

Response to Anonymous Referee #2. The original comment is given in blue font, followed by our response in black.

General Comments

This research covers the aspect of operational weather forecasting of cold pool events (or minimum air temperature episodes close to the surface). The research starts by highlighting the importance of predicting cold pool events and clearly indicates the current tools applied within the meteorological services. The research also crosses the boundaries (in terms of current practice) towards the usage of high spatial resolution numerical weather modeling for long term periods with the purpose of identifying the model biases and linking them to the physical and numerical process. The sensitivity of the cold pool prediction biases to subgrid scale cloud parametrization is clearly argued and valid in my opinion, and the effect of the underestimation partial cloud cover on cold nighttime temperatures appear to be clearly presented in the observations and sensitivity model experiments. The authors also acknowledge in the conclusion current scientific efforts in the literature that align with these findings.

As with any geophysical model development, the approach is often biased to the specific geography and in particular to meteorological science the predominant physical processes will vary from one location to the other. For this reason, it could be useful to indicate the limitations of this research in the conclusion section.

The limitations of this research based on its focus on a single location has now been noted in the conclusions section.

I would also like to see a few sentences that cover the relationship of vertical grid resolution sensitivity to cloud development (not too extensive analysis though), unfortunately most if not all of the discussion was limited to horizontal resolution.

Additional discussion of the sensitivity to vertical resolution has been added in the conclusions section, including both reference to previous COLPEX sensitivity tests (Vosper et al, Atmos. Sci. Let. 14: 193–199 (2013)) and briefly discussing results from an additional simulation in terms of the impact of vertical resolution on cloud cover.

Specific Comments

Page 4454, line 19: remove “it being” and include “as it is”.

This change has been applied.

Page 4455, line 6: sentence should read “. . .to significant spatial temperature. . .”.

This change has been applied.

Page 4455, line 8: Please expand the acronym “UK”.

This change has been applied.

Page 4455, line 21: Please mention the average dz here so one can relate this to the valley’s dimensions and model aspect ratio.

The average dz for the lowest 1km is now given here.

Page 4456, line 1: change “different” to “longer simulation period”. The simulations are not technically different but are longer allowing for a larger and more statistically significant parameter size to analyze.

This change has been applied.

Page 4456, line 15: The authors clearly discuss that the presented dataset is not within a climatological context, and I totally agree with that. However, the subsequent usage of the word climatology is adopted. I prefer, and to avoid confusion, to completely drop the term climatology and use “long term simulation” throughout the entire manuscript.

This change has been applied.

Page 4460, line 2: Please provide a technical definition of the term “spinning up”.

Additional description of the meaning of “spinning up” has now been added here.

Page 4460, line 2: “soil properties”, how do you spin up soil properties? I guess the authors might be referring to the “spin up” of a process, like the land surface model, please clarify.

“Soil properties” has been changed to “soil moisture” and “soil temperature” to avoid confusion and clarify the meaning.

Page 4460, Line 14: Please use “top soil” instead of “soil”

This change has been applied.

Page 4460, line 14: The sentence “The soil cools throughout August” might need some explanation, like why does it cool? Also why is there a “significant” warming trend in September? A brief mention of the area’s climatology in terms of prevailing weather patterns will be very useful.

Describing the synoptic causes of these changes is not the main focus of the manuscript, however a description of the synoptic conditions during this period has now been added to the manuscript here.

Page 4460, line 26: What is a “moist event”. Is it a result of precipitation? Please be more specific and clear.

“moist event” has been changed to “precipitation event”.

Page 4461, line 7: “. . .the temperature and humidity fields are not sensitive to the . . .” They appear to me as sensitive but differences not significant. For temperature the difference goes up to +/- 1 degC.

The sentence has been revised to address this concern.

Page 4461, lines 12 to 23: For figure 6b. Why is the mean bias reduced for the dx1.5km as opposed to the dx100m for the high elevation site? This is counter intuitive as the flowing argument presented by the authors so far indicate that higher spatial resolution equates to reduction in biases. This is an interesting issue.

The differences between dx100m and dx1.5km away from the valley are interesting and worthy of future investigation, however the reported biases are -0.7K and 0.4K and do not justify presenting further analysis here. That higher spatial resolution does not always equate to a reduction of biases is demonstrated in the following discussion.

Page 4462, line 5, 6: This is an overstatement in my opinion. The sites are around 4km apart and the heterogeneity of the valley cooling rates would create a heterogonous cold pool forcing. How is this represented in those two sites? Why not use a north –south cross section from the hobo temperature loggers? There are smaller valley outlets in the domain that would create drainage flows and turbulent kinetic energy modifying cold pools across the landscape; actually there is a valley of this king between the two observation sites (Fig. 1).

This statement is supported by a previous COLPEX publication (Vosper et al Q. J. Roy. Meteor. Soc., 140, 699–714, doi:10.1002/qj.2160, 2013) and a citation of this work has been added.

Page 4462, lines 6 to 14: Were these measurements taken from stable boundary layer periods? It is unclear in the text please clarify.

The analysis includes all simulation data, stable and unstable. The manuscript has been altered to clarify this issue.

Page 4463, line 15: change “. . .dominates the energy budget” to “. . .dominates the surface energy budget”.

This change has been applied.

Page 4466, lines 5 to 15: I seem not to find the figure from which this paragraph’s analysis is made from. Is there a missing a figure I wonder?

It was felt that this description was sufficient to avoid the need for an additional figure.

Page 4467, line 5: “(16-17)”, this seems to be inconsistent with the material mentioned in the figure captions, please correct.

This statement refers to the original intention of the RHcrit parameterization and a citation has been added to make this intention more clear.

Page 4473: Please add sub-graduation on the x and y-axis. It is difficult to assess the distance between the observational stations. Also, please add a geographic north vector for map reference.

This change has been applied.

Page 4475: Add month labels as you refer to months in the manuscript and not day number. Also indicate in the caption from which model simulation nest this plot is for.

These changes have been applied.

Page 4476: The y-axis label is not a soil moisture unit, please correct. Should be percent or mass by volume for example. Also indicate in the caption from which model simulation nest this plot is for.

Although the units given on the y-axis are consistent with the data presented, a new version of the figure has now been generated presenting data with units of kg/m³.

Page 4478: What do you exactly mean by “daily hourly minimum”? Please clarify in simpler language.

This has been clarified.

Page 4482: What does “F” in the figure legend mean? Also, please mention or indicate on the map from what area was the spatial averaging performed on.

“F” is now defined in the caption. Also, the area spatial averaging was performed on has been described.

Page 4484: The y-axis label “K” is not defined, please define in caption or reword.

This has now been addressed.