

Review of the paper

“Country-wide rainfall estimates from a commercial microwave link network in Belgium”
by K. Van Weverberg et al.

<https://doi.org/10.5194/egusphere-2026-457>

This manuscript presents an analysis of rainfall estimates derived from a nationwide network of commercial microwave links (CMLs) in Belgium, along with a comparison against a dense network of operational rain gauges and radar products. Although the analysis is limited to four rainfall events, the study is valuable, as innovative rainfall monitoring approaches that provide broad spatial coverage and fine spatiotemporal resolution are highly desirable for many applications.

Regarding the manuscript's content, I particularly appreciated the detailed description of the methodology and the implementation of different methods. However, I believe that several aspects require improvement before the manuscript is suitable for publication. The authors incorporated multiple data sources and processed a large, heterogeneous set of CMLs, which is inherently complex, especially given the challenges associated with CML data processing. In my view, a major revision is necessary.

General comments:

- Paper organization.
 - I suggest moving lines 89–92 into a new subsection within the Methods section (e.g., entitled “CML Data Reduction”), as they describe a key step in raw CML data processing rather than a marginal detail. The same applies to lines 99–105. Additionally, an important assumption underlying Equations (1) and (2) is that the measurement represents an equivalent path-averaged rainfall intensity. Assumptions behind the proposed formulas and models should be clearly and explicitly stated.
 - I suggest moving Figure 4 and Tables 4 and 5 to the end of the Results section, within a dedicated subsection. Since the authors apply different methods at each stage of CML processing, it would be clearer to first present the impact of these methods on the output of each individual step, without necessarily referring to accumulated precipitation as a performance indicator. For instance: 1) outlier identification could be evaluated showing the percentage of outliers as a function of the Z-threshold, 2) wet-dry classification yields a binary outcome and is best assessed using a contingency matrix or other suitable performance metrics for binary classification, 3) The effect of different WAA compensation methods can be efficiently illustrated with an example of time series, as done in Figure 6 (which should be simplified to show only one representative time series), 4) the effect of the conversion from attenuation into rainfall intensity is presented in Figure 3, which is a result in my view, hence it can be moved to the Results section. Finally, in a dedicated subsection, the authors should examine the impact of different combinations of methods on a rainfall product, such as the accumulated precipitation during each event.
 - As pointed out by the authors (Table 4 and lines 338-339), the four events have different characteristics. I think it would be good to produce a new figure similar to Figure 4 for case 3 or 4. Please also consider merging Tables 4 and

5 into one, retaining only the column in Table 4 relative to rain gauges. The remaining columns of Table 4 are already reflected in Table 5 through bias or RMSE with respect to rain gauges.

- Part of the content of Section 5 (Discussion) would be more appropriately placed in the Conclusions section. In its current form, Section 5 primarily enumerates the main findings of the analysis and emphasizes the importance of careful data processing, which is more typical of a conclusions section. A discussion section should instead focus on revisiting the underlying assumptions and limitations of the proposed approach, as well as considering alternative methodologies. In addition, the authors could use this section to present supplementary analyses (e.g., a sensitivity analysis) and to compare their results with those reported in similar studies. I recommend revising and rebalancing the final two sections accordingly.
- Datasets. Although the analysis is based on only a few case studies, the study is conducted at the country scale and therefore relies on a large volume of data collected from multiple sensor networks. This raises an important point: measurements from different sensors of the same type (e.g., disdrometers), or from different networks of the same sensor type (e.g., rain gauges), are implicitly treated as comparable in terms of accuracy and reliability. However, this assumption may not necessarily hold and should be explicitly addressed.
- Methods. The authors apply different algorithms at each step of CML data processing, which is a sound approach. However, I have some concerns regarding the selection of these methods and their specific settings:
 - In general, I would recommend using only methods and thresholds that were specifically developed for the same data format, in this case, 15-min MINMAX. For example, Schleiss and Berne developed the rolling standard-deviation method for wet–dry classification using 30-second instantaneous data. It is not guaranteed that this method will perform equally well when applied to 15-min MINMAX data..
 - The chosen thresholds are sometimes reported without justification. For instance, the Z-threshold of 2.5 in Section 3.1, as well as the 2-hour and 0.8 dB thresholds in Section 3.2, are not explained. Additionally, it is unclear which coefficients were used by the authors for WAA compensation when applying the method proposed by Pastorek et al. I suggest including a sensitivity analysis to evaluate how variations in these thresholds affect the results..
 - The comparison in Figure 5 is done averaging the radar pixels within 2.5 km from each rain gauge. A similar rule is used with CMLs. It sounds arbitrary and it should be justified. This choice appears somewhat arbitrary and should be explicitly justified.
- Additional analysis. It's important to understand whether all the 2800 CMLs are equally reliable and contribute equally informative data. I suggest the authors address the following points:
 - Malfunctioning CMLs should be discarded all the way, not only because they record anomalous rainfall values with respect to neighbors, but also because they exhibit anomalous or irregular patterns during dry periods. While the authors mention that some CMLs were discarded, they do not provide details on the criteria or procedure used. They only describe how outliers were

identified. I recommend that the authors detail how malfunctioning CMLs were identified and removed.

- CMLs exhibit markedly different sensitivity to rainfall depending on their frequency–length combination. In my view, including a figure illustrating CML sensitivity—such as the minimum measurable rainfall intensity for different CML types—is essential. Moreover, assessing CML performance across subsets of CMLs would be highly informative. The authors present an analysis dividing the CMLs in three frequency bands in Figure 7 of Section 4.4, which is briefly commented. Case 2 shows problematic performance, with numbers in Table 5 worsening when using RID, likely due to the relatively low accumulated rainfall (as seen in Figure 7). It would be valuable to explore whether focusing on the most sensitive CMLs can reduce this large bias. Additionally, the authors could provide a “rule of thumb” on CML accuracy under different rainfall conditions, which would help interpretation of CML-based rainfall estimates. Finally, apart from outliers, it’s also interesting to investigate whether there are CMLs constantly under/overestimating rainfall.

Comments on specific sections:

Introduction. I think it should be completed with some important information:

- Lines 28 on (limitations of CMLs): an important limitation of several CML datasets is the non-optimum format for rainfall estimation. In particular, the 1 dB signal quantization is a limitation in the presence of light rainfall. In addition, the 15-min MINMAX format is not ideal. And by way, this is the format of the CML dataset available in this work. Please mention these issues + references.
- Since this is a country-wide study, the authors should include a paragraph summarizing previous research that used nationwide CML networks. They should highlight how this study differs from earlier work, for example in terms of climatic region, rainfall characteristics, sensor types, network density, or data processing approaches. This comparison will help contextualize the novelty and contribution of this study.
- Line 43: The term “non-uniform beam-filling” may be somewhat misleading in this context. In radar meteorology, it refers to the fact that an elemental radar resolution volume (“radar cell”) may not be fully filled with precipitation, particularly at long range where the cell’s angular size can exceed 1 km while its radial size is typically around 100 m. For CMLs, this concept does not directly apply because: 1) a CML antenna beam is much narrower than the one of weather radars over practical CML ranges and 2) The received signal loss results from attenuation along the entire propagation path, whereas radar measures backscatter only from particles within the radar cell (and any path attenuation is considered a disturbance). In other words, the “beam” for CMLs corresponds to the full electromagnetic path between transmitter and receiver, while for pulse radars it refers only to the portion of the beam illuminating a specific radar cell.

Section 2

- Table 1, caption: the last statement is unclear and appears unnecessary..
- Line 81: the authors did not state what they mean by “sublink”.

- Line 99: the statement is unclear: the authors are stating that “specific attenuation” (dB/km) and “path-average attenuation” (dB) are the same thing.
- Lines 113–114: The term “independent data points” is unclear. It should be clarified in the context of the specific analysis being performed. For example, measurements taken at similar frequencies along the same CML path are highly correlated and cannot be considered independent. The authors should explain how they define independence and justify its use in the analysis.
- Sec 2.3: rain gauge data. Since these data serve as the ground truth for validating CML results and are collected from multiple networks, some basic considerations regarding their homogeneity are due. For example: Is the measurement resolution (e.g., 0.2 mm) consistent across all networks, or does it vary? Do all networks follow similar procedures for data quality control? These points are important to determine whether the rain gauge data can be considered equally reliable across networks. Additionally, Figure 1 shows that sensors from different networks are sometimes located very close together. It may be useful to compare their daily accumulation values or reference previous studies that have assessed inter-network consistency, to support the assumption of data reliability.
- Line 151: hydrometeor instead of hydrometer
- Figure 3: mm/h is used here, while mm/hr is used somewhere else in the manuscript. Please use consistent abbreviations.

Section 3

- The topic of CML-based rainfall estimation has been studied for some time, and the fundamental steps of data processing are well established. While the authors have selected and tested a few methods on their dataset, many alternative approaches have also been proposed. For the sake of completeness, the manuscript should acknowledge these alternatives or refer to relevant review papers on CML processing methods. For example, only three IEEE journal papers are currently cited, whereas numerous other studies—particularly focusing on CML signal processing—have been published and are pertinent.
- Line 160: The terminology is unclear. By “seconds,” do the authors mean individual samples? This should be clarified, as the raw CML data are not recorded at one-second intervals. Similarly, the term “background block error” is not defined.
- Lines 174–180: The rationale for outlier filtering is unclear. Initially, neighbors are described as being determined based on a long-term (30-year) estimate of spatial rainfall correlation (lines 174–176). However, in the following sentence, the authors state that neighboring CMLs are defined differently: “All CMLs within a circular distance of the CML in question, where the spatial correlation for that date exceeds 0.6, are considered neighboring CMLs.” It is therefore unclear which definition of neighboring CMLs is actually used in Equation (3).
- Section 3.1: A straightforward way to assess the effectiveness of outlier filtering is to compare the number of samples removed by SCI relative to SCI-U. This number likely depends on the chosen Z-threshold. Currently, there is only a brief mention of outliers later in the manuscript (line 299). I recommend that the authors analyze and report how the percentage of removed outliers varies with different Z values, as this would provide important insight into the sensitivity and robustness of the outlier filtering step.

- Section 3.2, last statement: It is unclear how the three 5-min radar samples were combined. Did the authors simply average them and then apply the 0.1 mm/h threshold? Another valid approach would be to take the maximum of the three time slots, since CML data are MINMAX and, in principle, can capture any rainfall occurrence within each 15-min interval (subject to sensitivity limits due to quantization).
- Section 3.3, regarding the Pastorek method for WAA compensation: It is unclear whether the authors used the same power-law coefficients as in Pastorek et al. Those coefficients were derived from CMLs operating in the 24–38 GHz range, which may differ from the frequencies of the links in this study. This point is important and should be explicitly clarified in the manuscript. While calibrating the WAA coefficients using local rain gauge data could improve the results, I understand that this is a substantial task. The authors appropriately note it as future work in the discussion. However, I think the authors can elaborate more on this item (maybe in the discussion section), as there are several WAA compensation methods based on local data and the authors have a very dense rain gauge network available. See, for instance: Valtr et al., 2019, 10.1109/LAWP.2019.2925455; Pu et al., 2020, 10.1109/LAWP.2020.3011463.
- Line 255: minimum or maximum deviation?
- Line 274: Add a reference that reports the DSD variability between maritime and continental environments
- Lines 285-286: it's the same method already referred to in Sec.2.3. Maybe a shorter statement is better "... with the same kriging method used for rain gauge data (see Sec. 3.1)"
- Figure 4a: The radar product shows a few linear stripes oriented SSW to NNW with one particularly evident in central Belgium. Are they artifacts rather than true rainfall structures?

Section 4

- Table 5 presents important results that deserve further investigation. I would expect SCI results to outperform SCI-U. However, in Cases 1 and 4, both bias and relative bias are clearly worse with SCI. In Case 2, the very high relative bias for CMLs is likely due to low accumulated rainfall measured by the gauges combined with the 1-dB quantization, which limits CML sensitivity. I am pretty sure that the results would improve if the analysis were restricted to the subset of CMLs with higher sensitivity, Can the authors elaborate more on these points?
- Figure 5: Averaging radar pixels within 2.5 km of each rain gauge may not be entirely justified unless there is a clear physical reason (e.g. advection of rain cells). One of the main advantages of radar is its high spatial coverage, and in fact each rain gauge falls within a radar pixel. A different approach could be to use the radar pixel containing the rain gauge or, alternatively, a bilinear interpolation of the four surrounding pixels.
- Figure 5: It would be important to check whether the performance of the RAD-QPE product changes when considering only the truly independent rain gauges from the IBGE network. The authors may elaborate on the RAD-QPE performance in BCR, where IBGE and other networks are deployed, comparing RAD-QPE against rain gauges of different networks.

- Figure 5 caption: “The colors denote the different cases represented as shown in the legend”. Are you referring to the legend of previous Figure 1b?
- Table 5: The header for “Case 1” is missing
- Figure 6: it should be modified as it is unclear. The subfigures should clearly show the dry periods before and after the end of the rain event (at least over a few hours), which is not the case for the links 25.4 and 81.2 GHz). In addition, I suggest showing only one example with TSRL, WAA and rain rate displayed over 3 rows.
- Reporting the outcomes of wet-dry classification outcomes is important. I would appreciate a contingency matrix or an histogram of any other performance indicator. I think that interpreting the maps in Figure 4 (final rainfall products), in terms of wet-dry classification (an initial step of CML data processing), is not entirely appropriate (lines 333-345). It can be done only for Figures 4d, e and f (NCI vs RCI vs SCI), where the difference lies in the wet/dry classification algorithm. By the way, these 3 figures are very similar. Moreover, as I wrote in the general comment, It would help adding a figure analogous to Figure 4 for another case and see how panels d-f look like.
- Lines 362-365: comment of WAA compensation. The difficulty in detecting light rainfall is primarily due to the 1 dB data quantization format.. Indeed, subtracting a WAA attenuation in the order of 2 dB or less is problematic, when the baseline itself may contain a 1 dB error. The authors should explicitly remind the reader of this fundamental data limitation when explaining the results, including in the discussion section..
- Lines 387-391: drawing this conclusion based on a single example is not correct in my view. The better agreement may be influenced by other factors, as a direct comparison with rain gauges over a time series depends on the relative distance between instruments, as well as on the characteristics and temporal evolution of the rainfall cell.
- Section 4.4: The authors should include a brief description of the characteristics of the CMLs within the BCR. For example, how many CMLs are included, and what are their mean frequency and link length?
- Figures 8 and 9: The authors conclude that RID is the best CML-based output. Therefore, it may not be necessary to present the other outputs at this stage of the paper.
- Figure 9 :in my view, it does not add significant value to the paper and could be removed.
- Table 7: It is not straightforward to compare Table 7 with Table 5, as both the CML population and the products differ (15-minute rainfall accumulation versus accumulation over an entire event). However, there is a striking difference in Case 2: the large bias observed in Table 5 is reduced to a small bias in Table 7 (RID). Could this be due to the different set of CMLs used?

Section 5 and Conclusions

- Lines 437–438 versus lines 433–435: It is unclear whether the dry-wet classification using radar data truly outperforms the other two methods or is only slightly better. In my view, the most appropriate way to assess this is through a contingency table. Indeed, the authors have not presented the combined cases of outlier elimination, WAA compensation via the Pastorek model, gamma-R estimation by local disdrometers, and the three wet/dry classification methods.

- Lines 456–457: I think the circumstance is not solely related to the exponent being close to 1. At mid-frequency ranges, the dependence of the gamma-R coefficient on microphysical properties is less pronounced.
- As noted in my general comments, the Discussion and Conclusions sections partially overlap and could be better distinguished.

References

- I don't have any comments..