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Discussions

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Comment on acp-2022-322

Anonymous Referee #1

Referee comment on "Impacts of combined microphysical and land-surface uncertainties on convective clouds and precipitation in different weather regimes" by Christian Barthlott et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-322-RC3, 2022

This paper examined the precipitation and updraft changes over Germany and parts of neighboring countries to four different parameters: CCN concentration, shape parameter, soil moisture, and synoptic forcing.

The first result section is a series of descriptions of what's shown in the simulations rather than a coherent synthesis/comparison. The reviewer strongly suggests that the authors develop a better way of organizing and displaying the results, particularly for the precipitation responses to three perturbed parameters. The authors need to separate materials into different subsections focusing on one perturbed parameter at a time and demonstrating the synergistic effects explicitly. Furthermore, the results are discussed when one of four parameters is constrained. For example, Figure 6 and associated texts illustrate the impact of soil moisture under different synoptic setups. As such, examination of the synergistic/interactive effects of more than two perturbed parameters is absent, which is critical in terms of adding novel findings to the existing literature on modulation of convective responses by interactions between aerosol and soil moisture (or other environmental parameters). Addressing the synergistic is imperative since the title is Impacts of "combined" microphysical and land-surface uncertainties. Also, since the simulation results are quite different depending on the synoptic setting, the reviewer recommends authors to revise the title to include the synoptic aspect.

Finally, the third result section only examines the updraft at 5 km, which is insufficient information to examine/determine convective invigoration. The authors need to consider expanding the updraft analysis to different levels or exclude this section from the paper. The following texts are point-by-point comments.

Table 1: What is the vertical resolution? Is it varying or constant throughout the column? If varying, what is the vertical resolution near the surface, which could impact the boundary layer processes.

Lines 121–123: Are these CCN concentrations based on the observation? The reasoning for selecting these numbers should be demonstrated clearly with appropriate references.

Line 156: maritime, intermediate, continental, and polluted are not intuitive and misleading unless these CCN values are based on observations over the maritime and continental part/airmass over the simulation domain. I suggest naming them CCN100, CCN500, CCN1700, and CCN3200, for example. If this is too long, please come up with something more informative. It's tough to remember whether continental is lower or higher than polluted. All the values are different degrees of pollution/aerosol loading.

Lins 168: Could you add one or more sentences explaining the definition of convective adjustment time scale?

Table 2: The authors need to include the justification for choosing 0.17 and 1.09 as weak strong forcing cases. Unlike weak forcing cases, where the convective adjustment time scales have only ~ 11% difference, the two numbers chosen for the strong instances have more than 146% difference. These could have impacted the dramatic deviation from the reference shown in Figure 5b, first panel. A similar concern goes for Figure 6, rows 3 and 4, where the diurnal behavior looks pretty different between these two forcings compared to similar peaks and shapes shown in rows 1 and 2.

Line 181: While the agreement between simulated precipitation and radar observations is not shown here, could you elaborate more regarding what radar observations were used and what parameter (e.g., accumulated rainfall, hourly precipitation rate) was chosen to make this comparison?

Line 188: Why did you choose "domain-integrated precipitation totals" to represent the precipitation response? The black boxed area in Figure 1 includes land with substantially different orography (the south edge of the domain and the north edge of the domain), water, and coastal regions, which all could show very different precipitation characteristics. Especially under weak synoptic scenarios, the coastal rain process could impact the domain-integrated precipitation.

Lines 191–195: Was this soil moisture response linear? Several pieces of literature have shown the nonlinear characteristics of the soil moisture impact on convection. This is also related to the limitation of the current study's design. I understand the computation limitation with many simulations considering the non-linearity of soil moisture and other parameters. However, the authors should demonstrate discussion on nonlinear responses found either in this study or in previous studies more clearly in this manuscript. Please check Drager et al. (2022) "A Non-Monotonic Precipitation Response to Changes in Soil Moisture in
the Presence of Vegetation" for this comment.

Lines 218–219: These referenced studies all used the COSMO model. And the paper you cited earlier, Marniescu et al. (2021), showed the different convective responses to enhanced aerosol loading resulting from different models. So the authors need to look into other papers that used the various numerical models and examined aerosol-induced convection changes under different synoptic setups.

Lines 231–234: Is there any way to show this using figures? For example, changes in instability between different simulations? While the explanation authors put here makes sense, it's merely speculation without supporting simulated results. The same goes for Lines 236–244. Where is the supporting evidence for cloud size changes?

Figure 6: Color shadings, instead of colored lines, are hard for readers to compare responses among different aerosol loadings. Please consider other ways of representing four aerosol loadings for clarity.

Line 256: Since Figure 6 only shows the shape parameter-averaged response, the (WETp0 minus WETp8) is not demonstrated via any figure or table. Could you also consider including different responses as a function of the shape parameter?

Line 284: Please also consider including other references not involving the first author.

Lines 363–365: Please explain why the authors examined this ratio. How does this ratio tell about latent heat release/updrafts? This ratio seems only relevant to the relative dominance of cold rain.

Line 377: the sensitivity to different shape parameters -> the sensitivity of w 5km(?) to different shape parameters; since there are sensitivities in the first and second rows as a function of the shape parameter.

Lines 383–384: Please include a table or a figure showing the mean updrafts to support these statements. At least, the authors should include the numbers of mean updraft value ranges or distribution.

Lines 390–391: Do you mean there is no evidence of convective invigoration (or suppression) throughout all vertical levels? No warm-phase invigoration either? Several recent studies (e.g., Igel and van den Heever, 2021 and references therein) have shown the robustness of the warm-phase invigoration, whereas the cold-phase invigoration is not robust but depends on the environment.

Line 400: Please check Grant and van den Heever (2014) for how they computed synergistic effects when multiple parameters were perturbed.