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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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Platt (2018) questions the main results of Marques et al. (2018), referred to as M2018 in the following, who use a two-dimensional numerical linear-viscous flow model to quantify magnitudes of dynamic pressure in a trapezoidal model domain. A main comment of Platt (2018) is: "I suggest that their estimates of dynamic pressure are at least one order of magnitude too high". This statement, like essentially all other statements in Platt (2018), is purely speculative and not substantiated by any mechanical calculation or alternative numerical model.

Platt (2018) argues that there are three main problems in the study of M2018:

In point 1 Platt (2018) states: "Whatever the details of the channel geometry, it must ultimately always taper downwards if it is to produce the corner-flow effect". M2018 show with their numerical simulations that return flow is generated in their model, which has an "upward-tapering" geometry. The results of M2018, hence, falsify the above statement of Platt (2018). M2018 never use the term "corner flow", but speak of "channel flow". Corner flow models commonly consider flow around a single corner. With respect to geometry, the trapezoidal model geometry of M2018 is more similar to a circulating cell model (e.g. Pollard and Fletcher, 2005; their figure 10.24).

In point 2 Platt (2018) states that the horizontal base of the model in M2018 does not move horizontally which does not fit the tectonic situation in which the Indian lower-crust and mantle lithosphere is underthrusting Tibet and hence the "base" of the Greater Himalayan Sequence should have a horizontal velocity component. This is a fair comment. However, Platt (2018) does not make any prediction about how a horizontally moving base would affect the results of M2018. In the model of M2018 there is a velocity singularity at the left edge of the model base and a model with a horizontally moving base can have a velocity singularity at the right edge of the base. The consequences of such different boundary condition have to be calculated with a corresponding numerical simulation in order to quantify the impact on the results of M2018.

Point 3: The statement of Platt (2018) that M2018 "do not allow for any motion normal to the channel boundaries" is, to the best of my knowledge, not correct. M2018 also show results for which the material above and below the channel can deform viscously. M2018 state: "This model allows for both channel walls to deform viscously, thus raising the question of how much overpressure they can retain inside the channel". Based on the description of the boundary conditions in section 3.4 of M2018, I conclude that this model allows for motion normal to the upper channel boundary.

The paragraph on page 2 from lines 6 to 18 in Platt (2018) includes mainly speculative "should-would-could" arguments, which are also mechanically unsound. For example, Platt (2018) argues that “an unbalanced upward load of 1.5 GPa should cause a substantial flexural upwarp of the upper plate, possibly tens of km in amplitude, producing a major topographic and gravity anomaly”. It is not logical why there should be an “unbalanced upward load” in a mechanical model, which is based on the equations of force balance. The dynamic pressure of 1.5 GPa is not an “unbalanced load”; this dynamic pressure and the associated pressure gradient is responsible for “pushing” the viscous material upwards, against gravity and against the downward direction of the applied boundary velocity. Platt (2018) further argues that “given that the material in the subduction channel is incompressible, even a small amount of flexural displacement would be enough to relieve the dynamic pressure”. Indeed, it is well established that the dynamic pressure depends on the strength of the channel walls and dynamic pressure decreases when channel walls get weaker and, hence, displace more (e.g. Mancktelow, 2008). Such pressure relieve has been quantified with numerical models in several studies (e.g. Mancktelow, 2008) and is also mentioned in M2018 in section 1.2. M2018 report significant dynamic pressure also for models in which the viscosity of the channel was 100 to 1000 times smaller than the viscosity of the material bounding the channel (their section 3.4 and their figure 8). Moreover, the impact of elastic flexure on the dynamic pressure is not so obvious. For example, for a different model configuration for a compressed lithosphere Petrini & Podladchikov (2000) show with an analytical solution that elastic flexural loads of the upper crust and upper mantle can increase the dynamic pressure in a weak lower crust. Therefore, the elastic flexural displacement, mentioned by Platt (2018), has to be calculated with an adequate model in order to test whether and for what conditions elastic flexure causes a significant pressure relieve.

Page 2, lines 23-24. The statement of Platt (2018), “I suggest that their estimates of dynamic pressure are at least one order of magnitude too high”, is not substantiated and not quantified by a mechanical calculation or model. I recommend to calculate dynamic pressure and not to suggest it.

In summary, Platt (2018) provides interesting suggestions for additional simulations, such as considering a horizontally moving model base and an elastically deformable “upper plate”. However, Platt (2018) provides not a single mechanically sound calculation or alternative mechanical model, which shows that dynamic pressure magnitudes calculated by M2018 are more than one order of magnitude too high.

Minor comments

Page 2, Line 29: “petrologically determined depths of burial”. This phrase reveals a common misunderstanding. One cannot petrologically determine a burial depth, one can only petrologically determine a thermodynamic pressure from phase equilibria calculations. To determine a burial depth, it is commonly assumed that this pressure is lithostatic. Whether pressure is lithostatic or not cannot be determined petrologically.

Page 2, 30-31: “. . .since the temperature determination would not be affected.” Models considering energy conservation show that dissipative deformation generates heat and, hence, temperature increase. How much temperature increases has to be calculated by thermo-mechanical models.

Page 2, Line 31-33: Penniston-Dorland et al. (2015) investigate pressure to temperature ratios and argue, amongst others, that numerical subduction models are “too cold”, likely because models did not incorporate all sources of heat. Since heat transfer was not calculated by M2018, comments on potential thermal results are speculative; one should perform thermo-mechanical simulations.

Best regards,

Stefan Schmalholz (P.S.: I was not reviewer of the manuscript of Marques et al. (2018))

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