General comments

This study performed a detailed process-based analysis of ICAR and its sensitivity to the calculation of the Brunt-Väisälä frequency, sounding, boundary conditions, mountain geometry, specifically focusing on cloud microphysics processes. Overall, I think the manuscript is organized well and most of the results are described clearly (except where noted). The method described to determine the minimum model top seems promising and the paper provides a useful example for researchers to test their own idealized simulations before moving into more costly 3D real cases with NWP models. I have provided numerous specific comments below, and overall, I think a revised manuscript would be a good contribution to GMD.

Specific comments

[P]age#, [L]ine#

- P1, L21 – "a large shift in" is vague. IS this a spatial/temporal shift? Is it a shift greater or less than the observations or simulations?
- P2, L5 – what is meant by "epistemological reasons"? It is vague and unclear to me what message is being conveyed. I read the abstract in Oreskes et al. and it helped to understand your "reasons", but I think it would be helpful for the reader to provide an example or paraphrase Oreskes and/or provide your own explanation.
- P2, L30 – "cannot be inferred from", why? What other information is needed? [Schlunzen 1997]
- P3, L14 – "distribution of precipitation", is this referring to spatial or temporal distribution?
- P3, L14 and throughout the text – "correct for the wrong/right reasons" is a catchy phrase, but I think there are places in the text where it would be good to state out right what you want to say. For example, you could replace the phrase with "results that compare well with observations, yet were produced by a different chain of processes than those found in the observations" or "model results that were produced by a chain of processes similar to those found in the observations".
- P5 – How frequent is the forcing timestep compared to the model time step?
- P7 – What is the model time step used for the ideal case configuration?
- P8, L2 – Do you mean that simulations had a constant RH with height at the extremes of no moisture (RH=0%) and a completely saturated vertical column (RH=100%)? You do test model tops from 4.4km to 14.4 km, and although saturated conditions are realistic for the lower troposphere, it's a bit unrealistic to have an orographic cloud (saturated conditions) be deeper than 10 km, especially going into heights of 14.4 km. Perhaps I'm missing something, but if RH=100% in the initial sounding, you would have a cloud moving over the mountain, as opposed to have cloud develop through orographic lift as the moisture encounters the barrier and reaches saturation. This needs to be clearer.
- P13, L31 – I don't see the "slight distortions" you speak of, that said, what is the physical importance of this distortions?
- P14, L7 – So the large deviations over a small spatial area are averaged out in the MAE calculation? If so, clearly state this point, don't allow for any misinterpretations. Tell the reader what you want them to understand.
- P14, L14-15 – What is meant by "an elevation dependence"? Explicitly describe these features and why they are relevant to note.

- P16, L8 – Potentially repetitive sentence starting with "Potential temperature…", this statement was essentially said on L3-4.
- P18, L10 – So the upper levels become more stable? How much do the upper levels "heat up"? Potential temperature increases on the order of 1K or 10K? Do you know why this is happening?
- P21 – In Figure 6 I noticed the spread in the RE in dependence of z_top has a large spread due to the scenario for q_sus, P_12h, Qv, and Qsus, while the other variables have a narrower spread, meaning that the dependence on scenario is much less. Could you discuss this result in the text and provide some insights on why the scenario sensitivity varies so much for some variables?
- P23, L12 – I wouldn't say "farther upwind", it's more like over the windward slope
- P25 – For Fig 9c, what do you think is happening very far downwind approaching the rightmost boundary, why does ICAR get drier with height?
- P26, L3 – Don't use "observations" here, I think it should probably be "both simulations"
- P26, L3-4 – This sentence was confusing to me. What is meant by "close to the surface"? upwind or downwind? Are you referring to the windward and leeward slope from the previous sentence?
- P26, L6 – Please reference Fig 11a after "upwind of the ridge"
- P27, L19-20 – So ICAR-N is making more cloud ice than cloud water, right? To trigger autoconversion you would need to reach a certain threshold of cloud water mixing ratio, then the scheme should convert water vapor to cloud water. This would make sense for WRF since the vertical velocities are faster over the windward slope relative to ICAR-N. Is there a significant change in the height of the freezing level upwind that could potentially impact the development of cloud ice in ICAR-N? What is the ice nucleation process in the scheme, i.e., what conditions must be met to convert water vapor to cloud ice? Do you think perhaps the Bergeron-Findeisen process (cloud ice grows at the expense of supercooled cloud water, leading to conversion from cloud ice to snow, and subsequent depositional growth of snow?) is more prominent in ICAR-N, thus leading to more cloud ice than cloud water in the suspended hydrometeors?
- P33, L18 – How computationally frugal is ICAR compared to WRF simulations in the case study explored here? Can you provide comparison of computational costs and wall-clock time?

Technical corrections
[P]age#, [L]ine#

- P4, L 7 – remove the word "eventually", removing it still keeps the same message and the word is unnecessary
- P5, L23 – change "is" to "are"
- P8, L3 – rewrite to "Since ICAR does not currently support…"
- P15 – the caption for Figure 1 seems incorrect… "Perturbations of the horizontal perturbation", should this be "Vertical cross-sections of the horizontal perturbation"?
- P20, L3 – it should be "Fig. 6a-g"
- P20, L23 – clarify that you mean spatial distribution of these quantities

- P21 – The blue contours in Figure 7a are difficult to distinguish for me, perhaps adding a different line type (dashed, dotted, etc) could help, or different colors that aren't so similar
- P24, L17 – should be Fig. 10d
- P24, L18 – should be Fig, 10b
- P26 – Can you have the isentropes be at the same interval and starting potential temperature as in Fig 9? This will facilitate comparison better.
- P30 – Caption for Fig 12, either the text is incorrect or panels c and d are mislabeled. In the text it says ICAR-O should be Fig 12c and ICAR-N is Fig 12d (P30, L1-2), but that's not how the panels are labeled. From the P24h results, it seems like panel d is ICAR-N, unless panel b is also mislabeled….please check these figures and make sure they're labeled correctly in the figure, in the caption, and in the manuscript text.
- P30, L5 – remove the "to" before "producing"
- P33, L18 – "an" should be "a" before "computationally"
- P35, L26 – delete "a" before "comparisons"
- P35, L28 – typo, should be "for"
- P35, L31 – typo, should be "following"
- P36, L1 – change to "produce"