

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.

Anonymous Referee #2

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The manuscript by Gilgen and co-authors describes a series of climate model simulations during the peak of the Roman Empire, in which they study the role of anthropogenic activities on climate at the continental scale. The manuscript is very well written, with ample information that explain the experiments, the data sources, the assumptions made, and the associated uncertainties. Had this been a more technical paper describing the method only, I would have had only trivial comments to make. However, since the goal of the paper is beyond the construction of a model, and tries to explain (with uncertainties) the anthropogenic influence on climate during the Roman era, I am afraid that I have more substantial comments.

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Major comments

CPD

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 SSTs). 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-

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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.

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 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.

3.30: “were reduced” needs to be expanded in the main text, right now this information only exists in S3.

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?

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8.5-8: Given the large differences in emissions factors for heating (presented in 7.32-33) isn't this a gross oversimplification?

8.20: I guess another important assumption made here is that the ratio between free/enslaved people also remained the same?

Page 11, "over 20 years" (line numbering is off there): Is this statement referring to a climatological analysis, or a transient simulation?

15.9-12: How much lower were the CDNCs in the AD 100 simulation compared to preindustrial? Also, what are other studies do in terms of the lower threshold of CDNC, for studies where humans were irrelevant (e.g. LGM)?

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.

24.6: "5 times stronger" refers to values over land only?

Page 25, first half: Why discuss so much about nitrate aerosols, which were not included in the study?

26.21: Please add "strongly" in front of "underestimated".

Section S3 mentions interannual variability. Is this in a transient or climatological sense?

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.

Section S4, page 4, middle: "over 1830 and 1840" means literally these years, or a climatology from 1830 to 1840, or something else?

Technical corrections

Page 1, line 6: Please explain HYDE and KK11 in the abstract.

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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.

12.32: “Sect. S7” should had been “Sect. S6”? S7 seems irrelevant there.

16.7: Please add a comma after “harvest”.

24.5: What is the depth of the mixed-layer ocean?

24.20: Please fix typo “smoldering”.

Table S1: please change “degree” to “degrees”.

Table S2 legend: Please define what is meant by “some” aerosol emissions.

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.

Section S3, line 4: please define what is meant by “some” selected 30-year periods.

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-56>, 2019.

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