

Interactive comment on “3-D thermo-mechanical laboratory modelling of plate-tectonics” by D. Boutelier and O. Oncken

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The referee, Dr. Manuele Faccenda, noted 3 main points that should be better addressed in order to improve the paper. Below we respond to the comments.

1) *The oceanic lithosphere is defined as elasto-plastic. The plastic yielding and the related softening/hardening are estimated. Such plastic behavior is similar to yielding in the brittle regime, therefore I think it would be worth to stress a bit more that the modeled plasticity pertain to the ductile regime, as indicated by the temperature dependence of the yield stress (fig. 4). In any case, is there any possibility to include and control the brittle behavior in these analogue materials?*

We described our model lithosphere as elasto-plastic because following reversible

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elastic deformation our materials deform permanently and this deformation is not very sensitive to rate but highly sensitive to strain and temperature. Following the terminology employed in material science, a material whose deformation depends on strain is plastic. Plastic deformation can be either brittle or ductile. Brittle deformation is characterized by rapid localized crack propagation and without significant irreversible macroscopic deformation. Ductile deformation is characterized by significant deformation distributed around the fracture zone (i.e. in a broad deformation zone) prior to failure (i.e. localization of strain within a narrow deformation zone). According to this description our materials exhibit plastic ductile failure, as permanent deformation can be observed prior to the formation of a narrow deformation/shear zone in rheological tests as well as in experiments. However, the amount of plastic deformation prior to failure decreases with the temperature. Therefore, our materials become stronger and more brittle – i.e. more localized - with lower temperature. To obtain within one layer a truly brittle behavior near the surface and a ductile plastic behavior at depth one would have to engineer a material whose brittleness decreases as quickly as the yield strength. However, our materials presently exhibit brittleness at very low temperatures and therefore have too large yield stresses. Furthermore, even if it we were able to modify our materials in order obtain more brittle behavior near the surface, we would not be able to reproduce the strength increase with depth in this part of the model. We acknowledge that the strength envelope is approximated near the model surface.

Following the reviewer's comment we have made some changes in the text to clarify and prevent misunderstanding on this issue. We now stress out more that the model plasticity corresponds to the ductile regime and that the strength envelop is approximated near the surface, as pressure-dependant brittle plasticity cannot yet be implemented.

2) *Each model uses kinematic boundary conditions, where the imposed rate of convergence is scaled accordingly with the thermal diffusivity of the system. Such boundary conditions are useful for example to initiate subduction. On the other hand, however,*

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they do not allow to investigate properly how the system would evolve dynamically without any imposed convergence rate after the subduction initiation (e.g., Faccenda et al., 2008, 2009). The authors could discuss if such dynamical and probably more realistic model is feasibly reproducible in the future with the present day techniques. In effect, by tuning the material parameters a fully dynamic analogue model should be able to reproduce the scaled convergence rates and stresses.

Our modeling apparatus is computer-controlled and a single computer simultaneously reads the force measured at the back of the upper plate and imposes the rate of the piston. The force sensor is interrogated 30 times per seconds and the computer can either simply store the values and maintain convergence rate at a set value or adjust convergence rate in order to achieve a set force. We have not yet fully tested this system and there are parameters that remain to be investigated/tuned such as the maximum allowed acceleration or filtering of the force signal in order to ignore the high-frequency force variations that do not pertain to the model mechanics. We expect to be conducting the experiments multiple times using various boundary conditions and employ the results from one experiment to feed the next. For example, we presented subduction experiments and measured constant force obtained when the slab is neutrally buoyant. We plan to use this force value (obtained in constant velocity experiment) to produce constant force experiments with oceanic subduction followed by continental subduction/collision. We expect to produce the proper subduction velocity during the oceanic subduction stage and obtain velocity reduction during burial of the buoyant continental crust. The more complex type of boundary condition used by the referee in numerical simulations (i.e. constant velocity for a certain time/amount of convergence, followed by a constant force/stress) will also be soon possible with our system. However, before using such a system we must replace the fluid used to model the asthenosphere in order to properly scale the viscous interaction between lithosphere and asthenosphere.

Following the reviewer's comment we made changes in the text in order to better present the possible evolution of this modeling. We previously simply mentioned that

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force-controlled experiments should be possible. Now we explain how this will be performed and what strategy will be employed. We note that fully unconstrained (dynamic) 3D models will be performed. However, we also argue that it is absolutely legitimate to maintain control of either force or rate because it gives us a better chance of learning which parameters and feedbacks are of relevance in our system.

3) The authors aim at producing complex 3-D models by varying the material properties along the convergent margin, like for example by changing the friction along the plate interface that will transmit different horizontal stresses on the upper plate or with a curved margin. I believe that this approach will give certainly useful insights into the local scale variation of the upper plate deformation along the trench, but the regional scale behavior of the system would remain substantially 2D or in any case strongly by the imposed initial geometrical set-up. Nowadays, most of the 3D numerical and analogue models regarding subduction zones aim at reproducing the observations with dynamic models accounting, for example, for the flow in the asthenospheric mantle that include a strong toroidal component (i.e., Schellart et al., 2007, Nature; Morra et al., 2006, Geology; Funicello et al., 2003). With the present model set-up, where the plates are as wide as the tank, no toroidal flow can be reproduced (and, therefore, no realistic dynamic model can be run). Other important parameters that were found to be important for the dynamic of a subduction margin are the slab width (Schellart et al., 2007, Nature) or along strike variations of the slab thickness (e.g., Morra et al., 2006, Geology). The presented experimental apparatus has the potential to merge all these dynamic effects and along strike heterogeneities together with the novel techniques implemented. Once again, wouldn't be better, then, to set-up dynamic models that would give a more complete understanding of the 3D evolution of the whole system when compared to kinematic models?

There is no fundamental difference between the cited 3D models and ours. In the presented experiment the model deformation was mostly cylindrical because the imposed initial and boundary conditions were cylindrical. We explained that the rationale

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for such an experiment is to estimate the stresses on the interplate zone. However, when imposing non-cylindrical initial and boundary conditions we obtain a truly three-dimensional model deformation. Furthermore, our set-up also allows toroidal flow in the asthenosphere as there are 5-cm-gaps between the model lithosphere and the walls of the experimental tank. The toroidal flow presently has no effect on the model because we use an inviscid asthenosphere, but it may become important once we have changed the material used to model the asthenosphere and properly scaled the viscosity of the asthenosphere. We will, in the future, develop our modeling apparatus towards a more dynamic approach, while keeping available the velocity control.

Following the reviewer's comment we have implemented changes in the text, mostly in the discussion section, to explain that the next step of this modeling is to use a high viscosity fluid to model the asthenosphere, and then to implement controlled-stress experiments.

Modifications made to the manuscript in response to the reviewer's comments are not yet apparent in the online discussion paper. When the discussion is closed, we will upload a revised manuscript with all modifications.

Interactive comment on Solid Earth Discuss., 3, 105, 2011.