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

J. Platt

jplatt@usc.edu

There are two aspects to numerical calculations like those presented by Marques et al (2018), referred to henceforth as M2018. One is the overall configuration and boundary conditions, the other is the actual calculations. The purpose of my comment was to point out that the configuration they suggest makes no geological sense, that the boundary conditions are non-physical, and that the conclusions are geologically unreasonable. Most of the points I raised should be obvious to anyone with an understanding of basic physical and geological principles. I will address them using the same structure as Schmalholz.

Point 1. In my comment on M2018, I pointed out that although the subduction channel they propose has a complicated geometry, it is closed at the base, and open at the top. It is therefore misleading to describe it as upward-tapering. I stand by this comment; the results of M2018 do not falsify it. There are several processes that can drive return flow in a subduction channel: buoyancy, topographic gradients, and dynamic pressure being the most widely recognized. Dynamic pressure results when a viscous fluid that is entrained by a moving boundary is forced away from the boundary and to flow in a different direction. In the subduction environment we are considering, this occurs when the subducting slab meets the upper plate, so that downward flow in the subduction channel is prevented, and the fluid is forced back up the channel by the resulting dynamic pressure. This process has been recognized and described as corner flow for more than a century. I don't know why M2018 and Schmalholz choose not to use this widely accepted term.

Point 2. Schmalholz mis-quotes me. I stated that the footwall flat, which M2018 suggest acts as the base of the channel, does move; it moves with the footwall, down the dip of the subduction zone, and will move at least as fast as the fluid near the base of the channel. Hence it cannot block the flow, and the channel will not be closed at the base. The base of the channel proposed by M2018 cannot be a footwall flat: it has to be fixed to the upper plate, and from a geological perspective it is has a highly improbable geometry. A numerical simulation isn't required to demonstrate this problem, but a basic knowledge of structural geology is helpful.

Point 3. Schmalholz disputes my statement that the models in M2018 do not allow for motion normal to the channel boundaries. M2018 clearly state that the upper plate is rigid and fixed; the lower plate can only move parallel to the channel boundary. For the model with deformable walls, they "chose a geometry with kinematic boundary conditions as in the reference model with rigid walls" (line 305-6 in M2018). The thicknesses of the deformable walls are not given, but they appear to be indicated in Figure 8A. The interfaces between the deformable walls and the rigid plates above and be-

low are given a no-slip boundary condition. This effectively constrains the flow in the deformable walls to be parallel to the rigid bounding plates.

Schmalholz also disputes my statement that the dynamic pressure in the models produces an unbalanced load on the upper plate. He is wrong. M2018 used the Navier-Stokes equations for creeping flow in the channel, which are based on the stress equilibrium equations, so the forces in the channel are in equilibrium. But the boundary conditions they chose are non-physical. The loads normal to the upper boundary of the channel consist of the pressure in the channel (lithostatic load + dynamic pressure) on one side, and the lithostatic load alone on the other side. In M2018 they try to justify this by stating that the upper plate is rigid. Even a rigid plate will exhibit elastic behavior, however, and it has been well known for more than 150 years that an unbalanced load normal to an elastic plate causes a flexural deflection. The deflection is resisted by bending moments in the plate, which increase with the deflection until the load is balanced. The resistance scales with the deflection: if there is no deflection, there is no resistance. Hence in the model proposed by M2018, where no motion normal to the boundaries is allowed, the forces will be unbalanced. My statement was neither speculative nor mechanically unsound.

Schmalholz, after some irrelevant discussion, states that the elastic flexure of the upper plate has to be calculated with an adequate model. I show here with an approximate analytical calculation that modeling is not required. The configuration suggested by M2018 can be approximated by the analysis for flexural doming above an igneous intrusion presented by Turcotte & Schubert (2002). In this analysis, the roof of the intrusion is flexed up by magmatic pressure that exceeds lithostatic. A separate file with the analysis is attached as a supplement. The predicted deflection is 50 km. This is so large that it violates one of the assumptions of the analysis, that the deflection is small compared to the distance over which the excess pressure is applied. The analysis also does not take into account the tapering geometry of the upper plate, and it is sensitive to the values chosen for the elastic modulus and the effective elastic thickness. But it is

sufficient to demonstrate that a dynamic pressure of 1.5 GPa in the Himalayan subduction zone is geologically unsustainable. No flexural upwarp of ∼50 km amplitude has been detected in southern Tibet. A full numerical analysis of the flexure is unwarranted at this point. What is needed is for the authors of M2018 to reconsider their modeling configuration, remove the non-physical boundary conditions, and come up with a revised model (including a deformable upper plate) that is consistent with geological constraints.

I suggested that the dynamic pressures estimated by M2018 are likely to be too high by at least an order of magnitude. This was based on the flexural effect discussed in the previous paragraph. Given that there are various free parameters in their model (e.g., the viscosity), I thought it would be helpful to indicate the range in which a geologically acceptable result might fall. It is not incumbent on me to reconfigure their model and carry out a numerical exercise: that's a job for the authors. I'm interested in the results, but this type of modeling is not my area of expertise.

Schmalholz's minor comments. "petrologically determined depths of burial". Perhaps I should have written petrologically inferred depths of burial. This doesn't affect the point I was making.

"the temperature determination would not be affected". Changing the petrologically determined depth of burial, without changing the pressure, does not affect the petrologically determined temperature, regardless of the heat sources for metamorphism. My point was that postulating a decreased depth of burial for UHP rocks exacerbates what has been identified as a potential problem with thermal gradients in subduction zones.

Jordan, T. A., and Watts, A. B., 2005, Gravity anomalies, flexure and the elastic thickness structure of the India-Eurasia collisional system: Earth and Planetary Science Letters, v. 236, p. 732-750 Marques, F.O., Mandal, N., Ghosh, S., and Ranalli, G ., Channel ïňĊow, tectonic overpressure, and exhumation of high-pressure rocks in

the Greater Himalayas. Solid Earth 9.5 (2018): 1061-1078. 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-AC1-supplement.pdf