# Review of «Multiscale meteorological controls and impact of soil moisture heterogeneity on radiation fog in complex terrain" by Lin et al., submitted to EGUsphere

By: Stephanie Westerhuis, January 2023

The manuscript by Lin at al. presents a modelling study on radiation fog around Christchurch, New Zealand. The authors design a fog case study combining WRF&PALM, remote-sensing observations of atmospheric profiles as well as satellite-derived soil moisture humidity. The study aims at answering two research questions with this experimental setup. 1) Which are the meteorological drivers for radiation fog in the Christchurch area? 2) What is the impact of soil moisture heterogeneity?

The first question is addressed by analysing near-surface water vapour and wind, fog duration and Modified Richardson number (an indicator for stability). The authors describe the spatial evolution of the nocturnal fog event. Fog formed mostly in regions where the regional wind systems did not prevent the formation of a near-surface stable layer.

The experiments targeting the second question comprise a set of simulations for which the soil moisture distribution has been adjusted to represent heterogeneity at different scales. While fog duration varies locally compared to the reference simulation, the spatial pattern does not change drastically and there is no obvious connection between the changes in soil moisture and the changes in fog duration. The authors conclude that the fog event is mostly dominated by larger-scale atmospheric drivers and regional conditions, but that the fog duration itself is determined by the micro-scale heterogeneities.

The authors have carefully designed a testbed for fog in complex terrain which holds the potential for valuable insights on fog processes. However, the analysis of the governing physical processes is incomplete and the conclusion that soil moisture heterogeneity dictates the fog duration lacks convincing arguments. The reader is left with the feeling that the authors had started off with a promising tool at hand and then stopped halfway to the finish line.

## Major items:

- The authors make the strong statement that soil moisture heterogeneity is highly important for fog simulations but are not able to provide any explanation connecting the changes in the soil moisture to fog occurrence. They do not provide any convincing arguments that this is not just "noise". First of all, additional, **more detailed analyses of the driving processes could shed light on causalities** (more details below). Secondly, the **significance level of the changes in fog duration could be estimated** with more data (e.g. running the simulation for multiple days or creating an ensemble with slightly varying initial conditions).

- To provide more context for experiments focusing on complicated features such as heterogeneity **I recommend conducting some simpler sensitivity tests first**. It would be helpful to see the impact of doubling/halving the soil moisture in HOM. In a second step, the differences between HOM and HET should be addressed. And only after all the simpler aspects have been dealt with, the analysis should focus on the impact of heterogeneity itself.

- A **more detailed analysis of the modelled processes is required** for a complete understanding of the fog event.
    - How much soil moisture does actually end up in the atmosphere in different regions and for different model configurations? (Keyword: Accumulated latent heat flux.)
    - Figure 9 shows a significant low stratus cloud. It is only mentioned as "clouds that formed over this area" on L289. Low stratus clouds are often closely related to fog with respect to the driving processes (see the paper by Dupont, 2012, about the lifting of fog into low stratus), but a discussion of it is completely missing. There might be an interesting connection between the topography and/or advected air masses and the low stratus which forms in place of fog.
    - On L302, vertical entrainment is mentioned. In case PALM offers the option to output budget terms, it would be illustrative to see a budget of the resolved and parameterized turbulent processes at the cloud top.

- There is an **imbalance** between aspects mentioned in the abstract/introduction/methods (heterogeneous soil moisture, land use, topography) and those essentially presented in the Results. These comprise only one figure (Fig. 10) dedicated to the soil moisture heterogeneity, five figures (Figs. 5-9) elucidating the fog event evolution of the reference simulation, but e.g. the impact of land use is not discussed at all despite being mentioned multiple times in the previous sections. The text is lengthy but at the same time misses a lot of explanations. A revised version of the manuscript should **only include what is relevant for the specified research questions**.
    - E.g. there is no need to highlight impervious areas in Figure 1d when the study is not touching upon this subject.
    - E.g. L131: The sentence stating that "ground-based observations of visibility are necessary to conduct fog simulations" is first of all wrong. Observations are at most "useful" for the validation of such simulations. However, fog simulations could also be compared to surface observations of relative humidity and incoming LW radiation or with remote sensing devices which are frequently employed at airports such as ceilometers, and satellite imagery. Anyways, information about the feasibility of a comparison to observations is obsolete when the simulations are not connected to a specific real case anymore. Be clearer: Either the simulations are idealized, hence eliminate all of those passages, or argue why comparison with observations is justified and then also present this.
    - Another example is the explanation about two-moment microphysics which are not used after all (L213-215).

    - Furthermore, the Introduction and Section on Data Description evoke expectations to learn about the impact of heterogeneous land use which is not at all addressed in the Results. Conduct and present additional experiments or eliminate those parts.
    - Figure 6 includes a shading indicating the land use, while this is not discussed but instead hampers the interpretation of the displayed visibility.

- The **experimental setup is partially unclear**:
    - How does the domain averaged soil moisture compare for the different heterogeneous setups to the homogeneous setup?
    - How are the soil moisture profiles (i.e. the soil moisture below the first soil layer) adapted compared to HOM? → L193/4: Please explain what a "3D profile of soil moisture" is and how this is used. Please also explain how "an adjustment corresponding to the Landsat 8 observations" is conducted in detail.
    - What about clouds in general at initialization and throughout the simulation?

- The **storytelling could be improved** if Methods and Results were not separated strictly. I would prefer reading about the setup of the reference simulation first, followed by the presentation of the event. Next, the changes made to the soil moisture were to be introduced and the results thereto follow afterwards.

- Figures should be refined: Some of them contain **features which are too small to interpret** (e.g. wind arrows in Fig. 9), many comprise axes labelling which is very small and would be easier to read if the employed units were km instead of m. Furthermore, some of the figure captions could be improved by adding more relevant information (e.g. which configuration does it refer to, e.g. Fig. 5) and removing irrelevant information (mentioning of impervious areas in Fig. 1).

- Personally, I have never made good experiences with calculating the horizontal visibility based on the liquid water content with the mentioned formula (1). Spatial plots of visibility often reproduce the distribution of the liquid water content. I see the benefit of being able to assign a "fog duration" to grid cells but for analyses of physical processes I have always found it **more revealing to analyse the prognostic model variables (liquid water content) directly**. Additionally, it is unclear whether the visibility is only calculated for the lowest model level or for a couple of levels close to the surface (L215).

## Detailed comments:

As I expect the text to be revised substantially I have refrained from listing syntax and typing errors. The comments below aim to add onto the previously mentioned major items.

- Title: Specify that the study is about "modelling" and preferably also include the information that the experiments are "semi-idealised".
- Abstract, L1: The terms "fog development/persistence/formation" seem to be used imprecisely throughout the document. "Fog development" is somewhat vague, existing literature often

employs “fog formation” for the first stage of the fog life cycle or refers to the “fog life cycle” for all stages from formation until dissipation. E.g. L37: Do you mean the formation phase or the development after the fog has formed? L27: Is the intention indeed to highlight the importance of land surface characteristics on the “fog formation” instead of overall “fog occurrence”? I suggest to adopt the wording as used in the Introduction of the publication of Bergot and Lestringant (2019).

- Abstract, L6: Do not include links in the abstract.
- Abstract, L15: Readability would be improved if the units were chosen differently. I prefer reading “10 - 200km” instead of “10^4 – 2x10^5m. This holds true for the whole document, including the figures, which always use meters while axes in kilometers would be easier to interpret.
- L 26: Please do not cite a 30-year old publication to state that “land surface dynamics are the MOST IMPORTANT” factor. I would prefer to read something along the lines of “Already Duynkerke (1991) had identified land surface characteristics as one of the driving factors for fog”.
- L 47: Suggestion to rephrase to “in a region in north-eastern France”.
- L53: Could you add a reference to a publication addressing “changes in fog duration”?
- L64: Eliminate repetitions of non-domain-specific terms in consecutive sentences: “Aforementioned studies” appears again on L67. Please also keep an eye on this elsewhere in the text when revising.
- L68: “…did not consider effects of heterogeneity in land use or soil moisture” could be misinterpreted as “those studies are based on experiments with homogeneous land use and soil moisture”. Could you please clarify that those studies did just not conduct sensitivity experiments specifically targeting this question?
- L69: Depending on how the additional work to provide a more in-depth analysis is addressed, this sentences could be re-phrased to “Therefore, the COMBINED effect of complex orography and heterogeneous soil moisture on…”.
- L95: I suggest to prominently state that this is a “semi-idealised” numerical simulation and which makes use of “nesting”.
- L175: Improve wording of “neither too high nor too low”.
- L205: How exactly does the configuration of the outer domain “maintain a stable boundary layer”?
- L225: Specify: What is the soil moisture INDEX used for and how does it relate to the soil moisture values in kg/kg which are finally used in the experiments.
- It is unclear what is meant exactly with “fog onset”. Does this time vary for each grid cell? If yes, I would be interested to see a spatial plot showing the timing of fog onset.
- L264: “greater qv” is not a proper expression. Greater than what exactly?
- L270: A “horizontal cross-section of the first model level” is just “the spatial distribution of xyz on the first model level”.
- L271: The fog life cycle is strongly dependent on the incoming solar radiation (or lack thereof). I would prefer to have all mentioned time stamps in local time and a clear indication of sunrise and sunset times in the text as well as in the figures comprising a time axis.
- L276: Instead of indicating the location of the cross-section in meters I would prefer to see a vertical line drawn on e.g. Figure 5.

- L297: The storytelling of the reference simulation could be improved by focusing on different areas one after another.
- L307: Figure 9 is not a good reference to illustrate the surface elevation.
- L308: What is the causality between the “convergence of the southeasterly and northerly flow” and the “development of another stable layer”?
- L315: Does “in the meantime” refer to the period after sunrise?
- L323: On Figure 10 it is not evident whether there are large differences regarding the spatial distribution of fog OCCURRENCE between HOM and the HET experiments. It is left to the reader to determine the fog duration for a specific area from 10a and then subtract the delta in fog duration indicated in the other subfigures. An additional figure showing 3 categories (fog in both setups, fog only in HOM, fog only in HETxx) would be useful.
- L385: Specify: What would you expect to learn from a simulation at higher resolution with respect to soil moisture heterogeneity?

## References


Bergot, T., & Lestringant, R. (2019). On the predictability of radiation fog formation in a mesoscale model: A case study in heterogeneous terrain. *Atmosphere*, *10*(4), 165.

Dupont, J. C., Haeffelin, M., Protat, A., Bouniol, D., Boyouk, N., & Morille, Y. (2012). Stratus–fog formation and dissipation: a 6-day case study. *Boundary-layer meteorology*, *143*(1), 207-225.

Duynkerke, P. G. (1991). Radiation fog: A comparison of model simulation with detailed observations. *Monthly Weather Review*, *119*(2), 324-341.