Manuscript number: egusphere-2022-1229
Title: Investigating multiscale meteorological controls and impacts of soil moisture heterogeneity on radiation fog in complex terrain using semi-idealised simulations
Authors: Dongqi Lin et al.

Reviewer recommendation: ***rejection***

Summary:
The authors present a modeling study focusing on radiation fog in a complex terrain using a multi-nesting model domain employing the models WRF and PALM. The topic is highly relevant, since the different mechanisms interacting in fog are not well understood, in particular those related to surface heterogeneity. However, I found severe flaws in the methodology. In particular, the authors use a large-eddy simulation (LES) code at grid spacings (horizontally 729 – 81 m, vertically 32 – 18 m) that violate the constraints of the technique. The full turbulence spectrum, and the inertial subrange in particular, are unresolved in all simulations presented in the manuscript. All turbulent transport thus is sub-grid scale, which is not allowed in LES modeling as the filtering length must be within the inertial subrange and the bulk part of the turbulence kinetic energy must lie in the resolved scales. The authors did not make any effort to discuss this flaw. As a direct consequence the presented results are not reliable and I did not make the effort to read the discussion of the results in details. In the discussion section they report different results than found by a previous study (Maronga & Bosveld, 2017), both using the same PALM model system. It is likely that these differences are simply due to insufficient grid resolution. As was pointed out by Maronga & Bosveld, the required grid spacing for a typical radiation fog event was 1 m (both vertically and horizontally). The authors here have at best a factor 18 coarser grid spacing. There are more issues, which I detail below. Given this severe flaw, which cannot be removed unless the authors use extremely higher resolution and repeat all analyses, I recommend to reject the paper.

Detailed comments are given below.

Detailed comments by the reviewer:

<u>Major comments</u>

1. A four-step multiple self-nesting of PALM is employed, with grid spacings between 729 m and 81 m horizontally and 162 m to 18 m vertically. As outlined in the general summary: none of these grid spacings are sufficient to resolve the turbulence in a typical environment prone to radiation fog. Even for a convective boundary layer, where the dominant eddies are large, the coarsest grid spacing allowed is around 100 m. The grid spacings used here in the domains D01-D04 are way beyond what is possible to use in an LES model. Under stable conditions, the largest eddies are usually not larger than 10 m, so the grid spacing must be way smaller than that. You either need much higher grid spacings (in LES of radiation fog, grid spacings in literature are in the order of 1 - 4 m horizontally and vertically!), or you need to use a RANS model. By violating the constraints of LES, you are parameterizing all turbulent transport with a subgrid-scale model, which assumes to only treat small-scale isotropic turbulent fluxes. As this is not the case, the transport will be totally wrong. There is no discussion about this in the paper, except one sentence, saying that most of the turbulence is parameterized. Furthermore, no vertical profiles and turbulent quantities are presented. One might suspect this is because they will immediately show these flaws. If the authors cannot correct for these flaws, they probably better go for a RANS model where the grid spacing issue is somewhat less severe (however, to resolve fog layers, small vertical grid spacings are still essential!).

2. You report you are using RRTMG as radiation code, but you also refer to have complex terrain and buildings in the domain. As RRTMG is operating as a single vertical column model, how do you calculate radiative fluxes at non-horizontal surfaces? As far as I know, PALM automatically uses a radiative transfer scheme (RTM) as soon as buildings or complex terrain is found in the domain. RTM,

however, cannot consider clouds and only works for clear-sky conditions. Also, it does not calculate flux divergences, which play a key role in fog development. I found no statement on how this problem is treated in the study.

3. Why is the most simply cloud microphysics available in PALM used? By default, PALM uses a two-moment scheme, which is kind of a standard for years. Is there any reasonable argument for switching to a simplistic Kessler scheme? Furthermore, cloud physics are only allowed in the D04 domain, which means that fog cannot be advected in the D04 domain. Does that make sense? Also, this means that there can be supersaturated air inside the D01-D03 domains. If this air is advected into D04 it will lead to spurious condensation.

4. The authors did a purely idealized study with no relation to any observed fog case. While I would agree that this might not be overly critical, in this particular case it makes me worry. As a combination of the technical flaw, the reader cannot evaluate whether the obtained results are by any means realistic.