Dear Reviewer #2,

Thank you very much for your very helpful suggestions. We will address all points and include them in the revised version. Please see below point-by-point responses. We very much appreciate your support and hope the revised version will meet your expectations.

On behalf of all co-authors, yours sincerely,

Eva Hartmann

RC2

Review of: Climatic Impacts of the 536 and 540 CE Volcanic Eruptions Simulated with COSMO-CLM over the Middle East and Africa

1. Does the paper address relevant scientific questions within the scope of CP?

This manuscript describes regional climate simulations over a region spanning the eastern mediterranean and middle east with a focus on the impacts of two major sixth century volcanic eruptions. These eruptions and their climate impacts have been widely studied, with proxy evidence and prior modeling studies suggesting strong temperature anomalies especially in Northern Europe, and eyewitness accounts of solar dimming in the Mediterranean. This general topic falls well within the remit of Climate of the Past, however, the authors fail to identify specific scientific questions that the study addresses. It reads as if the rationale is that no regional simulation has been performed yet for this period and region, so here it is.

Thank you very much for your positive feedback. In the revised version, we will improve the rationale towards more focused scientific questions and hypotheses (based on empirical and modelling evidence).

2. Does the paper present novel concepts, ideas, tools, or data?

This study appears to be the first to perform regional simulations for this time period over this particular region. However, it fails to highlight any advantage of the regional simulations compared to existing simulations or any aspect of the results which advances our understanding of the time period or events studied.

We will revise the manuscript to explain more clearly the advantages of our regional simulations over the coarser-resolution global simulations available in the literature.

We will also clarify how the results improve the representation of regional climatic variability and advance understanding of the periods and events examined.

3. Are substantial conclusions reached?

From the abstract, the main conclusions are vague and not novel—it has already been shown many times that the sixth century eruptions have a major impact on temperature, and it is not surprising that the precipitation anomalies are small and spatially variable. It is not clear what new knowledge this study provides. The conclusions section seems mostly to summarize the results, and suffers from a lack of comparing those results to prior studies, which would help to highlight the (potential) novelty of the regional simulations.

We will rewrite the conclusions. We will add a larger discussion with a comparison to prior studies to a merged Results and Discussion section, which will also help to improve the manuscript's consistency and sharpen its focus.

4. Are the scientific methods and assumptions valid and clearly outlined?

The models and methodology are fairly well described. Given the importance of the volcanic aerosol forcing, it could be useful to describe the spatial and temporal structure of the forcing in some detail. The use of regional subdomains in the analysis I think it detrimental to the description of results and attempt to understand them, more details are given below.

The implementation of the external forcings, including the volcanic aerosol forcing, is described in detail in Hartmann et al. (2025), also published in *Climate of the Past*:

Hartmann, E., Zhang, M., Wagner, S., Adakudlu, M., Luterbacher, J., and Xoplaki, E.: On the implementation of external forcings in a regional climate model – a sensitivity study around the Samalas volcanic eruption in the Eastern Mediterranean/Middle East, *Clim. Past*, 21, 1699–1724, https://doi.org/10.5194/cp-21-1699-2025, 2025.

Nevertheless, given the importance of volcanic forcing for the present study, we will add a short paragraph describing its spatial and temporal implementation. We acknowledge the reviewer's concern regarding the use of regional subdomains. However, the full model domain is large and characterised by substantial spatial variability, making it difficult to discuss the results solely as domain-wide aggregates. Conversely, relying only on maps would make the presentation overly

complex. We therefore intend to retain the subdomain-based analysis while revising the presentation to make the regional comparisons and their interpretation clearer.

5. Are the results sufficient to support the interpretations and conclusions?

Generally the manuscript provides few conclusions and interpretations based on the results. However, there are some instances of conjecture in the conclusions section, particularly starting at line 406 regarding the links between circulation and precipitation anomalies.

We will work on this issue by highlighting the novelty and drawing clearer and more focused conclusions (see also answer above and for Reviewer 1).

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

There is a fairly good description of the simulations.

Thank you! We will further elaborate this section as well.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

The introduction includes references to many relevant papers, although some important studies are missing. Foundational works from Stothers (1984) and Larsen et al. (2008) could well have been included. I was surprised that there was no reference to Arjava (2005), who discusses the documentary evidence collected in the Mediterranean region and argues that the climatic impacts in this region were minimal. Recent work on records the Nile flow and the impact of volcanic eruptions (e.g., Manning et al., 2017; Singh et al., 2023) are quite relevant. And more generally, the work of Iles et al. (2013, 2014, 2015) on volcanic eruptions and precipitation would help provide context to the description of precipitation anomalies in this work. On the other hand, aside from the final sentence, the conclusions included no references, which made it near impossible to understand how the study's results are or are not consistent with past studies and therefore what the present study contributes to the understanding of the climate impacts of the 6th Century eruptions.

Thank you very much for the links to the additional important literature which will be included in the revised version of the manuscript. It will also be used to expand

and improve the Discussion part, which is obviously falling short in the current version.

8. Does the title clearly reflect the contents of the paper?

Yes.

Thank you.

9. Does the abstract provide a concise and complete summary?

Yes, with a lack of concrete scientific questions and conclusions.

Thank you, we will improve those aspects.

10. Is the overall presentation well structured and clear?

Some figures are borderline unreadable, e.g., Fig. 4-7 due to having too small text and lines. These figures are also overwhelming in terms of the information content and would benefit greatly by further reduction of the data into manageable pieces.

Thank you very much for this very important hint. The figures will be revised accordingly to improve the presentation, readability, and interpretation of results.

11. Is the language fluent and precise?

Mostly the text is well-written but there are instances where a vagueness in the language is districting and potentially misleading. Examples given below in specific comments.

Thank you very much. We will work on it thoroughly.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Fine

Thank you.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Figures should be revised to increase readability.

We will account for this in the revised version.

14. Are the number and quality of references appropriate?

More references needed in Conclusions.

We will add additional and relevant literature throughout the manuscript, especially in the Results and Discussion section.

15. Is the amount and quality of supplementary material appropriate?

Figures in the appendix will generally be ignored, it could be considered whether the information there is important enough to include, and if so if it could be incorporated into the figures of the main text in a more efficient way.

Thank you very much for this comment. We will address this.

Specific comments:

Thank you for your very comprehensive comments on the specific points that are all of relevance to improving the manuscript. We will consider each of them carefully and revise the manuscript accordingly. Where no explicit response is provided, we will incorporate the suggested change into the revised manuscript. We used the subdomains to summarise the results across the very large model domain by grouping them into climatologically meaningful regions. We will nevertheless consider how to improve the presentation and interpretation of these regional results. We would also like to clarify that Fig. 1 does not represent the entire simulation domain: it shows only a small part of the model domain. The correct and full domain is shown below. Once the full domain is displayed, the division into seven subdomains, most of which are broadly comparable in size, should be easier to understand.

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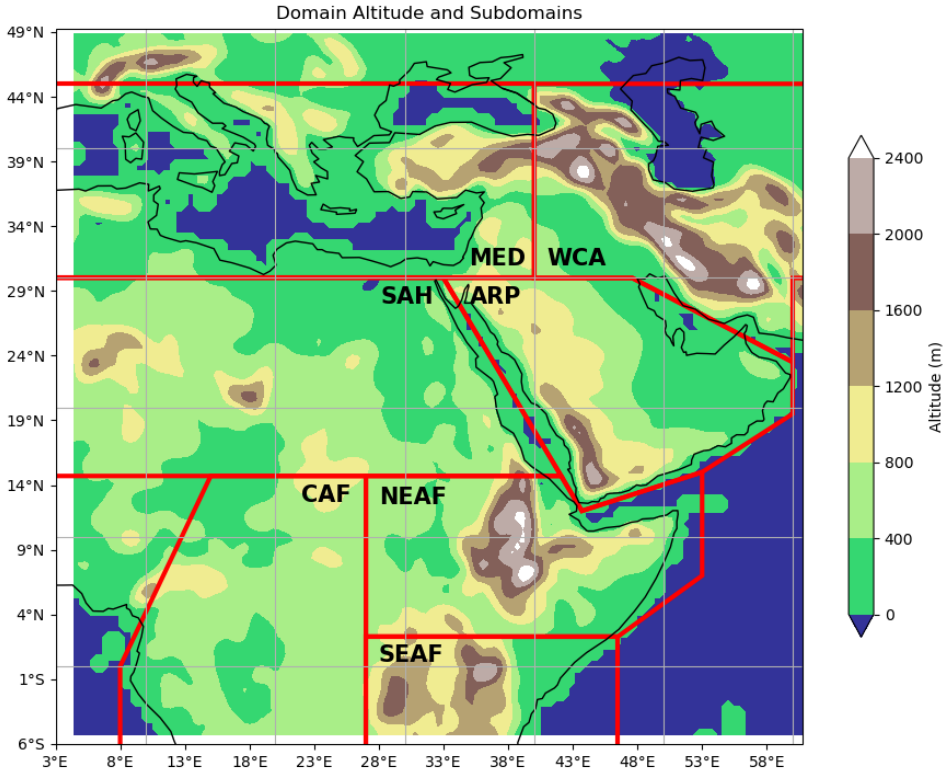

The results are presented by averaging over 7 sub-domains of the regional model domain, which are defined based on the IPCC reference regions for subcontinental analysis. There are a couple major problems with this. Firstly, it appears that few (perhaps only 1?) of the subdomains are included completely within the domain of the regional model—in fact, in Fig. 1 only 4 of the subdomains are labeled because it seems that 3 subdomains only have extremely small areas within the model domain. It is never explained, but I assume that when these subdomains are referred to in the text, and results given, these results apply to the very small region of the defined subdomain which is included in the regional model domain. This is extremely problematic since the values given in the study will be unrepresentative of the subdomain as a whole, and anybody taking results of the study at face value could be easily misinterpreting the results. I would suggest that at minimum, the 3 subdomains for which only a small part of the subdomain is included in the study region should be excluded completely from the study. Further, the authors should be very clear about what proportion of the other 4 subdomains are included in the simulation domain and so how representative the results given are to those subdomains in general.

On the other hand, I don't find that analyzing the results in the subdomains is generally helping the interpretation of the results, and that showing maps of the anomalies would instead be much easier for a reader to process, and that if a separation into subdomains was to be used, then one based on actual physical processes (e.g., dry vs. wet areas) would be much more intuitive.

We hope that the updated figure will help to better understand and reflect results. The subdomains were not selected arbitrarily, but represent areas with broadly similar climatic characteristics, including the timing of dry and wet periods and comparable precipitation and temperature distributions.

Line 1: There is nothing grammatically wrong with this sentence, but the way it is formed seems to imply that the historical developments in these regions are somehow more "marked" than in other regions, which would not be accurate. I would suggest a rephrasing which is more specific about why there is interest in these regions.

Line 3: Are efforts really "limited" in this region? I would guess most simulations that cover this period are global (i.e. there are few regional simulations for this period) and so the effort is fairly well distributed spatially over the globe. Isn't it more that the relative abundance of documentary and archaeological evidence in this region makes it so interesting?

*Line 7: The sentence structure is implying that the eruptions "are accompanied by large scale circulation anomalies", but importantly those anomalies are also *produced* by the volcanic aerosol radiative forcing.*

Line 8: "simulation" or "simulations"?

Line 27: there are many older studies that quantify the cooling of the 536 and 540 eruptions, we shouldn't forget about them.

Line 31: the two eruptions were not "concurrent".

Line 66: simulations cannot "confirm" that something happened or didn't happen in the real world. They can help explain how things happened, or help us test hypotheses, but they should not be taken as evidence about the real world.

Line 67: "However, discrepancies remain among data sources regarding the timing and regional extent of this cooling." This statement requires proof—is this the conclusion of

prior studies? Or is this your interpretation of a collection of past studies? Those studies need to be discussed and cited!

Line 73: "related contexts" here is precisely the same time period and set of eruptions as studied here! I think given the similarity, the works of van Dijk deserve more than a passing mention in this introduction section.

Line 81: here and elsewhere, some problems with using in text vs. parenthetical citation style.

We will increase the importance of previous studies mentioned in the text and be more precise in our wording.

Line 81: unclear what "reducing uncertainties" means here. What uncertainties? How can one or two simulations reduce those uncertainties?

Line 100: "historically important region" could be taken as somewhat culturally biased or insensitive, I would suggest to keep a neutral stance on the relative importance of different parts of the globe unless there are particular scientific reasons/questions to motivate focus on particular regions.

Line 101: again, isn't this region actually one where we have more data than is typical for this time period?

Yes, it is, especially for historical documents, societies and civilisations. And this is where the regional climate model can help us identify and study connections between processes, mechanisms, and consequences. We will make sure to take a more neutral position in the revised version.

Line 144: both the global and regional simulations are transient, so this label does not help to distinguish which simulations are being referred to here (and elsewhere).

Figure 1 caption: I believe "elevation" would be a more appropriate term than "altitude" for the topography. CAP, SEAF, and NEAF are mentioned in the caption but not labeled on the plot.

line 197: This statement seems rather clear but remains unquantified and so rather qualitative. It also is notably consistent with the analysis of tree ring records by Sigl et al. (2015).

Figure 2: unfortunately, the vertical bars highlighting the peak positive and negative T anomalies block out the T values in those years. A better choice of highlighting would allow the reader to see the full timeseries.

Line 199: it's important to point out these eruptions are strong in terms of their SO2 injection to the stratosphere and associated radiative forcing, not necessarily the magnitude of their magma release (which is how volcanologists rank the severity of eruptions).

Line 208: which two eruptions?

We will be working on a more comprehensive presentation of these volcanic eruptions of the years 536 and 540 in the LALIA period and explain the background.

Line 211: Is there a scientific reason to examine longer time periods?

The long transient RCM and GCM simulations are scientifically valuable for several reasons: It allows direct comparison with transient global simulations, climate reconstructions, and long proxy records. It also supports interdisciplinary analyses of different periods within a consistent modelling framework, as all periods are derived from the same simulation and therefore share the same data source and methodological basis.

Line 258: This section heading seems misplaced since the last section also dealt extensively with regional anomalies.

We agree on that point and suggest as new title: Regional DJF and JJA climate anomalies

Figure 3: see comment to Figure 2.

Figures 4 and 5: The text on these figures is way too small.

Figure 6: again, the text is way too small here, and the reader is wondering what the purpose of this level of detail on the anomalies is. What question is this all relevant to?

Figure 7: At a zoom level of 300% one can start to make out the text labels and arrows on the panels. The authors are asking for a great deal of effort from the reader to process the information in these figures…

The figures will be fully revised with a special focus on readability.

Line 388: This statement seems to be a rare case of an interpretation of the results, but I cannot understand it or how the results support the statement. By definition, "climate" includes more than just temperature, so how are the results of this study demonstrating that the climatic response to the 536 and 540 eruptions "cannot be understood in terms of cooling alone"?

Line 401: yes, the anomalies are regionally diverse, but can they be understood in terms of changes in large-scale circulations, e.g. shifting of the ITCZ?

Line 417: It is always hard to quantitatively define the "end" of a decaying signal, I wonder how you are doing it here?

Line 418: The "sustained cold period" you refer to, is this a hypothesis based on evidence that you are addressing with your study? If so, cite the source of this hypothesis and discuss more fully the implications.

Line 422: What modeling work? Give citations.

To answer the comments on the Conclusions section: It will be fully restructured and made more focused. A more extensive interpretation of the results, supported by the relevant literature, will be included in the new combined "Results and Discussion" section. We will also revise the wording carefully and ensure that all statements are appropriately supported by the literature.

References:

Arjava, A.: The Mystery Cloud of 536 CE in the Mediterranean Sources, Dumbart. Oaks Pap., 59, 73–94, 2005.

Iles, C. E. and Hegerl, G. C.: Systematic change in global patterns of streamflow following volcanic eruptions, Nat. Geosci., 8, 838–842, https://doi.org/10.1038/ngeo2545, 2015.

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Iles, C. E., Hegerl, G. C., Schurer, A. P., and Zhang, X.: The effect of volcanic eruptions on global precipitation, J. Geophys. Res. Atmos., n/a-n/a, https://doi.org/10.1002/jgrd.50678, 2013.

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Hantemirov, R. M., Naurzbaev, M. M., and Nicolussi, K.: New ice core evidence for a volcanic cause of the A.D. 536 dust veil, Geophys. Res. Lett., 35, https://doi.org/10.1029/2007GL032450, 2008.

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Singh, R., Tsigaridis, K., LeGrande, A. N., Ludlow, F., and Manning, J. G.: Investigating hydroclimatic impacts of the 168–158 BCE volcanic quartet and their relevance to the Nile River basin and Egyptian history, Clim. Past, 19, 249–275, https://doi.org/10.5194/cp-19-249-2023, 2023.

Stothers, R. B.: Mystery cloud of AD 536, Nature, 307, 344–345, https://doi.org/10.1038/307344a0, 1984.

Thank you for the suggestions, which we will include in the revised version.