Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2021-99

Anonymous Referee #1

Referee 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-RC1, 2021

General Comments:

The authors demonstrate an innovative technique of applying observed vertical gradient of backscatter profiles as well as cloud base height and visibility by way of a fuzzy-logic approach to predict the likelihood of fog onset. The new innovation serves to resolve a previously identified discrepancy in algorithm performance of shallow fog radiation fog identification. The methods are generally clear and well supported by the literature and through equations and figures. The authors provide several thorough, well-documented case study reviews demonstrating implementation followed by additional systematic statistical assessment at multiple stations. The approach appears to have applicability to additional stations globally where radiation and stratus-lowering fog are the predominant fog hazards, subject to some additional work needed to derive appropriate weights for the stratus lowering cases. The manuscript is very well written and the figures are of high quality.

I have listed several specific comments for the authors to address, but most concern suggestions to improve understanding of the methods and some clarifications. I recommend to accept following these minor revisions.

Specific Comments:

L 64-65: Would suggest that fog decay/dissipation is also a major challenge for NWP

L 67-68: Would suggest that the authors broaden this statement from "land-atmosphere" to "surface-atmosphere" interaction as similar dependencies of fog to surface turbulent flux occur over ocean as well as land surfaces. Would also suggest that the authors acknowledge the additional components that yield difficulty in fog prediction for NWP beyond surface-atmosphere interaction, vertical resolution and atmospheric boundary layer physics, namely: cloud microphysics parameterization, radiation parameterization and potentially shallow convection parameterization in coarser models. For limited area models, boundary conditions also help determine advection, another potentially significant contribution to fog presence/absence.

Figure 2: This is a helpful diagram of the algorithm. I am slightly confused, however, why there is no sensitivity to wind speed or vertical shear in the algorithm? For example, there could be a condition of high relative humidity near the ground, but the speed of the ambient horizontal wind may generate sufficient turbulence to prevent fog formation? Have the authors evaluated this as a predictor?

L 207-209: Using a two-hour averaged cloud fraction seems somewhat temporally coarse relative to the 1-min resolution of the algorithm (L 188). Was this choice made because of the coarse frequency of the data source? Did the authors use METAR observations of cloud to make this two-hour average determination?

L 219, L 221: Did the Zurich airport have a hit rate of 90% or 31%? There seems to be a conflict here.

Figure 3: I like how subplots (c) and (d) clearly indicate a clustering of patterns between the hit and miss incidents, complementing the explanation in the text well. In subplot (a), however, I do not understand how approximately 70 percent of all times observed during the verified fog periods for which PFG1 failed (misses) have observed visibilities greater than 1 km? On L 168, the authors define events with visibility less than 1 km within at least three of five blocks of 10 min. Shouldn't there be no more than 40 percent of times showing visibilities exceeding 1 km? If this is not a mistake, then I think there needs to be much more clarification to the reader as to what is displayed in this figure.

Figure 3: Also, it is a bit bewildering how nearly 20 percent of times during these 'missed' verified fog periods yield observed visibility exceeding 6 km. Given the time restrictions imposed by the authors on defining a fog event, how these kind of outliers be explained physically? Are these outliers artifacts of fog having decayed away completely but while time remains inside the 50 min window?

L 277-278: This may be related my (mis) understanding of the alert level concept – but why is the RAD layer thickness discrimination only performed for RAD HIGH alerts? Is it not of interest at any likelihood (low, medium, high) to know whether the potential RAD fog event would be thick or thin? Or is it because the viability of the RG method breaks down under conditions of weaker likelihood?

L 288-289: I assume that the reason there are two separate Aggregation (A) equations to describe CBH lowering and lifting is that the former has some predictability through the time change of visibility and CBH quantities, whereas the latter does not (with respect to visibility and CBH alone)? Did the authors consider any other environmental predictor for CBH lifting? I think it would be helpful for the reader to have some understanding behind the authors' decision here regarding the two separate A equations. Regarding the weights, is the implication that future applications of this approach will require the assessment of a long period of fog climatology to generate these empirical values?

L 357: Section 5a describes important details about the methodology. I think this would be generally better suited earlier in the manuscript, before discussion of case studies. This might help with improving understanding of the 'alert' concept.

L 375 -390: It seems like the 'alarm' concept is really more of a decision to be made by an operational forecasting center given the 'alert' result of the algorithms. It's arguably beyond the scope of the scientific work presented here. It's OK to keep, but this could be one area to trim if adding content elsewhere, I think the quantification of "false alarms" is best done following the conventional contingency table methodology, as the authors do in the ensuing section.

Section 5c: Some lingering confusion here for me about the applicability of the alert in time. A HIGH alert multiple hours (or at least 45 minutes (L 369)) ahead of the first observation of fog is technically false alarm, yes? I think I understand the objective here to demonstrate that more first HIGH alerts happen nearer to the observed start of the fog, though Figure 10 doesn't seem to clearly distinguish the 'good' results from the 'bad'…Perhaps add some kind of marking at 45 minutes?

Technical Corrections:

L 51: add a space between '1' and 'km', same issue on L 138

L 56: again: word choice; perhaps the authors mean 'also' or "as well"?

L 75: Note that some LES models (e.g. Cloud Model 1 : https://www2.mmm.ucar.edu/people/bryan/cm1/) have the ability to incorporate cloud microphysical parameterizations.

L 78: wording: allows (someone/thing) to monitor fog – OR – allows monitoring of fog . . .'allows' here needs to be followed by a noun; This grammar issue happens again on L 93, L 184, L 203, L 251 and elsewhere (I stopped seeking this out after L 251).

L 92: recommend omitting 'true' – the measurements will also have some error

L 94: variable -> variables

L 103-104: The phrase "PFG1 retrieves pre-fog alert levels" is a bit unclear at this point in the text, specifically the "alert levels". There is brief mention in the abstract describing the levels as low, medium and high. Do pre-fog alert levels refer to designation of a relative likelihood of fog based on observed pre-fog conditions that portend a certain type (RAD, STL)? Is it meant to describe likelihood within the next 15 minute period? I would recommend to the authors to clarify this for the reader early on in the text. I did not get a clear context for the concept of the "alert level" early on and it affected my comprehension of the text and results later.

L 163: Please spell out '9' (any number less than 10). Same on line L 168, potentially elsewhere where a single digit is used that is not a measured quantity.

L 184: I think maybe the authors mean "key physical parameters"?

Section 3b: I think a full contingency table (hit, miss, false alarm, correct reject) would be useful for clarification and transparency here. The authors indicate hits and misses in the text here, but also important for more complete algorithm performance assessment are the complementary statistics of false alarms and correct rejections. The authors could choose to coordinate this with Figure 9, simply by expanding analysis to include FA and CR here and the M and CR in Figure 9 (i.e., show all four statistics for all four stations for PFG1 here, then make Figure 9 a complementary figure of comparison).

L 274: Apparent errant period between the sr^{-1} and m^{-1}; this also occurs elsewhere in the text

Figure 5: Could you please shift the colorbar from subplot (d) downward to straddle between (d) and 9e) so that the reader knows to associate the plotted field in (e) with that backscatter colorbar? Otherwise it's not immediately obvious what is being plotted in subplot (e).

L 343-351: Please follow a consistent format on the display of time (e.g. HH:MM UTC preferred over HHhMM UTC)

L 403, 405: These definitions should appear earlier in the text, when the authors first introduce the contingency table terms.

Figure 9: Building on my earlier comment in Section 3b, this figure should illustrate all four standard contingency table statistics (H, M, FA, CR) to provide a well-rounded assessment of the results.