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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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Reply to comment by E. Moulas J. P. Platt

In his first paragraph Moulas states "The 50km deflection calculated by Prof. Platt could be hypothetically observed as the result of unloading to a state with negligible tectonic overpressure".

This is incorrect. The elastic deflection of the upper plate results in a restoring force related to the bending moments in the deflected plate. This restoring force increases with the amount of deflection, and the deflection I calculated is such that the restoring

force is sufficient to balance the dynamic pressure in the channel. I proposed this because it is the only physically possible way to achieve force balance across the upper boundary of the channel. The fact that M18 did not include it in their model is the reason I criticized their boundary condition as unphysical. The reason the deflection is so large is because the dynamic pressure proposed by M18 is unrealistic. The rest of Moulas' first paragraph makes no sense.

In his second paragraph Moulas states "Naturally, one cannot predict the stress state in the first-type of models, as the overriding plate is outside of the model domain." This is precisely the problem with the approach taken by M18. Calculating flow and dynamic pressure in a channel that is isolated from its surroundings is completely pointless. The magnitude of the dynamic pressure in the channel is limited by the strength of the upper plate. The maximum possible value is determined by the flexural response, as this assumes the upper plate is strong enough to resist permanent deformation. Any flexural upwarp is unlikely to exceed a few kilometers at most, as otherwise it would have been detected by now from its topographic and gravity signature. A full calculation is needed to determine what magnitude and spatial extent of dynamic pressure is consistent with this limitation, but it is likely to be substantially less than 1.5 GPa. The calculations by Schmalholz in his comment are helpful in this regard. In actuality, however, the response of the upper plate is much more likely to involve permanent viscous and brittle deformation. As I pointed out in my response to Moulas' earlier review, several lines of evidence suggest that the upper plate is unlikely to be able to sustain differential stress in excess of 200 MPa, and this provides the most realistic limitation on the magnitude of the dynamic pressure.

Moulas' third paragraph is based entirely on the misconception discussed above. A model is supposed to be a simplified representation of the real world, allowing calculations that approximate the more complex response of the real system being studied. The model should be consistent with all physical laws, and produce results that can be tested against measurements on the real system. The model set-up by M18 does not conform with these important principles. In the real world, the load on the upper boundary of the subduction channel will be lithostatic, unless some other physical mechanism is present that can increase that load. Neither M18 nor Moulas have suggested any such mechanism.

With respect to the deformable walls model (model 2), M18 unfortunately did not describe the boundary conditions at all clearly, but careful examination of their Figure 8 indicates that the "unconstrained top boundary" refers to the mouth of the channel, and that the actual upper boundary of the model is fixed, as in model 1.

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