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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud and aerosol radiative effects as key players for anthropogenic changes in atmospheric dynamics over southernWest Africa" *by* Konrad Deetz et al.

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Answer to Referee #3 Konrad Deetz 19 June 2018

Dear Referee (Atmospheric Chemistry and Physics),

thank you for your report from 17 April 2018. We have accounted for the comments and suggestions in the revised manuscript version. Please find our replies (marked with #) to the individual comments in the following. Before the detailed replies to your comments we want to stress one important overarching point: This study mainly focuses on the sensitivity of atmospheric dynamics and cloud properties to aerosols and not on a

detailed validation of the model system. Nevertheless, we have done a comprehensive evaluation of the model with the available observations of the DACCIWA measurement campaign. We show corresponding figures in our replies.

Sincerely, Konrad Deetz on behalf of all coauthors

Referee comments:

(0) In this study, the authors ran a regional climate model COSMO-ART at convection-perming resolutions to examine the effects of aerosols on weather and climate based on a 2-day case study. They documented in detail how aerosol affects cloud and atmospheric dynamics over southern West Africa. They further presented detailed analysis of mechanisms that leads to these changes, and provide a conceptual model for this. I think overall the paper is well written, and it is great addition to existing literatures on aerosols effects on climate over Africa. I would recommend it publication after my following comments are addressed:

(1) I understand these are expensive simulations, but I still think it would be really nice if the authors can run model longer, say a month. The current results are interesting, but it is less clear how robust these results are. A longer simulation would definitely be more interesting, and may also produce more robust results.

We see your point. It would be great to have model results for the entire monsoon post-onset phase (22 June to 20 July), but as you said, the realizations are expensive, expensive with respect to computing time and also of handling the very large amounts of data. In terms of resources it is simply not feasible for us to realize this. Therefore, and after due consideration we decided to focus on the 3-4 July 2016, identified by the DACCIWA community as a golden day for further research. As denoted in the manuscript, from our experience during the measurement campaign, there is very small variation in the general meteorological conditions during the monsoon post-onset phase. Therefore we can assume well grounded, that our sample '3-4 July' within the post-onset phase is representative at least qualitatively for the entire post-onset phase

(nearly one month) without simulating the whole period. In our companion paper about the aerosol liquid water content in SWA, submitted on 26 April 2018 to ACP (also DACCIWA special issue) and which is likely soon also in the discussion phase, we added results of the 6-7 July in the Appendix (as a second sample within the monsoon post-onset phase), underlining that the conditions are similar from one day to another.

(2) Their model does have the capability to separately treat AIE and ADE. But in the paper, the authors examined the two effects together. Separating these two may help to answer whether AIE or ADE dominates in this case study.

The decision of using ADE and AIE together as well as using the same factor for both, within one realization, is made after due consideration. Generally, it is possible to have a realization just with AIE (ADE turned off). On the other hand it is not appropriate to have a realization just with ADE since you cannot simply switch off AIE. In any case you have to have aerosol as CCN. If you switch AIE off, you will use the aerosol climatology given in the two-moment scheme of COSMO instead. Simultaneously, in ADE you use the prognostic aerosol of COSMO-ART. This is totally inconsistent and does not allow any conclusions about the single effects of ADE and AIE. The alternative is to use the factors to change the aerosol that is seen by ADE or AIE, as it is done in this study. With this you can reduce e.g. AIE but still (consistently) the same aerosol description (COSMO-ART prognostic) is the basis for AIE and ADE. Now, the question about turning off one of the effects completely changed to the question of using different or the same factors within one realization. We have tested it to gain knowledge by using e.g. ADE0.1-AIE1.0. But again, this is physically inconsistent and cannot be interpreted in terms of realistic conditions in SWA. E.g. in ADE0.1-AIE1.0 the incoming solar radiation would be higher than it would appear in reality (less scattering and absorption), leading very likely to more convective processes (especially sensitive in tropical regions) and therefore implications on AIE that would not occur in the realistic setup ADE1.0-AIE1.0. Using different factors is only appropriate when using the factorialmethod proposed by Montgomery (2015). In my dissertation (Deetz,

2018) I used the factorial method to assess whether the AI frontal shift is caused by AIE or ADE. The findings underline that ADE is dominating the changes, nevertheless we decided not to have this method included in this publication because for robust results an ensemble approach is necessary as realized in Kraut (2015). To have one realization only with AIE (a) and one realization with AIE and ADE together (b) and assessing the effect of ADE by calculating (b) minus (a) is also problematic because this would not only include effects from ADE but also synergistic effects of AIE and ADE. By having the same factor for both effects, every realization is in itself consistent and allows for meaningful, physically-reasonable conclusions. Therefore we decided to use ADE and AIE together and with the same factors.

(3) Title: The paper is about aerosol effects on atmospheric dynamics in a case study. But the title said “cloud and aerosol radiative effects as key players for anthropogenic changes in atmospheric dynamics over southern West Africa”. I think the title is misleading and confusing. FirstïijNËĞ the paper is not about cloud radiative effectsïijN ËĞ though it does talk about aerosol radiative effects through its impact on clouds. But this is different from cloud radiatve effects. Second, the paper only documents aerosol effects on atmospheric dynamics based a case study from model simulations. “anthropogenic changes in atmospheric dynamics” may sound like this is what you observed. As this effect is purely a modeling study, I suggest the authors to clarify this in the title.

We rephrased the title: "Numerical simulations of aerosol radiative effects and their impact on clouds and atmospheric dynamics over southern West Africa"

(4) Section 2.1: model experiments and AIE. It looks like the authors can separately examine the effects of AIE and ADE, but in all model experiments documented here, AIE and ADE are examined together. If the authors examine AIE and ADE separately, this may help to clarify some points the authors made regarding the relative roles of AIE and ADE on SWA. This relates to some of the discussions in Section 6 (e.g., the last paragraph).

Refers to (2).

(5) Section 5: the first paragraph is overly long. Suggest to separate it into several short paragraphs with a focus theme in individual paragraphs.

We agree on that and have changed the manuscript accordingly.

(6) Page 9, line 21: what are these two numbers? The same question is also applied for next three lines (lines 22-24)

As denoted on page 9, line 18-19: "The following values in brackets indicate the median and the 99 th/1th percentile of the surface quantities considering the cloud-free inland area." With this we want to provide quantitative expressions of the observed changes that are consistent between the different meteorological parameters. We decided to present the median and the 99th percentile (in case an increase is observed) or 1th percentile (in case a decrease is observed). This is also consistent for the values in the next three lines.

(7) Page 10, line 4-5: an aerosol increase has large impacts than the aerosol decrease. This is a little bit surprise to me. I would expect when aerosol concentrations further increases, its effects saturate, and its effects decreases (e.g., numerous small particles compete for water vapor so a lower maximum supersaturation is expected). So can you elaborate what might happen here.

Figure 12 (old manuscript version) / Figure 14 (new manuscript version) shows that we have not reached the saturation point. Moreover the strong increase of the CDNC coincides with decrease in the cloud water content, a decrease in the effective radius and a decrease (increase) in light (heavy) precipitation in agreement with the convective invigoration mechanisms of warm clouds as described in Saleeby et al. (2014).

(8) Page 10, lines 12-14: The Twomey effect is also through changes in cloud optical thickness, but not through cloud water. So the second half of this statement is confusing.

We rephrased this sentence: Figure 12 reveals that the aerosol impact on radiation via the Twomey effect is very likely dominating the cloud-radiation interaction, whereas changes in the cloud water, that can also impact the radiative transfer and therefore the cloud formation, are insignificant.

(9) Page 10, lines 28-29: this statement is not clear to me (“it is interesting that ...”).

"Nevertheless, it is interesting that the location of the AI front during its stationary phase over Ivory Coast could be used as a proxy for the aerosol burden in that area (under otherwise identical conditions)." The balance between the onshore monsoon flow and the vertical mixture of momentum over land due to turbulence leads to stationarity of the AI front around noon. The higher the aerosol burden, the smaller the vertical mixture of momentum over land and the more the onshore monsoon flow dominates in the denoted balance. Therefore the AI front is shifted inland but is still stationary. With that, the location of the AI front around noon, relative to the coast, is a proxy for the aerosol burden over land (at least in the model under otherwise same conditions).

(10) Page 10, the pressure gradient mechanisms: Here sea surface temperature was not affected by aerosol loading. So this overestimates the effects of aerosols on land-sea temperature differences. Any discussion on this?

Theoretically, we can expect a decrease in SST with increasing aerosol due to less incoming shortwave radiation via scattering and absorption. This SST decrease might lead to a decrease of the near-surface temperature over the ocean. (The near-surface temperature decrease over land due to the aerosol is actually happening in the model.) All in all, this would lead to a stronger land-sea temperature gradient and therefore to a less weak AI inland propagation compared to the results of the polluted case in the manuscript. To study aerosol effects on the SST and its feedbacks on AI, the NWP mode of COSMO is not appropriate. For that, climate studies with the CLM version of COSMO are more meaningful. However, we don’t expect significant signals on SST, because on the one hand the surface water is continously moved by the Benguela

current and Southern Equatorial current and on the other hand, the biomass burning aerosol is reaching the Gulf of Guinea not continously but it arriving in plumes as visible e.g. in model realization of COSMO-ART and MACC. Therefore there will be no continuous cooling but a variation of cooled and warmed areas that might counterbalance. Furthermore, a regional (limited-area) model is less appropriate for studying this question, because only the near-surface water within the domain is affected but the incoming water is unaffected. Therefore a global model, consistently having the aersol impact on SST globally considered, would be more appropriate for this questions. We added the following sentence in the conclusions: "Effects on SST are not considered in this study. In case of considering the impact of reduced incoming solar radiation on the SST with increased aerosol, stronger land-sea temperature gradients are expected. Therefore, the estimations of this study with fixed SST denote the upper limit of the magnitude of the effects. However, this model setup in numerical weather prediction mode is less appropriate to study effects on SST. Global models on a longer time scale are more suitable to provide added value on this question."

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