Dear Referee,

We would like to thank you for taking the time to review our paper and for all your constructive suggestions, which definitely helped to improve the quality of the manuscript. We reply to your comments below. First, we treat the major comments and have grouped some of these (and corresponding detailed) comments under the same header, to answer these all in one. Further down, we reply to the remaining detailed comments. Our response to the comments appears in bold and revised text as *italic***.**

Major comments:

Comments on aperture averaging (Sect. 2):

- The paper focus on the potential use of commercial microwave links as scintillometers. In a first part, authors recall the basics of scintillometry, analyse and compare the scintillation signal and the deduced structure parameters from commercial microwave link with those from research reference instruments (Microwave scintillometer and Eddy Covariance data). For this first part a detailed control has to be done on text and formulas. They are both sometimes associated with small or large aperture hypothesis without specification of one or the other. Small aperture formulas are expected for Microwave links. This is confusing as the theoretical part doesn't match with the presented data. The reference cited could be more appropriate, using the original references rather than an handbook not completely dedicated to the scintillometry theory. This part ends with a clear result that should be relevant for publication showing the limitations of the CML devices to measure Cnn.
  In reality the microwave scintillometer used in this study is somewhere in between a small, i.e., point-source, and a large aperture system, due to the small wavelength (160GHz) and short path length (<1km). More on this in our replies to the detailed comments below. Also, we changed the reference based on your comments and the other reviewer's comments to the specific book chapter in that book, which is completely dedicated to scintillometry. See our reply to your detailed comments below.

- L97: Note that these functions accounts for the aperture averaging, may be not useful for MW scintillometers

- L107: This relationship is the one for small aperture links
  We included these aperture averaging functions, because they *are* important at microwave frequencies, especially but not exclusively for systems with higher frequencies and larger apertures installed over shorter paths (see the appendix of Ward et al. (2015a)). Moreover, we have added the relationship for small aperture scintillometers, i.e., point-source scintillometers, here, given that microwave links are often assumed to be point-source scintillometers. We added the following text to clarify the influence of aperture averaging on the integration constant *c* in Eq. (2):
  …For microwave links, this condition is usually valid (e.g., Ward et al., 2015). Note that in Eq. (2), we chose the analytical expression for a point-source scintillometer (F ≫ D), which is what most microwave scintillometers are or approximate. However, at the microwave frequencies range used in this study, in combination with a short path, the diameter of the Fresnel zone is such that the aperture averaging effect, i.e., the latter two terms in Eq. (1), is not negligible. Ward et al. (2015) show that for high transmitting frequencies, short path lengths and large apertures, these terms can have a significant effect at microwaves frequencies, which is

reflected in set-up dependent integration constant c. For example, the microwave scintillometer used in this study, transmitting at 160.8 GHz with an aperture of 0.3 m, c equals 2.60, while for the CML, transmitting at 38.2 GHz and also an aperture of 0.3 m, c equals 2.20. Neglecting the aperture averaging terms, i.e., assuming a perfect point-source scintillometer, c equals 2.01 for all frequencies, apertures and path lengths.

- L98: True for Large aperture scintillometers, but -8/3 for small aperture. MW scintillometers are generally used as small aperture.
  We agree with you that -8/3 is typical for point-source scintillometers. We referred in this case to the original Kolmogorov spectrum for the refractive index in terms of wavenumbers and did not mean to refer to the power spectrum in terms of frequency of the scintillometer intensity spectrum. We do realize that it would have been good to also mention the -8/3 for point-source scintillometers, and also added this to Fig. 4. We added as follows:
  … which shifts the scintillation spectrum to higher frequencies with higher u⊥ values, while retaining the variance (e.g., Medeiros Filho et al., 1983; van Dinther, 2015). For point-source scintillometers, typically assumed for microwave wavelengths, the power spectrum of the signal intensity typically follows the power law $f^{-8/3}$.

- L119: As Foken never worked directly on scintillometry, I would prefer to follow Hill et al. 1980 or his review Hill 1992.
  Based on your comment and the comment of the other reviewer, we changed this reference to the scintillometer chapter in Foken, which gives a more complete and more up-to-date description of the state of the art in scintillometry today than the references you suggest. The reference is now:
  Beyrich, F., Hartogensis, O. K., de Bruin, H. A. R., and Ward, H. C.: Scintillometers, in: Springer Handbook of Atmospheric Measurements, edited by Foken, T., pp. 969–997, Springer International Publishing, ISBN 978-3-030-52171-4, https://doi.org/10.1007/978-3-030-52171-4_34, 2021.

Comments on overall methodologies:

- The second part is less convincing as it is not clear at the end if the signal from the commercial link contains useful scintillation information or if the results are just a degradation of the signal from the MWS microwave research device, as the correction methods include the observed scintillation behavior from the MWS data

- In method 1 S_noise seems to includes the power -8/3 decaying part which evolves along the day with the turbulence activity and cross wind intensity. For method 2 said to be built on method 1 it is also not clear what part of the MWS signal is included in the corrected CML data. It seems this method can be considered as a spectral modeling method using a transfer function based on theoretical spectra functions for different crosswind values, but it is not clear if the noise reduction of the first methods has been applied or not.

- The overall description of both methods, even though it is "basic" methods, is not clear enough to help the reader understanding what is the quantity of the MWS signal is included in the correction (see detailed comments on the pdf). It is even not clear on which dataset the noise is caracterized, if an average noise (pre frequency bin) is removed for any half hour or if the noise is characterized for any half hour, and then not clear how the noise is removed in the 0.1 - 1 Hz frequency range using the 1Hz - 10Hz caracterisation.

- Section 5.1: With these methods 1 and 2, the corrected CML observation are completely linked to the MWS observations. You probably looked at CML data applying low pass filters? This should be tryed first because this is independant from the MWS signal and is by construction a more continuous approach than bin correction methods.

- L314: Not clear if method 2 is completely independant from method 1. It seems not, at least for f0 f1 identification. However, the way you discussed the method on the way often suggest they are independant methods.

The two methods directly built on each other and therefore were not independent, nor were they independent of the scintillometer measurements. The reviewer's comments prompted us to reconsider this approach and we have decided to revise the first method. We now solely use the CML to estimate C_{nn} by determining the noise floor as the x^{th} quantile of the variance (and C_{nn}) values. The second method, i.e., originally the crosswind-independent method, now also becomes independent from the first method. Also, we have decided to rename the correction methods into 'constant noise correction' and 'spectral noise correction', of which the latter can now also be interpreted as the best possible correction method, as it makes use of the MWS and selects only parts of the CML spectrum which behave similar to the MWS spectrum. Below, we included the largest and most important textual changes as a consequence of these methodological changes:

The introduction of Section 5, we revised as follows:

In this section, we provide two practical correction methods for the observed deviating parts in the power spectra of the Nokia CML. The first method is a basic noise correction based on CML signal itself, assuming that the CML noise always has the same influence on the C_{nn} estimates. We refer to this method as 'constant noise correction'. Our second method makes use of the MWS and selects parts of the power spectra where the Nokia CML behaves in correspondence with the MWS, dependent on crosswind conditions, and correct for the omitted part of the scintillation spectrum based on scintillation theory. We refer to this method as 'spectral noise correction'.

The introduction and step 1 of Section 5.1 has become as follows:

Our first method assumes there is a constant noise floor with (scintillation) frequency and over all time intervals present in the Nokia CML signal, probably as a consequence of the designed noise floor in the receiving antenna. Under this assumption, we can write the variance of the CML as:

$\sigma^2_{CML} = \sigma^2_{absorption} + \sigma^2_{scintillations} + \sigma^2_{noise}$. *(12)*

The method consists of estimating the contribution of the noise floor to $\sigma^2_{ln(I)}$ by subtracting a low quantile of all Nokia CML-derived values of $\sigma^2_{ln(I)}$ (or C_{nn}) from itself, based on the calibration part of the dataset. All values below this percentile are removed, since these would become negative after correction.

***Step 1. Noise estimation** (only calibration part of the dataset)*

***(a) 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.*

(b) ***Determine σ^2_{noise}:*** *We assume the 7th percentile of the $\sigma^2_{ln(I)}$ values of all time intervals belonging to the calibration dataset to represent σ^2_{noise}. Calibration of the RMBE in comparison to the MWS shows that this percentile results in a relatively low RMBE while still maintaining a large portion of the observations (i.e., 93 % of all time intervals). It should be noted that the influence of the selected quantile on the performance of this method is relatively low. Other quantiles in this range would result in a similar performance of the CML C_{nn} estimates.*

The introduction of Section 5.2, we revised as follows:

In this method, we make use of the MWS to determine the noise contribution to the Nokia CML signal. Also, we take into account the crosswind condition, as the scintillation spectrum shifts to higher frequencies with higher crosswind speeds. We therefore

select, depending on the crosswind, those parts of the spectrum where the Nokia CML and the MWS data behave similarly. 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. After computing the (partial) variance of the selected parts of the spectrum, we correct for the fraction of $\sigma^2_{ln(I)}$ omitted based on the theoretical spectra (Eq. 1). For operational CMLs this method is usually not possible, but it shows the potential of using CMLs as scintillometers.

Other parts of Section 5.2 remained the same, combined with some additions of parts that were previously in Section 5.1.

Comments regarding splitting the dataset in calibration and validation:

- Separating the data set in two, with a calibration segment and an evaluation segment could make the study more convincing.
- L409-411: This should be shown separating your dataset in two: one calibration segment and one evaluation segment.
- L328-333: It is a statistical crosswind model. This should be design and evaluated with different datasets.

We agree with the reviewer and split the dataset in a calibration and validation dataset, using 80% of the data for calibration and keeping 20% for validation. We do this random over the entire time series. We added this in Sect. 3, now refer to this in Sect. 5 at the start of the stepwise explanation of the methods and mention this in the caption of Fig. 9:

…Nokia CML vibrates above this wind speed, as we observe in our data an increase in variances above this limit (not shown). We divide all time intervals that do not meet the previously described conditions randomly over a calibration and a validation set. We use 80% of the data for calibration and 20% for validation. Additionally, ….

Step 1. Noise estimation *(only calibration part of the dataset)*

Figure 9. 30-min C_{nn} estimates obtained with the Nokia CML for the time intervals in the validation part of our data, post-processed with the constant-noise method (a and c) and spectral-noise method (b and d) versus the MWS (a and b) and the EC (c and d) estimates, corrected for the height difference (Sect. 3.3). The red line is the 1:1 line.

Comments regarding the structure:

- The structure of the paper should be reorganised, putting together all the methodology parts, including correction methods.
  From this remarks the overall feeling is that no general procedure could be applied to the CML dataset, without a continuous MWS beside. I suggest the authors should re-organise the paper and their argumentation with the only objective to demonstrate that useful scintillation informations are included in the CML data, which I believe looking at the only spectrum presented. Then eventually demonstrate for which conditions scintillation informations are extractable from the raw data or not.

- Section 5: The methodology part should be with in a clear methodology section. Part of it could be reported in annex, and analysis of the performance of different steps shown in this section

  We understand the reasoning of the reviewer; however, we have selected this structure on purpose. Section 3 is solely used to introduce the instruments and data. Section 4 contains the problem statement, in which we show what the actual issues are to use the Nokia CML "as is" as scintillometer. In Sect. 5, we present the methods to overcome these issues, i.e., the presented correction methods, which can be considered the main result of our study. Therefore, we prefer to leave the structure as is. To emphasize this structure, we adapted the final paragraph of our introduction as follows:

  …In Sect. 3, we give an overview of our experimental setup and in Sect. 4, we show what problems occur when using CMLs as scintillometers to directly obtain C_{nn} estimates. Based on these findings, we present our proposed correction methods to obtain improved C_{nn} estimates with CMLs in Sect. 5, partly based on the theory provided in Sect. 2.

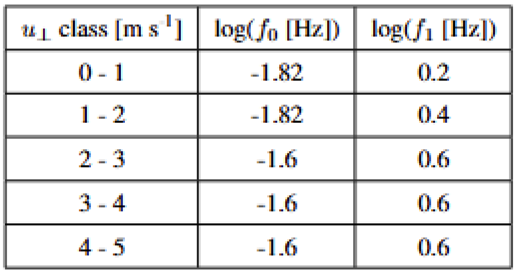
Comments specifically focussing on the second method:

- For model 2, the identified cutoff frequencies have an unexpected non monotonous dependency with the crosswind values. This is not discussed. From spectra modeling, this behavior means that there is other dependencies in the dataset. More over, it is not compatible with the applied high pass filter.

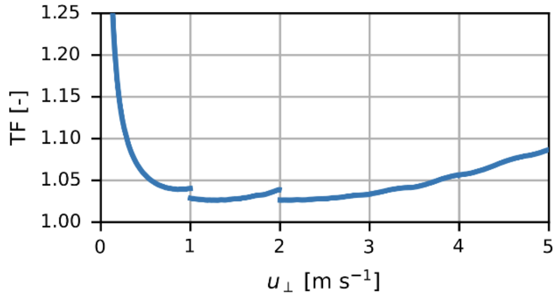
- Table 1: Non monotony of f0 with the crosswind is questionable. Can you comment on that ? One point could be the impact of attenuation, which randomized the lower frequency limit as it doesnt impact the scintillation the same way at 38GHz and 160GHz.

  We agree with the reviewer that a monotonous increase of frequencies with crosswinds would be expected. After the revision of both methods, we decided to only focus on RMBE as selection criterium for the frequencies (next to number of observations). This makes the selected frequency bands increase more monotonously, as expected. Moreover, it is now

compatible with the new HPF. The new frequency bands and values for the transfer function are as follows:



$u_\perp$ class [m s^{-1}]	log(f_0 [Hz])	log(f_1 [Hz])
0 - 1	-1.82	0.2
1 - 2	-1.82	0.4
2 - 3	-1.6	0.6
3 - 4	-1.6	0.6
4 - 5	-1.6	0.6



Moreover, we adapted the following text under the description of the transfer function: *The values for the transfer function are shown in Fig. 9. For $u_\perp$, we use the exact value and not the crosswind class values, so that within each class the value of the transfer function still varies, especially for the lowest crosswind speeds. Note that the values for TF between crosswind classes increase nearly monotonously with increasing crosswind, as would be expected since the power spectrum shifts to higher scintillation frequencies with higher crosswinds. The minor shifts in TF are a consequence of the different total width of the selected frequency bins of the power spectrum and location of these selected bins (Table 1). Stricter selection criteria would cause TF to show larger shifts between crosswind classes (not shown here).*

Other general comments:

- For both methods the impact of filtering and correcting should be better analysed. The reader has just some "hypothetical" plots which makes difficult the appreciation of the methods performances. For example, the noise removing procedure and sig2_noise estimation could be applied to the no-scintillation dataset (turned off transmitting antenna).

- A better characterization of the noise accross frequencies for different conditions, especially with T°, windspeed, should help to consolidate method 1.

- Figure 6: You should present results from observed data rather than hypotetical, and discussed when the method works and when it doesn't.

- L288: with is perturbating as the high pass filter has not been applied on the figure.

- Figure 7: perturbating as there should be 5 intervals with a 0.2 power step. the median should be indicated on the graph.

- Figure 8: Figure 6 7 and 8 should be grouped in one unique figure.
  We used hypothetical plots to clarify how our methods work, so that the reader can focus on the methodology rather than on the variable behaviour of individual power spectra. Based on the reviewer's suggestions, we revised them as follows:

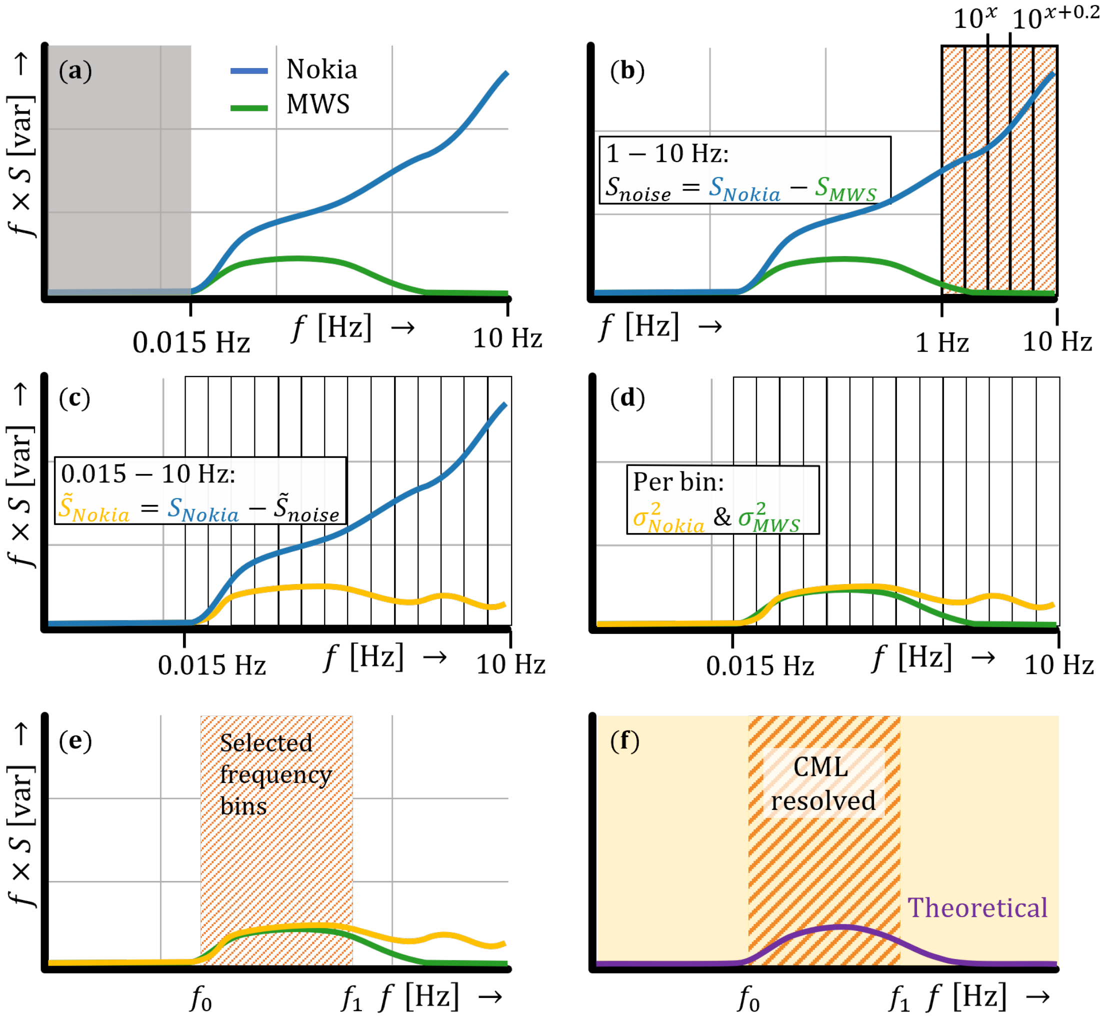


Figure 6. Visualisation of the spectral noise method using hypothetical power spectra. Step 1a: Hypothetical power spectrum with application of a high-pass filter at 0.015 Hz to the Nokia CML (blue) and the MWS (green) (a), step 1b: S_{noise} calculation between 1 and 10 Hz per 0.2 log(f) frequency bin for the Nokia CML and MWS for f × S spectrum (b), Step 1c: correcting S_{Nokia} with S_{noise} per frequency bin (c), Step 1d: Computing $\sigma^2_{ln(I)}$ per frequency bin for both devices (d), Step 1e: selected frequency bins for an individual power spectrum by comparing the corrected Nokia CML with the MWS (e) and Step 1f: theoretical spectrum in which red hatched area indicates the selected frequency bins based on step 1e (i.e., the denominator in Eq. 14) and the orange area indicates the full frequency axis (i.e., the numerator in Eq. 14). f_0 and f_1 in (e) and (f) depend on crosswind conditions and can be found in Table 1.

Moreover, we agree with the reviewer that adding an analysis regarding the performance of the methods as function of weather conditions is valuable. We decided to include this in the Appendix, as there is no clear relationship, other than an overestimation of C_{nn} by the

Nokia at lower temperatures, although this is not a very clear relation. We added the following statement in the main text (now L391-392):

…Moreover, the performance of both methods does not seem to show any large dependence on weather conditions, like temperature, crosswind, humidity and incoming shortwave radiation (Fig. F1).

In the appendix, we added the following:

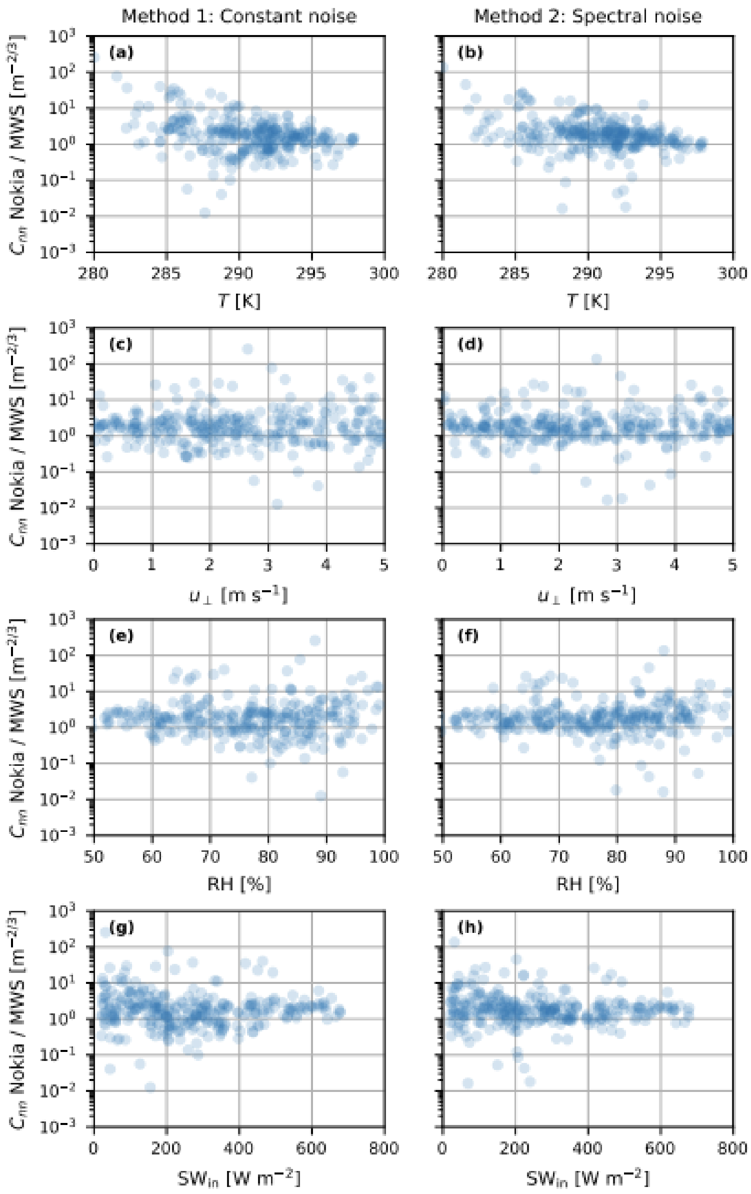


Figure F1. Ratio of C_{nn} estimates obtained with the Nokia CML correction methods and the MWS versus 2 m air temperature (a and b), 10 m crosswind conditions (c and d), 2 m relative humidity (e and f) and incoming shortwave radiation (g and h) for the calibration part of the dataset.

Detailed comments:

- L21: remove "is"
  We changed accordingly:
  The comparison and noise determination with the microwave scintillometer provides the best possible

- L28: evapotranspiration
  Even if urban areas are may be the target of such a study because of CML density.
  By referring to evaporation, we mean any form of phase change of water from liquid to gaseous. This also includes transpiration. See for example https://doi.org/10.1029/2020WR028055 for the definition we employ. Therefore, we prefer the use of evaporation over evapotranspiration.

- L32: Cohard et al. 2017, Descroix et al. 2011

  Cohard, Jean-Martial, Jean-Michel Rosant, Fabrice Rodriguez, Hervé Andrieu, Patrice G. Mestayer, et Pierre Guillevic. « Energy and water budgets of asphalt concrete pavement under simulated rain events ». Urban Climate 24 (1 juin 2018): 675-91. https://doi.org/10.1016/j.uclim.2017.08.009.
  Descloitres, M., L. Séguis, A. Legchenko, M. Wubda, A. Guyot, et J-M. Cohard. « The Contribution of MRS and Resistivity Methods to the Interpretation of Actual Evapo-Transpiration Measurements: A Case Study in Metamorphic Context in North Bénin ». Near Surface Geophysics 9, n° 1780 (avril 2011). https://doi.org/10.3997/1873-0604.2011003.
  We thank the reviewer for providing extra references. We implemented them as follows:
  Especially for evaporation, areal estimates can provide essential information for catchment-scale water budgets (e.g., Descloitres et al., 2011; Cohard et al., 2018) and, for example, for irrigation requirements or drought monitoring (e.g., Burt et al., 2005; West et al., 2019).

- L37: A bit short ! EC consists of 3D sonic anemometers ... by measuring vertical flux terms of conservation equations after using the Reynolds decomposition.
  We followed the suggestion of the reviewer and changed accordingly:
  …in order to determine the transport of momentum, temperature and moisture by measuring vertical flux terms of the conservation equations after using Reynolds decomposition.

- L49-50: Less sensitive to surface heterogeneity than EC stations because of the spatial averaging process and the less varying footprint
  We have elaborated on this sentence and used the suggestion of the reviewer:
  ….measurement method is less sensitive to surface heterogeneity than EC stations because of spatial averaging and the more homogeneous footprint.

- L50-51: partly true. Considering Turbulence intensity they are used in airports to measure turbulence activities on runaway. Dinther et al. ?
  The crosswind measurements of van Dinther et al. (2015) have only been used during short experimental campaigns. Long-term scintillometry experiments exist (e.g., at the Lindenberg observatory, Germany, since 1998), but are not common, mostly as a consequence of the high investment costs in installation. In this sentence, we wrote that

also maintenance costs can be high, however after reconsideration we decided to remove this, as this is not necessarily the case.

- L59: What fluxes do you mean ? Sensible and latent heat fluxes or rain and latent heat fluxes? Note that 2 scintillometers are required to estimate both turbulent fluxes, using 2 wavelengths favoring T° or moisture contributions. You mention it in the discussion part. You can may be temperate this perspective
  We clarified this statement. Moreover, we agree that nuancing this statement adds clarity. 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). Note that to compute the turbulent heat fluxes though scintillometry, and thus evaporation, additional information on the relative contributions of temperature and humidity fluctuations is required.

- L69-71: from both cited papers, it is not clear what are you meaning in term of deviating behaviour as tboth papers didn't mention Cnn estimations. Please detail more what you mean especially if it matters for the following.
  Indeed these studies focus on rainfall estimation. However, both studies noticed the deviating behaviour of the CML versus the research link during dry periods. Therefore, we added as follows:
  Moreover, in rainfall intercomparison studies (van Leth et al., 2018; van der Valk et al., 2024), a formerly employed 38 GHz CML was found to exhibit a deviating behaviour compared to a 38 GHz research link during dry periods.

- L95: may be use beam rather than link when talking about wave propagation.
  We agree that this clarifies the sentences and changed "link" to "beam" in this paragraph:
  …$u_\perp$ is the wind speed [m s^{-1}] perpendicular to the beam path…
  … x [-] is the relative location along the beam path…

- L139: cm precision may be not necessary.
  We agree. We removed two digits:
  The links and scintillometer transmit along an 856 meter path between 51.9743 N, 4.9235 E and 51.9676 N, 4.9296 E.

- L140: Do you mean that you checked/measured that vibrations can be neglected ?
  The towers are designed to have minimal amount of vibrations for a project funded by The Dutch Research Council (NWO). We added as follows:
  On both sides, the CMLs and MWS are mounted on a 10 meter high vibration-free mast (as designed for a project of NWO, 2021).

- L140: netherland climate ?
  We added as follows for clarification:
  The site is located in a European marine west coast climate (Cfb in the Köppen classification).

- L142: Do you mean within the scintillometer footprints ?
  To clarify what is most often in the footprint, we added as follows (also considering the comments of the other reviewer):

The surrounding terrain consists mostly of grass fields, regularly separated by open water ditches (see Fig. 1a), and some small villages. Under the prevailing south-westerly wind conditions the scintillometer footprint does not contain any obstacles within more than 2 km, except the 213 m flux tower. Elevation differences….

- L145-150: Can you precise aperture of all instruments/antenas ? THis is important to consider if you are in the small or large aperture range (compare with (lambda x L)^(0.5)

  We agree that it is important to specify the aperture of all antennas, in order to specify whether we are dealing with small aperture or large aperture range. Here we adapted as follows:

  …bandwidth of 7 MHz. The diameters of the antennas of both links are 0.3 m. Both links are bidirectional….

  …internal datalogger of the MWS. The aperture of the MWS is 0.3 m. Data from the MWS…

- L221: raw temperature and wind speed components EC data ?

  This is indeed not clear. With "raw", we referred to the 10 Hz measurements. Therefore, we rephrased as follows:

  It should be noted that the 10 Hz temperature and wind speed components for the EC show unexpected behaviour.

- L222: I don't get what this means

  In the histograms of Fig. S1, some temperatures and wind speeds are more frequently reported than temperatures and wind speeds that are approximately the same. We rephrased as follows to clarify:

  … because some temperatures and wind speeds occur much more frequently than other temperatures and wind speeds that are approximately the same…

- L222-223: S1 shows a full day of data, including several wind speed. This is not surprising and have no value for half hour flux calculation. stationarity is a relevant criteria at the averaging period scale.

  We use Fig. S1 to illustrate the behaviour of the 10 Hz EC data. We are not referring here to the performance of the half hour flux calculation, but to the previously reported behaviour. To emphasize this, we added as follows:

  (See Fig. S1 for a histogram of the wind speed, temperature and humidity measurements during a full day, i.e., 11 september 2023, to illustrate this unexpected behaviour)

- L236: orders → order

  We agree:

  the RMBE represents the order of magnitude the values

- L236-237: please rephrase or supress

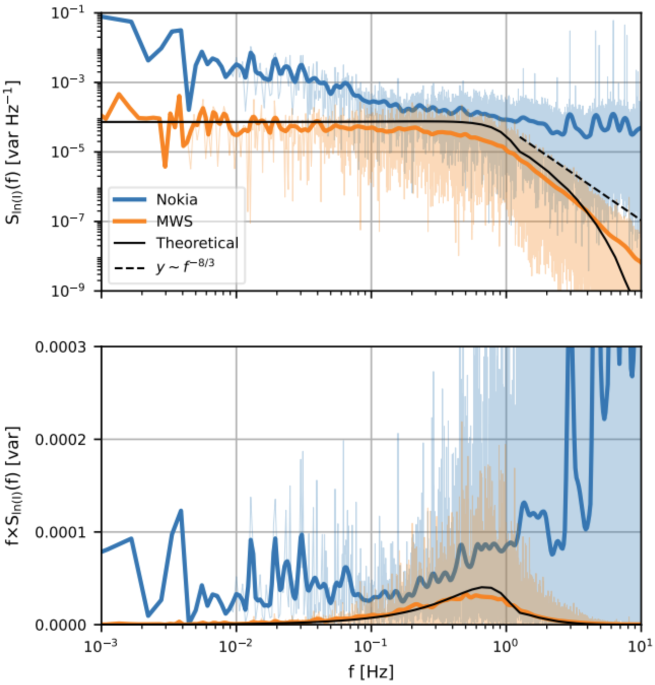
  We do not understand this comment. To us, this sentence seems clear. Therefore, we do not change anything.

- L245: and very less dynamic (less than one order of magnitude) compare to Cnn_MWS (More than 2), which can lead to the question : "is there useful scintillation signal within the CML signal ?"

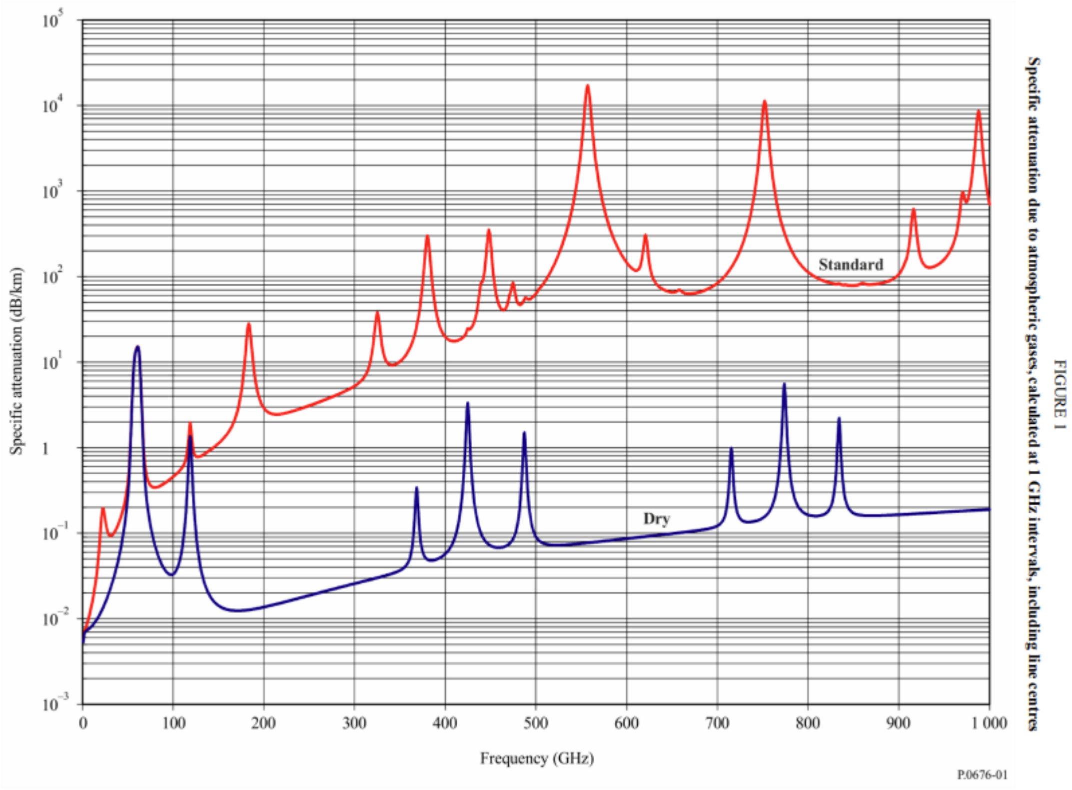
We agree that it also seems less dynamic, although it should be noted that the values for the Nokia CML are at least one order of magnitude larger than the values of the MWS, so that these dynamics are hard to see. We added as follows:
…in comparison to the MWS (Fig. 2). Also, the estimates of the Nokia CML are less dynamic than the MWS, although part of this is caused by the larger values of the Nokia CML, at least one order of magnitude, so that variations corresponding to those found in the MWS estimates are visually hard to identify in the Nokia CML estimates. Additionally, outliers are…

- L249: This is not what you said in sect2. Your results show a -8/3 slope and you presented a -11/3 slope in the theoretical sect.
  We adapted our text in the theoretical section, see our reply to your comment on L98.

- L249: Not so minor (factor of 2 for the cutting frequency. Can you precise the measured windspeed and direction at Cabaw and on your mast.
  A critical revision of our programming revealed an error in the definition of k (the wavenumber of the transmitted radiation). Instead of k = 2πf/c, we defined k as f/c, so that the location on the frequency axis of theoretical spectrum was slightly off. The correct theoretical spectrum is as follows:



- L253-254: Yes, The MW transparency window around 100GHz is closing. Atmosphere is less transparent at 38GHz than 160GHz
  We are not sure if this is the case. If we consider the technical report on "Attenuation by atmospheric gases and related effects" by the International Telecommunication Union (2005) (https://www.itu.int/dms_pubrec/itu-r/rec/p/R-REC-P.676-12-201908-S!!PDF-E.pdf), we see in Fig. 1 that the specific attenuation suffered by microwave signals propagating in a standard atmosphere is higher at 160 GHz than for 38 GHz.



- Figure 4: please plot f^(-11/3) or better the expected small aperture f^(-8/3) on the loglog graph

  We agree with the reviewer that this would improve insight in the degree of correspondence of the MWS spectrum with respect to theory. We added the $f^{-8/3}$ line (see our reply to the comment on L249) and adapted the caption as follows*:*

  Figure 4. (a) Power spectrum of the signal intensities of the MWS (orange), Nokia CML (blue) and a theoretical spectrum, using C_{nn} obtained with the MWS, of a theoretical 38 GHz MWS based on Eq. (1) on 12-09-2023 between 9:00 and 9:30 UTC and (b) the contribution to the variance of the signal intensity per logarithmic frequency interval. The dashed line in (a) represents the theoretical power law for point-source scintillometers, which is typically expected for microwave frequencies. Note that the MWS in our experimental setup does not perfectly behave as point-source scintillometer (Sect. 2). The shaded areas are the raw power spectra, while the lines are the smoothed versions of the spectra (following Hartogensis, 2006). Moreover, the MWS in this case is the equivalent 38 GHz MWS data (Eq. 7 in Sect. 3.2).

- L278: Constant with frquency ? or constant from half hour to half hour ? or both ??
  It is not clear if the noise floor is calculated just once or not and if it is calculated once, on which dataset it is calculated

  Indeed it was not clear what we meant with constant. We meant both in this case. We revised as follows:

  Our first method assumes there is a constant noise floor with (scintillation) frequency and over all time intervals present in the Nokia CML signal,…

  Moreover, as we have revised this method (see our reply to your comments on the methodology), we have also clarified our methods for computation.

- L292: frequency
  We are not sure why the reviewer would like us to replace *time* with *frequency.* We state here *For each time interval,* to clarify that we obtain values for each time interval and not a single value per 0.2 log(f) frequency bin. Therefore, we left this as it was.

- L296-297: Not clear if Snoise is a single value or if it is a function of f.
  We added the word *single* as follows for clarification (note that this is now part of Sect. 5.2; see our reply to your comments regarding the methodology):
  We take the median of all frequency bins and time intervals resulting in a single estimate of S_{noise} *between 1 and 10 Hz for all time intervals.*

- L299: Not clear how sig2_noise between 0.1 Hz to 10Hz is calculated from S_noise from 1Hz to 10Hz. Please develop.
  We are not sure what the reviewer means here. For any spectrum S, the variance between any range of frequencies can be computed using Eq. (13), in which we have also written how the single value S_{noise} is used to compute σ^2_{noise}.

- L365: THis is a rather large value !!
  We agree with the reviewer and we changed this to 20 W m^{-2}. We revised as follows in Sect. 3:
  For our analysis we do not consider nighttime time intervals (i.e., incoming shortwave radiation below 20 W m^{-2}*…*

- L399: Already said in the result part.
  Following the change of methodology, this is not a valid statement anymore. We removed this sentence.

- L419-421: Not available and often impacted by temperature !
  We agree that this sometimes is not available and that temperature can impact this noise floor. Therefore, we added as follows:
  For example, this could disclose the dependency of a noise floor on the signal intensity, temperature or the possible presence of any frequency-domain filter. Yet, usually this information is not available or shared publicly, complicating the C_{nn} *estimation.*

- L422: the discussion before concerns the MWS requirement. Here starts the comparison with literature. this should be 6.2
  We do not understand why Sect. 6.2 should start here. We deliberately chose this specific section arrangement. In Sect. 6.1, we focus on obtaining C_{nn} estimates and also compare how our estimates align with other literature. In Sect. 6.2, we discuss the use of CMLs as scintillometers in a broader context. Therefore, we left the section arrangement as it was.

- L439-440: what do you mean ? number of devices over the world ? If so, note that scintillation is impacted by saturation for long path. This will limit the global coverage to links shorter than some kms.

Indeed, we refer here to presence of CMLs around the world in comparison to high-quality meteorological measurements, both in number of devices and coverage. We agree that saturation is important to consider for longer path lengths. We added as follows:
…do not require any additional measurements and have a potentially larger number of devices as well as coverage globally. Note that for long paths, saturation of the scintillation signal could also influence obtained C_{nn} estimates (e.g., see Meijninger et al., 2006, for the saturation limit for microwave frequencies).

- L446: Rephrase to explicitely say that you tested quantization impact from the Nokia data in apendix C
  We rephrased as follows:
  We tested the impact of power quantization on our data and expect that for the smallest quantization steps, C_{nn} estimates could still be feasible, though quantization would be an additional source of uncertainty (Fig. C1a and b).

- L452: Note that CML internal microwave designs are probably different from one to the other (Micrwave source, noise source, frequency gabarit, ...) all the intern microwave pieces potentially add noise related to T° l !
  We agree with the reviewer and added the following to clarify:
  In order to determine how antennas modify the received signal intensity, e.g., as a consequence of different internal hardware design choices, a comparison with an MWS would be required for each specific type of CML antenna.

- L521: First Koojmans and Hartogensis 2016 didn't define these similarity functions they proposed a statistical framework to propose functions. second, there still has no framework to ensure that these universal functions exist for heterogeneous landscapes, rather there are good reason to think that for heterogeous landscapes similarity functions can vary from a site to the other (see Katul 2011). Then I would say :
  "computed these similarity functions from various experiments ..."
  We agree with the reviewer and followed their suggestion (partly based on the suggestions of the other reviewer):
  Kooijmans and Hartogensis (2016) computed the similarity functions f_{TT} and f_{qq} for unstable conditions from various experiments, …