Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-832-RC1, 2021

Comment on acp-2021-832

Robert Tardif (Referee)

Referee comment on "Demistify: a large-eddy simulation (LES) and single-column model (SCM) intercomparison of radiation fog" by Ian Boutle et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-832-RC1, 2021

Review of paper:

Demistify: An LES and SCM intercomparison of radiation fog

By Boutle, Angevine, Bao, Bergot, Bhattacharya, Bott, Ducongé, Forbes, Goecke, Grell, Hill, Igel, Kudzotsa, Lac, Maronga, Romakkaniemi, Schmidli, Schwenkel, Steeneveld, and Vié

Submitted to Atmospheric Chemistry and Physics

Manuscript ACP-2021-832

Recommended disposition: Accept, with minor revisions

General comments:

The manuscript presents the results of an intercomparison of simulations of a radiation fog event from a wide variety of numerical models. Models are composed of Large Eddy Simulation (LES) codes and single column (SCM) versions of numerical weather prediction models. The event is an idealization of a case observed during the LANFEX experiment. Despite the great care in constraining models with the observed surface temperature, large differences are observed in fog properties produced by the different models.

First, it is with great enthusiasm that I learn that this is a first report on coordinated efforts focused on improving the numerical modeling of fog, directed at improving real-world applications of fog forecasts. The manuscript is well-written and provides great, albeit disconcerting, insights into the current state of fog numerical modeling of. A novel aspect of the study consists of comparing LES and SCM models on the same reference case. The manuscript shows in a compelling way the shortcoming of current modeling systems, including the LES where the reliance on the parameterization of turbulent mixing is not as strong as in operational numerical weather prediction models.

Below are some comments and suggestions to help clarify some points and hopefully further improve the manuscript.

Specific comments and recommended revisions:

- Page 3, lines 47, 50-52: I am fine with the simplification of the event and agree that the strategy to force all models with the observed surface temperature allows for a more focused intercomparison. However, since comparison with real observations are presented later as the reference, you should justify that IOP1 did not involve other forcings (advection from drainage flows or other mesoscale circulations, upper tropospheric clouds etc). Please provide a brief description of these conditions and/or clearly point to the prior publication describing IOP1. That would help justify using real observations as the reference in your model intercomparison.
- Page 3, lines 49-50: Which temperature is used to constrain the models? Skin surface temperature? And how was it observed? Please clarify.
- Page 3, lines 69-70, here you could relate the parameters of the idealized simulation to the conditions at the Cardington site.
- Page 5, line 77: Models exhibiting positive fluxes are pointed out in the next section and the importance of behavior in fluxes in simulations are pointed out later in the paper, so this aspect seems more important than conveyed here. I would suggest revising the statement about the limited importance of fluxes (evapotranspiration).
- Page 5, line 86: How do you define "reasonable"? Can you provide a more quantitative statement?
- Page 7, Figure 2: the curve showing the observed conditions does not appear clearly on the figure. I would suggest revising the figure so that the observations "pop out" more visually.
- Page 11, line 146: How does LW cooling lead to an erosion of LWP? LWC in the upper part of the fog layer should increase due to LW cooling. Please provide a more complete but brief explanation related to your statement.
- Page 11, 148, 152: Are the different behavior in the two versions of the Meso-NH model only due to the different aerosol loads, or more from differences in model formulation? Please explain more.
- Page 11, paragraph between lines 149 and 164: by surface heat flux, you are referring to the **sensible** heat flux correct? You should make that clear.
- Page 11, line 163: What is meant by "feed back" here? What interaction(s) are implied here? Please clarify.
- Page 11, line 169, statement about better adaptation to the local environment: is it better or a deficiency in their BL turbulence scheme? Maybe UM does the mixing too

quickly, but no mixing at all seems like a strange behavior under those conditions.

- Page11, line 174: How was the latent heat flux in FV3 restricted? Where is this discussed? This point should be made clearer to the reader.
- Figures 5 and 6, same comment as for figure 2. The curves showing the observed values do not show clearly.
- Page 14, line 178, statement about unrestricted evaporation: I am not sure I understand this. How is it unrestricted? Why should it be restricted to be more realistic? Please clarify this statement.
- Page 14, line 180, statement about overlying cloud cover: This information should be provided earlier in the paper, likely with the discussion of Fig. 1 as the jump in LWP after 0800 UTC is a clear feature of the event that is not related to the behavior of the fog layer.
- Page 15, lines 196-197: The point you are making about using real time observations to evaluate model performance is really interesting. It could be useful here to expand/speculate about the suite of observations required to do this.
- Page 15, Figure 9: I do not see any dashed lines in the SCM plot. Does that mean that aerosol loads have no impact in the dissipation phase of the fog for all of the models? Despite the differences in overnight fog evolution? This is strange and should be discussed.
- Page 15, line 198, statement about fog burning-off versus lifting: I thinks this has a lot to do with fog depth, strength of fog-top inversion and entrainment, and possibly advections of temperature and moisture. You could expand on your statement a little more for greater clarity.
- Page 16, line 207: How is visibility calculated in the different models? Is visibility estimated at the same level for all models? What about the models where the lowest level is much higher than 2 m? Also is the same visibility parameterization used? What are the differences in the relationship uses among the various models? Please clarify these points.
- Page 16, lines 212-213: I am not 100% convinced about explaining the similar behavior by constraining to a common temperature at the surface. Could you provide further evidence to justify the statement? The large differences in observed in LWP and water deposition rates at the surface would suggest significant differences in screen-height LWC, the main variable in determining visibility. I think the reader would like more evidence here.
- Page 18, line 228: the focus of this section is only on microphysics (and rightfully so!), but the title suggests that a wider range of parameterizations are considered. I would suggest adding "Microphysics" at the beginning of the section title.
- Page 20, line 285: I agree that improvements have been made since Bergot’s intercomparison. However, the sample of cases on which models have been tested remains very small. I think this is a point that should be underlined here.
- Page 20, lines 288-289: We should be careful not to suggest the importance of droplet sedimentation as a recent discovery. This has been known for a long time (prior studies by Brown and Roach in 1976, Bergot and Guédalia in 1994 etc.). I think the current lack of inclusion in some of the models rather speak to how the problem of fog modeling has been neglected by the model developers in some of the research and operational centers. Perhaps a statement to that effect would be appropriate? Not absolutely necessary though. But I hope your study will contribute at changing that. Well done.
- Page 21, lines 298-299: Again, perhaps some suggestions of the needed observations would be helpful?
- Great work again! I would love to see more studies like this.