

# ***Interactive comment on “Direct radiative effect of dust-pollution interactions” by Klaus Klingmüller et al.***

## **Anonymous Referee #2**

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### General comment

This manuscript investigates how microphysical and chemical interactions between dust and pollution alters the properties of aerosols and their direct radiative effects (DRE). The experimental design is simple and effective. A set of four simulations have been used to model the properties of aerosols when dust and pollution are either emitted separately, or together so that they interact. By contrasting results from these simulations the study reveals the “interaction” term showing how the properties of the dust and the pollution-related aerosol change due to two-way microphysical and chemical interactions. It is interesting to explore such interactions and the changes in aerosol radiative effects are not trivial, so worth noting.

The main result is that the dust-pollution interactions lead to a  $-0.05 \text{ Wm}^{-2}$  change

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in net flux at TOA, dominated by increased SW reflection. This occurs mainly due to increases in accumulation-mode aerosol mass and AOD. The AAOD also drops slightly, though it is not completely clear why, though it is perhaps related to a drop in coarse-mode dust. As climate models and Earth-system models are increasingly adopting more complex microphysical aerosol schemes it is worthwhile understanding what happens as such interactions are enabled. To my knowledge this manuscript is novel and I would judge it to be relevant and worth publishing in ACP. The text is generally well written, well structured and concise. However, significant improvements in the analysis and interpretation of the results are required for the study to be published.

### Major comments

The main difficulty with this manuscript is that it is not very clear what has caused the negative change in aerosol DRE. The text interprets this as a change in dust forcing, or an “anthropogenic radiative forcing associated with dust”. However, the dust-pollution interaction is a two-way process and changes both the dust and the fine-mode anthropogenic aerosols. Figures S11, S12, S13 indicate significant changes to the “pollution-related” aerosol once the dust and pollution are emitted together so one can not attribute the change in DRE entirely to the dust.

The main cause of the negative change in aerosol DRE seems to be the increases in accumulation-mode aerosol, but it is not very clear from the study which aerosol components have contributed to this increase. More information is required to show how the aerosol properties have changed, including changes in aerosol mass, chemical composition, hygroscopicity, and possibly particle size across the relevant size modes.

Related to this, it is not very clear what has caused the changes in AAOD and SSA. Presumably AAOD reduces in dusty regions due to a drop in coarse-mode mass? The AAOD increases across the Sahel are apparently due to increases in BC and OC mass (lifetime) but it would be good to see the evidence to support this. However, it isn't clear why the SSA drops in non-dusty regions that are quite remote from dust sources.

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Presumably there is a relative reduction in non-absorbing aerosol components such as sulphate and/or nitrate, but why does this occur in regions very remote from dust. There are some clues in Figure S11 but there are many competing effects and the information is not comprehensive enough to understand what it going on.

The other major concern I have is the short duration of the simulations (only one year). Given the episodic nature of dust emissions there is likely to be considerable inter-annual variability in dust loadings and in how these interact with pollution outbreaks. This could affect both the magnitude and spatial patterns of the results. I would recommend extending the simulations to least a 10 years, unless the authors can provide evidence that a single year is sufficient to gauge the magnitude and characteristics of the dust-pollution interactions.

#### Minor comments

**Abstract:** The abstract is short and direct but needs to be altered to reflect the concerns above. In particular, the change in aerosol DRE is described as a “radiative forcing” here and throughout the manuscript. This could be confusing or misleading as the term “forcing” is usually used to indicate a change in radiation balance due to a perturbation in aerosol emissions. The  $\Delta F$  is really more “the change in aerosol DRE due to dust-pollution interactions”. This could be given a label such as  $\Delta DRE_{int}$  to avoid using this long definition each time in the text. P1 L5: Please spell out EMAC. P1 L7: Whilst the magnitude of the change in TOA radiation balance is worth noting, I would not describe it as large. In fact it is quite small compared to the total DRE of aerosol in present-day climate ( $\sim -2$  to  $-3 \text{ Wm}^{-2}$ ). P1 L10: Please quantify this “considerable fraction”.

**Methods:** P3 L10: It is not quite clear what the term “prognostic radiative-transfer calculations” means. Presumably this is the radiative-transfer calculations that are used to calculate fluxes and heating rates in the simulations. Using “prognostic” is a bit confusing since the prognostic aerosols are clearly not used in the radiation scheme, and a radiation scheme is not itself prognostic. P3 L13: Is the dynamical evolution of

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the atmosphere (wind, temperature, moisture, cloud) identical in all four simulations? I would have thought so if the prognostic aerosols neither interact with the clouds or the radiation scheme. P4 L17-18: I think that you need to swap F2 - F4 and F3 – F4 in this sentence. From my reading of the text F3- F4 corresponds to the dust radiative effect and F2 – F4 corresponds to the pollution. P4 L23: So it sounds like a radiation double-call procedure has been used, as outlined in Ghan et al. (2012). <https://journals.ametsoc.org/doi/10.1175/JCLI-D-11-00650.1> If so it might be useful to reference Ghan et al. (2012) as this paper outlines the double-call concept fully.

Results: P5 L20 / Fig 1: Does the mass shown in Fig 1 include the aerosol water content? Figures 1 – 3: Exactly how are the differences in mass and optical properties calculated? Are these calculated using the same logic as in equation 1? P5 L24: Would it be possible to provide a figure for the dust mass loading, or at least refer to Figure S1 here so the author can see where the “dust affected regions” are simulations. Section 3: I found this section difficult to follow (particularly the top half of P6) and it did not provide a full explanation of how / why the aerosol properties changed.

P6 L7-9: This argument needs explaining more fully. It is clear that the coarse-mode dust is removed more rapidly due to secondary aerosol forming on the particles leading to more rapid wet deposition. However, what happens to the accumulation-mode dust? Wouldn't the same process also speed the removal of the accumulation-mode dust compared to the simulation where pollution was not emitted with the dust? From figure S11 it looks like the overall mass of sulphate and nitrate aerosol in the accumulation-mode has decreased. So this would tend to decrease accumulation-mode mass and the hygroscopicity, yet total accumulation-mode mass and AOD have increased. Presumably the mass of BC and OC must have increased dramatically to compensate the decreases in sulphate and nitrate. Is there evidence of this? It would be good to see all the relevant mass components in figure S11 and have the full story explained.

P6 L25-26: The SSA has reduced in most non-dusty regions. Is this due to an increase

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in the relative proportion of BC and/or OC versus sulphate and nitrate? Why would this have occurred even in regions very remote from dust? It would be good to provide a table listing the global-mean values and global mean changes in relevant quantities, such as AOD, AAOD, radiative flux changes and the various aerosol mass components.

P6 L20: How has the aerosol become more reflective? I suspect the drop in AAOD is dominated by the decline in coarse-mode dust mass so I would omit “due to the higher reflectance” from this sentence. P6 L21. The AAOD is presumably increased in the Sahel due to increased BC mass (and brown carbon if this is included in the model). The explanation that this is due to AOD increase doesn’t make sense, only one can say that both may be increasing for similar reasons.

Conclusions: The main concerns given above need to be addressed throughout the conclusions. The change in aerosol DRE can not be interpreted as a change in dust forcing as it is caused by both changes in dust and pollution aerosol properties. The conclusions section is very concise, which is good, but a bit more discussion is required to explain what has caused the changes in aerosol DRE, and how these are linked to aerosol processes and changes in aerosol properties (mass, hygroscopicity, optical properties etc). P8 L20: The maximum impact to the south of the Sahel is given as  $-2.5 \text{ Wm}^{-2}$  here but  $-2 \text{ Wm}^{-2}$  in the abstract. From reading closer I see this is because the  $-2.5 \text{ Wm}^{-2}$  quoted here corresponds to the surface forcing and the abstract gives the TOA forcing. Please use the same headline result in abstract and conclusions.

Figures: There are a lot of additional figures in the supplementary material and some offer an unnecessary level of detail on the spatial and seasonal variability of aerosol radiative effects (S5, S9, S15, S19 - 23). Given that the spatial distributions and seasonality of results are probably very specific to the model and the meteorological evolution in this specific set of simulations, this level of detail is not particularly useful and could be misleading. The 3D visualizations of aerosol heating in particular are not at all useful.

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Figure S11: As expressed above, to really understand how the dust-pollution interaction affects aerosol properties this figure needs to include changes in OC, BC, dust, water and sea salt. The figure may need expanding to two or three figures to give a complete summary of the changes in aerosol mass and composition.

Figure S12 & S13: It is quite interesting to have this kind of information, but it was rather difficult to interpret how the mass of dust, pollution, water and natural aerosols change between the three scenarios. It would be clearer to have just one bar for each emission scenario (all emission, no pollution, no dust) and have each bar stacked showing the relevant mass components (dust, pollution, water and other natural aerosol). This way it would be totally clear how each mass contribution has changed depending on what has been emitted. It is still very interesting to provide this separately for both the accumulation and coarse-modes. Would it be possible to produce this kind of figure also for the global mean changes? As this analysis is really quite important to the story of the paper it would also be worth considering moving this (especially a plot with global-mean changes) to the main article.

Figure 6: This graph really emphasizes the interpretation that the dust-pollution interaction has strengthened dust forcing, which is misleading since the interaction has also altered the strength of the anthropogenic (pollution) aerosol forcing. For a more balanced summary it would be good to include a bar for the forcing from pollution and bars for “Dust + pollution with interaction”, and “Dust + pollution without interaction” instead of the bar with “Dust interacting with pollution”.

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