Review of the manuscript amt-2012-250

Title: “Modified Ultrafast Thermometer UFT-M and temperature measurements during Physics of Stratocumulus Top (POST)”

by W. Kumala et al.

General Comments

The present manuscript describes the improvement of an existing fast-response temperature sensor for airborne applications, which is not a novel topic.

At that, a more comprehensive introduction could help outlining what the research is important for, and what it is trying to achieve. For example, the applicability of findings from such sensor for improving cloud parameterizations in weather prediction etc.

The study itself is largely qualitative, and lacks rigorous quantitative assessment and proof. On p. 2096 l. 16, the phrase *differ only by a marginal degree* is symbolic for the mainly descriptive approach. For example, what are the propagated input uncertainty, but also accuracy, precision and resolution of the UFT-M? The use of statistical techniques (regression, variance statistics, uncertainty propagation…) would much aid the scientific significance of the paper. Also, why has only a subset of the available data been used for analysis? The exploitation of all available data could further help to improve the significance of the results.

The data processing techniques are not up to date, so the authors have to deal with residual spikes, lags among calibration variables etc.

Lastly, the manuscript should be crafted more thoroughly:

- Inconsistent formatting of in-text citations: p. 2086 l. 24, p. 2087 ll. 7, 11, 24, p. 2090 ll. 6, 7, p. 2094 l. 20.
- Frequent omission of articles, in particular *the*: p. 2088 ll. 21, 27, p. 2094 l. 28 and *a*: p. 2089 l. 9, p. 2090 l. 1.
- Hypotactic sentence structures, e.g. p. 2090 l. 24 f., p. 2096 l. 3 ff.
- Inconsistent bibliography, e.g. first-letter capitalization (p. 2099 l. 19 f., p. 2099 l. 19 f. p. 2099 ll. 24 ff., p. 2101 l. 1 ff., p. 2101 l. 14 f., p. 2102 l. 3 f., p. 2103 l. 5 f.).
- Incomplete bibliography (p. 2101 ll. 18 ff.).
- Figures are not prepared consistently.

Consequently I recommend major revisions for this manuscript.

Specific Comments

p. 2085 l. 1: Inconsistent naming convention: modified UFT-M in the title, modernized UFT-M in the text.

p. 2086 l. 6: What kind of interaction with the avionic system?

p. 2089 l. 11 ff.: The structure of the paper is not very intuitive. I suggest considering the following revision:

Currently		Suggested
2 Description of the UFT-M thermometer	→	2 Materials and methods
⋮		
3 Data processing and evaluation	→	2.4 Data processing and evaluation
3.1 Error correction…	→	2.5 Error correction
		3. Results
3.2 Performance of the UFT-M	→	3.1 Performance of the UFT-M
4 Example records	→	3.2 Example records
5 Conclusions	→	4 Conclusions

p. 2089 l. 19 ff.: Can this behaviour be described, and maybe be corrected, using the Bernoulli equation?

p. 2090 l. 19 f.: Why would you place the sensing wires in stainless steel tubes? This is not apparent from Fig. 1.

p. 2090 l. 25: What is a unit mVpp?

p. 2091 l. 4: If this temperature drift is constant, can it be characterized and corrected?

p. 2091 l. 9: *special* → particularly?

p. 2091 l. 15 ff.: As the thermometer is mounted at a fixed tilt, it cannot be parallel to the airstream at all true airspeed settings/ attack angles encountered during flight. Is the effect of such deviations negligible?

p. 2091 l. 18: How can the flow in the boundary layer around an aircraft be undisturbed?

p. 2092 l. 12: *middle* → centre?

p. 2093 l. 4 ff.: The proposed spike detection algorithm is unable to capture spike events that affect successive (and not just single) observations. Also, in Sect. 3.2 “residual spikes” are mentioned, and in Fig. 6 (bottom panel) it appears that there is a recurring spike at 0.2 K every 0.005 s. Is there any reason why established spike detection algorithms based on sliding windows (e.g., Hojstrup, 1993; Vickers and Mahrt, 1997) are not used? Moreover, the use of distribution statistics (e.g., median and median absolute deviation) would aid the robustness of such method (e.g., Mauder et al., 2013; Metzger et al., 2012; Papale et al., 2006), and replacement through linear interpolation helps minimizing the effect on power spectra.

p. 2093 l. 21 ff.: The sensitivity of the sensor is likely to change with aging of the wire. Has a periodical re-calibration/validation been performed?

p. 2093 l. 27 ff.: In particular for calibration purposes any lags should be corrected. This can be easily achieved through maximizing the lagged correlation between two measurements and subsequent shifting (e.g., Rebmann et al., 2012).

p. 2094 l. 8 ff.: *Records from all three sensors are representative of the same sampling volume*. For this statement to hold it would require mentioning the exact 3-D displacements among sensors. Even for displacements below 1 m at 25 m s^{-1} true airspeed, sensors are not representative of the same air volume in that conversions via the ideal gas law (e.g. calculation of dry mole fraction) do not hold true.

p. 2094 l. 28 ff.: Can the "signature" of the Rosemount housing wetting be quantified?

p. 2096 l. 1 ff.: See comment to p. 2093 l. 4 ff. Simply averaging over spikes is not a sound procedure, especially as capable spike detection algorithms are readily available (also see Goring and Nikora, 2002).

p. 2096 l. 3 ff., p. 2098 l. 22 f.: What is the calculation basis for these uncertainty estimates? Absolute error, accuracy, precision, median absolute deviation…?

p. 2096 l. 23, p. 2098 l. 5: *TO10*: This descriptor is meaningless without an overview (table of similar) of all available research flights.

p. 2097 l. 20: *The time scale corresponds to this scale in the upper panel*. I do not understand. Maybe a graphical depiction would help?

p. 2097 l. 25: Temporal resolution?

p. 2097 l. 27 ff.: It would be good to provide a table of 3-D sensor separations, which would also serve other use cases of the published/online available data

p. 2098 l. 1: What are *larger sizes*?

p. 2098 l. 5: S s^{-1}, p. 2098 l. 26: samples per second… – why not simply using Hertz (Hz)? Also, the units S s^{-1} and kS s^{-1} have never been formally introduced in the text.

p. 2097 l. 10: The power law decay with −5/3 slope applies to (i) homogeneous and isotropic turbulence, and (ii) eddy sizes much smaller than the integral scale but much larger than the Kolmogorov viscous dissipation scales. Can (i) be justified for cloud crossings, and (ii) for frequencies up to 500 Hz, as shown in Fig. 5? oes the spectral flattening above 100 indicate that the dissipation scales are reached?

Figs. 4, 8, 10: Information is provided in the figure legend, so single figure panels do not require an additional title.

Fig. 7: Figure legend is missing.

Fig. 9:

- What are *Sc*, *EIL*? Abbreviations should be introduced.
- Top panel: …in the course of descending…;
- Center and bottom panels: the colors are different from the legend in the top panel. What is the meaning of the different colors?
- Bottom panel: The averaged values appear time lagged. Has a leading window been used during the averaging process, or is this also a result from not lag-correcting the data?

References

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