

Interactive comment on “Quality assessment of water cycle parameters in REMO by Radar-Lidar synergy” by B. Hennemuth et al.

Anonymous Referee #1

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General comments

- This is a rather long - nevertheless - important paper on the use of state-of-the-art instrumentation for an integral evaluation of regional climate models. In order to make the paper more attractive for the reader it would be beneficial to shorten the paper (see some suggestions in the specific comments).
- The innovative aspect of the paper lies in an integral evaluation of the water cycle reproduced by a regional climate model. Therefore this should be stressed in the introduction for example by raising the question of an accelerated water cycle in the future climate.

- Bringing together observations (point/profile) together with model forecasts of one grid cell is a challenging task. In this respect 4.4 is one of the most important sections. For example the “effective” model resolution is certainly worse than two delta x (see for example Skamarock 2004. line 13 needs to be changed to “at least of one mesh size”. In order to justify the statement “one vertical grid box” the average height and standard deviation (on model resolution) as well as the vertical model resolution needs to be given. In respect to clouds several methods to match model and obs as well as the consideration of instrument limitations have been suggested (for example Protat et al., 2006, van Meijgaard et al., 2005). It might be worth discussing the pros and cons of the approach chosen to match model and observations.

Specific comments

- The paper attempts a statistical as well as a process oriented validation. For the statistical evaluation the number of occurrence of the observation is of utmost importance. Therefore the authors should provide more information on that - as well in the text (4.1) but also in the figures, e.g. Fig 14 18 should mention: number of samples, bias, RMS, correlation.
- The structure of section 2 is a bit confusing with 2.1 to 2.3 describing instruments and then moving on to 2.4 to 2.9 with meteorological parameters. I recommend to split section two in either two sections or two parts (2.1 Instrumentation and 2.2 Observed parameters and their accuracy). Also the order of the parameters is not clear to me: maybe it is more logical to start with soil moisture go on to humidity profiles, water vapor fluxes, boundary layer height and finally clouds and precip? In the same line the organisation of section 5 is confusing since in 5.1 the humidity profiles are used to detect a too low BL in the model and then

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- 5.2 is again called boundary layer height. It would be better to directly move the BL height studies to page 8477, line 15 and then move over to the fluxes.
- Shortening of the paper: For example the descriptions of figures in the text (upper panel shows..etc) could be shortened focusing more on the results of the figures and stating more clearly why the respective figures (“by looking at ..(Figx) one can deduce..”) are shown: Do you really need all 25 figures to come to your conclusions? Why do you need for example Fig. 23? Table 1 2 could also be combined. Fig. 12 could be integrated into Fig. 13 as well as Fig. 22 into Fig. 20. It is also quite confusing that so many different days are shown in the figures as examples. It would be nice to show all parameters for one or two days (clear cloudy) and then move to the statistical view.
 - 2.1 Lidar Systems: Please make the difference between the lidars and ceilometer clear, wavelength, sensitivity, operation mode
 - 2.8, line 9. This statement is a bit difficult since some cirrus clouds are also missed by lidar if there is a cloud below? Has this been included the estimation of the factor 2.7? A detailed investigation on this issue has been performed by Protat et al, 2006 and might be worth citing here.
 - 5.3, line 23: Why do lidar derived fluxes and surface observations differ so strongly?
 - 6. Discussion: 8486,lines 25+ and 8487, lines 19-20 both state the insufficient retrieval algorithm from cloud radar reflectivity. The two involved paragraphs are rather similar and need to be better separated in their scientific content.
 - 6. Discussion: While it is mentioned in the introduction mentions the important role of clouds in being a modulator for radiation this is not further investigated. Especially in respect to the underestimation of low and mid level cloud cover (8487,

line 14) in REMO it would be important to assess the impact on the available surface radiation. The necessary data should be available through the energy balance station. Why were these not considered?

Technical corrections

The paper suffers from some language problems. I only note a few here and recommend a thorough revision

- Abstract, line 13: replace “fit” by “match”
- 1. Introduction, line 14: replace “a lot of quantities can be derived” by “several quantities can be derived simultaneously”
- 1. Introduction, line 22: The 4D-Clouds project was a BMBF AFO 2000 project. It only joined experimental forces with BALTEX Bridge
- 2.1 Lidar systems, line 7: you shouldn’t use a single sentence as paragraph
- 2.4 Why is it called humidity field not humidity profiles?
- 2.7, line 21: restriction of fluxes to boundary layer: There shouldn’t be strong fluxes above the BL anyway
- 2.8, line 11: replace “is” by “are”
- 2.8.1, line 3. Better start the paragraph with the goal, e.g. that you want to determine continuous cloud fraction profiles. The phrase “evaluated” should be replaced by “derived” or “analysed” since evaluation is done later..
- 2.8.1, lines 7-10: formulation is vague and unclear

- 2.8.2, line 11: existS
- 2.9, line 14: figure 25 is cited before Fig. 17
- 4.3, line 28: point at end of sentence
- 5.1, line 13: diurnal evolution?
- 5.2 line 2: Please use clear names/variables for the different boundary layer heights in 4 and 5.2
- 6., 8486, line 14. Only one day is shown in Fig. 22. This is not convincing!
- caption Fig. 6, What does “single file” mean? Day? Season?
- 6., 8485, line 22: ..
- 6., 8486, line 5: ..
- 6, 8487, line 3: Change cleared up into “under revision”
- Fig. 13 shows some artefacts (high correlation) in the beginning and end of the observations which are probably due to the low number of samples. All grid boxes with less than about 10 samples should be omitted since a correlation coefficient or RMS is not meaningful.
- Fig. 21: What is the absolute number of cases used?
- Fig. 25: For IWC and LWC the PDF is shown - why not for precipitation?

Protat et al., 2006: Impact of conditional sampling and instrumental limitations on the statistics of cloud properties derived from cloud radar and lidar at SARTA GEOPHYSICAL RESEARCH LETTERS, VOL. 33, L11805, doi:10.1029/2005GL025340.

SKAMAROCK, WILLIAM C., 2004: Evaluating Mesoscale NWP Models Using Kinetic Energy Spectra, *Monthly Weather Review*, 3019-3032.

van Meijgaard, E. and S. Crewell, 2005: Comparison of model predicted liquid water path with ground-based measurements during CLIWA-NET, *Atmos. Res.*, 75(3), Pages 201 - 226.

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