We thank Reviewer #2 for his/her comments and feedback. The largest changes in the manuscript in response to these comments will be the improved presentation of technical details in addition to the other more specific suggestions given by the Reviewer which will also be implemented in the revised manuscript. Below, we will list all of the Reviewer comments, followed by our response highlighted in *italics.*

Paper content and a general impression

The submitted manuscript describes a newly-developed software package for research on aerosol-cloud-precipitation interactions. The presented framework is composed of a modified (adapted) version of the free and open-source Large Eddy Simulation (LES) system UCLALES, coupled with a modified (extended) version of the SALSA aerosol process modelling package. The paper consists of brief description of both pre-existing software packages and of how they were adapted, extended and coupled to result in the UCLALES-SALSA system. Moreover, the capabilities of the developed tool, in particular its applicability to capture aerosol-cloud interactions, are exemplified for two different simulation set-ups.

The topic of the paper matches well with the current interests of the cloud-modelling community – the study fits into an active stream of development of modelling techniques to study aerosol-cloud-precipitation interactions in LES-type frameworks. These concurrent endeavours are not referenced comprehensively in the paper, though.

My main major concern is the misalignment of the paper content with the scope of GMD. The model description itself amounts to ca. 3 pages out of 15 (not counting figures or references), while the rest of the paper deals with the case studies. Of course, this is not the page-count that matters and the case studies do provide valuable examples and validation of the model capabilities. Yet, the model formulation and implementation – in my understanding the key elements of the GMD scope – are clearly described in not sufficient detail. There is not a single equation used in describing the new extensions to the UCLALES and SALSA, even though the authors admit that “coupling the extended SALSA module into UCLALES yields extensive changes in the thermodynamic core of the model”. A key component newly introduced into the model formulation, representation of cloud droplet collisions and coalescence, is commented with just a single sentence without detailing the numerical method or its implementation. The reader is left without any information about software engineering aspects of the project (without studying the references, the reader would not even learn about the programming language in which the code is written; more importantly such aspects as parallelisation techniques, required environment and tools to use and extend the model are not mentioned and these cannot be guessed). There is no information on how the modified UCLALES and the extensions to SALSA are planned to be disseminated within the community, how existing UCLALES and SALSA users can benefit

from the described developments. If the authors would rather prefer to keep the paper focused on the case studies, and not the model formulation and implementation, I suggest submission of a revised version of the text to ACP or similarly scoped journal. The model code is also not publicly available as of now. It not only makes the paper not compliant with the GMD guidelines, but it also prevents me to fulfil the reviewer’s duties – the GMD board clearly states that all papers ”must be accompanied by the code, or means of accessing the code, for the purpose of peer-review”, moreover the journal guidelines ”strongly encourage referees to compile the code, and run test cases supplied by the authors”1. The authors do not detail how the paper readers may reproduce the discussed results.

For the reasons listed above, I am requesting a second round of the review to follow. In my opinion, the manuscript requires substantial changes to reach a good level of readability and to match contemporary standards of research reproducibility pioneered by GMD. Nevertheless, let me repeat that the described research and the developed tool are of prime interest to the community. In particular, the described system is capable of simulating aerosol processing by clouds through activation-collision-deactivation cycles as well as resolving aerosol sources – features not widely available in other LES-type systems.

As a general statement, we will improve on the description of the model technical details in the manuscript. In the current version, it is stated that the model will be available through Github, and upon request before its release. We are currently still in the process of cleaning up the code for a public release and expect this to be finished shortly.

General remarks

Few references to other LES aerosol-cloud-precipitation interaction studies

For the purpose of giving a comprehensive background, as well as of highlighting the unique features of UCLALES-SALSA, I strongly suggest supplementing the list of referenced works with some seminal
and/or recent papers on aerosol-cloud-precipitation interaction modelling with LES-type tools. In the list below, I suggest some that might be worth checking. The list is certainly not exhaustive, though:

- Lebo and Seinfeld, 2011: 10.5194/acp-11-12297-2011
- Ovchinnikov and Easter, 2010: 10.1029/2009D012816
- Andrejczuk et al. 2010: 10.1029/2010JD014248
- Shima et al. 2009 10.1002/qj.441
- Feingold et al. 1996: JGR 101 (D16)

In particular, citing some of these works could support or otherwise require rewording of some statements:

- p. 2/line 9/10: “extensive simulations with more detailed and interactive ... schemes ... are relatively sparse”
- p. 2/line 33: “innovative approach” (it is worth clearly stating precisely what is novel here)
- p. 4/lines 6-7: “not a computationally feasible approach”

We will adjust the manuscript as suggested by the Reviewer.

Lack of model formulation and implementation details

As outlined above, I strongly encourage the authors to increase the level of detail in which the model formulation and implementation is described. Here are some examples:

- p. 4/line 20: How the substepping is implemented? Are the grid-mean values kept constant for all substeps within a timestep (in particular, the supersaturation)?
- p. 5/line 13,15: statements seem contrasting: “not knowing the wet droplet diameter exactly” but “bin mean cloud droplet wet diameter” is used, perhaps it is worth summarising clearly what are the variables and constants per bin for each spectrum, and which processes change them – a table would likely give best readability
- p. 5/line 29-30: here the treatment of coalescence is described by just one single sentence without any reference. What are the numerics behind, how the kernels are supplied (if look-up tables, please detail interpolation method)?

We will increase the level of technical details in the model description, including all the points above raised by the Reviewer. As also requested by Reviewer #1, equations describing the key model processes will be added.

Simulation setup description

The fact that two contrasting cloud regimes are simulated gives a nice opportunity to pick this a criterion for mentioning or not a given simulation parameter. I suggest thus creating a table listing all model parameters that needed to be changed (or where arbitrarily switched on or off) in order to make the simulation depict fog instead of stratocumulus. This could perhaps allow to shorten a bit the setup descriptions in the text, the initial profile given by eq. 1-4 could then be part of the table (why not just cite the relevant equations in the paper in which the DYCOMS profiles where defined). If adaptive timestepping was used, please provide some statistics on the timestep values for the two different setups.If a spinup period is used for model initialisation, please clearly indicate which processes are on or off for how long, and what are other differences between the spinup and the rest of the simulation.

We will include more detailed information about the model setups as well as the spinup configuration. We will also elaborate on how the setup for fog simulations differs from the stratocumulus case. These differences are mainly comprised of model resolution, surface conditions and the input sounding. Adaptive timestep is used – we will add more detailed information about the values to the manuscript.

Also, some of the model features advertised in the beginning of the paper seem not used in the simulations (e.g., condensation of precursor gases and new particle formation mentioned on p. 3/line 30) – please state it explicitly. In contrast, features such as inclusion of the diurnal cycle or the soil energy balance are not mentioned in model description part.

The condensation of aerosol precursors was active in the model simulations, but it's effect was negligible in the current simulations setups. New particle formation was not active, although available. We will mention these and the other features suggested by the Reviewer.

Statements such as “large cloud droplet are considered as drizzle” (p. 8/line 27) or “the surface heat capacity is used as a tunable parameter” (p. 12/line 5) call for numbers.

We will add this information in the manuscript.

If I understood correctly, presented simulations lack aerosol sources. In contrast, the setups like DYCOMS-II implicitly assume an infinite reservoir of CCN brought in to the domain by advection. If that is correct, this difference is worth mentioning and perhaps discussing.

This is true, as well as the fact that the interpretation of the model simulations indeed does change when switching from the default UCLALES with prescribed CCN concentration to UCLALES-SALSA with a more dynamic description. The latter points more towards a “Lagrangian” simulation in the sense that the depletion of aerosols by clouds and precipitation resembles a domain moving with the flow. We will add discussion of this in the manuscript.

Aerosol processing nomenclature

Depending on the community “aerosol processing” is associated with different processes if put out of context. Please clearly state, at least in the abstract and introduction, whether chemical processing or collisional processing is addressed. Especially, since condensation of sulphates is mentioned on page 3.

Despite the condensation of aerosol precursors included in the model, for now the model does not have chemical processing. This will be stated more clearly.

Section scope
The introduction section mentions such, distant from the scope of the paper, matters as challenges in climate modelling, arctic temperatures changes, decrease in fog occurrence in Central Europe. For fellow cloud modellers, the links between those topics and the paper scope might be "obvious", for other members of the GMD audience these will seem puzzling, tough. Please either elaborate on how and why these topics are related with the development of UCLALES-SALSA or keep the introduction closer to the paper scope.

Since this modelling work ultimately aims at providing a research tool to improve the above mentioned features in global and regional climate models, we will keep these topics in the introduction, but will provide more in depth description, as suggested by the Reviewer.

The DYCOMS-II section uses up to three-digit section numbering (e.g., 3.2.1) while the fog case is just divided between two case description and Results subsections. I suggest some work on restructuring the two sections to be more similar in both section numbering and, more importantly, the level of detail.

We will break Section 4 into smaller divisions.

The specific suggestions below will be implemented into the manuscript unless otherwise mentioned.
Specific comments and rewording/correction suggestions
• Paper-wide:
– drizzle → precipitation (in particular in the title, the model is not limited to drizzle and since one of the quantities analysed is the surface precipitation rate, the simulated precipitation is by definition not drizzle)
– aerosols → aerosol (e.g., in the title, I don't have a strong opinion on it - just a suggestion)
– computational burden → computational cost
– high computational burden → resource intensive, etc
– interactive, fully interactive scheme, interactive description of particles – please explain what you mean exactly (by explaining which models are non-interactive), especially as it is mentioned in the title
– please ensure that acronyms are explained on first occurrence (e.g., SALSA is only deciphered in section 2.1)
• Abstract:
– line 1: impacts of → impacts on
– line 1,3: improved over what?, more sophisticated than what?, what kind of observations? Please be precise, please try to cater to a wider community, please make sure that the abstract summarises the presented research – global climate and gaps in observations seem not relevant enough to pop up in the very first sentences of the abstract
– line 4: model, coupled → model (LES), coupled
– line 5/6: "microphysical model components" is vague – please state if you refer to SALSA or something else as well
– line 6: "strategies for ... bin layouts" reads awkward, perhaps the keyword discretisation could help to better convey what is meant? I understand bin layout as a parameter of a given simulation, what is perhaps worth mentioning in the abstract is how the modelled particles are classified and which classes of particles are subject to which processes

– line 8: “computational cost of the model acceptable” – this is not only subjective but also likely to be objectively false soon
– line 8: two different cases: one comprising a case with marine stratocumulus . . . → two different simulation setups: the DYCOMS-II marine stratocumulus setup
– line 9-10: It is shown that, in both cases, . . .
– line 13: In radiation fogs, the growth
3
– line 14: strongly affects → strongly affect
• **Introduction:**
– p. 1/line 18: simulators → simulations
– p. 1/line 19: Moeng 1984 – please either use a few references to support the use of LES for decades or cite a recent review paper (preferably)
– p. 2/line 7: please mention also particle-based models (in addition to bulk, modal and sectional)
– p. 2/line 10: “mostly due to their high computational burden” – isn’t it the multi-scale and multidisciplinary nature of clouds that limits us most and not the computer power?
– p. 2/line 25: “fogs also feature many different aspects” – please reword
– p. 2/line 30: four references given to support statement that fogs are affected by anthropogenic emissions – please try to keep balance with the paper and journal scope
– p. 3/line 6,7: “well-characterised” vs. “findings of” could hint that one is superior to the other, please reword
– p. 3/line “previous model versions” versioning suggests something linear, in this context we are rather faced with multiple diverging branches, please try to avoid the version when giving a precise version number would be tricky
• **Section 2:**
– p. 3/line 16: drizzle/rain → precipitation
– p. 3/line 17: since the number 10 is just a setting for a particular simulation, perhaps it is worth explaining instead how the bins are laid out (logarithmically?)
– p. 3/line 18: what does the Bergman et al. citation refer to? (10 bins?, Figure?)
– p. 4/line 8: “parallel bins” is hard to understand
– p. 4/line 15: non-chronological order of citations
– p. 4/line 16: “very fast relative changes” is vague, isn’t it anyhow the stiffness of the governing equations due to presence of multiple size scales that is the crux of the matter?
– p. 4/line 32/33: “unwanted discontinuities in ... calculation”, please reword so it is clear what is discontinuous
– p. 5/line 8: I suggest removing the last sentence of this paragraph
– p. 5/line 9: drizzle → precipitation
– p. 5/line 12: please rephrase, perhaps referring to statistical moments resolved within each bin would make it clearer
– p. 5/line 21: please rephrase so that it is clear that wet diameter is the relevant quantity for condensation and collision, currently the sentence suggests that drizzle condensational growth is critical to produce realistic precipitation
– p. 5/line 22: does SALSA share the implementation of the Abdul-Razzak and Ghan (2002) scheme with some other (open-source?) model?
– p. 5/line 28: please explain (mathematically) how the bin layout is formulated
– p. 5/lines 31-32: please refer to the ”aerosol processing” in the sentence
– p. 5/lines 31-32: perhaps citing Mitra et al. 1992 could be used to support the assumption?
– p. 6/line 10: “default version” might mean something different for each user, please be specific

– p. 6/line 20: “raising the number of prognostic scalars” → “increasing the number of advected scalars from O(?) to O(100)
– p. 6/line 22: please hint the level of concurrency used – otherwise just the computer type makes the statement very vague
4
• **Section 3:**
– p. 6/line 31: is there any limit on the magnitude of the supersaturation during the initialisation?
– p. 7/line 22: would one of “Reference case”/”Reference setup”/”Reference run” be more apt than “Default case”?
– p. 7/line 27: from previous statements, I understood model initialisation to be equivalentthe spinup period, here it seems to mean pre-time-zero calculation – please use consistent wording
– p. 7/line 28: please reserve the word “parallel” for calculation concurrency
– p. 7/line 28: please define somewhere the “default UCLALES configuration” – again, this might mean different settings to different users
– p. 7/line 29: “default UCLALES” – does it refer to the “default case”, “default configuration” or something else
– p. 8/line 17: the domain-mean plot discussed was likely not the basis for statements“massive shallow convective cumulus elements” or open cells; please clearly define where you discuss the figure
– p. 9/line 20: “By the same token” sounds strange to me (but I’m not a native speaker)
– p. 9/line 29-31: please reword, “performs this task with very high detail” could be omitted, the use of “beyond” is unclear here
– p. 9/line 32-33: another example where the reader can be puzzled about differencesspinup and model initialisation
– p. 10/line 12: please reword “model with interactive aerosols”
– p. 10/line 24: “to the their” → “to their”
– p. 10/line 33: I assume this means very small in one simulation and non-existent in the other, please reword
– p. 11/line 5: isn’t coagulation part of the processing
• **Section 4:**
– p. 11/line 28: “water surface pressure” → saturation vapour pressure?
– p. 12/line 7-8: “is not available” suggests lack of availability, here it was simply not partthe setup
– p. 13/line 5: “-radiative” → “-radiation”
– p. 13/line 5: if “and feedbacks” is needed, please explain how do you differentiate them from interactions
– p. 13/line 29: “connect the aerosol concentration into fog existence” – I suggest rewording
• **Conclusions:**
– p. 13/line 33: “A new large-eddy simulation model” suggests some new fluid dynamics methodology, while the novelty is elsewhere – please reword
– p. 14/line 5: please precise what type of processing (i.e., non-aqueous-chemistry related)
– p. 14/line 27: “observed behavior Price” needs a parenthesis
• **References:**
– line 23: korolev → Korolev
– line 28: kokkola → Kokkola
– line 30: korhonen → Korhonen
• **Figure 2: please sort out the background colour issue in panel b** *There is in fact no background color issue. In UCLALES-SALSA the condensed water in aerosol particles is classified as “cloud*

water” which, even though small, becomes visible because of the log-scale of the colormap. We will note this in the caption.