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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validating the water vapour content from a reanalysis product and a regional climate model over Europe based on GNSS observations" *by* J. Berckmans et al.

Anonymous Referee #2

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The study is about a comparison of Integrated Water Vapour (IWV) between a product derived from ground based GNSS stations and two products from atmospheric models: the global ECMWF ERA-Interim reanalysis at 80 km resolution and a Regional Climate Model (RCM) ALARO-SURFEX at 20 km resolution. The comparison is undertaken over Europe for a 19-year period (1996-2014) thanks to an homogeneous reprocessed GNSS dataset "EPN Repro2" described in Pacione et al. (2017). Even though the authors conclude that both models reproduce reasonably well the behaviour of IWV from GNSS in terms of seasonal cycle and interannual variability, I found that the discrepancies noticed, in particular in terms of biases, lack of satisfactory explanations (they are most of the time highly speculative). The strengths and weaknesses of each model

are not exploited for an improved understanding of the different behaviours: the RCM has a high horizontal resolution (e.g. better descriptions of orography and sea-land contrast) but it is not constrained by observations ; ERA-Interim has a coarser resolution but the atmospheric state is constrained by observations (in particular in terms of water vapour). It is not clear at the end of the study to know if the RCM could be used for climate change scenarios with confidence. For example, could the trend in differences with GNSS noticed in Figure 3 over the 19-years jeopardize the confidence in trends that the RCM could simulate in a climate changing scenario ? I also have the impression that the added value of the "EPN Repro2" dataset does not show up since the conclusions from the study of Ning et al. (2013) with an older GNSS dataset over 11 years are rather similar to the ones reached by the authors for ERA-Interim. Since the domain is different (northern Europe) as well as the RCM some conclusions are necessarily different. From all these elements I am not favourable to the publication of this paper in Atmospheric Chemistry and Physics. To my opinion, the authors should go deeper in the analysis of the three datasets for an improved understanding of the strengths and weaknesses of their RCM in terms of IWV. They should ask themselves: what a reader of our paper can learn from the results and methodology we are presenting ? In its present form, most statements given in the paper are either descriptive or without proper explanations.

Please find below a number of specific comments (P = Page and L = Line ; from page 7 the line numbering does not make sense):

P1L3-5: You should also mention that you will also compare ERA-Interim analyses against IWV observations

P1L9-10: From this sentence the reader may think that this bias comes from the lateral boundary conditions. This is not obvious. When examining Figure 6, stations closer to the boundaries do not have biaises more consistent with ERA-Interim. I have the impression that there is more consistency with stations in the inner part of the domain (but this is probably just consistent problems between the two models with orography

over the Alps).

P1L11-12: This explanation is far from being convincing. Summer biases linked to evapotranspiration are generally producing warm/dry biaises or cold/wet biaises. A cold and dry bias is more likely to come from an underestimation of the downward radiation at the surface reducing both turbulent sensible and latent heat fluxes.

P1L20: I am surprized to learn that radiosonde and satellite instruments measuring water vapour are not adequate for the validation of climate models. I can understand that they are not sufficient but nevertheless there are useful climate records in terms of radiosonde measurements (e.g. the GRUAN programme) or satellite humidity from sounding and imaging instruments (e.g. AMSU-B or SSMI sensors ; Climate Monitoring Satellite Application Facility from EUMETSAT).

P1L4: I do not understand why the high temporal variability is important for climate applications, whereas I can understand that it is crucial for short-range weather forecasts.

P2L2: It is wrong to state implicitly that radiosondes cannot provide measurements in all weather conditions. They provide vertical profiles of atmospheric parameters in clear sky and cloudy conditions. The issue is more relevant for satellite measurements and particularly in the infra-red and the high frequency microwave spectral regions. Putting forward the argument for GNSS of high temporal and spatial resolutions is misleading. Radiosondes have a much better spatial resolution on the vertical than ZTD or STD (Slant Total Delay) values from GNSS receivers. On the other hand, geostationary satellites provide measurements every 15 min. This opposition of GNSS observations vs. "traditional systems" is presented is an unfair manner.

P2L6: GNSS signals are also delayed by the ionosphere. This delay is non negligible and has to be removed for a meaningful interpretation of the tropospheric part of the signal.

P2L9: The "water vapour weighted mean temperature" is a very specific concept in

order to convert the ZTD into a IWV. The proper reference is more likely Davis et al. (1985).

P3-4: Section 2.1 could be shorten. It is a very classical description (I found exactly the same with similar formulae in Ning et al., 2013). Since numerical models can easily compute ZTD values that are closer to the actual measurements, why are comparisons done in terms of IWV that require additional assumptions on the observation side ?

P4L12-17: What is the purpose of this discussion ? Are the numbers proposed for these 3 stations representative ? Could they be used to put an error bar on IWV from GNSS data (e.g. interpretation of Table 3) ? I am wondering how useful they can be given the strong seasonal cycle of IWV. Different values are expected between winter and summer seasons.

P5L25: I am wondering why the soil moisture and soil temperature are included in the lateral boundary conditions since soil processes only describe vertical transfers (no lateral transfers). It is rather surprising since the surface scheme SURFEX is very different from the ECMWF land surface model.

P5L8: A reference to the description of the AROME model is needed : Seity et al. (2011)

P5L27: Can you explain the sentence "they are introduced as initial conditions across the domain" since the previous sentence is about latereal boundary conditions ?

P6L1-8: It is not clear if this adjustment to orography is different from the one described for ERA-Interim (making reference to Hagemannn and Bengtsson (2003)) ? If it the case please explain why.

P6L11-12: The sentence can be simplified as "station up to a pressure level corresponding to a height of about 20 km (that is sufficient to capture the entire columnar water vapour)"

P6L17: replace "(Fig. 1)" by "displayed in Fig. 1"

P6L18: Statement already mentioned

P6L21-26: Six lines of explanations for one specific station are not necessary

P8L13-14: There is nothing to support such statement : "this is due to the lack of assimilated ground-based observations by ALARO-SURFEX". The paper of Ning et al. (2013) does not provide any support.

P9L17: The statement cannot be seen in Figure 3a

P9L23-2: Why the fact that there is strong seasonal cycle of the bias in ALARO-SURFEX explains the mean bias values ? The statement "This results in a mean IWV ..." does not make sense to me. It has more to do with positive and negative values between winter and summer.

P10: Figure 3 : add labels a) and b) as mentioned in the text. Once you have defined IWV and GNSS there is no need to rewrite again the meaning of the acronyms.

P10: Legend of Table 2 is incomplete

P11L4: Rewrite the sentence as : "explanation is the increased number of observations in the ERA-Interim data assimilation system in the most recent years"

P11L11-12: Rewrite the sentence as : "This bias in statistically significant in winter and spring because of lower IWV values in these seasons"

P13L3: The coincidence of the precipitation biases with the IWV biases for some seasons (autumn, winter, spring) is unclear to me (are you talking about the amplitude, the sign, ... ?).

P13L8: The link between the negative temperature bias in summer and lower evapotranspiration rates is not straightforward and not explained.

P13L12: The link between positive bias in precipitation and in IWV is not straightforward. One could argue that if more water is lost by the atmosphere through precipita-

tion the column contains less water vapour.

P14: The comment of Figure 6 is very limited. The biases along coastal regions that the ALARO-SURFEX model at higher resolution do not seem to handle better with respect to ERA-Interim are not discussed. Issues the orography over the Alps appear clearly but they are also presented and discussed in Table 3.

P16: Table 3: are the statistics for stations above 1000 m (only 4) reliable ?

P16L6: You should provide an explanation why ALARO-SURFEX is more sensitive to station height than ERA-Interim. This is not intuitive. Since this RCM has a higher resolution than the ECMWF model used for ERA-Interim, the differences should be smaller and therefore less sensitive to extrapolation assumptions.

P17: Section 3.4 on hourly variability is difficult to understand since as presented in Figure 7 and 8 the amplitude of the diurnal cycle varies during the day (different values at 00, 06, 12 and 18 UTC) with small end even negative values. This amplitude should be the difference between the minimum and maximum values over 24 hours. Moreover, I do not see the interest of presenting both Figures 7 and 8. To my opinion, Figure 8 is sufficient.

P19L4: The statement "we believe" should be avoided in a scientific paper. In a scientific context, this is an hypothesis. Do you have any mean to verify or support it ?

P19L11-12: It is difficult to understand why when the RCM is strongly influenced by the boundary conditions of ERA-Interim, it can even outperform it.

P19L13-14: The improvement of the IWV diurnal cycle due to an upgraded microphysical scheme is not obvious to me. It is also not clear to which seasons you are referring to.

P20: Again in the conclusions the various explanations given along the paper are far from being convincing.

P21L1-2: Why at 20 km the feedbacks between water vapour and other meteorological variables "could lack sufficient representation" ? Which processes to you have in mind ?

P21L4-5: I am not sure to understand what the proposed methodology is. How would you perform assimilation of GNSS data since the ALARO-SURFEX model is run in climate mode ?

P21L7: Add a reference to ERA5

I have many additional minor changes (mostly rephrasing for clarifications) that I do not find necessary to include at this stage. A recommendation would be to have the paper corrected by a native English speaking person.

References :

Davis, J. L., T. A. Herring, I. I. Shapiro, A. E. Rogers, and G. Elgered, 1985 : Geodesy by radio interferometry : Effects of atmospheric modeling errors on estimates of baseline length. Radio Science, 20, 1593-1607

Seity, Y., P. Brousseau, S. Malardel, G. Hello, P. Bénard, F. Bouttier, C. Lac, and V. Masson, 201 : The AROME-France Convective-Scale Operational Model. Mon. Wea. Rev., 139, 976–991, https://doi.org/10.1175/2010MWR3425.1

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