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Comment on amt-2021-99

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Community comment on "PARAFOG v2.0: a near real-time decision tool to support nowcasting fog formation events at local scales" by Jean-François Ribaud et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-99-CC1>, 2021

The article introduces the tool PARAFOG v2.0, which is used to predict the occurrence of fog at certain locations. It distinguishes between radiation fog and deep, lowered stratus clouds. The authors claim to be able to predict fog with a hit rate of almost 100%, with a false alarm ratio of 10 and 30% respectively.

The methodology is a further development of PARAFOG, with a significantly better hit rate. Overall, the methodology is worth publishing. However, in my opinion, there are some major weaknesses, which I would like to discuss in more detail below. I will limit myself to the things worthy of criticism. Language, style, title, credit to other authors etc. are well done. My main criticism relates to performance analysis. After reading the abstract, the reader is curious how the authors managed to achieve such a good hit rate. However, when reading the article, disillusion sets in.

- The weights of the fuzzy logic algorithm were derived from events at the SIRTAs station. However, the same fog events were also used for performance analysis. There is no differentiation between calibration and validation period.

- Only the results for "high" fog alert are shown. What happens after a low or medium fog alert, is not mentioned. Does every low fog alert lead to a medium and this to a high fog alert? Are there very often false alarms for low or medium fog alerts? We do not know. Nevertheless, it would be interesting for the overall assessment.

- In the abstract and also in the article itself, it is written about minute resolution. However, in the corresponding place it says that the minute resolution is used to make aggregations to 45 minutes. A fog forecast is therefore often only possible in the

aftermath (reanalysis), as the authors also write themselves. The operational mode, is mentioned, but results are not shown. A prediction in a reanalysis, when both the event itself and events from the future have entered the algorithm, has only little value as a quality analysis.

- The exact decision-making process for assigning an alert level to the 45-minute windows is described in the paper. All assessment steps described in 5.a) sound plausible for themselves. However, especially in 5.a)-3, it is not described how exactly the numbers are derived. Why do you choose exactly 45 minutes, exactly 10 alerts and form the gradient over exactly 15 minutes? It can be assumed that the values were determined on the basis of the fog events themselves. Is that the case? If so, on which ones? One can assume, that this further weakens the statement of the performance analysis

- line 382++ The removal of certain events from the performance analysis manipulates the same. If the model cannot handle the situations described, then this is a weakness of the model that can either be named as such or be improved. Filtering out these events afterwards is not a solution.

In summary: The method and also the performance analysis have numerous degrees of freedom. The number of fog events is very small. This potentially leads to an overfitting of the method. In addition, an unknown number of events were filtered out of the performance analysis.

Don't get me wrong: the tool is probably good or even very good. It's just that the validation method used is not suitable to prove this quality.

minor remarks:

-l 143: how are 35 fog events a year fog- prone? Is that compared to other regions in france, or is there a general definition? In my ears, 35 fog events a year does not sound much.

-l 366: "All other alerts occurring outside this period are not considered." Can you give an example? Erasing/not considering alerts in the aftermath is not suitable for a forecasting tool.

The paper may be published after major revision.