

Atmos. Meas. Tech. Discuss., referee comment RC2
<https://doi.org/10.5194/amt-2020-468-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on amt-2020-468

Anonymous Referee #2

Referee comment on "W-band radar observations for fog forecast improvement: an analysis of model and forward operator errors" by Alistair Bell et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2020-468-RC2>, 2021

The manuscript provides detailed analyses in model and forward operator errors for 94 GHz vertically pointing radar observations, with fog forecast improvement in mind. The manuscript is generally well written; figures are very clear and effective; and the methodology appears appropriate. I thought the results are amazing. The work presented here is necessary for assimilating cloud radar reflectivity, and thus warrants publishing. However, while the title of the manuscript says exactly what is presented, some wording in the manuscript is confusing and misleading, which must be revised.

1) In the end, the authors have NOT yet assimilated radar reflectivity and demonstrated the improvement in fog forecast. Therefore, wording like "after selecting the best background profile, a good agreement was found between observations and simulations" in the abstract is really misleading. What the authors show is the agreement between observations and "selected background profiles", which is not surprising because the "best background profile" was selected using observations as a reference. If I somehow misunderstood the manuscript and if new simulations were indeed performed using the best background profiles, then this leads to an even bigger issue. By definition, the prior is NOT supposed to see the observations beforehand. Therefore, if new simulations were performed, they must be performed for a different case or time period, and I don't see any other cases different from those listed in Table 3. This kind of misleading statements can be found in Section 4.4 and Conclusions as well, which needs to be more precise. Additionally, I think the use of "Innovations" is too strong and not accurate. It is an improvement, not an innovation.

2) Descriptions in Section 3.2 are quite confusing to me, and I am not sure that readers are able to replicate results. What is "fog profile"? "Visibility measurements were averaged over 10-min period", meaning for both observations or simulations or both? How might a choice of 28 km x 28 km domain relate to the sample size of 15248 in total in Table 4? Perhaps a figure or flowchart for this part, or more information in Table 3 (e.g., starting time, duration, statistics thickness, etc.) would help readers to understand better the work involved here.

3) The manuscript will read better if things are defined and clearly stated in a slightly different order. For example, how to define fog thickness in observations AND simulations? The term is introduced in 3.1 (page 7) but is not defined/explained until Page 11. Even so, it is still unclear how exactly it is done and if it is the same for both observations and simulations. Another example is the information on parameter ranges on Page 15. It would be nice to mention that earlier, so readers can connect Fig. 4 and the all exercises/results better.

4) Fig.2: Please explain why (c) only has 20 events? What happened to the other 11 events? If one wants to improve fog forecast, shouldn't we worry more about those 11 events? Can the authors comment if the newly selected background profiles will help improve the forecast for those 11 events? Additionally, the caption is confusing. Do you mean "where the event "occurs/dissipates" later in the observations"? If statistics are derived using simulations minus observations, then it is best to be consistent throughout the manuscript (e.g., fig. 2 and fig.3).

5) Fig. 3: Do you mean fog thickness can be exchanged with fog top height, since the figure title is fog thickness, not top height?