

Responses to the reviewers

Impacts of combined microphysical and land-surface uncertainties on convective clouds and precipitation

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We thank both reviewers for reading the manuscript and providing detailed comments. We have carefully considered all comments and changed the manuscript accordingly. Please find below our responses in blue.

Reviewer 1

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.

We appreciate the suggestion from the reviewer but believe that a separation into different subsections focusing on one perturbed parameter alone would rather complicate the demonstration of the synergistic effects. We also believe that the synergistic effects are visible from the precipitation deviations (Fig. 5) and precipitation rates (Fig. 6), because all realizations of the ensemble are visualized here and the synergetic effect of changing 3 parameters can be identified. In order to better differentiate between the individual and combined impacts, we changed the marker type in Fig. 5 to distinguish between single effects (circles), double synergies (stars) and triple synergies (triangles). Reviewer 2 recommended to include the individual contribution to the spread from the 3 different parameters and we included a new Figure 7 for that. We believe that this new content is related to this reviewer's comment as well. Based on the last review comment (check Grant and van den Heever (2014) for how they computed synergistic effects), we applied the factor separation methodology of Stein and Alpert (1993) to our simulation results and give an example here (Fig. R.1). As already mentioned in the article of Grant and van den Heever (2014), the interpretation of the results obtained from the factor separation calculations, particularly for a field that has a finite range like total precipitation, is not always trivial. We find that all double synergies work to enhance accumulated precipitation, whereas all triple synergistic interactions reduce the precipitation in agreement with findings from Grant and van den Heever (2014) who studied the impacts of soil moisture, CCN concentrations, and roughness length on tropical sea breeze convection. The magnitudes of all double and all triple synergies are quite similar, respectively. The single impacts displayed in blue are much smaller and are correlated

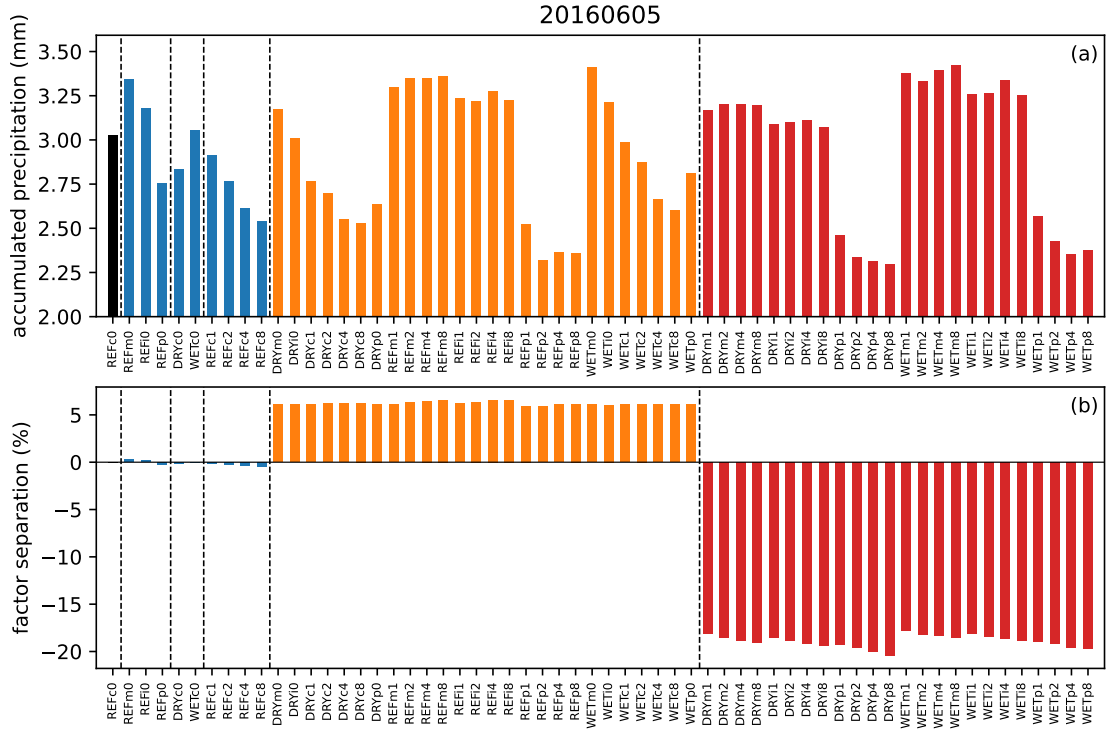


Figure R.1: Factor separation analysis of single impacts (blue), double synergies (orange), and triple synergies (red) for the case of 05 June 2016.

to the precipitation amount. The double and triple synergy terms are not simply the differences in rainfall, but rather represent the contributions of the synergistic interactions that occur (Grant and van den Heever, 2014). The focus of this study was on the impacts of combined effects, i.e. the different response of cloud and rain formation. To identify and quantify the involved nonlinearities is out of the scope of the present study and could be pursued in future work. Therefore, we decided not to include an additional section on the factor separation methodology with a figure, but mentioned the main outcome at the end of section 3.1.

“The spread of the model results and the impact of double and triple synergies was demonstrated so far with precipitation deviations, precipitation intensities, and the normalized ensemble spread. Although the quantitative interpretation of nonlinear interactions is not the main focus of this study, we used the factor separation methodology of Stein and Alpert (1993) in order to understand how aerosols, the shape parameter, and soil moisture may interact synergistically. This methodology is a simple way to show how multiple factors and their nonlinear interactions influence a predicted field and has been applied many times in atmospheric sciences, e.g. for aerosol-cloud-land surface interactions within tropical sea breeze convection (Grant and Heever, 2014) or for the effects of topography, convection, latent, and sensible heat fluxes on Alpine lee cyclogenesis (Alpert, 2011). For the four cases analyzed here, we find that the single impact of changing one parameter has a much weaker response as the double or triple synergies (not shown). Furthermore, all double synergies work to enhance accumulated precipitation, whereas all triple synergistic interactions reduce the precipitation in agreement with findings from Grant and Heever (2014). The triple synergies are greater than the double synergies by a factor of approximately three. Whereas the factor separation for the single impacts is always correlated to the rainfall difference compared to the respective reference run, the double and triple synergy terms rather represent the contributions of the synergistic interactions that occur. We must emphasize that synergy terms may not be meaningful for a field that has a finite range like total precipitation and when individual impacts of one parameter dominate the change of precipitation. Nevertheless, the results from the factor separation and ensemble spread shown before demonstrate the importance of

considering synergistic effects for convective-scale predictability. ”

Conclusions:

“Similarly, the factor separation methodology showed a larger impact of triple synergies to the simulation results compared to double synergies or single impacts which demonstrates the importance of considering synergistic effects for convective-scale predictability.”

As recommended, we changed to title of the manuscript to “Impacts of combined microphysical and land-surface uncertainties on convective clouds and precipitation in different weather regimes”.

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.

Please see our reply to the second to last comment of the reviewer. We included profiles of updraft velocities for each day where one can see a systematic reduction of updraft velocities across all vertical levels.

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.

We use a vertically stretched grid with 14 levels in the lowest 1000 m, which is assumed to be high enough for representing boundary-layer processes. We included this information in Table 1.

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

There are 4 different concentrations of cloud nuclei available in the Segal-Khain activation. We included another reference to this paper and new reference to observations in central Europe. The text now reads:

“Pre-calculated activation ratios stored in look-up tables (Segal and Khain, 2006) are used to compute the activation of CCN from aerosol particles. The condensation nuclei are all assumed to be soluble and follow a bi-model size distribution (Seifert et al., 2012). Using the Segal and Khain (2006) activation, four different values of the number density of CNN (N_{CCN}) are available, representing maritime ($N_{CCN} = 100 \text{ cm}^{-3}$), intermediate ($N_{CCN} = 500 \text{ cm}^{-3}$), continental ($N_{CCN} = 1700 \text{ cm}^{-3}$), and continental polluted conditions ($N_{CCN} = 3200 \text{ cm}^{-3}$). Typical conditions of central Europe are represented by the continental aerosol assumption (Hande et al., 2016).”

- 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.

We believe that the suggested naming is too long. It might work good in the text, but it is not applicable to use these as labels in our figures. We would therefore like to keep it that way. Moreover, in all figures we have always the same order from maritime (m), to intermediate (i), to continental (c) and to continental polluted (p). It is our hope that this increasing order is sufficient, the same labeling has also been applied in the previous companion study in ACP (Barthlott et al., 2022).

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

We included the formula to compute τ and also some more text in the manuscript.

- 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.

For the differentiation of strong and weak large-scale forcing, it is only important if the daily mean values of this time scale are below (indicating strong forcing) or above (indicating weak forcing) a threshold of 3 h. Although the strength of large-scale forcing might be stronger on 10 June 2019 than on 17 Aug 2020, the precipitation deviations in Figure 5b are very similar for both strong forcing cases despite their difference in the value of the convective-adjustment time scale. Only for dry soil moisture conditions, the case of 10 June 2019 shows larger negative deviations to the reference run. We believe that this behaviour is more related to the soil moisture-precipitation feedback than to different strengths of the large-scale forcing. Moreover, we did not intend to have similar forcing strengths within each forcing type and find it rather advantageous to have some variability in the forcing strength.

- 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?

We included some more information about the product in the text. It now reads:

“We also compared the simulated precipitation to data from the precipitation analysis algorithm RADOLAN (Radar Online Adjustment) which combines weather radar data with hourly surface precipitation measurements of about 1300 automated rain gauges (not shown). For 24-h accumulated precipitation, we find an overall good agreement, even if the precise location of individual convective cells are not always simulated at the right place. However, the model succeeds reasonably well in reproducing the observed cloud and precipitation evolution which implies that these runs serve as a good basis for our combined perturbation experiments.”

- 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. We intended to investigate the total precipitation deviation for a representative area in central Europe and chose a box covering most of Germany with only little amounts of sea at the north-western edge. This area comprises flat orography to the north and more complex orography in the center and southern part. A separation of the analysis for different orographic regions was beyond the scope of this study but is ongoing work at our group right now. Moreover, the same evaluation domain was used in Baur et al. (2022) and Barthlott et al. (2022).
- 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

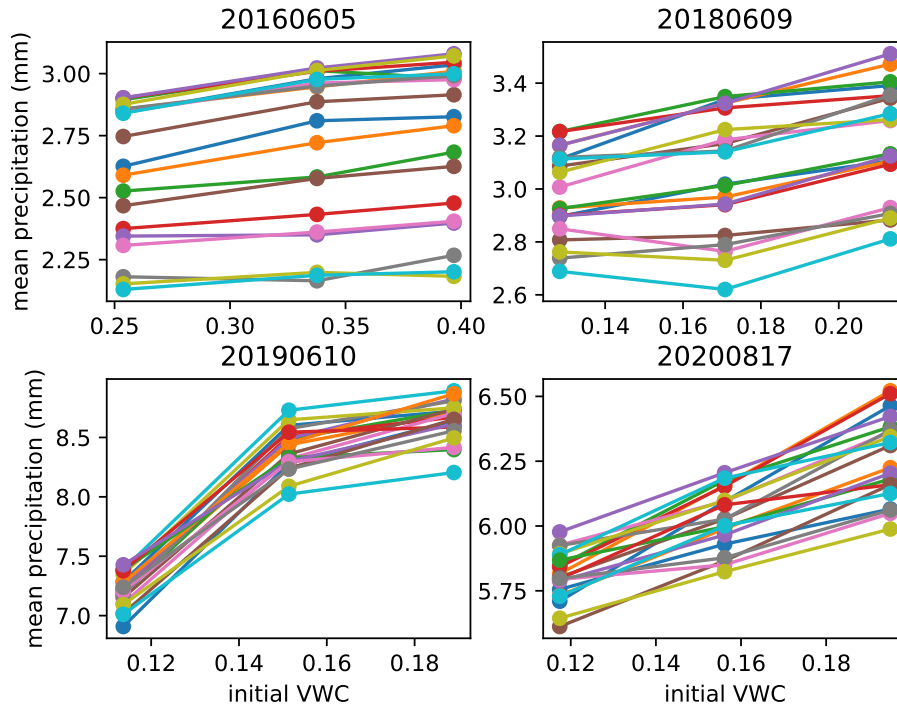


Figure R.2: Mean precipitation amount as a function of initial volumetric water content (VCW).

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.

The (non)-linearity of the soil-moisture precipitation feedback was not the main focus of this study and is hard to derive from simulations with three different initial soil moisture fields. However, for each day, 20 realisations with identical CCN concentration and shape parameter are available with 3 different initial soil moisture fields. We computed the correlation of accumulated mean precipitation to the initial soil moisture and find high mean values for all days: 20160605: $r = 0.938$; 20180609: $r = 0.914$; 20190610: $r = 0.933$; 20200817: $r = 0.988$

In Fig. R.2, the high correlation is visible for many of the different realizations. We included these sentences in the manuscript:

"Although this study comprises only three different soil moisture realizations, we tested if there is a linear response of accumulated precipitation to initial soil moisture. Recently, a modeling study by Drager et al. (2022) suggests a new type of rainfall response to soil moisture in which intermediate-moisture soils receive less rainfall than do the driest or wettest soils. This non-monotonic soil moisture–precipitation relationship was found to result from the permanent wilting point’s modulation of transpiration of water vapor by plants. Our simulations revealed a monotonic soil moisture–precipitation relationship for all runs under strong synoptic forcing and for 85% of the runs under weak synoptic forcing. Mean correlation coefficients were also high and ranged between 0.914 and 0.988. For more robust results, however, a higher number of soil moisture scenarios as applied in Drager et al. (2022) or Barthlott and Kalthoff (2011) would be necessary."

- 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.

The reviewer is right about the fact that two of the three cited papers here are based on results with the COSMO model. We are aware of the fact that ICON is another model and differs from COSMO in many ways, and that different models can produce different responses to enhanced aerosol loading. But COSMO was and ICON still is the operational model used at the German Weather Service. And since both models used the same double-moment scheme and investigated the same geographical domain, we believe that these references can be included here. Furthermore, we only state that the magnitude of the precipitation response is larger for weak forcing than for strong forcing. We believe that mentioning further model intercomparison studies are not necessary for the reader. However, we included a statement indicating that also COSMO results were cited.

“...which is in agreement with previous findings regarding aerosol–cloud interactions with the COSMO model (Barthlott and Hoose, 2018; Keil et al., 2019) and with ICON (Barthlott et al., 2022). Note that different models may produce different responses to aerosol perturbations, but these studies used the same double-moment scheme for simulating convective episodes over central Europe.”

- 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?

Although the absolute magnitudes of CAPE and autoconversion/accretion can not be compared to each other, we computed their percentage deviations compared to the reference run for this case. We find that they almost balance each other which supports our statement in the text. We included these sentences in the paper:

“To support this statement, we calculated the percentage deviations of CAPE and autoconversion/accretion of those model runs to the reference run. We find that the percentage magnitudes are almost identical: CAPE decreases by 11.8% whereas the warm-rain process increases by 11.5%. Although these variables cannot be used to quantitatively determine their impact on the total rain amount, it nevertheless supports our hypothesis that the CAPE reduction with drier soils can be compensated by the effects of a strengthened warm-rain process.”

The part about the cloud sizes was speculative only and is not necessary here as the differing rain amounts can be explained just by the intensity and lifetime of precipitation cells. We removed this part of the text:

“The answer to this is twofold: (i) the reduced warm-rain process as a result of a less efficient collision–coalescence process leads to a longer cloud lifetime or ~~and possibly to an increase of cloud sizes~~; (ii) stronger rain intensities must compensate the smaller cloud cover. We therefore now analyze the daily cycle of 30 min precipitation rates...”

- 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.

We are aware of the fact that individual model runs cannot be identified here. However, plotting individual lines for each model run would result in 20 different lines which would be even harder to distinguish. We believe that the essential characteristics, namely the higher rain intensities for maritime CCN (blue shadings) and weaker ones for polluted conditions (grey shadings) are still visible.

- 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?

We are sorry about the confusion, but "(WETp0–WETp8)" did not mean a difference, we wanted to list all runs in the polluted wet scenario (shape parameters from 0 to 8). But having read the sentence again, we believe that it does not fit here and removed it:

~~Except for the polluted runs (WETp0–WETp8), lower precipitation rates over wet soils result in lower precipitation totals.~~

- Line 284: Please also consider including other references not involving the first author.
As suggested, we included two additional references.
- 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.
We agree with the reviewer that the ratio of cold- to warm-rain formation is only relevant for the relative dominance of the cold-rain processes. We therefore moved this part of the analysis to the previous section with the budget calculations of microphysical process rates where it fits much better. New Figure 10 now shows the ratio of cold- to warm-rain formation and new Figure 11 shows only latent heat release and updraft velocities.
- 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.
No, this text part is about the sensitivity of latent heat release. We added this information in the respective sentence.

- 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.
This part of the text is still referring to old Figure 9, we included the reference to this figure now in this sentence as well. It now reads:

To study the impact on the dynamics, we computed spatiotemporal averages of updraft velocities (i.e. only positive values were accounted for) for cloudy grid points defined with a total cloud water content larger than 0.3 kg m^{-2} (Fig. 11b).

- 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.
Our simulations do not show any evidence of convective invigoration in all vertical levels. We computed mean profiles of updraft velocities for cloudy grid points for all members of our ensemble and all analyzed days. The results for a shape parameter of 0 is given in Fig. R.3. We find a robust signal throughout all vertical levels namely a systematic reduction of updraft speeds with increasing CCN concentration. It may be that individual cloud systems show an invigoration, but on average over the evaluation region, we don't find convective invigoration. We added this statement in the text to make clear that all vertical levels show this behaviour and that the level of 5 km is suitable to characterize it.

"For this analysis we selected the vertical updrafts at a height of 5 km agl because mean pro-

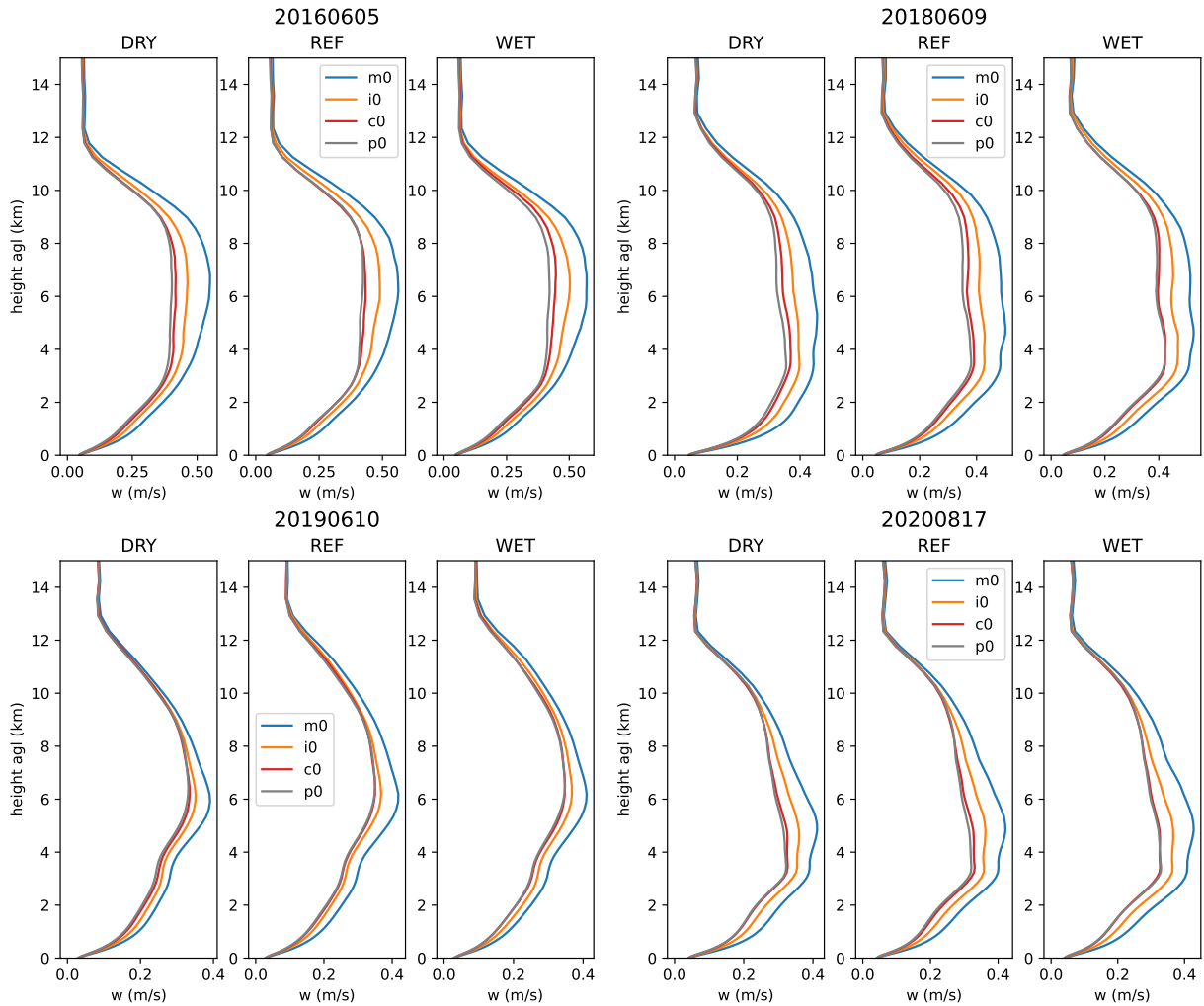


Figure R.3: Mean updraft velocities for cloudy grid points having a total cloud water content larger than 0.3 kg m^{-2} . Only runs with the default shape parameter of 0 are displayed for better readability.

files reveal a systematic response of updraft velocities throughout the entire troposphere and the maximum differences between different aerosol loads occur between 5 and 6 km agl.”

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

We followed the reviewer’s suggestion and used the factor separation methodology as in Grant and van den Heever (2014). Please see our reply to the first comment.

Additional corrections

We re-phrased some of the text in order to remove passages which had similarities to previous work. The meaning of the sentences was not changed, therefore the changes are not highlighted in the tracked-changes version.