## Dear Dongqi Lin and co-authors

Many thanks for your patience. Evaluating your paper is one of the most difficult cases I ever had as an editor, because of the strongly diverging opinions. Here I briefly summarize the status of the evaluation of your paper:

## First round

Referee 1 was critical and made many constructive comments about how to improve the paper. Referee 2 was very critical and recommended to reject the paper. One important comment was "The numerical choices on the model configuration are not always clearly presented, and are sometimes debatable". This problem seems to remain, see below.

## Second round

Referee 1 was positive about the improvements and recommended to accept the paper with minor revisions (which you implemented).

Referee 2 declined to do another review.

I invited a new Referee 3 who was very critical ("flawed methodology") and recommended rejection.

## Third round

Referee 3 remains very critical. See their comments below. I therefore asked advice from two additional LES and fog experts. They agree with the limitations of your modeling approach, but overall, see value in your study for the fog modeling community. Below you find their comments (referees 4 and 5). Importantly, referee 4 makes very good suggestions to do some additional sensitivity tests.

#### Decision

There is clearly no consensus view from the experts about your paper, but it is time to soon conclude this long review process. And since three experts see value in your study, my decision is to accept your paper for publication in ACP, on the basis that you add a few sensitivity tests as suggested by reviewer 4. Thanks to the transparent review process of ACP, this difficult decision, and the reasoning behind, will be apparent to the community. I see my decision in the spirit of "in dubio pro reo". Some doubts remain about the model setup, but often in science, it is also valuable to publish results with limitations and caveats, as long as they are explicitly discussed in the paper.

Therefore, my request is that you submit a revised version of your paper where you

- 1) Check again that you explain as transparently as possible your decisions for the model setup and its limitations. The comments from reviewers 3-5 are certainly helpful for this.
- 2) Include additional sensitivity experiments (e.g., in the form of an Appendix), as suggested by reviewer 4.

With best regards, Heini Wernli

## **Referees' comments in round 3**

#### **Referee 3**

I recommend rejection of the manuscript because the authors (and they confirm this) are misusing an LES model in a way which is simply not allowed. This is a severe technical flaw, but I do not see that the authors realize this.

The authors did address my major concern that they used a non-adequate grid spacing in PALM to simulate a fog event. However, they agree that the resolution is not sufficient and that they in fact did not perform an LES simulation. They provide some reasoning why they did not use a RANS model, but that does not make the methodology sound and safe. In fact, using an LES model (i.e. if most of the flow is parameterized by a subgrid-scale model which assumes that the flow is isotropic, turbulent and the eddies to be parameterized are in the inertial subrange of turbulence will simply not work. The results will be wrong. Point. The reasons that PALM does a better job in representing the complex surface does not count, since the flow dynamics are faulty. I thus have to recommend rejection.

## **Referee 4**

Firstly, I do not agree with the expert (referee 3) that the results will simply be wrong because PALM is an 'LES' model being used outside of the LES regime. I'm not an expert in PALM, but if asked to construct a numerical model at the hectometric (~100m) scale it would require: A dynamical equation set capable of representing all important processes at that scale – I've no reason to suspect PALM does not have this.

A suite of physical parametrizations to represent unresolved processes – again, I've no reason to suspect PALM does not have this. In particular, the key issue is likely to be around the subgrid turbulence parametrization. The one they do use is perhaps not the best, but it's also not wrong. We'd want the scheme to be primarily local in nature (i.e. non-local fluxes required in NWP/climate models aren't needed at this scale), which it is, and the scheme definitely needs to be 3D (the 1D assumption in NWP/climate models is also not valid any more), which again it is. So it's fair to say that I do see merit in their study, and don't believe their choice of tool precludes publishing the study. This is the scale at which we're going to be doing NWP of fog in the coming years, so it is a valuable resolution for research.

However, I would also raise criticisms of the model setup that I don't believe have been ac

However, I would also raise criticisms of the model setup that I don't believe have been adequately addressed by the authors. In particular:

It would be important to characterise the sensitivity of their results to the subgrid mixing scheme they are using. If the scheme is just taken unaltered from LES scales, it's likely to generate a mixing length which is proportional to the model grid-length, rather than any physical scale. This is likely to be too large given their coarse resolution, therefore **it would be nice to see some alternative simulations** with an appropriately tuned scheme to give a more physical mixing length for stable boundary layer / fog conditions. I think this would help alleviate the other reviewers concern, particularly if it didn't change their overall conclusions.

I'm slightly concerned about their vertical grid setup – the key feature of NWP-style models is usually having a stretched vertical grid, to give enhanced resolution near the surface and properly represent the near surface processes. This can have a huge impact on fog simulation. 18m in the vertical feels way to coarse to me – I'd expect to see a lowest level around 1-2m and some sort of quadratic stretching away from this (maybe ending up at a uniform 18m when that is reached). So again, I'd probably **like to see some sensitivity test** of how vertical resolution affects their results.

Hope that helps! I'd be happy to act as a reviewer, but don't think I'd have much to add on top of what I've said above if I did read the full paper.

# **Referee 5**

I agree with the reviewer and the authors that the simulations should not be termed "LES", but rather "high-resolution mesoscale simulations" (as indicated by the authors in the revised manuscript; following also the suggestion by Cuxart 2015).

This review seems to touch upon a long-standing debate between LES purists and more applied users of LES and mesoscale models, as for example, in the mountain meteorology community.

While the former rightly claim that the majority of the turbulence should be explicitly resolved in a LES (say 90% or more) in order to fulfill the basic assumption of LES. This is because the formulation of the LES subgrid-scale turbulence model assumes that the large eddies are well resolved and that only the universal inertial subrange of turbulence needs to be parameterized.

On the other hand, if you are interested in multiscale problems, which include, for example, local circulations due to topography or land surface heterogeneity in addition to the turbulent scales, then high-resolution mesoscale simulations are also of value. And due to computational constraints often the only realistic possibility, as larger domain sizes are required. The focus of these simulations is then typically to study the impact of the local circulations on the problem, taking into account a less accurate representation of turbulence.

As the current simulations are carried out in the turbulence grey zone, neither the assumptions of LES, nor those of RANS, are fulfilled. My first impulse, was also, why did the authors not use WRF in RANS mode. However, considering the authors arguments and that we are anyway in the turbulence grey zone, for which no universally accepted parameterizations exist, I can accept the authors reasoning for the current setup. In that case the LES closure can, in my opinion, be considered as a poor parameterization for the turbulence grey zone. A setup which is not unusual in the literature.

As long as the setup is clearly communicated and aptly named (i.e., high-resolution mesoscale simulations), I deem it acceptable.