Point by point answers to the comments from reviewer#1 to the manuscript entitled "Effects of Transient Processes for Thermal Simulations of the Central European Basin" by Degen and Cacace.

Anonymous Referee #1

Received and published: 2 December 2020

I reviewed the manuscript of Degen & Cacace titled "Effects of Transient Processes for Thermal Simulations of the Central European Basin". The manuscript discusses the influence of transient processes in the subsurface temperature distribution for sedimentary basin systems. The numerical model of the Central European Basin and the sensitivity analysis of its thermal parameters are presented as a case study to evaluate their influence on the temperature field.

The work presents a new methodological approach (i.e., Reduced Basis method) to address the sensitivity analysis in thermal numerical models. This approach has been never tested in this context, and its novelty is well stated in the Introduction and method. The results of the model and the sensitivity analysis of the thermal properties are well presented and discussed.

However, the architecture of the model should be described more in detail. The Authors reported the references of the works from which they derived the lithosphere-scale geological model of the CEBS (l. 156-157), its lateral extent, and general, brief, list of the investigated units. However, at l. 169-170, the Authors state that they are investigating the impact of the thermal properties of different chrono-stratigraphic units. At l. 193, the "Upper Crust Baltica and Avalonia" are cited and later on their thermal conductivity is reported as an "influencing thermal property". How can the Reader understand or partially figure out the extent of these units (i.e., chronostratigraphic or crustal units) and if they are spatially relevant? I suggest to: i) slightly extent the description of the geological model in the text adding some relevant information about the model architecture (for example, the thickness of the model is missing), construction, and the main units geometries, ii) add a brief lithological and spatial (i.e., maximum and minimum depths, geographical position if relevant, total volume, etc.) description of the chronostratigraphic or tectonic units in table form (this would be additionally helpful since acronyms are used to refer to these units in the figures of the results section, but these acronyms have been never explicitly stated), and iii) add a few cross sections of the geological model. I think that the impact of the obtained

result would be clearer/more relevant if these data are explicitly stated in the manuscript. Especially the volume of the units could be of interest since, as the Authors are stating at l. 343, "the sensitivities of the steady-state model are mainly controlled by a combination of the volumetric contributions of the individual layers and … ". Although the information about the model could be gathered from the literature, the work would be more complete and self-standing in my opinion.

In our original submission we did not provide details of the 3D structural model used as input for the sensitivity analysis. This was done to keep the paper light and easy to read/follow and not to "distract" the reader from the main topic of the study, that is, the description of a novel methodology to address global sensitivity analysis for thermal model in sedimentary basins. This is also manifested in our decision to target GMD as a journal where to publish our results, given the novel theoretical and computational aspects that make up the bulk of the study. In this regard, we consider the application to the CEBS as a "proof of concept" rather than the core of the study as also detailed in our final discussion to the manuscript.

This said, given also a similar comment from reviewer#2, we have integrated all requested information on the 3D structural model by expanding its description in the main text as well as in the form of two additional figures and a descriptive table in the revised version of the paper.

In addition, I have a concern regarding the long-term simulation (0 ka – 255.7 Ma). This simulation is divided in 3 periods but it was not specified how the division in periods were performed. In addition, and more important, it seems that all the sedimentary sequence was used in this model, but it is clear that, for example, the Cenozoic units were not present in the time period 75.8 Ma – 255.7 Ma since the Cenozoic started 66 Ma. Furthermore, the sedimentation of the units was different through time and, for example, the Cretaceous, Jurassic, and Triassic units were progressively not present after a certain simulation time during the time period 75.8 Ma – 255.7 Ma. I suppose that the occurrence (or not occurrence) of some units could change the final result of the sensitivity analysis considering also that the Cenozoic, Cretaceous, and Triassic units generally have the highest sensitivity

indices. Was this problem considered during the simulation and eventually how was it accounted? If not, a discussion on this topic should be added.

We have opted for subdividing the total simulation time into three main periods because we wanted to being able to isolate short-term, mid-term and long-term effects on the model results. This said, we added some extensive explanatory text to the original manuscript (l. 375-377).
Regarding the geological model, the reviewer is right in that we have made use of a single model throughout the whole paper. The reason behind our choice stems from our aim to provide and test our approach to isolate and quantify the non-linear and therefore superposed effects induced by considering a change in the system dynamics (from a steady- to a transient state), an aspect that still provides difficult in the context of basin-wide thermal modelling and that as such has been never quantified so far. To allow variations in the input geological model over time (and questions would then arise on how to best represent those variations if only based on the present-day information available) would have increased the parameter space in a rather unpredictable manner, thereby resulting in an over-parameterised problem. All these aspects would have hindered a proper assessment of the model outcomes and of the consequences of the physical processes that we targeted in the study. In order to provide some clarification on this aspect, we have added some explanatory text to the original manuscript following the same reasoning as outlined above (l. 533-542).

The language is generally fluent, but some sentences are quite complex and should be simplified to achieve a better readability of the manuscript. I provided a list of these sentences and of other minor language reviews in the COMMENTS part of this letter.
We have acknowledged all comments from reviewer#1 and modified the original text accordingly.

Considering these points, I suggest to accept the manuscript after MINOR REVISIONS.

COMMENTS

L. 13: I am not sure if “where” is correct since the Authors are talking about what is happening in the “case nowadays”. I suggest to rewrite the initial part of the sentence making it more straightforward: “This topic is especially actual since systematic efforts … ”.

The sentence was rephrased.

L. 15: delete “their”.

Has been deleted.

L. 19: “observations” instead of “observables”? It would fit better with the sentence at l. 20: “these datasets are spare and lacking in coverage”.

Has been changed.

L. 24-25: this sentence is not clear.

We have rephrased the sentence to improve its readability.

L. 25-28: I suggest to describe the factors influencing the heat distribution using a list. This would improve the readability of the sentence. In addition, I suggest to avoid referring to “the plate” since it could be misleading. The “tectonothermal configuration of the plate” could be changed as “regional tectonothermal configuration”, and the “dissipative processes within the plate” as “dissipative underground processes”.

All points have been addressed in the revised version of the manuscript.

L. 31: Probably a “and” is missing. Is it “with the square root of the internal period times AND the thermal diffusivity of the plate”? As before, plate could be change with “bedrock thermal diffusivity”.

The sentence was correct. There, is no missing “and”.

L. 33-35: I suggest to split the sentence. It could improve the readability.

The sentence was split.

L. 38-40: I suggest to rephrase or split this long and complex sentence.

The sentence was split.

L. 41: “require”, not “requires”.

This has been changed accordingly.

L. 53: I suggest to delete “from instance”.

Done.

L. 57: If the original meaning is maintained, I suggest to change with “on the influence

of the rock thermal properties". This would in better accordance with the statement at l. 61.

The original meaning would change with the reformulation. For this reason we kept the original formulation.

L. 59: as above.

As above.

L. 63-65: I suggest to rephrase this sentence. The expression "from the results of previous efforts by one of the co-authors" sounds a bit strange. I would simply put the reference as reported afterwards in the sentence. I.e: "from Degen et al. (2020)., who demonstrated … ".

The sentence has been rephrased.

L. 75: check the style of references.

The style has been corrected.

L. 80: "in Section 3".

Has been corrected.

L. 101: I suggest to specify the "many other methods" as done in the Introduction (l. 73). Maybe a brief description about how these other methods constructs surrogate models could be useful. This could be useful to compare with the RB method described in section 2.2.1 of the manuscript and it could highlight/strengthen the novelty of the used approach.

We have followed the reviewer's suggestion and provide a better description of the referred methods. In doing so, we did not further enter detailed descriptions of other surrogate methods since they do not fit with the main topic addressed in this study given their focus on the observation space (as we stated in the paper). Indeed, in our study, we focus on the entire temperature distribution thereby making other kinds of surrogate model unsuitable.

L. 103: replace the comma at the end of sentence with dot.

Has been addressed.

L. 110-113: I suggest to delete this sentence since it is partially repeating previous sentences (l.91 for Sobol and Santelli methodologies; l. 85 for Wainwright et al. comparison;

l. 87 for Degen et al.).

The sentence was been moved to another location as suggested by reviewer#2.

Caption 1: use the MPI-ESM abbreviation.

Has been changed

L. 158: A comma is missing. I.e.: “upper and lower crust, and the underlying mantle”.

Has been changed

L. 158: I suggest to detail here the description of the model.

Some informative text has been added, as well as additional materials (in the form of a descriptive table plus base maps of major horizons in the Appendix to the revised text).

L. 159: I suggest to delete “to assign” or to rephrase the sentence making it more straightforward.

Has been deleted.

L. 170: this sentence would benefit from the lithological description of the units. Zechstein and Rotliegend are two chronostratigraphic units / periods that correspond to the Middle – Late Permian and Early Permian – Late Carboniferous, respectively. One could argue that they are the same units as the “Permo-Carboniferous Volcanics”.

We have added a first order lithological description of the different units (consider the main lithology as being representative of the bulk volume of the respective layer) in the Table that can be found in the Appendix to the revised manuscript.

L. 172: how can we assess that the parameter correlations are negligible from the sensitivity indices? I suggest to specify it, either in brackets or in a subsequent sentence.

Parameter correlations can be asset through the difference between first- and total-order indices. An explanation has been added to the revised paper.

L: 179: I suggest to add the acronyms of the units in brackets together with their full name. In my opinion, they should be mentioned at least once when the Authors describe the result referring specifically to a figure.

The acronyms have been added to within a table in the Appendix to the revised manuscript.

L. 192-195: as above.

As above

L. 201-206: as above.

As above

Figure 2: what does the numbers in the X-axis mean? Just a consecutive numbering for the variable? I suggest either to remove them or to use 1 as unit for the axis. With the second option, You should get a vertical line for variable favouring the reading on the vertical axis. If you prefer to maintain a unit different than 1, I suggest to maintain it constant among the figures (i.e., the unit on X-axis in Figure 3 is unit 2).

The plot has been modified to better illustrate the meaning of the x-axis as a discontinuous unit.

Caption 2: please report the reference where the Reader can find the acronyms of the different units.

A reference to the table containing the acronyms has been added.

Figure 3: see the comment for Figure 2.

The plot has been modified to better illustrate the meaning of the x-axis as a discontinuous unit.

Caption 3: see the comment for Caption 2.

A reference to the table containing the acronyms has been added.

L. 223: I suggest to put the accuracy value in brackets.

The accuracy has been put in brackets.

L. 225: I suggest to blend the short sentence together with the previous. Otherwise, replace the comma at the end of sentence with dot.

The sentence has been modified.

L. 230-231: these two sentences are slightly misleading. Firstly, the Authors say that "the results are the same for all accuracies tested", but then they state that there are differences among the different accuracies for parameters with low sensitivity. It is clear that the impact of the accuracies is minor since the sensitivity of parameters is low, but the first statement goes in the opposite direction. I suggest to rephrase the sentence at l. 230 describing more in detail the results shown in the figure. A better description of the results will avoid any misinterpretation.

The sentences have been reformulated to improve their readability.

Figure 4: see the comment for Figure 2.

The plot has been modified to better illustrate the meaning of the x-axis as a discontinuous unit.

Caption 4: see the comment for Caption 2.

A reference to the table containing the acronyms has been added.

L. 238-239: I suggest to split or rephrase this sentence to increase its readability. The construction of the sentence is quite complex.

The sentence has been modified.

L. 240-242: as above.

The sentence has been modified.

Figure 5: see the comment for Figure 2.

The plot has been modified to better illustrate the meaning of the x-axis as a discontinuous unit.

Caption 5: see the comment for Caption 2.

A reference to the table containing the acronyms has been added.

L. 253-259: as comment for L. 179.

The acronyms have been added to Table A1.

Figure 7: the title on the Y-axis is missing.

The title has been added.

Caption 7: see the comment for Caption 2.

The acronyms have been added to Table A1.

L. 280: I suggest to use "a time-variable scaling factor". The term "increasing" could be misleading since it is stated that the uncertainties in the temperatures should decrease with time.

The formulation has been adopted.

L. 293: as comment for L. 179.

The acronyms have been added to Table A1.

L. 296: what does the "the errors in their sensitivities" mean? How can the Reader assess this error? Is it the accuracy discussed in section 3.2 or another parameter? I suggest to specify it and eventually restate the accuracy value of the model.

An explanation has been added to the paper.

Caption 11: see the comment for Caption 2.

A reference to the table containing the acronyms has been added.

L. 303-305: why did the Author choose to perform the sensitivity analysis in 4 different periods? Does the segmentation in periods have a geological meaning? These aspects should be specified. In addition, I suggest to start the bullet list from the period 0 – 22.8 ka.

A bullet list has been inserted and we also provide an explanation of the subdivision made (see also our comments to the reviewer's general remarks).

L. 307-308: as comment for L. 179.

The acronyms have been added to the table in the Appendix of the revised manuscript.

L. 321-324: as comment for L. 179.

The acronyms have been added to the table in the Appendix of the revised manuscript.

L. 348: I suggest to put a set of representative values or a reference for validating the sentence "This is caused by the higher radiogenic heat production of the latter rocks". The same suggestion can be referred to the thermal conductivity of Zechstein (l. 351) and of the lithospheric mantle (l. 352).

Crustal rocks have a granitoid prevailing lithological composition, thus their higher than sedimentary rocks heat production rates. Zechstein is a salt rock, while the mantle is considered as peridotite enriched, therefore their higher thermal conductivity.

L. 353: please specify the percentage of the lithospheric mantle volume with respect of the total volume of the model.

Information about the volume of the respective volume for each specific unit has been added in a table, which we have added to the revised version of the manuscript. By considering the values provided in the table, we derived that the mantle makes up around 76% of the total volume. We have added this information in the text.

L. 385: check the "to imposed" and eventually rephrase the sentence.

Has been corrected.

L. 393: "make improve" is not correct in my opinion, I would just keep "improve".

Has been corrected.

L. 430: "an additional"

Has been corrected.