Author's response for egusphere-2022-1229:

Investigating multiscale meteorological controls and impacts of soil moisture heterogeneity on radiation fog in complex terrain using semi-idealised simulations

The authors thank the reviewers and the editor for their time and careful consideration given to this manuscript. We agree with the major concerns from Reviewer #3 regarding the configuration of Large Eddy Simulations (LES), while we would like to further clarify and emphasise here that we only performed high-resolution mesoscale simulations for the experiments presented in the manuscript. On reflection, it is clear that our simulations are actually not, and should not be called, LES. We have therefore now followed the terminology described by *Cuxart (2015) – '**When Can a High-Resolution Simulation Over Complex Terrain be Called LES?'*** We have acknowledged in the manuscript that, with the grid spacing we have used in our simulations, we did not resolve large eddies and hence our simulations should not be called LES. In addition, there are several practical reasons why we are not able to use Reynolds-averaged (Navier–Stokes) (RANS) simulation models as suggested by the editor, and we have outlined these in detail below in response to Reviewer #3's major comment 1.

It should be noted that the main focus of this manuscript is not to replicate a fog event accurately, resolve turbulence, or investigate the impact of turbulence in fog. To properly resolve large eddies in a stable boundary layer, the grid spacing needs to be finer than at least 4 m, which is extremely computationally expensive considering the domain size of the simulations. The purpose of our configuration is to carry out experiments on soil moisture heterogeneity quickly, with the heterogeneity of terrain and other surface structures included in the simulation. It is clear to us that with a grid spacing finer than 4 m, such experiments on soil moisture heterogeneity are not computationally feasible.

We understand the limitations of our approach, although it is clear that we did not explain them in sufficient detail in the previous versions of the manuscript. This appears to have diverted discussion of the manuscript to focus on whether proper LES configuration was used. We have therefore added additional description to clarify that our simulations are high-resolution mesoscale simulations rather than LES. We have also added more discussion regarding the limitations of our approach.

The reviewer's comments have been listed below in *black italics* and responded to individually in *blue italics*. Revised sentences are in *red italics.*

Reply to Stephanie Westerhuis:

1. *L18 and 38: As the model's horizontal resolution is on the order of several decametres, I would rather specify the microscale as "on the order of 100m to 1km".*
   We have changed "(1 cm to 1 km)" in L18 to "on the order of 100 m to 1 km" as this is what this study has shown. We did not change this for L38 as it is for the broader concept of microscale in the introduction.

2. *L159: "This means THAT…"*
   This has been corrected.

3. *L178: It is not self-explanatory what is meant by a "3D profile in west-east, south-north, and the vertical direction". I would replace the term "profile", as this in itself is associated with an extract of a quantity along the vertical dimension. Do you not just mean "soil moisture which varies both in the horizontal and vertical"?*
   Yes, this means "soil moisture which varies both in the horizontal and vertical dimensions". We have replaced the term "profile" with "field" as follows:

   … a 3D field of soil moisture (west-east, south-north, and eight vertical soil layers) …

4. *L193 and 198: Repetition of "bulk cloud model only enabled in D04."*
   We have removed "The bulk cloud model was only applied in D04." in L198.

5. *L199: "Cloud water sedimentation IS based on…"*
   We have revised this as follows:

   Cloud water sedimentation based on Ackerman et al. (2009) is enabled.

6. *L213: Improve wording of "fog event is the most significant".*
   We have revised this as follows:

   We focus on four sites in D04 where fog events are the most recognisable and have a relatively long duration.

7. *L223: Swap ending of sentences: "..cross sections. The sunset time…on Day 1 and sunrise…"*
   This has been corrected.

8. *L227: Either "temperature decreases" or "air cools"*
   We have revised this as follows:

   … where the temperature decreased faster …

9. *L237: no "9" in "03900 LST".*
   This has been corrected.

10. *L253: "High qv was PRESENT…"*
    This has been corrected.

11. *L254 – 263: I find the explanation that clouds instead of fog formed very convincing and would replace "This is suspected to be due to" with "This is likely caused by". I would however specify the clouds to be "low stratus" which is often related to fog occurrence. The physical drivers could be more precise: "The layer of low stratus reflecting the outgoing LW radiation results in a reduced surface cooling and.." E.g. on the Swiss Plateau, the transition from fog to low stratus (and back) is often associated to increase (decrease) in wind speed. Is this also the case here?*
    We have revised this as follows:

    This is likely caused by the layer of low stratus that formed over this area at around 400 m above the ground, while the southern part of the simulation domain was under clear sky conditions (see Figure 6i). The formation of the low stratus layer is considered to result from high qv at around 400 m at model initialisation (Figure 2c), with high values of qv at approximately 400 m also visible in Figures 6e-h. The layer of low stratus absorbed the outgoing longwave radiation from the surface and re-radiated it to the surface over the northern section of D04, which is likely to have led to reduced surface cooling and subsequently a less stable near-surface layer over this area.

Regarding the transition between fog and low stratus, we do think that higher wind speed is responsible for the formation of low stratus instead of fog in the north part of the domain. The processes involved in this transition are interesting. However, as this is out of the scope of this study, we do not discuss it further. Future work may be carried out to investigate this topic in greater depth.

12. *L263: Suggestion: "Besides the low stratus clouds the simulation featured clear-sky conditions."*
    This has been corrected.

13. *L331: T missing in "HET12p"*
    This has been corrected.

14. *Figure 11: Specify not only the black solid line but also the grey shading in the first two columns.*
    We have added a description for the grey dots in Figure 11 as follows:

    In the first two columns, the grey dots are obtained from all grid points at 1 km and 3 km scales, respectively.

15. *L389: "Develop INto"*
    This has been corrected.

16. *L408 and 409: The reasoning is not logically consistent: a) High LHF = high QL b) High soil moisture = high LHF c) High soil moisture =/ high QL -> Probably a) should be phrased differently.*
    As shown in Figure 12a, we can see that a) high LHF = high QL and b) high soil moisture = high LHF for fixed values of QL, but c) high soil moisture ≠ high QL, which leads to the discussion in L445 that advection of water vapour in the atmosphere plays a more important role compared to evaporation from the soil. We understand that the logic here could be confusing and have rephrased this as follows:

    As shown in Figure 12a, at HAP, higher LHF_{Acc} is generally associated with higher ql_{Acc}. In addition, although higher soil moisture shows a positive correlation with LHF_{Acc} for fixed values of ql_{Acc}, an increase in soil moisture does not coincide with a higher ql_{Acc}.

17. *L420: I suggest to replace "fact" with "conclusion/reasoning/hypothesis".*
    We have replaced "fact" with "conclusion".

18. *L440: At least for the two sites which feature classic radiation fog, Double and Half seemed to consistently increase and decrease certain aspects.*
    We agree with the reviewer and have added the following discussion:

    Nevertheless, the changes in fog occurrence, formation time, and dissipation time do not show a linear correlation with changes in soil moisture across the domain. At HAP and PTH, where fog reflected more localised processes, fog duration decreased more than 20 minutes when soil moisture was halved. However, this relationship is less clear for SWC and WMR.

19. *L464: "at THE microscale"*
    This has been corrected.

20. *L465: "The occurrence of overlying clouds"*
    This has been corrected.

21. *L476: "thorough"*
    We have revised this as follows:

    … requires further research through, for example thorough additional case studies, as only one case study is presented here.

Reply to Anonymous Referee #3:
Major comments:

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!).*

As we mentioned in the general summary above, our simulations are not LES, and should be called "high-resolution mesoscale simulations". After inspecting the model results, the maximum ratio of resolved TKE in our simulations is only around 35%, which confirms the reviewer's concerns and affirms the need to call the experiments high resolution mesoscale simulations. We did not show vertical profiles and turbulent quantities because the main focus of this study is the impact of spatial heterogeneity in soil moisture on fog in a complex environment. If one aims to simulate radiation fog and understand the impacts at the microscale with LES, then we do agree that the grid spacing should be finer.

There are several reasons why we were unable to use a RANS model:
1) The RANS code in PALM is not fully implemented for the application of fog and several users including the PALM developers have reported that RANS in PALM does not improve the computation time (based on personal communication and PALM ticket system, e.g. https://palm.muk.uni-hannover.de/trac/ticket/1444; as login credentials are required to access PALM ticket system, a screenshot is attached below). In this study, we aim to have multiple experiments without excessively using computational resources. Therefore, using RANS in PALM is not useable in our study.

Please note that the way the TKE-e and TKE-l closures are implemented, the simulation will most likely not be faster compared to the LES approach. Certain optimizations are not implemented in the RANS mode so far that would allow for a larger time step and, hence, a faster integration with regards to computing time.

There is no fixed plan when these turbulence closures will be further developed and optimized, yet. So, it might take some time until this mode offers a considerably faster alternative to the LES mode.

2) An alternative suggestion could be to use other RANS models such as WRF. However, WRF does not include and resolve surface heterogeneity well compared to PALM. PALM offers more features in its land surface modules, which has enabled us to conduct the simulations we have presented. In addition, previous studies such as Cui et al. (2019)

compared WRF with WRF-LES at the same grid spacing in radiation fog and showed that WRF-LES has better performance with the advent of resolving fluctuations in the state parameters that subsequently reduced the mean bias when compared to observations. Please note that in Cui et al. (2019) their finest grid spacing is 333.33 m, which should also not be called LES. Despite this limitation, their simulation results still show good agreement with observations, demonstrating that using LES model at a coarse grid spacing (i.e., as a high-resolution mesoscale model) still has practical value.

This is why we decided to conduct high-resolution mesoscale simulations using the PALM LES code. We discuss the reason why we have chosen this approach in the manuscript (L56-90) as follows:

Considering the high computational cost, the optimal approach is to carry out high-resolution mesoscale simulations (Cuxart, 2015) at sub-km grid spacing. The surface and topographic heterogeneities can be partially resolved in such high-resolution mesoscale simulations, and consequently the dynamical processes and spatial variability of fog can be captured (Vosper et al., 2013, 2014). This study therefore aims to investigate the impact of soil moisture on radiation fog duration using high-resolution mesoscale simulations for Christchurch.

We agree with the reviewer that we did not provide sufficiently detailed discussion on the limitations of our approach, which could have generated confusion among the LES community. We have further clarified the terminology relating to our approach in Section 3 as follows:

Therefore, following the terminology discussed by Cuxart (2015), our simulations are high-resolution mesoscale simulations rather than LES, despite using the LES model PALM.

In addition, we have added discussion regarding this limitation as follows:

Due to the difficulty in spatial analysis and the significant computational cost, we only carried out simulations at a horizontal grid spacing of 81 m. In our high-resolution mesoscale simulations, most of the eddies are not resolved. With PALM's high scalability at microscale, the grid spacing of the simulations should typically be finer so that turbulence structures can be better resolved and captured.

Furthermore, the reviewer mentioned in their general summary that:
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 difference between this study and Maronga and Bosveld (2017) has been discussed in Section 5.3. We believe the main reason for the difference is that our simulations include different types of fog, while Maronga and Bosveld (2017) only simulated radiation fog. As reviewer Dr Stefanie Westerhuis mentioned in her comment #18, for the two sites that experienced classic radiation fog, doubled and halved soil moisture do show consistent changes in some fog characteristics. For example, as shown in Figure 10a, at HAP, Half did not lead to significant changes in formation time, but dissipation time was affected. This agrees with Maronga and Bosveld (2017). We understand that the study of Maronga and Bosveld (2017) showed that to replicate the vertical structure and duration of a historic fog event, the grid spacing should be as fine as 1 m. It should be noted that they also have pointed out that this could be extremely computationally expensive. In this manuscript, we do not aim to forecast fog or to replicate any historic fog events. As we did not discuss the

difference in grid spacing between the two studies, we have added the discussion as follows:

It should be noted that this study uses a coarse grid spacing (horizontal grid spacing of 81 m) compared to Maronga and Bosveld (2017) (horizontal grid spacing finer than 4 m). Our simulations did not resolve large eddies and hence the turbulence transport could be expected to differ significantly. However, running simulations over an area of approximately 17.5 km × 17.5 km with grid spacing finer than 4 m is not computationally feasible. Future work should therefore be carried out using a finer grid spacing when suitable computation resources become available.

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.*

As we have a plant canopy and an urban canopy with the microphysics module switched on, the RTM in PALM is required to be switched off. Details can be found in PALM documentation in this link:
https://palm.muk.uni-hannover.de/trac/wiki/doc/app/radiation_parameters#radiation_interactions_on
which presents the following warning:

Warning: `radiation_interactions_on = .T.` is not allowed, in case the bulk cloud model (BCM) is used together with the urban- and/or land-surface model (USM and/or LSM) and the radiation model.

Due to the aforementioned model limitation we have to neglect the radiative transfer processes within the canopy, including shade and reflections which RTM usually perform. Therefore, no 3D RTM was used in our simulations. We understand that this means that 3D features, such as shadows, are not included in the simulations. However, as the reviewer stated, RTM neglects the absorption, scattering and thermal emission within the air mass and hence is not suitable to use for fog simulations. In addition, in PALM, RTM is only applied from the lowest model level to the top of the highest surface obstacle (such as buildings or plant canopy), and above that height, the atmosphere uses a radiation model like RRTMG to calculate radiation. If RTM is switched on with RRTMG, then the exchange between surface radiation and the atmosphere is controlled by RTM. The surface radiation passed from RTM to RRTMG uses averages and therefore RRTMG does not see any surface heterogeneity, which could be a potential downside for studies that focus on the effects of heterogeneous features on the surface. For more details, please refer to Krč et al. (2021) and Salim et al. (2022).

In our simulations, we have utilised the RRTMG with the RTM deactivated. This allows the calculation of radiation to include the heterogeneous surface properties. Although this approach results in the absence of shadows in the simulations during daytime, this limitation is consistently applied across all our simulations. Furthermore, our study exclusively focuses on the nocturnal development of fog, during which the presence of shadows is not relevant.

We also would like to highlight that the RRTMG employed in our study is consistent with the methodology utilised in the PALM fog studies conducted by Maronga and Bosveld (2017) and Schwenkel and Maronga (2019). As these studies specifically focused on fog simulations without incorporating plant canopy or urban surfaces, the use of an RTM was not necessary. Nevertheless, their findings demonstrated that RRTMG is suitable for

simulating fog. While our primary objective is not to precisely replicate all processes involved in the fog lifecycle, we acknowledge that the choice of radiation schemes in our research may present limitations. In response to this concern, we have added a detailed description of the radiation model in Section 3 of the manuscript as follows:

Due to the inclusion of the plant canopy model and bulk cloud model in PALM, the three-dimensional Radiative Transfer Model (RTM; Krč et al., 2021) was switched off. The surface radiation transfer is then directly computed by the RRTMG model embedded in PALM. This configuration of the radiation model in PALM fog simulations is similar to those described in Maronga and Bosveld (2017) and Schwenkel and Maronga (2019).

And we have added discussion on this limitation in Section 6 as follows:

Furthermore, our simulations only used the RRTMG scheme in PALM for computation of radiation and did not include the three-dimensional RTM. As discussed in Salim et al. (2022), choices of the radiative transfer processes included in PALM can change the flow field considerably in an urban environment. The RTM applied in PALM neglects the absorption, scattering, and thermal emission by air masses (Krč et al., 2021; Salim et al., 2022), and hence its application is limited in case of fog simulations. A recent ongoing development regarding the three-dimensional radiative transfer in PALM is the implementation and integration of the TenStream radiative transfer model. TenStream is capable to consider the effects of three-dimensional radiative transfer on the atmospheric heating rates or dynamic heterogeneities such as moving clouds or fog (Jakub and Mayer, 2015, 2016). With such development and implementation of the radiation model in PALM, TenStream should be considered and utilised for future fog simulations.

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.*

As we mentioned in Line 198 of the manuscript, "Two-moment schemes are not compatible with the plant canopy model of PALM" (see also PALM error message here https://palm.muk.uni-hannover.de/trac/wiki/doc/app/errmsg#PA0360). In our simulations, we included plant canopy and hence were not able to use two-moment schemes. Schwenkel and Maronga (2019) indicated that the general fog life cycle simulated using a one-moment scheme is acceptable. However, ideally, we do agree that one would aim to use a two-moment scheme, which performs better in simulating fog as stated in e.g., Schwenkel and Maronga (2019). We have added discussion on this limitation as follows:

Due to the inclusion of the plant canopy, only a one-moment microphysical scheme was used in this study. Although, as described in Schwenkel and Maronga (2019), the use of a one-moment scheme does not affect the general structure of fog life cycle, future work may aim to apply two-moment microphysical schemes for a more realistic representation of the microphysics.

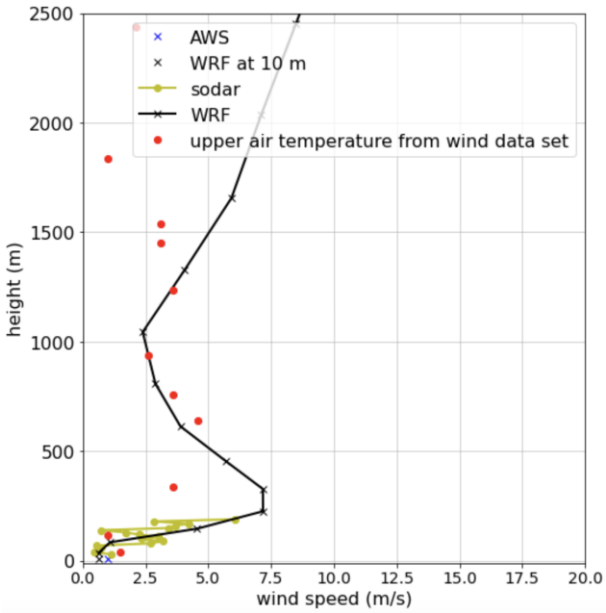
We only enabled microphysics in the D04 domain because we wanted to simplify the processes involved in the fog events that we simulated. We do not aim to forecast or replicate fog events. Rather, we aim to investigate the impact of soil moisture heterogeneity on fog at the surface. The processes involved in the current simulations are already complex to analyse, as discussed in Section 5. If the microphysics were switched

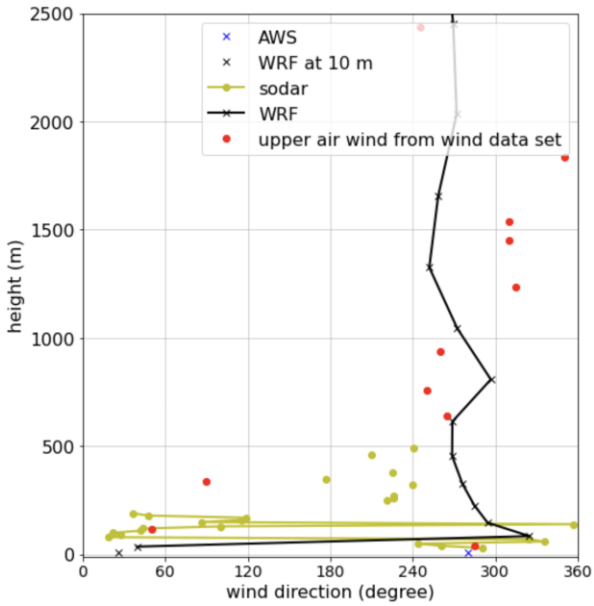
on for the parent domains, analysis of the processes related to soil moisture heterogeneity would be very difficult. We have added the following clarification in Section 3:

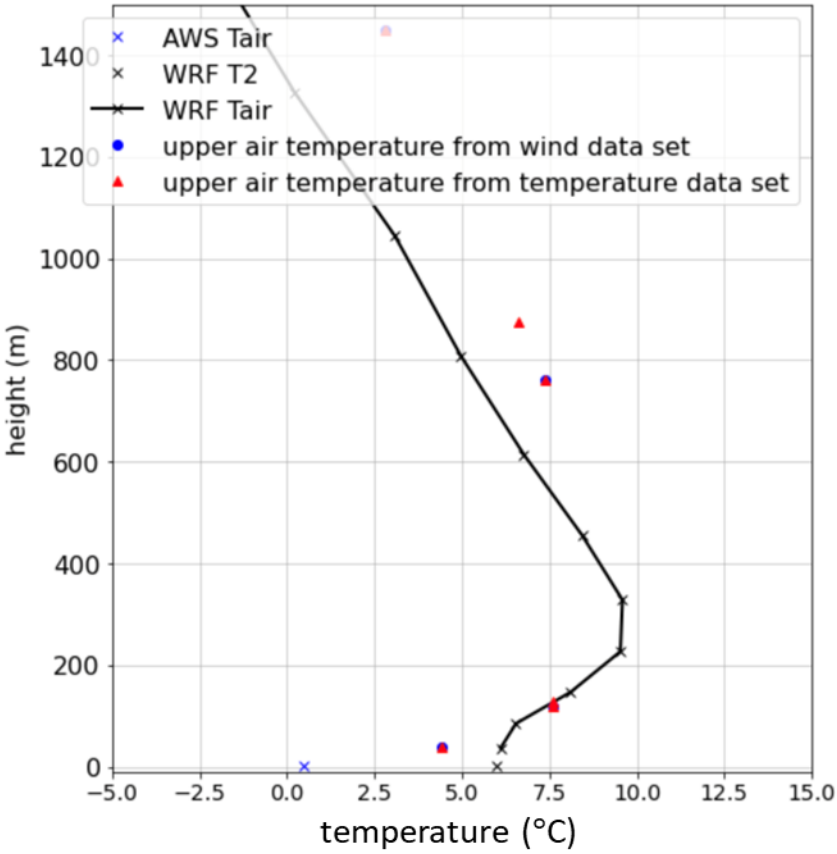
The bulk cloud model was switched off for domains D01, D02, and D03, to simplify the processes involved in the simulated fog.

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.*

We would like to point out that we carried out our simulations using data obtained from an observed fog case and WRF simulations as described in Section 2.2 (Lines 135-141), Section 3 (Line 169-172), and Appendix A. Furthermore, as we stated in our previous response to Reviewer #2, the initialisation profiles were carefully selected so that radiation fog can be simulated in PALM. We carried out a comparison between WRF and the observations regarding the vertical profiles of winds and temperatures as shown in the figures below:







In these figures, 'sodar' indicates observations obtained from the Sound Detection and Ranging (SoDAR) wind profiler deployed at the airport, 'AWS' indicates data obtained from the automatic weather station (AWS) located near the sodar (temperature at 1.25 m and wind speed and direction at 10 m), 'T2' is the air temperature at 2 m simulated by WRF, and 'WRF Tair' indicates WRF air temperature at various heights. The upper air observations were obtained from the national climate database (CliFlo; https://cliflo.niwa.co.nz/) operated by the National Institute of Water and Atmospheric Research (NIWA) include two sets of data: one named as temperature data set, and one named as wind data set. These upper air measurements were recorded by sensors on aircraft arriving at and departing from Christchurch airport. The CliFlo temperature data set includes vertical profiles of temperature only, while the CliFlo wind data set includes vertical profiles of wind in addition to temperature. In general, WRF shows good agreement with all the observations.

We agree with the reviewer that the simulations presented in this study are to some extent idealised. We did not present any validation of the simulations against observations as we did not aim to replicate the fog event accurately. However, we believe that this does not rule out the value of our study. When comparing the simulated results to observations, the PALM simulations show better performance than the WRF simulations. Figures AC1-AC4 below show the comparison between observations and WRF and PALM simulations. The observational data were obtained from the AWS and the sodar located at Christchurch International Airport. As shown in Figure AC1, WRF consistently overestimates temperature. PALM follows the trend presented in WRF, but exhibits a smaller temperature bias. The time series of 10 m wind speed shown in Figure AC2 also show agreement between WRF, PALM, and the observations. At 50 m above ground level (AGL), WRF highly overestimated the wind speed towards the end of the simulation period while PALM still shows quite good agreement with sodar observations (Figure AC3). In addition, PALM-simulated wind speed anomalies agree with the sodar observed wind speed anomalies at 30 m AGL as shown in Figure AC4, despite an underestimation of wind anomalies in PALM. The wind anomalies are calculated by subtracting the instantaneous wind speed from the hourly averaged wind speed for each

hour during the period between 2000 LST 5th August 2001 and 0000 7th August 2001. The temporal frequency of PALM output is 1 minute while the sodar data were obtained every 10 minutes, which could be one of the explanations regarding the underestimation in PALM. The wind anomaly statistics of WRF are not shown here because the WRF simulation we have used here only has hourly output and only the average properties of airflows are presented in WRF simulations with the RANS mode.

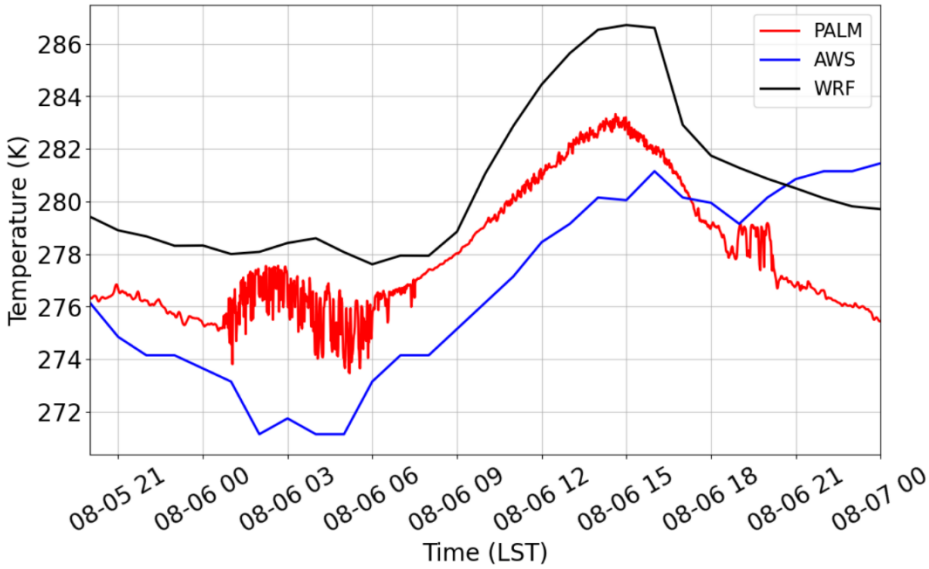


Figure AC1: Temperature at 2 m above ground level (AGL) observed at the AWS located at Christchurch airport and obtained from WRF and PALM.

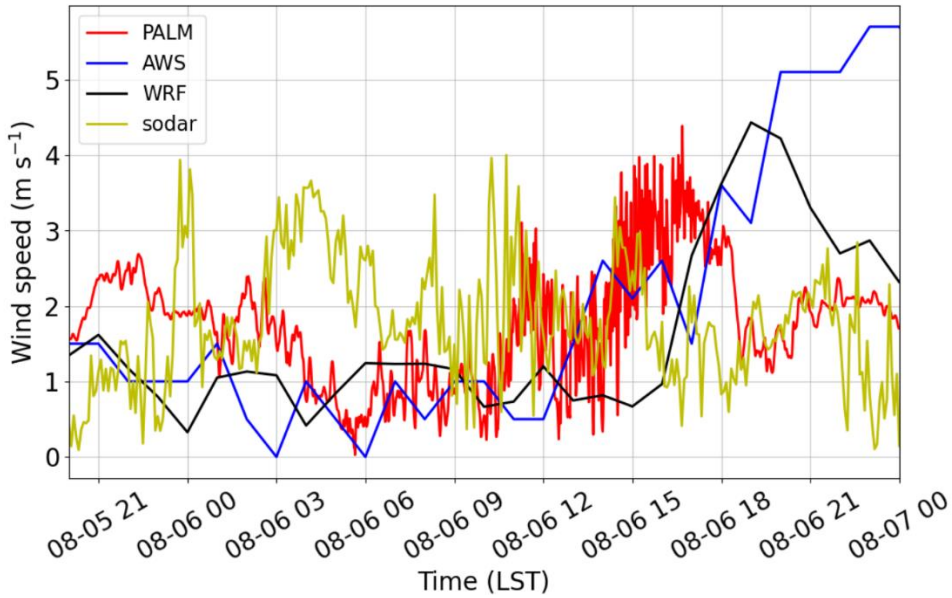


Figure AC2: Wind speed observed at 10 m AGL obtained from the AWS located at Christchurch airport, observed at 30 m AGL from the sodar located at Christchurch airport, and obtained from WRF (10 m AGL) and PALM (9 m AGL).

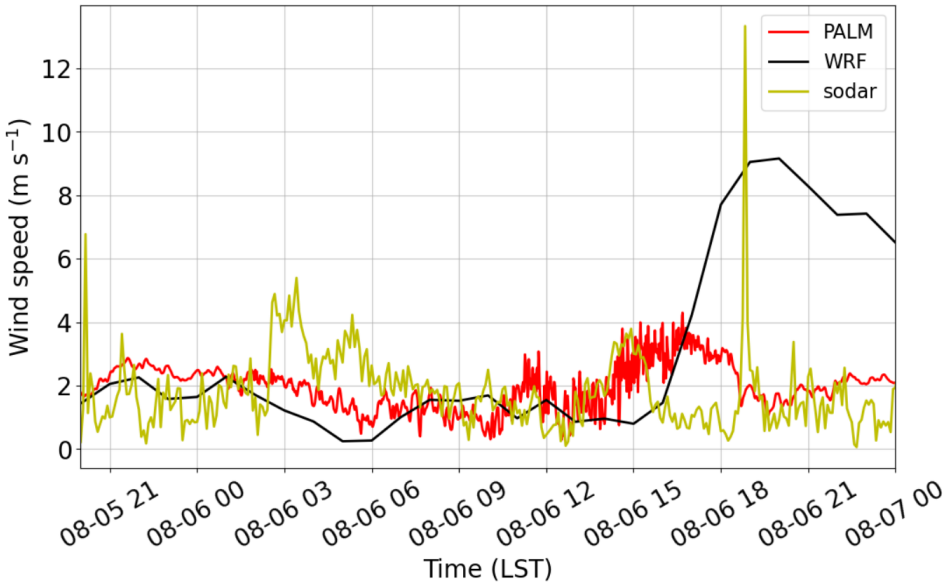


Figure AC3: Wind speed observed at 50 m AGL from the sodar located at Christchurch airport, and at the nearest model levels from WRF and PALM.

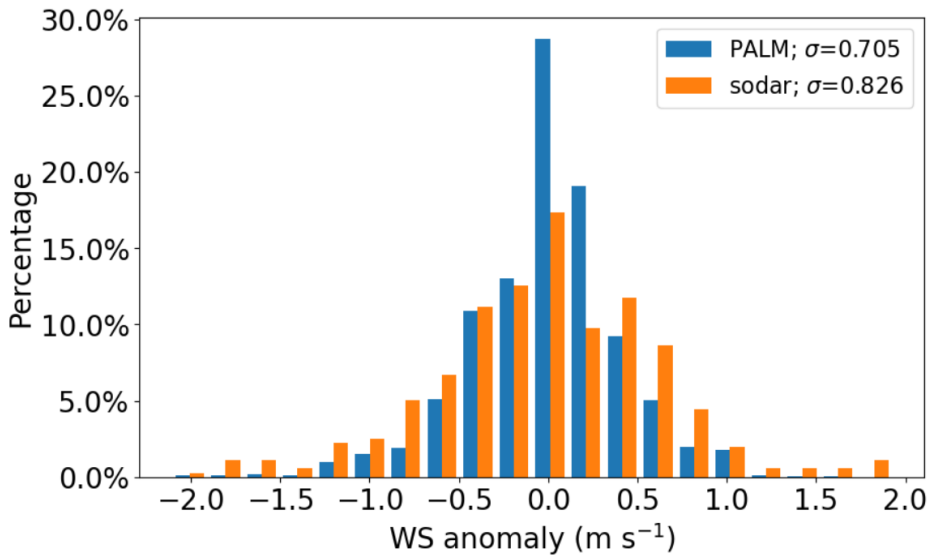


Figure AC4: Comparison of the range and distributions of hourly wind speed anomalies between PALM (27 m AGL) and the sodar data (30 m AGL) for the period between 2000 LST 5th August 2001 and 0000 7th August 2001. σ is the standard deviation of the anomaly data.

Furthermore, the simulated surface wind convergence fields shown in Figure 4 of the manuscript agree with Figure 14 presented by Corsmeier et al. (2006). Corsmeier et al. (2006) carried out a nocturnal boundary layer study along with an observational field campaign for Christchurch in July 2000 (Southern Hemisphere winter). Figure 14 in Corsmeier et al. (2006) and Figure 4 of our study are attached below:

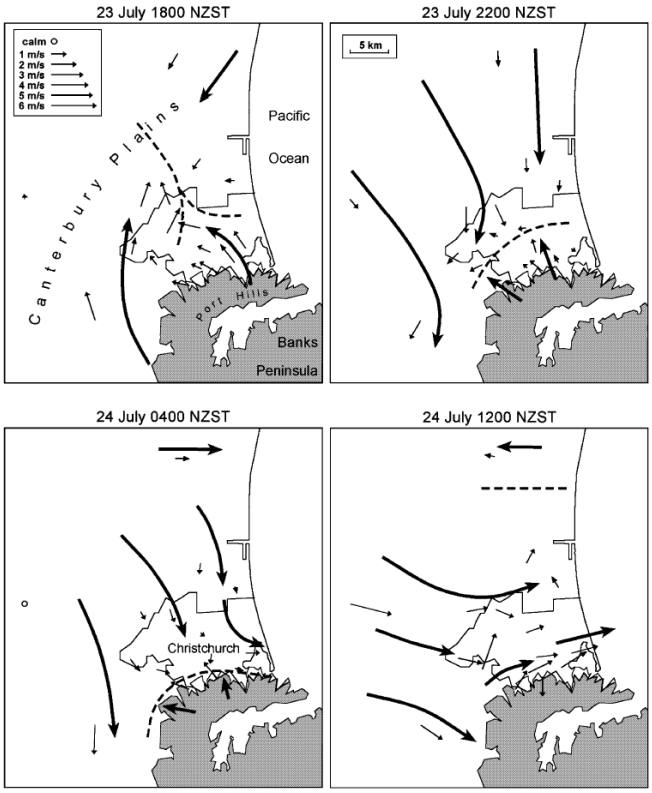
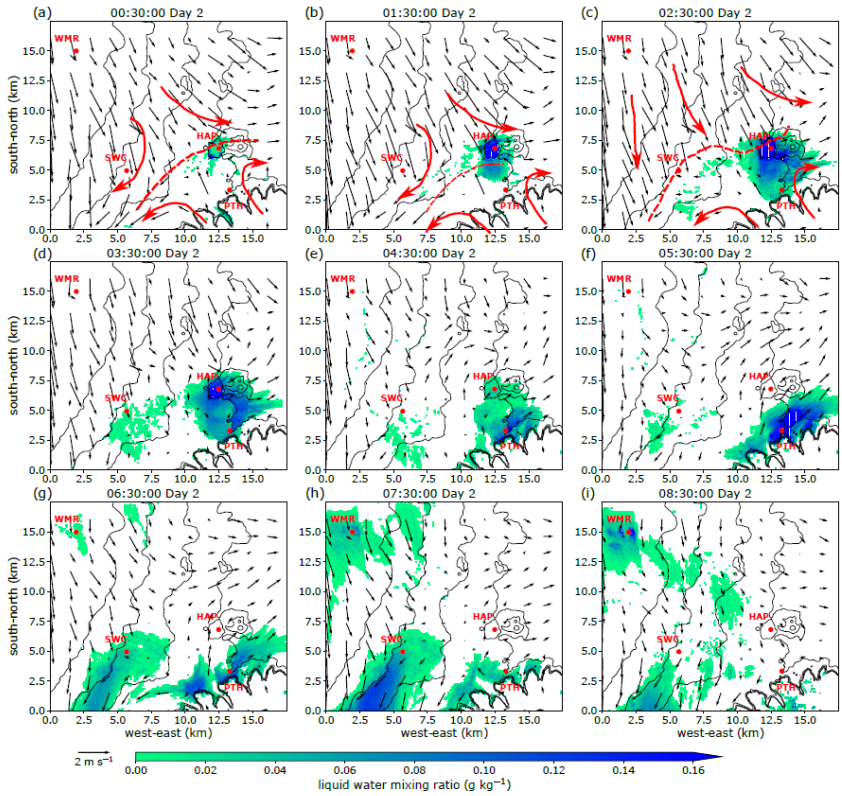


Fig. 14. Measured surface wind field in greater Christchurch between 1800 NZST on 23 July and 1200 NZST on 24 July (thin arrows). The authors' interpretation of the Christchurch mesoscale circulation system is given by the thicker arrows



In conclusion, even though the simulations are semi-idealised and not LES, our high-resolution mesoscale simulations have presented realistic meteorological fields for a fog case study. We therefore believe that the results of our experiments on effects of soil moisture heterogeneity are valuable to the fog modelling community.

References:

Corsmeier, U., Kossmann, M., Kalthoff, N., and Sturman, A.: Temporal evolution of winter smog within a nocturnal boundary layer at Christchurch, New Zealand, Meteorology and Atmospheric Physics, 91, 129–148, 2006.

Cui, C., Bao, Y., Yuan, C., Li, Z., and Zong, C.: Comparison of the performances between the WRF and WRF-LES models in radiation fog – A case study, Atmospheric Research, 226, 76-86, 2019.

Cuxart, J.: When Can a High-Resolution Simulation Over Complex Terrain be Called LES?, Frontiers in Earth Science, 3, 2015.

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