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Atmospheric Measurement Techniques Discussions

Interactive comment on "Statistical modelling of collocation uncertainty in atmospheric thermodynamic profiles" *by* A. Fassò et al.

Anonymous Referee #2

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This paper addresses the determination of uncertainties when measuring an atmospheric parameter, which is a topic of great interest for the community, mainly in the frame of NWP assimilation and inter-comparison of atmospheric measurements from different instruments.

Unfortunately, the paper is very cryptic and difficult to understand. It is not clear what the final objective of the paper is. It is not clear what the mathematical development is and what the underlying assumptions behind the maths are. The example given only creates more confusion.

I would advise the authors to modify the paper in such a way that is potentially easily understandable by a regular atmospheric scientist. They could include a simple

example at the beginning, which could accompany the explaining of the theory.

In particular:

1) It is not clear what the main objective of the paper is in the abstract and the introduction. Is it to determine the errors in a given atmospheric parameter profile? Or is it to determine the co-location error between two atmospheric measurements? Or all of them at the same time?

2) Please define adequately what are the definitions of the parameters being used in the paper. It is clear what an atmospheric observation is. But, what is an "environmental forcing factor"? The only field where I have heard this word is in climate science. Is it meant as other parameters which are measured at the same time as the main observed parameter under study? Or is it something else. Maybe a change in the name and a proper definition would help.

3) The mathematical notation in section 2 is very strange. I have never seen a function written as h->(s,t). Using a central dot instead of a variable also adds to the confusion.

4) In the equation before Eq. 1, what is a "true smooth profile"? Why does the true profile need to be smooth?

5) In Eq. 1, Why is the true smooth profile a linear function of the forcing factors?

6) In Eq. 2. Why is the variance a linear function of the forcing factors?

7) In Eq. 5, a variance is defined as U, which is confusing, because before the parameter was called u and the variance was denoted by sigma.

8) Eq. 6. The variance is decomposed in three terms. Could you give a physical justification why this is so, besides citing a reference to the appendix? Could you explain more clearly what the different terms are? For example, is drift uncertainty the same as or related to co-location error? What is the environmental error? Does atmospheric turbulence, to cite one example, have anything to do with these errors?

9) Eq. 7. Why is this equation like this? What is the "variance covariance matrix of x(.)"?

10) In the equation U_omega = E(sigmaˆ2(.|x)), what is meant by the expected value of a variance?

11) S(h) is introduced. Is this used later anywhere in the paper?

12) Section 4. Delta mu, which should be the difference of the smooth profile is now a co-location drift, why?

13) Section 5, example. Eq. 10. After all the mathematics developed before, the difference in relative humidity between two atmospheric measurements is a linear combination of the absolute profile and the differences of other measured parameters. Was all the mathematics developed before necessary to arrive to this solution?

14) Eq. 10. The differences in relative humidity between two instruments are a linear combination of the differences of the mixing ratios between these two instruments. Relative humidity _is_ directly calculated from mixing ratios, so a statistical relationship with a high correlation is trivially expected.

15) Eq. 11 is not clear what is meant with this. And again, what is the final result of the example and what is the final objective of the paper?

16) In the conclusions the main objective of the paper should be explained and whether these have been achieved in the example.

I hope these comments are useful for the authors to improve the paper. I would strongly advise and encourage them to modify the paper aiming at an atmospheric scientist as reader. I think the technique has great potential and, if properly explained, could hold great potential for future applications in atmospheric sciences.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 7505, 2013.