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Interactive comment on “The Atmospheric Radiation Measurement (ARM) program network of microwave radiometers: instrumentation, data, and retrievals” by M. P. Cadeddu et al.

Anonymous Referee #1

Received and published: 6 May 2013

The paper amtd-6-3723-2013 by M. Cadeddu et al. describes the current status of the ARM network of microwave radiometers. The topic is interesting and the manuscript is clear and informative. I suggest to accept the manuscript subject to the minor revisions listed below, which I believe will improve the clarity of the final paper.

page 6, line 3: It has been demonstrated in the past that the consistency between clear sky observations from two colocated MWR does improve when instantaneous tip curve is applied to both instruments. The authors should discuss the implications of their choice to not updating the calibration every time a new tip curve is collected.

page 6, line 11: The authors should explain what happens in case of long periods with

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no clear sky, as there are situations in which such conditions may persist for days. The authors say that "the receiver gain is monitored through frequent viewing of the internal black body target", but it is not clear if the monitoring is followed by any kind of correction. Is this method sufficient to meet 0.3 K estimated accuracy?

page 7, line 16: How well is known the absorber emissivity? What is the uncertainty of the black-body assumption?

page 7, line 21: Even if the accuracy of the temperature sensors is approximately 0.1 K (it was 0.2 K in the first draft, which one is correct?), the uncertainty in measuring the absorbers' physical temperatures is larger, due to thermal drifts within the absorber material. How many sensors are in place? The authors should give more information and explain if this effect is taken into account in their error propagation analysis.

page 10, line 1: If the continuity of operations with the two-channel MWRs was a requirement, the authors should explain why the new radiometers MWR3C have different field of view and central frequency (at one channel at least) with respect to MWR.

page 13, line 16: The authors should refer to previous studies to support the statement that higher frequency channels reduces the PWV and LWP uncertainties. Some good ones are already in the reference list.

page 15, line 5: The authors should motivate their choice to include only positive LWP values, as this may bias the mean value with respect to the mean of the set used for training the statistical retrieval.

page 15, line 13: The authors should state clearly that the radiosonde profiles used to compute PWV in Figure 12 are the original measurement, i.e. not scaled by any MWR measurement (as the ARM Isonde value-added product).

page 15, line 25: The sentence "Uncertainties in the MWR-STAT retrievals are derived by applying the derived coefficients to the training ensemble" seems to suggest that the coefficients are derived from and applied to the same ensemble; this is probably not

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the case, so I'd suggest to change "training" with "test" or rephrase the whole sentence.

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