Platt commented on my review of his original comment on Marques et al. (2018), referred to as M2018. I disagree with the arguments of Platt, but I focus here only on the main points.

M2018 use a model with kinematic boundary conditions in which they set the velocities of the upper channel wall to zero. Platt argues that this boundary condition is non-physical. Actually, most corner and channel flow models applied in the Earth Sciences (e.g. flow in a mantle wedge, flow mélanges or return flow in a subduction channel; e.g. Cloos, 1982; England and Holland, 1979; Turcotte and Schubert, 2014) are based on such kinematic boundary condition. Usually, a corner/channel-parallel velocity is applied on one side and on the other side, typically the hanging wall, the velocities are zero. This boundary condition of zero velocity and the mechanical constraint of force balance require that the loads normal to the upper channel boundary, which are exerted by the channel, must be equal to the loads normal to the upper channel boundary exerted by the hanging wall. However, Platt argues that "The loads normal to the upper boundary of the channel consist of the pressure in the channel (lithostatic load + dynamic pressure) on one side, and the lithostatic load alone on the other side". This statement of Platt is wrong because it violates the force balance across the upper channel wall. Force balance requires that the load (more precisely the total stress normal to the boundary) of the hanging wall must balance the load of the channel (for slow deformation if inertial forces are neglected). It is, hence, not the boundary condition of M2018, which is non-physical, it is the assumption of Platt of a lithostatic load in the hanging wall which is non-physical in the context of the model of M2018. Therefore, I argue that Platt's argument is mechanically not sound, because the stress state he assumes violates the force balance across the upper channel wall.

I explain the force balance between channel and hanging wall for illustrative purposes with the simple elastic flexure model used by Platt. This flexural model quantifies the maximal deflection of an elastic beam, mimicking the resistance of the hanging wall, under the action of an external pressure, representing the dynamic pressure in the channel, acting normal to the initially straight and unstressed beam (Turcotte and Schubert, 2014). The maximal deflection of the beam is

$$w_{\max} = \frac{L^4 p}{D384} = \frac{L^4}{h^3}\frac{p\left(1-\nu^2\right)}{E}\frac{12}{384} \qquad (1)$$

where L is the length of the beam, p is the external pressure, D is the flexural rigidity of the beam, h is the effective elastic thickness of the beam, E is Young's modulus and ν is Poisson's ratio. Platt used L = 175 km, p = 1.5 GPa, E = 10^{11} Pa and ν = 0.25, and he assumed for the effective elastic thickness (Burov and Diament, 1995) of the beam a value of h = 20 km so that he obtained $w_{\max} \approx 50$ km (Figure 1A). Obviously, Platt's assumption of h = 20 km disagrees with the assumption of M2018 who assumed that the hanging wall is strong enough so that the deflection of the hanging wall under the action of a dynamic pressure is small compared to the length of the channel wall. Equation (1) shows the strong non-linear dependence of $w_{\max}$ on both L and h. If one uses h = 60 km then $w_{\max}$ < 2 km (Figure 1A). In Figure 1B I plot the corresponding deflected geometry of the beam for h = 20, 40 and 60 km using also the solution from Turcotte and Schubert (2014):

$$w(x) = w_{\max}\left(1-8\frac{x^2}{L^2}+16\frac{x^4}{L^4}\right) \qquad (2)$$

where x is the coordinate along the beam. For h = 60 km the deflection of ca. 2 km is very small compared to the length of the beam of 175 km (ca. two orders of magnitude). A deflected, and hence stressed, elastic beam with h = ca. 60 km and with a deflection of ca. 2 km has deviatoric stresses which statically balance the applied external load of p = 1.5 GPa. If one assumes that the hanging wall has sufficient strength, which would correspond for our simple example to the flexural resistance of a beam with h ≥ ca. 60 km, then one can approximate the upper channel boundary as straight and stationary, that is, the velocities are zero under the action of p = 1.5 GPa, as assumed by M2018. The mechanically correct description of the scenario considered by M2018 is a strong hanging wall with non-lithostatic stresses which statically balance the dynamic pressure of the channel. Therefore, the boundary conditions of M2018 are neither non-physical nor are the loads between channel and hanging wall unbalanced. The boundary condition of M2018 simply requires that the hanging wall is strong enough to balance the dynamic pressure with a small deflection of the channel wall. For h = 40 km the maximal deflection is ca. 6 km. How much a curved channel wall corresponding to a deflection of up to ca. 10 km affects the dynamic pressure in the channel should be quantified with corresponding 2D numerical simulations including an elastically deformable hanging wall.

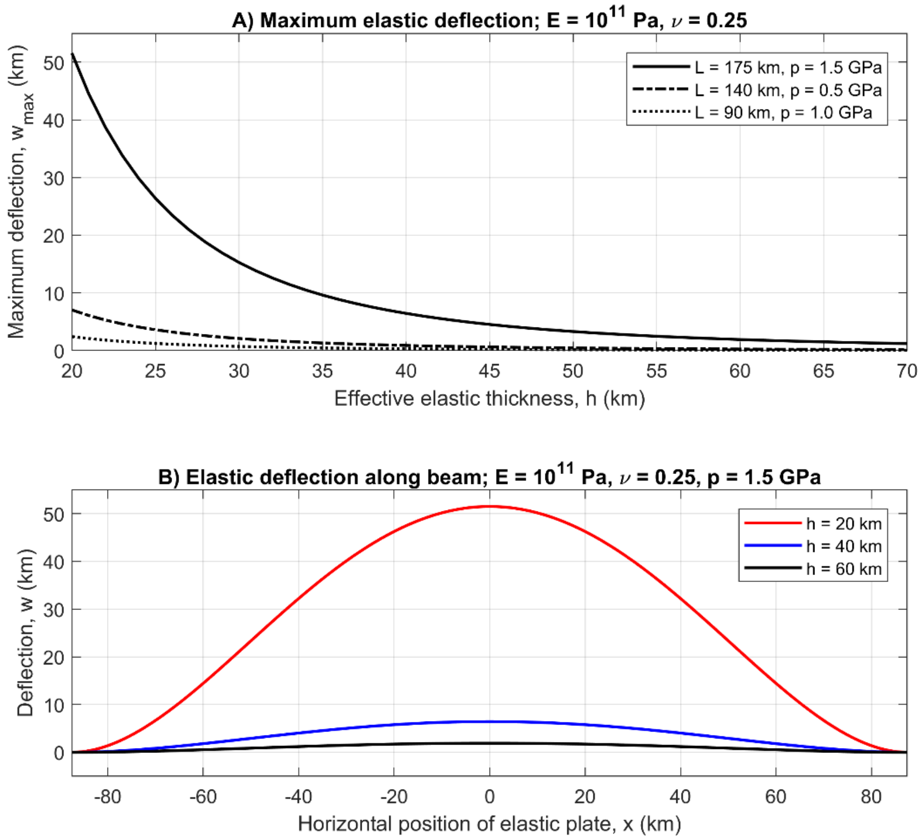

Figure 1. A) Maximum deflection, $w_{\max}$ (see equation 1), as function of effective elastic thickness, h. B) Variation of elastic deflection along a 175 km long beam. Maximum deflection (in A) occurs at the horizontal center (0 km).

A mechanically sound, and in my opinion justified, critical comment on the model of M2018 would be that they assume a strength of the hanging wall which might be too large for the situation in Tibet. Platt argues that estimates of dynamic pressure of M2018 are at least one order of magnitude too high, that is, dynamic pressure should be < 0.15 GPa. To test whether the order of magnitude of dynamic pressure suggested by M2018 is feasible, I consider p = 0.5 GPa, which is still a significant dynamic pressure, and reduce L = 175 km by 20% to L = 140 km (Figure 1A). The maximum deflection is then dramatically reduced (Figure 1A) and if we assume that values of h between 20 and 30 km for the hanging wall might be possible then the model of M2018 seems feasible with respect to the strength of the hanging wall. Therefore, I argue that the strong statement of Platt, that estimates of dynamic pressure of M2018 are at least one order of magnitude too high, is not justified.

The elastic beam model is not very suitable to quantify the mechanical resistance of the hanging wall because its deformation behavior in depths larger than 40 km is likely dominated by plastic and ductile deformation. Hence, its effective strength depends on the assumed flow law (feldspar or quartz dominated, wet or dry etc.), the temperature variation in the hanging wall and its geometry. However, I agree with Platt that the elastic beam model is a good model to check, to first-order, the feasibility of channel models with high dynamic pressure. M2018 expose themselves to critics of Platt because they consider a very long channel in which high dynamic pressure acts on the upper channel boundary over a length, L, of ca. 175 km. Equation (1) shows the extreme nonlinear dependence of $w_{\max}$ on L and h ($w_{\max} \sim L^4 / h^3$). Since I am interested whether lithospheric channel flow models in general could support dynamic pressures in the order of 1 GPa I discuss, based on equation (1), the feasible magnitudes of dynamic pressure in lithospheric channels. For example, M2018 used a channel with a base at 100 km depth but they did not perform models with a smaller channel height (the vertical distance between base and surface). If one considers Figure 1B of M2018 one could also consider a channel with a base at 70 km depth so that the upper channel wall exposed to high dynamic pressure of ca. 1 GPa would likely be ca. 85 km shorter (30 km / sin(20°)). For a beam with L = 90 km (175 km – 85 km) and p = 1 GPa the maximal deflection for h = 20 km is only ca. 2 km (Figure 1A). Therefore, reducing the channel height and, hence, the length of the channel wall exposed to dynamic pressures of ca. 1 GPa causes a dramatic decrease of the potential deflection of the hanging wall. The model of an elastic beam, representing the resistance of the hanging wall, shows that dynamic pressure in the order of 1 GPa can be easily balanced by channel walls with a length of several tens of kilometer, whereby the associated deflection of the channel wall is small and the required effective elastic thickness of the hanging wall is reasonable. 2D thermo-mechanical numerical simulations of a compressed lithosphere with viscoelastoplastic deformation behavior also support this conclusion and show that dynamic pressure between 0.5 and 1 GPa can develop in weak crustal-scale shear zones and that these dynamic pressures are balanced by surrounding rock units without tens of kilometers deflection of the shear zone walls (e.g. Schmalholz and Podladchikov, 2013). Furthermore, one should keep in mind that it is well established by host-inclusion studies of minerals that pressure on the mineral scale can deviate from the lithostatic pressure by magnitudes in the order of 1.5 GPa (e.g. Angel et al., 2015; and references therein). Whether such pressure deviations occur in nature on significantly larger scales is currently disputed, because the effective strength of polymineralic rock units at depth is still contentious. However, some field- and petrology-based studies provide strong support for dynamic pressure variations between ca. 0.5 and 2 GPa in natural rock units on the outcrop scale (Chu et al., 2017; Vrijmoed et al., 2009). Recently, Jamtveit et al. (2018) argued that petrological and geochronological data and corresponding field observations of meter-scale eclogite shear zones in granulites in Western Norway are best explained if a dynamic (or tectonic) pressure of ca. 0.5 GPa is considered.

Concerning point 3 of my review whether the model of M2018 with a viscously deforming hanging wall allows motion normal to the upper channel boundary: The authors of M2018 have already replied to Platt and they confirmed my evaluation that the comment of Platt, claiming that motion normal to the channel wall was not possible, is incorrect. I trust that the authors of M2018 have checked their model and boundary condition before their reply to Platt.

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