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

Response to Referees.

Referee 1 (Schmalholz). First Review.

Comment: Platt (2018) questions the main results of Marques et al. (2018), referred to as M2018 in the following, who use a two-dimensional numerical linear-viscous flow model to quantify magnitudes of dynamic pressure in a trapezoidal model domain. A main comment of Platt (2018) is: "I suggest that their estimates of dynamic pressure are at least one order of magnitude too high". This statement, like essentially all other statements in Platt (2018), is purely speculative and not substantiated by any mechanical calculation or alternative numerical model.

Response: I have added an analysis of the flexural response of the upper plate to an unbalanced load of 1.5 GPa, which predicts a 50 km flexural upwarp; at least an order of magnitude larger than anything that is observed. I have also added discussion of the likely strength of the upper plate, which is < 120 MPa shear stress. This also limits the ability of the upper plate to confine the dynamic pressure.

Comment: In point 1 Platt (2018) states: "Whatever the details of the channel geometry, it must ultimately always taper downwards if it is to produce the corner-flow effect". M2018 show with their numerical simulations that return flow is generated in their model, which has an "upward-tapering" geometry. The results of M2018, hence, falsify the above statement of Platt (2018). M2018 never use the term "corner flow", but speak of "channel flow". Corner flow models commonly consider flow around a single corner. With respect to geometry, the trapezoidal model geometry of M2018 is more similar to a circulating cell model (e.g. Pollard and Fletcher, 2005; their figure 10.24).

Response: I have deleted my criticism of the description of the channel in terms of an "upward-tapering" geometry. I discuss the use of the term "corner flow", adding a simple description of the physics of this process, and justifying my use of this term to describe the model of M2018.

Comment: In point 2 Platt (2018) states that the horizontal base of the model in M2018 does not move horizontally which does not fit the tectonic situation in which the Indian lower-crust and mantle lithosphere is underthrusting Tibet and hence the "base" of the Greater Himalayan Sequence should have a horizontal velocity component. This is a fair comment. However, Platt (2018) does not make any prediction about how a horizontally moving base would affect the results of M2018. In the model of M2018 there is a velocity singularity at the left edge of the model base and a model with a horizontally moving base can have a velocity singularity at the right edge of the base. The consequences of such different boundary condition have to be calculated with a corresponding numerical simulation in order to quantify the impact on the results of M2018.

Response: I have added a more detailed discussion of the geometrical and kinematic configuration described by M2018, and how it differs from the configuration that they actually use, with the help of a figure. I hope that my revised discussion adds clarity to this important but potentially confusing issue. I don't think it is appropriate that I should be asked to carry out a full numerical simulation to justify this discussion, but in the revised version I point out that simply by inspection it is easy to see that the two configurations will produce dynamic pressures that are of opposite sign.

Comment: Point 3: The statement of Platt (2018) that M2018 "do not allow for any motion normal to the channel boundaries" is, to the best of my knowledge, not correct. M2018 also show results for which the material above and below the channel can deform viscously. M2018 state: "This model allows for both channel walls to deform viscously, thus raising the question of how much overpressure they can retain inside the channel". Based on the description of the boundary conditions in section 3.4 of M2018, I conclude that this model allows for motion normal to the upper channel boundary.

Response: The section in M2018 on deformable walls is unfortunately very difficult to follow, as they do not define the thickness or geometry of the 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, above and below the channel. The model as a whole still has fixed upper and lower boundaries, however, so the system behaves in much the same way as the model without the deformable walls, and the predicted dynamic pressure is almost identical. I discuss this point in the revised version.

Comment: The paragraph on page 2 from lines 6 to 18 in Platt (2018) includes mainly speculative "should-would-could" arguments, which are also mechanically unsound. For example, Platt (2018) argues that "an unbalanced upward load of 1.5 GPa should cause a substantial flexural upwarp of the upper plate, possibly tens of km in amplitude, producing a major topographic and gravity anomaly". It is not logical why there should be an "unbalanced upward load" in a mechanical model, which is based on the equations of force balance. The dynamic pressure of 1.5 GPa is not an "unbalanced load"; this dynamic pressure and the associated pressure gradient is responsible for "pushing" the viscous material upwards, against gravity and against the downward direction of the applied boundary velocity.

Response: This comment addresses my fundamental disagreement with M2018 and with the referees. 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 modeled 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 pressure 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. I argue that the only mechanism that can balance forces across the boundary is the flexural response of the upper plate. I have added a section to the revised version that explains this point in detail.

Comment: Platt (2018) further argues that "given that the material in the subduction channel is incompressible, even a small amount of flexural displacement would be enough to relieve the dynamic pressure". Indeed, it is well established that the dynamic pressure depends on the strength of the channel walls and dynamic pressure decreases when channel walls get weaker and, hence, displace more (e.g. Mancktelow, 2008). Such pressure relieve has been quantified with numerical models in several studies (e.g. Mancktelow, 2008) and is also mentioned in M2018 in section 1.2. M2018 report significant dynamic pressure also for models in which the viscosity of the channel was 100 to 1000 times smaller than the viscosity of the material bounding the channel (their section 3.4 and their figure 8). Therefore, the elastic flexural displacement, mentioned by Platt (2018), has to be calculated with an adequate model in order to test whether and for what conditions elastic flexure causes a significant pressure relieve.

Response: I have deleted from my comment the discussion of the possible response of the channel to deformation of the hangingwall.

Comment: Page 2, lines 23-24. The statement of Platt (2018), "I suggest that their estimates of dynamic pressure are at least one order of magnitude too high", is not substantiated and not quantified by a mechanical calculation or model. I recommend to calculate dynamic pressure and not to suggest it.

Response: See my response to the referee's first comment.

*Minor comment*s. I have deleted all the sections of my Comment concerning petrological issues, except to point out that the model of M2018 does not provide an adequate basis to disccuss Himalayan metamorphism.

Referee 1 (Schmalholz). Second Review.

Comment: 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.

Response: It is actually a bit odd to say that my argument is unsound because it violates force balance, when my argument was that the boundary condition in M2018 violates force balance! As noted above, if a dynamic pressure 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. I argue that the only mechanism that can balance forces of this magnitude across the boundary is the flexural response of the upper plate, but for this to produce a force that could balance the dynamic pressure, there has to be a finite deflection. The only other possible response is an upward acceleration of the upper plate, which is inconsistent with it being fixed. Hence my argument that the boundary condition is unphysical. I have tried to clarify these points in my revised Comment.

Comment: Schmalholz has added an extensive discussion of the flexural response of the upper plate, and has proposed situations that could reduce the amount of flexure to reasonable values. He concludes "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."

Response: This section is an implicit recognition that my suggestion of a flexural response to the dynamic pressure was neither speculative nor mechanically unsound, as Schmalholz indicated in his initial review. I agree that the analysis is sensitive to the values taken for *L* and *h*, but I note that the value for *L* I used was measured directly from Figure 2 of M18, being the distance along the upper boundary of the channel over which the dynamic pressure exceeds 1.5 GPa, and my value for *h* was taken from Jordan & Watts (2005), who determined the effective elastic thickness over the whole region from topography and Bouguer gravity data. 20 km is the maximum value for southern Tibet: they give the range as 0-20 km. To get a 60 km value for *h* we would need to go into the Indian shield, which is composed of dry granulite facies rocks overlying cold lithospheric mantle. I agree that the precise amount of the elastic deflection is open to discussion, but using the values I determined for *L* and *h*, the dynamic pressure would have to be reduced to 60 MPa to get a flexural deflection of 2 km, which might be small enough to escape detection. I elaborate on this point in my revised Comment.

Comment: The next section of Schmalholz's comment starting "The elastic beam model is not very suitable to quantify the mechanical resistance of the hanging wall" is mainly concerned with possible modifications to the geometry of the model of M2018 that could reduce the flexural response to reasonable levels. It then continues to consider processes in shear zones and at the scale of individual mineral grains that could produce some sort of overpressure.

Response: I agree with some of these points, and disagree with others, but this does not affect my Comment, which is concerned with the model presented by M2018. I would note that M2018 do not even consider a flexural response, so we cannot use that possibility to justify their boundary condition, which assumes that the upper boundary is fixed.

Comment: 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.

Response: As pointed out above, this section of M2018 is very confusing and ambiguous. My assessment is that the authors of M2018 have simply dismissed my criticisms without evaluating them, and that their deformable walls model has the same non-physical boundary condition as the others. I have discussed this point as clearly as I can in the revised version.

Referee 2 (Moulas). First review.

Comment: The comment posted by Prof. Platt (hereafter P18) highlights some points of the model proposed by Marques and co-workers (hereafter M18) in their publication entitled "Channel flow, tectonic overpressure, and exhumation of high-pressure rocks in the Greater Himalayas" in a very critical manner. To the author's opinion, the most essential criticism of P18 on M18 model is the model configuration. Based on P18, the contact between the subduction channel and its overriding plate, as presented in the model of M18, is "unphysical" and it leads to erroneous predictions of tectonic overpressure (TOP). The characterization "unphysical" and the subsequent arguments used by P18 are based on erroneous assumptions that lead to unphysical conclusions, and are therefore unjustified.

Response: I have addressed these issues in my responses to Schmalholz's comments, including the points raised by Moulas about the model with deformable walls.

Comment: Following P18, the deflection of the overriding plate is a consequence of having "unbalanced loads" in the channel. This criticism by P18 reveals a misconception of P18 regarding force balance in Stokes' equations. The model of M18 satisfies force balance everywhere within the model.

Response: My criticism was that the forces are unbalanced across the upper boundary, not within the channel.

Comment: One cannot make predictions of the magnitude of the applied stresses in regions outside the model domain.

Response: M2018 presented their model as a calculation of the dynamic pressure in a real subduction channel in the Himalayas, and they draw conclusions from it about Himalayan metamorphism. They cannot restrict their analysis to the subduction channel and pretend that the rest of the universe doesn't exist. We have to ask whether the upper boundary condition for their model is consistent with what we know about the tectonic setting. I make the case that it is not.

Comment: Platt argued that the TOP in the viscous channel is unbalanced since the overriding plate experiences lithostatic pressure. The last statement clearly reveals a mechanical misconception, i.e. it was the implicit assumption of P18 that pressure is lithostatic in the overriding plate. The last statement is mechanically unfeasible and violates force balance. In other words, there is no moment in time where there would have been significant TOP in the channel and lithostatic stress in the channel wall. Therefore, if there is a significant TOP in the channel then one needs to solve for the state of stress outside of the channel in order to have any meaningful stress estimates. In summary, the large values of TOP predicted by M18 are just the outcome of model inputs regarding the geometry, the specific rheology, the overall boundary conditions etc. How appropriate are these estimates needs to be verified and quantified. Without specific information on the stress distribution on the models of M18, one cannot judge how realistic these results are with respect to their stress magnitudes and the strength that they imply for the overriding plate.

Response: In the absence of any tectonic mechanism to increase stress in the upper plate, it is reasonable to assume that the pressure is lithostatic. M2018 make no suggestion about this, and neither does Moulas. Moulas' comment that the situation is mechanically unfeasible and violates force balance is precisely my criticism of M2018. My suggestion of a flexural response of the upper plate to the applied load was an attempt to find a solution, and to discover whether their estimates of dynamic pressure are appropriate. I conclude that they are not. I would also argue that it is the responsibility of the authors of M2018 to find out how realistic their results are and what the implications are for the strength of the upper plate. In the face of considerable resistance, I have been attempting to do this for them.

Specific comments.

P1-l.15-25, M18 did not state that they have a typical corner-flow model. Therefore, there is no justification for the suggestions of P18 regarding the tapering angles of the models of M18.

Response: It is a corner flow model. The channel has to close downward, and M18 state this. I have removed my comments on the taper from my revised comment.

P1-l.28. There is no specific reason on why the downward velocity (of the footwall ramp) must be exactly the same as the one of the plate especially when rheological boundaries are considered. Perhaps, having it perfectly immobile like in the case of M18 may be an exaggeration.

Response: M18 state that the underthrust plate is rigid. The footwall ramp is part of the underthrust plate, so it must move with it. M18 make no reference to "rheological boundaries". They take a feature that they describe as part of the lower plate, and give it the same velocity as the upper plate. That isn't an exaggeration: it's simply wrong.

P2-l.3-5 In their paper, M18 clearly state that they consider also the case of deformable walls therefore all this section is not justified.

P2.l.6-24 All this part is speculative and based on erroneous implicit assumptions. i.e. there is no reason why the load must be unbalanced. Therefore, all the arguments that follow (e.g. about unrealistically large flexures etc.) are based on faulty assumptions.

Responses: I have addressed both these issues in my responses to Schmalholz's comments.

With respect to Moulas' remaining comments, I have removed the sections in question from the revised version.

Referee 2 (Moulas). Second review.

Much of this review is so confused that I have difficulty in formulating responses. I have extracted sentences that I understand, and responded to them as best I can.

Comment: The 50km deflection calculated by Prof. Platt could be hypothetically observed as the result of unloading to a state with negligible tectonic overpressure (TOP).

Response: 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.

Comment: Naturally, one cannot predict the stress state in the first-type of models, as the overriding plate is outside of the model domain.

Response: 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.

Comment: 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 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.

Response: This is an attempt to defend the indefensible. If you set up a model that predicts a 1.5 GPa pressure in the crust in excess of lithostatic, without having a physical mechanism to confine that pressure, then it's a bad model.

All these points are addressed in the revised version of my Comment.

Revised version with markup:

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) has a "base" that forms part of the subducting footwall, and will therefore not close the channel. As a result there will be no return flow, and no dynamic overpressure will develop in the channel. The fixed upper boundary condition in their models violates force balance and is unphysical. 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 the orogenic dynamics by high-lighting the role of dynamic pressure associated with return flow in subduction channels. 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. $P_L = \int \rho(z) g dz$, where ρ is density, and z is depth. 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 (Schmalholz et al., 2014; Gerya, 2015).

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). The first two mechanisms do not require the channel to be closed, but dynamic overpressure is most likely

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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 Napier-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. The first problem is that 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 non-physical 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 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. This is quite different from the geometrical and kinematic configuration they use in the model (panel C in the figure). Although Marques et al (2018) do not explicitly state the

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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.

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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. A fixed upper boundary is therefore non-physical.

In the real world, how can we achieve force balance on the upper boundary? A load of 1.5 GPa is likely to cause permanent deformation in the hanging wall; in the absence of such deformation, the upper plate should flex elastically. An elastic plate subjected to a normal load experiences an elastic deflection. The deflection is resisted by bending moments in the plate, which increase with the deflection until the load is balanced. The resistance scales with the deflection: if there is no deflection, there is no resistance. To put this into perspective, consider the effect of the 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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Deleted: arises from the assumption that the footwall and hanging wall are rigid. Marques et al (2018) try to bypass this problem by allowing viscous shear in footwall and hanging wall, but they do not allow for any motion normal to the channel boundaries. This restriction means that the deformation they allow has virtually no effect on the dynamics of the channel, as their results demonstrate; in effect, all they have done is widen the channel somewhat by incorporating part of the footwall and hanging wall into it. ... [2]

$$w = \frac{pL^4}{384D},$$

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 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,

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

$\nu = 0.25$.

The predicted deflection is 50 km. This is so large that it violates one of the assumptions of the analysis, that w is small compared to L. The analysis does not take into account the tapering geometry of the upper plate (which will increase the flexural 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. Lower values for the flexural upwarp could be obtained with lower values and spatial extents of the dynamic overpressure, as shown by Schmalholz in the Discussion session. The values I have used were taken directly from M2018.

Deformable walls

In practice, the rocks in the upper plate of the Himalayas are more likely to deform by brittle and plastic mechanisms if subjected to a load of this magnitude. 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, above and below the channel. The model as a whole still has fixed upper and lower boundaries, however, so the system behaves in much the same way as the model without the deformable walls, and the predicted dynamic overpressure is almost identical. The problem with boundary conditions discussed in the previous section remains unchanged, and little of value can be inferred from this model.

Anelastic deformation in the upper plate

It is likely that the dynamic overpressure in the channel will be limited by the brittle or plastic yield 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). The upper plate in the Himalayas consists of a variety of sedimentary and metamorphic rocks, minor amounts of granite, and serpentinite. It has a complicated internal structure, and is cut by abundant faults: reverse, normal and strike-slip. Differential stresses inferred from dynamically recrystallized grain sizes in quartz range up to 28 MPa (Law et al., 2013). 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, with very low strength. The effective elastic thickness of the lithosphere calculated from Bouguer gravity and topography data is in the range 0-20 km (Jordan & Watts (2005), implying that the lithosphere is unable to sustain loads of more than a few tens of MPa. A full analysis of the state of stress in 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).

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. The model set-up by M2018 does not conform with these important principles. They presented their model as a calculation of the dynamic 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. The boundary conditions for the model are non-physical, and fail to allow for the response of the upper plate to the dynamic overpressure. As a result, the predicted magnitude of the dynamic overpressure is likely to be at least an order of magnitude too high.

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Marques et al (2018) proposed that high values of values of dynamic pressure could explain the high pressures determined by petrological methods from blueschists and eclogites, and hence get around the problem of how they are exhumed. A variety of mechanisms have been proposed to explain the exhumation of these rocks, most of which are based on some type of corner flow circulation in the subduction zone. Marques et al (2018) did not explain why they find these mechanisms inadequate. A point worth making is that if petrologically determined depths of burial are too high, as a result of the dynamic pressure effect, then depth/temperature ratios calculated from these rocks would also be too high, since the temperature determination would not be affected. Yet a review of HP and UHP metamorphism by Penniston-Dorland et al. (2015) suggests that in fact petrologically determined depth/temperature ratios are significantly lower than those calculated in numerical models of subduction zones.

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Figure Caption

Figure 1. A) Downward tapering subduction channel illustrating the configuration that can lead to corner flow and positive dynamic overpressure (ΔP). B) Geometrical and kinematic configuration of the Himalayan subduction zone as described 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.