Dear Referee,

Thank you for your time to review our manuscript and for all your constructive suggestions considering our study. It helped to improve the quality of the manuscript. We reply to your comments below. Our response to the comments appears in bold and revised text as *italic*.

Specific comments:

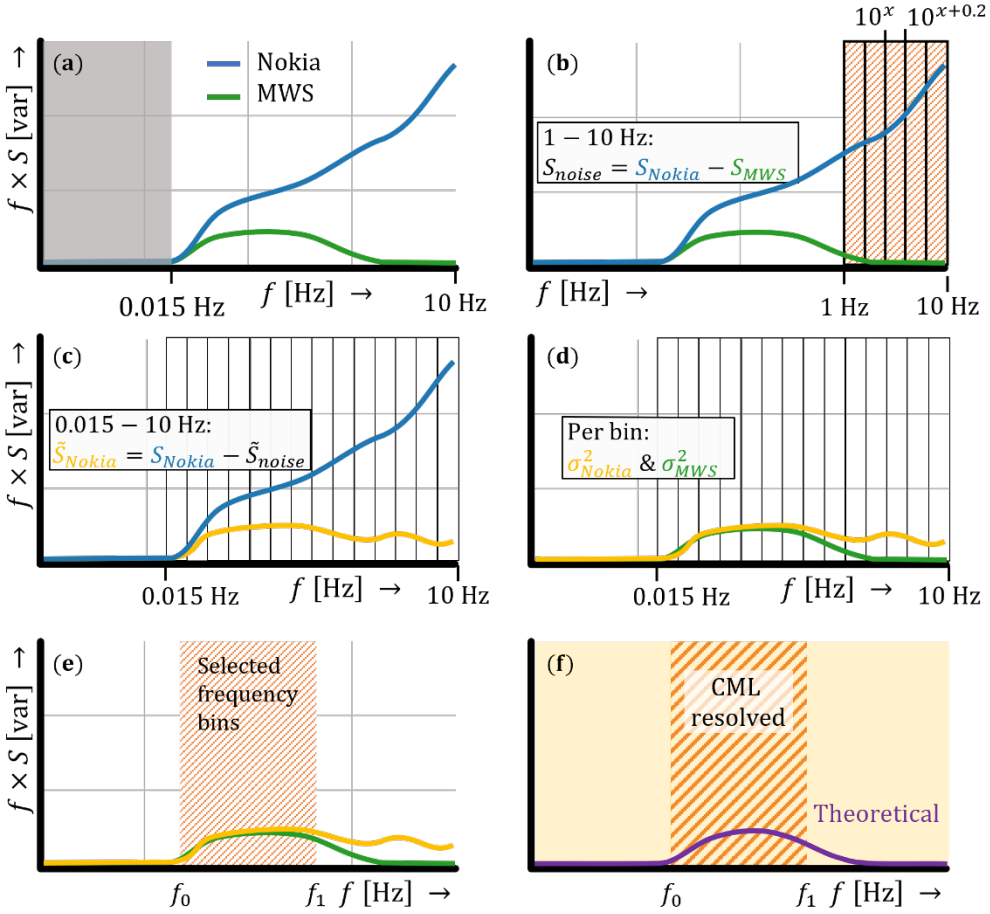
1. A few more methodical details / comments could be given, e.g.
    - In line 114-116 the high-pass filter to remove absorption-based fluctuations is briefly introduced, a short comment on its value / choice might be helpful.
      Based on your comments and the comments of the other reviewer, we have decided to revise our methods. This also includes a change in HPF. Now, we select 0.015 Hz for the 38 GHz CML, since this corresponds to retaining 95% of the variance under a crosswind of 0.5 m s^{-1} for our setup. For higher crosswinds, the spectrum shifts to higher scintillation frequencies, so that an even higher fraction of the variance is retained. We elaborated on this in Sect. 5, where we introduce the revised methods:
      Absorption filter: For each time interval, we apply a high-pass filter at 0.015 Hz, by subtracting the moving average with a window size of 1/0.015 = 66.7 s from the signal intensity time series. We have selected this high-pass filter value, as it retains 95% of the variance due to scintillation for the CML at crosswind speeds of 0.5 m s^{-1} for our setup. For higher crosswind speeds, the spectrum shifts towards higher frequencies, so that an even larger fraction of the variance is retained.

    - Line 120: Here the AT and Aq 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.
      We agree that typically these factors are dependent on wavelengths. However, for the used frequencies of 38 GHz and 160 GHz, these factors are equal according to Ward et al. (2013), see Sect. 3.2. Other studies, such as Andreas (1989), suggest a minor difference in factor (and thus C_{nn}); however, this difference is relatively minor in comparison to the uncertainty in our study. We rephrased as follows:
      A_T and A_q depend on temperature, humidity and pressure as well as the wavelength of the transmitted radiation (e.g., see Ward et al., 2013).

    - 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).
      We agree that these are also important to mention, even though this is especially important after having obtained C_{nn} estimates. We adapted as follows after Eq. (A4):
      ….in which the a_T and b_T are on average 5.6 (uncertainty range based on the 10_{th} and 90_{th} quantiles: $5.1 < a_T < 6.3$) and 6.5 (uncertainty range: $5.5 < b_T < 7.6$), respectively. For a_q and b_q the average values are 4.5 (uncertainty range: $4.3 < a_q < 4.7$) and 7.3 (uncertainty range: $7.0 < b_q < 7.7$). L_{Ob} is defined as….

    - To be honest, I did not fully understand where the bin clustering vanishes between Figures 7 and 8, could this be clarified?

We agree that this was unclear in the previous version and could be wrongly interpreted. It would have been more clear to show the bins also in the original Fig. 8. We revised our methods based on the suggestions of the other reviewer and also revised these figures by assembling them into one larger figure. The figure now looks as follows:



- 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)
  See our reply to your first comment.

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.
     We agree with the reviewer, we changed accordingly:
     Scintillometers are used to estimate path-integrated evaporation and sensible heat fluxes

   - 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 …"
     We agree that this phrasing was unclear. We followed the suggestion of the reviewer:

We perform our analysis based on 30-minute time intervals, a typical time interval for the determination of turbulent heat fluxes (e.g., Green et al., 2001; Meijninger et al., 2002), until 18 October 2023.

- Line 202: What is an "absolute wind speed" – is there also a "relative wind speed"?
  We referred here to the horizontal wind speed independent of the direction. We agree that this term suggests the existence of relative wind speeds. We changed as follows:
  …and intervals with horizontal wind speeds above 8 m s^{-1} independent of the wind direction…

- 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).
  We agree with the reviewer that this statement was unfortunately phrased. We removed this statement.

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"?
   We understand the reasoning of the reviewer. However, we do not consider the MWS to be the absolute truth, but rather as a trustworthy reference (which is also why we make use of two reference instruments). We define the RMBE in comparison to our reference instruments, so that we think that the term "error" is appropriate here. To emphasise this definition, we added as follows:
   We define the RMBE in comparison to our reference instruments and calculate it as…

   Moreover, we use the term "relative", as the RMBE is also equal to log(y/x), given that log(a) – log(b) = log(a/b).

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.
   We understand the reasoning of the reviewer behind this, but we think it would be good to still show panel (c) and (d) for completeness. Therefore, we left this as it was.

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.
  We agree with the reviewer that the abstract could be shortened. We removed the introduction on the scintillometers, so that the start of the abstract has become:

Scintillometers are used to estimate path-integrated evaporation and sensible heat fluxes. Commercial Microwave Links (CMLs), such as used in cellular telecommunication networks, are similar line-of-sight instruments that also measure signal intensity of microwave signals, just like microwave scintillometers. However, CMLs are not designed to capture scintillation fluctuations….

Moreover, in the introduction we added as follows:
Other dedicated evaporation measurements can be performed with scintillometers, which make use of the scattering by turbulent eddies of electromagnetic radiation propagating through the atmosphere (e.g., Beyrich et al., 2021). They consist of a transmitter and a receiver separated along a line of sight of several hundreds of meters to a few kilometers. As a consequence of the different temperatures and humidities of turbulent eddies,…

- Line 26: Given all the limitations discussed later (Section 6) I would be a bit reserved to speak about an "unprecedented potential".
  We agree with the reviewer, and rephrased as follows:
  If these would be overcome, given their global coverage, there is potential of CMLs for large scale evaporation monitoring

- Line 30: What is a "ground-truth for model simulations"? Normally the term "ground truth" is related to satellite data.
  We referred here to the use of observations as references for model simulations. Therefore, we adapted as follows:
  …or serve as reference for model simulations…

- Line 34: What to you mean with "spatial estimates … with a high … spatial resolution"?
  Here, we tried to build a connection between the previous and the next paragraph. In the next paragraph, we elaborate on the characteristics of ECs (high temporal resolution, but low coverage) and satellites (low temporal resolution, but high coverage). We consider CMLs as middle ground between the two. Therefore, we tried to emphasize the high temporal and the high spatial resolution. To put an additional emphasis on this, we added as follows:
  However, areal estimates of actual evaporation with both a high temporal and high spatial resolution are difficult to obtain.

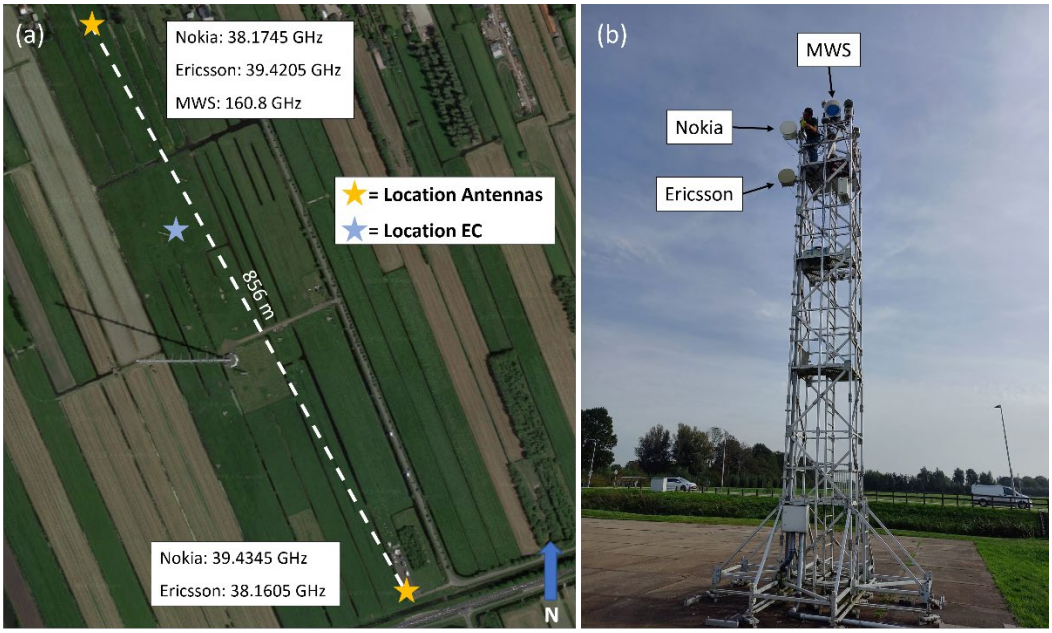
- Line 42: The argument with "strong theoretical assumptions" could also be claimed for scintillometer measurements, there is quite a number of assumptions behind the derivation of heat and evaporative fluxes from the scintillations.
  We agree with the reviewer that this also holds for scintillometry. Therefore, we removed accordingly.

- 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"
  We agree that this is a more correct reference. We changed accordingly.

- Line 59: What do you mean with “both fluxes that are part of the water balance”?
  We agree that this statement was unclear. We adapted as follows:
  …that CML signals could be used to estimate rainfall and evaporation, both part of the water balance (similar to Leijnse et al., 2007b, c).

- Line 140: The “grassland fields” at the Cabauw site are stripes regularly separated by open water ditches which might be of relevance for evaporation.
  We agree that this is important to mention and followed the suggestion of the reviewer:
  The surrounding terrain consists mostly of grass fields, regularly separated by open water ditches (see Fig. 1a), and some small villages.

- Figure 1b: The three microwave antennas could be marked by arrows.
  We added textboxes with arrows to the figure. The figure has become:



- Line 150 (and also line 209): Try to avoid separation of numbers and units trough CRLF.
  In the new version of the manuscript, we paid attention to this, and will do the same for a final version during the typesetting phase.

- 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.
  We agree with the reviewer that this reference was unclear. A reference to Figure 4a would have been more appropriate in combination with the remark that there is an offset between the two devices. We revised as follows:
  For example, in Fig. 4a between approximately 0.1 and 1 Hz, the Nokia CML and the MWS show a similar behaviour, although with an offset for the Nokia CML.

- Table 1, headings of the 2nd and 3rd columns: Shouldn’t it be “log (f0[Hz])” instead of f0 [log(Hz)], same for f1?
  During preparation of the manuscript, we were not entirely sure what the correct order is to write this. We changed following the suggestion of the reviewer, and will pay attention to this during the typesetting phase.

- Line 363: Isn't it the variances instead of the structure parameters that might get negative?
  We agree with the reviewer that indeed the variances would get negative. However, after revision of our methods this sentence has been removed from this section.

- 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.
  We agree and changed accordingly.

- Line 518: specific heat capacity at constant pressure
  We agree and changed accordingly:
  c_p is the specific heat capacity of air at constant pressure...

Technical Issues / Misprints:

- Line 21: delete "is"
  We agree, we removed "is":
  The comparison and noise determination with the microwave scintillometer provides the best possible

- Line 62: Should be "millions" instead of "million"?
  We changed to "millions":
  …from 4.6 millions in 2021 to 6 millions in 2027…

- Line 80: "In" after the ":"
  We agree, we changed accordingly:
  This paper is organized as follows: In Sect…

- Line 82: "with the CMLs" might be deleted here, because a few words later the CMLs are mentioned again.
  This indeed improves the sentence:
  …we show the initial C_{nn} estimates obtained when directly applying our CMLs as scintillometers…

- Line 99: better "based on the Kolmogorov law" (to avoid the doubled use of "follow")
  We agree, we changed the sentence according to your suggestion:
  …in the inertial subrange, based on the Kolmogorov law for three-dimensional…

- Line 358: insert "that" after "show".
  We adopted your suggestion:
  ….show that both methods capture the daily cycle typically….