Response to Reviewer 3

Abbreviations:

AR Author Response (Johannes Horak)

RC Reviewer Comment

Note that in this response orange bold text**, such as this,** or a red equation, indicates a change in the manuscript text (either an addition or some rephrasing). Blue or crossed out text ~~(example)~~ indicates the removal of text. Additionally, each modification in the manuscript is complemented by the respective page number and line where the modification occurred (e.g. **P01L01**). In the case where a table was altered, the respective modification is highlighted by an orange box around the modification. A modified figure is indicated by an orange border.

RC: 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.

AR:

We want to thank the reviewer for their time invested in reading through our manuscript, providing constructive criticism and feedback, as well as putting forward additional questions. We went through all reviewer comments and addressed the issues that were raised.

RC: 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?

AR: We clarified to "spatial shift" and rephrased the sentence to clarify that the shift is in relation to a simulation performed with an unmodified version of ICAR.

Adjustment to the manuscript

P01L22: The case study indicates ~~a large shift in~~ **that** the precipitation maximum ~~for~~ **calculated by** the ICAR simulation employing the developed recommendations ~~in contrast~~ **is spatially shifted upwind in comparison** to an unmodified version of ICAR.

RC: 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.

AR: We extended the corresponding paragraph to clarify.

Adjustment to the manuscript

P02L03: All numerical models of natural systems are approximations to reality. They generate predictions that may further the understanding of natural processes and allow the model to be tested against measurements. However, the complete verification **or demonstration of the truth of such** a model is impossible for epistemological **and practical** reasons (Popper, 1935; Oreskes et al., 1994). **While the correct prediction of an observation increases trust in a model it does not verify the model, e.g. correct predictions for one situation do not imply that the model works in other situations or even that the model arrived at the prediction through what would be considered the correct chain of events according to scientific consensus. In contrast, a model prediction that disagrees with a measurement falsifies the model, thereby indicating, for instance, issues with the underlying assumptions. From a practical point of view, the incompleteness and scarcity of data, as well as the imperfections of observing systems place further limits on the verifiability of models. The same limitations apply to model evaluation as well, however, evaluation focuses on establishing the reliability of a model rather than its truth.**

RC: P2, L30 – "cannot be inferred from", why? What other information is needed?

AR: We rephrased the corresponding part of the paragraph since, without more context, this statement may be considered misleading.

Adjustment to the manuscript

P03L03: Therefore, a direct comparison to a full physics-based model is generally not sufficient for an evaluation of ICAR~~. Note that~~ **since** ICAR is not intended to provide a full representation of atmospheric physics. Furthermore, whether the results obtained from ICAR simulations are correct for the right reasons cannot be inferred from ~~comparisons to measurements alone (Schlünzen, 1997)~~, **for instance, precipitation measurements, alone. Similar spatial distributions of precipitation may result from a variety of different atmospheric states. Therefore, the modelled processes yielding the investigated results need to be considered as well.**

RC: P3, L14 – "distribution of precipitation", is this referring to spatial or temporal distribution?

AR: We rephrased the sentence to clarify to "spatial distribution of precipitation".

Adjustment to the manuscript

P03L22: This study aims to improve the understanding of the ICAR model and develop recommendations that maximize the probability that the results of ICAR simulations, such as the **spatial** distribution of precipitation, ...

RC: 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".

AR: We went through all eight instances of this phrase used in the manuscript and added additional clarification in one case and removed the phrase in three cases. Note that the phrase is used once in the abstract, five times in the introduction (not counting the direct quote from Zhang et al., 2013), once Section 4.6 and once in the conclusions. A variation of it was used in Section 3.6 and has been rephrased.

Adjustment to the manuscript

P03L21: This study aims to improve the understanding of the ICAR model and develop recommendations that maximize the probability that the results of ICAR simulations, such as the **spatial** distribution of precipitation, are correct **and caused by the physical processes modelled by ICAR and not by numerical artifacts or any influence of the model top (correct** for the right reasons**)**.

Adjustment to the manuscript

P03L25: For a given initial state, a correct representation of the fields of wind, temperature and moisture as well as of the microphysical processes are a necessity to obtain the correct distribution of precipitation~~ for the right reasons~~.

Adjustment to the manuscript

P15L18: **Section 4.6** additionally investigates whether this seemingly optimal result, as suggested by the lowest mean squared errors, was achieved~~ for the wrong reasons. ~~**due to the low model top potentially influencing the microphysical processes within the domain and the calculation of N being based on the perturbed fields.**

Adjustment to the manuscript

P36L27: Hence, it seems that the underestimation in precipitation near the crest and to its lee of an ICAR simulation with reasonably high model top compared to WRF (Fig. 9) is partly compensated in an ICAR simulation with a too low model top (ICAR-$O_{4\,km}$ in Fig. 14) by spurious effects introduced by the upper boundary conditions. ~~It follows that the seeming imrpovement in the latter case is right but for the wrong reasons. ~~**Note that this seeming improvement is not due to a more realistic representation of cloud formation processes.**

RC: P5 – How frequent is the forcing timestep compared to the model time step?

AR: For the presented idealized simulations, where a forcing time step was 6 hours and the model time step of ICAR approximately 40 s, the ratio is roughly 540 model timesteps per forcing time step. The ratio was similar for the real case scenario. We added the model timestep durations to the section “Simulation Setup”.

Adjustment to the manuscript

P09L14: The model time step of ICAR is automatically calculated by ICAR to satisfy the Courant-Friedrichs-Lewy criterion (Courant et al., 1928; Gutmann et al., 2016) and is approximately 40 s while for WRF it is set to 2 s.

RC: P7 – What is the model time step used for the ideal case configuration?

AR: We added another sentence to indicate both, the ICAR and the WRF model time steps.

Adjustment to the manuscript

> **P09L14: The model time step of ICAR is automatically calculated by ICAR to satisfy the Courant-Friedrichs-Lewy criterion (Courant et al., 1928; Gutmann et al., 2016) and is approximately 40 s while for WRF it is set to 2 s.**

RC: 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.

AR: The moisture is indeed set to RH=100% across the entire vertical column. The main reason behind this choice was to maximize hydrometeor concentrations and the precipitation within the domain. We rephrased the sentence to put more emphasize on the fact that RH=100% across all vertical levels. Due to the slight orographic lifting throughout the domain upwind of the topographical ridge, the saturation drops below 100% almost immediately after onset of the simulation and stabilizes during the 18 hour spinup such that the cloud formation then only occurs when the flow encounters the barrier.

Adjustment to the manuscript

> **P10L04:** For the comparison of the ICAR and WRF wind fields to an analytical solution, dry conditions with RH = 0 % are employed while otherwise saturated conditions with RH = 100 % are prescribed **throughout the vertical column** at all heights.

RC: P13, L31 – I don't see the "slight distortions" you speak of, that said, what is the physical importance of this distortions?

AR: We rephrased the respective sentence to clarify that the description referred to the slight differences of the ICAR-N u' field in comparison to the analytical u' field and that this results in correspondingly higher wind speeds in the referred region.

Adjustment to the manuscript

> **P16L21:** In comparison to the analytical fields (Fig. 1a) the u' field in ICAR-N exhibits **slightly lower values of u'**, particularly visible in the region where u' < 0 m s^{-1} from approximately 8 km upward, **resulting in higher horizontal wind speeds in this region** (Fig. 1b).

RC: 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.

AR: We rephrased accordingly to clarify.

Adjustment to the manuscript

P16L28: The reason for the relatively small difference to the MAEs of ICAR-N is that the MAE ~~is calculated~~ **calculation** across the entire cross section ~~while the largest deviations are localized in a comparatively small region~~ **averages out the large deviations in the small spatial area** around the topographical ridge at the center.

RC: P14, L14-15 **–** What is meant by "an elevation dependence"? Explicitly describe these features and why they are relevant to note.

AR: We rephrased according to the reviewers suggestions.

Adjustment to the manuscript

P17L04: The amplitudes in the perturbation fields in WRF are larger and exhibit ~~an elevation dependence. For u'~~ **the elevation dependence indicated by Eq. (21). For w', for instance, the amplitude increases by 0.7 ms^{-1} from 4 km to 10 km, resulting in an increased orographic lift compared to ICAR. The** range of observed values **for u'** ...

RC: P16, L8 **–** Potentially repetitive sentence starting with "Potential temperature…", this statement was essentially said on L3-4.

AR: We removed the sentence as it was indeed a repetition of what was said above.

Adjustment to the manuscript

P19 First Paragraph: ~~Potential temperature fields are improved the most when a CG BC is imposed on θ (Fig. 2a).~~

RC: 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?

AR: The heating of the upper levels was at least ~300K and could potentially be more. However, ICAR enforces a maximum potential temperature value and outputs warnings to inform the user of exceedingly high or low (thresholds are user definable per quantity) values of quantities and the associated problems with a particular simulation. We hypothesize that under highly stable conditions the constant gradient boundary condition drastically overestimates the potential temperature in the level above the model domain, thereby advecting hot air into the uppermost level(s). While this might not be problematic for just a couple of time steps over multiple model timesteps the Theta gradient between the topmost two levels drastically steepens, and facilitates the calculated influx of hot air, eventually causing a numerical instability.

RC: 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?

AR: The differences mainly stem from scenarios that generate clouds with a large vertical extension. We added an additional paragraph to discuss this observation.

Adjustment to the manuscript

P24L03:

Note that the spread of RE in dependence of z_{top} (Fig. 6) for $\overline{q}_{sus}$, $\overline{Q}_v$, $\overline{Q}_{sus}$ and P_{12h} is mainly caused by scenarios that generate clouds with large vertical extensions. To better approximate the microphysical processes in the scenarios, and the resulting distribution of precipitation, higher model tops are required, leading to the observed spread. This affects, in particular, $\overline{Q}_v$, $\overline{Q}_{sus}$ and $\overline{Q}_{prc}$ since missing vertical levels may significantly impact the total masses. In addition, note that while total masses are always compared to the respective mass found in the reference simulations, $\overline{q}_v$, $\overline{q}_{sus}$ and $\overline{q}_{prc}$ can only be compared within the vertical extent simulated by the simulation with the lower model top.

RC: P23, L12 – I wouldn't say "farther upwind", it's more like over the windward slope

AR: We rephrased the paragraph to better clarify and avoid misunderstandings.

Adjustment to the manuscript

P27L11: With respect to water vapor ICAR-N is drier upwind of the topographical ridge and wetter downwind in comparison to WRF (see Fig. 9a-c). The regions with this dry and wet bias extend up to an elevation of approximately 6 km in which, ~~farther~~ **up to 200 km** upwind of the ridge, ~~WRF~~ **ICAR-N** exhibits slightly stronger updrafts than **WRF. This stronger orographic lift in** ICAR-N ~~(Fig. 10c and d). Similarly~~ **yields a higher conversion rate of water vapor to hydrometeors. On the other hand,** above the ridge the downdrafts calculated by WRF are of a higher magnitude than those predicted by ICAR-N, see Fig. 10c and d. ~~Therefore, upwind of the ridge WRF transports more moist air from close to the surface to higher elevations. Above the ridge, on the other hand,~~ **Here,** WRF advects drier air from higher elevations to lower levels. Hence, the two large regions in ICAR-N exhibiting a dry and wet bias in q_v respectively are likely caused by the differences in the wind field. ~~However, a~~ **Additionally, a small region with a** wet bias close to the ~~mountain~~ **ridge** slope on the windward side is presumably caused by microphysical conversion processes (Fig. 10c).

RC: P25 – For Fig 9c, what do you think is happening very far downwind approaching the rightmost boundary, why does ICAR get drier with height?

AR: We rephrased the sentence addressing this issue to clarify.

Adjustment to the manuscript

P28L04: This low level dry bias is likely increased by ICAR-N, overall, extracting more precipitation from the moist atmosphere than WRF (see 4.5.2).

RC: P26, L3 – Don't use "observations" here, I think it should probably be "both simulations"

AR: We rephrased the corresponding sentence and, as a result, had to slightly modify the preceding part of the paragraph as well.

Adjustment to the manuscript

P30L02: Figure 11a illustrates that P_{12h} on the windward slope is substantially higher in ICAR-N than in WRF~~. Conversly~~ **and, conversely**, ICAR-N is drier along the leeward slope. ~~Both observations correspond~~ **This corresponds** well to ...

RC: 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?

AR: We rephrased the corresponding sentence for clarity.

Adjustment to the manuscript

P30L03: ~~Both observations correspond~~ **This corresponds** well to the distribution and shape of the precipitating hydrometeors ~~close to the surface~~ **above the windward and leeward slope** (see Fig. 9g and h) and the differences of q_{prc} between ICAR-N and WRF (see Fig. 9i).

RC: P26, L6 – Please reference Fig 11a after "upwind of the ridge"

AR: We added the corresponding reference.

Adjustment to the manuscript

P30L04: The precipitation maximum predicted by ICAR-N is approximately 25 mm and lies 6 km upwind of the ridge peak in comparison to the 32 mm maximum in WRF, which lies 4 km upwind of the ridge **(Fig. 11a)**.

RC 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?

AR: Yes, ICAR produces more cloud ice than cloud water. We assume that the reviewer was referring to ice nucleation or growth of cloud ice due to water vapor deposition, since autoconversion, to our knowledge, usually refers to conversion of cloud water to rain. Indeed WRF simulations exhibit a stronger updraft in comparison to ICAR above the upwind ridge (Fig. 10d), triggering the conversion of water vapor to super cooled cloud water. However, no, significant difference in the freezing level between ICAR and WRF is found upwind of the ridge.

RC P27, L19-20 – (continued): 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?

AR: The conditions for the onset of ice nucleation (Thompson, 2008) directly from water vapor are either that (i) RH exceeds saturation by 25% with respect to ice or (ii) saturated conditions with respect to water exist (RH >= 100%) and T < -12°C. Upwind of the ridge ICAR exhibits RH values between 100% and 125% with respect to ice and RH < 100% with respect to water. This is similarly found in the WRF simulation. Overall these atmospheric condition make it unlikely that the microphysics scheme directly converts water vapor to cloud ice and the similar atmospheric conditions would rather lead to the conclusion that ICAR-N and WRF should yield the same results.

Therefore, a potential alternative explanation is that in ICAR cloud water droplets heterogeneously freeze to cloud ice according to the mechanism described by Bigg (1953) with larger ice particles (>= 200 micrometers) being directly converted to snow, and Snow growing by depositional growth according to Srivastava (1992). However, in Bigg (1953) the probability of a droplet of cloud water freezing is related to its diameter and the air temperature. Droplets with higher diameters are more likely to freeze. In the Thompson MP scheme, the droplet size distribution is determined by the cloud water mixing ratio (e.g. Jones, 2014): Larger cloud water mixing ratios correspond to the median droplet diameter of the distribution. Note that WRF exhibits higher cloud water mixing ratios than ICAR-N and should therefore be more likely to convert cloud water to ice or snow. Overall, the issue remains inconclusive.

We have excluded, to the best of our knowledge, that differences in the implementation of the microphysics code in ICAR-N and WRF are the cause for the differences in the simulations:

We went through the Thompson MP code and compared the definitions of the variables definable in the ICAR options and the values of the constants defined in the first 386 lines of code. The only difference found was for the value of C_sqrd where ICAR Thompson uses 0.3 and WRF Thompson 0.15. We then ran simulations with the C_sqrd value set to 0.15 but this only yielded negligible differences in the simulation results for the idealized default scenario. Additionally we checked the code for differences in the modifications made since it was forked from the WRF repository. Where we found differences we undid the changes and tested whether the idealized simulations were affected – we did not find any indication that the functionality of ICAR Thompson differed from WRF Thompson. As an additional check we simulated the idealized default scenario with the WRF version from which the ICAR Thompson code was forked from (WRF-3.4) and noticed only negligible differences to the results obtained with the WRF 4.1.1 version.

We rephrased to better indicate the code review and WRF 3.4 simulations employed to rule out differences in the Thompson microphysics implementation.

Adjustment to the manuscript

> **P09L23:** The Thompson microphysics scheme as described in Sect. 2 is employed in all models. The **ICAR implementation of the Thompson MP was forked from WRF version 3.4. Preliminary tests were conducted, showing that WRF 3.4 and WRF 4.1.1 yielded the same results for the default scenario, with only negligible differences. Additionally, the** code of the Thompson MP implementation in ICAR and WRF **4.1.1** was reviewed and tested to ensure that ~~both implementations produce the same results for the same input~~ **differences between the implementations did not affect the results**. All input files and model configurations are available for download (Horak, 2020).

AR (continued):

We therefore attribute the different results of ICAR-N and WRF to subtle differences in the wind fields. Nonetheless, despite our many attempts to identify the process responsible for this difference, the results are not conclusive. Overall we believe that a more detailed analysis that would get to the bottom of this is outside the scope of this paper and that the focus of our manuscript was not the detailed investigation of the Thompson microphysics scheme. To better clarify that this is an open issue we added a sentence to the corresponding paragraph in the discussion.

Adjustment to the manuscript

> **P39L16: However, not all reasons for the differences could be identified, results remain inconclusive as to why ICAR-N mainly produces cloud ice while it is cloud water in WRF.**

Thompson, G., Field, P. R., Rasmussen, R. M., & Hall, W. D. (2008). Explicit forecasts of winter precipitation using an improved bulk microphysics scheme. Part II: Implementation of a new snow parameterization. *Monthly Weather Review*, *136*(12), 5095-5115.

Bigg, E. K. (1953). The supercooling of water. *Proceedings of the Physical Society. Section B*, *66*(8), 688.

Srivastava, R. C., & Coen, J. L. (1992). New Explicit Equations for the Accurate Calculation of the Growth and Evaporation of Hydrometeors by the Diffusion of Water Vapor, *Journal of Atmospheric Sciences*, *49*(17), 1643-1651. Retrieved Jan 3, 2021, from https://journals.ametsoc.org/view/journals/atsc/49

Jones, K. F., Thompson, G., Claffey, K. J., & Kelsey, E. P. (2014). Gamma Distribution Parameters for Cloud Drop Distributions from Multicylinder Measurements, *Journal of Applied Meteorology and Climatology*, *53*(6), 1606-1617.

RC: 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 wallclock time?

AR: While one of the main advantages of ICAR is computational frugality we considerate not to be the focus of the manuscript. Our intent was to develop recommendations that improve the reliability of the model and the representation of cloud formation processes in it. Therefore, we chose not to include this aspect in the, already long, manuscript since this would additionally require a discussion of the influence of other parameters in the setup (such as model time step for instance). Note that Gutmann (2016) state in their Section 5 that ICAR conducted simulations for their central US domain were 140-400 times faster than WRF. In our RH=100% case, the WRF simulation with a model top at 26 km that simulated a 30 hour period required approximately 300 core hours to complete (model time step 2 s). In contrast to that, the ICAR-N simulation with a model top at 20.4 km required 29 core hours (model time step approximately 40 s), and the ICAR-N simulation with z_{top}=10.4 km required 16 core hours (model time step approximately 40 s). Note that longer model time steps for WRF would have led to numerical instabilities.

RC: P4, L 7 – remove the word "eventually", removing it still keeps the same message and the word is unnecessary

AR: Due to a larger revision of Section 2.1 the corresponding sentence was removed.

RC: P5, L23 – change "is" to "are"

AR: Due to a larger revision of Section 2.2.2 the corresponding sentence was altered and now reads: **P07L16:** "**Note that** the vertical wind components ~~is defined as k-1/2 and k+1/2~~ **are**

calculated at half levels with Eq. (8) and that, in particular, no boundary condition is required to determine w at the model top."

P8, L3 – rewrite to "Since ICAR does not currently support…"

AR: Rephrased accordingly and now reads: "Since ICAR **does not** currently ~~does not~~ support periodic boundary conditions,"

RC: P15 – the caption for Figure 1 seems incorrect… "Perturbations of the horizontal perturbation", should this be "Vertical cross-sections of the horizontal perturbation"?

AR: We corrected the caption accordingly, the caption now reads: "~~Perturbations~~**Vertical cross-sections** of the horizontal perturbation wind component u' (top row) and vertical perturbation wind component w'"

RC: P20, L3 – it should be "Fig. 6a-g"

AR: We corrected the reference accordingly, the sentence now reads: "As shown in Fig. 6a-**g**, for most investigated quantities"

RC: P20, L23 – clarify that you mean spatial distribution of these quantities

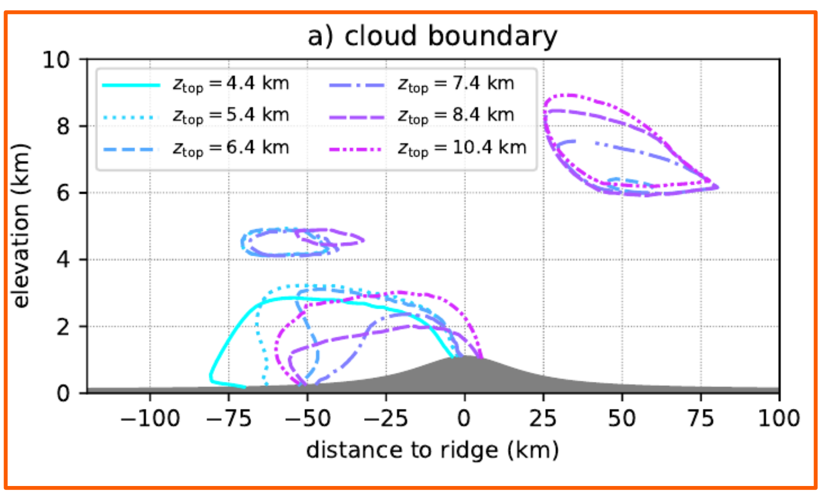
AR: We added the word spatial, the sentence now reads: "In other words, the **spatial** distribution of these quantities needs to be taken into account as well."

RC: 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

AR: We modified the linestyles in the figure to emphasize differences in the contours.

Adjustment to the manuscript

P25:



RC: P24, L17 – should be Fig. 10d

AR: We corrected the reference, see the next AR below for the adjustment to the manuscript.

RC: P24, L18 – should be Fig, 10b

AR: We corrected the reference accordingly.

Adjustment to the manuscript

P28L16:

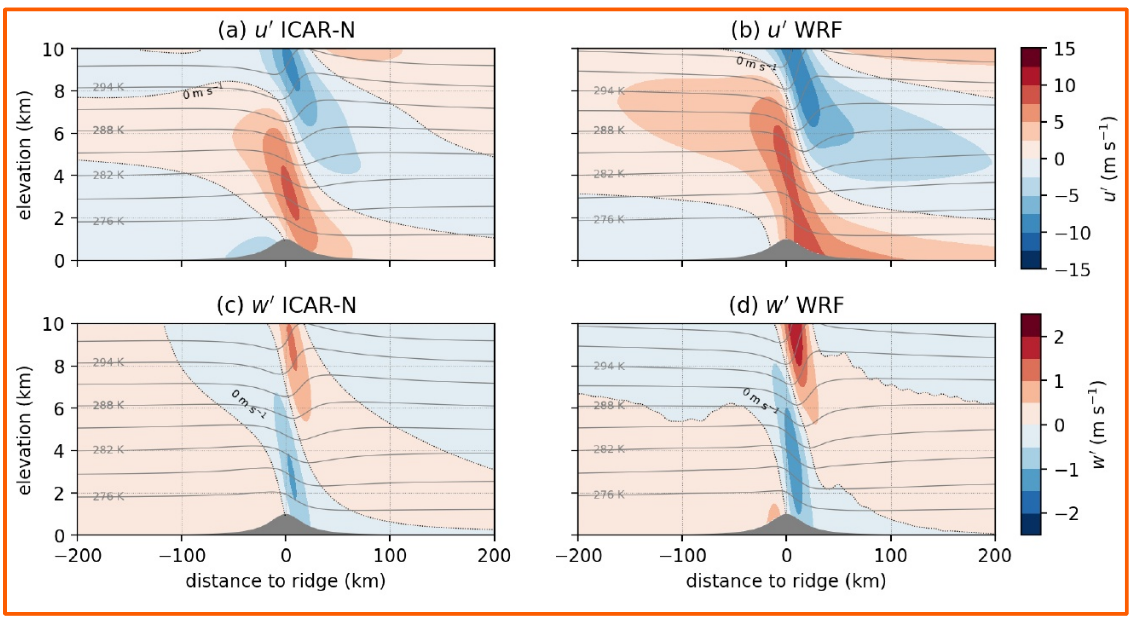
This is caused by a combination of two factors: (i) The higher vertical wind speeds above the windward slope of the topographical ridge predicted by WRF, lead to lower effective falls speeds of the hydrometeors (see Fig.10**d**). (ii) Higher horizontal wind speeds additionally contribute to a larger horizontal drift of q_{prc} and precipitation spill-over in WRF (see Fig. 10**b** and, for a basic estimation of the drift distances, Sect. 4.5.2).

RC: P26 – Can you have the isentropes be at the same interval and starting potential temperature as in Fig 9? This will facilitate comparison better.

AR: We plotted the isentropes in Fig. 10 to the same interval as in Fig. 9.

Adjustment to the manuscript

P30:



RC: 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.

AR: Note that due to panels for an additional simulation being introduced in Figure 12 (now Figure 13), we checked the according references once more and corrected where necessary. Please refer to the updated manuscript to view the changes.

RC: P30, L5 – remove the "to" before "producing"

AR: We removed as suggested, the sentence now reads: "The reason for ICAR-$O_{4\,km}$ ~~to~~ producing precipitation further downwind than ICAR-N can be found in the cross-sections of hydrometeor distributions shown in Fig. 14."

RC: P33, L18 – "an" should be "a" before "computationally"

AR: We corrected accordingly, the sentence now states: "ICAR is intended as a~~n~~ computationally frugal alternative to full physics models, in principle allowing for very low model top elevations."

RC: P35, L26 – delete "a" before "comparisons"

AR: We corrected accordingly. Additionally we added the word isolated to better indicate that the process resulting in the measurements is also of importance (which can of course be investigated by comparing to measurements). The sentence now reads: **P40L28:** "This additionally exemplifies why ~~a~~ comparisons to **isolated** measurements alone cannot determine whether the model results are correct for the correct reason."

RC: P35, L28 – typo, should be "for"

AR: We corrected accordingly, the sentence now reads: "Only a detailed consideration of the underlying processes can be the basis f**o**r such a conclusion."

RC: P35, L31 – typo, should be "following"

AR: We corrected accordingly, the sentence now reads: "The key findings and recommendations based on the extensive process-based evaluation of ICAR are summarized in the foll**o**wing:"

RC: P36, L1 – change to "produce"

AR: We corrected accordingly, the sentence now reads: "There is a minimum possible model top elevation Z_{min} to produce~~s~~ physically meaningful results with ICAR. If the model top elevation is lower, cloud formation and precipitation processes within the domain are affected by the model top."