Paper: wcd-2022-17 “Impact of combined microphysical uncertainties on convective clouds and precipitation in ICON-D2-EPS forecasts during different synoptic control”

General Comment.

The paper “Impact of combined microphysical uncertainties on convective clouds and precipitation in ICON-D2-EPS forecasts during different synoptic control” presents the impact of the perturbation of two parameters of the microphysics scheme, individually and combined, with respect to perturbation of initial and boundary conditions, in a convection-permitting ensemble based on the ICON model. The topic is an interesting one and the method of investigation proposed by the authors is sound and insightful. However, I have found the paper very difficult to read, partly due to the language and party due to the way the results are presented and discussed. The reader is not well guided through the interpretation of the results and some statements and conclusions not well justified. Since the material of the research is very good and the methodology for sure interesting for the scientific community, I suggest to the authors to carry out a major re-writing of their analysis and findings. I provided a list of points where improvements would be beneficial. I therefore recommend the paper for a major revision.

Detailed comments.

Page 1, line 5: inspect -> inspected. The abstract is a bit dense of data, it may be difficult for the reader to follow and get an informative summary about the paper content.

Page 2, line 22-23. “The uncertainties originate from the chaotic nature of the atmospheric flow …” This statement should be modified. The uncertainties originate from the unknown initial state and the imperfect representation of physical processes, as well as from approximations in the model, but not from the chaotic nature of the atmosphere. The latter is instead the responsible of the large effect that the (even small) uncertainties have on the weather forecast.

Page 2 line 27: the chaotic nature of convection: I guess the authors mean the stochastic nature of convection? Chaoticity applies to the atmosphere as a whole, therefore the sentence would not have the intended meaning.

Page 2 line 29-30: “An ensemble facilitates the distinction between systematic effects of perturbations and the chaotic signal.” I think that this sentence needs to be better explained, for example, how does the ensemble facilitate this distinction?

Page 2, line 31: represents -> is?

Page 2, lines 31-32: data assimilation systems are for initial conditions only, not for IBC.

Page 2 line 34: constitutes -> is constituted by? (or better: is)

Page 2, line 35: “and to represent the subgrid-scale variability.” Is this a reference to the intrinsically stochastic physics schemes?

Page 3 line 72: constitutes -> is constituted by?

Page 3, lines 80-81: please revise the language of this sentence

Section 2.1. I find a bit confusing the model description. ICON-D2 is the operational 2 km run at DWD, and not the model name, while you use here the ICON model, in its limited-area version, at 2km, model version 2.6.2.2.

Page 4, line 99: ICON-D2-EPS is not introduced, neither it is mentioned in Section 2.1 that an ensemble system is used. How is it built? I would suggest also here to refer to ICON-D2-EPS if the operational ensemble of DWD in mentioned, to an ensemble based on ICON at 2km if own experiments are made.

Page 6, line 143: I guess that something is missing in “is less about 2 hours”

Page 6, line 145: texture -> pattern?

Page 7, line 148: boundary -> boundary layer?

Page 7, line 159: spanned by diverse IBC?

Section 4: the first paragraph (page 7, lines 157-164) is very difficult to follow, could the authors explain better what they did? It is difficult to evaluate the results, since the method is not clearly described.

Page 7, line 166: prototype? Have prototype cases been introduced?

Figure 3: I suggest to describe the meaning of the plot, first, since it is not a standard visualisation method (though appropriate for this paper and insightful, once it is explained): what is the meaning of the dots being in the upper or lower part, what is the meaning of being grouped together or sparse, in order to guide the reader to an understanding of the results. In the text some conclusions are drawn from inspecting the plots but the reader cannot understand on what they are based. An example: “At first sight and independent of the prevailing weather situation, the IBC perturbations largely control the precipitation amount.” From what is this visible? I also suggest to revise the caption of the figure. In it is also mentioned “the nine coloured dots” but the coloured dots in the plot are many more than nine …

Page 9, line 180: where are the +3,5% and – 3,5% values visible? I think that in presenting this result it should be made the difference between the two extremes (polluted negative difference (less rain?), maritime positive difference)

Page 9, line 185-186: “Shape parameters of CDSD also exhibit a systematic impact in the weak forcing situation”: from what is this visible? The dots with different nuances of the same colour are more disperse, and systematically? Please mention this explicitly to help the reader.

Page 9, line 191: can you explain better what is meant with reverse, with respect to which order?

Page 9, lines 192-195: I do not understand the meaning of this sentence, could this be clarified?

Page 9, lines 197-200: I would suggest to explain more clearly what is shown in Figure 4, it is very difficult to follow (e.g. does “a sub-ensemble mean sharing the same unperturbed parameters” mean “the mean of the sub-ensemble sharing the same unperturbed parameters?”)

Page 10, line 213: to +22% to -20% ?

Page 10, line 219: “the interquartile range becomes smaller than those…”: which interquartile range? I guess the one of the MP sub-ensemble, gray bars? Please specify. Does this also have a meaning? Perturbing the 2 microphysical parameters together (gray bars) leads to larger extremes than perturbing only one of the two parameters, but to smaller interquartile range than perturbing CCN with ν=2 and 8, as said. Can this also be explained?

Page 11, line 234-235: please substitute from “three of them” with “the experiments in panels b, c and d share with the experiment in panel a identical IBC, CCN concentration and shape parameter of CDSD, respectively”.

Page 11, line 240: I am not sure if Luxemburg can be identified clearly on this map by a non-European reader, I suggest to change the reference (using lat lon lines? A red circle?)

Page 11, lines 242-250: I do not understand from what can be evaluated the effect of the CCN and CDSD perturbations from the plots of Figure 5. Comparing panels a and b one can see the impact of changing the shape parameter in the polluted setup, while comparing a and c one can see the impact of changing the CCN condition with shape parameter equal to 8. Therefore, from where can be evaluated that “The relatively small impact of CDSD perturbations in maritime CCN conditions”, when there is only one plot referring to maritime conditions? And where is it shown that “Positions of strong rain cells are shifted by the CCN perturbation at a scale of 20-30 kilometres, whereas an increase of the shape parameter of CDSD hardly shows a clear difference.”? Plots b and c look anyway very similar. Given to this, the conclusion drawn in lines 247-250 cannot be judged.

Page 13, line 260: why should the choice of the 99th percentile of precipitation as threshold guarantee that the number of grid points used for FSS calculation is constant?

Page 14, lines 275-279: I suggest to move this specification after Figure 8 has been introduced.

Page 17, lines 340-344. From panels 8c and 8d it appears that the impact of the CDSD perturbation starts to appear only at 7 UTC, while for CCN it only starts at 6 UTC. Since, as pointed out by the authors, there is continuous rainfall, this characteristic is a bit puzzling me. I understand the argument that microphysical perturbations may need a longer spin-up time to modulate the fields, but it seems to me a bit too regular, and the same for all the 3 experiments in each case. On top, the gray thin lines, for all the possible combinations of members, also start abruptly at, likely, 5 and 4 UTC respectively. I would suggest to the authors to check what happens here, since I am not sure that this can be explained by the physical spin-up of the perturbations.

Figure 9. Caption: in c is “total column rain water content”. The unity of measure for cloud fraction is missing.

Section 4.3. When the plots are described, the experiments are mentioned with their label (e.g. nu8c) but there are no labels in these plots. I would therefore suggest to mention the colours, instead, or add the labels. Actually this problem has presented itself in other points through the paper.

Page 17, line 355. “These values are much larger compared to the impact of microphysical perturbations on precipitation.”: I agree with the authors that the effect on TQC is remarkable, in particular when the variability of the IBC sub-ensemble values is compared with the variability between different setup of the microphysics parameters. However, it is difficult to compare even the relative increase or decrease of TQC with the one of precipitation, since the two variables and not directly comparable, in terms of unity of measure and scale of their distributions. I suggest to find a different formulation to express the result, maybe lines 356-358 are already enough.

Page 18, lines 360-361: The meaning of this sentence is not clear.

Page 19. Since the description of Figure 10 starts from the bars on the right, I suggest to signalise this to the reader, e.g. at line 391: “total precipitation, TP (right column of Figure 10)”. In the text is also mentioned a Figure 10a, not present.

Page 19-20, lines 404-405: these figures are not visible form the plot, please remove the sentence.

Page 20, lines 410-411: this result cannot be discuss from Figure 10, please remove it.

Page 20, lines 422-425: I would suggest to remove these lines, since these numbers are likely dependent on the considered cases and anyway very difficult to compare (see my comment before).

Page 20, line 426: I do not think that the microphysical uncertainties have been in this work implemented in the operational ICON-D2-EPS, right?

Page 21, lines 440: I do not understand the meaning of this sentence. Does it refer to the usefulness of a probabilistic approach at all? And how uncertainty would be quantified, in that case?

Page 21-22, Summary and concluding remarks. There are some points in this section which I would ask the authors either to reconsider or to reformulate, but since the conclusions are mainly drawn from the results presented in section 4, which I suggest to consider for a major modification, I prefer not to go into details of Section 5 at present. I suggest to re-write Section 5 once the results are presented in a clearer way.