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Interactive comment on "Did the Roman Empire affect European climate? A new look at the effects of land use and anthropogenic aerosol emissions" *by* Anina Gilgen et al.

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We cordially thank the reviewer for his/her careful reading, the well-conceived comments, and the helpful suggestions. Our answers are italicised.

Major comments

The discussion on climate is fairly thin, compared to the bulk work presented in the manuscript. Granted, a number of metrics are being provided, but no in-depth analysis is provided, at least nowhere near the technical description of the model.

For example, terms like evaporative fluxes, turbulent fluxes, precipitation, cloud cover, liquid water path, are simply presented but never analyzed. In a climate-focused paper, I would expect a much deeper analysis of these results, and discussions on how they influence each other, and how each one is affecting climate. Other examples of interest might be how precipitation changes between simulations might lead to drought frequency changes, how cloud cover can alter lightning (important for fires), etc. Last but not least, I was not convinced that the anthropogenic influence 2000 years ago did indeed impact climate significantly, as was promised in the abstract. The paragraph in page 26, around line 30, was a strong contributor to this. There is no discussion on how uncertainties of the rest of the world affect results. I would expect that these uncertainties would be at least as large as in Europe, if not larger. How do these affect the base model climate over the region, via teleconnections and long-range transport? The authors correctly claim (and cite relevant literature) that the quantification of the anthropogenic impact on present-day climate depends on the choice of the preindustrial year of reference, and on whether this is 1750 or 1850. Why they don't explore this in their model for this study via rest-of-the-world influences? No sophisticated analysis would be required, just a ballpark halving/doubling of the global emissions. As clearly stated a couple of times in the manuscript, surface air temperature is not a good metric, due to the model design (fixed SST). Still, the authors frequently refer to it, and even present several results and a figure about it. I would strongly recommend to not do so, since what they get is a muted response due to the fixed SST assumption, and the fact that they present surface temperatures over land only does not change this fact. This is evident when they used a mixed-layer ocean, and the signal increased 5- fold. As a matter of fact, since the model is capable of simulating a mixed-layer ocean, and the authors did a couple of experiments with it, why not do all of them with such a configuration? The supplementary material contains a lot of very useful and interesting information. I found that almost the whole supplementary material can fit into a standalone technical paper, and then the main manuscript can be a climate-focused paper. Then tables S8-S12, which contain a lot of numbers which remained virtually uncommented in the text, can be promoted to the main body instead of the supplement, where they belong, at least in my opinion.

We understand the reviewer's point of view and agree that the climate analysis is expected to be deeper considering the title and the abstract of the paper. Our main interest was whether changes in anthropogenic land cover and aerosol emissions had an impact on some climate variables around AD 100. We admit that we were especially interested in how the land surface temperature changed and thus focused in our analysis on variables that could lead to such changes (e.g. changes in surface albedo, turbulent flux, or aerosol/cloud radiative effects). However, we fully agree with the reviewer that the focus on surface temperature is somehow problematic because of the model setup with fixed SST. We conducted simulations with fixed SST because the simulations take a lot of (computational) time (mainly because of the sectional aerosol model including secondary organic aerosols) and simulations with fixed SST are considerably shorter than those with a mixed-layer ocean model since a much longer spin-up is needed to equilibrate even a mixed-layer ocean model. For the same reason, we were limited in the number of simulations that we conducted. Furthermore, one anyhow first needs simulations with fixed SST to drive the mixed-layer ocean model. Out of curiosity, we then repeated two of the simulations with the mixed-layer ocean model to see how different the results are. As mentioned in the paper, the surface temperature showed stronger and more widespread changes with the mixed-layer ocean model. Nevertheless, the changes in surface temperature are qualitatively similar (see new Supplementary Fig. S4): a regionally very limited warming in Syria likely due to the changes in land cover and, more importantly, a more distinct and more widespread cooling in Europe due to the changes in aerosols. (Since we only calculated anthropogenic aerosol emissions for the Roman Empire, the cooling effect of the aerosols is mainly restricted to the study domain.) We thus believe that our simulations with fixed SST have some value and can provide information about the approximate location and sign of the expected surface temperature change, although we agree that the result is

rather qualitative than quantitative. We wrote a paragraph in the Section "Uncertainties and Limitations" about this.

Considering that the climate analysis is restricted to some variables and in the case of land surface temperature of qualitative nature, we followed the reviewer's suggestion and shifted the focus of this paper somewhat more to the technical aspects, i.e. the method and the experimental setup. As a consequence, we rephrased the title, the abstract, and part of the conclusions. Moreover, we integrated a large part of the Supplementary Material to the main manuscript. The results of the simulations with the fixed SST are now shortened and it is stated that more simulations using a mixed-layer ocean model are needed for more quantitative results. We now refrain from showing specific numbers of changes in surface temperature in the text and the tables. As a consequence, the section "Combined climate impact of anthropogenic land cover change and aerosol emissions" was deleted. However, we still leave some figures showing changes in surface temperature for qualitative purposes (e.g. to show that the changes in turbulent heat fluxes induce a local warming for KK11). In a follow-up paper including more simulations with the mixed-layer ocean model, we have then the possibility to have a deeper look at climate processes, including e.g. changes in precipitation and teleconnections as suggested by the reviewer.

We agree that the paragraph on page 26 sounds like the anthropogenic aerosols only have an effect due to a possible underestimation in natural aerosols and the lowered minimum CDNC. However, we expect that the anthropogenic aerosol emissions would still have a cooling effect (though a reduced one) with the standard CDNC minimum threshold of ECHAM-HAM (40 cm^{-3}) because the CDNC background concentrations in our study region are between 100 and 200 cm^{-3} in the lower troposphere (averaged over time and the study domain for the simulation no_human) and the absolute increases in CDNC are rather pronounced, especially for the high emission scenario (roughly 100 cm^{-3}). We therefore rephrased the abstract and added the following paragraph to Section "Uncertainties and limitations": ***"Nevertheless, we expect that***

the anthropogenic aerosol emissions around AD 100 would still increase the CDNC in the Roman Empire and thus induce a cooling effect with the standard minimum CDNC value of 40 cm^{-3}: the simulated CDNCs are above 40 cm^{-3} in the lower troposphere for all seasons when averaged over our study domain (not shown). The strong total ERF$_{\mathrm{ari+aci}}$ with a minimum CDNC of 1 cm^{-3} therefore mainly results from other regions, e.g. the Arctic and the Antarctic."

Specific comments (please read e.g. 3.30 as page 3, line 30)

- Throughout the manuscript, the years reported (e.g. AD 1, AD 10, AD 100) are described as literally *those* years. Instead, I believe the authors mean that these are climatological means around those years, and not that e.g. AD 1 is different from AD 2. This should be rephrased across the manuscript.
- *The reviewer is correct. Some data that we used has a low temporal resolution since the uncertainties are large. As an example, the data from the HYDE database has a resolution of 100 years. This data is therefore indeed representative for the period around, for example, AD 100. However, when we selected one "year" from such a database, we refer to this specific year (e.g. "AD 100") to make our method comprehensible and also to match the data as presented in the database exactly. Other data, e.g. the output of the simulations, is indeed always referring to climatological means. We went through the paper again and rephrased the sentences when necessary.*
- The authors do acknowledge their attempt to construct consistent scenarios when modifying model parameters for the low/intermediate/high estimates. However, it is not clear whether they checked that crop yield (and the implied animal husbandry from pasture lands) consistently provides the necessary food to feed the different populations across the scenarios, or the food chain is broken so that

either too little or too much food is being produced. A comment on this would be appreciated.

- *Concerning the crop yield, we considered the population size to assure that the food chain is not broken. We now explicitly mention this in the text. When estimating pasture burning, the link between population and the pasture burning emissions was less strong. We used the HYDE reconstruction with the lower pasture area for the low and the intermediate population size, while we used the KK11 reconstruction with the higher total pasture area for the scenario with the highest population size. This results in pasture areas per capita between 0.58 ha and 1.0 ha for the different emission scenarios, which is in reasonable agreement with other studies; the values are very similar to Goldewijk et al. (2011, 2017) and somewhat lower than in Weiberg et al. (2019) (1.75 ha). We now mention these numbers in the paper.*
- 3.30: "were reduced" needs to be expanded in the main text, right now this information only exists in S3.
- *As suggested by the reviewer, we moved Sect. S3 from the supplementary material to the main manuscript.*
- Page 6, paragraph around line 5: The differences between the model-used data and the original estimates is very large. I was very much surprised by the values up to 41%. Is this really due to different datasets and regridding? Which assumptions introduce the largest changes from original to model-used estimates?
- *We apologise for having made a mistake in the text, for KK11 the underestimation is even 47% instead of 41%. Part of the underestimation is related to the binary land sea mask. Part of the crop and pasture areas lie in locations that are ocean grid points in our model, where of course no vegetation can grow. The much larger underestimation for KK11 compared to HYDE11 is however related*

to areas that are subject to anthropogenic land use in the KK11-reconstruction, but not hospitable to plants in the model because they are desert (e.g. large part of the Arabian Peninsula). We added these explanations to the paper.

- 8.5-8: Given the large differences in emissions factors for heating (presented in 7.32- 33) isn't this a gross oversimplification?
- *This is true. We changed the text to:* ***"Although more fuel for heating was generally consumed where and when it was cold (Malanima et al. 2006, Warde et al. 2006), we assume a constant fuel consumption over the year and over latitudes since we do not differentiate between heating, cooking, iron production, and other burning activities in our calculation. ... Given the large uncertainties about the relative importance of residential heating to total fuel consumption, this seems justified as a first order approximation."***
- 8.20: I guess another important assumption made here is that the ratio between free/enslaved people also remained the same?
- *The ratio between free people and slaves needs indeed also to be considered. We now mention this in the paper.*
- Page 11, "over 20 years" (line numbering is off there): Is this statement referring to a climatological analysis, or a transient simulation?
- *Climatological; we added this for clarification.*
- 15.9-12: How much lower were the CDNCs in the AD 100 simulation compared to preindustrial?
- *In our simulations, the CDNC burdens in the Roman Empire in AD 100 are between 2.6 and* $5.8 \cdot 10^{10}$ *m*$^{-2}$ *(depending on emission scenario). In our preindustrial simulation, it is* $6.0 \cdot 10^{10}$ *m*$^{-2}$*. On the global average, the values were*

between 3.1 and $3.2 \cdot 10^{10}$ *m*$^{-2}$ *for AD 100 and* $3.6 \cdot 10^{10}$ *m*$^{-2}$ *for preindustrial (we didn't estimate anthropogenic emissions outside the Roman Empire for AD 100). This highlights that using a high CDNC minimum threshold would generally have a larger effect in our AD 100 simulations than in our preindustrial simulations. The threshold has the largest effect in the polar regions (Arctic and Antarctic).*

- Also, what are other studies do in terms of the lower threshold of CDNC, for studies where humans were irrelevant (e.g. LGM)?
- *This is a very interesting question. For PMIP4, the LGM experiments should generally have the same protocols and external forcings than the CMIP6 DECK piControl simulations for 1850 (Kageyama et al. 2017), i.e. the CDNC minimum threshold seems to remain unchanged. However, aerosols and especially aerosol-cloud interactions have not yet been the focus of LGM studies, with the exception of dust (Albani et al. 2018). The study by Takemura et al. (2009) analyses the radiative effect of soil dust in the LGM, and their model further includes indirect aerosol effects. They found that "the positive radiative forcing from the indirect effect of soil dust aerosols is mainly caused by their properties to act as ice nuclei". However, they refer in their paper to the study by Takemura et al. (2005), where a minimum CDNC of 300 cm*$^{-3}$ *over land and 30 cm*$^{-3}$ *over ocean is mentioned. We therefore expect that indirect effects that affect liquid clouds (e.g. the Twomey effect) are heavily suppressed.*
- 15.16 and 19.8: I believe S8-12 contain useful information and they should not be in the supplement. Their discussion should be largely expanded as well.
- *As mentioned previously, we decided to shift the focus of this paper to the technical aspects. Therefore, we decided to leave the tables in the supplement.*
- 24.6: "5 times stronger" refers to values over land only?

- *The "5 times stronger" referred to both land and sea. Since we use the land surface temperature in the rest of the paper, we changed the sentence. Over land, the increase is 4 times stronger.*
- Page 25, first half: Why discuss so much about nitrate aerosols, which were not included in the study?
- *The omission of nitrate aerosols in the model could lead to regionally too low CCN concentrations. However, we agree with the reviewer that our discussion on this topic was too long and we shortened it accordingly.*
- 26.21: Please add "strongly" in front of "underestimated".
- *Done.*
- Section S3 mentions interannual variability. Is this in a transient or climatological sense?
- *In a climatological sense; there is no trend in fire emissions over the years.*
- Section S4, page 4, middle: Why not scale a_n with population density or crop use (or both)? Even an empirical scaling would have been better than a constant value with time, something like what was done with the vegetated area per gridbox.
- *This section of the text was not well formulated. The fire emissions for AD 1850 depend on the population density. a_n is just a scaling factor which should reflect cultural differences. We rephrased the section to make this clearer.*
- Section S4, page 4, middle: "over 1830 and 1840" means literally these years, or a climatology from 1830 to 1840, or something else?

- *It means literally these years; data is available on a 10-year resolution. We rephrased the sentence.*

Technical corrections

- Page 1, line 6: Please explain HYDE and KK11 in the abstract.
- *We changed the abstract and do not mention HYDE11 and KK11 anymore.*
- 6.13: Is "e.g." correct, or "i.e." was meant to be there? In either case, this sounds like a vague statement, where a solid reference should be provided for the model description.
- *The evaluation paper of the VBS model (planned to be submitted to GMD) is unfortunately still in preparation, but will be submitted soon. The model is described briefly in Mielonen et al. (2018) and Stadtler et al. (2018). We added these and other references as well as some text to describe the VBS.*
- 12.32: "Sect. S7" should had been "Sect. S6"? S7 seems irrelevant there.
- *Yes, thank you for the careful reading, we corrected it.*
- 16.7: Please add a comma after "harvest".
- *Done.*
- 24.5: What is the depth of the mixed-layer ocean?
- *50 m; we now mention this in the text.*
- 24.20: Please fix typo "smoldering".
- *Done.*

- Table S1: please change “degree” to “degrees”.
- *Done.*
- Table S2 legend: Please define what is meant by “some” aerosol emissions.
- *We changed the sentence to: "Overview of natural fire aerosol, anthropogenic aerosol, and SOA precursor emissions in the different ECHAM-HAM-SALSA simulations."*
- Section S3 is very interesting and important, similar to section 2.5 in the main text, I feel it belongs to the main manuscript text.
- *Done.*
- Section S3, line 4: please define what is meant by “some” selected 30-year periods.
- *We changed the sentence to: "... from which output at a high temporal resolution was saved for a few selected 30-year periods (among them a period around AD 1 and one around AD 1835)."*

References (not included in the paper):

- Kageyama, M. et al.: The PMIP4 contribution to CMIP6 – Part 4: Scientific objectives and experimental design of the PMIP4-CMIP6 Last Glacial Maximum experiments and PMIP4 sensitivity experiments, Geosci. Model Dev., 10(11), 2017
- Albani, S. et al.: Aerosol-Climate Interactions During the Last Glacial Maximum, Current Climate Change Reports, 4(2), 2018

- Takemura, T. et al.: A simulation of the global distribution and radiative forcing of soil dust aerosols at the Last Glacial Maximum, Atmos. Chem. Phys., 9(9), 2009
- Takemura, T. et al.: Simulation of climate response to aerosol direct and indirect effects with aerosol transport-radiation model, J. Geophys. Res.-Atmos., 110(D2), 2005

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