Review of: “Combining commercial microwave links and weather radar for classification of dry snow and rainfall” by Erlend Øydvin et al. (2024)

This paper presents an interesting novel method to classify dry snow and rainfall by merging two precipitation measurement techniques. Previous studies has shown the difficulty to estimate precipitation phase, yet is a relatively important process in the hydrological cycle. The authors perform an elaborate analysis of their novel method and compare this with two more established methods. In general, this is a well-written manuscript. However, some parts would benefit from a revision, in order to improve the overall quality of the manuscript. Below I have stated more general and specific comments, which I hope the authors consider to be constructive.

General:

- Results:
  Parts of the result section are hard to understand due to the combination of complex figures in combination with it being unclear to which subplot is being referred. It would already be helpful if all subplots are labelled with a,b,c… (which might be a requirement by AMT, check their website) in combination with adding in-text references to the specific subplots. Moreover, for the reader, especially those who are less familiar with CMLs, it would be perhaps worth considering to add a basic timeseries (like Fig. 6) to the start of the results section, in order to show the reader how the methods workout in reality and help get an idea about the differences between the CR and RT method. In fact, such a figure already illustrates a lot and prepares the reader for the following more in-depth analysis. Additionally, for some figures it is not clear how you used the data to create the figure (e.g., Fig. 2, see specific comments).

- Road surface conditions:
  I am struggling to see the added value of Sect. 3.5 in which the road friction is related to the classification methods (except the discussion of Fig. 6 which helps understanding the methods). I understand that the new method could also be used to estimate road conditions and I do agree that it is a nice illustration of the potential of these methods, but I think discussing road friction would require a more elaborate analysis. As you write in the discussion, road conditions are not simply a result of dry snow or rainfall, but also previous conditions (e.g., was there already a snow pack, has there been any snow/ice removal, road temperature preceding rain event) while also other precipitation phases should be considered (e.g., what about freezing rain?). Also until Sect 2.6, there hasn’t been any reference of the road conditions, so it came as a bit of a surprise. Based on the introduction, I would have expected that your new method would have been applied to something like a hydrological model.

- Discussion:
  I like that the discussion is relatively concise, but it does not include many references to previous studies on similar topics. I encourage you to include some references to previous studies, so that the reader would be better able to place your results into a wider perspective. For example, how do your findings compare with other studies that use similar methods (mostly for the RT method)? See also the specific comments.

Specific comments:

- L9: perhaps include the location of your study.
- L12: There is no mention of the application to road conditions.
- L54-58: You introduce the dewpoint temperature here, but would it be an idea to include the explanation as to why both are important (L19-33) here? This will help the reader relate the dewpoint temperature to the underlying processes. Or perhaps even referring back to the previously mentioned importance of profiles would already help the reader.
- L73-85: I suggest to include some more studies where they show the influence of various precipitation types on the CML signal intensity in more detail.
  See for example:
  Hansryd, J., Li, Y., Chen, J., & Ligander, P. (2010). Long term path attenuation measurement of the 71–76 GHz band in a 70/80 GHz microwave link. *Proceedings of the Fourth European Conference on Antennas and Propagation*
  van Leth, T. C., Overeem, A., Leijnse, H., & Uijlenhoet, R. (2018). A measurement campaign to assess sources of error in microwave link rainfall estimation. *Atmospheric Measurement Techniques*, *11*(8), 4645-4669. https://doi.org/10.5194/amt-11-4645-2018
- L79-80: Based on this line, I would have expected the paper to focus on applying/elaborating on the methods of Cherkassky and Ostrometzky. I suggest to rephrase this.
- L83: reports → reported, in order to use the same tense as the previous references (very minor comment)
- L90: I would suggest to already mention here that you cannot share the location of the CMLs due to data security reasons.
- L103-104 (and L118-120): I struggle to understand why you do this. As I understand you extend the wet periods in order to make sure that the rain event has also completely passed the disdrometer, is that correct? Yet in your analysis, there is no comparison of rain events but you compare individual time steps and average/summed periods, so I don't see how this period extension exactly works. Or am I missing something here?
- L107: Could you explain briefly on what concept the method of Leijnse et al. (2008) is based? Not every reader will know how their method works.
- L110-114: Could you provide a reference to this radar product for readers who would be interested to know more about this product?
- L113-115: Has the seaclutter and other large peaks been removed by you or was that already done in the data you downloaded?
- L121-136: How well do these disdrometers work? Are there any known biases or uncertainties? Especially because in Sect. 4.2 you refer to their potential uncertainty. This could also be discussed in the discussion.
- L132: Why do you use 8km? Is there a specific reason for this? Is this based on previous studies, or based on your own data? Or is this a common value in CML studies?
- L138: Are there any common biases or uncertainties in this data? If so, please mention to help the reader.
- L147: Marks et al. use 0 C, but that is for the USA right? Why would that be applicable to Norway? As you write in your introduction, these threshold values can have a relatively large range, and thus can have a relatively large influence on your results.
- L155: Perhaps show the equation for MCC. I think not all readers will be familiar with it.
- Fig 2.: The caption is hard to understand here, because you refer to RT and the disdrometers both as references. Also, it is not clear what you mean with "the MCC's are computed for

each CML-disdrometer pairs using 1 month of data". What do you mean with 1 month of data? Just the December data? It almost seems like there is an additional step between the data described in Sect. 2 and this figure. (also minor comment: I think it should be pair instead of pairs)

- Fig. 3: Is this the average of the MCC's for all CML-disdrometer pairs as a function of Td?
- L198-210: Here the addition of in-text references to subplots a,b,etc. would help to guide the reader.
- L211-221: I got confused here because of the text in the parentheses. Why are both the RT and the CR method referred to as CML rain/snow/dry? Wouldn't it be an idea to just refer to them as rain/snow/dry? Or is there a specific reason you included CML?
- L214: to be fully correct the radar observes precipitation instead of rain (very minor comment)
- Fig 5. I understand that you cannot share the exact location of the CMLs because of data security reasons, but is there the possibility to describe the landscape a bit? For example, are there any large elevation differences (i.e., mountiains/fjords) in this area that could for example create orographic precipitation or cause beam blockage of the radar? Additionally, I suggest to add titles to the columns, so that it is immediately clear which method is shown in which column.
- Discussion: Are there any previous studies in which the RT method (or something similar) has been compared with disdrometers (or other precipitation phase observations). If so, I would recommend to include a short discussion on this. How do your results compare to those studies? Are they similar or is it the RT method more difficult in Norway because of for example the elevation differences? Such a discussion would allow to put the performance of the CR method into a wider context.
- L264: the circle of degree Celsius should be in superscript (very minor comment)
- L266: How often does a disdrometer misclassify precipitation phase? I would advise you to include a brief discussion based on previous literature regarding uncertainty when using disdrometers to estimate precipitation phase.
- Fig 6: In the bottom subplot, I suggest to use a different shading color for the diff<-5 mm, because it overlaps with the shadings above. Or perhaps you can even leave this out, as you do not discuss this in the text if I'm correct.
- L270-285: Based on this section, it seems that only wet snow could cause misclassifications. I would suggest to make clear that any form of wet precipitation causes the CML signal to drop. Additionally could it also be that spatial temperature and humidity differences can cause that at the disdrometer location dry snow is falling while somewhere else the precipitation has started melting? I can imagine that this happens when temperatures are in the transition region between rain and snow.
- L288-289: Are there any previous studies that show differences between radar and CML/disdrometer measurements? If so, I suggest to discuss these here.