General comment

The study "Mitigation of bias sources for atmospheric temperature and humidity in the mobile Weather & Aerosol Raman Lidar (WALI)" by Julien Totems, Patrick Chazette and Alexandre Baron provides a thorough description of the the WALI system both from the point of view of the technical characterization of the lidar transciever and the performances in terms of bias and RMS. The article is well written, easy to read and exhaustive. All topics are described and supported by either previous literature or statistical studies performed by the authors. In Sect. 4.3, the comparison with the radiosounding, in addition to calibration purposes, brings useful information about the quality of the WVMR and temperature data. Accepting the 12 km distance between lidar and in-situ measurements, and then accepting a higher RMS due to slightly different atmosphere, it allows to use the bias and RMS values as solid evaluation of the WALI performance. There are only few technical comments/remarks that should be addressed before publication. I strongly recommend this manuscript to be published in AMT.

Technical comments

Abstract: while the abstract sets the main objectives of the study within the state of the art, it does not state any quantitative result. In this ways, the abstract fails to deliver to the reader a concise summary of the obtained results. the main results regarding calibrations and comparisons with radiosounding presented in section 4 should be reported in the abstract by stating the mean daytime and nighttime biases and RMS values.

Introduction, ln 55, 64, 71, 76: the authors mention the "sources of biases". A statistical bias is typically a systematic error, a difference between the measurement and the truth. In this sense it would be more appropriate to refer to the "sources of uncertainty" or "sources of error".

Sect.2.1, Pg 4, ln 90-91: which are the "required altitude and time"?

Sect.2.1, Pg 4, ln 101: "corrected for"

Sect.2.1, Pg 4, ln 106-109: have the authors actually did some simulation to asses the reliability of the 5%-impact of the differential extinction or the estimate by Whiteman 2003 is taken directly?

Sect.2.1, Pg 4, ln 110-111: do the authors mean that the N2 has a constant mixing ratio through troposphere and stratosphere? The statement is not formulated in a clear way.

Sect.2.2, Pg 5, ln. 126-129: the authors set the requirement for successful monitoring, verification and data assimilation into models by listing noise errors and biases. If I interpret correctly what the authors mean by bias, this should not be part of the requirement as they can be efficiently removed by the calibration process.

Sect. 2.3, pg 8, ln 187: "thus"

Sect. 2.3 pg 9, Figure 1b: the caption does not say what the green lines represent. One can imagine that is the return beam from the IF, but it is not clear.

Sect. 3.3,pg 17 ln 398: what material the cage system is made of? Is the cage subject to thermal expansion?

Sect. 3.3-3.4: the authors perform a thorough analysis of the detectors' sensitivity, calibration and responses. PMT sensitivity and gain are also analysed in detail, which allows correcting for inequalities at the PMT output. As it is shown in Fig.5, each channel in the polychromator is output to an independent PMT. How the authors deal with the differential aging of the the N2 and H20 PMTs? Since the ratio of the two signal is used to calculate the mixing ratio, a drift in gain or sensitivity of the PMT of one channel will not necessarily match nor correspond to a possible drift of the other PMT. This is a well-known problem in literature, and different groups apply different solutions. could you comment on that?