Atmos. Meas. Tech. Discuss., 7, C3094–C3097, 2014
www.atmos-meas-tech-discuss.net/7/C3094/2014/

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Evaluation of the MOZAIC Capacitive Hygrometer during the airborne field study CIRRUS-III" *by* P. Neis et al.

Anonymous Referee #2

Received and published: 8 October 2014

General remarks

This paper relates to the study of an instrument that has already been extensively described elsewhere, the MOZAIC Capacitative Hygrometer (MCH). However, this present study exploits the opportunity to perform in-flight comparisons on the same aircraft platform with other well-established research-grade humidity measurements. This is a worthwhile objective since it provides additional confidence in the MCH measurements with their stated methods of calibration and their subsequent use, for example, in the assessment of atmospheric model performance. Having sensors mounted on the same platform overcomes some of the limitations of the earlier study of Helten et al. (1999).

As such, I consider that this paper merits publication, but subject to substantial changes as indicated below.

The authors describe early on (page 3/4) reasons by which the operational conditions of the MCH on the Learjet differ from those in its normal operating environment. This is principally a result of the lower operating Mach number of the aircraft, reduced dynamical heating in the instrument intake and hence operation at lower sensor temperatures where the capacitative sensor is known to not perform well. To maximize the value of this present study, this difference in operating conditions should ideally be quantified, for example by presentation of pdf's of sensor temperature from the data used in this study and from a large sample of regular MOZAIC data. Such a presentation is lacking, although the authors subsequently describe why they filter data with Tsensor<-40C.

The reasons by which the air in the MCH sensor housing is dynamically-heated are made clear. However, there is then a possibility that cloud particles may be partially-evaporated in the intake and so result in enhanced water vapour content at the sensor location. This process is thought to contribute at least partially to the suppression of temperature measurements in water clouds due to the latent cooling induced by droplet evaporation. There are, therefore, good reasons to examine the performance of the MCH separately for cloud-free and in-cloud conditions although these reasons are not acknowledged in the present text. Observations of the variation of humidity within cirrus clouds are of great value to understanding their subsequent microphysical evolution and there is therefore significant value to be had in describing the ability of the MCH to measure in these conditions.

What is not very clear to me at this point is the rationale for selecting in-cloud data and the choice of different reference hygrometers for in-cloud and cloud-free conditions. The FISH instrument has a forward-facing intake that accepts any cloud particles and so clearly cannot be used as a reference in-cloud. However, it is not clear to me why the OJSTER instrument, an open-path TDL, cannot be used as a reference out of cloud. From the data presented in Fig.4, there appears to be very good agreement between

OJSTER and FISH, such that the majority of OJSTER data points are overlain by FISH and not visible. The method of selecting in-cloud data in this study is the same as that in Kramer et al. 2009, relying on thresholds of ratios of RHice from FISH, OJSTER and other instruments. The reason why this method of classification of in-cloud points had to be applied by Kramer et al is clear, since they were working with an inhomogeneous dataset from different aircraft with different instrumentation, and some without any cloud particle detection instruments. Since the present CIRRUS-III study was intended also to sample cirrus microphysical properties, it seems likely that some form of in-situ particle measurement would have been available, although none is mentioned. This would presumably have provided a much more straightforward in-cloud classifier. However, even if this cloud classification method is retained, it ought to be possible to present separate analyses of MCH vs OJSTER for all data (and also separately for in-cloud and cloud-free) and MCH vs FISH for cloud-free data. Can the authors comment on all of this?

Detailed remarks

1. Eq.1 and following. The Helten et al (1998) reference makes clear that the measured temperature inside the instrument housing is actually the recovery temperature, which differs from the Total Air Temperature by a factor that accounts for the incomplete recovery of kinetic energy in the housing and losses to the housing itself. It would be helpful to give some clarification of this point, because although the direct impact on temperature will be relatively small it will, nevertheless, introduce a small bias in RH.

2. line 193 and Fig.5. The increased departures between MCH and Reference would be seen more easily if these data were presented as a scatter plot of (RHref – RHMCH) vs. Tsensor.

3. Fig.5 caption. The last line should refer to Fig.4. Since Fi.4 also has a different time axis, it would be clearer if ambient temperature data were also included in Fig.5.

4. Fig.5. It would be helpful to indicate which parts of the time-series correspond to

in-cloud data – presumably the section with RHliquid $\sim$60%.

5. section 4.2.1 paragraphs 1/2. It would be of interest to know what fraction of the observations was retained after these filtering operations and perhaps also the extent to which this is dependent on the choice of sensor temperature threshold, since the latter is fairly arbitrary.

6. line 235. This is confusing to me. It states that the increasing difference between MCH and Reference RH shown in Fig.8 above 60% is because the reference sensor is measuring total water. Surely though, the reference sensor used in cloud is the OJSTER which only measures vapour?

7. line 249. This is again confusing. I think what the authors intended to say was more like this: At or near cloud edges, conditions may be such that the data are not classified as in-cloud using the algorithm of Kraemer et al (2009). Since reference humidity may then still be taken from the FISH instrument measuring total water they may, therefore be biased high.

8. line 263 and Fig.11. Table 2 indicates that only 4 minutes of data from this case are in-cloud whereas the figure suggests $\sim$18 min. Can the authors clarify this discrepancy?

9. line 274. These data would presumably be excluded from the analysis in section 4.2.1 due to the ascent/descent rate criterion that was applied. Is this the case?

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 9803, 2014.