1 C. Thieulot

The manuscript has undergone a major overhaul is has been substantially improved. I recommend publications after some minor corrections be made:

eq 8,9: tau should be bold

Corrected in Eqs. (8) and (9).

l117 eq 12, not 15 ?

Indeed, we corrected that line: 116

l 120: I may have missed it but I don't see a definition of $\hat{\boldsymbol{t}}$

We added: "with $\hat{\boldsymbol{t}}$ a tangent unit vector to the boundary such that $\hat{\boldsymbol{n}} \cdot \hat{\boldsymbol{t}} = 0$." line: 120

l 121: eq 15 or eq 12?

Eq. 12, we corrected that line: 121

l 148: do not provide

Corrected line 147

l 178: eq 28 or eq 25?

We keep Eq. (25) as it is the general form before applying any choice for the Neumann boundary conditions.

eq 34: missing parenthesis

Corrected line: 336

eqs in 3.4.1: dot on ε*, but not in section 2*

We added the dot in section 2 for coherence.

l 470: wither

Corrected line: 461

fig 4: colorscales for Pa and Pd are inverted

We modified the figure so all the models share the same colour scale.

2 R. Gassmöeller

Thank you for thoughtfully addressing my questions and concerns. I think the manuscript is now in a much better state and gives reader a much clearer picture about the benefits of using the poisson pressure equation. Due to the large amount of additional material in the manuscript I have accumulated a somewhat lengthy list of minor comments that I would like to see addressed. Even my one major comment is mostly a request to clearly state the conclusion of your new sections 2.1./3.3. If you carefully address my remaining comments I am happy to suggest this manuscript for publication.

2.1 Major comment:

- I appreciate that the boundary conditions for equation (7) are now spelled out in more detail in section 2.1 (lines 119-154), however I think the current description leaves the reader more confused than enlightened. You split the Neumann boundaries in two categories (omega perpendicular and omega parallel). But you do not discuss why you do that, or what happens if your side boundaries were not parallel/perpendicular to the gravity field. You also do not explain how you distinguish boundaries into the two categories. Next, your split version of the Neumann BC has some nonintuitive properties that you discuss in detail, to conclude at the end of the paragraph that equation (23) is a single version of the BC that would give reasonable results. Indeed equation (23) seems perfectly reasonable to me, and I would have expected that to be the Neumann BC for all boundaries. So at the moment I am at a loss why you had to introduce the complications before (and the reader willprobably be too). There may be a simple explanation, I just think it is important to spell this out in the manuscript.

Later comment: After reading section 3.3. I understand this issue better, in particular that there are multiple possible choices for the Neumann BC, and that results between them differ (I think this should already be pointed out in Section 2.1, you can refer the reader to the later section for the results). But I still do not understand why you chose equation (15) and (16) instead of choosing (23) for everything. I think section 2.1 requires a sentence similar to the following: "We chose to use eq.(15) and (16) as Neumann boundary conditions for the following reasons .... ." If you chose (15),(16) for technical reasons, add something like: "If possible we suggest using equation ... as the best option for Neumann BC for the PPE problem." or some other qualification. In its current form this section is missing a concluding point that the reader can take away from the discussion of the different options.

We added "Nevertheless, for an arbitrarily shaped domain, using boundary conditions Eq. (15), (16) or (23) does not yield the same result (see section 3.3). For a general use (i.e. when considering arbitrarily shaped domains) we suggest to employ Eq. (15) & (16) as they are a direct extension of the 1D hydrostatic assumptions to 2D and 3D domains."
Lines: 153-156

2.2 Minor points:

- line 67,68: "it is still important to approximate the total pressure in(!) the best possible way"

Corrected line: 68

- line 79: you now explain tau using eta, which itself is not defined. Add an explanation for that.

Corrected lines: 79-80

- line 84: "along which" seems wrong

Corrected line: 83

- line 207/208: Here you state that you use the equation for $\Omega_{\|}$ for the base of the domain and $\Omega_{\perp}$ for the sides of the domain. This suggests parallel and perpendicular are referring to the normal of the boundary, not the boundary itself. This makes sense, but was not explicitly stated before, and I intuitively expected it to be the other way (in 2D). Please add an explanation for this in line 126 so that readers are aware of it from the start. Also line 126 is missing a partial derivative for the parallel boundary.

We added the missing partial sign here (line: 126). The parallel and perpendicular signs are not defined with respect to the boundary or the normal to the boundary, they are defined with respect to the gravity vector. $\partial\Omega_{\perp}$ defines the boundaries on which the constraint $\nabla P \cdot \hat{\boldsymbol{g}}_{\perp} = 0$ is applied while $\partial\Omega_{\|}$ defines the boundaries on which the constraint $\nabla P \cdot \hat{\boldsymbol{g}} = \rho g$ is applied. It is already stated by equations (15) and (16) under the form for all $\boldsymbol{x} \in \partial\Omega_{\perp/\|}$.

- line 220: "aims showing" seems wrong

Corrected line: 218

- line 233: "very small": you give a good explanation for why the solution in spherical coordinates is not as accurate, but please quantify "very small" either in absolute (x MPa) or relative terms (are we speaking about 1% different or 1e-6 difference?). This question is relevant if you want others to adapt your method to give them a way to quantify if they implemented your method correctly.

We added a plot on Figure 4 (d). We also refer to that plot in the text line: 231

- Fig. 4.: This is a great figure to show the difference between the two approaches on a well chosen example. I have one problem with the current visuals however: The colorscale in plot b) and c) emphasizes regions with very small errors (yellow) and deemphasizes regions with very large error (black). It took me more than a minute to figure out that your new method is indeed better, because I thought clearly plot b) shows larger errors for your new method. Consider flipping the colorscale I think that would greatly help the visual clarity of the figure.

We flipped the colour scale of Figure 4.

- line 283: The Neumann BC described here are correct (because you use a cartesian box), but earlier you always describe them relative to the normal of the boundary, while here you describe them using g and $\hat{\boldsymbol{g}}$. Wouldnt it be more consistent to stick to describing them using the boundary normal?

We stated the boundary conditions with respect to $\hat{\boldsymbol{n}}$ lines 278-279

- line 293: "On Figure ..." -> "In Figure ..."

Corrected line: 288

- line 322: I think you mean "force the pressure gradient to be parallel (!) to g"

Indeed, corrected line: 316

- line 325-327: I think this statement (PPE=depth integrated approach for certain BC) is only true for constant gravity and density fields as in this model. Please clarify in manuscript.

No, it is also true for a varying density field. We will not show this on a figure but it is easily verifiable with the firedrake scripts we provided. Taking the box model and setting $\rho = 1 + x$ you will see that using the BCs from Eq. (15) on faces of normal x and Eq. (16) on the bottom face will produce the same solution than applying the 1D approximation.

- line 342: thank you for adapting the equation as I requested, however now you are missing parentheses around the material derivative on the left-hand side of the equation.

Corrected line: 336

- line 376-377: This phrasing is somewhat unusual. You do not really use k=3.3 to avoid preventing convection, you use it because it is a realistic value for the thermal conductivity. 70 was previously just used to simulate convection and an adiabatic temperature gradient. Rephrase maybe to: "However, for the actual model run we used a more realistic conductivity of ...".

Corrected line: 370

- line 428: "solve" -> "sole"

Corrected line: 420

- Fig. 14: Please clarify in the Figure caption which color scale refers to which feature in the figure, they are sufficiently similar to make that hard to see. Also the figure caption does not make clear what the background color shows vs the isocontours, the caption only mentions a single quantity for each figure. Are the contours just contours of the background color? Also why is the color scale flipped between the two columns? To make clear that one is Ph vs PN? This could be made clear with a label. Using the same color scale would make comparisons easier.

We modified the figure so all models share the same colour scale. We also indicate that the contour lines represent iso-values of pressure every 0.1.

- line 470: "wither" -> probably "whether", also this sentence is pretty complex with multiple parentheses and seems to be missing a final statement, or I do not understand the structure of this sentence. Please simplify.

We corrected whiter to whether, removed the parentheses and added (i) and (ii) at the beginning of the possible choices. Line: ??

- line 473,474: "Therefore" and "thus" are duplicative

We removed thus, lines: 461-462

- Line 478: You now discuss a lot of other topics since presenting the geodynamic lithosphere model. Clarify which model you are talking about in this section.

We added at the beginning of the section: "In the geodynamic rift model". We also added references to figures illustrating what is stated in the text to avoid any ambiguity. Lines: 469-471

- Section 4.4.: You state that this equation could become nonlinear, but you do not discuss what to do about it. Do you suggest using the reference quantities to avoid nonlinearities since it is just an approximation to the real pressure, or do you suggest to include the nonlinearity and handle it somehow?

Actually in the geodynamic model section we state that we evaluate the PPE using a non-linear solver and that we apply its value as a boundary condition at each non-linear Stokes solve iterations. We do not specifically suggest anything, it is up to modellers to choose if they want to simplify the non-linear problem to a linear one, or to solve it as it is.

- line 506: demonstrates -> demonstrate

Corrected line: 496