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Atmospheric Measurement Techniques Discussions

Interactive comment on "Cloud liquid water path in the sub-Arctic region of Europe as derived from ground-based and space-borne remote observations" *by* Vladimir S. Kostsov et al.

Anonymous Referee #4

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Liquid water path (LWP) is an essential climate variable which is rather difficult to measure. This results in a large spread of global mean LWP observations between 30m to 90 gm-2 which does not allow a proper evaluation of climate models today. Therefore, the investigation of LWP measured from the ground by microwave radiometry (MWR) and observed from geostationary orbit by SEVIRI at the site of St. Petersberg is of great interest. The study includes many interesting aspects but I would like to get a more solid statement to which degree one can rely on the different data sets.

While needing some editorial work, the manuscript is clearly written, addresses an important topic and presents interesting results which can have high impact on the

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Discussion paper

future observation network. Therefore, I recommend publication once the following points are addressed.

MAJOR POINTS

1) The manuscript needs to address the issue of LWP accuracy in more detail. This starts with emphasizing the lack of high quality measurements of LWP (being an essential climate variable), see for example the discussion by Lohmann and Neubauer (2018) who show that the global mean LWP varies between 30 and 90 gm-2 in the different global data sets. Most important, more information on the accuracy of the two LWP measurement techniques is needed. The manuscript frequently mentions the high quality of the ground-based microwave (MW) measurements but no quantitative values are given. Can they be used as a reference to estimate SEVIRI LWP accuracy? What are the advantages and disadvantages of both methods? What is their uncertainty? Do they have the same detection limit, i.e. I would expect SEVIRI to have higher sensitivity for low LWP values? I am wondering why the authors do not show the joint LWP distribution, i.e. two dimensional histogram with frequency of occurrence color code, which best illustrates the agreement of both data sets. The authors only provide the mean of WH (17 gm-2) and the RMS (16 gm-2) but do not make a statement that this would relate to an relative error of about 100%.

2) The LWP difference between land and sea for is strong and is one of the most interesting points of the paper. The paper takes it for granted that this is real but there needs to be a discussion/investigation whether this might be caused by a shortcoming of the SEVIRI, e.g. maybe due to the difference in surface albedo between land and sea. Furthermore, if it is true, a physical explanation for the LWP gradient needs to be provided. A potential explanation is the frequent presence of a high pressure system over the Baltic Sea and the associated subsidence which causes adiabatic warming and low cloudiness. With this explanation it might be better to separate the LWP time series into weather type situations rather than warm & humid (WH) and cold & dry (CD).

3) LWP statistics: LWP is highly variable in time and space and this variability strongly depends on the cloud type, i.e. is strongest for convective boundary layer clouds. Therefore, it is difficult to make solid statistics even if a two year data set is considered. By spitting the data further into individual months and climatic conditions the distributions become rather erratic and should not be overinterpreted. Smooth distributions require rather long time series (see Caddedu et al., 2013, Kniffka et al., 2014). Therefore, I recommend to just separate into the warm/humid and cold/dry regime or seasons at the most.

4) In respect to statistics the comparison with reanalysis is also difficult as only one instantaneous value every 3 hours is provided and thus only 8 per day and is not comparable with the better sampling of SEVIRI and MWR measurements. Thus it is the question whether the interannual variability shown add the end study is due to sampling or real and would require testing of the statistical significance. While I find it very important to make the point of high interannual variability I would remove the reanalysis aspect from the study as the data are not comparable in terms of spatial (80 vs 10 km) and temporal (8 to about 50) scales even with the coarser SEVIRI LWP data and also represent a mixed land & sea pixel.

5) The paper contains many plots and many could be eliminated. Why are s lengthy by showing both median and mean LWP. What is the benefit? The LWP distribution is strongly skewed towards low values and thus the median LWP values are typically lower than the accuracy. I would suggest to keep only the mean. If you would like to show the median then you could put it into an appendix.

M. P. Cadeddu, , J. C. Liljegren„ and D. D. Turner, 2013: The Atmospheric radiation measurement (ARM) program network of microwave radiometers: instrumentation, data, and retrievals. https://www.atmos-meas-tech.net/6/2359/2013/amt-6-2359-2013.pdf

MINOR COMMENTS:

L13: Provide also the relative error as the mean LWP is rather low and thus errors should often be in the order of 100 %.

L22: You just report on what you did but what was the result? Please extent.

L40: There are certainly many more studies on (sub)arctic clouds than the one by Garett and Zhao, the point to make here is that the measurement network is rather coarse in that region.

L52: Here, you need to emphasize on the importance of LWP as essential climate variable which is difficult to assess due to its high spatio-temporal variability, cf. Van Meijgaard and Crewell (2005) for the difficulties to compare LWP with models of different grid size.

L54: You need to explain the two satellite measurement principles for LWP from satellite and provide their limitations and uncertainties. 1) VIS/NIR observations only possible during daylight (not mentioned in manuscript) but available from geostationary satellite. This method needs to make assumptions on the vertical structure of the clouds as only the top can be sensed. 2) Microwave imagers on polar orbiters measure the emission signal which is only possible over the radiatively cold ocean - here you can cite Elsaesser et al. (2017) for the climatology and Greenwald et al. (2018) who investigates the uncertainty albeit by taking VIS/NIR as truth. Note, that due to the large footprints and the differences in emissivity between land and ocean no information for coastal pixels is available.

L55: I don't think it is necessary to list the satellite instrument names - if you do you need to provide the explanations for all acronyms.

L69: The uncertainty of ground-based MWR for LWP needs to be given

L87: Also the size of the footprint is larger than at the other locations

L92: "Since the LWP values can be essentially different over.." has this been shown in the literature or is this your result?

L95: Not give only one of the main goals but provide them all, ...for example the investigation of reanalysis quality...

L99: Why not cite Rose et al, 2005 at this stage - it is cited on 160

L129: 11km? The size at St. Petersberg needs to be mentioned

L144: It is not clear to me why only days which are completely free of rain are included in the comparison? This eliminates many data which are needed as the high variability of LWP makes statistical analysis difficult. If days with precipitation are excluded the mean diurnal cycle derived later on is only the mean of a subsample and not overall. At least this needs to be made clear maybe by giving it another name.

L146: How is a gap defined - a single 1 sec measurement?

L147: You speak about convergence - is the quality flag from the physical retrieval?

L160: In general, I think you need to explain why this is important: The retrieval algorithms are typically non-linear. Therefore, the retrieval needs to be made on high resolution brightness temperature data and subsequently then the LWP can be averaged but not vice versa. Note, that since the paper by Rose et al is already more than 10 years ago and since when resolutions of NWP have increased. It should be mentioned that situations can be very different and in particular convective boundary layer clouds have high variability.

L165: Please also write "stable" as this has nothing to do with the thermal stability but more with constant conditions. . L170: For a comprehensive discussion on how to compare LWP from ground-based observations and with spatial estimates from NWP models of different resolutions see Van Meijgaard and Crewell., 2005.

L180: Why boxcar? For simplicity?

L190: As Kostov et al. (2016) is not an open access paper the values for distinguishing WH and CD should be provided here. There is no surprise that WH is more frequent

as SEVIRI needs light.

L200: How good is the the geolocation of SEVIRI?

L203: especially as liquid clouds are at low heights.

L227: The statement is not correct - I still see the gradient.

L240: Different sampling and averaging times are chosen which provides some information on the error introduced by representativeness of the measurements. However, the optimal combination should depend on the actual weather situation - which should be discussed. Could the spatial distribution of SEVIRI or NWP might provide information to optimze this on an individual base.

L257: The agreement in daily mean LWP between SEVIRI and HATPRO in Fig. 7 is claimed "very good". I don't think so as many days especially in the beginning have zero LWP in SEVIRI and relevant values for HATPRO. Try to make a more quantitative statement here. You could also make a table for the daily means similar to table 4

L262: Is this for instantaneous or daily LWP?

L290: Section 2.2. needs to mention that SEVIRI retrievals fail in the case of strong vertical gradients and especially during rain. Why not use standard deviation of 3x3 SEVIRI pixels to identify (and eliminate) inhomogeneous situations?

L329 and Table 4: Why has this analysis not been done over the whole time period? It is important to do for generalization and it is a good transition to the next section.

L332: The title of the section should be changed to something like "Statistical LWP assessment". In this section I would have expected you - after looking at the mariginal distributions - to show the joint distribution which I am strongly missing here. This provides a much more direct view on the systematic and unsystematic components. In this sense L349: The name of section "5.1 Instrument differences" is a bit misleading. With this title I would have expected a discussion on the different sensitivities of the

instruments. I think no subsection headings are needed here. I recommend the authors to look at Tian et al. 2016 https://journals.ametsoc.org/doi/pdf/10.1175/MWR-D-15-0087.1 for a better error model.

L335: If you say "low compared to" but don't give any values this becomes difficult for the reader. It is also not possible to compare it with the full disk as many different climate zones are - better compare with other mid-latitude sites, cf. Cloudnet sites.. Her you should integrate the section 5.2 which somehow does not fit to the end. There a range for the distribution 0.2 - 0.6 kkgm-2 is given. This is better quantified by an interquartile or interpercentile range. Would be also good to give the numbers for the mean in both regimes. i

L340: I don't think that you can generalize the bimodal distribution for all February and September months with only two years and so much fluctuation in LWP. This is just due to certain weather patterns.

L340: Talking about 17% creates an impression of high accuracy - albeit the distributions are highly uncertain as they are determined by very view events and bimodality just comes by chance. You should provide the total number of samples for each distribution and as I suggest in the beginning just concentrate on the broader picture.

L345: Kniffka et al have a much larger and solid data base so that the comparison of the distributions is not fair here. Especially the bimodality (secondary maximum) is most likely just by chance and would not exist if a decade of data would be used - this could be checked by SEVIRI.

L355: Some background needs to be given on the method and an assessment of the "uncertainty".

L361: I would have expected the highest error in summer as the high variability of convective clouds reduces the representativeness of the HATPRO measurements for the SEVIRI footprint and complicates the comparison in contrast to situations with more

stratiform conditions typically more frequent in winter. cf. Slobodda, J., A. Hünerbein, R. Lindstrot, R. Preusker, K. Ebell, and J. Fischer, 2015: Multichannel analysis of correlation length of SEVIRI images around ground-based cloud observatories to determine their representativeness, Atmos. Meas. Tech. , 8, 567-578, doi:10.5194/amt-8-567-2015.

L360: As the authors also mention later on clouds with LWP of >0.4 kgm-2 are likely to precipitate and therefore I would suggest the authors to eliminate all SEVIRI retrievals above a certain threshold as the retrieval is not built for rain. Furthermore, it is only fair as rainy HATPRO measurements are eliminated as well.

L458: Why not mention the corresponding advection velocity?

L464: What is the accuracy of the 3.1 km estimate? I would simply say around 3 km.

L368: and the large footprint at this high altitude.

L479: Why do you know that it is log-normal? Did you test it? For this you probably need many more data so I would be more careful with the statement. Similar with the bimodality which is certainly due to poor sampling of different weather types and not a climate phenomenon.

Table 3: You also need to mention the number of samples, the mean value (so one can assess the relative error), bias corrected RMS and the correlation. Which SEVIRI pixel is taken?

Table 4: Not only the four cases but also the whole time period and WH and CD should be shown.

Figure 1: It is a bit irritating that pixel 242 is discussed but not shown.

Figure 3: The contouring provokes more interpretation than possible from only 3x3 points. Please plot them as blocks. Maybe you can even integrate them into the lowere left corner of fig. 2 as a zoom in. Same holds for all figures with 3x3 points, e.g. 4 and

5. Then you can also show WH and CD together.

Figure 6: You should give the number of samples, bias, rms, bias corrected RMS and correlation in a table similar to Table 3.

Figure13: Which averaging do you use here? I am again concerned with the statistics. Either leave this out as monthly data have poor statistics (give number) or if you want to do it keep it then use the full Taylor diagram. https://climatedataguide.ucar.edu/climate-data-tools-and-analysis/taylor-diagramspart

van Meijgaard, E., and S. Crewell, 2005: Comparison of model predicted liquid water path with ground-based measurements during CLIWA-NET. Atmos. Res., Special issue: CLIWA-NET: Observation and Modelling of Liquid Water Clouds, 75(3), 201 - 226, doi:10.1016/j.atmosres.2004.12.006.

Grammar, typos and reformulations

L10 - spell out SEVIRI and mention geostationary

L4: "have shown considerable differences" in you study (cf Fig.4) or in literature. Always make clear what are your results and what is known from the literature

L25: - this is not the only interest, I suggest: "There is increasing interest in the sub-Arctic region due to the so-called “arctic amplification” effect that .."

L28: "The large seasonal and interannual variation in low- and high-pressure systems and associated environmental variability due the location of the Baltic Sea between the North Atlantic and Eurasian air masses makes North Europe especially important to study.."

L57: "This study exploits the ... operating at..."

L65 Cloudnet not explained

L112:"in every detail" -> in detail

L125: Order of Dee and Dankers

L133: Channel "at" 1.6

L129: One sentence does not make a paragraph.

L171: Suggestion: Roebeling et al. (2008a) assumed Taylor's frozen turbulence hypothesis to relate an assumed wind speed of 10 m/s SEVIRI field of view (4 x 7 km2 resulting in a 20 min averaging period which Roebeling et al (2008b) extended to 30 min.

L182: The term synchonization does not fit so well - better use collocation or coincidences.

L184: CPH - all acronyms need to be explained period assuming the wind speed about 10 m s-1 and the).

L185: I think the argument goes into the wrong direction. The MWR gives you LWP in all situations but SEVIRI can not do so in case there is ice. Better write "Since SEVIRI cannot retrieve LWP..."

L201: Greuell and Roebeling (2009) studied the influence of the parallax effect (the horizontal displacement of a cloud viewed by SEVIRI due to its elevated height) on the comparison of SEVIRI and ground-based microwave LWP:

L285: rain

L354: "The coloured circles...." The figure caption does not need to be repeated.

L369: ..that the possible„

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