The manuscript presents an innovative approach to estimating turbulent heat fluxes using commercial microwave links (CMLs), which are originally deployed for telecommunication. While the focus appears to be primarily on latent heat flux, likely due to the emphasis on the growing season, the study contributes significantly to the development of novel methods for estimating turbulent fluxes.

The author begins by correcting the structure parameter of the refractive index (C_{nn}) obtained from CMLs using two approaches: (a) a constant noise correction and (b) a spectral noise correction method. Subsequently, the temperature (C_{TT}) and humidity (C_{qq}) structure parameters are derived and used to estimate sensible and latent heat fluxes. This is done via two main frameworks: (i) the two-wavelength method, involving microwave scintillometers (MWS) and large aperture scintillometers (LAS), and (ii) the energy balance method, which is constrained using (a) eddy covariance-based net radiation and ground heat flux, and (b) remote sensing-derived net radiation.

In addition, the author compares different scaling assumptions for turbulence—free convection versus full Monin-Obukhov similarity theory (MOST).

While these methods are comprehensive and valuable, the current structure of the manuscript makes it difficult for the reader to follow. All methods and comparisons are presented together, which overwhelms the narrative. I strongly suggest reorganizing the content into a more hierarchical or modular format. For instance, the manuscript could be structured as follows: a) Evaluation of the noise correction methods for C_{nn}, b) Performance assessment using remote sensing vs eddy covariance constraints, and c) Sensitivity to turbulence scaling assumptions (free vs full MOST).

This is just a suggested outline, and the author is welcome to be creative. However, a clearer and more modular structure would greatly improve the readability and impact of the paper. Please find some other concerns below:

A) In my opinion, Figure 1 does not represent the core contribution of the paper and mainly provides background or contextual information. Therefore, I suggest moving it to the supplementary materials.
B) The manuscript states that the observation period spans from April 1 to October 1. However, the full time series (TS) over this period is not shown anywhere. While it is reasonable to highlight selected days to illustrate the diurnal cycle, an overview of the entire time series is important to assess the consistency and overall performance of the method.
C) For the Monin–Obukhov Similarity Theory (MOST) scaling, two additional parameters—z0m (roughness length for momentum) and d (displacement height)—are required. These parameters depend on the characteristics of the roughness elements, vegetation height, and atmospheric conditions. However, the manuscript does not provide any information on how these values were determined or set.

D) The current version of Figure 6 is not very clear or easily readable, especially regarding the size of the symbols in relation to the scale of the graph.

E) Line 241-242: The statement *"When considering the H estimates as well, the overall performance of both EBM versions reduces"* is unclear. Does this mean that when metrics are computed jointly for both H (sensible heat flux) and LvE (latent heat flux), the overall performance of the Energy Balance Methods (EBM) decreases? If so, how was the joint evaluation done?

F) Line 245: "*For the two-wavelength method, this overestimation is fully attributed to the LvE, since H is constrained by the LAS, while for the EBM, this overestimation can be distributed among LvE and H." while for the EBM, this overestimation can be distributed among LvE and H.* The reasoning that overestimation is "fully attributed to the LvE" in the two-wavelength method, due to LAS constraining H, is understood. However, this relies on the implicit assumption that H is accurately captured by the LAS. Could the authors clarify whether this assumption holds true in this context?

G) The discussion and conclusion sections feel somewhat verbose. Please streamline the content to avoid redundancy.

H) The manuscript lacks a discussion on how differences in footprints, sampling frequencies, and spatial/temporal representativeness among the various systems, such as eddy covariance (EC), microwave scintillometer (MWS-LAS), and commercial microwave links (CMLs), might affect the flux estimates. While a detailed intercomparison may be beyond the scope of this study, a short paragraph acknowledging these differences and citing relevant literature would greatly help readers interpret discrepancies in the time series and understand the strengths and limitations of each measurement system.

I) Additional minor thought: While comparing to the EC tower's available energy (R_n – G), it would be very helpful if the authors could plot the sum of H + LvE derived from the CML data vs EC tower H+LvE. Although the footprints and instruments are not perfectly collocated, such a comparison would provide valuable insight into the total energy captured by the CML-based approach. This could also be benchmarked against typical energy closure rates observed at flux towers (approximately 80% on clear days), offering an overview of the method's overall performance.

Thank you!

Prajwal