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Atmospheric Measurement Techniques Discussions

Interactive comment on "Fiber optic distributed temperature sensing for the determination of air temperature" *by* S. A. P. de Jong et al.

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This paper has been waiting for quite some time for the second review. It is almost the oldest paper in AMTD so I hope the associate editor will find a second review(er) soon. Perhaps some reader will provide a second review. . .

In the meantime, there is an opportunity to have a bit of a general discussion. Let me first thank Dr. Drüe for his careful review and pointing out many ways to make the article better. He clearly went in-depth, checking the environment and distances. Special thanks for bringing us back to The Netherlands from between Moscow and Saratov! We will address all these improvements in the final draft, preferably in one go once the comments of the second reviewer are known as well.

If I may crudely summarize the main comments, it says 'nice experiment, but you should have done it differently'. In this case, I would even agree, having the hindsight that the experiment has given us. But in our case, as is often the case, we can not repeat the experiment without great expense and practical problems (the cables have been consumed by other experiments and the site now has a concept house on it (http://goo.gl/AVOxsp)). Because the main findings are already on-line in AMT-Discussions, it would not even be possible to then publish the improved experiment. The overall result would be that this proof-of-concept of an interesting new measurement technique would simply not be published in a peer reviewed journal. So, as a general discussion point, I would like to ask to what extent one can ask researchers to do another experiment with different goals? Environmental experiment papers are fundamentally different from model studies and even lab experiments, as one can normally not simply do these additional experiments within the cycle of a paper review. Every experiment can be improved once the results are known. Would it not be better in such cases to ask for a clear description of the limitations of the experiment in its original set up?

Also some brief replies to the main comments given:

"a) The temperature reference measurement is crucial for the whole study but there is a number of open questions on the quality of these measurements. Foremost there is nothing stated on the radiation shielding, ventilation, and position (relative to the fibers), height above ground, calibration of the sensor, sampling frequency (only on the averaging interval of 5min), variance of the values averaged in each interval etc. "

True, and this touches the essence of the main discussion point earlier. We will provide more detail on the reference temperature but the reference temperature was not a high grade temperature measurement. There was shielding but no (forced) ventilation, variation was not reported, etc. This is because when we set out to do the experiment, we wanted to know if the cables would come close to a standard low cost agro-met station as we use a lot during hydrological field studies. We did not know if we would

get within one K and were hoping we would. In the end, the method proved to be much better than anticipated and the reference temperature measurement became the limiting factor. With the given results, one can not even say that the reference temperature is closer to the true air temperature than the cable measurements. So the original goal was to find out if we could get to, say, within one K. If we would repeat the experiment, which is not practical, we would have better reference temperature measurements but the goal would then be different, namely if we can get to within 0.1 K. Clearly, we did not sufficiently explain the modest original experimental goals.

“b) There was no wind speed measurement at the site. The wind data used were taken 6km away. If I look at satellite images covering both positions, it looks like the measurements were on some sort of campus with buildings, whereas the wind data are from a nearby airport. I do not remember reading of any correction applied to account for a the different height of the wind measurements compared to height of the fibers nor for the different roughness lengths of both sites. Furthermore, the averaging of five minutes appear too short under these circumstances (at 5 m/s wind, 5min=3000s correspond to a spatial range of 15km, which is only above the largest size of turbulent eddies that could exist between the two positions), which gives room for unnecessary large random error of the wind speed and in consequence the radiation error.”

Also true and, again, we would have had a wind measurement device on site if we had known that the results would be so much better than expected. In the case of windspeed, it is not really critical. The wind measurement simply serves to check if the Archimedes number«1. In our case it does not come above 0.001 so even large deviations in wind speeds of 50% would not have influenced the conclusions. They served mainly as check to see if there were extended wind-still periods.

“c) Although the DTS temperature values were averaged over a number of a number of individual measurements, it is not attempted to estimate the random error of an individual measurement, which would be probably the most valuable outcome.”

This we can, and will, fix.

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