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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparing the impact of environmental conditions and microphysics on the forecast uncertainty of deep convective clouds and hail" *by* Constanze Wellmann et al.

Anonymous Referee #2

Received and published: 27 August 2019

General Summary: This well-written work explores the environmental and microphysical uncertainties that produce the largest variability in deep convection and hailfall characteristics. The authors make an excellent point that the impact of such uncertainties needs to be considered not only in isolation, as previous studies have largely done, but also in relationship. Hence, the work will be an excellent contribution to the literature. There are a few points about the effect of the chosen microphysical parameterization and comparison of variability to other studies that I would like clarified, but generally I support the acceptance of the article pending minor revisions.

Specific comments: 1. These results have to potential to be highly related to the choice

of microphysical scheme, and as such, this impact needs to be discussed in the paper. How much of an effect do certain choices made in the microphysical scheme have on these results – for example, could the chosen hail/graupel ice collection efficiency relationships affect the high variability found to be associated with the ice multiplication coefficient? What about the density of the rimed ice? (Frankly, I was surprised that wasn't chosen as an input parameter to vary as multiple studies have noted its importance; e.g. Morrison et al. 2015, JAS.) Most importantly, are these results transferrable to other double-moment microphysical parameterizations with a similar number of classes, or are they unique to this scheme alone?

2. The environmental condition input parameters (surface potential temperature and wind shear) are varied over a smaller range than most environmental sensitivity test studies. The authors explain this range of environmental conditions was chosen as it corresponds to typical environmental uncertainties seen in COSMO. Limiting the perturbation to that range is important, in my opinion, as it allows the work to make judgements about which model improvements are most likely to improve simulations of convection and hailfall. However, the results cannot be compared to other works examining the impact of the full range of environmental conditions that can produce hail, such as Dennis and Kumjian (2017) and Storer et al. (2010), without explicitly comparing the ranges of inputs of environmental conditions in all studies. I would like the article, especially the abstract, to emphasize that the input range of environmental conditions is only meant to encompass model uncertainty.

To that end, more information about how the variations in surface potential temperature and the scaling factor for 0-6 km shear translate to typically cited ambient environment conditions, such as CAPE and 0-6 km shear, would be helpful. The shear in particular is important given the results of Dennis and Kumjian (2017). A hodograph plot showing the range over which the shear profile was varied would be useful.

3. I'd like more information about the emulators, inputs, and training data. Pg. 8 line 6 mentions using a "choice of input combinations of the parameters" to train the

emulator. What combinators are selected and how is the choice made? How many simulations were required to train the data, and what outputs were used? How many emulations were eventually produced – one for each possible combination of input variables? Are the 10,000 realizations of vertical heating profiles produced using the same combination of input parameters and the same emulator method?

4. The discussion of the variability of the hail size distribution caused by the different input parameters focused solely on the maximum and minimum number concentrations and not the distribution of responses within those bounds. Within Fig. 6, could box and whisker plots be used to show the distribution of number concentrations within each of the three setups within a set range of size bins? That would allow the distribution of distributions, as it were, to be discussed.

To Figs. 3a and 6 I'd also like to see added the range over which N(D) and D are allowed to vary within the microphysical scheme used, for the range of ïĄő used. That would place the amount of variability in context. I'd also like to see Fig. 3b repeated with the data analyzed in Setup 3 and Fig. 6, as I feel it lets the reader more easily grasp the key ingredients in the output variability.

Minor comments: Pg 4, line 7: Do the authors feel 1 km is of a fine enough resolution for this study? From the literature, would they expect any of the results to change if this resolution were reduced?

Pg 4, line 15: A quick sentence here clarifying the difference between saturation adjustment and explicit diffusional growth would be helpful.

Pg 6, Table 2: Over what intervals were these values varied?

Pg. 7, line 11: Why is mu held constant?

Pg. 7, line 16: "chosen such that the most important parameters. . .are considered" – how were these chosen?

Pg. 9, lines 4-7: Nice description.

Pg. 9, Section 4.1, lines 15 – 5: The units of these variables need to be included. Is hail at ground and max hail at ground accumulation-based over the final 5 hours of the simulation? Are the mean and maximum values mentioned calculated in both space and time? Is precipitation rate of hail a flux of the mixing ratio through the lowest model level?

Pg. 10, Figure 1: Reorder the colors in the bar plot so they are the same order as the legend – many of them are similar shades.

Pg. 10, line 24: "to examine how the simulated storm impacts the ambient conditions" is an odd phrasing. "Ambient", to me, indicates the environmental air surrounding the convection. Diabatic heating profiles can modify this region through gravity waves and other atmospheric responses, but here the authors are focused on in-storm effects. I would reword to "examine how the heating profiles of the simulated storm change".

Pg. 11, line 7: "covering the whole parameter space" – is this the whole input parameter space?

Pg. 12, line 4: Instead of number density, should this be number concentration?

Pg. 12, line13: when referring to the "lowest number concentrations of hail", to what diameter are you referring?

Pg. 12, last two sentences: From this figure, it appears to me CCN has a larger effect than the strength of the ice multiplication.

Pg. 13, Table 3: Per Khain et al. 2011 (Atmospheric Research), the signal of CCN changes associated with hail fall switches sign around 3000 cm-3. Do the authors think their results are in line with this study?

Pg. 14, lines 3-4: I would argue the contribution due to CCN is larger in both S1 and S2 than S3.

Pg. 15, lines 34-40: See specific comment #2.

Pg. 16, line 10: See specific comment #2.

Pg. 15, second line 5: it seems like the authors are arguing there is a difference between “the cloud” and “the integrated amount of cloud water” in the Storer et al. (2010) study. Could they explain what his difference is?

Pg. 16, Fig. 5: I’m unable to see the different standard deviation distributions. Perhaps instead a similar plotting method as in Fig. 6, with individual lines of different styles marking the edges of the standard deviations.

Pg. 17, lines 28-29: Saturation adjustment was not one of the input parameters selected to test, so how can the authors make this claim?

Pg. 17, lines 3-4 (just before section 5.3): See specific comment #2.

Pg. 18, lines 17-18: Can this sentence be worded more clearly? Perhaps “the spread of the distributions in S2 is larger than S1, particularly for smaller diameters of hail.” Can the authors comment why the uncertainty is so much larger for smaller diameters?

Pg. 19, first and second lines: The authors note the relationship between low fall velocity of graupel and high number concentrations of hail several times in the paper. Could they provide a physical explanation for this relationship?

Pg. 19, line 20: “than the inputs related to environmental conditions”. . .on the scale of uncertainty seen in COSMO. See specific comment # 2.

Pg. 21, line 23-26: An excellent summary of the potential impact of this research – but it depends on the transferability of the results to other microphysical parameterizations (see specific comment #1).

Typographical: Pg. 4, line 31: Add a comma after profile. Pg. 10, Eq. 3: instead of nu/s, which denotes division, I’d use nu,s. Pg. 10 line 34: “such that”–> where

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