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 C. Thieulot

The manuscript presents a method which allows to compute the lithostatic pressure in geodynamic models. This method is not new, but it is here clearly explained, as is its advantage over another common approach (the introduction does a great job at highlighting the difficulty/complexity of computing the lithostatic pressure in various common cases). It has also the merit to work in all kinds of geometries. The manuscript is well structured and reads well. The chosen examples speak for themselves.

☐ *I have quite a few minor comments/questions which I list hereafter. I believe that there is one missed opportunity: the authors do not discuss the case of compressible materials (with potentially with self-consistent gravity) at all. Since it is expected to render the pressure calculation much more complex, and since compressible models are common (esp. in mantle dynamics) I believe this warrants at least a discussion in the manuscript.*

We added a discussion part about the non linearity that arises from compressibility. Lines 489-496.

☐ *line 13: may be add Glerum et al 2018? (https://doi.org/10.5194/se-9-267-2018)*

Glerum et al. 2018 has been added to the list.

☐ *line 20: why not denoting the pressure at the surface x'_s simply P_s and avoid overbars altogether?*

Thanks for the suggestion. We adopted this.

☐ *line 21: overbar missing on P_0*

This now appears as P_s.

☐ *line 72: it is obvious why the lhs terms of eq3 become zero when $\boldsymbol{v} = 0$, but maybe a short sentence could be added to explain why the deviatoric stress is then also zero.*

We added $\tau = 2\eta\varepsilon(\boldsymbol{v})$ in the text so it appears that τ depends on $\boldsymbol{v}$ and hopefully makes it clear why the deviatoric is zero if $\boldsymbol{v} = \mathbf{0}$. Line 79

☐ *line 78: taking the divergence of the momentum equation is indeed common practice in CFD, but this does not justify why the approach is taken here. After all, $\nabla(P) = \rho\boldsymbol{g}$ is a differential equation that could be tackled 'as is'. I think why the* *divergence approach is necessary should be made clear.*

Taking $\nabla(P) = \rho\boldsymbol{g}$ 'as is' means that there are 3 differential equations but 1 unknown (P). If we consider that $\boldsymbol{g}$ is aligned with the coordinate system and ρ is only varying in that same direction then $\nabla(P) = \rho\boldsymbol{g}$ can be reduced to 1 differential equation and 1 unknown but for any other case we need to use the divergence to obtain 1 differential equation and 1 unknown. We added some details about that point lines 91-95. In addition, we also added a discussion in Sec 4.1 to show how a unique solution can be obtained from discretising $\nabla(P) = \rho\boldsymbol{g}$ directly and then by solving the normal equations.

☐ *lines 80+: maybe a short discussion is warranted about the nodes at the corner of the domain? since the intersection of $\partial\Omega_i$ and $\partial\Omega_{surf}$ is zero, these nodes belong to one or the other.*

In domains containing corners, the corners are associated with surfaces where Dirichlet constraints are prescribed. We clarified this point in lines 210.

☐ *line 90: existing->existed*

That part has been removed.

☐ *line 103: why introduce $\boldsymbol{F} = \nabla(P)$ in Eq.11 and never use it further? $\nabla(P)$ could replace $\boldsymbol{F}$ in Eq.10 and it would make the presence of Eq.8 terms more obvious.*

We detailed a lot more the boundary conditions and how to handle the flux term appearing in the LHS in sections 2.1 and 2.2.

☐ *line 114: straightforward*

Corrected line 181.

☐ *line 132: is 'rad' commonly used?*

We removed 'rad'.

☐ *line 125: '2D spherical coordinates' -> polar coordinates?*

Corrected line 223.

☐ *line 126: one usually speaks of the CMB, so core-mantle boundary.*

Corrected line 224.

☐ *line 127: this is a bit unusual. In polar coordinates one would take $\theta \in [0, \pi]$ and $x = r\cos\theta$.*

The notation corresponds to the Figure 2a with the zero centered in the x direction and y positive towards the surface.

☐ *line 131: 'aims showing'*

☐ *line 142: there is a minus sign issue wrt to Eq.3*

We removed the equation from that place as it is now in section 2 and correctly written.

☐ *line 145: Eq.17 is not needed, simply refer to Eq.4*

Corrected line 340.

☐ *line 146: dependent*

Corrected line 341.

☐ *line 148: in the previous section rho depends on position. If so, it cannot be inserted in the diffusion term to make the heat diffusivity coefficient κ*

We corrected that, line 342.

☐ *lines 149-150: are the weak forms of the Stokes equations really needed here? they are presented in May et al 2015 about the pTatin3D code. Case in point these equations are not numbered so they are not referred to in the text.*

They are written here to make clear from where the Neumann boundary condition comes from.

☐ *line 151: is Q1 really used for temperature ?*

The way temperature is solved is not really relevant to the manuscript so we just removed this.

☐ *line 156: in Eq.19, the exponent should read (1-n)/n or (1/n)-1*

Corrected line 350.

☐ *line 158: the $\dot{\varepsilon}$ term in Eq.19 is not the second invariant of the strain rate, but rather the square root of the second moment invariant.*

Corrected line 352.

☐ *line 159: second ε_{ij} missing in equation*

Corrected line 353.

☐ *line 161: Although not mandatory, there usually is a factor 2 in the denominator of eq21 (e.g. see eq 7 of Glerum et al) because of the relationship $\tau = 2\eta\dot{\varepsilon}$.*

Corrected line 356.

☐ *line 172: is it really a Boussinesq approximation if density depends on pressure?*

Yes, because the Boussinesq approximation is for small density variations regardless of the process involved in these variations. We added "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.

☐ *line 173: ρ_0 is not the 'initial' density. It is the density at $T = T_0$ (T_0 is missing in Eq.22)*

Corrected line 367.

☐ *line 189: Eq.24 could be written in a more compact form, eg.* $\boldsymbol{v} = (1, 0, 0)\ \mathrm{cm.yr}^{-1}$

This notation has been removed.

☐ *line 192: mismatch of parenthesis/square bracket*

The parenthesis is accepted as a symbolic way to indicate an open interval.

☐ *line 194: the pressure dependence of the density in Eq.22 makes Eq.5 nonlinear but this is not discussed.*

Indeed, we account for this non-linearity. It was not stated in text previously, but the reference pressure evaluation and its use as a traction for the Neumann boundary condition occurs at every non-linear iteration for each time step. We added this lines 390-392.

☐ *Aside from this, why is P_l computed only once per time step (and not even at every non-linear iteration?)*

P_l (now renamed as P_d for consistency with the new section) is computed at each non linear iteration. This information was missing in the text and has been added lines 390-392.

☐ *Also, prescribing P_l below te Dirichlet b.c. on the x faces echoes the work of Chertova et al 2014 (for example), but I am a bit puzzled by what it means to prescribe P_l on the z faces (In the free-slip model, it is akin to say that the model is infinite in the z direction but quid when P_l is prescribed?)*

We added a whole new section with simple models to address the effect of using this pressure as a normal stress boundary condition (section 3.2.2). The free-slip is indeed often considered as "the model is infinite in the z direction", but it also means that the material along the faces exerts an infinite resistance to fluid motion in that direction because flow is prescribed to be zero in this direction and a null resistance to shear (because free-slip also requires a zero shear stress condition). The free-slip condition forces any deformation to be orthogonal to the direction in which the velocity is prescribed to be zero. It can be considered as “infinite” only if the 3rd dimension is an extruded plan and that every displacement and deformation are cylindrical but it is a barrier for non cylindrical deformation and 3D displacements.

☐ *line 246: it is*

Corrected, line 500.