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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 comment posted by Prof. Platt (hereafter P18) highlights some points of the model proposed by Marques and co-workers (hereafter M18) in their publication entitled "Channel flow, tectonic overpressure, and exhumation of high-pressure rocks in the Greater Himalayas" in a very critical manner. To the author's opinion, the most essential criticism of P18 on M18 model is the model configuration. Based on P18, the contact between the subduction channel and its overriding plate, as presented in the model of M18, is "unphysical" and it leads to erroneous predictions of tectonic overpressure (TOP). The characterization "unphysical" and the subsequent arguments



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Discussion paper

used by P18 are based on erroneous assumptions that lead to unphysical conclusions, and are therefore unjustified. Before I go into the details of the comment of P18 I will highlight the main aspects of the model configuration presented by M18. The models made by M18 can be broadly separated into 2 categories A. Models that concern only the channel. B. Models that consider a broader model domain where the channel and its overriding plate are deformable (section "Viscous deformable walls" p.1067-1068 in M18). The first category of models was criticized by P18 on the basis that, in order to have such velocities, the material of the overriding plate must be undeformed. However, for the second category of models shown by M18, the authors clearly state that they have relaxed this assumption and they actually considered deformable walls. In addition, M18 suggest that if the viscosity of the channel walls is at least 100 times larger than that of the channel then TOP can be significant and the overriding plate remains essentially undeformed. However, M18 do not provide details on how "significant" is the magnitude of TOP in that case. In the case where the viscosity of the channel is 1000 times larger than that of the walls then TOP is in the order of a GPa and then clearly the authors state that their conclusions regarding the TOP depend on the strength of the walls. P18 argues that if there is a significant amount of TOP, then, the "excess" pressure will cause elastic flexure of the upper plate that would be unrealistic. Following P18, the deflection of the overriding plate is a consequence of having "unbalanced loads" in the channel. This criticism by P18 reveals a misconception of P18 regarding force balance in Stokes' equations. The model of M18 satisfies force balance everywhere within the model. One cannot make predictions of the magnitude of the applied stresses in regions outside the model domain. For example, in a follow-up comment (29-Oct-2018), Prof. Platt argued that the TOP in the viscous channel is unbalanced since the overriding plate experiences lithostatic pressure. The last statement clearly reveals a mechanical misconception, i.e. it was the implicit assumption of P18 that pressure is lithostatic in the overriding plate. The last statement is mechanically unfeasible and violates force balance. In other words, there is no moment in time where there would have been significant TOP in the channel and lithostatic stress in the channel wall. Therefore, if there is a significant TOP in the channel then one needs to solve for the state of stress outside of the channel in order to have any meaningful stress estimates. In summary, the large values of TOP predicted by M18 are just the outcome of model inputs regarding the geometry, the specific rheology, the overall boundary conditions etc. How appropriate are these estimates needs to be verified and quantified. Without specific information on the stress distribution on the models of M18, one cannot judge how realistic these results are with respect to their stress magnitudes and the strength that they imply for the overriding plate. The plot given by M18 regarding TOP (their figure 8b) is not sufficient to draw meaningful conclusions for the state of stress in the channel and its bounding wall. In addition, when large regions of the lithosphere are considered, the pressure and temperature changes that occur within the lithosphere will have a large effect on the active deformation mechanisms and rheological parameters. Consequently, the choice of the rheological behavior and the geometrical configuration will have a first-order control on the stress predictions.

Specific comments P1-l.15-25, M18 did not state that they have a typical corner-flow model. Therefore, there is no justification for the suggestions of P18 regarding the tapering angles of the models of M18. P1-l.28 "…at the same velocity as the downward flow in the channel" There is no specific reason on why the downward velocity must be exactly the same as the one of the plate especially when rheological boundaries are considered. Perhaps, having it perfectly immobile like in the case of M18 may be an exaggeration. However, without having a self-consistent model where this channel would be an emerging rather than an imposed feature, no quantitative estimates can be given. P2-l.3-5 In their paper, M18 clearly state that they consider also the case of deformable walls therefore all this section is not justified. P2.l.6-24 All this part is speculative and based on erroneous implicit assumptions. i.e. there is no reason why the load must be unbalanced. Therefore, all the arguments that follow (e.g. about unrealistically large flexures etc.) are based on faulty assumptions. Additionally, the question posed by P18 on "why are the predictions of M18 so dramatically at variance with what we observe?" is misleading since the "predictions" were made by P18 and not by M18. P2.29-33 The statement “petrologically determined depths” by P18 confirms the author’s view about mechanical misconceptions in the arguments used by P18. I would like to highlight that the petrologically “determined” (or inferred) depths are usually a result of the lithostatic pressure formula. The lithostatic pressure formula can be derived from Stokes’ equations if one assumes that there are no differential stresses, no topography and that density is stratified e.g. (Gerya, 2015; Moulas et al., 2018). The pressure is therefore an outcome of a mechanical model and not an independent constraint.

References Gerya, T., 2015. Tectonic overpressure and underpressure in lithospheric tectonics and metamorphism. Journal of Metamorphic Geology 33, 785–800. https://doi.org/10.1111/jmg.12144 Moulas, E., Schmalholz, S.M., Podladchikov, Y., Tajčmanová, L., Kostopoulos, D., Baumgartner, L., 2018. Relation between mean stress, thermodynamic and lithostatic pressure. Journal of Metamorphic Geology in press.

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