Comments on the manuscript

# Use of commercial microwave links as scintillometers: potential and limitations towards evaporation estimation

Submitted by

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**General Assessment**

The paper deals with the potential use of commercial microwave links (CML) as scintillometers to enable area-representative evaporation estimates from this opportunistic data source, such estimates would be of great value for regional water budget studies in hydrology or agricultural management. The authors present a very well-designed experiment to study and assess the possibilities and limitations of this approach. They demonstrate, for one type of CML device (Nokia Flexihopper), that reasonable values of the refractive index structure parameter $C_{nn}$, which is the basic atmospheric variable derived from scintillometers to estimate evaporation therefrom, can be obtained provided careful corrections are applied which are based in the present study on a comparison with a microwave scintillometer. The authors critically discuss the perspectives for a wider application of the proposed method which appear to be limited due to several reasons (need for a microwave scintillometer as a reference to derive the necessary corrections, limited amplitude and time resolution of CML signal intensity data, missing availability of intensity variances, sensitivity of CML antennas to vibrations causing unwanted variance contributions, …). Moreover, the derivation of evaporation estimates would additionally require $C_{nn}$ estimates for optical (near-infrared) radiation.

The paper is well written and well structured. The approach chosen follows a sound methodology on a solid theoretical base, and the results obtained are comprehensively presented and discussed in the manuscript. I therefore unanimously recommend the paper for publication.

However, I have a small number of specific comments and some minor remarks the authors might wish to address before the manuscript will finally be published. These are specified below.

**Specific comments**

1. A few more methodical details / comments could be given, e.g.
    - In line 114-116 the high-pass filter to remove absorption-based fluctuations is briefly introduced, a short comment on its value / choice might be helpful.
    - Line 120: Here the $A_T$ and $A_q$ factors are introduced. It should be mentioned that these factors also depend on the wavelength of the electromagnetic radiation, possibly discussing the relevance of this dependence for the present study.

- Line 174: Here, the authors refer to "… other uncertainties in our study …". An uncertainty in scintillometry they do not mention at all is the choice of the similarity function to derive the heat fluxes from the structure parameters (could be done in Appendix A).
- To be honest, I did not fully understand where the bin clustering vanishes between Figures 7 and 8, could this be clarified?
- Line 323: The variance calculation is said to start at f = 0.01 Hz, isn't this frequency not cut by the high-pass filter? (this relates to the first bullet above)

2. In several places, formulations are a bit sloppy and imprecise:
   - Line 1: I would not say that scintillometers are used to measure evaporation, I think "derive", "determine" or "estimate" would be more appropriate.
   - Line 161: What do you mean with "a typical time interval for turbulent heat fluxes" – The heat fluxes per se do not have a time interval, probably better "… for the determination of …"
   - Line 202: What is an "absolute wind speed" – is there also a "relative wind speed"?
   - Line 220: Strictly speaking, the turbulent heat fluxes are also not constant with height (the warming of air to a considerable extent comes from the divergence of the sensible heat flux), however, heat flux varies much less with height than the structure parameters such that heat flux is usually assumed as constant in the near-surface layer (also considering the limits of the accuracy with which it can be determined).

3. Metrics: Strictly speaking, and taking into account the assumptions behind, the scintillometer can not be seen as an absolute truth for determining the structure parameters, although it is used as a reference here. I would therefore prefer to replace the "bias error" by "bias difference" or "bias deviation". I also do not fully realize (see also eq. (10)) why this should be a "relative bias"?

4. Figure 13: I would limit this Figure to panels (a) and (b), it has been shown earlier (Figure 3) that the structure parameters from EC and MWS agree quite well. It does therefore not add much to show both here, and the MWS is assumed as the natural reference for the CML because it measures over the same path.

**Minor Issues**

- The abstract could be shortened a bit, in particular in the first part where the scintillometer principle is described over several sentences which I would see more appropriate to do in the main text.
- Line 26: Given all the limitations discussed later (Section 6) I would be a bit reserved to speak about an "unprecedented potential".
- Line 30: What is a "ground-truth for model simulations"? Normally the term "ground truth" is related to satellite data.
- Line 34: What to you mean with "spatial estimates … with a high … spatial resolution"?

- Line 42: The argument with “strong theoretical assumptions” could also be claimed for scintillometer measurements, there is quite a number of assumptions behind the derivation of heat and evaporative fluxes from the scintillations.
- Line 44 (and also lines 88, 118, 271): The handbook edited by Foken (2021) is a collection of articles contributed by several author teams. Citation should therefore be “Beyrich et al. (2021)” with the reference: “Beyrich, F., H.A.R. de Bruin, O.K. Hartogensis, H.C. Ward, 2021: Scintillometry. In: Foken T (ed.), Handbook of Atmospheric Measurements. Springer Nature, Switzerland, 969-997. https://doi.org/10.1007/978-3-030-52171-4_34”
- Line 59: What do you mean with “both fluxes that are part of the water balance”?
- Line 140: The “grassland fields” at the Cabauw site are stripes regularly separated by open water ditches which might be of relevance for evaporation.
- Figure 1b: The three microwave antennas could be marked by arrows.
- Line 150 (and also line 209): Try to avoid separation of numbers and units trough CRLF.
- Line 317: This reference to Figure 4b is a bit unclear, looking at Figure 4b I would say that at least for f > 0.4 Hz the CML and the MWS do not show a similar behavior anymore.
- Table 1, headings of the 2nd and 3rd columns: Shouldn’t it be “log ($f_0$[Hz])” instead of $f_0$ [log(Hz)], same for $f_1$?
- Line 363: Isn’t it the variances instead of the structure parameters that might get negative?
- Section 7 should probably be better named “Summary and conclusions”, because the first three paragraphs are just a summary of the results and discussions presented before, real conclusions is just the fourth paragraph.
- Line 518: specific heat capacity at constant pressure

**Technical Issues / Misprints:**

- Line 21: delete “is”
- Line 62: Should be “millions” instead of “million”?
- Line 80: “In” after the “:”
- Line 82: “with the CMLs” might be deleted here, because a few words later the CMLs are mentioned again.
- Line 99: better “based on the Kolmogorov law” (to avoid the doubled use of “follow”)
- Line 358: insert “that” after “show”.

**Recommendation: Minor Revision**