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Responses to reviews

The sensitivity of intense rainfall to aerosol particle loading - a comparison of bin-resolved microphysics modelling with observations of heavy precipitation from HyMeX IOP7a

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Note to the editor

We thank all of the reviewers, whose comments have led to significant improvements in the analysis and our manuscript. Each question and remark of the reviewer is answered below point by point.

Changes in the manuscript and the reply to the individual remarks of the reviewers are marked in red for easier notice.

We would like to point out, however, that our choice of NHESS as publication journal has motivated our focus on the study of the surface precipitation. Following the request of the reviewer we have added some more discussion on in-cloud processes, however the in-depth analysis of the cloud microphysics and their comparison with the available airborne probes will be published in another more appropriate journal.

Responses to reviewer #2's comments

The specific comments below refer to the following Major comments bullets:

- The study employed the DESCAM bin scheme but the manuscript is short in description how the bin scheme is doing better compared to previous studies that used 1M/2M/3M bulk microphysics (at least qualitatively). This should be (at least) included in the discussion section.

  The objective of this paper is not to compare the bin vs. the bulk representation of the microphysics but to evaluate the performances of a bin model. We clarified this in the text.

- There is a serious lack in physical argumentation for several deficiencies in the model results and (1) lack of proper references for the cloud-precipitation-aerosol interaction work done by the community. There is also lack of vertical cloud structure (2) (tendencies or budget analysis) that could links/leads to the surface rain characteristics that is the focus of the paper.

Concerning point (1), we added more references on recent publications in the field of aerosol – cloud interaction. Point (2): In order to provide for the reader a better description of the characteristics of the macroscopic cloud system (cloud height, vertical cloud composition) we included in chapter 4 two vertical cross sections indicating IWC and RWC as well as temperature and humidity conditions for the cloud system. This also clarifies several individual questions of both reviewers.

The inserted text:
The vertical structure of the simulated cloud and rain field is illustrated in Figs. 6a and b. Both figures show the same vertical cross section for the innermost domain reaching from the southern border (at x=529, y=560 km) to the northern limit (at x=579, y=688 km). Fig. 6a gives the ice water content (IWC), Fig. 6b the rainwater content RWC for values larger 0.1 g/m^3. For the calculation of the RWC from the modelled drop size distribution only drop sizes larger 100 µm were considered. The illustration Fig.6b shows a quite continuous rain field during the intense rain episode at 8:20 h. Important RWC of 2-2.5 g/m^3 mainly forms close to the melting level. The 0°C levels varied due to the strong vertical motion over the complex terrain between altitudes from 3.3 and 3.7 km. We can also detect in Fig. 6b that raindrops appear in elevated layers up to -20°C. The IWC, however, reached much higher altitudes but the presences of ice values larger than 1 g/m^3 rarely exceeded a height of 8 km, which is in agreement with aircraft in-situ and cloud radar observations performed during the same time period. The illustration of the field of IWC indicates that the cloud system mainly developed to mid-tropospheric layers and convection did not exceed 7-8 km. Thus, the tropopause level could not be attained and consequently no anvil formation took place. Fig. 6a also includes two contour lines for relative humidity of 90% and 98%. The high humidity in the lower layers is caused by the southern flow from the nearby Mediterranean Sea. Relative humidity of 90% appears around 1000 m asl, 98% 200 to 300 m above. Cloud base height, i.e. the formation of cloud droplets is located at altitudes around 1200-1300 m.

The formation of the convective system was triggered by orographic lifting over the Cevennes Vivarais Mountains. The rapid cloud formation and intensification was in addition favoured by the high vapor loading in the lower atmospheric layers, arriving from the warm Mediterranean Sea and persisting for several hours.

- There is a systematic problem in using the three test cases (HymRef, HymLow and Remote) to deduce the sensitivity to aerosols number concentration. In order to test the sensitivity to aerosol concentration, the systematic way would be to change the concentration of a certain mode of the same aerosols size distribution (ASD). The concentrations in the ASDs modes as shown in the paper differ substantially, which leads to different physical response of the clouds. Otherwise, if the authors stored the number of drops nucleated as function of supersaturation and/or as a function of the ASD dry size ranges -- this would be a preferable approach to isolate the aerosols effect. In case this is not available, they would need to rephrase their conclusions as far as the aerosol sensitivity is concern. This is further required because they do not present the corresponding vertical cloud structure to help assessing certain deficiencies and/or aerosol sensitivity.

In fact, there was a typo in our Table 1. The diameters of mode 2 and mode 3 for the Remote case are a factor 10 smaller than given. We apologize for this inaccuracy.
The new figure 2 gives the size distribution with a linear ordinate for the number concentration to better illustrate the differences between the 3 scenarios.

Regarding the sensitivity tests: in previous papers, we have done extensive sensitivity studies varying aerosol modes. The objective here was to study the potential response in precipitation to different actually observed pollution scenarios in the region.

© Pg. 2 lines 2-3: There is no indication which microphysical schemes were used in the referenced papers. (minor)

The following sentence was added:

In particular, the model studies done in the HyMex context applied the one moment ICE3 scheme (Pinty and Jabouille, 1998), the work of Tauffour et al. (2018) compares in addition with a two-moment scheme LIMA (Vié et al, 2016).

© Pg. 2 lines 5-6: if previous studies ''succeed'', why were there significant differences in location, intensity and microphysical characteristics? They ''succeed'' according to what standard?

The wording was changed.

© Pg. 2 line 15: Again, agree in what sense? Did they use large temporal and spatial averaging technique? Is this sufficiently good? I would argue that any reasonable microphysical scheme can be compared to observations to some extent. In that case, why do we invest time to calculate spatial and temporal changes of hydrometeor spectra?

The wording was changed.

© Pg. 2 lines 20-24: In addition to mentioned above, you might want to stress that 1M/2M/3M bulk schemes have much more tuned microphysical processes / parameters, where bin microphysics have very few constraints apart from discrete grid for hydrometer mass/size into bins. See a comprehensive review in Khain et al. (2015).

The text was changed and now reads : "One major objective of this study is, thus, to test if a bin resolved microphysics module in a 3D mesoscale model is able to reproduce a real case of intense precipitation using the dataset obtained during IOP7a of HyMeX which then can help in the future to improve the bulk models that often have difficulties to simulate intense precipitation, in particular regarding the rain maxima requiring alerts for the population, due to the constraints of the prescribed spectra (Flossmann and Wobrock, 2019)."

© Pg. 2 line 32 – Pg. 3 line 2: Well, there is substantially larger amount of work being done in the cloud physics community than mentioned here. Please read (at least) the following references (and the references within) for a more complete 3D cloud-aerosol-precipitation interactions studies: Lynn et al. (2016), Marinescu et al. (2016), Fan et al. (2018), Marinescu et al. (2018), Shpund et al. (2019a, 2019b).

We included references of recent publication from Marinescu et al. (2018) et Shpund et al. (2019a) dealing with similar scientific objectives.

© Pg. 3 lines 29-30: This needs to be justified as the homogenous nucleation level and/or stratiform parts can easily get to 12-12.5 km easily. In addition, it is probably a way to reduce the computational loading, is this means the interaction between the outer-most and the inner domains are one-way? This should be clearly written.

Information on the vertical extension of the cloud field was available from cloud radar observations above before the model was set up. This is now illustrated by the new Fig.6 displaying the field of IWC and rain as a vertical cross-section. All nesting interactions were treated in the "two way" mode. We added this information at the end of line 29.

© Pg. 4 lines 1-2: It looks the DESCAM scheme calculates the aerosol mass dissolves within drops and ice crystals. Within the cloud microphysics community, it is debatable if this worth the additional calculation loading. In part, this is why most of the modeling work uses this method only in warm clouds and/or idealize setup. Apart from the calculation loading, can you comment on how significant is it to your simulations, facing your goals to improve the precipitation characteristics?

A fundament of the microphysical modeling in DESCAM is to respect the presence of aerosol and cloud particles at the same time, since without aerosols, clouds would not form in most parts of the atmosphere. This also holds for precipitation formation. The condensed water mass (liquid or solid) present in a cloud is determined by the strength of the phase transition of water vapor. This process is measured by supersaturation, which depends not only from atmospheric dynamics but also from the presence of aerosol particles. Once served as CCN, many models abandon thereafter the pursuit of the aerosol mass in drops and ice. This might appear plausible when we assume that all drops and ice crystals end up as precipitation on the ground. But this is not true as drops evaporate and ice particles sublimate in the atmosphere. (This is the real setup and holds also for mixed phase and cold clouds). Why should we ignore the remaining aerosol mass after a cloud particle cycle? We also respect the water vapor budget when droplets evaporate!
The significance of the aerosol concentration is most obvious in this study where all precipitation characteristics are influenced by the different aerosol concentration which we applied.

© Pg. 4 line 5: Again, facing your goals the reader should understand how main features of the microphysical scheme works. Raindrops of 10mm are extremely rare (some thinks they just do not exist); as such it is important to understand how the scheme handles these potentially numerical artifacts of very large rain drops that aren't stable. This affects for sure your rain size distribution.
The two largest bins in our numerical grid for drops have no physical importance. They serve to guarantee the mass conservation when collision-coalescence is calculated. Once formed, they are immediately redistributed to smaller sizes by stochastic break-up (Hall, 1980). A complete presentation of the microphysical scheme is given in Flossmann and Wobrock (2010), referenced in the text.

© Pg. 6 line 18: Why do you start with describing Figure 4? (minor)
Figure 3a (now 4a) is used first - in the paragraph of the section 3. So we didn't change the order of the figures.

© Pg. 6 line 20: should be sixth moment, not "sixth momentum"? what do you mean in "normalized" here? (minor)

Done. The radar reflectivity *Z* is normally in mm^6/m^3 but the radar reflectivity factor commonly used in order to facilitate comparisons between observations from different radars is in dBZ using:
$Z_{[in\ dBZ]} = 10 \log (Z_{[in\ mm6/m3]})$. That is, what we meant by 'normalized'. We clarified the text.

© Pg. 7 line 3: can you please explain from physical perspective what prevent the model (dynamical core + microphysical scheme) from being able to reproduce the change in orientation?

This is essentially due to the initial large scale data set. The model was initialized by the 3D fields of wind, temperature and humidity of ECMWF at 0:00 UTC with a horizontal resolution of about 44km in x and 55 km in y direction. All parameters are horizontally quite homogeneous, especially the humidity field over the Mediterranean Sea. The low level air masses responsible for the cloud formation in the morning were advected during night from the Balearic Islands. The coarse resolution of the outmost model (dx=8km) did not notably change the initial homogeneous structure of temperature and humidity and thus the inflowing air kept quite uniform until 10 UTC without a change in orientation. Thus, horizontal patches of air deviating in humidity, temperature or momentum from the large-scale conditions, did not developed in the simulations.

© Pg. 7 line 16-19: can you please comment from the microphysical scheme perspective -- why the area of rain accumulation is different, especially the area of the accumulated rain of ~38 (mm) and below is significantly underestimated. It looks like the scheme (or the setup) has problems in simulating shallow convection and/or stratiform clouds.

We attribute the missing rainfall over the western area of the mountains again to a lack in humidity and also to differences in wind with the real conditions. From ground radar observations (now Fig.5) and also from the profiles observed by the airborne cloud radar (not illustrated) we can exclude the presence of shallow convection or stratiform precipitation.

As a follow up query, how was the corresponding forecast of the 1M/2M/3M bulk microphysical schemes? Could you please comment on that.
The simulations available for IOP7a in the article of Hally et al (2014) don't give insights in the precipitation structure but restrict to 24 h rainfall amounts.

© Pg. 7 line 30: Indeed, but the reader may ask himself what in the microphysical scheme lead to this changes? If the paper would have a more coherent microphysical analysis (vertical cloud structure) you would be able to explain that from physical point of view.
Adding a microphysical analysis of the vertical cloud structure for the different aerosol scenarios comprises another research article, and thus we refrain for including it in this paper. In order to better communicate the vertical structure of cloud field, we already added the new Figs. 6a and b.

In order to moderate the Reviewer some deeper insights into the differences in cloud microphysics between the scenarios, we added Fig.R1 wherein two contoured frequency diagrams of the relative humidity with altitude are given, one for the Remote case, another for the HymRef. The frequency analysis uses 8300 soundings and restrict to the strongest area of the cloud and precipitation formation. Modeled relative humidity (RH) in the range from 90 to 103% was counted for the PDF, which was binned by 1%. We can see that in the Remote case that RH dominates between 100 to 101 % in the lower part from 1.3 to 5 km. In the levels from 5 to 6.7 km most RH take even 101 to 102%. For the HymRef scenario with higher aerosol loading, RH values between 100 - 101% contribute dominantly over all altitudes up to 6.7 km but the shift to 102% for z > 5 km is negligible. In addition, for altitudes above 4 km a significant part (30-35%) of the simulated RH remain below 100%.
This result clearly demonstrates that the cleaner atmosphere allows the formation of higher supersaturation which consequently effects a stronger phase transition to water and ice, explaining the increase in surface rain for the Remote case.

In addition, this is an example to the systematic problem in the ASD setup, where the Remote setup has 600 #/cm3 and 250 #/cm3 in the accumulation and the coarse modes, respectively. These modes are

readily nucleate to droplets in any typical deep convection systems, and should lead to early rain formation (especially the coarse mode with 250 #/cm3). This is quite different aerosols regime.
As already explained above, there is a typo for the coarse mode diameters in the Remote case in Table 1. This is now corrected.

© Pg. 8 lines 10-11: Have you checked your low-surface "cold pool"? The question is whether the limited spatial changes in rain results from a dynamical reason or underestimation in low-level rain amount and sizes which limits the evaporation and thus decrease the "cold pool". This is related to the large scale forcing vs. local convective instability.
Cold pools do not develop over the quite complex terrain and under high humidity conditions as for this as given for this event. The contribution of evaporation of rain is negligible as the cloud base is low and the subjacent air still holds a high relative humidity (see new Fig. 6).

Cold pools as a consequence of heavy rain in the southern part of France do typically from over the Mediterranean Sea as demonstrated by Duffourg et al (2016) or Martinet et al (2017).

© Pg. 9 lines 4-5: Regarding your conclusion that "more rain occurs when low particle number prevails" – this is likely to be true for 2 ASDs with different number concentration per modal size in a warm convective system. When you convolve number concentration between ASD modes, the rain can be initiated from different level in the cloud.
As explained above, there was a mistake in the mean diameters for modes 2 and 3 for the Remote aerosol spectrum in Table 1.

As the convection becomes deep enough, lower CCN size penetrates to areas where high vertical velocity occurs and thus higher supersaturation above liquid/ice occurs (Sw, Si), and more smaller drops nucleates, which means more vapor is extracted from the atmospheric column (Si > Sw) compared to nucleation at lower levels where Sw is limited by warm rain; this serves as positive feedback that intensify the convection as more drop freezes at higher levels, as well as lead to increase in large/dense hydrometeor size which sediment and force downdraft and further positive feedback.
It is right that high velocity occurs and higher supersaturation forms in 5-7 km as illustrated in the CFAD Fig.R1 for RH in the *Remote* scenario. But the higher supersaturation is due the lack of new droplet activation from aerosols. The number and the size of non-activated CCN are low in these cloud levels. Even if they nucleate their number contribution and mass is negligible compared to the already existing hydrometeor number and mass. In addition, diffusivity of water vapor and thermal conductivity are strongly reduced for temperatures below -15° and the supersaturation peaks needed for nucleation have to hold for quite long periods to allow activation of small particles. Fig.R2 (from Leroy et al, 2007, Atm. Research) gives the time scales for activation a particle with D= 40 nm under different conditions of supersaturation and temperature. In this temperature range from -15 to -25 °C we have to calculate aerosol activation time dependent and cannot apply equilibrium Köhler theory.

The above is called 'convective invigoration' that leads to more intense rain rate. In your Remote ASD setup you are not only reducing total aerosol number concentration, but you also "pushes" the clouds to rain-out (warm-rain) substantially earlier due to the increased number concentration in the accumulation and especially the coarse modes. Therefore, based on the limited analysis presented here, your conclusion needs to be rephrased to include the information about the differences in ASDs modal concentration
Number concentration in the accumulation and especially the coarse modes are not increased. Again sorry for our mistake in Table 1

© Pg. 9 lines 12-13: again, you have forced at least 2 more degrees of freedom in that the Remote ASD has substantially different aerosol number concentration distributed between the modes. You need to address this by dedicated sensitivity test, or at least restrict your conclusions.
See above.

© Pg. 10 line 24: the value of 9mm / 5min in Figure 9 cannot be seen. Please comment or correct this value. (minor)
We modified the phrase on page 10 line 24.

© Pg. 11 lines 25-30: There is no indication of the temperature near the surface and where is the freezing level placed.
This is done now by adding the new Fig.6b)

There is no clear indication how the averaging has been performed (space-wise). Since the model simulations clearly underestimates the area of ~25 mm (and below) and the averaging was made between 900-1000m, it is not clear to me how the RWC = 0.5 g/m3 rain size distribution in Figure 11b are reasonably compared to observations (as shown in Figure 11a).
All model grid point at z=0 m and an underlying topography between 900 to1000 m were selected when their RWC was in between 0.4-0.6 g/m^3.

Such RWC are largely in the underestimated area and can be attributed to shallow convection or even to heavily stratiform precipitating clouds. Can you explain this apparent discrepancy?
The Radar observations (as well as the model results) don't give any hint for shallow convection or even to heavily stratiform precipitating clouds.

Furthermore, in principle raindrops grows at the expense of small-medium size raindrops (0.3 – 1.5 mm) as these fall through the cloudy area, but this is quite a simplistic point of view as observations (Figure 11a) indicates that other processes are likely to be responsible for the ongoing supply of these small-medium raindrop near the surface and for vast range of RWC. These processes are being determined well above the surface (for instance: melting process; breakup of large raindrop). Thus, based on this simplistic microphysical analysis made here, the conclusion drawn should be very careful as probably the model has some drawbacks in this aspect.
You are right. We learned from this model study that precipitation microphysics can be improved in DESCAM and another article will focus on this subject.

Pg. 12 line 22: what is the context for "superficial" here? (minor)

Was changed

Technical comments

© Pg. 2 line 27: There is no meaning in "bin resolved"; you probably mean "size resolved".

The term "Bin-resolved" is extensively used in the literature, as bulk schemes also give size resolved information, in a parameterized way

© Pg. 3 line 26: Maybe "outermost model domain" is preferable. Also, the resolution increases, where the grid spacing decreases.

Done

© Pg. 4 line 1: A microphysical scheme (like the DESCAM) calculates (or prognoses) the temporal and spatial changes in the distribution functions. The overall set up of the dynamical core coupled to the microphysical scheme with the BC/IC "simulates" a particular test case and the corresponding fields (rain, CWC, RWC, etc.).

Done

© Pg. 4 line 26: … a third aerosol distribution with lower number concentration is used.

Done

© Pg. 4 line 33 (and throughout the text): number distribution is confusing. Use number concentration or/and aerosol/droplet/rain size distribution.

Done

© Pg. 11 lines 3: rain size distribution should be noted as RSD and not DSD. DSD is droplet/drops size distribution.

Done

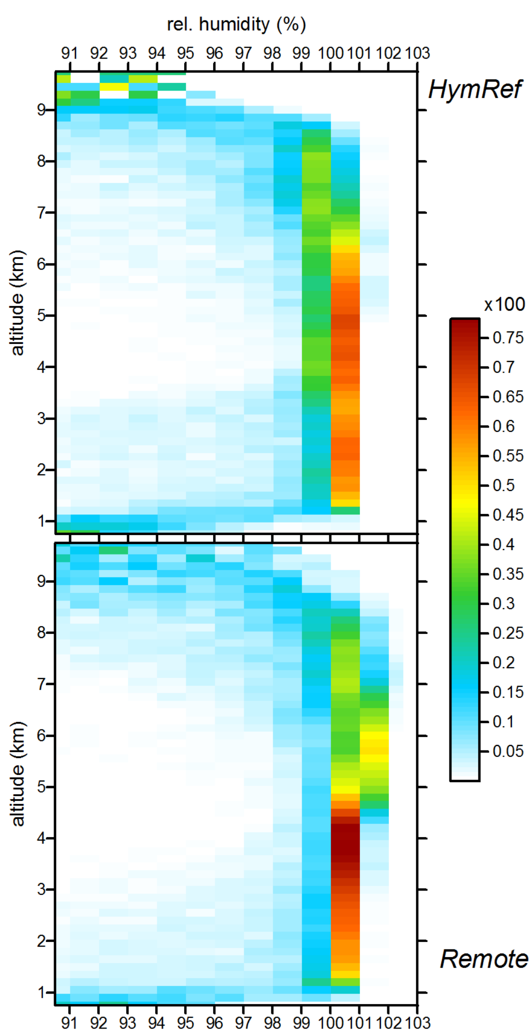

Fig.R1: Contoured frequency diagram of relative humidity RH as a function of altitude. The color scale give the frequency from 0 to 77%.

Relative humidity is given in bins of 1% from 90 to 103%. Altitudes a.s.l range from 750 to 9750 m with an increment of 150 m.

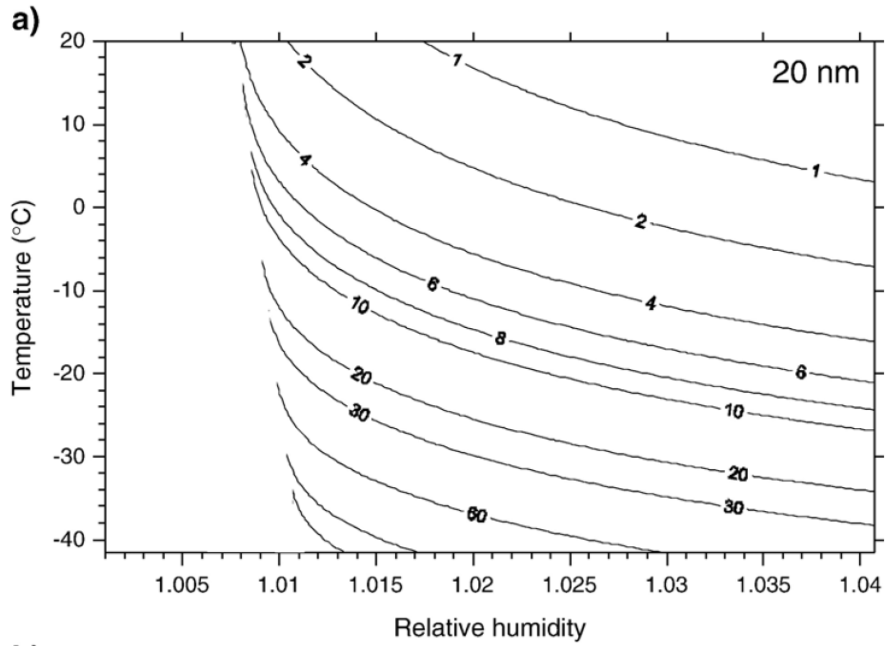

Fig. R2: Time of activation of a D=40 nm aerosol particle from its equilibrium size at 99% to 1 µm droplet size as a function of temperature and supersaturation.