Interactive comment on "The Influence of Simulated Surface Dust Lofting Erodible Fraction on Radiative Forcing" by Stephen M. Saleeby et al.

Reviewer comments below are in standard black font, while the author responses are in blue italic font for contrast.

General reply to reviewers based on overarching comments:

We thank the reviewers for their time in examining our manuscript and offering constructive criticism, comments, and suggestions. We feel that reviewer comments have led to an improved manuscript. As will be discussed in detail below in response to specific comments and questions, this paper presents a theoretical modeling study placed in the context of a dust lofting event over the Arabian Peninsula that explores the potential radiative response to variable dust loading using dust lofting models and dust-sensitive radiation schemes embedded within sophisticated high-resolution model environments. The main goal of the paper is to examine the mean differences in radiative quantities and atmospheric temperature resulting from differences in dust loading that result from applying different dust erodible fraction datasets to the lofting model.

While the Arabian Peninsula is well-known for its expansive dust storms, few dust lofting studies have been performed over this region. This is, perhaps, because aerosol related data in this region are limited. As such, we have provided a more qualitative model comparison to the limited aerosol observations in the area in order to broadly demonstrate that one of the models (RAMS) does a favorable job in simulating dust lofting when the dust erodible fraction is constrained by geographical datasets, while noting that precisely simulating the magnitude and location of individual dust plumes is incredibly difficult. Following this, the RAMS model was then used to investigate dust radiative effects in the simulated environment. It is not our intent to determine which dataset leads to the best model representation of dust lofting. Walker et al. (2009) provide such an assessment with regards to dust lofting and surface visibility. Our focus is on determining the potential range of dust radiative effects by comparing a simulation with no-dust to those with varying amounts of dust generated by use of different specifications of surface dust erodible fraction.

Overall, we have worked to more clearly frame the focus of this paper as a theoretical examination of dust radiative effects in a case study context, while noting that dust AOD observations are limited, yet they compare favorably to RAMS simulations when dust erodible fraction appropriately constrains the amount of lofting.

<u>Anonymous Referee #1</u>

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The authors present results of a regional NWP model simulations over the Arabian Peninsula region, including mineral dust aerosol, for a case study during August 2015. They test sensitivity of dust simulations to two different models used, and to three different dust source representations. They compare these results to observations. One of the models is then used to examine the radiative effects of the dust in a cloud-free region, with a particular emphasis on radiative divergence, net radiative flux, and vertical temperature profiles, contrasting the differences due to the different dust source representations.

The paper is mostly clearly and succinctly written, and easy to follow in terms of methodology and analysis. The interpretation of impacts of dust loading on radiative fluxes, vertical temperature profile, and surface fluxes are a valuable addition to the literature and will help inform future studies on the potential impact of dust on cloud development. However, the earlier part of the paper (the impact of dust source representation on dust loading and AOD) is less clearly analyzed and the main conclusions of this section are a little weak. The justification for including the "idealized" lofting method is unclear (see major points below). The observations are not really sufficient to inform which of the two realistic lofting experiments (Ginoux and Walker) performs better and as such the first part of the paper is not particularly illuminating.

We thank this reviewer for your overall assessment of this manuscript. Our general reply to reviewers at the top of this document is meant to provide focus on the intent of the paper while addressing the concerns regarding the comparisons to limited observations. We have revised the manuscript to help focus the direction and intent of the paper and address the utility of the Idealized lofting experiment, as noted above.

The abstract is fairly poor in describing the experiments the authors have conducted, why these were done, and their conclusions. A number of minor clarifications are necessary and are detailed below. However, with some additional clarifications and explanations the authors should be able to suitably address all these points and provide a paper suitable for ACP.

We have examined the abstract and have rewritten it to better describe the motivation, experiments, and conclusions. We have also responded to each specific comment below.

Major points

1) Idealized lofting
It is not clear why the authors choose to implement the 'idealized' lofting method when it generates such unrealistic results, and is also physically unrealistic. I suspect it is because this 'extreme' case becomes useful in section 4 when evaluating the radiative fluxes in terms of understanding how the system reacts to a 'kick'. Much more justification and explanation of the idealized method should be provided, as well as a statement that the authors do not expect it to respond realistically, and that it is retained for evaluation of 'extreme' purposes in section 4 (if that is the case). In terms of conclusions and abstract, it is not surprising that the idealized case produces inferior results – this is not a scientific finding.

We noted in the discussion of the simulations that the "Idealized" lofting method was included as one of our experiments since this method has been used in another study that simulates idealized conditions (e.g. Seigel and van den Heever 2012). In idealized simulations the "Idealized" lofting method, that can loft dust in any grid cell containing dry soil, certain clay fractions, and low vegetation, has been shown to produce reasonable amounts of dust for localized dust events. It seems fair to extend this to a case study for testing to examine the upper *end of potential dust lofting, even if this may be unrealistic. As such, we have revised the manuscript to present this as being an upper limit to dust lofting that could occur in this model setting, and then examine the upper limit of radiative response. We have modified the text to better clarify this motivation.*

2) Abstract
The abstract needs a complete re-write to follow a typical structure of description of a) the field/problem, b) description of experiments carried out and why, and c) results found and their significance. Currently a) and b) are completely missing. Idealized lofting, if mentioned in the abstract, should be explained. It would be useful to relate 'extreme' and 'moderate' dust references to specific AOD ranges. L23-25 – this statement is not justified. The authors have not shown that the higher resolution source database produced better results (though the word 'detail' is ambiguous) – simply that it provided more spatial variability in the dust load. The fact that the Ginoux and Walker uplift experiments do not produce particularly difference radiative effects should be stated (and also discussed in the paper).

The abstract has been re-written to provide a concise summary of the work presented in this manuscript. Also, we have added discussion regarding the similarities in the results comparing the Ginoux and Walker experiments.

3) Significance of Section 3
Overall the observational evidence for evaluating the Ginoux vs. Walker uplift experiments is fairly weak. The Walker simulation provides much greater spatial variability due to the higher resolution of the input surface data compared to the Ginoux dataset. However, the sparsity of the data over the region prevents the authors from reliably evaluating whether one dataset is better than the other. The MODIS data shown is rather patchy and also only show for part of the simulation region. The AERONET data is not conclusive in the evaluation and a small offset in model analysis region for the AERONET comparison produces significantly different results. The authors should either attempt to expand their observational comparison to inform the model comparison, or if this is not possible, modify the text and conclusions appropriately to say that lack of observations prevent a proper evaluation of the two dust source datasets. Even without being able to say which dataset is better, it is a useful finding that more resolution in surface dust source area translates to more spatial variability in the atmosphere, even after several days of transport.

Thank you for this comment. We agree that it would be desirable to have a more extensive AERONET array and better MODIS coverage. However, we have presented what limited observations are available for comparing dust. We have modified the text to highlight that observations are limited and thus our observational comparison is intended to be qualitative in nature. The single southern AERONET site provides us with only a single point comparison near the UAE / Persian Gulf dust plume. Performing grid point comparisons between models and observations often provides limited utility in events where key features, such as dust plumes, are slightly displaced in the simulations. In our case the simulated dust plume over the UAE and Persian Gulf is slightly displaced, but magnitudes of AOD are similar to the in-plume MODIS AODs. We have added text that addresses the limited nature of the observations and their comparisons to model results.

4) Comparison against literature
There is rather little comparison against other literature in general – this would add to the significance of the article – both in the context of implementing different dust source maps, and in terms of the radiative effect (Section 4) results.

Throughout the manuscript we have added more comparison between the results of this study and past work including some comparisons with the following papers: Slingo et al. (2006), Shell et al. (2007), Lau and Kim (2007), Marsham et al. (2016), Hansell et al. (2010), Kosmopoulos et al. (2017).

Minor points and clarifications

Title – I encourage the authors to make this clearer – e.g. remove 'erodible fraction' and possibly include 'and atmospheric loading' before 'radiative forcing'

We have changed the title to remove "erodible fraction" and include "and atmospheric loading".

P2L9 – dust can cause atmospheric cooling in the LW also

We have added a statement here to the effect that LW emission in the dust layer adds a cooling tendency within the dust layer, but warming effect via LW emission adjacent to the dust layer (e.g. Slingo et al. 2006; Wang et al. 2013).

Section 2.2 – GOCART should be briefly described (e.g. size bins, uplift scheme) to give the equivalent information provided on the dust scheme in RAMS.

We have added some text to section 2.1 to indicate that RAMS' dust scheme is largely based on GOCART with some additional modifications related to soil type, vegetation, and dust lofting size bins. WRF-Chem uses GOCART dust lofting. The details of GOCART dust lofting can be found in Ginoux et al. (2001) as referenced.

Section 2.3 – p5 L27 onwards – Does this mean that the erodible fraction over the whole land-domain is 100%? Please clarify.

In the Idealized simulation, the erodible fraction over the whole land domain is 1.0 (100%). This was done for the Seigel and van den Heever (2012) limited area domain and produced quite favorable dust amounts over a limited time frame involving outflows from deep convection. We have added text to section 2.3 to clarify this. We have also placed the Idealized simulation in the context of representing the expected upper bound on dust lofting in this type of case study. We found it quite informative to know the potential upper limit of radiative effects that could be expected within the given modeling framework and parameterization.

Section 2.3 – p5 L27-35 – more background should be provided on each of the 3 surface lofting methodologies/datasets, since this is a key process and result within the paper. E.g. How were the datasets produced? What are they based on? Why are they different. Is the Ginoux dataset the topographic low source function?

We have added into this section several sentences that clarify the application of the Idealized lofting method, the Ginoux method based on topographic depressions, and the Walker method based on manual satellite identification of dust lofted areas. Each of these methods has an associated citation for which the referenced paper can provide the intricate details of the lofting methods/databases.

Figure 2a – why are there lines around some of the grid boxes? Is this an artefact? It seems unphysical.

The lines are just an artifact of the plotting tool and the discrete application of Ginoux 1-deg gridded dust sources to the model grid.

P7L3 – refs to Fig 4a – it's pretty difficult to see the dust over the desert. It would be helpful to refer the reader to AOD figure 7 here too (see also comment about domain shown in fig 7).

The dust over the desert is, indeed, difficult to discern in the visible imagery due to the similar colors of the dust and land area. We have updated the text to also point to the MODIS imagery in Figure 7 that shows some of the dust presence associated with the two plumes.

P7L8-9 – could this also be the higher resolution between the reanalysis and the model runs?

The differences in the magnitude of the 1000mb temperature field between the reanalysis and model data are probably more the result of the differences in the representation of topography and the land surface parameterizations between the models used here and the model portion of the reanalysis technique. The differences in the horizontal variability and spatial details between the models and reanalysis are likely due to resolution differences. We have added text in the manuscript to clarify these differences.

P7L11-19 – The inclusion of the NAAPS plot is confusing and unhelpful. The inclusion of data from NAAPS is sudden and unexplained. Comparing a model to another model is not helpful. I suggest removing the NAAPS figure and text completely. It does not add anything to the paper.

We have removed all discussion and figures related to NAAPS.

P7L31-32 – 'In both models, the Walker simulations captures more dust mass detail with respect to the lofting locations due to the precise, high resolution nature of the database.' – This should be reworded. The simulation may show more 'detail' – (spatial variability is probably a better word) but there are no constraints to show that this is correct. Due to the source database being higher resolution, one would expect the atmospheric dust loading to be more spatially variable. The does not show it is better or correct though.

We have restated this sentence to note that the high-resolution Walker dust source database leads to the generation of comparatively greater fine-scale spatial variability in lofted dust in *association with known dust source locations. We have also added a statement that while there is increased precision in lofted locations with the Walker database, that does not imply that the net amount of lofted dust is more accurate than that lofted via the Ginoux database. Walker et al. (2009) provide such an assessment.*

P7L30 onwards – WRF results are quite different to RAMS – the authors should discuss this and attempt to explain why.

Yes, WRF and RAMS dust amounts and AOD are quite different. We state at the end of Section 3.1 that these differences exist and that there is a separate study under way to perform an extensive model inter-comparison involving RAMS, WRF, and another model as well. This type of model inter-comparison is beyond the scope of this paper and will appear in a separate manuscript in the future. However, we have also added a statement to the end of Section 3.1 which says that both RAMS and WRF use the dust lofting techniques of the GOCART model (Ginoux et al. 2001) and the same erodible fraction databases; as such, we speculate that the prediction of the near-surface wind speed, the soil moisture, dust deposition rates, and dust binning may all be playing a role in contributing to the differences. A separate in-depth study will help shed light on this.

P8L1-2 – See above points about NAAPS – no need for NAAPS data here. Actual observations should be used to verify simulations, no another model! (And if there are no observations, a simple statement to this effect is sufficient).

We have removed NAAPS from the paper and have noted in the paper the limited aerosol observations available for comparison.

P8L5 – does this mean that RAMS does not include radiative feedbacks of dust, onto dynamics, etc.?

No. It means that we used an offline model to compute diagnostic AOD for comparison with MODIS and AERONET. The RAMS model does not provide AOD as a standard output diagnostic, thus we had to generate this offline. However, the aerosols are radiatively interactive, thus, impacting the radiation flux profiles and providing feedbacks to the dynamics and thermodynamics. We have added a statement and reference in this regards in the section that describes the RAMS aerosol model.

P8L8 – refractive index at which wavelength? Assuming this is 500-550nm, the imaginary part is relatively high (e.g. see Song et al. 2018, Balkanski et al. 2017). This will impact the radiative results in section 4 by causing increased absorption and atmospheric heating, and should be discussed. E.g. Strong et al. (2018) show that small changes in optical properties can have huge effects on circulation.

This index of refraction is referring to the 550nm mentioned above on line 5 for the offline analysis of AOD. Per this reviewer question, we have added additional text and citations in section 2.1 regarding the use of a dust complex index of refraction of 1.53+0.0015i for dust for *wavelengths up to ~2000nm wavelength for generating RAMS lookup tables of aerosol optical properties. Further, we note that AOD is not sensitive to the imaginary index of refraction.*

P8L9 – 'spheroid-like index of refraction' – clarify this – index of refraction does not have a shape.

The wording has been changed in the text to clarify the assignment of the dust index of refraction used for computing AOD from our offline model of aerosol extinction.

P8L8-10 – what refractive index in the LW is used?

The AOD analysis was only done at 550nm. However, we have added text to Section 2.1 to better describe the assignment of the indices of refraction for dust at various wavelengths. As noted in a response above we use in RAMS a complex index of refraction of 1.53+0.0015i for dust up to ~2000nm. We state that Stokowski (2005) provides a plot of refractive index as it varies with wavelength in the RAMS model.

P8L15-16 – what dust optical properties are used in WRF?

We have added a statement in the text indicating that the dust real index of refraction for computing AOD at 550nm in WRF is set at 1.53.

P9L1 – 'similar predicted synoptic situations' – this doesn't seem justified – the streamlines are quite different between RAMS and WRF – and dust uplift is extremely sensitive to small differences in wind pattern, speed and strength.

We have modified this section and section 2.4 to better state the similarities and differences between the synoptic fields shown in figure 3. The streamlines are shown so as to demonstrate that both models produced the northerly flow associated with the Saudi dust plume and the southerly to south-westerly flow associated with the UAE plume. We have added statements in the text that address the differences in AOD between RAMS and WRF and offer speculation that differences in wind speed and other conditions could explain the differences in dust lofting between the models. As noted earlier, this involves an on-going model inter-comparison study for a separate manuscript.

P9L3 – 'trends' – which ones? The authors have only discussed differences between idealized vs. Ginoux/Walker, not Ginoux vs. Walker, which are clearly not the same for WRF and RAMS.

We have reworded this paragraph to better summarize the overarching differences between simulations and models and between modeled and observed dust AOD.

P9L24-27 – and also impacts the Walker expt more because there is more spatial variability in the atmospheric dust load?

We have added discussion throughout the manuscript regarding the differences between simulated Ginoux and Walker dust plume concentration and AOD. We specifically discuss that *the widespread, small erodible fraction with Ginoux dust data tend to produce more broad dust plumes with lower maximum AOD. The Walker data tend to generate more focused plumes with higher maximum AOD and greater spatial variability. This certainly impacts the interpretation of the grid point comparisons to AERONET sites.*

P12L33 – 'small warming' – how much?

We have added the detail that the small warming is ~0.3-0.4C for the Ginoux and Walker simulations compared to No-Dust.

Section 4 – there is no comparison between Ginoux vs. Walker results here – why not?

While some comparisons are made between the Ginoux and Walker results, they are noted as being quite similar compared to the Idealized simulation. Many places throughout section 4 indicate monotonic changes in radiative fluxes with dust loading, and we have noted that the mean dust loading in the analysis region increases from the Ginoux to Walker to Idealized simulations (see figure 9a dust profiles). The discussion of monotonic changes implicitly compares all three dust-lofting simulations to the No-Dust simulation and to each other. However, in the revised manuscript we have included additional statements to compare the Ginoux and Walker simulations.

Section 4 – are the radiative results consistent with other work? E.g. Marcham et al. (2016)?

We noted in section 4.2 that the reductions in shortwave radiation for the Ginoux and Walker simulations are similar to those seen in Slingo et al. (2006) and Kosmopoulos et al. (2017) for similar AOD. We added more detail to this to note that both studies show surface shortwave reductions of approximately 200-250W/m2 for dust AOD on the order of 1.5-2.5.

We have added more comparisons to past work that is comparable to this study including comparisons to shortwave and longwave fluxes as well as estimated heating rates. (e.g. Marsham et al. 2016, Hansell et al. 2010).

Conclusion – more text should be added to cover the results of the source dataset experiments – e.g. the effects of Walker vs. Ginoux simulations, and the fact that the Walker simulations produced more patchy dust loadings than Ginoux.

We have updated the conclusions to offer a more comprehensive summary of the results and include more summary of the differences between the Ginoux and Walker simulations.

Figures – take care that the same country boundaries are shown on all maps. E.g. fig 3 – the WRF plots show different country boundaries to the other plots. H and I do not show boundaries. Check ACP guidelines for international borders.

We have worked to make the country boundaries similar among the RAMS and WRF plots.

Figures 5-6 – the authors should show the analysis region on figures a-c

We have added the analysis region box to figures 5&6 panels a-c.

Fig 7 – why is the same geographical domain as figs 5-6 not shown? A larger area would be more appropriate, especially since the radiative analysis region is not even covered in fig 7.

The model AOD (figure 6) is available over the full simulation domain, but the MODIS AOD (figure 7) does not cover the full model domain, but rather a limited swath. We had zoomed in over the plumes to be able to see some of the higher AOD pixels associated with the Persian Gulf plume. However, we have modified the figure to show the full domain, which does provide a better view of the Saudi dust plume.

References

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