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Atmospheric Measurement Techniques Discussions

Interactive comment on "4DVAR assimilation of GNSS zenith path delays and precipitable water into a numerical weather prediction model WRF" *by* Witold Rohm et al.

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Anonymous Referee #1 General comments This Paper is concerned with evaluating the impact of GNSS observations within the WRF limited area data assimilation and modelling system over an area covering Poland. Previous work in the field of research is throughly reviewed. However, unfortunately, misinterpetations were found. The Paper has a sound scientific basis in the sense the effect of utilizing GNSS-based observations on the quality of numerical weather prediction is investigated. The experiments carried out are quite clearly described, although some improvements can be made. Unfortunately, in my opinion, the results of the experiments is not well enough evalu-

ated and presented. Over such a small area and for a humidity related observations it is not advisable to evaluate the impact including forecasts up to 48 h and focus on surface observations and precipitation. In my opinion one should focus on forecasts up to roughly +12h and on verification also on upper air fields. In addition the statistical significance of the results should be presented. Major Revisions of the Paper are needed, in particular regarding the evaluation of the parallel experiment. After these have been carried out the Paper can be considered for scientific publication in Journal Atmos. Meas. Tech..

[WR] We are grateful for time and effort Reviewer has spent on the manuscript, we highly appreciate these comments. With analysis of majority of the weather events in May and June 2013 we actually see that these were regional – scale events: in May a westerly flows brought a series of precipitation and advection of cold air mases, in June, south east flow brought a humid and instable air mases. In both cases analysis of cases should be limited to 12-24h as the air volume that is observed by GNSS will be entirely replaced during one full day. Therefore analysis performed in this study should be either supported by observations from most of the countries to the West and South of Poland or limited in time to one day. We decide for the second as we have only full control over GNSS data from Poland, the processing strategies, whereas all data that are not process by WUEL E-GVAP might introduce biases that are not in agreement with the one introduced by WUEL. We modified therefore Tables 2 to 4 and Figures 4 and 5 (now Figure 7 and 8) to reflect this overall change.

Please find below more detailed comments and suggestions are listed. More Specific Comments Page 1. line 11: I suggest change 'codified' to 'represented'

[WR]Corrected

Abstract.lines 15-30: Too much details in Abstract. Remove that WRF can be applied for both 3DVAR and 4DVAR and tell only what you used. Also, you do not need to tell all dates of experiments in Abstract.

[WR] We decided to correct the details regarding available VAR schemes in WRF. However we would like to keep the exact dates of experiments as it is repeatedly used in the manuscript, more as an event label, and less as time indicators.

Page 2. lines 23-24. Here I am confident that you have misinterpreted the findings of Lindskog et al. when it comes to benefit of 4DVAR against 3DVAR. As far as I can read, Lindskog et al. did not apply 4DVAR, only 3DVAR. They did an experiment with modified background error statistics in 3DVAR and from the results they concluded that the assimilation of GNSS ZTD in NWP can benefit from more general data assimilation improvements, such as enhanced description of statistical information or improved data assimilation algorithms.'.

[WR] This is rather a problem with a following paragraph: "The results were mixed: for all cases the introduction of GPS ZTD increased the humidity bias, however the improvements of clouds forecasts were observed. Authors also identify no clear benefit of 4DVAR against 3DVAR. (Lindskog et al., 2017) in their Nordic country study of GNSS ZTD impact on forecasts, confirmed that the forecasts are sensitive to thinning distance." Which suggest that Lindskog et al. (2017) were testing 4DVAR against 3DVAR. However it should point to paper by Bennitt and Jupp, (2012) that were actually verifying impact of these two varational assimilation schemes. Therefore to clear this misunderstanding we change this paragraph to: "The results were mixed: for all cases the introduction of GPS ZTD increased the humidity bias, however the improvements of clouds forecasts were observed. (Bennitt and Jupp, 2012) also identify no clear benefit of 4DVAR against 3DVAR. (Lindskog et al., 2017) in their Nordic country study of GNSS ZTD impact on forecasts, confirmed that the forecasts are sensitive to thinning distance."

Page 2, line 19. ZTD stands for 'Zenith Total Delay' not 'Zenith Tropospheric Delay'? [WR] Well in literature both terms are used, however the first one is clearly related more to the meteorology (e.g. Bennitt and Jupp, 2012; Poli et al., 2007) while the second is more often is used in the navigation/positioning type of research (e.g. Lanyi, 1984;

Askne and Nordius, 1987; Ge et al., 2000). However the opposite is also possible (Vedel et al., 2004). We decide to change the "Zenith Tropospheric Delay" into "Zenith Total Delay"

Askne, J., & Nordius, H. (1987). Estimation of tropospheric delay for microwaves from surface weather data. Radio Science, 22(3), 379-386. Ge, M., Calais, E., & Haase, J. (2000). Reducing satellite orbit error effects in near realâĂŘtime GPS zenith tropospheric delay estimation for meteorology. Geophysical Research Letters, 27(13), 1915-1918. Bennitt, G. V., & Jupp, A. (2012). Operational assimilation of GPS zenith total delay observations into the Met Office numerical weather prediction models. Monthly Weather Review, 140(8), 2706-2719. Vedel, H., Huang, X. Y., Haase, J., Ge, M., & Calais, E. (2004). Impact of GPS zenith tropospheric delay data on precipitation forecasts in Mediterranean France and Spain. Geophysical research letters, 31(2). Lanyi, G. (1984). Tropospheric delay effects in radio interferometry. TDA Prog. Rep. 42-78, vol. April, 152-159. Poli, P., Moll, P., Rabier, F., Desroziers, G., Chapnik, B., Berre, L., ... & El Guelai, F. Z. (2007). Forecast impact studies of zenith total delay data from European near realâĂŘtime GPS stations in Météo France 4DVAR. Journal of Geophysical Research: Atmospheres, 112(D6).

Section 2.1, pages 4-6. Here you need to describe more details regarding the data assimilation setup. For example, is data assimilation only carried out in the inner domain or in both. How often are model runs started (once a day?) and when (00 UTC?). Is surface data assimilation applied and what kinds of background error representation and quality control is applied. In addition please justify the model set-up choices presented in Table 1, for example why you did not use cloud microphysics in Domain 2. Also refer to Papers presenting the details of the various schemes applied.

[JG] Data assimilation was run using 4DVAR WRF DA system, only for inner domain (d02). Prediction model was started once a day, at 00 UTC. Assimilation window was prepared at 00 UTC. Background Error covariance (BE) was selected for regional application (cv_options=5) (BE depends on the WRF domain). BE was constructed based on a forecast for convection event in the first week of May 2013. Quality control was selected for SYNOP and RS data in observation processor (obsproc) and in WRFDA. For ZTD and PWAT data, quality control was conducted during it prepare process, in obsproc and last step in 4DVAR assimilation. The WRF configuration based on the best ensemble members (ens1) with small modification from ensemble system dedicated for Poland area during summertime (Guzikowski, et al. 2015).

Guzikowski, J., Czerwińska, A. E., Krzyścin, J. W., & Czerwiński, M. A. (2017). Controlling sunbathing safety during the summer holidays-The solar UV campaign at Baltic Sea coast in 2015. Journal of Photochemistry and Photobiology B: Biology, 173, 271-281.

Section 3, methodology: The cost function can be derived from Bayesian probability theory as is done in: Lorenc, A., 1986, Analysis methods for numerical weather prediction. Q. J. R. Meteor. Soc., 112, 1177-1194. then you will see that a factor 1/2 is missing in equation 1 page 29.In addition a recommend to use standard notations defined in Ide K, Courtier P, Ghil M, Lorenc A. 1997. Unified notation for data assimilation: Operational, sequential and variational.J. Met. Soc. of Japan 75 : 181–189. throughout the Paper. For example R instead of O. [WR] Thank you for this valuable remark $\frac{1}{2}$ term was inserted to the equation 1, the notation now follows the recommended one by Ide et al., (1997).

page 8, line 23 Please do not use long subroutine names from model code, not so clear.

[WR] We would like to keep the module names, as for us finding the exact location of operator in the WRFDA code was very difficult and learning it from this manuscript could greatly improve further research on the code development (e.g. better parameterisation of ZHD).

Section 2.3, Model evaluation: Here I see the main weakness of this Paper. The GNSS ZTD and PW in the first place mainly affect the 3-D distribution of humidity.

That will affect rain later on. Modifying the initial humidity state will mainly influence short range forecasts due short predictability time scales and small model domain. In addition statistical significance of results needs to be addresses .In fact already from Figures 4 in the Paper one can get a hint that one should not look for impact at ranges beyond 12-24 h. In my opinion verification scores should be re-derived using shorter forecast ranges and looking and the dependence on forecast range. In addition please look at what the data assimilation is doing at range 0 to start with and also look at forecast fields. For statistical verification do not look only at the surface but use the radiosondes you show you have in the domain for verification for different altitudes in the atmosphere. Please also prove confidence intervals to your results together with an explanation how these were derived.

[MK] We agree with this comment and we have modified and extended the model evaluation by: Tables 2-4 have been changed. The error statistics were recalculated for the first 24h (it was 48h before). This is now clarified in text. The direct comparison between previous version of tables and current one can be find in the attachments. We have compared the forecasts with high-resolution radiosonde data. The radiosonde data were available for three stations in Poland (Wrocław, Warszawa and Łeba). 95% confidence intervals were added in the plots. Similar plots, but for relative humidity only, have been presented earlier by Guerova et al (2005). The confidence intervals were calculated by adding/substracting 1.96 * standard error to the mean error. We have provided the following plots for the 12h lead time forecast (left is for relative humidity, right for air temperature). However, adding the confidence intervals makes the plots hard to read, especially in the lower layers of the model. It clearly shows that the impact of ZTD is larger than IWV in both Temperature and Humidity. However for RH the impact is more significant especially between 2.5km and 10km.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2018-263/amt-2018-263-AC1-supplement.pdf

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