might ask why not go the direct way and take the humidity profiles from HATPRO directly.

One motivation of the authors for the study is to characterize the uncertainty of the

method and apply it at locations without any measurements of humidity other than GPS, provided there are temperature profile measurements. This can be the case at locations where there are data from temperature radiometers (TEMPRO or other less expensive radiometers) or from airplanes (AMDAR data that most often do not have a humidity sensor).

Initially, the project aim was to create a denser network of temperature/humidity profiles in Switzerland with radiometers. Since radiometers are relatively expensive, the question arose, if the water vapor sensor could be replaced by some other sensors of lower cost by only employing radiometers with temperature sensors. The permanent GPS network of Switzerland was an obvious choice to go.

In Maier2012, we could show that the humidity profiles retrieved with GPS+Temp-Radiometer are of similar quality as profiles from WaterVap-Radiometer+Temp-Radiometer. As is mentioned in Löhnert2012, the degrees of freedom of the vertical resolution of a water vapor radiometer retrieval is less than 2, which underlines that profiling with water vapor radiometer bears some problems.

@INPROCEEDINGS{Maier2012, author = "Maier, Olaf and Hurter, Fabian", title = "{A}ccuracy of {B}oundary {L}ayer {H}umidity {P}rofiles retrieved by {G}{N}{S}{S} {M}eteorology and {M}icrowave {R}adiometry", booktitle = "Proceedings of the 9th International Symposium on Tropospheric Profiling", year = 2012, publisher = "ESA Conference Bureau" }

@ARTICLE{Löhnert2012, author = {L\"ohnert, U. and Maier, O.}, title = {Operational profiling of temperature using ground-based microwave radiometry at Payerne: prospects and challenges}, journal = {Atmospheric Measurement Techniques}, year = {2012}, volume = {5}, pages = {1121–1134}, number = {5}, doi = {10.5194/amt-5-1121-2012}, }

– There are some more questions that arise: The authors show that they use data from a whole network in Switzerland. In principle they should be able to retrieve a map of the refractivity but they only show a profile for Payerne what is somewhat disappointing.

The authors agree that a refractivity map across the investigation area would have been a nice add-on. A single-case however, would not have added a real benefit to the paper. Several unusual weather situations would have been a nicer demonstration of the use of such maps. It was a point of discussion between the authors and has eventually been omitted since we thought the paper to be already of considerable length.

– The paper shows (p. 4917,line 17) that the combination of GPS etc. plus microwave radiometers can provide humidity profiles. Unfortunately the authors do not point out what they gain by this combination compared to the humidity profiles that are obtained from the microwave instrument alone.

The gain is to have humidity profiles from GPS at locations without any other measurements of humidity, provided there are temperature profile measurements. The fact that the radiometer in Payerne is a HATPRO (with temperature and humidity sensors) is not relevant for the study. Some sentences will be added to the paper to clarify.

– In the discussion the authors include some results from lidar but they do not go in depth. As lidar is not mentioned in section 2 I recommend to delete this part.

The structure of the discussion is not very concise, which is probably the reason for your comment. However, to refer to the Lidar in section 2 and to rephrase the Lidar part of the discussion might be more appropriate than to delete it. Since the referenced Lidar paper validates the water vapor measurements at the same location as we do in our paper, we think it worth mentioning.

– I do not understand why the scale height, H, is the same in equation (7),(8) and (9).

They are not the same, but it does not matter since they are never estimated in one common collocation. We will add corresponding subscripts to avoid confusion. Correspondingly, we will also subscript the gradient parameters.

– Minor issues:

– p. 4897, line 14 and 15: it should read microwave radiometry and ... solar spectrometry. The radiometer and the spectrometer are the instruments whereas radiometry and spectrometry is the technique.

Indeed...

– p. 4901, line 4: April 31 does not exist ;-)

Should be May, 1st, 2011. Thank you for pointing out and reading so carefully!

– p. 4912, line : Explain why the correlation lengths have to be chosen larger for a coarser network

In order that collocation can accurately determine the signal part from data, it needs a sufficient number of data samples per correlation length. It is somewhat comparable to the Nyquist frequency: Without a sufficient number of samples per sinusoidal wavelength, the amplitude of this wavelength can not be reliably determined with a Fourier Analysis. In collocation, the base functions are not trigonometric functions but are covariance functions (as for example shown in Moritz1978). To phrase it sketchily: There is a kind of Nyquist frequency for the covariance function.

The problem of correlation length choice becomes very difficult, if the samples are not regularly spaced. We solved this problem with the heuristics mentioned in the paper: Stable results were obtained, if correlation lengths were at least 4 times the average sampling in either space or time.

@ARTICLE{Moritz1978, author = {Moritz, H.}, title = {Least-squares collocation}, journal = {Rev. Geophys.}, year = {1978}, volume = {16(3)}, pages = {421-430}, number = {doi:10.1029/RG016i003p00421}, }

– Figure 5 is difficult to read Figure 10 is too small.

Agreed.

– In summary, I recommend publication of the paper after revision with emphasis on explaining and making more clear the link between refractivity and humidity and by considering the issues mentioned above.

The way we understand the reviewer's concern: We do not sufficiently explain the motivation behind our study. We will thus improve the justification why we use refractivity to obtain humidity and why we dedicate more than half of the paper to the validation of refractivity.

##
– Witold Rohm

– Despite high standard of the presented work, authors have not discussed several major issues linked with weather prediction, GNSS meteorology and data fusion:

– 1. My understanding of fusion is that the fused data set should has all strengths of each data set combined and inherit limited noise, providing there is no bias in the original data set. This is very ambitious target, however it might not be feasible to apply in real world meteorological networks where the systematic errors are of unknown magnitude and need to be removed before incorporating the data into the observation system. Therefore my concern in regards to the fusion is that it might not be in position to replace the assimilation systems currently present in the Numerical Weather Prediction systems.

We do not aim at replacing the assimilation systems of numerical weather prediction systems. We hypothesize that the presented method might be used for nowcasting purposes. Furthermore, the assimilation of the original data (in the GPS case: slant or zenith path delays and radiometer temperature) into modern 4DVar/Kalman-/Particle-filter systems is definitely the better way to go than to do a data fusion before assimilation.

– The basic difference between assimilation and fusion would be that fusion finds optimal state between observations while assimilation finds solution that agrees the most

with the background model state. This actually prevents introducing new systematic errors.

In your comments, you mix two different types of systematic errors:

1) First, you refer to systematic errors in meteorological networks, which manifest themselves in the parameter to be measured by the observation system.

Observations to be assimilated should be devoid of systematic errors of this type. The reasons, why measurements are not wildly assimilated into NWP models, are the unknown characteristics of the systematic errors of many measurements (and how measurements fit into the 'model orography world' of NWP models).

This type of error is very difficult to detect. Under certain circumstances, data fusion can help finding systematic measurement errors as each observation system is hopefully different with respect to these errors.

2) Secondly, you talk about the systematic errors that result from the reconstruction of derived quantities through an inversion algorithm. These systematic errors result from ill-posed problems, as for example water vapor tomography from ground GNSS networks.

A background model can mitigate this error type, but not prevent it. The only way to completely prevent such an error type would be the assimilation of as many (mathematically) orthogonal observations as needed till the observability of the system becomes perfect. Data fusion systems are the play-ground to understand the degree of ill-posedness of some data reconstruction without having to deal with the full complexity of a NWP model.

– Would it be interesting to know authors opinion as to how address this issue. Maybe, post processing of NWP outputs in particular for severe weather events is a solution to obatain short term forecasts?

You propose a hidden Kalman filter approach: Predict a state and improve it with some data from an observation system. To the knowledge of the authors, meteorologists have already created such systems. They are also used to set up model initialization for new NWP model runs.

– 2. I was reading the paper twice trying to pick up how authors are taking into account the fact that not all of the observations are taken in the same time and have different latency (GPS RO, radiosonde, ground-based GNSS, automatic weather stations). The analysed parameters (ZWD, e, p) have the deterministic part as well as stochastic part time dependent, that is true but what is the time resolution of the collocation is unclear to me.

There are two types of time resolution you might refer to:

a) If your question concerns the frequency at which we can provide updated profiles and ingest new data: In an 'operational' setup, one would need to recalculate the data over a certain past time window, if new data arrives. There also exist updating procedures for stepwise collocation. Output of data can be provided at any desired time. The restriction of the output frequency is therefore the computing time.

b) If you refer to the atmospheric variations in time that we still can resolve with collocation: This largely depends on the correlation length of time, which is 4 hours in our case (p. 4925, Table 3). Similarly to the resolution in space, a smoothing and smearing effect is caused by the correlation length, acting as a lowpass filter.

– In this regard the Kalman filter seems to be bit more applicable. The reason why I would choose to use Kalman filter instead of fitting procedure (collocation) is that it would allow me to use all the elements of covariance matrices (15,16), as a measurement and process noise. Moreover, the clear time step procedure of Kalman filter will help to address the issue with latency and non time collocated measurements, [...]

The Kalman filter definitely offers a very elegant framework to address the latency issues.

– [...] additionally the short term prediction would be available from Kalman filter.

Also true for collocation within the limits of the stochastic and deterministic model used.

– The question remains if the quality would be comparable to the one obtained from extrapolation of collocation field.

As you pointed out, the covariance matrices from collocation can be used to tune the state vector covariances and the process noise of a Kalman filter. The deterministic functions could be included into the model matrix of the filter. Comparable results should be expected.

– 3. In the discussion section authors compare the solution obtained from collocation to the GNSS tomography, concluding that the same accuracy has been demonstrated for both methods. Therefore, authors question the applicability of tomography principle and use of slant GNSS observation instead propose to use only zenith integrated values. I have to agree that tomography solution could carry very little information if use with to many constraints. What I can't agree is the pointless of using slant delays,

We do not state that it is pointless to use slant delays. We say that with the currently investigated station networks and reconstruction techniques, the slant path delays add little additional information. What we mean is that this information does not easily become available to us when we try to profile the atmosphere (with tomography or collocation). Including radio occultation slant paths into a tomography with ground stations is a very different story and definitely adds value to the information contained in slant paths. With horizontal rays passing the investigation area, real profiling above the top ground station becomes feasible. Since on an area the size of Switzerland we get at most one occultation per day, using such profiles for nowcasting is rather tentative :-). We will consider reformulating p4916,01-03 in order to incorporate your arguments.

– there are couple of very good reasons to use it:

– (a) The azimuthal non homogeneity will manifest itself especially priori severe weather conditions and this information might become essential for proper now casting of severe weather event. I would rather encourage authors to apply slant observation in their collocation software, however there would be modiïňĄcations needed in the equation (9).

This would be an obvious extension of the study and has been discussed in a question from Anonymous Referee 1. Taking advantage of azimuthal information from GPS observations is still a topic of research and bears interesting questions.

– The figure 6 shows clearly that the collocation is not performing well especially in the height around 2km and during summer months on the Northern Hemisphere. This is the time when there is much more water vapour present in troposphere (carried in mid latitudes by weather fronts). In contrary to the collaction results, tomography at precisely this height and with this type of events perfoms best.

What is meant by 'performs best'? On what basis do you quantitatively claim that tomography performs better at 2km height than the collocation? On the basis of Figure 7 of the referenced paper (Manning2012)?

@ARTICLE{Manning2012, author = {Manning, T. and Zhang, K. and Rohm, W. and Choy, S. and Hurter, F.}, title = {Detecting Severe Weather using {GPS} Tomography: An Australian Case Study}, journal = {Journal of Global Positioning Systems}, year = {2012}, volume = {11}, pages = {58-70}, number = {1}, doi = {10.5081/jgps.11.1.58}, }

– The AWATOS2 model, developed at the ETH Zurich, proved to be very effective in estimating the storm surge in Victoria, Australia (Manning et al., 2012), showing all synoptic and mesoscale features of Mesoscale Convective System.

It is indeed an instructive case study.

– (b) The tomography models in generally suffers from over constraining that is used as a remedy for unconditioness and ill possedness of the inverse problem. However, the

use of unconstrained model (Rohm, 2013) with strict observations selection policy (Robust Kalman Filter) produces results with an accuracy of 5-6 mm/km (paper submitted to ANGEO).

A Kalman filter needs some starting state with a state covariance matrix. It contains implicit apriori information and is constrained to the state covariance, subject to and weakened by the process noise.

– Simulations suggests that the accuracy of tomography retrieval might be even 5 times better (1 mm/km) (Rohm and Bosy, 2009), but in order to achieve that high quality of results the noise in the input observations should be 0.001 mm. The question remains if this is achievable.

Which would mean that all other contributions to the path delay (tropospheric bending, multipath, ionosphere, clock errors, receiver errors) have to be modelled to an accuracy of that order of magnitude ...

– (c) The tomography models might be improved with RO measurements that goes across vertical domain of tomography model and therefore improve the geometry of the problem. Therefore, I believe there is still a room for further development of tomography models.

Yes, this is probably true. See Foelsche2011.

@ARTICLE{Foelsche2001, author = {Foelsche, Ulrich and Kirchengast, Gottfried}, title = {Tropospheric water vapor imaging by combination of ground-based and spaceborne GNSS sounding data}, journal = {Journal of Geophysical Research: Atmospheres}, year = {2001}, volume = {106}, pages = {27221–27231}, number = {D21}, doi = {10.1029/2001JD900230}, issn = {2156-2202} }

– I'm looking forward to future works of authors showing impacts of slant observations and possible conversion of their methodology to Kalman filter (e.g. Zeng and Zhang (2010)).

Interesting approach. Thanks for the paper recommendation.

– I agree that usually tomography suffers from over constraining, however in the severe weather conditions the tomography models could be quite useful. The unconstrained tomography method is feasible to produce accurate results without additional artificial parameters, which makes it competitve to the models currently used.

The author Fabian Hurter strongly doubts that in a real-world case, an unconstrained approach yields profiling performance certifiably better than collocation does.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 4895, 2013.