

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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Prof. Platt uses an elastic flexure formula (from analytical solution) in order to calculate the corresponding deflection of the overriding plate in the models of Marques et al. (2018). This loading will instantaneously deform the overriding plate as much as 50km upward. This is a consequence of the elastic rheology that is utilized by Prof. Platt and not included in the models of Marques et al. (2018). Since the elastic response is instantaneous, therefore, the current subduction configuration in nature corresponds to the stressed state, i.e. the stress state AFTER the loading. The 50km deflection calculated by Prof. Platt could be hypothetically observed as the result of unloading

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to a state with negligible tectonic overpressure (TOP). Since such deflections seem unrealistic, the hypothetical stress state where TOP would be insignificant is equally unrealistic. This stress state is expected after the gravitational collapse of the mountain ranges and their roots, i.e. towards flat Moho and topography. This stress state would therefore not be envisioned in active belts like the Himalayas.

Minor issues: Prof. Platt states that my view regarding the predictive ability of Stokes' equations outside the model domain "is an abrogation of scientific responsibility". I agree that it is the responsibility of the researchers to check the applicability of their boundary conditions in their models. As I already mentioned in my previous comment, Marques et al. considered two types of models. 1) A subduction channel with kinematic boundary conditions. And 2) an extended model where the overriding plate is included, the channel boundary is deformable and its deflection is not a boundary condition but a model prediction. In my statement that: "One cannot make predictions of the magnitude of the applied stresses in regions outside the model domain" was used in the context of predicting the stress state of the overriding plate. Naturally, one cannot predict the stress state in the first-type of models, as the overriding plate is outside of the model domain. In the second case the stress state of the overriding plate is predicted. The overriding plate IS within the model domain. Therefore, my aforementioned quote is rather stating the obvious and is not a topic for discussion.

A related topic for discussion is the statement by Prof. Platt regarding the model of Marques et al. (2018): "In their model set up, the only load acting on the upper boundary is the weight of the overlying rock". This statement is unrelated to both types of models that Marques et al. (2018) considered. The first type of model considers kinematic boundary condition for the top channel boundary and the second type considers that "[the] top boundary was also left unconstrained, allowing the material to extrude upward freely" p. 1068 in Marques et al. (2018). With respect to Model 1, kinematic boundary conditions at the top channel boundary imply zero velocities on that boundary. Specification of any other loads on that boundary would not be admissible in the

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model set up. Stresses on that boundary could be predicted and as such would be an outcome of the model and not an input to the model set up. With respect to Model 2, the top channel boundary is not a boundary of the model domain, and therefore, one cannot specify its load as a boundary condition. Consequently, the loads on the top-channel boundary will be a model prediction and not part of the model configuration. To conclude, none of the two model configurations considered by Marques et al. (2018) has the weight of the overlying rock as the sole load on the top-channel boundary.

[Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2018-92](https://doi.org/10.5194/se-2018-92), 2018.

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