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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

F. Ornelas Marques

fomarques@fc.ul.pt

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REPLY TO PLATT'S COMMENT

F.O. Marques, N. Mandal, S. Ghosh, G. Ranalli, S. Bose

Abstract The points raised by Platt refer not to the formal correctness of our model, but rather to its relevance, given our assumptions and boundary conditions. Platt's main concern regards flexure, but his considerations, in our opinion, suffer from oversimplifications leading to unwarranted conclusions. A proper evaluation of the flexural effects of dynamic overpressure in channel flow would require a complete dynamic model in-

cluding realistic geometry and rheology, the knowledge of the elasticity and viscosity parameters, temperature, and mass transfer. At this stage, this is not available, neither to us nor to Platt. Consequently, the statement that the model results are "dramatically at variance with what we observe" is at worst unjustified, at best premature. On the contrary, the upward tapering model can explain several observations at the surface (cf. first paragraph of the Abstract in Marques et al., 2018a), and can help constrain the viscosity in the channel by keeping overpressure and outward flow within realistic values. What happens deep in a subduction zone can only be inferred, and that is why modelling is used to find possible explanations.

We thank Platt for his comments, because they give us the opportunity to clarify some critical and common questions raised when discussing tectonic/dynamic overpressure. Platt's comments reflect in great part the reasoning used by colleagues arguing against the hypothesis of high values of tectonic overpressure in subduction zones. In this reply we will contend that Platt's comments suffer from conceptual problems, and lack a quantitative analysis of the process of continental collision and tectonic overpressure development.

Regarding the three specific questions raised by Platt, we have the following considerations to offer: Upward tapering channel: It is correct to point out that the actual channel depicted in Fig. 1b of Marques et al. (2018a) tapers downward near its bottom; its overall shape, however, tapers upwards when regarded in its entirety. We therefore have chosen the shape of the model channel as shown in Fig. 1c of Marques et al. (2018a). The stated purpose of the model is to simplify geometry and rheology in order to analyse individually the effects of variations of single parameters. With the chosen boundary conditions, we obtain dynamic overpressure at relatively shallow levels with this geometry.

Boundary conditions: We state explicitly several times, including the Abstract, that no significant overpressure develops if the bottom of the channel is "open". This therefore excludes subduction channels with outflow into the sub-lithospheric mantle. As to collision-type channels, especially when both converging plates are very "hard", the possibility of a closed base is, at least, worth exploring, especially in view of the possible consequences of overpressure for palaeodepth determinations from metamorphic peak conditions. It is true that the footwall moves down with the lower plate, but we do not see how this invalidates the model shown in Fig. 2, even if it refers only to a transient stage.

Mechanical properties of the walls: We agree that the assumption of rigid walls is an important factor affecting the results, and that the results for viscous walls are relevant only in the absence of additional external forces. The interfaces between the channel and viscous walls were mechanically coherent, and they were not kinematically constrained to restrict normal flow across the channel boundaries. Platt's claim that we did not allow for any motion normal to the channel boundaries is not correct because viscous walls must flow according to Stokes' equation, and are therefore free to deform. The inclusion of more complex dynamics would require a completely different model. We have, however, considered the case of transpression in both Marques et al. (2018a, 2018b), which has significant effects on overpressure (Fig. 7 in Marques et al., 2018a), at least in the case of rigid walls.

A major point in Platt's comment is that the possible effects of overpressure on the flexural deformation of the upper plate may invalidate the results of the model. We cannot predict what these effects could be, but we suggest that neither can Platt, at least on the basis of his comments (no quantitative analysis presented). Several parameters govern the flexural effects of vertical loads: flexural rigidity, flexural parameter, wavelength of the load, and – in the case of viscoelasticity – the age of the load (cf. e. g. Turcotte & Schubert, 2002, pp. 119-125). For wavelengths short in comparison to the thickness of the plate, the load is substantially supported by the rigidity of the lithosphere (Turcotte & Schubert, op. cit., eqn 3-111 ff.), as the bending moment required for flexural deformation would be very large. Platt used an inappropriate example, where flexural deformation occurs at a plate scale, with wavelengths far exceeding

that applicable to the present case. The geometric scale of overpressure in channel flow bears no resemblance to the scale of the whole Himalayan mountain range. In the case of overpressure from below (laccolith formation), the maximum deflection depends on the fourth power of the distance between the "pinning points" of the upper plate (Turcotte & Schubert, op. cit., eqn 3-99). Furthermore, we specifically state that the generation of overpressure is a transient process. A proper evaluation of the flexural effects of channel flow would require a complete dynamic model including realistic rheology, temperature, and mass transfer. At this stage, this is not available, neither to us nor to Platt. Consequently, the statement that the model results are "dramatically at variance with what we observe" is at worst unjustified, at best premature.

Another major point in Platt's comment is the way dynamic pressure builds up in the upward tapering channel. Platt argues (cf. Abstract of his comment) that "As a result there will be no return flow, and excess pressure will not develop in the channel . . . excess pressure is maintained by continued corner flow ". This comment gives the impression that dynamic overpressure depends on return flow or corner flow. Pressure in the Stokes' equation depends on the Laplacian of the velocity, which means that it depends on the divergence of the gradient: it can be positive, negative or zero, giving rise to underpressure, overpressure, or zero pressure, depending upon the nature of velocity gradients. The terms return flow and corner flow should be avoided, because they bear no obvious relationship to the Laplacian of the velocity, and are not the sine qua non conditions to produce dynamic pressure.

Regarding Platt's detailed comments, we have the following considerations to offer: "The upward-tapering channel model proposed by Marques et al (2018a) has a "base" that forms part of the subducting footwall, and will therefore not close the channel." (cf. first sentence of the Abstract). We cannot see how Platt reaches such a conclusion. We actually do not know what is going on down below the Himalayas, but the seismic image we used for Fig. 1b in Marques et al. (2018a) shows an overall upward tapering channel. Therefore, this is the geometry we used for the modelling of tectonic overpressure. A simplistic downward tapering channel does not portray the complexity of a continental collision zone, which can change its geometry over time and space. Furthermore, making use of Platt's reasoning, a natural downward tapering channel can leak downwards, so hampering the development of significant overpressure.

"The excess (dynamic) pressures calculated from their model, which exceed lithostatic pressure by as much as 1.5 GPa, will cause elastic flexure of the upper plate, which will relieve the excess pressure. If the excess pressure is maintained by continued corner flow, flexure of the upper plate will lead to geologically unrealistic topographic and gravity anomalies." (cf. last sentence of the Abstract). This strong statement is given without a quantitative analysis of the problem. A careful reading of the reference given by Platt (Watts & Zhong, 2000), in particular their Fig. 5, shows the effects of Maxwell's Relaxation Time and wavelength on the behaviour of a lithospheric plate. Despite citing the op. cit., Platt does not take these important parameters into account in his comments. In fact, Platt does not either take into consideration the elastic thickness of the plates in his calculations or his discussion of the rigid plates we used in the numerical model. Platt directly relates the flexural deformation with pressure drop in the channel. However, the fundamental requirement for pressure drop is by increase of the overall volume of the channel, which is difficult to quantify or even qualitatively appreciate by invoking a simple mechanical model of wall bending. The local bending or inflation the way Platt imagines in terms of ballooning may cause space accommodations locally, instead of bulk distortion of the entire lithosphere (the mechanical aspect of this phenomenon has already been discussed above). Such local adjustment will conserve the channel volume, and thereby retain the tectonic overpressure produced in the deeper section.

"A more fundamental problem arises from the assumption that the footwall and hanging wall are rigid". As we discuss in Marques et al. (2018a; cf. point 5 of section "4.3 Comparison between model and nature"), and Marques et al. (2018b), we assume rigid walls given the age of both subducting and overriding plates. See further discussion

below.

"Marques et al (2018a) try to bypass this problem by allowing viscous shear in footwall and hanging wall". We certainly did not wish to bypass any problem, on the contrary we wanted to analyse the problem of different mechanical behaviour in the bounding walls.

". . . they do not allow for any motion normal to the channel boundaries.". This is not true; this comment gives the impression that Platt misread Marques et al. (2018a): (i) the shear flow partitioning, as pointed out by Platt, results not from any imposed boundary conditions at the channel walls (viscous walls do allow for any motion normal to the channel boundaries) but essentially due to the large viscosity contrast between the channel and its walls; (ii) we did allow for motion normal to the boundaries in the contractional (so-called transpressive) model (cf. Fig. 6 in Marques et al., 2018a) to investigate its effects on overpressure (cf. last paragraph of section "Boundary conditions and model set-up" and Marques et al., 2018b); (iii) the analysis of an expansional model (so-called transtensive, as in roll-back subduction) makes no sense given the existence, in the hanging wall, of the largest mountain belt and plateau on Earth.

". . . all they have done is widen the channel somewhat by incorporating part of the footwall and hanging wall into it.". This is certainly not the case, because the viscosities of wall and channel are not the same. What the model shows is that three to two orders of magnitude difference in viscosity between walls (higher viscosity) and channel filling (lower viscosity) is very similar to having rigid walls when analysing tectonic overpressure.

Finally, we have never stated that proposed exhumation mechanisms are "inadequate". We wished simply to point out that the development of overpressure is a serious possibility, at least in given tectonic situations. We are aware that the reconciliation of palaeotemperature and palaeopressure estimates is a potential problem, therefore we have discussed it in Marques et al. (2018b).

We would like to take this opportunity to correct a couple of misprints in equation (A1) of our paper. The term in parentheses on the l.h.s. is the material derivative of the velocity u and should of course read ∂u/∂t + uËŰâĹĞu; the term F on the r.h.s. is the body force per unit volume, i.e. gravity times density.

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