Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., author comment AC1
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Reply on RC1

Xavier Calbet et al.

Author comment on "Horizontal small-scale variability of water vapor in the atmosphere: implications for intercomparison of data from different measuring systems" by Xavier Calbet et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-111-AC1, 2022

Answers to the Reviewer are hghlighted in bold and in sentences starting with **

Comment on amt-2022-111
Anonymous Referee #1

Referee comment on "Small scale variability of water vapor in the atmosphere: implications for inter-comparison of data from different measuring systems" by Xavier Calbet et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-111-RC1, 2022

Review of "Small scale variability of water vapor in the atmosphere: implications for inter-comparison of data from different measuring systems", Calbet et al., ATMD 2022.

**** Many thanks for the positive review and constructive comments which will for sure add to the readibility of the paper.**

The study and results presented in the manuscript address an interesting question to further understand atmospheric processes. The organisation of small-scale WV structures is analysed with a small set of satellite data and a global NWP field. The present study corroborates previous results obtained with radiosondes, making the experience that WV is indeed organised as per the Kolmogorow 2/3-turbulent law.

The paper is found well structured, making a clear didactic introduction of the turbulent theory and how it can relate to satellite images. The redaction and explanations of the datasets and results are overall of good quality, though inequal in places.

I recommend the publication of the manuscript subject to revision considering the comments below:

General:

- try and harmonise the style and level of details provided. It is found too vague/hasted in places.
**** Tried to refine this in this new version.**

- the authors talk about a demonstration with satellite data the WV obey the 2/3-rule. However the study is made with a rather limited set of satellite observations (1 scene of SEVIRI, 2 scenes of OLCI) which per se statistically limits the generalisation of the conclusions. The figure 9 is a nice result, but is qualitative and one of a kind. The authors should acknowledge this, repeat the experience with e.g. other seasons, climate zones and/or discuss the representativeness aspects vs generalisation.
**** Added in the conclusions "Given the caveat that this is a limited study (not all seasons or climate zones are covered), the fact that similar results are obtained from measurements at different layers and from various instruments seem to proof that this is a universal property which applies at all these ranges."**

- the ECMWF global model seems a rather unapropriate choice for this study which focus on atmospheric processes below 6km. The authors are encouraged to include regional models fields, at least in addition to the IFS, and in any case to convolve OLCI data at the model resolution in an additional analyses. In particular for the region Germany/Czechia, there should be valuable high-res regional models to work with. The author's findings would be also certainly of interest to the modellers.
**** Absolutely agree, See coments below. We did not have a regional NWP model available at the time of writing the paper (and still do not have one now).**

- the authors should make clearer what are the stakes of characterising the WV organisation with satellite data. This is briefly touched on in the introduction (e.g. for OBS-CALC computations) or in the conclusion (e.g. bringing the stochastic components in weather forecasting), but would deserve some more elaborating. Are there anything interesting beyond just observing with satellite data that WV is organised after the 2/3-law. Is there a metric about WV variance/turbulence that could be derived from analysing satellite data and which be supplied to the forecasters to understand better a given situation?
**** Many thanks for this proposal. Added one sentence in the abstract and another one in the conclusions.**
Asbtract: In terms of weather forecasting or nowcasting, the water vapor variability could be important in estimating the uncertainty of the atmospheric processes driving convection.

Specific:

Fig.1: Typo "field AT larger spatial scales"
**** Changed**
Fig.1: would deserve a little more explanations in the caption: what are the geophysical parameters in those (left and right) fields?
**** Extended**

P8.L10-19: The strategy is not clear. What is meant by perturbing WV at all levels? Are you simply training a lienar regression based on synthetic data? And then only applying the "retrievals" to cloud-free real observations? The concepts behind the approach could be elaborated more explicitely upfront.
**** It is explained,hopefully, better now**

P8.L12: "we start by using an atmospheric profile representing all other atmospheric profiles in the selected region" it is hard to believe that the profile in the cyan tile is representative of the large red-squared area... it that what is meant? Can you clarify and elaborate the assumptions behind?
**** Please note only a rough first order approximation of the WV is needed. To make the considerations more understandable to the reader, a simple regression is used. This is clarified in the tex now.**

P9.L11: vague style. You should speak about "uncertainty estimates associated to each pixel, which on average is expected to lie around 0.33mm".
This by the way sounds extremely ambitious ! Error estimates in OEM greatly depend on the assumptions made on the background and observation errors, and sometimes may not be fully representative. Has the uncertainty estimate been validated? It should be referred here.
**** Clarified and reference given in this sentence.**

§3.4: Have the authors considered using a regional (convective scale) model? They would have the potential advantage to resolve more atmospheric processes and at finer scales - hence be of potential higher relevance for the present study which explores WV structures on kilometeric if not subkilometric scales. In particular in view of the OLCI study over a smaller continental portion, this would be very informative. The NWP field from the global IFS model feels a bit disapointing compared to Fig. 3. In what is it or is it not a limitation for your study? This should be envisaged or at least explained why regional models are excluded.
**** We agree with the referee. Ideally we would like to compare to a high resolution regional model. Unfortunately, we do not have direct access to a high resolution NWP model,so we settled for a global one in order to finish the paper. This is now clarified in the paper. Hopefully, in the future, this exercise will be performed. Please also note that this is not a paper in which NWP models are thoroughly compared to measurements.**

P9.25: typo "it is A structure"
**** Seems correct as it is. No change.**

P10.L18-20: stats do mix-up spatial correlations from very different altitudes, which one would expect for water-vapour would obey to very different atmospheric processes and turbulence regimes. How is that an issue for the study and what your are trying to evaluate regarding the implications of WV spatial structure (beyond the fact that satellite confirm the 2/3-law expectations)?
**** To make this clearer, added in the first paragraph of the conclusiuons "Given the caveat that this is a limited study (not all seasons or climate zones are covered), the fact that similar results are obtained from measurements at different layers and from various instruments seem to proof that this is a universal property which applies at all these ranges."**

P10.L32: "These kind of figures could be reproduced for any other pixel on the complete OLCI field, showing similar results.". Evasive statement. Do you mean such analyses WERE successfully performed and showed similar results, but you're displaying just 2 here? Or that you could potentially repeat this approach and you expect finding the same results? If the former, I suggest working more explicitely and indicating how many such cases were computed. If the latter, I would avoid what comes across a hypothetical statement, and would support it by additional experiments.
**** They have been made for all pixels. Liekwise for the 2/3 law fitting. Explained better in the text now.**

P11.L17: indeed! a regional model would be a better option (and interesting feed-back to the modellers too). In what is a 10-km sampling (resolution of phyiscal processes is typically 2 to 3x coarser) model interesting for your study?
**** Agreed. As explained above, no regional high res model was available.**

P13.L15-20 The discussion about ECMWF data is too short. The sampling is about 10km but the resolution of the physical processes is much coarser. How about smoothing OLCI TCWV field with e.g. a 20-km or 30-km Gaussian running window and repeating the same analysis and intercomparison? The value of the discussion on the two fields at teir

respective scales is not clear. Using a regional model (with same consideration for smoothing OLCI to a kilometric Gaussian average) apperas more interesting at first glance.
**** Tried to explain it better now. Also added that further investigation is required. Possibly using a high res NWP model.**