General modifications

We are grateful to the reviewers for their questions and remarks. These helped improve the presentation clarity and motivated us to better charaterise the behaviour of the pressure solutions obtained from the proposed Poisson problem. To answer these questions we designed 2D experiments to show the results of the method in hydrostatic and non-hydrostatic cases.

Moreover, we also re-work the boundary conditions for the pressure Poisson problem to include a more general formulation that can be used in arbitrarily shaped domains (it was not the case with the previous formulation).

We also added a discussion section about the different PDE formulations for the pressure depending on the objectives that one might want to achieve.

We added more figures to illustrate all these modifications and discussions in the new version of the manuscript.

Finally, in the tracked change version of the manuscript, we coloured in blue the changes related to R. Gassmöeller remarks and in red the changes related to C. Thieulot remarks. Shown in light blue are changes that were not directly related to reviewers remarks which we thought important to include for clarity and completeness of the study.

Responses to R. Gassmöeller

This manuscript discusses a finite-element approach to compute a "lithostatic" reference pressure that can be utilized for thermo-mechanical geodynamic models.

The computation of a reasonable reference pressure is of large (but often overlooked) importance for geodynamic modeling. Not only can an accurate reference pressure be used to apply open boundary conditions (like proposed in this manuscript), it can also be used to create better reference properties for the equation of state and a better definition of the "dynamic pressure" relevant for rheological behavior and comparisons to field studies. I therefore think the topic of the manuscript is relevant and appropriate for Solid Earth.

The manuscript has a number of strong points, such as the great care that the authors have taken to make their software and models available and the results of this study reproducible. In addition the authors have found a good balance between the mathematical and numerical foundation of the method, numerical benchmarks, and example applications. The figures are visually clear and well labeled.

Major points:

☐ *1. In the current state, there is no evidence in the manuscript that the computed pressure is quantitatively correct. I.e. the included benchmarks only provide qualitative figures that show the computed pressure field is similar to what we would intuitively expect. A quantitative comparison to a simple 1D hydrostatic profile, which is mentioned as the main competitive algorithm for computing static pressure profiles, would be easy especially for the sheared rectangle benchmark. But also for a radially layered density profile like the half annulus benchmark a quantitative comparison would be crucial to assess the influence of a discontinuous density on accuracy of the algorithm (see my minor comment below). Finally there is no benchmark to illustrate the accuracy of the algorithm for a laterally varying density distribution, like the one in the application model. I would imagine a benchmark like $\rho = \rho(\boldsymbol{x})$, $g_x = 0$, $g_y = const$ in a undeformed box would suffice (I chose this on purpose to illustrate that in this case equation (6) would simplify to laplace $p = 0$, which is exactly the same as for $\rho = const$; is this what you or your readers would intuitively expect?). To have such a benchmark would already shed some interesting light on my second major concern below.*

We added two new sections to show the efficiency of the method in hydrostatic and non-hydrostatic cases (section 3.1 and section 3.2). Moreover, we show and discuss the accuracy of the method with respect to the 1D depth integrated method.

☐ *2. This brings me to the second large concern, which may be more fundamental than the first. The results of the presented method are occasionally unexpected, and there is an inconsistency between what the equation of interest (equation 5) implies (that ∇p always points in the direction of $\rho\boldsymbol{g}$) and the solution of the actually solved equation (6) in Fig 2. f) and g). Although gravity is vertical there is clearly a lateral pressure gradient present, which can be seen by the varying depth of the isopressure contours. This inconsistency can also be illustrated by stating the Dirichlet boundary condition that $p = 0$ at the whole outer surface, but allowing that boundary to be non-horizontal. This shows that solving equation (6) is not necessarily the same as solving equation (5) and the authors need to make that clear, and explain the constraints of this step. Currently they justify the transformation by stating "Taking the divergence of the momentum equation is common practice when studying iso-viscous fluids, and or in the derivation of projection based Navier-Stokes flow solvers (Chorin, 1967)". This is clearly not detailed enough. For example this statement does not explain under which condition this operation is allowed (certain boundary conditions? gravity as the gradient of a potential field? $\rho\boldsymbol{g}$ equal to the actual gravity force computed from the actual gravity potential?). In addition I have read Chorin 1967, it is a 4 page paper that describes a finite-difference algorithm for the Navier-Stokes equation, but I found no reference to taking the divergence of the momentum equation. Is it possible that the authors cited the wrong paper?*

We addressed that remark through several 2D models that are in a non-hydrostatic state specifically due to the presence of a topography (section 3.2). We show the pressure field computed with the 1D depth integrated approach as long as the pressure field computed with the Laplacian approach (Figures 4, 5, 6). We then compare these results with respect to the total pressure computed with the Stokes equation to discuss the accuracy of the total pressure approximation of these two methods. We also show the effect of applying these different pressures as normal stress boundary condition on the flow field. All these results can be found in the section 3 and on figures 4, 5, 6.

In Sec. 2 (around Eq. (6)) we had additional information to motivate why we take the divergence of the momentum equations. We also provide a different interpretation of the pressure Poisson problem - see Eqs. 8-11. Accordingly we have removed the reference to Chorin 1967.

☐ *3. In my opinion the presentation of the example rift model as a comparison between the authors new lithostatic pressure boundaries and free-slip boundaries at the front/back is misleading. The authors have modified nearly every other boundary condition of the model as well (in particular the bottom BC). Changing the bottom prescribed inflow BC into a no-slip boundary condition may well be the reason that more material has to enter the domain through the front/back boundaries (in addition to the now open side boundaries), which could easily lead to the observed change in convective pattern. Therefore it is impossible to tell which of the differences between the two models are due to the use of open boundaries for the front/back, and which are simply due to the change in bottom/side boundary conditions. A better comparison would have left the bottom boundary and left/right boundary condition untouched, and only switched the front/back boundary from a free-slip to the lithostatic boundary condition. I agree that for a more realistic model one would also want to change the bottom BC, but not to a no-slip, but to a open/lithostatic boundary (I would consider that an acceptable comparison as well). It may well be the case that with an open/lithostatic bottom boundary (or a prescribed inflow as the reference case) the observed flow would be significantly more similar to the reference model. I am afraid this question can only be answered by rerunning the model with the modified boundary conditions, but would urge the authors to do so, since their main conclusion (section 4.1) depends on it.*

We made a new model with normal stress boundary conditions to replace the previous one. This new model uses the same boundary conditions than the free-slip model except for the faces of normal z on which we apply the normal stress instead of the free-slip condition. The results are indeed different in the asthenosphere because there are no more convection cells (thus we removed the figure showing the asthenosphere flow since it is no more interesting to show). However, the deformation in the lithosphere is extremely similar. The strike-slip faults are again developing as long as the triple junctions.

☐ *Additional information I would have liked to see:*

☐ *I am concerned that the manuscript contains very little information about the limitations of this approach to compute the pressure. In particular the following questions are not answered:*

☐ *- What is the expected accuracy (convergence order) of the algorithm? This is in particular important, because you intend to use the computed pressure field as a boundary condition (as in the application example).*

For the Poisson problem, the convergence order is known to be $\mathcal{O}(h^{k+1})$ in L_2 where h is the mesh size and k the polynomial order of the FE approximation. Moreover, in `pTatin3D` we use Q_1 elements overlapping the Q_2 mesh. So the pressure from the flow problem and the approximated pressure from the poisson share the same convergence order which is $\mathcal{O}(h^2)$ in L_2. The RHS of the pressure Poisson problem also requires a projection from particles to quadrature points. This projection is done using a bilinear approximation, which is the same projection used for the density to compute the flow problem. This projection does not affect the order of convergence.

☐ *- You mention that the weak formulation is valid for a discontinuous density, but is it expected to affect the accuracy of the solution?*

We addressed this point in section 3.1 for hydrostatic cases. We specifically made a discontinuous density model. line 213-218: "Since the P_2 FE approximation contains the monomials 1, y and y^2, the FE solution exactly reproduces the analytic solution for case 1 and case 2 – independent of the number of finite elements used in the domain (e.g. sub-dividing the box into two triangles would be sufficient to obtain an exact solution). For case 3, the analytic pressure solution is piecewise linear, hence provided the density discontinuity is exactly resolved by the faces of the triangular FE mesh (which was the case here), the FE method exactly reproduces the analytic solution."

☐ *- Is it important that **g** is the gradient of a potential field? In reality that will always be the case, but in numerical models, in particular benchmarks it may not be (e.g. it may be a purely rotational vector field).*

No. No part of our formulation requires or assumes that $\boldsymbol{g}$ is the gradient of a potential.

Minor comments:

☐ *- lines 48-49 the sentence is missing a verb, or 'if' should be 'of'*

Corrected line 56

☐ *- line 90: 'if there' seems wrong*

That part has been removed

☐ *- line 105: The current reference to equation (10) is ambiguous (does it reference the BC or the surface integrals?). The definition of the boundary condition happens in eq (8). Either reference eq (8), or reword to: "Furthermore, from the definition of the boundary conditions the two surface integrals on the LHS and RHS of equation(10) cancel.*

This part has been modified to better develop the general boundary conditions for any geometric case (*i.e.* for domains with arbitrary boundaries), section 2.1

☐ *- line 125: this is usually called the 'polar angle' or 'azimuth'. 'angle' is ambiguous.*

Corrected line 223

☐ *- line 125: I understand that you only provide an example model, but quoting the Earth's radius as 6375 km and the depth of the core-mantle boundary as 2700 km without qualification is extremely inaccurate. The canonical value for an averaged spherical Earth radius is 6371 km (no matter the exact definition), and the depth of the core-mantle boundary is 2891 km (+/- a few km depending on source). Either qualify that you use simple values for illustration purposes or correct the values.*

We changed for the values proposed. But we also state that we provide an approximation, lines 222-225

☐ *- line 132: This sentence is grammatically not correct: "aims showing" -> "intends to show"*

We removed that example in favour of a more detailed study for a deformed domain in a non-hydrostatic case.

☐ *- equation (18) is written in a slightly unusual form in that the equation was divided by ρC_p and the factor was incorporated into the the thermal conduction term to form the thermal diffusivity. This is strictly only possible if the density and specific heat capacity are spatially constant. In many simplifications of the temperature equation this is actually the case (e.g. the Boussinesq or the Anelastic Liquid Approximation), but it is unclear if you used these. Additionally the heat source H in the equation seems to be the volumetric heat source, while typically the term is written as ρH with H being the specific heat source (which is easier to determine for rocks). Please clarify these terms, or use a more conventional form of the equation (e.g. eq. 6.10.49 on pp. 273 of Schuberth,Turcotte,Olson "Mantle Convection in the Earth and Planets").*

We changed the form of the equation to indeed match the equation we actually solve line 342.

☐ *- eq(20) does not include the definition of the second invariant as it claims to do, it only specifies the square root and factor $\frac{1}{2}$, but it does not specify how to convert the tensor into the second invariant*

The initial formulation missed one term. It has been corrected. As well as we added in the text that we take the square root of the second invariant. Line 352-353

☐ *- line 167-168: "takes place" and "lays" are weird formulations to describe something that exists/extends. I suppose you tried to avoid repetitive use, but if there is one word that describes what you want to say, use it repeatedly instead of replacing it with less precise versions. The same holds true for "modelled" vs "simulated" in the same paragraph. Using the same words will improve the readability of this paragraph.*

Corrected line 361-362

☐ *- eq(22) is actually an extension of the original Boussinesq approximation. The original BA explicitly neglects density changes due to pressure. Since these changes are typically at least an order of magnitude smaller than density changes due to temperature this will not affect your models much, but you can not claim to precisely implement the Boussinesq approximation here. Seeing this equation also raises the question which density you used for the temperature equation? The BA requires to use ρ_0 the reference density, but in Fig. 2. d) you show a density that increases with depth.*

We corrected the equation. We also corrected the temperature equation to show that we indeed use ρ_0 (line 342). As for the Stokes equation, we use the Boussinesq approximation, *i.e.* the density variations due to pressure and temperature are only accounted in the buoyancy forces term. Nevertheless, we also use the density variations to compute the pressure approximation in the pressure Poisson equation. We also added more details about the Boussinesq approximation to the text in the manuscript: "The Boussinesq approximation states that perturbations of density, if sufficiently small, can only be considered in the buoyancy term and neglected elsewhere regardless of the origin of the perturbation." (lines 369-371)

☐ *- eq(23) This is an unusual choice as initial condition for a lithosphere model. A steady-state solution will be a conductive profile across the whole domain (down to a depth of 450 km), while in real models everything below the lithosphere will be convecting sufficiently to create an adiabatic temperature profile following the average mantle potential temperature. This convection would lead to much higher temperature at the boundary between lithosphere and asthenosphere and could therefore significantly change the strength of the lithosphere in your model. For a science application this would be crucial to correct, but since you here only show the difference between the boundary conditions it is likely ok. However, you should at least mention that this is a simplification of a realistic profile.*

Indeed, we forgot to mention that we do simulate an adiabatic gradient for the initial temperature. We added it in the text line 374-377: "Moreover, to simulate an adiabatic thermal gradient in the asthenosphere due to thermal convection, the initial temperature field is solved with a conductivity of $k = 70\ \mathrm{W.m^{-1}.K^{-1}}$ in the asthenospheric mantle. However, to avoid preventing convection in the asthenosphere during the time-dependant simulation, a conductivity of $k = 3.3\ \mathrm{W.m^{-1}.K^{-1}}$ is used to solve Eq. (34)."

☐ *- Section 4.1: Since you only have a single subsection in the discussion, do not introduce 4.1., instead reword the heading of "Discussion"*

We added more discussion parts.

☐ *- Discussion and Conclusions are very brief. In particular these should contain a reference if these new patterns of deformations are also observed on Earth, what kinds of applications are additionally available through your new method, and what kinds of limitations or challenges remain.*

The discussion in now more detailed. There are references to studies that produced 3D geodynamic models with results that are relatively similar. However, in this paper the 3D model with normal stress boundary conditions is mainly a demonstrator. We did not intend to reproduce an actual geodynamic system on Earth.

☐ *- line 247 "amenable parallel computing environmnents" is missing a "to", however what you really want to say is probably "applicable in"*

Corrected line 500

☐ *- line 253 - 255 this sentence is too long and complicated and you forgot at least one "that". Split the sentence to make the argument easier to follow.*

Corrected line 510