Review of “Comment on Marques et al. (2018), Channel flow, tectonic overpressure, and exhumation of high-pressure rocks in the Greater Himalayas” by John Platt.

Dear editor,

please find below my specific comments regarding the criticism of Prof. Platt on the paper by (Marques et al., 2018). The author has revised some major points that were raised by my previous review, however I have some major points to add which I list in detail below. Most importantly, I would like to highlight that Prof. Platt uses an analytical solution of elastic deformation in order to criticize the purely viscous model proposed by Marques et al (2018). Clearly, different assumptions are lying behind the different models (elastic vs. viscous) and therefore I recommend that the author separates what is actually the result of Marques et al (2018) analysis and what is a consequence of his own model. In particular, Prof. Platt uses an elastic model to predict the flexure that would result from the stresses calculated by the viscous model of Marques et al. (2018). At this point, I would like to highlight (see also my point P8) that the geometrical configuration used by Marques et al. (2018) approximates the one currently observed, and not an initial condition. Consequently, in order to predict the time evolution of such a system and check out how realistic it is, one can model the time evolution of the system. Any change in geometry and viscosity structure of a purely viscous system will result to different stress/velocity distribution.

Best

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Specific Comments (I refer to P1, P2 as points 1, 2 etc)

P.1 “Note that dynamic overpressure as used here is generated by flow in a viscous fluid, and differs in this respect from the more widely recognized concept of tectonic overpressure, which is related directly to deviatoric stress, and can exist in a static situation, with or without deformation”.
>> This statement is confusing. The flow of a viscous fluid cannot be unrelated to the deviatoric stresses. By definition, the flow of viscous materials requires the presence of deviatoric stresses.

P2. “Return flow in subduction channels has been proposed as a mechanism for exhuming high-pressure metamorphic rocks from deep in the subduction zone (e.g., Cloos, 1982). Possible drivers are buoyancy (e.g., England & Holland, 1979; Beaumont et al., 2009), topographic gradients (e.g., Beaumont et al., 2001), or dynamic overpressure (e.g., Gerya & Stockhert, 2002).”
>> In the case of corner flow (Cloos, 1982), the return flow is independent of buoyancy stresses (Batchelor, 1967). In fact, the dynamic overpressure (difference from the lithostatic; also associated with deviatoric stresses) is responsible for the return flow. In other words, there is no corner flow s.s. without pressure deviations from the lithostatic. By contrast, the main driver for exhumation in the channel-flow model of England and Holland (England and Holland, 1979), is buoyancy. I would therefore recommend rephrasing of the related paragraph.

P3. “the dynamic overpressure is limited by the ability of the channel walls to contain it. If the walls deform, the pattern of flow will change, and the dynamic overpressure is likely to decrease.”

>> The author has a point here, however one needs to model the time evolution of the wall deformation. For example, a system where the wall deflects in 10,000 years is different from a system where the wall deformation would take tens of millions of years to evolve. The specifics of the evolution would, in turn, depend on the particular mechanical response of the wall and the boundary conditions assumed. Therefore, without being more specific this point is rather weak.

P4. “The second problem is that they assume a fixed upper boundary to the subduction channel, which cannot be defended in geological terms, and leads to unrealistic conclusions”

>> I have stated my disagreement with this comment in my previous review. The author in one of his response comments suggested that a careful investigation of the boundary conditions of Marques et al (2018) reveals that the wall is fixed. Based on the description of the model setup with deformable walls by Marques and co-workers, I find this statement misleading (i.e. in the models with deformable walls the walls are not fixed).

P5. “A more fundamental problem concerns their use of a fixed upper boundary to the channel. It is true that fixed boundaries are commonly assumed 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 the subduction channel modelled by M2018 in their Figure 2, the viscosity is orders of magnitude greater than that of water, and the viscous stresses are correspondingly larger.”

>> Fluid dynamics solutions are not restricted to water; in fact, it is pointless to use water as a reference. This is actually why fluid mechanics are successfully applied to structural geology and geodynamics problems (Pollard and Fletcher, 2005; Turcotte and Schubert, 2014). Fluid dynamics solutions depend on the viscosity ratios of different materials. Even when a rock has a viscosity much larger than that of water, it can still behave as a low-viscosity fluid compared to the rock that exhibits even higher viscosity (see for example Gerya, 2010, p. 245). Marques and co-workers used viscosity ratios differing by 2-3 orders of magnitude. When the viscosity of the wall is 3 orders of magnitude or larger than the viscosity of the convecting fluid, then, the deformation of the wall would be negligible. Importantly, even if the initial boundary is assumed perfectly straight, time integration of the mechanical solution allows for conclusions to be drawn on the deformation of the strong lid.

P6 “If a dynamic overpressure of 1.5 GPa is applied from below to the upper boundary of the channel, a physical mechanism is required that is capable of keeping the boundary fixed, and M2018 give no indication what this might be.”
>> This statement is not true. Marques and co-workers clearly state that this can occur if the walls are strong, so that the boundary would behave as if the lid were rigid. It is the high viscosity of the channel wall (that can build up large stresses) that is responsible for keeping the boundary nearly fixed.

P7 “the only load acting downwards on the upper boundary is the lithostatic pressure”
>> This statement cannot be true for a deforming lithosphere with topography and density changes.

P8 "In the case of a subduction channel, the configuration can be approximated by the analysis for flexural doming above an igneous intrusion presented by Turcotte & Schubert (2002)."
>> The applicability of this solution in the subduction channel is not entirely justified. An important assumption for the application of the solution of Turcotte and Schubert (2002) is that the initial condition is known. Firstly, the layers of rocks are assumed to be horizontal and secondly, the deflections are calculated from this initial stage. By contrast, the configuration of Marques and co-workers is not an initial condition. Their solution is meant to depict the current configuration that satisfies force balance. Therefore, the plate deflection evolution in the Marques et al (2018) viscous model must be integrated over time as it is commonly done in Geodynamic modelling of slow viscous flow e.g. Gerya (2010).

P9. "Various lines of evidence suggest than an upper limit of ~120 MPa shear stress is reasonable for continental lithosphere in actively deforming regions"
>> These values for shear stress have no universal applicability. There are numerous models that use experimentally determined flow laws that would not agree with such a statement. Stresses on the order of 100MPa are the minimum required to support topography in mountainous regions only if the entire lithosphere is stressed in a uniform manner (average stresses). Clearly, this is highly improbable since the presence of viscosity heterogeneities would result to regions in which the shear stress would be significantly higher or lower (Schmalholz et al., 2018, 2014).

P10. "The model set-up by M2018 does not conform with these important principles …."
>> As mentioned in my previous review, instead of comparing the results of the Marques et al. (2018) model with natural observations, Prof. Platt compares the results of his own elastic flexure model with natural observations. However, the assumptions lying behind the elastic flexure formula are different to those invoked by Marques and co-authors (2018) in their viscous-flow model.

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