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Solid Earth Discussions

Interactive comment on "3D crustal stress state of Western Central Europe according to a data-calibrated geomechanical model – first results" *by* Steffen Ahlers et al.

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Answer to anonymous referee #1:

A) General comments: The manuscript by Ahlers et al., (se-2020-199) aims to evaluate the state of stress in Western Central Europe using a 3D geomechanical approach. Knowledge of contemporary stresses play a critical role in numerous practical applications and, therefore, the results of this paper/model can be used in different aspects of geomechanics in the study area. In particular, this large scale model can provide information on boundary conditions of any small scale models in Western Central Europe for any geothermal exploration/production and waste disposal. Overall, the modelling

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technique used in this work (even the 'cookie-cutting' approach) has been tested in several tectonic setting so far (I'm not sure why it has not been mentioned in this paper). This contribution is a valuable step forward in our collective efforts to stress modelling of the Western Central Europe. This is a very useful paper dealing with a topic which might be of broad interest to the readership of the Solid Earth. While this study is a step forward modelling effort, some discussion is needed about what could be added in the next iteration to better understand the controls of the stress field in Western Central Europe. In particular, it is important to highlight how these "cookie-cutting" stress modelling could help us to evaluate the causes of tectonic stress in this region. I know the evaluation of the tectonic forces are not the scope of this paper, however, I think some discussion is required, as tectonic forces play the key role on the state of stress (note that these forces have been investigated, somehow, in the literature). The geology, tectonic setting and the description of previous models (generally, the literature review part of the paper) needs more work (please see my specific comments below). In particular, for those who are not familiar with the tectonic setting of the region. I'd say readers need to know what really control the stresses in Western Central Europe and how this model can be used to deal with large tectonic forces? Again, I know the evaluation of tectonic forces are not the aim of this model, but some information would be really helpful for future work. If this model is a predictive one, then you need to provide some implication of the results. For example, how the stress changes (orientation of magnitude) predicted by this model, can be used in practical applications? All in all, this paper provide the first attempt on the regional scale 3D stress modelling in Western Central Europe, which provide interesting and useful information on the crustal stress of this region. Therefore, I'd suggest the publication of this paper following a moderate revision.

We thank the anonymous referee #1 for reviewing our manuscript carefully and for his/her suggestions. In the following we will address the specific comments in detail.

B) Specific comments: B1) Title: The current title says 'first results'. So, it is not clear

if there will second/third results as future papers? I'd suggest to make this paper as a stand-alone one. The current title is a kind of confusing in a way that the second results will be different from this one. If yes, then someone might say what is the point of this paper if different results are going to be published sooner or later? So, some clarification would be appreciated.

We have changed the title to '3D crustal stress state of Germany according to a data-calibrated geomechanical model'

B2) Abstract: I am not a big fan of putting references in the Abstract. So, I'd suggest to remove the references and re-write the abstract to represent your work. I think the abstract needs to be re-written. By saying this "The model is open for further refinements regarding model geometry ..." the readers might get confused as you are publishing and incomplete model and results. So, I'd suggest to be clear if it is a final model or not?

We have removed the references. Since we have removed '- first results' from the title, we think there should be no misunderstanding regarding the last sentence anymore.

B3) Introduction: I'd suggest to expand the introduction and provide some background on the stress forces and how stresses can be perturbed, at different scales (add some real examples as references). Here, you can also explain stress orders, and their importance (note that you have already talked about them at different sections, but never explained them). In the following section (i.e. previous models), you can explain how these orders of stress can affect state of stress in your area (based on the literature).

We have added a short introduction regarding different orders of stress sources in Sect. 2.2.

In the beginning of the introduction, heaps of the cited papers are not included in the reference list! So, I am not sure if these papers are relevant to the statement or not (please see my comments in technical corrections for more details). So, I'd suggest to

make sure that the cited references are appropriate and represent your statement.

We apologize for the missing citations throughout the introduction, there was a problem linking them to our reference list, but this has been corrected.

B4) Fundamentals and state of the art: The heading says ‘state of the art’. So, some information on the ‘state of the art’ aspects of the work would be more than welcome. The geology of the study area needs more information on the tectonic setting as well. I think a map is required to show the location of study area in relation to tectonic plate. This map and information then will help you to discuss if pull and push forces around your model can be explained by tectonic forces or not?

We have added some additional information (line 70ff and 75ff) about the evolution from cretaceous to recent times including potential stress sources which we take up again in the discussion (line 454ff). We think that a map showing the tectonic plates forces is not useful here since this could give the reader the impression that we assume this as boundary conditions for the model.

Figure 1 needs more work. In different parts of the paper, you mentioned different country names. Could you please add these names on the map? In addition, you need to add the dimension of the model (length of each edges) on this figure as well.

We have added a new subfigure (1 b) with country names and the model dimensions.
B5) Previous models:

I assume this part is a literature review section for the previous attempts on the stress modelling of the region. However, I believe, it is not well-organised and needs more detailed information. By reading this section, the readers need to understand the controls on the stress pattern in your study area. In the current version, you have explained that the previous results highlighted ‘lateral stiffness contrasts in the lithosphere’ and ‘isostatic effects’ are the main cause of ‘stress perturbation’. But you did not discuss the causes of stress in the region? I am talking about the main forces that control the

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stress pattern (not those that provide perturbations).

We think that a detailed literature review regarding the stress sources is not required here since the objective of our study is not to identify the stress sources.

Table 1: What is the difference between X and (X)?

You are right, we forgot to mention it. The (X) describes the same model approach as we use, where the boundary conditions are not derived from the plate boundary forces, but still represent them. We have added a statement to the table caption (line 141f).

B6) Model Setup: The modelling setup and strategy has been widely used by the authors in a wide range of setting and scales. I'd suggest to add a statement in the beginning of Chapter 3 (i.e. modelling setup), and highlight that this setup and strategy has been used in both local and large scales (and cite them). In addition, in this section or somewhere else you need to explain that this model doesn't aim to evaluate the tectonic forces that control the stresses in this region. But, it mainly provides some information on the 3D description of stresses.

Thank you for your suggestion, we have added a statement at the end of the introduction (line 45ff) and we have and we have clarified that this model does not aim to evaluate the sources of stress (line 150ff).

B7) Initial Stress State: This section is really important and needs to be re-written. The current version is not clear. So, I'd suggest to give a better explanation on this paragraph.

You are right this is an important section. We have rewritten the whole section and added a new figure (line 251-272).

B8) Displacement boundary conditions: This section needs a bit of work as well. It needs to be clearer. So, I'd suggest to re-write this section. Line 245: It's been mentioned" The orientations of the model boundaries are chosen parallel or perpendicular to the observed SHmax orientation". Could you please show it in a map? You probably

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can show the statistical results (that show the mean SH orientation) on Fig 2a? The current version of map in Fig 2a does not show it clearly, as there is not much data for the top right edge as well as whole western side of the model (and even the bottom edge!). So, it is not clear on what basis you are claiming that the model edges are parallel or perpendicular to SH.

We have added the mean SHmax orientations to the new subfigure (1 b).

Line 246-248: To me it is not clear how you have chosen to pull or push the model edges? So, it would be great to give us a better explanation on how these pull and pushing approach resemble the tectonic forces?

We have rewritten this paragraph (line 275ff). Additionally, please see our comment to B6).

B9) Results: Line 259: Why you did not calibrate the model with point-wise SH orientation? So, does it mean that you are calibrating your modelling results (in terms of SH orientation) with statistical models (inferred from WSM and pointwise data)? In all different part of the paper, it has been mentioned that the model will be calibrated against the WSM database for SH orientation. But, when we reach to the calibration stage, we see that the model is really calibrated with another statistical model (inferred from point-wise stress data). It is Okay, and I have no issue with the calibration procedure, but make it clear in the early part of the manuscript in order to avoid confusion.

The reasons why we use a mean SHmax orientation is described in Sect 4.1. We now have made it clear in the entire manuscript (e.g. line 159, line 281).

Figure 6: I'd suggest to show WSM data on panel c as well.

We think that this information is not helpful in this figure. The detailed comparison is now shown Fig. 1a and b.

Figure 9: Please use another colour for one of SH or SV. It is a bit difficult to follow orange and red (quiet similar) in the plots. In addition, could you please show the

different layers of the model on this figure? I also suggest to show the azimuth of SH orientation to see if SH rotate with depth, in particular once we move from one layer to another (basically change in layers means change in rock mechanical properties).

You are right the lines have been quite thin and the colors were quite similar. We have changed the color of SHmax and increased the thicknesses of the lines. This is a good suggestion! We added the model units at the right side of the plots. We think that is it not necessary to show the SHmax orientations in this figure since they are constant over the entire depth shown. However, we mention this in line 419f now and added a corresponding statement to the figure caption in line 365f.

Figure 10: This is an interesting figure that clearly shows the variation of stress regime (represented by (RSR) with depth in your study area. However, I'd love to see the SH orientation on each of them as well. By showing the SH on each panel, you can clearly show/discuss if there is any variation for SH with depth or not.

Please see our comment to Fig. 9.

B10) Discussion: Change of predicted stress regime with depth is very important that needs more explanation. So, I expect more discussion on the implications of these changes at 1km, 2km and 4km. I expect to see how these changes can affect the geomechanics of sub-surface for any geothermal activity or waste disposal? As I mentioned somewhere else, I'd like to see the SH for each depths on each of these panels. In addition, it would be great to provide some explanation on how these stress magnitude (and changes in RSR) evolve? Have you seen any similar changes in other regions with similar spatial scales (by means of stress data or stress models)? If yes, then would be great to cite them for those who are interested in this issue. Similar explanation is required for SH orientation. How does SH orientation evolve?

You are right this is an important point. We have expanded the discussion section regarding the stress regime change with depth and added some references (line 485 - 497).

Line 377: Instead of saying ‘good’ I’d suggest to be quantitative. Of course you have explained it in the next sentences. But, I’d suggest to re-write these sentences to be more quantitative.

We have restructured these two sentences (line 416ff).

Line 382: You need some references where you have mentioned ‘salt can act as mechanical contrast’.

You are right, we have added some references in line 423f.

Lines 391 and 392: I’d suggest to cite some papers who has explained the stress variations by using real data, not those who show modelling results.

You are right, it is better to cite some papers with ‘real data’ here. We have removed the ‘modelling papers’ and added some additional references with ‘real data’ in line 427f.

Line 395: low far-field horizontal stresses? What does it mean? Some clarification would be great.

To make it clearer, we now additionally use the term ‘high order stress source’. Please see also our comment to B3)

Line 405: The authors mentioned “our model results also show no impact of mechanical contrasts on the orientation of SHmax”. So, I think some explanation here is required. In particular, it should be expanded in relation to Reiter 2020, where these parameters play a critical role on stress perturbation. So, why we do not see these orientations here?

You are right with regard to Reiter (2020) this needs a little more explanation. We have added a small discussion on this topic and calculated an additional model to verify our hypothesis (line 446- 452).

Line 409: Instead of saying ‘very good’ I’d suggest to be quantitative.

We have rewritten this statement (line 459f).

C) Technical corrections: Lines 9 and 10: What do you mean by ‘basic research likewise’? I’d suggest to be more specific.

We think it is enough to mention the specific topics in the introduction (line 25-32).

Line 17: I’d suggest not to use “lithostratigraphic units” for such a large and regional scale model. It then could be confusing with the terminology used by the International Commission on Stratigraphy (https://stratigraphy.org/guide/litho). Similar issue in Lines 42 and 43.

To avoid misunderstandings, we have changed it to ‘sedimentary unit’ (e.g. in line 17)

Line 27: Bell (2003) & Kristiansen (2004) is not in the reference list! Please make sure that these papers have something replate to wellbore stability.

You are right, please see our comment to B3)

Lines 29 & 30: Altmann et al., (2014); Henk (2009); Smart et al., (2014); Hettema, (2020); Konstantinovskaya et al., (2012); Brady & Brown, (2004) are not in the reference list!

You are right, please see our comment to B3)

Line 30: use ‘and’ instead of ‘or’

We have changed it, as suggested

Line 31: Diederichs et al., (2004) is not in the reference list

Please see our comment to B3)

Line 40: remove ‘in this study’

We have removed it

Lines 41-43: These lines are repeated from abstract.

Yes, they are almost similar since it is a very important statement.

Line 49: Change it to Geology and Tectonic Setting of the study area.

Thank you for the suggestion, we have changed it.

Figure 2: I think it is modified from the one presented in Heidbach et al., (2018). So, it needs a reference! Do you think you really need to show this figure at all?

We have added the reference in line 104. Yes, we think this small figure is very useful for readers who are not so familiar with this topic and the definition of the reduced stress tensor.

Lines 117 & 118: Put this statement “If several model versions are published by one author, the most current one is listed” in the Table caption.

You are right it is good the mention it again in the table captions. We have added it (line 134f).

Line 118: Remove “with a wide range”

We have removed it.

Line 168: Be consistent. Sometimes five units, sometimes seven units.

You are right, we have changed it.

Line 194: Please re-write this statement (In all the models used and also ..... rocks.), as it is not clear.

We are sorry but we cannot understand where this sentence is incomprehensible to you. The top of the basement is not necessarily the top of the crystalline rock. However, usually the top of the basement is also defined as the base of the sedimentary layer. But, e.g. low-grade metamorphic rocks are more like sedimentary than crystalline rocks

Line 244: What do you mean by ‘these’ in this statement “At the five lateral boundaries of the model displacement boundary conditions are applied perpendicular to these”?

You are right this was not clear we have rewritten this paragraph.

Line 332: Do not

Thank you, we have changed it as mentioned.

Line 513: Conclusions

We have changed it, as suggested.

Line 546: The references list needs to be completed. There are many references that used in the text, but are not in the reference list.

Please see our comment to B3).

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