Atmospheric
Measurement
Techniques

Discussions

Interactive comment on "An empirical method to correct for temperature dependent variations in the overlap function of CHM15k ceilometers" *by* M. Hervo et al.

F. Wagner (Referee)

frank.wagner@dwd.de

General comments:

The manuscript by Hervo et al. investigates measurement artefacts originating from an imperfectly known overlap function of a ceilometer. The authors developed an automated method for the determination of improved overlap functions based on the assumption of temporal and vertical homogeneity of the backscatter and extinction coefficient. Finally they showed that this improved overlap functions are correlated with temperature changes.

The paper is clearly written clearly I recommend it for acceptance with minor correc-

tions.

Detailed comments:

* Several times (e.g. page 2 line 31; page 3 line6) the authors used the term "homogeneous atmosphere". This term is misleading. The authors should be more precise. Only on page 5 the authors state "The aerosol extinction and backscatter coefficients are constant in a range interval during the time period of observation (assumption of homogeneous atmosphere".

* page3 line 26: "built by the company Lufft Mess- und Regeltechnik GmbH (previously Jenoptik)" For avoiding misunderstandings, the ceilometer is now build by Lufft and was in former times built by Jenoptik. Lufft and Jenoptik are two different companies.

* page 5, equation 3: The authors should re-check the equations. I am missing the normalization with respect to the number of laser shots.

* page 7 line 16: "The overlap function provided by the manufacturer agrees well down to 600 m." If I understood the algorithm correctly, that statement is not a result of the algorithm, but a necessary condition for the quality tests (see appendix) and hence not at all surprising. This should be clearly written in the manuscript.

* section 4.2: although the data show a correlation with the internal temperature of the instrument, the explanation and discussion is not convincing. In case that such a correlation exists then the daily cycle of the internal temperature should be visible in the data as well. However this daily variability was removed in the algorithm, because the final overlap for a day is the median overlap function of all selected individual overlap functions for this particular day. Hence this section should be re-written and discussed why daily temperature variations are discarded by the algorithm but still an annual cycle of the overlap function remains which is correlated to the internal temperature. Maybe a different explanation/correlation can be thought of? Please note that any changes in this section will affect other sections of the manuscript, too.

* page 9, lines 11-12: "Major advantages of the model are the possibilities to correct for short term variations on scales of hours (day/night) and to correct data in real time." I would agree on that in case that daily variations would not have been discarded by the algorithm. See my comments to section 4.2 (above).

* annex: page 17 line 4: "Under the assumption that the overlap function does not change" If I understood the algorithm correction, the authors don't consider the whole overlap function, but just the part between Rok and Rmax. Could you clarify please?

* figure 1: the dashed lines are barely visible in the printed version of the pdf. Please make them thicker.

* Figure 7: the caption is inconsistent. I guess correct would be Before correction (a and b), daily correction (c and d), model correction (e and f)

* Spelling: Caption figure 4: Payerne (starts with capital letter)

* page 19, lines 4-5: Re-check the reference for Stull. Is the publisher really Springer Science & Business Media.? Or it is Kluwer Academic publishers?