Solid Earth
Discussions

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Interactive comment on "Comment on Marques et al. (2018), Channel flow, tectonic overpressure, and exhumation of high-pressure rocks in the Greater Himalayas" *by* John P. Platt

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The Reply by Marques et al. to my Comment on their paper suggests that some of my criticisms were insufficiently clear and precise. I take the opportunity here to clarify the most important points with the help of some diagrams and a simple mechanical analysis.

Firstly, there is a clear conflict between the geological configuration they use to justify their model, and the configuration they actually use. The geological situation, based on the present-day Himalayan orogen, involves an irregular footwall with features that they describe in terms of a ramp and flat geometry (panel A in the figure, attached as a

supplement). These features are part of the down-going Indian plate, and hence move with the footwall at least as fast as the material in the subduction channel. The footwall flat, which they describe as the "base" to the channel, will not obstruct the downward flow of the material in the channel, and will therefore not lead to return flow. The material in the channel will simply move down along with the footwall and the base, and the dynamic pressure will in fact be negative where the base meets the upper plate. This is quite different from the geometrical and kinematic configuration they use in the model (panel B in the figure). Although Marques et al (2018) do not explicitly state the boundary conditions used for the base, it is clear from their model results that it is fixed with respect to the upper plate. This results in an abrupt change in the boundary conditions at the point marked with a red dot in panel B. This is the "corner" that leads to the positive dynamic pressure and the return flow. This configuration does not resemble that in the present-day Himalaya, and is geologically highly improbable. No present-day subduction zone has this configuration, and there is no evidence that it existed in the Himalayan subduction zone in the past. It would have been much more useful had they used a simple downward tapering geometry for the channel (e.g., panel C in the figure), and calculated the resulting dynamic pressure as a function of viscosity and rate of subduction. This would be a real contribution to the more general problem of return flow in subduction zones, though it would have to be done together with a realistic analysis of the response of the upper plate (see next point).

My second criticism concerns their use of a fixed upper boundary to the channel. The kinematic boundary condition they specify is essentially non-physical, because it results in unbalanced forces across it: the normal load on one side is the total pressure in the channel (lithostatic plus dynamic pressure), and on the other it is just the lithostatic load. It is true that rigid boundaries are commonly used in fluid mechanics problems, because the mechanical contrast between a low-viscosity fluid such as water and a steel pipe, for example, is so large that deformation of the boundaries can be neglected. In the case of a subduction channel, however, the modeled viscosity is more than 20 orders of magnitude greater than that of water, and the viscous stresses are

correspondingly larger. No realistic geological material can withstand an unbalanced normal load of 1.5 GPa without either significant elastic flexure or permanent deformation. The upper plate in the Himalayas consists of thick continental crust with a complex internal structure, and is not particularly strong. Unless it deforms permanently (which is likely), it will flex until the bending moments are large enough to balance the enormous load. The laccolith analysis from Turcotte & Schubert (2002) provides a good enough approximation to the situation – it's a pity Marques et al. (2018) didn't try this calculation before publishing their paper. Using realistic parameters, it predicts a flexural upwarp of 50 km (see supplement), which is so absurdly large that it demonstrates unequivocally how unrealistic their predictions of dynamic pressure are in geological terms.

The "deformable walls" model described in Marques et al (2018) does not change this conclusion: they still specify a fixed upper plate; this, and the no-slip boundary condition between the deforming walls and the upper plate, restrict deformation in the walls to shear parallel to the boundaries. Hence the unbalanced force condition across the boundary with the fixed upper plate remains.

It seems logical that the dynamic pressure will in practice be limited by the strength of the upper plate. Various lines of evidence suggest than an upper limit of $\sim$120 MPa shear stress is reasonable for continental lithosphere in actively deforming regions (e.g., England & Molnar, 1991; Behr & Platt, 2014), and this is consistent with values calculated from experimental rock mechanics data (e.g., Platt & Behr, 2011). This limits the stresses associated with the flexural response to the dynamic pressure, and hence could be used to place an upper bound on the magnitude of the dynamic pressure.

The remarks about the causes of dynamic pressure and return flow in the Reply serve only to obscure the underlying physics of the situation, so some clarification is needed. The Navier-Stokes equations relate the pressure gradient to the Laplacian of the velocity and the body force in the viscous channel. The Laplacian, which comprises second derivatives of velocity, is directly related to the stress gradients in the stress equilibrium

equations, from which Navier-Stokes is derived. In a subduction channel viscous material is entrained by the down-going slab, but if the upper and lower plates converge, so as to close the channel, this material is forced away from the slab at the resulting corner (indicated by the red dot in panel C of the figure). As a result it experiences an abrupt change in stress, and the resulting steep stress gradients require correspondingly steep pressure gradients, as shown by Navier-Stokes. The pressure gradients result in a build-up of pressure near the corner, and this in turn drives the return flow along the upper boundary of the channel. That's how corner flow works, and that's why it's called corner flow.

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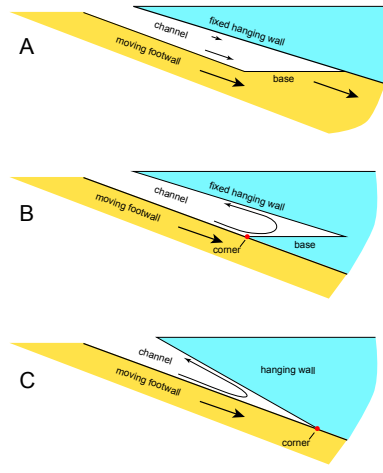
Turcotte D. L, Schubert, G. (2002). Geodynamics, 2nd ed., Cambridge University Press, 456pp.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2018-92/se-2018-92-AC2-supplement.pdf

Fig. 1. A) Configuration of the Himalayan subduction zone described by Marques et al. B) Configuration used in the model. C) Standard configuration for corner flow in a subduction zone.