Review of Sensitivity of Large-Aperture Scintillometer Measurements of Area-Average Heat Fluxes to Uncertainties in Topographic Heights

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MS No.: amt-2013-300
MS Type: Research Article

Introduction

The article introduces a method of analysis for uncertainty in the scintillometer derived heat fluxes to uncertainties in spatially distributed topographic measurements. As such, the authors explore the new and very relevant issue of scintillometer sensitivity to each single height measurement of the scintillometer path. So far, research only has explored the effect of the resulting average scintillometer height. The authors present a comprehensive derivation of the sensitivity functions, which in the second part of the manuscript are applied to an experimental set-up. Especially, the derivation of the sensitivity functions is well documented and structure, although its readability could be improved somewhat. The only weakness is that the authors assume the friction velocity to be known, which significantly simplifies matters and which in practice is usually not the case. Nevertheless, the authors are aware of this and discuss the limitations of their approach. Finally, the authors present some practical results, which greatly clarify the usability of these sensitivity functions. Therefore, I suggest publication after taking into account the comments below.

Specific comments

Eqs. (2), (3), (5), and (7): I suggest rewriting Eqs. (2) and (3) with the parts at the right-hand side of the arrow in Eqs. (5) and (7), i.e. as $(\pm)T_* =$…. . This fits better with what you write at line 6 of page 35, and as such you can leave out Eqs. (5) and (7), which basically bring no new information. Furthermore, I want to remark that to an unexperienced reader it may not be obvious that the "2" in C_T^2 and T_*^2 have a fully different meaning. In C_T^2 (or C_n^2) it indicates that there should be two Ts, i.e. it actually is C_{TT} or C_{nn}, whereas in T_*^2 it is a squared sign.

P 35 line 14 – 22: does your definition of z include the displacement distance that accounts for vegetation? See e.g. Eqs. (3) – (5) of Geli et al. (2012), where all equations read z_{eff}-d instead of z_{eff}. I would not be surprised when uncertainties in this displacement distance are more significant than uncertainties in z. Discussions about this are lacking in the manuscript so far; nevertheless I think it is a very relevant issue for this manuscript, because the displacement distance can greatly vary both spatially and temporally.

P 36 line 11-16: "Heterogeneous terrain implies (…) Appendix A of Hartogensis et al (2003)." – I have two questions about this part. First, what is the relevance for your study? Second, the blending height concept is not uncontradictable. It is under discussion since large-eddy simulations show that it does neither really exist under convective conditions, nor when the heterogeneity scale is larger than the boundary layer height. See e.g. Maronga and Raasch (2013, BLM 146, 17-44).

P 36 line 16-18: "Hartogensis et al. (2003) (…) independent u_* measurements." – It is unclear to me what you want to say with this statement. Do you proceed with his idea?

P 39 line 2-3: "The source measurement variables being considered (…)" – I miss the humidity and the Bowen ratio. LAS measurements are not only affected by temperature fluctuations, but also by humidity fluctuations. Hence, you cannot simply say C_T^2 =

$(T/A_T)^2 * C_n^2$, see Moene (2003). In case you neglect this humidity dependence on the C_n^2 estimates you should mention this explicitly somewhere.

P 40 line 1: "To illustrate this we re-write for example Eq. (6) as" – I cannot fully follow the logic here. You are talking about continuous variables and you randomly ("for example") start discretising one of these variables. If you would introduce Eqs. (14) and (15) immediately and leave out Eqs (12) and (13) I could understand it (although I would directly give the derivation of the derivative of Hs – line 7). However, I suspect that you need this discretisation to introduce your Dirac-Leibnitz derivative? If yes, then mention it in the text.

P 42 line 19 – P 43 line 5: I am approaching the following question from the perspective of solving fluxes, i.e. not from the perspective of sensitivity analysis. The fact that you assume the friction velocity to be an independent measurement makes it logic that there is a solution – this is scientifically and mathematically nothing new. Rightly you state that this solution is "quite an unwieldy equation" and that fixed-point recursion is a practical alternative. I am interested to learn, however, whether in case of unknown u* this equation still has an explicit solution? Zero is one solution, but must there be another solution that is different from zero and can this solution also be complex?

P 43 – P 46: I get lost here; I miss some guidance through these equations. What I think to get is that you want to solve Eq. (26), which is similar to Eq. (21). Subsequently, you need to solve the derivatives in Eq. (22), Eq. (27), and Eq. (31) implicitly, whereas the other derivatives can be solved explicitly? It would greatly help if you explain that in the text. Part of the confusion lies therein that I don't get the "for example" above Eq. (21); if you want to solve Eq. (15) you need this one. Furthermore, I don't know what you mean with "We will need some derivatives" below Eq. (21) if you only give one derivative. Using a parallel writing style could enhance readability here.

Abstract lines 17-18, Conclusions line 9-10, and P 51 line 20 – P 52 line 2: Again, I approach this remark from the point of view of solving fluxes, not from the perspective of the error analysis. You state: "We have developed techniques to eliminate this error" – I doubt this statement for two reasons. First, because you use an independent measurement of u_*, you force the equation to have a solution. This is not guaranteed for the case when your u_* measurements are dependent on wind-speed measurements I think. Especially for the stable situation challenges are great.
Second, you already introduced the recursion methodology in another paper (Gruber and Fochesatto, 2013), so technically it is not new.
Third, I do not see the difference between fixed-point recursion and iteration. Fig 5 is a school book example of two equations for which iteration will converge to a solution. Me and my co-authors stated it like this before in responding on your other article: "Fixed-point recursion means that a function calls itself repeatedly, until the outcome of the function equals the function itself, i.e. $z/L_O = f(z/L_O)$. In a programming language such as Matlab this repeated calling of a function requires much extra memory allocation, because of program overhead. We found that it is faster to iterate until the $z/L_O = f(z/L_O)$ requirement is met; the results are identical in every aspect." (Van Kesteren et al., 2013, http://link.springer.com/article/10.1007/s10546-013-9891-1). In other words, we found that iterating Eqs. (20) and (25) will give identical results as to using recursion – even in the fact that both methods do not always converge to a solution. That is basically what is done in the scintillometer community for years already, although we never wrote the equations so neatly down as you did in your papers. To summarize: you introduce recursion as a new technique, but I strongly doubt whether it is truly better than iteration in practice.

P 52 lines 12-13: “wind speed replaces u_* as a source measurement” – are there not two other source measurements in that case as well: the height of the u measurement and the roughness length?

P 53 lines 11-13: “Perhaps more important than (…) analysis technique in other disciplines.” –I have the idea it is an important result, but it now gets hardly any attention in the manuscript, and in the discussion it is neglected. I would encourage you to say more about it in the discussion section, e.g. give some examples of possible applications.

Technical comments

P 34 – line 22: “(…) scintillometers measure the index of refraction (…)” - not really, scintillometers measure the light intensity of the scintillometer beam; reword.

P 35 line 10: “(…) as seen in Wyngaard et al. (1971) (…)“ – Wyngaard does not show these values, because he uses a von-Karman constant of 0.35. Andreas (1989) adapts the values of Wyngaard to correspond with a von-Karman constant of 0.4; these latter values you show. I suggest to reword this sentence and add on P 38, line 2 the value of the constant you use in brackets after you defined it, i.e. “ (…) von Kármán constant (0.4).”

P 36 – line 9: “While the topography is not flat (…)” – you did not introduce any study area yet, so it is unclear what you mean with “the topography”.

P 37 – Eq. (4): you only define T – the temperature on P 38 line 19, I think you better define it on line 20-21 of P 37 with the other previously undefined variables of Eqs. (4) – (6).

P 39 – line 12: “(…) resulting from relative error on (…)” – I expected “resulting from a relative error”.

Eqs. (25), (32), and (34) have become unclear the way they are edited by the journal now.

Eqs. (31) and (32): I think the reference “Eq. (32)” should be removed. I think that what is now referenced as Eq. (32) is still part of Eq. (31), just as in Eq. (14)?

P 48 line 1: can you somehow make clear that this is a results section? That prevents the reader to look for discussion in this section.

P 48 line 3: I suggest including geographic coordinates in brackets after “Imnavait Creek Basin field site”.

Fig 3a Some comments:

- Why does the path have a different angle as the one in Fig 3b? Maybe you can plot the legend left outside the figure; the figure would become smaller and the legend can be turned by 90 degrees, so that the text even could get a little bigger.
- I would suggest to leave out the geographic coordinates (give them in text, see previous comment), set the x-axis and y-axis left lower corner to zero, and have that as your reference point. That makes it much easier to see that the path is about 1 km long and about 100 m wide; This is the relevant information here, not how far it is away from the equator and Greenwich.

- Give the height units not in meters above sea level. The sea level is not the interesting information. Set 927 m (the lowest point in your map) to zero, so that we can readily see that the maximum height variations in your map are about 12 m.

Fig 3b I find the small bar plot very difficult to interpret.

- It is very small to read
- What is on the x-axis? The first reading reads: [-0.353;-0.164), which has a different opening and the closing bracket and which is slightly incomprehensible to me. The label of the axis: elevation difference survey – DEM, does not bring more clarification to me either.

Fig 4 Maybe you could put the units on the left and the right y-axis instead of putting them in the legend?

Fig 5 – I think you do not need this figure. It gives only one example of something that is trivial; the equation has one solution, so it will always converge to the correct solution.

Fig 6 – I think it would be interesting to plot the stable line, Eq. (20) in this Figure as well. Or is it not bijective?

Fig 9 – Put "beam height above topography z(u) (m)" on the y-axis label instead of in the legend. Nevertheless, I think you don't need this figure either. The paths can be well defined in the text (section 4.2) with only a little more descriptive text.

P 50 lines 3-5: "firstly scintillometers are more (…) strengthens with greater instability" – this you mention already in the introduction, see P 36 line 28 – P 27 line 5. I would shorten it.

P 50 line 9: what do you mean with "concentrations"? It is unclear to me. The largest values in Fig 7 are found around u = 0.55 and u = 0.6

P 50 lines 18-19: "the scintillometer is still (…) along the whole path." – no it is not, the scintillometer is hardly sensitive to CT2 at the edges of its path.

P 50 lines 15-21: "$S_{HS,z}$(u) is not ()","$S_{HS,z}$(u) should not be interpreted (…)" . You put a lot of emphasis on what $S_{HS,z}$(u) is not. Maybe you first explain what it is and then warn what you should not do with it. Furthermore, I have the question what the numbers imply. Is a value of 4 large or can it, for other variables, also be much larger?