

Platt Discussion: Referee Responses

Responses to Referee 1.

I find the revised version of the comment by Platt now acceptable for publication. After the online discussions (which are in my opinion useful and interesting) related to the first version of the comment of Platt I am looking forward to the reply of Marques and co-authors. I have only a few comments to the revised comment of Platt.

Line 22-23, page 3: I find this sentence misleading. If the resistance of the plate becomes larger, then the corresponding deflection, due to a given normal load, becomes smaller. Therefore, an extremely small deflection can correspond to an extremely large resistance. The boundary condition of a straight channel wall with zero deflection applied by Marques et al. (2018) can be justified by two assumptions: (i) the deflection is small with respect to the length of the channel wall so that it is negligible. (ii) the straight channel wall corresponds to the elastically-deflected state implying that without dynamic pressure the upper channel wall would be curved.

Agreed. The confusion was caused by my use of the word “resistance”, which I was using to refer to the restoring forces created by the bending moments produced by the deflection. I have deleted the sentence and modified the text.

Note that the calculated deflection is not small (50 km over an up-dip length of 175 km), and it is improbable that the channel wall could have had that degree of curvature initially.

Line 2-11, page 5: This is a selective view on the topic of the strength of the lithosphere. There are many studies suggesting much higher shear stresses in a deforming continental lithosphere, especially when flexure is considered. Many studies on the strength of the lithosphere considering so-called yield-strength envelopes, based on flow laws from rock deformation experiments, exhibit one or more levels in the lithosphere with stresses corresponding to shear stresses significantly larger than 120 MPa (e.g. Burov, 2011, and references therein). For example, if strength in the hanging wall in depths larger than 40 km would be dominated by diabase then the stress could be significantly larger than 120 MPa (e.g. Burov, 2011, his figure 7). Furthermore, flow laws for dry anorthite indicate that stresses in the lower crust could be significantly larger than 120 MPa (e.g. Rybacki et al., 2006).

I agree that there are different views on the strength of the lithosphere, which largely reflect the fact that the lithosphere varies a lot in both composition and thermal structure. An intermediate to mafic lower crust dominated by feldspar, pyroxene, and garnet, with a low thermal gradient, can probably support >800 MPa differential stress near the Moho (e.g., Platt & Behr, GRL, 2011, Figure 2). But there is no evidence that the upper plate in the Himalayas had this composition. The velocity structure reported from the INDEPTH profile across southern Tibet suggests that the composition is typical of mid-crustal rocks (e.g., granite and metamorphic rocks with hydrous mineral assemblages) (Haines et al., Tectonics, 2003). As I point out in the text, at depths of 50 km or more in the Himalayas, these rocks were close to their melting point, and would have been very weak. I have added a few sentences to the text on this point.

Line 14-16, page 5: This suggestion that “the values of dynamic overpressure calculated by

M2018 are at least an order of magnitude too high” is based on the assumption that the rocks forming the hanging wall exhibit shear stresses smaller than ca. 120 MPa. Marques et al. (2018) are aware of the fact that the magnitude of the dynamic pressure depends on the effective strength of the hanging wall, and they write in their manuscript: “Ultimately, tectonic overpressure (TOP) can only exist if the channel walls are strong enough.”. The effective strength of the hanging wall is a key parameter controlling the magnitude of dynamic pressure in the model of Marques et al. (2018). I agree that a dynamic pressure of 1.5 GPa within large channel regions, as suggested by Marques et al. (2018), is very difficult to justify. However, a magnitude of the dynamic pressure of 0.3 to 0.5 GPa is a factor of 5 to 3 times smaller than the value of 1.5 GPa and I think that values between 0.3 and 0.5 GPa for dynamic pressure cannot be excluded based on currently available flow laws for the lower crust. Hence, I still think that the above “suggestion” that values of dynamic overpressure are “at least an order of magnitude too high” is also very difficult to justify.

The statement that “The effective strength of the hanging wall is a key parameter controlling the magnitude of dynamic pressure in the model of Marques et al. (2018)” is a bit too generous. By assuming a rigid upper boundary they effectively assign an infinite strength to the upper plate. Their model with “deformable walls” is misleading: what they have done is to incorporate higher viscosity layers on either side of the channel into the model domain, but they retain the fixed boundary conditions above and below the model domain. This model therefore fails to test the effect of limited strength in the hangingwall as a whole. Taras Gerya kindly provided me with a Matlab code that models the system described by M2018, but using a free upper boundary to the upper plate, instead of a fixed boundary to the channel. In this model the upper boundary of the channel is pushed upwards by the overpressure in the channel, creating a substantial topographic deflection of the free surface. The magnitude and dimensions of the region of dynamic overpressure is greatly reduced as a result. As discussed above, I would defend a strength limit of ~120 MPa for the upper plate, but I have softened the statement about the overpressure.

Responses to Referee 2.

Prof. Platt uses an analytical solution of elastic deformation in order to criticize the purely viscous model proposed by Marques et al (2018).

I didn’t criticize the viscous model as such, I criticized the use of a rigid boundary condition. For a model to have any applicability to the real world, the boundary conditions must correspond at least approximately to the constraints that the real world would impose. The assumption of a rigid upper boundary is equivalent to a statement that the upper plate of the Himalaya is infinitely strong. My point is that even if we neglect permanent deformation, the upper plate will respond elastically, and we have enough information to calculate that response.

I would like to highlight (see also my point P8) that the geometrical configuration used by Marques et al. (2018) approximates the one currently observed, and not an initial condition.

I discuss this issue under point P8

P.1 “Note that dynamic overpressure as used here is generated by flow in a viscous fluid, and differs in this respect from the more widely recognized concept of tectonic overpressure, which is related directly to deviatoric stress, and can exist in a static situation, with or without deformation”. This statement is confusing. The flow of a viscous fluid cannot be unrelated to the deviatoric stresses. By definition, the flow of viscous materials requires the presence of deviatoric stresses.

Yes, the flow of a viscous fluid requires deviatoric stress, but the dynamic pressure is not calculated from the deviatoric stresses themselves. It is calculated using the Navier-Stokes equations, which relate the pressure *gradient* to the deviatoric stress gradients. This differs from the definition of tectonic overpressure in the usual sense, where one of the principal stresses (usually the minimum) is assumed to be equal to the lithostatic load, and the overpressure is calculated as the difference between the mean stress and the principal stress in question, which by definition is the deviatoric stress in that plane.

P2. “Return flow in subduction channels has been proposed as a mechanism for exhuming high-pressure metamorphic rocks from deep in the subduction zone (e.g., Cloos, 1982). Possible drivers are buoyancy (e.g., England & Holland, 1979; Beaumont et al., 2009), topographic gradients (e.g., Beaumont et al., 2001), or dynamic overpressure (e.g., Gerya & Stockhert, 2002).” In the case of corner flow (Cloos, 1982), the return flow is independent of buoyancy stresses (Batchelor, 1967). In fact, the dynamic overpressure (difference from the lithostatic; also associated with deviatoric stresses) is responsible for the return flow. In other words, there is no corner flow s.s. without pressure deviations from the lithostatic. By contrast, the main driver for exhumation in the channel-flow model of England and Holland (England and Holland, 1979), is buoyancy. I would therefore recommend rephrasing of the related paragraph.

Agreed – the model of Cloos (1982) does involve dynamic overpressure, and I have modified the text to make that clear. But return flow, in the more general sense, may commonly involve several drivers, and the buoyancy of a low-density fluid in the subduction channel is one of them.

P3. “the dynamic overpressure is limited by the ability of the channel walls to contain it. If the walls deform, the pattern of flow will change, and the dynamic overpressure is likely to decrease.” The author has a point here, however one needs to model the time evolution of the wall deformation. For example, a system where the wall deflects in 10,000 years is different from a system where the wall deformation would take tens of millions of years to evolve. The specifics of the evolution would, in turn, depend on the particular mechanical response of the wall and the boundary conditions assumed. Therefore, without being more specific this point is rather weak.

I discuss the specifics later in the text.

P4. “The second problem is that they assume a fixed upper boundary to the subduction channel, which cannot be defended in geological terms, and leads to unrealistic conclusions”

I have stated my disagreement with this comment in my previous review. The author in one of his response comments suggested that a careful investigation of the boundary conditions of

Marques et al (2018) reveals that the wall is fixed. Based on the description of the model setup with deformable walls by Marques and co-workers, I find this statement misleading (i.e. in the models with deformable walls the walls are not fixed).

In most of the models the boundaries are clearly stated to be fixed. The model with “deformable walls” is misleading: what M2018 have done is to incorporate higher viscosity layers on either side of the channel into the model domain, but they retain the fixed boundary conditions above and below the model domain. This model therefore fails to test the effect of limited strength in the hangingwall as a whole.

P5. “A more fundamental problem concerns their use of a fixed upper boundary to the channel. It is true that fixed boundaries are commonly assumed in fluid mechanics problems, because the mechanical contrast between a low-viscosity fluid such as water and a steel pipe, for example, is so large that deformation of the boundaries can be neglected. In the case of the subduction channel modelled by M2018 in their Figure 2, the viscosity is orders of magnitude greater than that of water, and the viscous stresses are correspondingly larger.”

Fluid dynamics solutions are not restricted to water; in fact, it is pointless to use water as a reference. This is actually why fluid mechanics are successfully applied to structural geology and geodynamics problems (Pollard and Fletcher, 2005; Turcotte and Schubert, 2014). Fluid dynamics solutions depend on the viscosity ratios of different materials. Even when a rock has a viscosity much larger than that of water, it can still behave as a low-viscosity fluid compared to the rock that exhibits even higher viscosity (see for example Gerya, 2010, p. 245). Marques and co-workers used viscosity ratios differing by 2-3 orders of magnitude. When the viscosity of the wall is 3 orders of magnitude or larger than the viscosity of the convecting fluid, then, the deformation of the wall would be negligible. Importantly, even if the initial boundary is assumed perfectly straight, time integration of the mechanical solution allows for conclusions to be drawn on the deformation of the strong lid.

The widespread use of fixed boundary conditions does have historical roots in fluid dynamics models developed for air and water. In geological situations we should be a lot more careful. It is easy to “assume” viscosity contrasts of 3 orders of magnitude, but the rocks in the Himalayan subduction channel were pretty much the same as those outside it – mainly metasedimentary gneisses and schists. More critically, these rocks did not behave as Newtonian fluids; they deformed predominantly by dislocation creep (Law et al., 2013; Waters et al., 2018), which shows a power-law relationship between strain-rate and stress, with a stress exponent n usually taken to be between 3 and 5. The viscosity is therefore a function of the stress; for $n = 3$, doubling the stress results in a drop in viscosity by a factor of 4. In the model illustrated in Figure 2 of M2018, the shear strain rate in the upper part of the channel is $\sim 6 \times 10^{-14} \text{ s}^{-1}$ so if the viscosity is 10^{21} Pa s , the shear stress will be $\sim 60 \text{ MPa}$. If the channel exerts a dynamic overpressure of 1.5 GPa on the upper plate, the shear stress in the upper plate will increase to $\sim 1.5 \text{ GPa}$. For power law creep with $n = 3$, this will reduce the viscosity of the upper plate by a factor of 600, which makes a nonsense of the assumption of M2018 that it has a viscosity 3 orders of magnitude larger than that in the channel.

P6 “If a dynamic overpressure of 1.5 GPa is applied from below to the upper boundary of the channel, a physical mechanism is required that is capable of keeping the boundary fixed, and

M2018 give no indication what this might be.” This statement is not true. Marques and co-workers clearly state that this can occur if the walls are strong, so that the boundary would behave as if the lid were rigid. It is the high viscosity of the channel wall (that can build up large stresses) that is responsible for keeping the boundary nearly fixed.

The point that neither M2018 nor Moulas consider is that even if we assume the walls have a high enough viscosity to confine the overpressure, the load will still produce an elastic response. We know the elastic properties of rocks in general fairly well, and we have geophysical estimates of the effective elastic thickness in southern Tibet, so we can calculate that response. The results, as I show, are clearly incompatible with what we observe.

P7 “the only load acting downwards on the upper boundary is the lithostatic pressure” This statement cannot be true for a deforming lithosphere with topography and density changes. It’s close enough. Technically, it’s only true on horizontal length scales larger than the scales of uncompensated topographic and density contrasts, which given the low effective elastic thickness in the region, means a few tens of km at most (note that the Himalayas as a whole are supported by the elastic strength of the underthrust Indian plate, but that doesn’t affect our arguments about the upper plate). M2018 assume the upper plate is not deforming, but it could still sustain significant tectonic overpressure due to deviatoric stress. On that point M2018 have another problem, however: the shear stress exerted by the return flow in the channel tends to drag the upper plate up the dip of the subduction zone. Force balance then requires a tensile deviatoric normal stress in the upper plate parallel to the dip of the subduction zone, so the minimum principal stress will be ~ horizontal, and the tectonic overpressure will be negative. As a result, the load exerted by the upper plate will actually be less than lithostatic.

P8 “In the case of a subduction channel, the configuration can be approximated by the analysis for flexural doming above an igneous intrusion presented by Turcotte & Schubert (2002).” The applicability of this solution in the subduction channel is not entirely justified. An important assumption for the application of the solution of Turcotte and Schubert (2002) is that the initial condition is known. Firstly, the layers of rocks are assumed to be horizontal and secondly, the deflections are calculated from this initial stage. By contrast, the configuration of Marques and co-workers is not an initial condition. Their solution is meant to depict the current configuration that satisfies force balance. Therefore, the plate deflection evolution in the Marques et al (2018) viscous model must be integrated over time as it is commonly done in Geodynamic modelling of slow viscous flow e.g. Gerya (2010).

To be frank, I find this comment a bit silly. M2018 do not discuss the time evolution of their model, and because they assume the upper plate is rigid, it won’t change its geometry with time anyway. So there is no point to answer here.

P9. “Various lines of evidence suggest than an upper limit of ~120 MPa shear stress is reasonable for continental lithosphere in actively deforming regions”. These values for shear stress have no universal applicability. There are numerous models that use experimentally determined flow laws that would not agree with such a statement. Stresses on the order of 100MPa are the minimum required to support topography in mountainous regions only if the entire lithosphere is stressed in a uniform manner (average stresses). Clearly, this is highly

improbable since the presence of viscosity heterogeneities would result to regions in which the shear stress would be significantly higher or lower (Schmalholz et al., 2018, 2014).

I discuss this issue in some detail in my response to Referee 1. Mechanical data on dry diabase or granulite is not really relevant to a mountain range made up largely of mica schist and quartz-feldspar-biotite gneiss.

P10. “The model set-up by M2018 does not conform with these important principles ...”
As mentioned in my previous review, instead of comparing the results of the Marques et al. (2018) model with natural observations, Prof. Platt compares the results of his own elastic flexure model with natural observations. However, the assumptions lying behind the elastic flexure formula are different to those invoked by Marques and co-authors (2018) in their viscous-flow model.

It may be reasonable to assume Newtonian viscous flow when modeling a subduction channel. My discussion concerns the behaviour of the upper plate, which provides the upper boundary condition for the model. Real rocks do exhibit elastic behaviour, and this can coexist with a viscous response, as in Maxwell and Bingham viscoelasticity. The elastic response provides a useful way to test the conclusions of the channel flow model in this case: the model fails spectacularly.

Comment on Marques et al. (2018), Channel flow, tectonic overpressure, and exhumation of high-pressure rocks in the Greater Himalayas

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Abstract. The upward-tapering channel model proposed by Marques et al (2018) for the Himalayas has a “base” that forms part of the subducting footwall, and therefore does not close the channel. This configuration does not produce return flow, and no dynamic overpressure develops in the channel. The geometrical and kinematic configuration they actually use for their calculations differs from this, and is both geologically and mechanically improbable. In addition, the fixed upper boundary condition in their models is mechanically unrealistic, and inconsistent with geological and geophysical constraints from the Himalayan orogen. In reality, the dynamic pressures calculated from their model, which exceed lithostatic pressure by as much as 1.5 GPa, would cause elastic flexure or permanent deformation of the upper plate. I estimate that a flexural upwarp of 50 km of the upper plate would be required to balance forces, which would lead to geologically unrealistic topographic and gravity anomalies. The magnitude of the dynamic overpressure that could be confined is in fact limited by the shear strength of the upper plate in the Himalayas, which is likely to be <120 MPa.

Introduction

Marques et al (2018) (henceforth M2018) make a valuable contribution to the study of orogenic dynamics by highlighting the role of dynamic pressure associated with return flow in subduction channels. They calculate dynamic pressures that exceed lithostatic by 1.5 GPa or more in a large part of the channel, and suggest that the depths of metamorphism inferred from petrological data for Himalayan eclogites may therefore be overestimated by a factor of two. Before launching on this discussion, we need a couple of definitions. I will refer to the material in the subduction channel as a fluid, but we should bear in mind that in reality it is likely to be solid rock, deforming by some type of non-Newtonian creep. Second, I will use dynamic overpressure to refer to the difference ΔP between the dynamic pressure in the fluid and the lithostatic pressure P_L exerted by the weight of the overlying rock. $P_L = \int \rho(z)gz$, where ρ is density, and z is depth. Note that dynamic overpressure as used here is generated by viscous flow in the channel, and differs in this respect from the more widely recognized concept of tectonic overpressure, which is related directly to deviatoric stress, and can exist in a static situation, with or without deformation (Schmalholz et al., 2014; Gerya, 2015).

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Return flow in subduction channels has long been proposed as a mechanism for exhuming high-pressure metamorphic rocks from deep in the subduction zone. Possible drivers are buoyancy (e.g., England & Holland, 1979; Beaumont et al., 2009), topographic gradients (e.g., Beaumont et al., 2001), or dynamic overpressure (e.g., Cloos, 1982; Gerya & Stockhert, 2002). The first two mechanisms do not require the channel to be closed, but dynamic overpressure is most likely to develop if the subduction zone is closed at depth (Gerya, 2015). This can occur where the subducting slab meets the upper plate, so that downward flow in the subduction channel is prevented, and the fluid is forced back up along the upper side of the subduction channel (Panel A in Figure 1). This phenomenon is known in the fluid-mechanics community as corner flow. Corner flow is also thought to occur in the mantle wedge above the subducting slab (e.g., Spiegelman & McKenzie, 1987). Here the symmetry is reversed, and ΔP in the corner is negative, so that asthenospheric mantle flows from the back-arc towards the corner.

Corner flow can be analyzed by solving the Navier-Stokes equations for creeping incompressible flow:

$-\nabla p + \mu \nabla^2 \mathbf{v} + \rho \mathbf{g} = 0$. These relate the spatial gradient in pressure (p) to the Laplacian of the velocity (\mathbf{v}) and the body force in the viscous channel (μ is viscosity, \mathbf{g} is gravitational acceleration). The Laplacian, which comprises the second derivatives of velocity, is directly related to the stress gradients in the stress equilibrium equations, from which Navier-Stokes is derived.

In a subduction channel the viscous fluid is entrained by the down-going slab, but if the upper and lower plates converge, so as to close the channel, fluid is forced away from the slab at the resulting corner (indicated by the red dot in panels A and C in Figure 1). As a result, it experiences an abrupt change in stress, and the resulting steep stress gradients require correspondingly steep pressure gradients, as shown by Navier-Stokes. The pressure gradients result in a build-up of pressure near the corner, and this in turn drives the return flow along the upper boundary of the channel. Navier-Stokes does not predict unique solutions: the dynamic overpressure is limited by the ability of the channel walls to contain it. If the walls deform, the pattern of flow will change, and the dynamic overpressure is likely to decrease.

The analysis by M2018 suffers from some serious problems, which largely undermine their conclusions. These problems are first, there is a clear conflict between the geological configuration they use to justify their model, and the configuration they actually use. The second problem is that they assume a fixed upper boundary to the subduction channel, which cannot be defended in geological terms, and leads to unrealistic conclusions. These problems are discussed in more detail below.

Geological configuration

M2018 base their model on the present-day Himalayan orogen, which they interpret in terms of a subduction channel with a trapezoidal geometry produced by an irregular footwall, with features that they describe in terms of a ramp and flat geometry, as illustrated in Figure 1 of their paper. M2018 regard the channel as being closed off by a "base" (see panel B in Figure 1 of this paper), which is clearly part of the footwall. The base is therefore part of the down-going Indian plate, and will move with the footwall at least as fast as the fluid in the subduction channel. The resulting configuration is transient; the

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base will not obstruct the downward flow of the fluid, and will therefore not lead to return flow. The fluid will move down along with the footwall and the base, and because the fluid in the upper part of the channel moves more slowly than the base, ΔP will be negative where the base meets the upper plate (see panel B in Figure 1). This situation is quite different from the geometrical and kinematic configuration they actually use in the model (panel C Figure 1). Although Marques et al (2018) do not explicitly state the boundary conditions used for the base, it is clear from their model results that it is fixed with respect to the upper plate. This results in an abrupt change in the boundary conditions at the point marked with a red dot in panel C. This is the “corner” that leads to the positive dynamic overpressure and the return flow. This configuration does not resemble that in the present-day Himalaya. No present-day subduction zone has this configuration, and there is no evidence that it existed in the Himalayan subduction zone in the past. It is geologically and mechanically highly improbable, and does not provide a valid basis for statements about Himalayan orogeny or metamorphism.

Boundary conditions

A more fundamental problem concerns their use of a fixed upper boundary to the channel. It is true that fixed boundaries are commonly assumed in fluid mechanics problems, because the mechanical contrast between a low-viscosity fluid such as water and a steel pipe, for example, is so large that deformation of the boundaries can be neglected. In the case of the subduction channel modelled by M2018 in their Figure 2, the viscosity is 24 orders of magnitude greater than that of water, and the viscous stresses are correspondingly larger. If a dynamic overpressure of 1.5 GPa is applied from below to the upper boundary of the channel, a physical mechanism is required that is capable of keeping the boundary fixed, and M2018 give no indication what this might be. In the absence of such a mechanism, the only load acting downwards on the upper boundary is the lithostatic pressure. The forces are then unbalanced across the boundary, and Newton’s laws of motion dictate that the upper plate in the Himalayas will accelerate upwards. We therefore need to discuss what mechanisms could maintain a fixed upper boundary to the channel, and whether these are geologically and mechanically reasonable.

In the real world, how can we achieve force balance on the upper boundary? The implication of a fixed boundary is that the upper plate is effectively infinitely rigid. If we accept for the moment the possibility that the upper plate is strong enough to resist permanent deformation, the upward load of 1.5 GPa will still produce an elastic response in the upper plate. An elastic plate subject to a normal load experiences an elastic deflection. The deflection produces bending moments in the plate, which counter the torque produced by the load, so the deflection increases until the load is balanced. To put this into perspective, consider the effect of the downward load of the Himalayan mountain range (5 km high on average along the crest), which amounts to ~135 MPa. It has long been established that this load produces a flexural downwarp of the underthrusting Indian plate of several km (Karner & Watts, 1983). Flexural downwarps of similar magnitude have also been documented in front of many other mountain belts, beneath ocean island volcanoes such as Hawaii, and along major transform faults (e.g., Watts & Zhong, 2000). In the case of a subduction channel, the configuration can be approximated by the analysis for flexural doming above an igneous intrusion presented by Turcotte & Schubert (2002). In this analysis, the roof of the intrusion is flexed up by magmatic pressure that exceeds lithostatic. The maximum deflection w is given by:

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if there is no deflection, there is no resistance.

$w = \frac{pL^4}{384D}$, where p in our case is the dynamic overpressure (total pressure less lithostatic), L is the distance along the upper plate boundary over which this pressure is applied, and D is the flexural rigidity. D is given by:

$D = \frac{Eh^3}{12(1-\nu)}$, where E is Young's modulus, h is the effective elastic thickness of the upper plate, and ν is Poisson's ratio. I

5 estimate the following values, based on Figure 2A from M2018, for the region between 40 and 100 km depth in the subduction zone:

$L = 175$ km;

$p = 1.5$ GPa averaged over L . For the mechanical parameters, I have taken the following values from Jordan and Watts (2005) for the upper plate:

$E = 10^{11}$ Pa,

10 $h = 20$ km (Jordan and Watts give a range from 0 – 20 km for the effective elastic thickness in southern Tibet, so I have

~~chosen the upper limit, which minimizes the deflection~~),

$\nu = 0.25$.

The predicted deflection is 50 km: this is what is required to produce a restoring force equal to the upward load of 1.5 GPa predicted by M2018. The deflection is so large that it violates one of the assumptions of the analysis, that w is small

15 compared to L . The analysis does not take into account the ~~apered~~ geometry of the upper plate (which will increase the deflection), and it is sensitive to the values chosen for E and h . But it is sufficient to demonstrate that a dynamic overpressure of 1.5 GPa in the Himalayan subduction zone is geologically unsustainable. No flexural upwarp of ~50 km

amplitude has been detected in southern Tibet. To achieve a more reasonable value for the deflection (say 2 km) we would need either to choose a value of 60 km for h , or to reduce the dynamic overpressure to <60 MPa. A value of 60 km for the

20 effective elastic thickness is characteristic of the Indian plate, which is made up of granulite facies crustal rocks overlying thick and cold lithospheric mantle, but it is quite outside the range of values found for Tibet and the upper plate of the Himalayas.

effective elastic thickness is characteristic of the Indian plate, which is made up of granulite facies crustal rocks overlying thick and cold lithospheric mantle, but it is quite outside the range of values found for Tibet and the upper plate of the Himalayas.

Deformable walls

25 In practice, the rocks in the upper plate of the Himalayas are ~~likely to deform permanently~~ if subjected to significant dynamic overpressure. M2018 recognize that some permanent deformation is likely, and they attempt to address this with their deformable walls model. This section of their paper is very difficult to follow, as they do not define the thickness or

geometry of the deformable walls, and their description of the boundary conditions is confusing and ambiguous. It appears that they have incorporated a layer of relatively high viscosity material into the model ~~domain~~, above and below the channel.

30 The model ~~domain~~ as a whole still has fixed upper and lower boundaries, however, so the system behaves in much the same way as the model without deformable walls, and the predicted dynamic overpressure is almost identical. ~~This model~~

~~therefore fails to test the effect of deformation in the upper plate as a whole~~.

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Permanent deformation in the upper plate

In the real geological situation the dynamic overpressure in the channel will be limited by the brittle or plastic strength of the upper plate. Various lines of evidence suggest than an upper limit of ~120 MPa shear stress is reasonable for continental lithosphere in actively deforming regions (e.g., England & Molnar, 1991; Behr & Platt, 2014), and this is consistent with values calculated from experimental rock mechanics data (e.g., Platt & Behr, 2011, [Figure 1](#)). [Cratonic lithosphere with an anhydrous feldspar-dominated lower crust can support significantly higher stresses \(see Platt & Behr, 2011, Figure 2\) but there is no evidence that the upper plate in the Himalayas ever had this composition. The geological evidence is that it consists of a variety of sedimentary and metamorphic rocks, minor amounts of granite, and serpentinite, and that it has a complicated internal structure, cut by abundant faults: reverse, normal and strike-slip. This is supported by the velocity structure for southern Tibet, which is typical of mid-crustal rocks \(e.g., granite and metamorphic rocks with hydrous mineral assemblages\) \(Haines et al., 2003\).](#) Differential stresses inferred from dynamically recrystallized grain sizes in quartz range up to 28 MPa [near the Main Central thrust](#) (Law et al., 2013), [and 47 MPa close to the South Tibetan detachment](#) (Waters et al., 2018). The thermal gradient is high, and the lower part of the very thick crust in this region is likely to be close to the solidus. Wet (but solid) granitic rocks at 750°C deform readily at a differential stress of 1 MPa, with an effective viscosity ~2x10¹⁸ Pa s (Platt 2015). The values for the effective elastic thickness of the lithosphere calculated by Jordan & Watts (2005) imply that the lithosphere as a whole is unable to sustain loads of more than a few tens of MPa. A full analysis of the response of the upper plate is beyond the scope of this discussion, but it is unlikely that it could confine a dynamic overpressure in the channel greater than the shear strength of the material (Schmalholz et al., 2014). The channel and upper plate will therefore deform and change shape, invalidating the model geometry used by M2018, [modifying the pattern of flow in the channel, and reducing the dynamic overpressure.](#)

The geological and geophysical evidence therefore [suggests that the upper plate of the Himalayan orogen lacks the strength required to confine dynamic overpressure with the magnitude and spatial distribution calculated by M2018. The observable limits on the both the elastic and permanent deformational responses suggest that their calculated values of dynamic overpressure are substantially too high, and do not justify the conclusions they draw about the depths at which Himalayan eclogites were metamorphosed.](#)

Concluding remarks

The problems I have identified with this study raise questions about the purpose and methodology of this type of modeling. A good model is 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. [For a model to have any applicability to the real world, the boundary conditions must correspond at least approximately to the constraints that the real world would impose.](#) The model set-up by M2018 does not conform with these important principles. They presented their model as a calculation of the dynamic

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overpressure in a real subduction channel in the Himalayas, and they draw conclusions from it about Himalayan metamorphism. Their representation of the geometry and kinematics of the subduction channel bears so little resemblance to the real system, however, that the model predictions have to be regarded as completely unreliable. In addition, the upper boundary condition for their model is geologically and mechanically unrealistic, and fails to allow for the response of the upper plate to the enormous values of dynamic overpressure they predict. As a result, these values are unlikely to have any relevance to deformational or metamorphic processes in the Himalayas.

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Figure Caption

- Figure 1. A) Downward tapering subduction channel illustrating a configuration that can lead to corner flow and positive dynamic overpressure (ΔP). B) Geometrical and kinematic configuration of the Himalayan subduction zone as described
25 by Marques et al. (2018). The base of the channel moves with the lower plate, and ΔP is negative. C) Configuration used for calculations in the model by Marques et al. (2018). The base is attached to the upper plate.

