EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-1229-RC2, 2023
© Author(s) 2023. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on egusphere-2022-1229

Anonymous Referee #2

Referee comment on "Investigating multiscale meteorological controls and impact of soil moisture heterogeneity on radiation fog in complex terrain using semi-idealised simulations" by Dongqi Lin et al., EGUsphere,
https://doi.org/10.5194/egusphere-2022-1229-RC2, 2023

Review of https://doi.org/10.5194/egusphere-2022-1229 : «Multiscale meteorological controls and impact of soil moisture heterogeneity on radiation fog in complex terrain" by Lin et al., submitted to EGUsphere

Recommendation : **Rejected**

This work proposes a modelling study on a radiation fog case around Christchurch to investigate first the mechanisms leading to spatial heterogeneities of the fog life cycle and then the impact of soil moisture heterogeneity. The fog simulations are conducted with the PALM LES model already used for fog studies. The first part analyzes the spatial variability of fog duration, near-surface visibility, water vapour and modified Richardson number as an indicator of stability for the study case. The second part deals with a range of spatially heterogeneous soil moisture conditions in order to assess the impact of soil moisture on radiation fog duration.

The topic is very interesting, the objectives are relevant and promising (the introduction is very well written and promising), but the results are very disappointing with an incomplete analysis, and do not present any progress on the topic.

My major concerns are that:

- The numerical choices on the model configuration are not always clearly presented, and are sometimes debatable (see more details below). This does not give confidence in the quality of the results.

- The first part of the results concerning the meteorological controls is confusing, explanations are difficult to understand in relation to the geography of the maps, and we

do not understand the results that emerge. I really found the 4.1 part very difficult to read: it is not always clear which part of the map is commented on.

- The conclusion of this part is that the macrostructure of fog occurrence and distribution is highly controlled by topography and the mesoscale meteorology: although we are not very surprised by this conclusion, it is a bit hasty and we do not learn much.

- There is no comparison to observations, which would allow to know if the simulations are relevant and can be considered as reliable. We understand that authors do not attempt to replicate a real radiation fog event in the simulations. But a direct comparison of the simulation to the observations presented in A1 would have been beneficial to give confidence in the simulations, as well as a comparison of cloud fraction or cloud water content to satellite images.

- For the second part of the results concerning different soil moisture conditions, the test only focuses on the magnitude of soil moisture heterogeneity. But a preliminary test would be necessary, dealing with the sensitivity to the soil moisture itself.

- Results concerning soil moisture heterogeneity do not show a clear impact on the fog duration. Indeed, the authors first underline that "there is no direct evidence to link the changes in soil moisture to the changes in fog duration" but just after they conclude that fog duration is sensitive to changes in soil moisture heterogeneity at microscale: here again, this is not really acceptable.

- The presented fields are rather poor compared to the potential of diagnostics classically available in a LES (vertical temporal evolution, budgets ...). For instance, the fog life cycle is only represented through fog duration 2D maps, without separating initiation and dissipation times.

- In the same way, the paper deals with the impact of soil moisture, but no surface heat flux is presented.

To summarize, I would say that the objectives of the paper are very interesting, but the results and the analysis are not up to the objectives. For a new submission, it would be necessary to produce new fields and diagnostics, and to analyze more deeply the processes driving the fog life cycle and the spatial heterogeneities, and the role of soil humidity.

Model configuration :

The choices of simulation configuration are not clearly enough presented and argued, with sometimes questionable choices:

- It seems that the first vertical level is at 18 m height: it is not suitable at all to fog simulations as we know the necessity to have a first level very close to the ground (max 2 m) (see Tardif, 2007)

- The initialisation of PALM mixes observed atmospheric vertical profiles (for U, V, q) and simulated ones (for rv) from WRF: are they consistent? In other words, are the WRF simulated U, V, q profiles close to the observed ones?

- I suppose that D01 is used without orography for the cyclic boundary conditions: this should be indicated.

- what turbulence scheme is used ? What are its characteristics (order of closure, mixing length, 3D or 1D …) ?

- why is the microphysical scheme off in D02 and D03? Is it a way to prevent cloud advection from D02 to D03 ?

- for the Kessler scheme, what constant value is used for droplet concentration ? Is there droplet settling and if yes, how is it implemented ? Is there droplet deposition ?

References:

Tardif, R. (2007). The impact of vertical resolution in the explicit numerical forecasting of radiation fog: A case study. Pure and Applied Geophysics, 164, 1221-1240.