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Solid Earth
Discussions

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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Response to Comment by Stefan Schmalholz J. P. Platt jplatt@usc.edu

What a pleasure to see some genuine scientific discussion of the issues! Up to this point the discussion seems to have consisted of flat denial that there is a problem, combined with pejorative remarks about my professional competence.

Schmalholz's analysis of the magnitude of the elastic flexure of the upper plate in this situation is very useful. My only comment is that the value for L that I used was measured directly from Figure 2 of M18, being the distance along the upper boundary of

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the channel over which the dynamic pressure exceeds 1.5 GPa. My value for h was taken from Jordan & Watts (2005), who determine the effective elastic thickness over the whole region from the admittance between topography and free air gravity. 20 km is the maximum possible value for southern Tibet: they give the range as 0-20 km. This provides direct observational evidence that the overriding plate is weak, and unable to sustain topographic loads of more than a few tens of MPa. To get a 60 km value for h we would need to go into the Indian shield, which is composed of dry granulite facies rocks overlying cold lithospheric mantle.

It is quite true, as Schmalholz states, that many fluid-mechanical models assume fixed boundaries. That doesn't mean this assumption is right. For low viscosity fluids such as water flowing in a steel pipe, it is a reasonable approximation. In the case of the subduction channel modeled by M18 in their Figure 2, the viscosity is 24 orders of magnitude greater than that of water, and the viscous stresses are correspondingly larger. A physical process is required that is capable of keeping the boundary fixed, and M18 gave no indication what this might be. In the absence of such a process, the only load acting downwards on the upper boundary of the model is the weight of the overlying rock. The forces are then unbalanced, which would lead to the uncomfortable conclusion that the entire upper plate in the Himalayas would accelerate upwards at $\sim$1g. Schmalholz helpfully provides some physical scenarios that could avert this catastrophe, involving elastic deflection of the upper plate, but M18 did not: hence my critical comment.

With respect to the model with deformable walls, I based my comment on the statement in M18 that they "chose a geometry with kinematic boundary conditions as in the reference model with rigid walls" (line 305-6 in M2018). This effectively constrains the flow in the deformable walls to be parallel to the rigid bounding plates, and this is confirmed by the velocity trajectories shown in Figure 8 of M18. In their Reply, M18 did not address this issue.

My thanks once again to Schmalholz for his constructive discussion.

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