Authors' response for egusphere-2022-1229:

Multiscale meteorological controls and impact of soil moisture heterogeneity on radiation fog in complex terrain

The authors thank the reviewers for their time and consideration given to this manuscript. The reviewer's comments have been listed below in *black italics* and responded to individually in *blue italics*. Revised sentences are in *red italics.*

We understand both the reviewers' concerns that more detailed analysis is necessary to better support our argument presented in this manuscript. The goal of this paper is to investigate the impact of soil moisture heterogeneity on fog. The main argument is that in this case study fog is highly controlled by mesoscale meteorology and topographical forcings, while the spatial heterogeneity in fog duration at microscale can be altered by soil moisture heterogeneity. We agree that the previous version of the manuscript may have failed to sufficiently and clearly deliver the scientific message. We have followed Dr Westerhuis' suggestions and have conducted two additional simulations of doubling/halving the soil moisture of the homogeneous setup. Analysis of fog spatial heterogeneity and pseudo-process diagrams have been added to the revised version of the manuscript. We have conducted a significant amount of analysis before the manuscript re-submission after receiving reviewers' comments, but no quantitative conclusions can be made due to the high heterogeneity and complexity of the simulations. Although idealised simulations mentioned by the reviewers can single out the processes involved, when all effects of surface heterogeneity were combined, the interactions between each process make definitive analysis difficult. However, more discussion has been added to the manuscript and the manuscript structure has been revised.

Due to technical issues, the data output file for the HET6p simulation was corrupted and cannot be used for analysis. In particular, after recent major maintenance of New Zealand's high-performance computing (HPC) system, we are experiencing technical issues running PALM and hence cannot currently re-run any of the fog simulations. We have therefore had to remove HET6p from the revised manuscript, although the exclusion of HET6p does not alter our conclusions.

Reply to Stephanie Westerhuis:

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).*

We agree with Dr Westerhuis that more detailed analysis could better support our argument presented in this manuscript. We therefore followed the reviewer's suggestion and conducted two additional simulations of doubling/halving the soil moisture in HOM. The two simulations are presented in the revised manuscript in Section 5.2. We have added more analysis using pseudo-process diagrams in respect to the driving processes in Section 5.2.3. Running simulations for multiple days or running an ensemble is not feasible due to the high computation cost. Each one of our current simulations already takes about 1 day of wall clock time on 244 Intel Xeon Skylake Gold 6148 processor cores, running at 2.4 GHz.

- *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.*

Before conducting the simulations presented in the papers, we did conduct a few sensitivity tests with/without flat terrain and with/without land use. While simple sensitivity tests allow us to interpret the processes easily, the analysis gets complicated when more heterogeneity is involved. The interactions between different processes make fog formation complex and this is why fog research is particularly difficult. We have added two additional simulations of doubling/halving the soil moisture in HOM (see Section 5.2), along with additional analysis and discussion (Sections 5.2.2, 5.2.3, and 5.2.4). The results support our argument in the original manuscript that soil moisture and its heterogeneity does not alter the general structure of fog but can impact fog duration.

- *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.*

We agree with the reviewer that more detailed analysis is required. To aid the understanding of the fog event, we have added more analysis regarding spatial heterogeneity of fog (Section 5 in the revised manuscript). We have conducted analysis on accumulated latent heat flux, but the results do not show a significant difference between HOM and heterogeneous simulations (Section 5.2.3 and Appendix B Figure B2). Changes in soil moisture and its heterogeneity do lead to changes in accumulated latent heat flux (Figure B2), but do not lead to an increase/decrease in liquid water mixing ratio (Figure 12). Screenshots of Figure B2 and Figure 12 are attached below. For better image quality, please refer to the manuscript pdf.

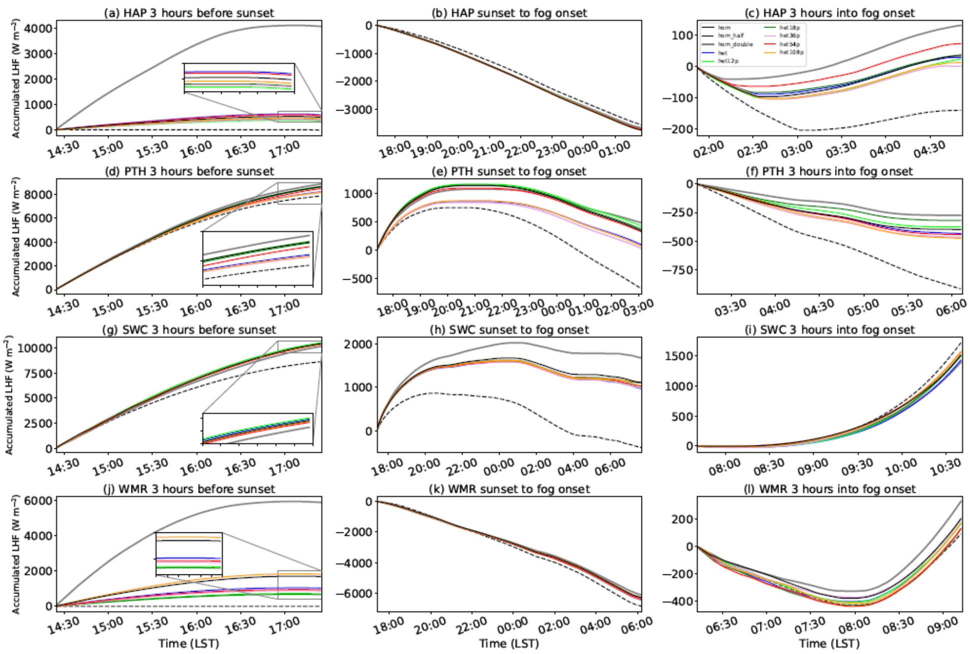

Figure B2. Accumulated latent heat flux (LHF) for the 3-hour period before sunset (a, d, g, j), the period between sunset and fog onset (b, e, h, k), and the 3-hour period into fog onset (c, f, i, l) for HAP, PTH, SWC, and WMR. Colours for all panels refer to panel (c) legend.

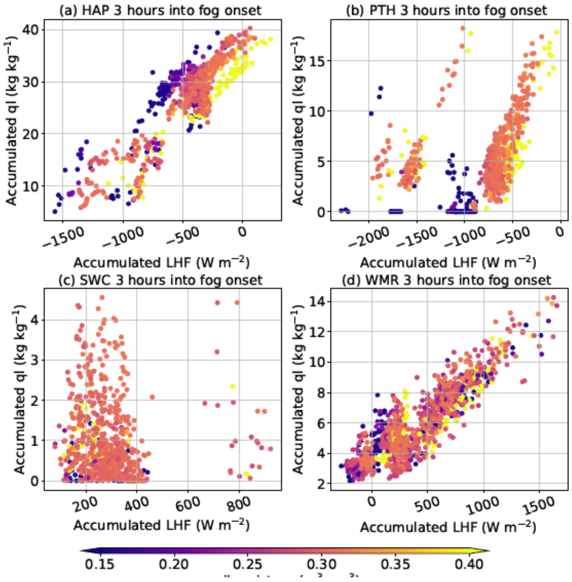

Figure 12. Scatter plots showing Accumulated latent heat flux (LHF) against accumulated ql coloured by soil moisture for HAP (a), PTH (b), SWC (c), and WMR (d). Data samples were obtained from each grid point at 1 km scale of all simulations. The accumulation period is 3 hours into fog onset.

To accurately quantify the amount of soil moisture ending up in the atmosphere and advected to where fog occurred, we believe more analysis is required using a Lagrangian approach. However, the amount of work involved in Lagrangian analysis would require an additional research paper.

We agree that the clouds could be an interesting factor. However, in this manuscript, we only want to focus on the near-surface processes. Analysis of the role of low cloud could be part of future work.

- *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.*

We have revised the Figure 1 caption as follows:

Maps of the case study simulation domains: (a) a New Zealand topographic map with a red square indicating the location of the simulation domains, (b) a topographic map (height above sea level) showing the simulation domain configuration for the case study, (c) a topographic map of simulation domains 3 and 4 (D03 and D04), the locations of the FENZ (Fire and Emergency New Zealand) weather stations, and the location of the sodar operated at Christchurch airport, and (d) a land use map of D04 with buildings and streets in white. The simulation domain 1 (D01) configured as flat terrain with short grass, and hence no topography, is shown in panel (b). The logarithmic topographic height colormap in panel (b) applies to panels (b) and (c), while grey indicates the ocean.

We consider the illustration of heterogeneous land use important because such heterogeneity has led to spatial heterogeneity in fog. Additional discussion regarding land use has therefore been added in Section 5.2.3 (between Line 408 ad Line 412) in the revised manuscript.

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    - *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.

We agree with the reviewer that the information regarding observations and field campaigns is redundant for this case study. We have revised the manuscript, moved the discussion of observations to Appendix A, and acknowledged that this study is "semi-idealised". The simulations are not completely idealised as the initialisation profiles of atmosphere were obtained from observational data. Realistic geospatial data were used in the simulations. However, the simulations are not realistic that they were not conducted for the purpose of replicating a real fog event. In addition, the simulation results were not validated using observational data.

- *Another example is the explanation about two-moment microphysics which are not used after all (L213-215).*

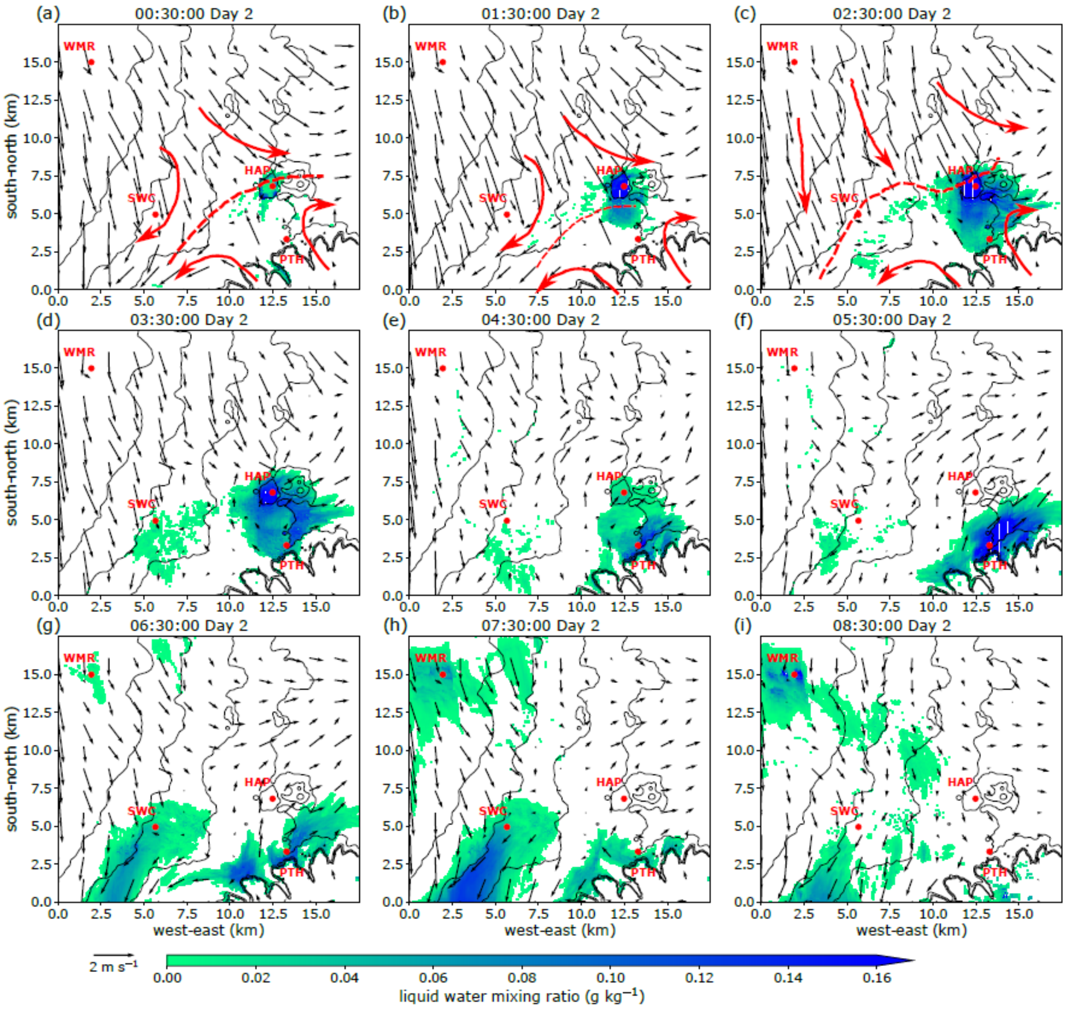
The explanation about two-moment microphysics has been removed in the revised manuscript.

- *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.*

Inclusion of contents related to land use in the Introduction and Data Description is to show that heterogeneous land use is considered in this study. Additional discussion regarding land use has been added in Section 5.2.3 (between Line 408 ad Line 412) in the revised manuscript.

- *Figure 6 includes a shading indicating the land use, while this is not discussed but instead hampers the interpretation of the displayed visibility.*

We have revised Figure 6 (now Figure 4 in the revised manuscript, see figure attached below). The shading of land use has been removed. The units of axes are now in km. Liquid water mixing ratio was plotted instead of visibility. Topography height contours were added. Density of wind vectors has been decreased for better visualisation. Timestamps are in local standard time (LST).



- The ***experimental setup is partially unclear***:

    - *How does the domain averaged soil moisture compare for the different heterogeneous setups to the homogeneous setup?*

The domain averaged soil moisture is identical in the homogeneous setup and all the heterogeneous setups. This has been clarified in the revised manuscript (Section 5.2.1):
The spatially heterogeneously distributed soil moisture of the simulation domain was first adjusted to have the same mean value as HOM. The soil moisture was then adjusted to amplify the signal of soil moisture heterogeneity. Finally, the soil moisture at each grid point was adjusted again such that the mean value of the entire domain is identical to HOM.

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    - *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.*

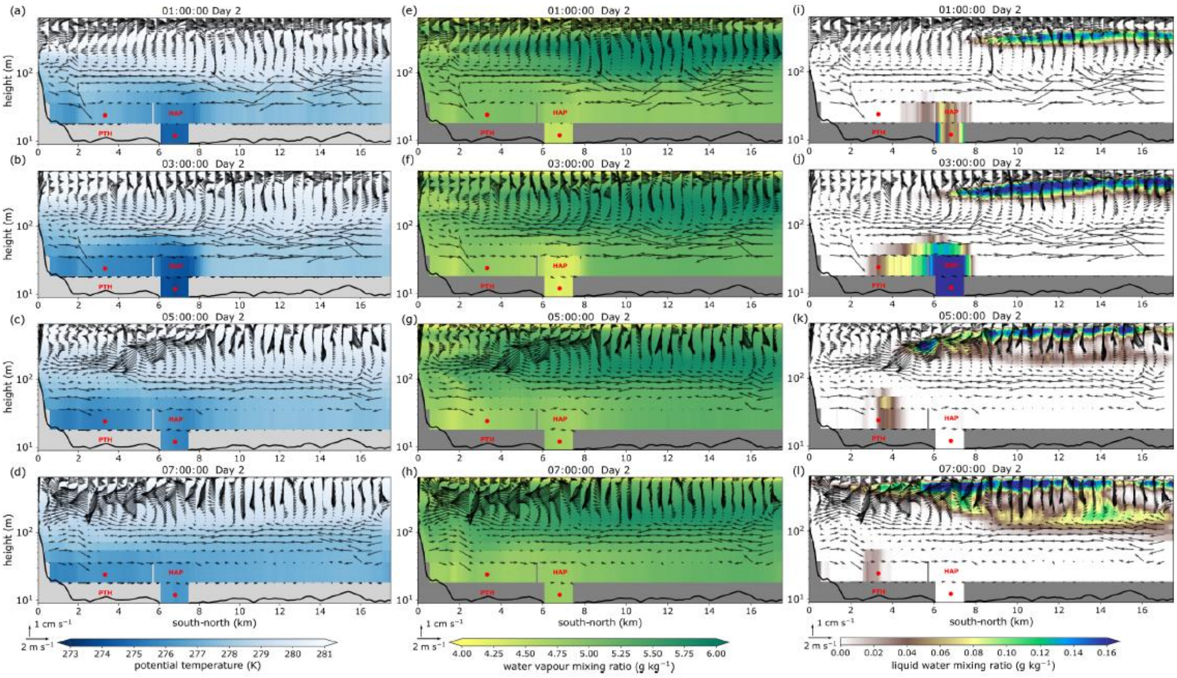
To clarify the adjustment method used for soil moisture, we have added the following details in Appendix B1 of the revised manuscript:

An example of the readjustment introduced in Section 5 is shown in Figure B1. For other adjustment of soil moisture values, the following method was applied to make all heterogeneous simulation shave the same mean value as HOM. The mean soil moisture of HOM is denoted as HOMmean and the mean soil moisture of HET as HETmean. The difference between the mean values is Δmean = HETmean −HOMmean. Then Δmean was added to each grid point to adjust the soil moisture value.

The adjustment is the same for the three-dimensional soil moisture field (west-east, south-north, and eight vertical levels of soil). For each soil layer, the difference between the domain mean value (HOMmean) and the original mean soil moisture value (SMmean) was first calculated as Δi = SMmean −HOMmean for each grid. Then Δi was added to each grid point at each soil layer.

- *What about clouds in general at initialization and throughout the simulation?*

All simulations presented in the manuscript have clear sky at initialisation and throughout the period before fog formation. Clouds only appeared around 0100 LST (Figure 6 in the revised manuscript, attached below). The cloud layer lifted after sunrise, but patchy clouds were still present in the simulation until late afternoon. This has been clarified in the manuscript as follows:
The simulation had clear sky from initialisation (not shown). The cloud and fog started to lift and dissipate around sunrise (0736 LST), while patchy clouds were still present 3 hours after sunrise.



- *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.*

We have followed the reviewer's suggestion and have revised the manuscript as follows:

1. *Introduction*
2. *Data description*
3. *Model and simulation configuration*
4. *Meteorological controls in the baseline scenario*
5. *Investigation of the impact of soil moisture heterogeneity*
   5.1 Soil moisture heterogeneity configuration
   5.2 Results and analysis
   5.3 Discussion
6. *Conclusions*

For more details, please refer to the revised manuscript.

- *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).*

All figures have been refined following this suggestion. All units have been changed from m to km. Plots with wind arrows have been refined for better visualisation and readability. All figure captions have been revised.

- *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).*

*We agree with the reviewer that analysis of liquid water mixing ratio (ql) directly is more revealing. Visibility calculated using Equation 1 (vis =0.02 * LWC^-0.88, in the original manuscript) usually reaches < 1 km once ql > 0. We therefore decided to use liquid water mixing ratio directly in the revised manuscript. The analysis of this manuscript only focuses on the lowest model level. We have clarified this as follows (Line 200 in the revised manuscript):*
Fog is identified when liquid water is present at the first model level, i.e. when liquid water mixing ratio (ql) is greater than zero.

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 in that the experiments are "semi-idealised".*

The title of this paper has been revised as follows:
Investigating multiscale meteorological controls and impact of soil moisture heterogeneity on radiation fog in complex terrain using semi-idealised simulations

- *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).*

We have revised the manuscript and adopted the wording based on Bergot and Lestringant (2019).
Abstract Line 1:
Coupled surface-atmosphere high-resolution simulations were carried out to understand meteorological processes involved in radiation fog life cycle in a city surrounded by complex terrain.
Line 27 (now Line 28 in the revised manuscript):
Over 30 years ago, Duynkerke (1991) identified land surface physical characteristics as the most important factor for fog occurrence, among several thermal and dynamical processes in the ABL
Line 37:
Over the past decade, many studies have included heterogeneous land surface characteristics in radiation fog simulations in order to understand the microscale processes (occurring from 1 cm to 1 km, and from seconds to hours) and associated feedback during fog life cycle.

- *Abstract, L6: Do not include links in the abstract.*

The link has been removed.

- *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.*

We have revised the units in the manuscript and changed axes in metres to kilometres. For example, Line 16-17, Line 36, and Line 227.

- *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".*

This sentence has been rephrased as follows:
Over 30 years ago, Duynkerke (1991) identified land surface physical characteristics as the most important factor for fog occurrence, among several thermal and dynamical processes in the ABL

- *L 47: Suggestion to rephrase to "in a region in north-eastern France".*

This has been rephrased as follows:
… in a region in north-eastern France

- *L53: Could you add a reference to a publication addressing "changes in fog duration"?*

Most papers do not address "fog duration" directly and they focus more on fog formation and dissipation. We hence have revised this sentence and added citations as follows:
Changes in soil moisture lead to variability in the surface energy balance, and consequently changes in fog formation and dissipation times (e.g. Bergot and Guedalia 1994, Remy and Bergot 2009, and Maronga and Bosveld 2017).

- *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…".*

We agree that these sentences are lengthy and can be misleading. We have revised Line 64-70 as follows:
However, to the best of our knowledge, no study has investigated the impact of soil moisture heterogeneity on radiation fog in combination with heterogeneous land use and topography.

- *L95: I suggest to prominently state that this is a "semi-idealised" numerical simulation and which makes use of "nesting".*

This has been revised as follows:
A radiation fog scenario was created in semi-idealised numerical simulations containing nested domains with the finest grid spacing of 81 m.

- *L175: Improve wording of "neither too high nor too low".*

We have reworded "neither too high nor too low" to suitable.

- *L205: How exactly does the configuration of the outer domain "maintain a stable boundary layer"?*

The flat parent domain was used as a buffer domain. When cyclic boundary conditions are used with steep terrain near the lateral boundary, there could be instability at the lateral boundaries where *inflow/outflow directions could be opposite. This may prevent a stable near-surface layer developing, while extra tests and investigation may be needed to verify that the configuration is responsible for maintaining a stable boundary layer. In the simulation, the evolution of the boundary layer has its own diurnal cycle. Therefore, we have revised this sentence as follows:*

The purpose of this domain is to pass down the synoptic forcing to the finer domains and to avoid numerical instability caused by steep terrain near the periodic lateral boundaries.

- *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.*

As this study mainly focuses on soil moisture heterogeneity instead of accurate soil moisture content itself, we directly used the derived soil moisture index (SMI) as a measure of soil moisture for the simulations. The conversion between SMI and soil moisture requires a known wilting point and field capacity, for which extra work would be needed. We have added more information regarding SMI as follows:

To include soil moisture heterogeneity in the simulations, this study adopted the soil moisture index (SMI) calculation method for Landsat 8 imagery described in Avdan and Jovanovska (2016) and Potic et al. (2017). SMI describes the proportion of actual soil water content relative to a known wilting point and field capacity (e.g. Zeng et al. 2004, Hunt et al. 2009). Our case study focuses on the spatial heterogeneity rather than accurate soil moisture values. Therefore, in this paper the derived SMI is used as a surrogate for actual soil moisture.

- *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.*

In the original manuscript, “fog onset” is recognised at a grid point, when visibility at the first modelled level became less than 1 km. In the revised version, we define fog onset as when ql>0 was first detected at a grid point at the lowest model level. The fog onset time varies for each grid cell. Figures of the timing of fog onset are attached below:

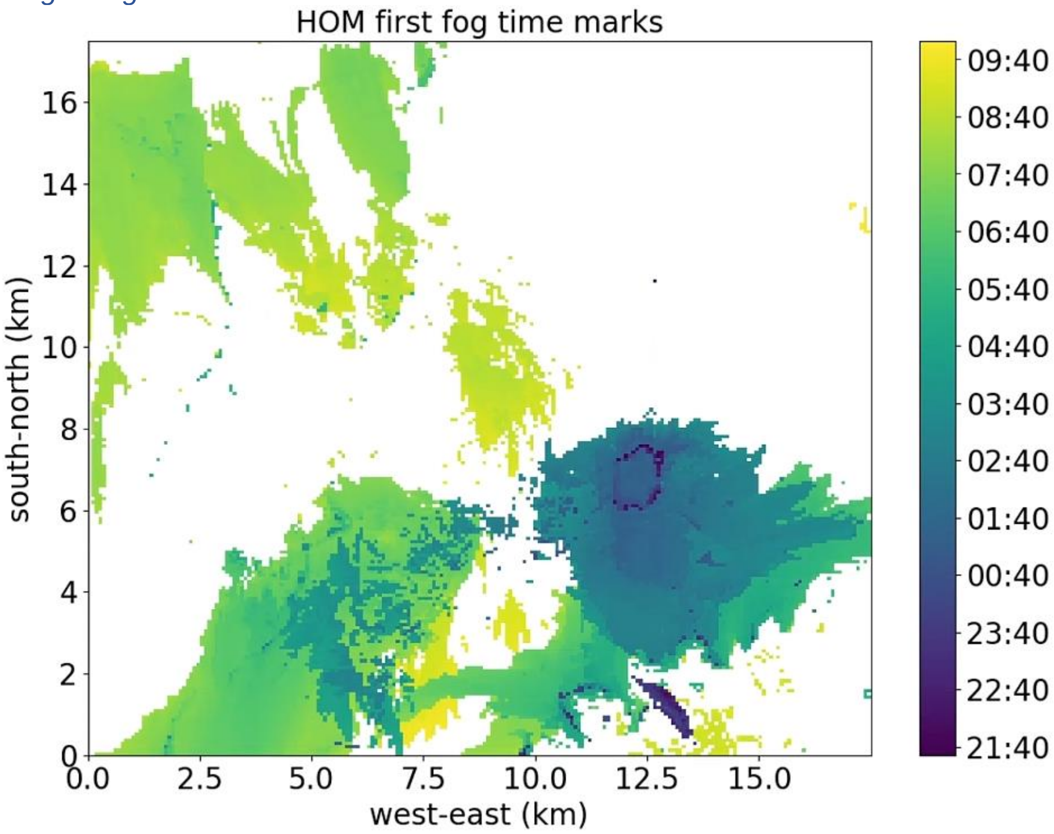


Figure AC1. Fog onset time at each grid point for HOM. Timestamps are in LST.

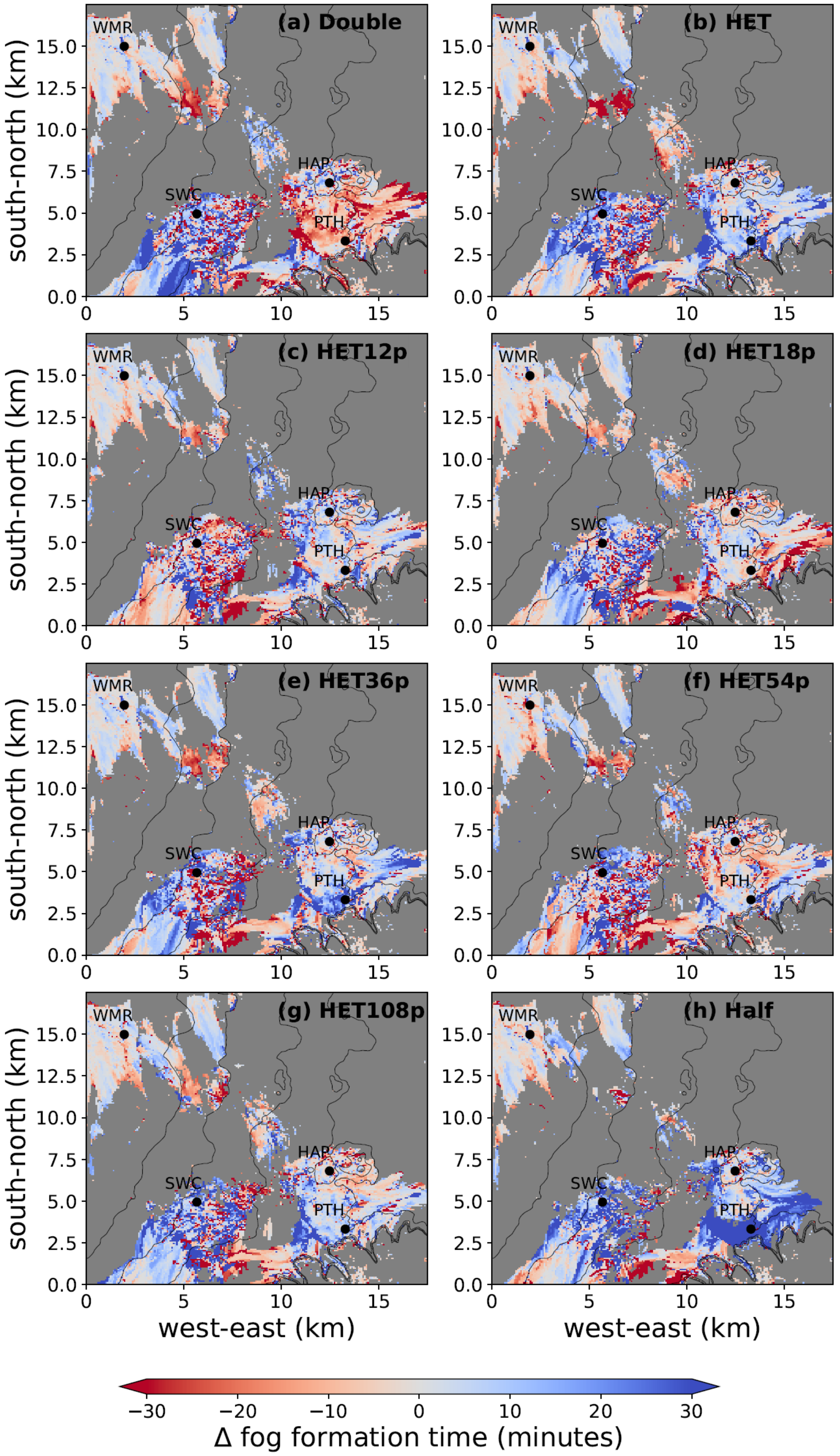


Figure AC2. Fog onset time difference between HOM and all other simulations. Red indicates formation is delayed in HOM and blue indicates the opposite.

Due to the significant spatial heterogeneity of fog onset time, dissipation time, and duration, we decided to only focus on four specific sites (see the revised manuscript for more details) instead of the entire simulation domain. We recognise fog formation time of a chosen area when fog coverage of the area rises from 5% to 95%, following the definition by Bergot and Lestringant (2019).

- *L264: "greater qv" is not a proper expression. Greater than what exactly?*

We have revised this as follows:
relatively high qv (> 5.0 g kg^{-1})

- *L270: A "horizontal cross-section of the first model level" is just "the spatial distribution of xyz on the first model level".*

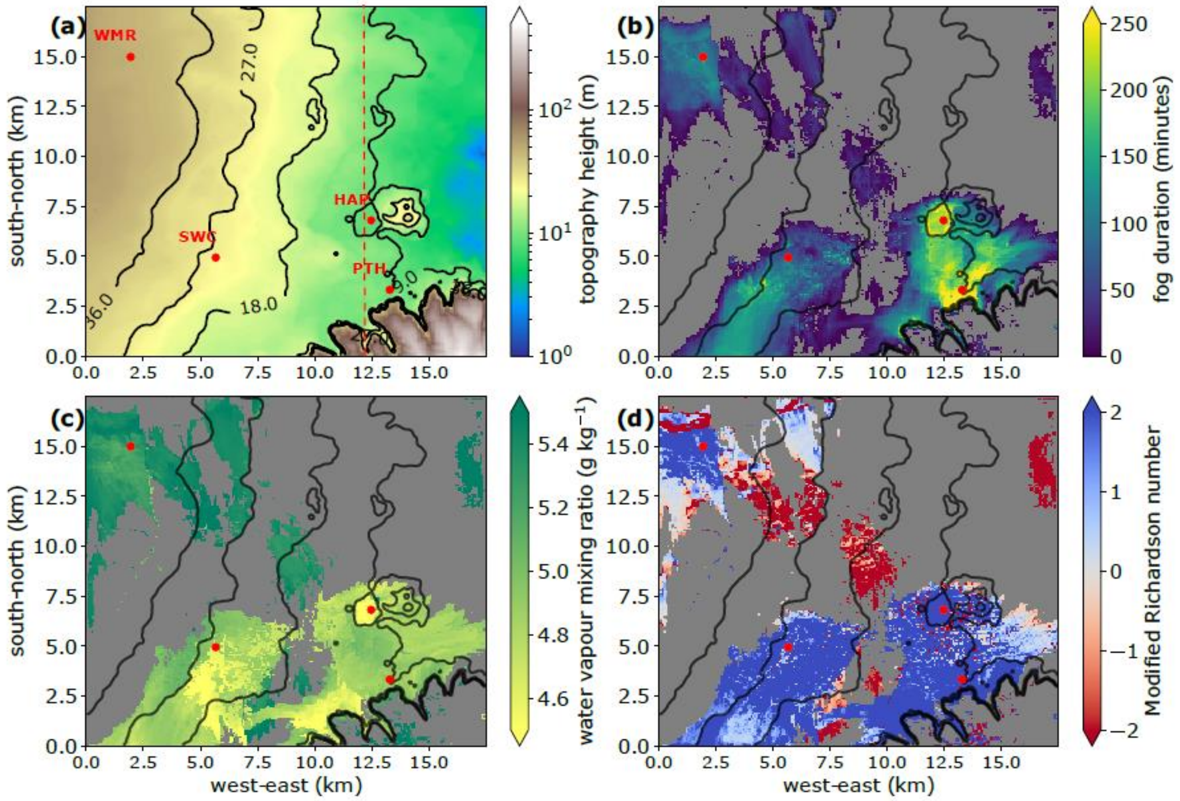
This has been revised as follows:
To provide more insight into the fog simulation, the spatial distribution of ql and MRi on the first model level…

- *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.*

We have revised the manuscript and all time stamps are now in local standard time (LST).

- *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.*

A vertical line has been added on Figure 5 (now Figure 3 in the revised manuscript, attached below).

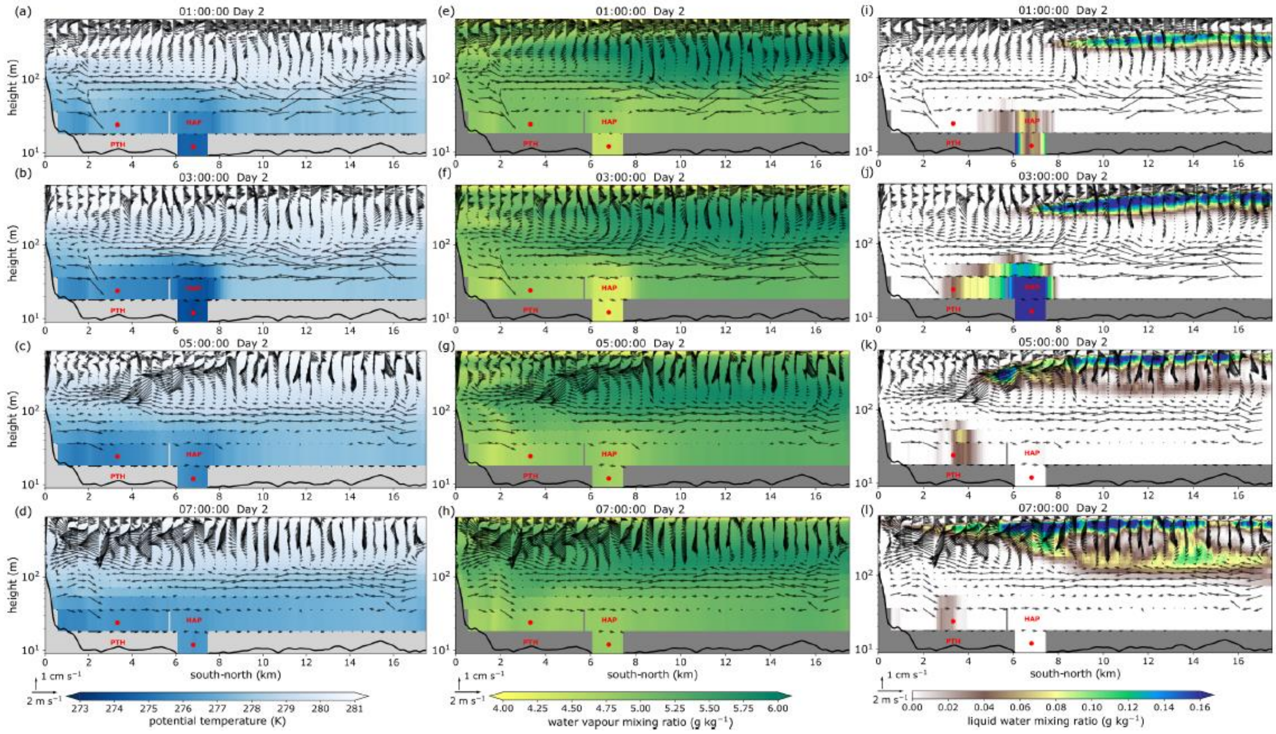


- *L297: The storytelling of the reference simulation could be improved by focusing on different areas one after another.*

We have revised the entire section (Section 4 of the revised manuscript) focusing on four selected areas - Hagley Park (HAP), Port Hills (PTH), Southwest of Christchurch (SWC), and Waimakariri River (WMR).

- *L307: Figure 9 is not a good reference to illustrate the surface elevation.*

We have revised Figure 9 (now Figure 6 in the revised manuscript, attached below) and a logarithmic scale is now used for the height axis to illustrate surface elevation.



- *L308: What is the causality between the “convergence of the southeasterly and northerly flow” and the “development of another stable layer”?*

This is similar to the drainage zone illustrated in the publication of Corsmeier et al. (2006), where air is stagnant and turbulence suppressed, and therefore stable layer can develop. This has been revised as follows:

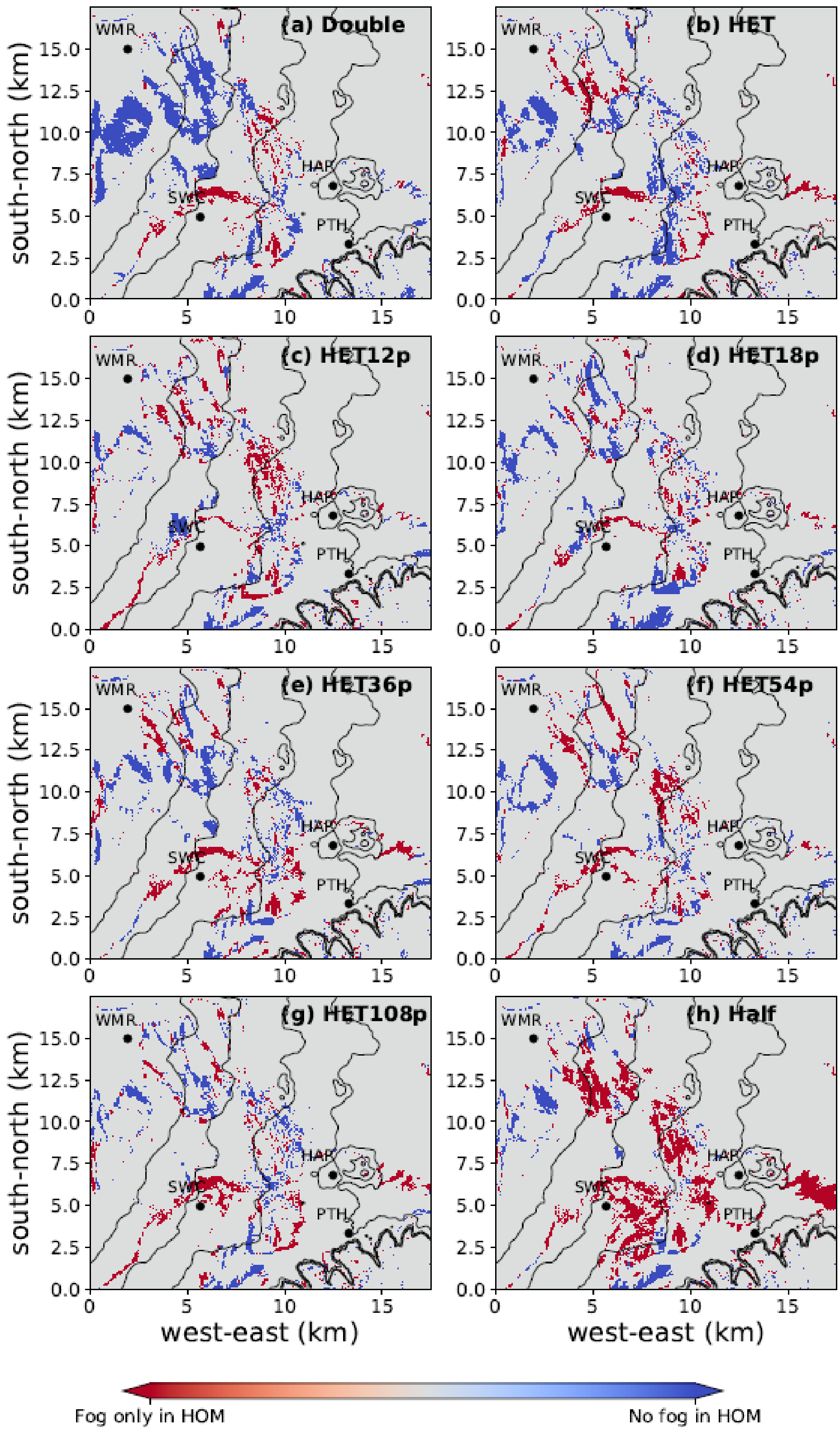
Another drainage zone developed due to flow convergence, where turbulence was suppressed and a stable layer started to grow.

- *L315: Does “in the meantime” refer to the period after sunrise?*

Yes. To make this clear, we have replaced “In the meantime” with 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.*

We agree that it is difficult to determine differences in spatial distribution of fog occurrence. A figure (Figure 9 in the revised manuscript, attached below) has therefore been added in the revised manuscript to illustrate the spatial distribution of fog occurrence.



- *L385: Specify: What would you expect to learn from a simulation at higher resolution with respect to soil moisture heterogeneity?*

A finer grid spacing may reveal more regarding how soil moisture interacts with the atmosphere under different land use. At current grid spacing (81 m), the land use is extrapolated. The plant and urban canopy are not well represented. A better resolved plant canopy may give more results regarding evapotranspiration so that identifying the contribution of soil moisture may be less difficult.

If observational data are available, it would be possible to use finer grid spacing to better resolve the small turbulent eddies and better simulate and forecast fog.

Reply to Anonymous Referee #2:

Major concerns:

- *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.*

We thank the reviewer for the detailed comments. Our response to the concern regarding the technical specifications of the simulations is listed below.

- *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.*

We have revised Section 4 so that the entire section could be more readable. All figures have been revised for better visualisation and readability. We have selected four areas to focus on and added labels for location references - Hagley Park (HAP), Port Hills (PTH), Southwest of Christchurch (SWC), and Waimakariri River (WMR). For details, please refer to the revised manuscript (Sections 4 and 5).

- *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.*

We understand that the original version of the manuscript may lack good story telling, and we have revised the manuscript following helpful suggestions from both the reviewers.

- *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.*

We understand the value of using observations to verify the simulations. However, as stated in Appendix A, it is not possible for us to do such comparison with the very limited observational network of Christchurch. Therefore, we have clarified that this study is semi-idealised, and unfortunately no direct comparison between simulations and observations can be undertaken due to this limitation.

- *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.*

Fog sensitivity to soil moisture has been studied particularly using PALM by Maronga and Bosveld (2017). We have added two extra simulations with doubled and halved soil moisture compared to HOM to show the fog sensitivity to soil moisture. More details are provided in the revised manuscript, Section 5.2.

- *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.*

We understand that more analysis is required to support our argument. Following helpful suggestions provided by the reviewers, we have carried out more analysis and the results are presented in the revised manuscript.

- *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.*

We would like to clarify that our simulations are not typical LES. Rather they are high resolution mesoscale simulations (see Cuxart 2015). As the simulations include heterogeneous land use, topography and soil moisture, it is difficult to present vertical temporal evolution of fog as each grid cell has its own unique profile. In this study, we want to focus on the spatial distribution of fog near the surface. To address the reviewers' concerns, we have revised the manuscript and added more analysis on the temporal evolution of fog and the processes involved.

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

We have added more analysis on accumulated latent heat flux and revised the manuscript accordingly to address the lack of data analysis.

Model configuration:

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

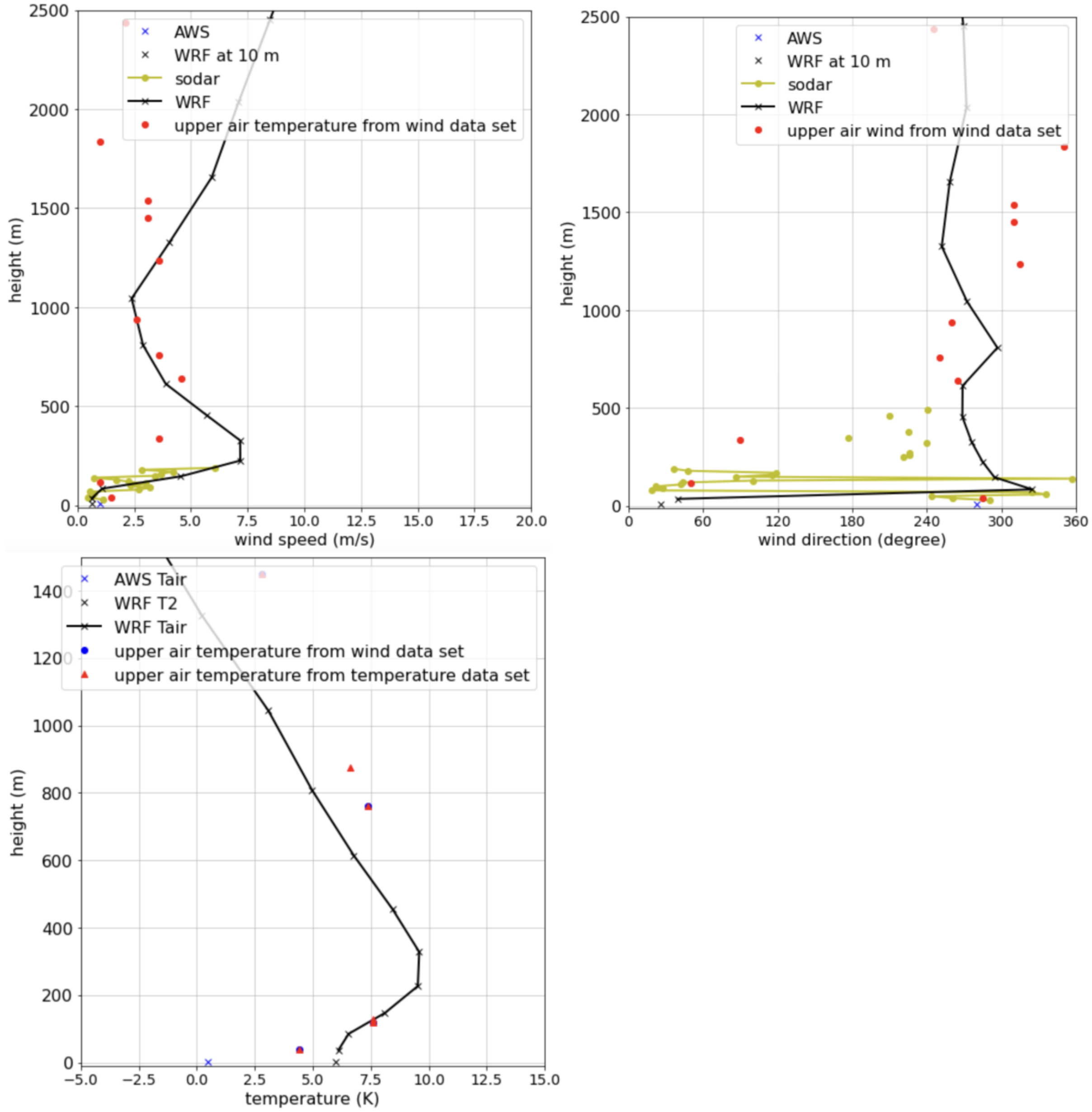
- *It seems that the first vertical level is at 18 m height: it is not suitable at all to fog simulations as we know the necessity to have a first level very close to the ground (max 2 m) (see Tardif, 2007)*

First, we would like to point out that the first vertical level is at 9 m above the ground instead of 18 m. The vertical grid spacing is 18 m. We understand that it is essential to have fine grid spacing close to the ground for numerical fog forecasting. While ideally we would like to aim at finer grid spacing to better resolve all processes involved in fog, it is extremely expensive to conduct simulations for such a big domain. Each one of our current simulations already takes about 1 day of wall clock time on 244 Intel Xeon Skylake Gold 6148 processor cores, running at 2.4 GHz. Therefore, we have clarified in the manuscript that our simulations are high resolution mesoscale simulations rather than real large eddy simulations (LES).

We only aim to investigate the impact of spatial soil moisture heterogeneity within a complex environment using the high-resolution mesoscale approach and are hoping this could provide guidance for fog forecasting. We understand this is a limitation of this study which we have discussed in Section 6.

- *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?*

We would like to clarify that this study does not aim to replicate a specific fog event, as it is a semi-idealised study. The vertical profiles of temperature, U, and V were obtained from observational data. The observational data do not include any vertical profile of q. The vertical profile of qv (water vapour mixing ratio) was obtained from WRF simulations because no observational data are available for Christchurch. It is not clear what the rv variable that the reviewer referred to is?
The initialisation profiles were carefully selected so that radiation fog can be simulated in PALM. We did carry 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 sodar deployed at the airport, AWS indicates data obtained from the automatic weather station located near the sodar, T2 is air temperature at 2 m simulated by WRF, and Tair indicates air temperature at various levels. The upper air observations include two sets of data: one for temperature and one for wind. The temperature data set includes vertical profiles of temperature only, while the wind data set includes vertical profiles of both wind and temperature. In general, WRF shows good agreement with all the observations.

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

Yes, it is correct that D01 uses flat topography, which was stated in the manuscript in the Figure 1 caption, model configuration (Table 3), and Line 203 in the original manuscript (Line 188 in the revised manuscript).

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

We used PALM's default 1.5 order scheme for turbulence closure, and for more details of this scheme refer to https://palm.muk.uni-hannover.de/trac/wiki/doc/tec/sgs and Maronga et al. (2015). We have added more description in Line 165 as follows:

All simulations used a modified three-dimensional Deardorff 1.5-order turbulence closure scheme, in which the energy transport by sub-grid scale eddies is assumed to be proportional to the local *gradient of the average quantities (Deardorff 1980; Moeng and Wyngaard 1988; Saiki et al. 2000; Maronga et al. 2015).*

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

Yes, the microphysical scheme was switched off in D02 and D03. This is to simplify the processes involved in fog formation because the simulation in its current form is already very complex to analyse. If both D02 and D03 have the microphysical scheme turned on, it would be even more difficult to understand the impact of soil moisture heterogeneity. In this semi-idealised study, we want to understand the processes involved in D04 first before conducting more complex simulations.

- *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?*

The Kessler scheme embedded in PALM is a simplified one-moment bulk cloud scheme which does not include any microphysical cloud droplet concentration in the parameterisation. This scheme simply automatically converts supersaturation into liquid water without any prognostic quantities for microphysical variables. Cloud water sedimentation was enabled. For clarification, the following sentence has been added in the revised manuscript:

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

Ideally, one would aim at using a two-moment microphysical scheme, but this is not allowed in PALM when there is a plant canopy in the simulation, as described in Section 3.