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

Anonymous Referee #1

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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 analysed 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.

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.

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.

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 different radiative effects should be stated (and also discussed in the paper).

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 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 shown 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 prevented 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 areas translates to more spatial variability in the atmosphere, even after several (?) days of transport.

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.

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'

P2L9 – dust can cause atmospheric cooling in the LW also

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.

Section 2.3, p5L27 onwards – Does this mean that the erodible fraction over the whole land-domain is 100%? Please clarify.

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?

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

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 the AOD figure 7 here too (see also comment about domain shown in fig 7).

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

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.

P7 L31-32 – 'In both models, the Walker simulation 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. This does not show it is better or correct though.

P7 l30 onwards – WRF results are quite different to RAMS – the authors should discuss this and attempt to explain why.

P8 L1-2 – See above points about NAAPS – no need for NAAPS data here. Actual observations should be used to verify simulations, not another model! (And if there are no observations, a simple statement to this effect is sufficient).

P8 L5 – does this mean that RAMS does not include radiative feedbacks of dust, onto dynamics, etc.?

P8 L8 – 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.

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

P8 L8-10 – what refractive index in the LW is used?

P8 L15-16 – what dust optical properties are used in WRF?

P9 L1 – '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.

P9 L3 – '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.

P9 L24-27 – and also impacts the Walker expt more because there is more spatial variability in the atmospheric dust load?

P12 L33 – 'small warming' – how much?

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

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

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.

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.

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

References

Balkanski, Y., et al.: Reevaluation of Mineral aerosol radiative forcings suggests a better agreement with satellite and AERONET data, Atmos. Chem. Phys., 7, 81-95, https://doi.org/10.5194/acp-7-81-2007, 2007.

Marsham, J. H., et al.: The contrasting roles of water and dust in controlling daily variations in radiative heating of the summertime Saharan heat low, Atmos. Chem.

Phys., 16, 3563-3575, https://doi.org/10.5194/acp-16-3563-2016, 2016.

Song, Q., et al.: Net radiative effects of dust in the tropical North Atlantic based on integrated satellite observations and in situ measurements, Atmos. Chem. Phys., 18, 11303-11322, https://doi.org/10.5194/acp-18-11303-2018, 2018.

Strong, J. D. O., Vecchi, G. A., Ginoux, P. (2018). The climatological effect of Saharan dust on global tropical cyclones in a fully coupled GCM. Journal of Geophysical Research: Atmospheres, 123. https://doi.org/10.1029/2017JD027808

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