24 February 2013
Response to Referee 1
"Mineral Dust Indirect Effects and Cloud Radiative Feedbacks of a Simulated Idealized Nocturnal Squall Line"
Robert B. Seigel, Susan C. van den Heever, and Stephen M. Saleeby

General Response to Referees
The authors thank the two anonymous referees for reviewing and commenting on this manuscript. We appreciate your thoughtful comments and suggestions, as they have helped to improve the quality of this manuscript. We are also pleased that both reviewers have found merit in this work. In addition to many specific changes that have been made, the following more major modifications have been made to the revised manuscript:

1) The title has been changed to avoid confusion regarding aerosol direct effects. It is now "Mineral Dust Indirect Effects and Cloud-radiation Feedbacks of a Simulated Idealized Nocturnal Squall Line".
2) More details and information are included regarding the microphysics and aerosol schemes used in this study and in RAMS. We have included a table that summarizes some pertinent microphysical and aerosol information, along with many additions to the Methods section.
3) Additional text has been added to the Summary section that describes the exclusion of aerosol direct effects in this study, and the need to investigate them in future studies. Please see the specific responses for further details regarding the exclusion of direct effects.
4) The references to the various microphysical quantities discussed throughout the Results section have been changed to make the manuscript easier to follow. The only words in uppercase are the factors, while the microphysical quantities are now simple shorthand references.

Please see the responses to your specific comments below.

Specific Response to Referee #1: Author comments in bold

Anonymous Referee #1

The paper investigates in a comparative way the individual and synergistic impacts of mineral dust and the cloud radiation feedback for the case of an idealized nocturnal squall line. For this a sophisticated model framework is used with a high spatial resolution (in comparison to other available three dimensional studies dealing with dust) and a factor separation analysis is applied to separate the impact of dust, the impact of the cloud-radiation feedback, and non-linear feedbacks caused by the combination of both. The results are well structured and are sufficient to support the conclusions. The impact of mineral dust on cloud microphysics and the non-linear feedbacks introduced in cloud development are still poorly understood and the paper, using simulations with high spatial resolutions and factor separation analysis, is a good contribution to improve our understanding in that field. I find the paper suitable for publications in Atmospheric Chemistry and Physics after the following comments and corrections have been taken into account:

- I think the title can be confusing. It can be read in a way that suggests that the direct impact of dust on radiation is studied in the paper (which is - so far – not the case). Maybe 'cloud-radiation feedbacks' or something else is more specific than 'radiation impacts'.

We agree. The title has been changed to avoid the misconception that direct effects are included in the analysis; it is now "Mineral Dust Indirect Effects and Cloud Radiative Feedbacks of a Simulated Idealized Nocturnal Squall Line".

• If technically possible, I highly recommend to include the direct impact of dust on long-wave radiation in the analysis. The dust particles may have a strong impact on radiation in this case. The discussion of synergistic effects of dust and radiation therefore needs the direct contribution of the dust. I know that the focus of the paper is on the microphysical impact of dust particles, but accounting for the impact of dust on radiation and using factor separation to analyze the synergistic effects would be a great step forward in understanding reality. I think the paper would benefit a lot.

Thank you for these comments. The authors recognize and fully agree with you in that the inclusion of the dust-radiation interactions in this study would be a next great step in understanding total aerosol impacts on organized deep convection. The inclusion of the direct effect would have an impact on the radiative budget of the pre-squall line environment, potentially leading to changes in thermodynamic forcing of the squall line. A brief discussion regarding our decision for deliberately neglecting the direct effect has been placed in the Introduction and Summary sections.

While this is an interesting and important feedback to better understand the interactions of aerosols and deep convection, we feel that the inclusion of the direct effect would be too much analysis for this manuscript. To include the direct effect in the factor separation analysis, three additional simulations (two of which being synergistic) would need to be run, analyzed and discussed. The extra analysis and discussion would force a reduction of the current analysis, which we feel is important to keep in tact due to its complexity. While the direct effect is relatively well-known and can be directly simulated within all scales of models (e.g. GCM, regional, LES), aerosol indirect effects are far more complex and more difficult to simulate within models utilizing convective parameterizations. In this study, we deliberately chose to isolate the indirect effects to aid our understanding and interpretation of aerosol-cloud interactions. By isolating the indirect effects and improving our understanding of these processes, we hope to assist the science community in furthering modeling capabilities, especially those working on mesoscale organization in GCMs.

The direct effects are currently being analyzed and will be submitted for publication elsewhere.

• To put the results in context, more details about the realization of the dust-cloud interaction are needed. Especially because the cited paper including all the details (Saleeby and van den Heever, 2013) is not published yet and therefore not accessible.

This paper has now been included in the references and has been accepted for publication. Because it is not yet online for public downloading, we have included the manuscript with our responses for your convenience.

– Please provide details about the representation of the dust size distribution (log-normal distribution? Parameters used?) and the dust properties (For example, what is the hygroscopicity of the dust? How is the activation of the dust treated in the model).

These parameters and characteristic are all now included in the manuscript. We have added a table and additional text throughout the manuscript.

– Is there only homogeneous freezing in simulation dOFFrOFF and dOFFrON? If no, what assumptions are made? Which parameterizations are used?

That is correct for primary ice nucleation. However, RAMS also includes the Hallet-Mossop secondary ice production mechanisms.

– Since the parameterization of DeMott et al. 2010 only accounts for temperature, is there a specific supersaturation threshold w.r.t ice applied for het. nucleation?

Supersaturation with respect to water is required for the DeMott et al. (2010) formula.

– A table giving an overview about the parameters used in the cloud microphysics would be nice (e.g. for the gamma functions used, : : :).

This has been included in the revised manuscript.

– How is the parameterization of the cloud optical properties realized in the model? Do the optical properties depend on droplet and crystals size?

It follows Mie Theory and is dependent on droplet/crystal size and number concentration.

– You mentioned that emission by saltation etc is included in the model. Is the emission of dust treated or is the dust concentration only prescribed by the initial conditions? If emission is treated, what are the assumptions (emission everywhere, soil properties, etc.)

This has been clarified in the new manuscript. While the RAMS dust model does include parameterizations for surface dust emission, it is not used in this study. Please see Saleeby and van den Heever (2013) manuscript that we have attached for the details of the emission scheme.

• I recommend not to use the term 'aerosol indirect effect' in the context of this paper. In the paper only the impact on cloud microphysical properties is discussed, not the impact on the radiation budget or consequences for the climate. Using 'aerosol cloud interactions' (ACI) is maybe an alternative to AIE.

The authors disagree that 'aerosol indirect effect' should be substituted with 'aerosol cloud interactions'. Twomey (1977) and Albrecht (1989) refer to AIEs as aerosol induced changes to the microphysics, and subsequent changes to the radiative interactions with the cloud. All of these processes are being investigated here.

• Please introduce variables (italic, lowercase letters) instead of lengthy abbreviations like TMIX, CL2RT, ... The results section is very uncomfortable to read because of the many uppercase abbreviations. Only the different factors should be in uppercase.

This has been changed both in the text and in Figures 10, 12, and 13. The uppercase abbreviations have been replaced with a shorthand reference to the full variable names, which have been introduced in lowercase italic.

Minor comments:
29610 l. 29 In order to improve ...
This has been changed.

29612 l. 27 Saleeby et al., 2012 not in References
This has been fixed. We have also included a copy of the accepted manuscript for your convenience.

29613 l. 3 two-stream radiation; l. 5 NACL is not used in this study please mention this somewhere; l. 15-17 more details needed about hygroscopicity, feedback with supersaturation,...
These have been changed.

29614 l. 11 ... mineral dust and to the radiation feedback; l. 13 ... vertical grid spacing from ...;
These have been fixed.

29615 l. 1 Aerosol data measured near...; l. 5 The DeMott paper discusses measurements from CRYSTAL FACE in 2002. I do not see how this confirms that the data used here is a 'good' sampling of the SAL;
We agree. That probably was not the best choice because of the measurements large distance from Africa. We have replaced the citation with Ismail et al. (2010), as they measured the SAL just off the coast of Africa during NAMMA. Their observations still match well with the dust observations used in our study.

29617 l. 23 no " "
The quotations have been removed.

29619 l. 19 something is wrong with this sentence. ...as for precipitation... (?!)
This sentence has been changed to "The factor separation analysis for cold pool speed shows a trend similar to that for precipitation for the contributions of RADIATION, DUST MICRO and SYNERGY."

29625 l. 16 is the "cloud-to-rain process" in this case autoconversion or accretion or both?
Yes, this includes both the autoconversion and accretion processes.

29627 l. 4-6 I don't understand this sentence. We are talking about water vapor diffusion here, and not the condensation of e.g. sulfuric acid vapor or something which is related to aerosol nucleation (... nucleate a dry aerosol...).
We see your point and have changed the sentence to "Because the energy barrier to diffuse vapor onto an already present droplet is less than that to nucleate a cloud droplet from a dry aerosol (Prupacher and Klett, 1997), the process of vapor diffusion (COND) takes precedence over droplet nucleation (C+D NUC) on average." This should now make sense.

29629 18-... Since the overall impact of SYNERGY is small, I think its impact is overemphasized in the summary (roughly one third of the whole summary).
We feel that because SYNERGY is small, it is highly interesting. We have added some text throughout the Summary section to better express this.

Table 1 'Dust acting as CCN and IN' maybe more specific than 'being microphysically active'
This has been changed.

Table 2 What is meant by "Part of the predicted field ..."?
Because the factors presented and discussed in the paper are not the full fields (e.g. precipitation, total condensate mixing ratio, etc.), we intend for "Part of the predicted field" to indicate that this is the *part* of the field due to the factor. This description has been added to the manuscript.

Figure 3 Please enlarge the label of the maximum surface wind reference vector.
The label has been enlarged and repositioned above the vector for better clarity.

Figure 4 and 5 Units are missing at the colorbar.
The units are described in the captions.

Please switch the plot axis to be consistent in Fig 4 and Fig 5.
For these two plots, the authors would like to remain consistent with squall line orientation in figures. As you may have noticed, all two-dimensional figures have a left-to-right (or west-to-east based on the model configuration) orientation of the squall line. In order to maintain this perspective, time must be on the y-axis in Figure 4 because the precipitation fields have been meridionally averaged along the gust front and the squall line is moving from left to right in time. In Figure 5, no averaging has taken place, and therefore in order to preserve the left-to-right perspective, time must be on the x-axis.