

Response to Interactive comment of Dr. May (referee) on “Implementing nonlinear viscoplasticity in ASPECT: benchmarking and applications to 3D subduction modeling” by Anne Glerum et al.

We thank Dr. May for his extensive and detailed comments, which greatly improved the manuscript. Below we address his points, and changes in the actual manuscript are indicated in bold.

General comments:

1. *The title of the paper is not appropriate. The majority of the paper is focused on benchmarking / verification. There is very little content related to the actual implementation details. Please choose a more appropriate title which is consistent with the focal point of your paper.*

We have changed the title to “Nonlinear viscoplasticity in ASPECT: benchmarking and applications to subduction”.

2. *Two of the stated objectives of the paper were (i) “provide hands-on examples” and (ii) to provide “community code for high-resolution, nonlinear rheology subduction modeling”.*

To facilitate these points, for each reference model presented in this paper, you need to provide specific details defining: (a) where all necessary input files / data can be located (e.g. provide the a URL pointing us to your branch, pull request, web-page); (b) any special instructions required to run each reference model. Currently in Sec. 7, it just says “Input parameter files to reproduce the benchmarks will be incorporated as well.” I don’t know what this means. I scanned through the ASPECT GitHub repository and couldn’t find the input files which define your models. Also, your ASPECT citation in the reference list says “developer version” - what does that mean? The master branch? Please clarify these points. Reproducibility of results from open-source codes should be possible. To facilitate this, you should provide the exact release / version number, or Git hash of ASPECT which was used for this study. Stating “The plasticity formulation has become part of the ASPECT distribution“ is incomplete and does not enable an interested user to reproduce your results (assuming they had access to your input data).

We have created a GitHub repository (<https://github.com/anne-glerum/paper-aspect-plasticity-subduction-data>) with all the input files and scripts to create them, plugins to ASPECT release 1.5 (<https://github.com/geodynamics/aspect/tree/aspect-1.5>) required for running the benchmarks, postprocessing scripts for all the gnuplot graphs and some of the ParaView images as well as instructions on how to use them.

We have changed the Code availability section to:

Our simulations were performed with ASPECT version 1.5.0 (Bangerth et al. 2017), available on GitHub. This version includes the plastic rheology described in this paper as a material model plugin. It can also be found on <https://github.com/anne-glerum/paper-aspect-plasticity-subduction-data>, together with all the plugins and input files needed to reproduce the benchmarks and 3D subduction models. This directory includes postprocessing scripts to produce the plots in this paper as well.

3. *When reporting the value and units of quantities, (i) please leave a single white space character between the value and the unit. When writing the unit, please leave a small half space between any two units. e.g. write Pa s and not Pas. Use the tex command \, for the half space.*

Done.

4. Please punctuate all equations in the manuscript.

Done.

5. Throughout the paper, there are several instances where new features, or recently added features to ASPECT are mentioned. e.g. "(and, since recently, tracers)" and "Note that as of 2016 it is also possible to use active as well as passive tracers in ASPECT (version 1.4.0)." Your manuscript should concisely describe the method you used for the studies presented. Your discussion section should relate to the results you have presented. When you provide throughout the text, notes or remarks about features outside the scope of your results, you break the flow of the text.

All material related to new features, or upcoming features should be confined to your "outlook" section. Please move all mention of tracers and Newton solvers into the outlook section as these components are not within the scope of this paper.

We have moved all mention of tracers, Newton solvers, material properties averaging (Appendix A3), strain tracking (Discussion) to the "Conclusions and Outlook" section.

6. There are a number of missing details and undefined quantities in the methods section of the manuscript (Sec. 2) which are required to understand the exact implementation being used in ASPECT. These need to be addressed in the revision. I'll highlight the specific issues in the Corrections section below. To be a useful guide for users of ASPECT who wish to conduct experiments with non-linear flow laws, the underlying non-linear solver needs to be clearly defined.

Done, see "Corrections" section below.

7. I think there is little value in citing papers which are "in prep." as no one can access them, or read them (in whatever state they are in). As such, the citation is pointless. Please remove all citations to the "in prep." papers.

Done.

Corrections:

1. [pg. 1, line 10] Your study doesn't involve "validation". This term is used to make a statement about whether the PDE you chose accurately describes a physical process (e.g. a lab experiment). Your study is concerned with "verification" which involves confirming that your implementation correctly solves the PDEs. Please change all instances of the word validate (and validation) to verify (verification).

Done.

2. [pg. 3, line 25] Please re-phase the sentence to be "Default settings employ second order polynomials for velocity and first order polynomials for pressure (Q2Q1 elements, e.g. Donea and Huerta, 2003), and second order polynomials for temperature and composition."

Done.

3. [pg. 4, line 5] *The discrete form of equations (3) and (4) will result in a nonsymmetric operator. You cannot use the conjugate gradient method to solve this system. CG is for symmetric positive definite systems. Furthermore, the entropy viscosity method is by definition non-linear as the artificial viscosity is a function of the scalar (in your case c_i or T). How are you solving this non-linear problem?* ASPECT's timestepping for the temperature equation has changed. Before, ASPECT used a semi-implicit BDF-2 scheme for the time discretization of this equation (see Kronbichler et al. 2012). The finite-difference approximation of the temperature (and velocity) time-derivatives at time t^n was obtained through quadratic interpolation of temperature (and velocity) values at time t^n , t^{n-1} and t^{n-2} . A linear interpolation of temperature T^{n-1} and T^{n-2} (\mathbf{u}^{n-1} and \mathbf{u}^{n-2}) to find T^n (\mathbf{u}^n) was then used in the advection term, while the diffusion term without the artificial diffusion was made implicit. Artificial diffusion was explicit by extrapolation. Hence the discrete form of the temperature/composition equations were spd and could be solved with CG, see also Eq. (16) of Kronbichler et al. (2012).

ASPECT 1.5 uses fully implicit time discretization. In this case, the system matrix is no longer spd and a GMRES solver with ILU preconditioner is used instead. Please refer to Heister et al. (2017) for a full discussion on this change in time discretization.

We have changed the sentence on the discretization of the temperature (and composition) equation to:

*The **GMRES** method with an incomplete LU decomposition preconditioner is used for the temperature and composition systems.*

4. [pg. 4, line 5] *Your statement about how you terminate the non-linear solver is incomplete. It should read something like this: "...until the relative nonlinear residual ... has fallen below a user-set tolerance (default value of 10^{-6}), or the user specified maximum number of iterations is reached."*

We have changed the sentence to:

... until the relative nonlinear residual ... has fallen below a user-set tolerance (default value of $1e-6$), or the user-specified maximum number of iterations is reached.

5. [pg. 4, line 5] *The variables $A(_)$, b and x have not been defined. Without this definition, I have no idea what your non-linear problem is, or how you are solving it. For example is $x = (u; p)$ or $(u; p; T)$? Each choice will change the definition of $A(_)$ and b . I ask for clarification on this point as you solve an equation for T , and T appears in your flow law.*

The solving of the temperature and Stokes equations is decoupled and the nonlinear iterations performed in this paper only concern the Stokes equations. Hence, x contains u and P only. Temperature and composition advection equations are solved once at the beginning of each time step. We have changed the description to the following:

Nonlinearities in the rheology are resolved with Picard- type (fixed point) iterations, iteratively updating the velocity and pressure, strain rate and viscosity (Ismail-Zadeh

and Tackley, 2010) until the relative nonlinear residual for iteration i $\|A(x_{i-1})x_{i-1}-b\|_2 / \|A(x_0)x_0-b\|_2$ has fallen below a user-set tolerance (default value of 10^{-6}), **or the user-specified maximum number of iterations is reached**. The initial residual $\|A(x_0)x_0-b\|_2$ is computed with zero velocities **and a lithostatic pressure profile calculated at the center horizontal coordinate**. **x contains the velocity and pressure solutions of the previous iteration, b represents the right hand side of the Stokes equations and A is the Stokes part of the system matrix.**

[pg. 4, line 5] *You state you use zero velocities to compute the initial residual.*

What value is used for the other quantities included in the definition of x ?

The initial guess for pressure considers a lithostatic pressure profile based on the model settings for density and gravity along the center of the domain. This pressure is also used in the computation of the initial residual.

7. [pg. 4, line 5] *You define the non-linear residual as $A(x)x \square b$. Defining it this way gives the reader the impression you might actually be computing the residual this way, e.g. by assembling a matrix and multiplying it by a vector. I hope that is not the case as this is an extremely inefficient way to evaluate the residual.*

This is indeed how ASPECT computes the residual: the matrix is already assembled for the new solve and is then multiplied with the previous solution.

8. [pg. 4, line 15] *Strain-rate is not a solution variable as you don't explicitly solve for $_{ij}$. The strain-rate is a derived quantity obtained from the velocity solution variable.*

Done.

9. [pg. 4, line 20] *For rheology 1, why don't you just call it "Grain boundary sliding or diffusion creep".*

The atom migration either occurs along the grain boundaries or within the grains; we wished to express this explicitly.

10. [Eq. (14)] *Suppose $\mu_{vp_eff} = 1$ throughout the domain, and I chose $\mu_{min} = 10e-10$ and $\mu_{max} = 10e10$. In this case, $\mu_{vp_eff} = 1$ and this obviously causes no issues for the solver. Hence I think it is not meaningful to report you solved problems with $\mu_{max}/\mu_{min} = 10e7$ without specifying that the min/max limits were approached by the flow law adopted.*

Agreed. We meant that these limits were approached and have therefore rephrased the sentence to:

*We have successfully run the models presented here with **overall viscosity contrasts** of up to 7 orders of magnitude.*

11. [pg. 5, line 10] *If you examine Eq. 9, you'll notice that when $_ = 0$, the expression you've written down does not reduce to the von Mises conditions (as you state it should). Please correct.*

Done.

12. [pg. 5, line 25] "...avoid extreme excursion..." - what does this mean? Please re-phrase.

Rephrased to:

... to avoid extremely low or high viscosity values due to possible velocity anomalies feeding back into the rheology as well as large viscosity jumps and thus ensure stability of the numerical scheme...

13. [pg. 5, line 25] Eq. (13) is stated in terms of eta whereas it should be stated in terms of mu. Please correct.

Done.

14. [pg. 6, line 5] Regarding the sentence "...how to average their properties (viscosity, density and other)." Be specific and list all properties which are required to averaged. Don't say "other" as the reader has to guess what you actually mean.

Done.

You never actually indicate how mu_average is used in the finite element computations. If you replaced the symbol mu_average with just mu there would be an obvious connection to Eq. 1.

Done.

Furthermore, you should write or explain that mu_i is computed by evaluating Eq. 14 with the material constants for composition i.

We added to Section 2.2.2:

μ_i is obtained by evaluating Eq. (11) or (12) using the material constants of composition i.

15. [pg. 6, line 15] Please change "infinite norm" to "infinity norm". Please change all other instances of "infinite" to "infinity".

The term "infinite norm" was used by Schmeling et al. 2008, which we cite here. However, we have changed all instances to the generally accepted "infinity norm".

16. [Eq. (4)] When you introduce c , you should indicate that valid bounds of c_i . I think in your implementation you should enforce that $c_i \in [0; 1]$ but I have to guess that as it is not explained. Does the entropy viscosity actually enforce those bounds rigorously? I don't think your implementation introduces a limiter to enforce these bounds. What do you do in situations when $c_i < 0$ or $c_i > 1$? These details need to be explained somewhere in the manuscript.

Initial conditions for the compositional field provide values on the [0,1] interval. Despite the entropy viscosity method, these limits can be slightly exceeded near the compositional boundaries; they are not enforced by the entropy viscosity method. Therefore, before using the field values for averaging, we cap them at 0 and 1. The division by the sum of the fields ensures proper averaging in case the fields do not add up to 1 in a particular point. We have added this explanation to the text:

Note that each field c_i is initialized with values on the interval [0,1] and capped values $0 \leq c_i \leq 1$ are used for averaging, as compositional field values may come to slightly exceed this interval over time despite artificial viscosity (Eq. (4)).

17. [Eq. (5,6,8,9)] It would be useful if you defined these flow laws in a manner which made it clear which variables are constants associated with a particular composition (i); e.g. $\gamma = C_i \cos(\phi_i) + \sin(\phi_i)P$; where the index i indicates a specific material (composition). I note you have done this (partially) in the tables of parameters, however I think adding an explicit sub-script i on the constants in your flow law would be much clearer.

We did not add the subscript i to the flow laws for two reasons: 1) We discuss the averaging *after* the flow laws are introduced. 2) We average the viscosities after computing the effective viscoplastic viscosity for each compositional field (i.e. after Eq. 12); which means the yield stresses and effective viscosities also require a subscript in Eq. 6-12, leading to very cluttered equations.

18. [Eq. (10)] You did not explicitly define what μ_{df} and μ_{dl} are. We've changed the superscript of viscosity in Eq. (6) and added the following sentence after Eq. (6) to define them:

The superscript df here indicates diffusion creep, dl dislocation creep.

19. [Eq. (18)] I don't understand your definition of the infinity norm as μ doesn't have an index. I can think of two definitions:

$$\mu_{av} = \max \mu_i, i=1, \dots, nc$$

or

$$\mu_{av} = \mu_k;$$

where k is compositional field index satisfying $c_k \geq c_i$ for all $i = k$. Please clarify your definition.

We intended the latter definition and have clarified it.

20. [pg. 6, line 25] The statement "All experiments were conducted on an in-house computer with 1, 000 cores" gives the reader the impression you conducted all experiments on 1000 cores, when you want to say that the machine you used has a 1000 cores. Please re-phase. Rather than tells as the clock speed (2.34 GHz), it would be more meaningful to report the type of compute node and the processor type.

Rephrased:

All experiments were conducted on an in-house computer consisting of 1 Dell PE-R515 master node and 15 Dell PE-C6145 compute servers made up of 2x4 AMD Opteron 6136 CPUs with Qlogic InfiniBand QDR interconnect. ASPECT was compiled using GCC 4.9.2.

21. [pg. 6, line 25] Remove the statement "Wall times quoted can have changed with versions of ASPECT newer than those used for the described experiments". Just provide information pertaining to your experiments - anything else is speculation. Your comment is vague and makes me think the run-times might have decreased with newer versions of ASPECT. In reality CPU times are impossible to reproduce anyway. Best thing is to report the machine spec, the compiler used (version) and leave it at that.

We have removed the phrase.

However, as our response to point 4 of reviewer Boris Kaus demonstrates, more recent versions of ASPECT do provide new ways of reducing run times. We have optimized wall times for the detachment benchmark discussed in point 4, but not for the other models. All benchmark run times are now reported for ASPECT 1.5.

For machine specs and compiler version, see point 20.

22. [Fig. 1] This figure is quite cluttered and unclear as you show the boundary conditions, the slip direction and try and label different regions within the solution. I suggest adding arrow heads to the red lines so the locations are more clearly defined.

We've tried to unclutter the figure by making a better distinction between boundary conditions and slip directions. Also we have removed the labels and used shaded areas instead. The footer of the figure now includes both velocity and pressure solutions for an unspecified punch velocity:

Figure 1. Prandtl's analytical solution of a rigid die indenting a rigid-plastic half space (Davis and Selvadurai, 2002; Kachanov, 2004; Thieulot et al., 2008).

Dark red arrows indicate the prescribed punch velocity v_p , shaded area CDE has a resulting velocity of $v_{CDE} = v_p$, while velocities in the lightest shaded areas are $v_{ABDC} = v_{EDFG} = v_p / \sqrt{2}$. Pressure at point I is $P_I = \sigma y(1 + \pi)$ and $P_{ABC} = P_{EFG} = \sigma y$.

23. [pg. 8, line 5] "...analytical solution is exactly reproduced ..." the numerical solution does not exactly match the analytic solution as you report 0.2% error. Rephrase.

Done:

The analytical solution is reproduced with errors < 0.14% for a smooth punch, but the velocity vectors in Fig. 3g show some horizontal motion of triangle CDE and the velocity field is more diffuse.

24. [pg. 8, line 5] The statement "This trade-off is as expected, because the horizontal component of surface velocity is left free for the smooth punch, while it is fixed to zero for the rough punch" doesn't explain the discrepancy. Please remove this statement.

Done.

25. [pg. 8, line 15] Why are you taking about results related to 3D experiments when you models examine 2D solutions? Remove the following text as it's not relevant to your work or results. "In 3D, literature does suggest that a rough interface between indenter and medium results in a Prandtl slip-line geometry, while Hill's solution is invoked by a smooth surface. Compare, for example, Fig. 11a and 11b of Gourvenec et al. (2006), Fig. 10e and 10f of Thieulot et al. (2008) or Fig. 13a and 13d of Braun et al. (2008)."

Done.

26. [Fig. 3] Please add to this figure snapshots of the pressure field.

Done, see changed caption in point 27.

27. [Fig. 3] Are you plotting a component of the strain-rate, or the second invariant? Please be more clear. The same comment applies for the velocity plot. Is this the magnitude of the velocity field?

We are plotting the Frobenius norm of the strain rate and the magnitude of the velocity. We have changed the figure capture to reflect that:

Figure 3. The punch benchmark results after 500 NI for a rough punch (left column) and a smooth punch (right column). (a) & (f): Viscosity field with analytical slip lines. (b) & (g): Strain rate **norm** ($\sqrt{\epsilon' : \epsilon}$) with measured shear band angles. (c) & (h): Velocity **magnitude** with velocity vectors along the surface of the domain and velocity measurements in points K and L. (d) & (i): **Pressure field**. (e) & (j) Pressure along the surface of the domain (colored line) and analytical solution values $\pi + 1$ and 1 (grey lines). Rough punch: $P_I = 4.7382$ and $P_H = P_J = 0.6224$. Smooth punch: $P_I = 4.1415$ and $P_H = P_J = 0.9999$.

28. [pg. 10, line 15] The statement "...The red symbols in Fig. 5 indicate runs for which the residual is not monotonously decreasing (after the first peak in residual)..." gives the impression you expect the residual to decrease monotonically.

You use Picard without any type of globalization, so you are not guaranteed that the residuals will decrease monotonically.

We rephrased as follows:

The red symbols in Fig. 5 indicate runs for which the residual **did not drop below the convergence criterion $\epsilon_u = 10^{-4}$ after 1000 iterations, as is evident from the corresponding red lines in Fig. 6.**

29. [pg. 11, line 5] You have already justified why you consider pressure dependent plasticity models. I think you can remove (or relocate to your motivation sections) the sentence "As brittle failure in rocks is more appropriately described by pressure-dependent plasticity than by the perfectly-plastic deformation (Gerbault et al., 1998) used in the punch problem, our material model plugin includes frictional plasticity."

This sentence is now the first sentence of Section 3.2.

30. [pg. 14, line 10] "Through AMR, the total (velocity, pressure, temperature, composition)..." these models don't include temperature so you should remove the word "temperature" from your statement.

Even though temperature is not considered in the setup of the experiment, the temperature system is set up for the nonlinear solver scheme that we picked. In runs with ASPECT 1.5, the solving of the temperature equation is skipped however.

31. [Fig. 10] Why is your adaptivity criterion performing so much refinement in the sticky-air? I can understand you want to resolve the air-rock interface, but refinement is occurring far from the interface. In one case, you have an isolated patch of refinement within the sticky air layer. Please comment on this.

The refinement was based on the norm of the strain rate and the approximate gradient of the density field (normalized to the same interval [0,1]) and a user-set percentage of the fraction of cells with the highest error that should be refined or coarsened. The refinement fraction was set to 95%, which led to some refinement in the sticky-air based on the strain rate there (see Fig. 9 b and f). We have rerun the sandbox with

refinement based on viscosity and density gradients, which greatly improves the focus of the refinement on the material interfaces and the shear bands. Figure 10 and its caption are updated accordingly.

32. [Fig. 10] In the caption you say “density leads to an elemental resolution varying from 512 x 128 to 32 x 8 elements”. I presume this means an “effective” resolution, i.e. these are the element resolutions which correspond to the smallest and largest elements. I think it would be more clear if you just stated the min/max element edge length in the units used to define the model. This comment applies to all other descriptions of your results which involve an adaptive mesh.

We have rephrased this particular caption as:

Adaptive mesh refinement and coarsening based on the viscosity and density leads to a minimum resolution of 6.25×6.25 mm and a maximum resolution of 0.39×0.39 mm.

All other similar descriptions are also adapted.

33. [pg. 15, line 5] Regarding this statement: “Although the right shear band angles of 62 and 60 ...” Who is to say what the “right / correct” shear band angle is. Please re-phase.

We meant the shear bands directly to the right of the velocity discontinuity, not the ‘correct’ angle. Rephrased to:

Although the shear band angles to the right of the velocity discontinuity of ...

34. [pg. 15, line 15] The following comment is incorrect “These are numerical effects tied to finite element models that should be taken into consideration when interpreting and comparing model results.” What you are observing are not numerical effects. They are also not confined to finite element discretisations. The “effect” you are observing (lack of length scale) is due to the fact that your model configuration (specifically the geometry of your regions and boundary condition) creates singularities in the strain-rate field (and pressure field). With your plasticity formulation, this singularity wants to drive the shear band thickness to zero. However your numerical method cannot resolve the singularity, the best it can do is approximate it. This approximation improves as you refine the grid, and as a result your shear bands become thinner. We discuss this in Spiegelman et al “On the solvability of incompressible Stokes with viscoplastic rheologies in geodynamics” (2016).

Agreed. We have changed the sentence to:

As explained by Spiegelman et al. (2016), this lack of internal length scale is caused by the singularities in strain rate and pressure deriving from the model set-up (e.g. sharp corners of the silicon layer and the discontinuous velocity boundary condition) that are resolved better at higher resolutions, thereby decreasing shear band width.

35. [pg. 18, line 15] *Again, it is not purely the rheology which is mesh dependent. The lack of a length scale stems from your choice of geometry of the slab (sharp corners) which induces singularities in the strain-rate field. If the problem is nonlinear, then the non-linear residual should always be monitored. There is no need to make a special note of that here. Please remove the statement "...iterative convergence should be monitored as for plastic rheologies."*

Yes, rephrased to:

It should be noted that the particular geometry of the slab with its sharp corners results in a mesh-dependence of the solution. Differences in model evolution can also arise from the particular viscosity and material averaging method applied.

36. [Fig. 18] *Caption: Please clarify if you are plotting the strain-rate invariant.*
We plot the Frobenius norm of the strain rate and have added this description to all captions.

37. [Fig. 18] *Top panel. Please explain why the strain-rate (invariant?) field at the upper surface (over riding plate side) contains discontinuities on the order of 1000 s^{-1} .*

For this model we applied an additional material averaging step where the viscosities and other material properties computed on the quadrature points of one element are averaged to obtain a constant value throughout the element. The contours of the plates cross different elements and therefore show the step-like discontinuities.

38. [pg. 24, line 20] *Since the non-linear solver and rheology used by the ASPECT models in Tosi et al differ from the implementation used in this work, you cannot cite Tosi et al to support your verification study. Please remove the last part of the first sentence in Sec. 5. Again, use the word verify and not validate.*

The non-linear solver used in the Tosi et al. paper was the same; the rheology was different. We have removed the last part of the sentence and used "verify".

39. [pg. 25, line 15] *The term "Newton iterations" is inappropriate to describe the methods used in Popov & Sobolev, May et al and Rudi et al. Newton is not an iteration - it is a non-linear solver. Changing from Picard to Newton doesn't just require change the iteration procedure. Many other solver components have to be introduced. Please correct the text to reflect this.*

We have changed the sentence to:

The more sophisticated and efficient Newton solver...

You should also add the following paper to your list of geodynamics codes using Newton's method:

@article {GGGE:GGGE21224,

author = {Wilson, Cian R. and Spiegelman, Marc and van Keken, Peter E.},

title = {TerraFERMA: The Transparent Finite Element Rapid Model Assembler for multiphysics problems in Earth sciences},

journal = {Geochemistry, Geophysics, Geosystems},

volume = {18},
number = {2},
issn = {1525-2027},
url = {http://dx.doi.org/10.1002/2016GC006702},
doi = {10.1002/2016GC006702},
pages = {769--810},
year = {2017},}
Done.

40. [Table 1] The parameter listed as “Effective deviatoric strain rate” is the symbol used to identify the second invariant of the strain-rate tensor. Please correct the parameter name so it is consistent with the rest of the text.

In Eq. (5), we define the symbol as the effective deviatoric strain rate, which is the square-root of the second moment invariant of the strain rate (Zienkiewicz and Taylor, 2002). We have added this definition to Table 1.

41. [Table 1] The symbol identified μ_{ref} identified with the name “Reference viscosity” does not appear in any equation shown in this paper. What is it? If it is not used - remove it from the table. It seems to appear in nearly every table, but I have no idea what this parameter actually means or how it relates to the rheological models used in this study.

The reference viscosity is used by ASPECT to compute a factor $\frac{\mu_{ref}}{L}$ for scaling the continuity equation (Eq. (2)) to obtain similar orders of magnitude for the dimensional momentum and mass equations (Eq. (1) and (2)). Characteristic length scale L should be set to a typical value for model features, while the choice of μ_{ref} should be guided by the viscosities present in the model. For variable viscosity models, this choice is not completely self-evident and as it affects the number of inner iterations, we listed the parameter value we used in the tables.

42. [Table 5] As per an earlier comment, I think the parameter “Local resolution” would be better defined in terms of cm (in this model), rather than in terms of number of elements. Specifying the equivalent number of elements required if a structured, non-adaptive mesh was used is overly confusing.

Changed to “Element size” for Table 5, 6, 7, 9 and 11.

43. [Table 6] The symbol for the reference viscosity given is μ_{max} - this looks like a typo.

Fixed.

44. [Table 6] The symbol for “viscosity capping” contains a latex typo.

Done.

45. [Table 8] The parameters $V_{dl}; Q_{dl}; B_{dl}$ have not been defined. These should be introduced when you define the specific flow laws for diffusion creep and dislocation creep.

They are declared in Table 1 and we’ve added the superscripts to Eq. 5 as well as the sentence after Eq. 5:

The superscript df here indicates diffusion creep, dl dislocation creep.

Additional changes:

The wall time for the indenter benchmark was quoted for the smooth indenter only, which was much smaller than for the rough indenter. We now report the wall time for both with ASPECT 1.5. Also, we changed the measurements of the velocity and pressure in Fig. 3, as it is now possible to extract solution variables at specific points based on the finite element solution instead of through ParaView.

The Stokes solver tolerance of the sandbox experiment was actually $1e-6$ instead of the initially reported $1e-5$.

References

Heister, T., Dannberg, J., Gassmüller, R., and Bangerth, W.: High accuracy mantle convection simulation through modern numerical methods – II: realistic models and problems, *Geophysical Journal International*, 210, 833–851.

Kronbichler, M., Heister, T., and Bangerth, W.: High accuracy mantle convection simulation through modern numerical methods, *Geophysical Journal International*, 191, 12–29, 2012.

Schmeling, H. A. et al.: A benchmark comparison of spontaneous subduction models-Towards a free surface, *Physics of the Earth and Planetary Interiors*, 171, 198–223, 2008.

Tosi, N., Stein, C., Noack, L., Hüttig, C., Maierova, P., Samual, H., Davies, D. R., Wilson, C. R., Kramer, S. C., Thieulot, C., Glerum, A., Fraters, M., Spakman, W., Rozel, A., and Tackley, P. J.: A community benchmark for viscoplastic thermal convection in a 2-D square box, *Geochemistry, Geophysics, Geosystems*, 16, 2175–2196, 2015.