Reviewer 2

General assessment

This manuscript presents an exploratory machine-learning analysis of fog-related attenuation patterns in commercial microwave links, using a previously published high-resolution dataset from Wageningen and a national dataset. The study is thoughtful, transparent about limitations, and relevant to the scope of AMT. However, several physical interpretation and methodological clarity issues must be addressed before the results can be interpreted by the authors. These issues are substantial but fixable, and I therefore recommend **major revisions**.

Major comments

1. Physical detectability of fog at 38 GHz

The manuscript does not quantify the expected magnitude of fog-induced microwave attenuation at 38 GHz. According to ITU-R P.840, typical fog liquid water contents (≈ 0.02–0.5 g m^{-3}) correspond to specific attenuations of roughly 0.02–0.5 dB km^{-1} at this frequency. For the 2.2 km Wageningen link, this implies total path attenuations for standard fog liquid water contents of 0.1 $g.m^{-3}$ to be equal to 0.22 dB, which is comparable to instrumental effects and far smaller than wet-antenna attenuation. It has to be noted that some attenuation patterns during fog events are of magnitude of 4–5 dB (e.g. Figure 7, Figure 10)

We thank the reviewer for the provided insights, and we agree the Wageningen link signal includes also wet antenna effects. E.g. this is also visible when comparing the attenuation signal and the wind speed, where increased wind speed facilitates evaporation from the antenna and reduces the attenuation signal. We have now addressed this in the revised paper. However, note that our goal is to estimate fog *detection* rather than the estimation of its density. Moreover, we do not see it as a problem that the wet antenna attenuation signal contributes to the machine learning method to predict fog detection.

This physical context is essential for interpreting the ML results, particularly the inconsistent detectability of fog events. I recommend explicitly adding ITU-R P.840–based attenuation estimates and clarifying that the ML models operate close to the physical sensitivity limit of the measurement system.

-> We thank the reviewer for this reflection, and great insights. See next point for the rebuttal to both comment 1 and 2.

2. Wet-antenna attenuation (WAA) as a confounding mechanism

Wet-antenna attenuation is not explicitly discussed (it is not even mentioned in the manuscript) in the manuscript, despite being a known dominant attenuation mechanism at frequencies around 38 GHz. WAA can produce smooth, sustained attenuation during nighttime, drizzle, or post-rain conditions and cannot be distinguished from fog-induced path attenuation using RSL data alone.

Although precipitation is excluded using disdrometers, this does not eliminate WAA caused by antenna wetting, dew, or light drizzle. The ML models are not provided with inputs that allow physical separation of these effects. I recommend explicitly acknowledging WAA as a confounding factor and reframing general interpretations from “fog attenuation” or “fog detection” to “attenuation patterns associated with fog conditions” or similar

-> We thank the reviewer for this reflection. Indeed we see that for some fog cases the attenuation follows the typical 0.22dB – 0.1 g/m3 rule, but for some other we see larger amplitude of the attenuation up to 4-5. The latter can be attributed to the wet antenna attenuation as suggested by the reviewer. However it is important to note that the goal of the paper is not to detect the liquid water path or liquid water concentration, but the detect the presence of fog. The WAA is apparently a statistically relevant variable that assists the machine learning algorithm in fog detection. In the revised version of the manuscript we have added some text to underline both points (see the revised discussion of Fig 7).

We have changed the wording “fog detection” to “attenuation patterns associated with fog conditions” accordingly throughout the full manuscript.

3. Scientific goal of the manuscript

If fog attenuation at 38 GHz is near the noise floor (comment #1) or hidden by WAA (comment #2), then almost any strong proxy for low visibility (traffic speed, incident reports, camera visibility, etc.) could beat it for “fog/not fog” prediction. The key question becomes: what is the scientific goal of the manuscript—detecting fog from CMLs (opportunistic sensing), or forecasting/detecting fog by any means? The manuscript tends to answer first question, but this is difficult without reflecting comment #1 and #2.

->The goal of the study is mainly the opportunistic sensing. The reflections in points 1 and 2 have been added to the revised manuscript.

4. National-scale application (Section 3.4)

Section 3.4 applies the model to a nationwide microwave link dataset, but no spatial fog reference data exist for validation. Comparisons with nearby weather stations and radar provide plausibility but do not constitute quantitative validation.

I recommend relocating this section to the Discussion as an "exploratory national-scale application". This would prevent over-interpretation while preserving the illustrative value of the analysis.

In addition, I suggest explicitly stating at the beginning of the subsection that no quantitative validation is performed due to the absence of link-scale fog reference data, and that the purpose of the analysis is to demonstrate spatial coherence and feasibility rather than detection accuracy.

We thanks the reviewer for these suggestions, which we have implemented. Also the original Fig 14 and its original discussion has been removed.

Methodological comments

5. Train/validation/test splitting and temporal leakage

The Wageningen dataset is randomly split into training, validation, and test subsets at 30 s resolution. Given the strong temporal persistence of fog, this allows adjacent timesteps from the same fog event to appear in all subsets, leading to temporal leakage. The reported metrics therefore reflect within-event interpolation rather than generalization to unseen events.

At minimum, this limitation should be explicitly discussed. If feasible, a time-blocked or event-based sensitivity analysis would strengthen the conclusions.

We agree with the reviewer that the temporal aspect play a role here. Therefore the original analysis already included a trend variable as a remedy in the machine learning experiments (see section 2.4.3).

6. Dataset statistics and class balance

The manuscript does not report:

- the total number of 30 s timesteps,
- the number and fraction of fog-labelled timesteps,
- or the number of independent fog events.

Given the strong class imbalance and use of F1-score, these statistics are essential for reproducibility and interpretation.

- ➔ We have added the balanced accuracy (BACC) metric to tables 1 and 2. The BACC is seen as an alternative metric that provides a more robust metric in case of unbalanced data (Diallo et al, 2025). The ranking of the different metrics and model types remains the same as for the F1 scoring approach. In addition we have added the Matthews correlation coefficient (MCC) score in both Table 1 and Table 2.
- ➔ The experiment was operational from 22 August 2014 up to and including 8 January 2016. Not all instruments had been operational during this entire period though, as is indicated in Fig. 2 of VL18. From 7 to 25 August 2015 all transmitters were not operational due to a local power

outage. So there are 1.4M 30 sec time slots in the record. This has now been included in the revised manuscript.

Minor / clarity comments

7. Minor comments on abstract

- The mention of the McFly framework in the abstract could be replaced by a more general description of the applied machine-learning approach (e.g. "automated deep-learning time-series classification" or "AutoML-based deep learning"), with details on the specific software implementation provided later in the manuscript.

This suggestion has been implemented accordingly.

-The abstract concludes by suggesting the exploration of higher-frequency telecommunication links for enhanced fog detection. While this is a reasonable and widely discussed perspective, the present manuscript does not directly evaluate frequency dependence or provide quantitative evidence that higher frequencies would improve fog detectability. The argument appears primarily inferential, based on the observed limitations at 38 GHz and on prior literature. I therefore suggest relocating it entirely to ensure that the abstract strictly reflects results demonstrated within this study.

The specific sentence has been removed from the abstract and the message is now discussed in the Discussion section.

8. Definition of "channels"

The term "channel" in Section 2.4 refers to parallel CNN input feature streams of length 600 samples, with scalar predictors padded for compatibility. This should be stated explicitly to avoid confusion among readers unfamiliar with CNN terminology.

Thanks for this clarification, that we have now incorporated in ln 169-170.

9. Minor comment on length and structure

The manuscript is relatively long compared to the strength of the quantitative validation. The clarity of the paper could be improved by condensing some of the case descriptions and/or relocating illustrative material to the Supplement or Discussion. This would help align the manuscript length more closely with the scope of the demonstrated results.

We thank the reviewer for this suggestion. The revised manuscript has been shortened by about 15%/word count (from 11285 to 9602 words incl fig captions). In particular the discussion of the snow case has been removed (2 figures) and Fig 14 and its discussion has been removed.

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