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Interactive comment on "Evaluation of large-eddy simulations forced with mesoscale model output for a multi-week period during a measurement campaign" *by* Rieke Heinze et al.

Anonymous Referee #1

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This paper describes a set of long ranging LES simulations under realistic conditions, as opposed to the more traditional idealized LES simulations. While the paper does not show an enormous amount of scientific novelty, the long continuous run clearly differentiates itself from previous, similar work done by Neggers or Schalkwijk, and is therefore viable on it's own. Also, it is especially good as a reference for future work by this team and others to have clear description of the methodology, as it is provided in this paper. I can therefore recommend this paper for publication in ACP. However, I would enjoy it if the authors would further the scientific aspects a bit more, for instance in some of the following ways:

*) Presumably, the reason to do these LES simulations in the first place is to retrieve

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better quality data than from the COSMO host model. The current results (e.g., fig 6) mainly suggest that the LESs are just close to COSMO. There also seems to be little added value in using 2 different LES codes, at least for the results that are presented here. Does that suggest that the host model strongly dictates the LES result? I understand that it is not the point of this paper to make any of the models (or indeed, observations) look bad, but it would be nice to highlight the benefit of your method by pointing out areas where LES outperforms COSMO, and other areas (such as the nocturnal BL, most likely), where COSMO keeps LES in check.

*) Fig 1: As you mention in the text, the chosen site shows some pretty strong orography and heterogeneity. Is there a way that you could assess the influence of this on your simulations? For instance, a run with interactive land surface with one of the models would be nice.

*) p8, l5: Could this lagging (that you also note later in the paper) be caused by the way the nudging is set up? Since the nudging is always towards the current COSMO state, and with a finite time scale, I'd expect some lag to be present. An experiment with nudging towards a future state (may be an e-folding time away?) could be interesting to test this.

*) p8, l9: Could you quantify the magnitude of the nudging tendencies? For instance with some normalized scatter plots of each of the tendencies for several cases? Figure 2 is not perfect to distinguish what happens for instance during the night, or under specific circumstances. Another way of presenting this could be to look at the aggregate diurnal cycle of the tendencie terms.

*) Table 2: I notice that there are no false positive cumulus days in your simulations. Is this a coincidence, or is this a sign of some bias, for instance in the surface energy balance?

*) p12, l 5: Is there a way you could prove that the advection schemes are to blame for the difference between the LESs? Would the difference for instance be mitigated

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at a higher resolution, or is there a way to change the advection scheme in one of the models? A single day run would be sufficient to make the point that numerics matter.

*) p12, l 21: I'm not entirely convinced about the need for your Richardson based BL depth assessment. I see that this is an attempt to come up with something that applies to all BL types, but then you never discuss the stable boundary layer anyway. Why not stick with something as close as possible to the observed quantities (e.g., max gradient in some profile)? That would make the comparisons you make with those observations a lot more fair, and you could produce a more realistic error/bias assessment.

*) p18, l 12: I'm not sure why May 5 was chosen as a highlight, at least not as a cumulus day. As the figure shows, it mostly displays forced clouds, with little buoyancy of their own. I would expect this BL to behave like a dry CBL, as it does. If you want to show case a cumulus day, it may be nice to pick another one (if available).

Other comments: *) p1, l 20: There is quite some newer literature on LES of the stable BL. For instance the GABLS 3 cases (Basu, 2012 or Edwards, BLM 2014) *) p4, l7: both; momentum <- extra semicolon *) p5, l20: SUB is not homogeneous because phi=phi(x,y,z), right? so uLS=uLS(z) *) p6, eq 2: Does this mean that your nudging time scale is constant over height? Have you tried playing with this? *) p7, l 11: depth of the boundary layer, not height. This may be present a few more times in the manuscript *) p11, l 31: "Anyhow...." very colloquial. Anyhow's happen a few more times in the manuscript *) p11, l 31 "the more challenging..." not entirely clear what you mean with this sentence. *) p13, l9: How does the BL depth perform in SCu topped BLs? I assume that the cloud base is then the desired BL depth, but I can imagine that a strong variability can be observed in those cases. *) p13, l 21: Why this threshold? It seems arbitrary to me. Can't this be normalized with the maximum variance or so? *) p15, l9: I printed the paper in Black and White (yes, my department is old fashioned). While I can follow the rest of the manuscript without problems, I could not see the highlighted yellow sections. A better highlight, or just a table with a description of all the days (clear/cumulus/frontal/strong forcing/...) would be nice. *) p20, l 12: What does RPS mean? *) p20, l 33: "the question arose" may be a bit more formal? *) P24, l 9: The URL is not yet complete. I assume that will happen later?

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