Dear Editor, dear Reviewers,

Please find below our revised detailed responses in green to the minor revisions required by the reviewers. The modified manuscript can be find in the attachments. We also added a link to an online repository where our figures can be downloaded in various formats and in various styles. We would like to thank you all once again for your detailed constructive feedback, which greatly improved the manuscript.

Yours sincerely,

Iris van Zelst (corresponding author),
Fabio Crameri, Adina E. Pusok, Anne Glerum, Juliane Dannberg, Cedric Thieulot

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Boris Kaus

Thanks, you did a great job with correcting the manuscript which I believe is significantly clarified now.

My only minor comment is on line 812, where I believe part of the sentence is missing. I guess it should be: … experimental numerical applications “and scale those” to large-parallelized production code (text between apostrophes added). I guess this can be added during typesetting.

Thanks for pointing that out! We rectified this.

Paul Tackley

I have no further revisions to recommend, except to point out that rock is not made of molecules - it has a crystalline structure:
l163 “we ignore that the material is made up of individual molecules”
Change "molecules" -> “atoms"

Good point. We changed this.

Laurent Montesi

Once again, it was a pleasure to read this important and well-organized manuscript. The authors have generally changed the manuscript where appropriate in response to my comments. In a few cases, they disagree with my suggestions and gave clear explanations why. As I wrote in my original review most comments express personal opinions or preferences, and so I am satisfied with the authors’ response. I do have a couple of further requests for clarification and wish to return to some of our discussions.

- The title has been updated to mention communication, which is a great addition and emphasizes a particularly original part of the paper. However, the authors declined my suggestion to mention the mantle, crust, or lithosphere in the paper, as that distinction is made in the abstract. While that is true, I still feel that readers from different geodynamic subdisciplines may be frustrated by the emphasis on convection and could use a clarification of the context of the paper when they first meet it, i.e., when they read the title. Elements of discussion (e.g., writing, study organization) are universal, but a large part of the paper discusses specific equations. Someone working on magma dynamics (two-phase flow), groundwater flow (Darcy’s law), or even earthquake mechanics (elastodynamics) would be frustrated with the appearance that geodynamics is assumed to be equivalent to mantle convection. You refer to Schubert et al. (2001) for the derivation of the equations, but please note that this book, although very important, is focused on mantle convection, not geodynamics in general. Your response to my concern of Eq. 15 is that it is the most common additional equation in geodynamics models. This ignores every problem based on elasticity or fluid transport. It is your paper, and you have the right to frame it as you wish. It just is not as general as you might think.

We recognise the point you are making. In order to reflect this within the paper, we modified our title to "101 Geodynamic modelling: How to design, interpret, and communicate numerical studies **of the solid Earth**", which we hope sufficiently emphasises that we are predominantly concerned with the geodynamics of the solid Earth (i.e., lithosphere and mantle dynamics).

- In response to my comment to line 415, you say that the two criteria are equivalent for an "incompressible, 2D, plane strain model". That does not make sense to me. The failure criteria only involve stress. Incompressibility and plane strain assumptions involve strains, so unless you assume a rheology, there is no general link between the conditions you express and the failure criteria. Even in 2D, Mohr-Coulomb features corners, as it is based on the maximum differential stress, while Drucker-Prager is smooth, as it is based on invariants (as you added to section 2.2.4).

The two failure criteria are equivalent under these sets of circumstances. For a full derivation of the equivalence of these two failure criteria under these assumptions, we refer you to Section 3.26.13 in the Fieldstone code manual, which can be found here: http://cedricthieulot.net/manual.pdf

• The following should not lead to a change in the paper, but I would like to push you a little on the topic of non-uniform color scales. Note that I am a fan of your color scale and try to use them in all my recent work, so, fundamentally, I agree with you. However, the effect of using a color scale that highlights a certain value is not very different from one of the "alternatives" you mention, superposing a contour. The key is that the person using a non-perceptually uniform scale should do it with a purpose (most use those scales without giving it a thought, and that's bad). It is not easy to use these scales properly, so I agree that there is no need for going through the trouble, but I personally prefer to leave options open.

We appreciate the reviewer's thoughts and that the topic is being discussed. The difference between using an even colour map (i.e., a correct scale) with a superposed contour (i.e., a superposed interpretation) and solely using an uneven colour map (i.e., a faulty scale) is that in the latter approach, the data itself is not represented, but only the author's interpretation. This makes an independent evaluation of the author's interpretation/conclusion impossible and the scientific procedure fails.
If the author decides to interpret the data with a distorted scale (according to the reviewer's suggestion), then that is fine if – and only if – the data is additionally separately presented with an accurate scale.

Minor comments on the updated text:

- Line 54: there have been approaches for gravity scaling using centrifuges (e.g., Ramberg, H., (1967) Model Experimentation of the Effect of Gravity on Tectonic Processes, Geophysical Journal International, 14, 307–329, doi:10.1111/j.1365-246X.1967.tb06247.x)

This is a good point. We changed the formulation of our sentence by mentioning that it is difficult (and not impossible) to scale gravity in a lab setting and we added several references (Ramberg et al., 1967; Mart et al., 2005; Noble and Dixon, 2011).

- Figure 3: I just noticed that the drawing for a plastic material is actually brittle. We use a plastic approximation for frictional sliding, where, unlike that snapping bar, the materials stay in contact after failure.

We modified the figure by writing 'brittle' instead of 'plastic' and we added a sentence to the caption to clarify that brittle rheology in geodynamics is approximated by plasticity, as depicted by the dark grey plastic slider in the figure. We also refer the reader to the relevant section in the caption. We hope this sufficiently clarifies this.

- Lines 208-210: I am confused by the discussion of a “viscous isentropic relaxation time scale”. If you discuss “viscous relaxation”, how can “elastic forces” dominate the time scale? A quick look at Curbelo et al. (2019) paper shows that the time scale does involve viscosity but no elastic moduli. Please include the reference to back up these statements for a viscoelastic body and the time scales shown on line 210. By the way, it may be worth quickly mentioning that fluid transport can lead to additional volume changes (viscous compaction; see McKenzie 1984, Ricard et al. 2001, Wilson et al., 2014a).

The reviewer is right that this is formulated in a way that is misleading. We have changed the text to the following:

“Because the first term explicitly includes a time-dependence, it introduces a characteristic time scale into the model due to viscous (Curbelo et al., 2019) and elastic forces (Patocka et al., 2019). For viscous forces, this is called the viscous isentropic relaxation time scale and is on the order of a few hundred years for the upper mantle to a few tens of thousands of years for the lower mantle (Curbelo et al., 2019). For visco-elastic deformation, this time scale is the Maxwell time (see above). When we consider the Earth as a visco-elastic body (see Section 2.2.1), the relaxation time is dominated by elastic forces.”

The viscous relaxation time scales with viscosity over pressure, so using Earth-like values of 1e20 Pa s (upper mantle viscosity) to 1e22 Pa s (lower mantle viscosity) and 10GPa (pressure), we get 1e10-1e12 s, or 300 to 30 000 years using equation 4.5 from Curbelo et al. (2019).

- Line 235: Without a rheology, the stress tensor does not contain information on the deformation of the material. That would make it a deformation vector.

We removed our explanation of the stress tensor containing information on deformation and instead only mention that sigma represents the stress tensor.

- Line 381: Very confusing sentence. “Rocks can deform elastically, by brittle failure” implies that “brittle failure” enables elastic deformation, and of course, it’s not the case.

We rephrased this sentence.