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Discussions

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 excess pressure will not develop in the channel. 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.

Marques et al (2018) make a valuable contribution to the study of the orogenic dynamics by high-lighting the role of excess pressure (dynamic pressure) associated with return flow in subduction channels, which is widely invoked to explain the exhumation of high-pressure metamorphic rocks. Return flow in subduction channels is well known, and is a variety of the fluid-mechanics phenomenon known as corner flow. Corner flow also occurs in the mantle wedge above the subducting slab (e.g., Spiegelman & McKenzie, 1987). Here the symmetry is reversed, and the dynamic pressure in the corner is negative, so that asthenospheric mantle flows from the back-arc towards the corner.

The analysis by Marques et al (2018) suffers from some serious problems, however, which largely undermine their conclusions about its application to high-pressure metamorphism. These problems are detailed below.

1. It is misleading to describe the channel geometry in the Himalayas as "upward tapering". Corner flow depends on the channel being closed at the corner, where the subducting slab comes into contact with the upper plate. Whatever the details of the channel geometry, it must ultimately always taper downwards if it is to produce the corner-flow effect. The Himalayan channel illustrated in Figure 1B of Marques et al (2018) does in fact taper downwards to a corner at 90 km depth on the N side of the section.
2. A related problem is that Marques et al (2018) regard the channel as being closed off by a "base" that they relate to footwall ramp and flat geometry in Figure 1B. This ignores the fact that geometrical features in the footwall move downward with the lower plate, at the same velocity as the downward flow in the channel. Footwall ramps and flats therefore do not contribute in any way to blocking the flow in the channel and driving the return flow, as suggested in Figure 2 of their paper.

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3. A more fundamental problem 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.

The dynamic pressure calculated in their models, which ranges up to ~1.5 GPa in excess of lithostatic pressure, will exert an outward load of this magnitude on the channel walls. Even if they are strong enough to resist permanent deformation under this load, the upper and lower plates should flex elastically. 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). 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. 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. If the dynamic pressure were continually maintained by flow in the channel, the upper plate could end up looking like a balloon. In practice, of course, the upper plate would undergo brittle or ductile failure long before this happens, but permanent deformation would also serve to relieve the excess pressure.

So why are the predictions of Marques et al (2018) so dramatically at variance with what we observe? The answer may lie in some combination of their assumptions about the channel viscosity, the channel geometry (as discussed above), and the degree of coupling between the down-going slab and the channel. Another possibility is that the excess pressure may resist continued downflow of material in the lower part of the channel. The downflow is what drives the dynamic pressure and the return flow, so this resistance may act as a negative feedback, limiting the magnitude of the dynamic pressure. I suggest that their estimates of dynamic pressure are at least one order of magnitude too high.

Marques et al (2018) proposed that high 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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References

Karner, G. D., and Watts, A. B.: Gravity anomalies and flexure of the lithosphere at mountain ranges, Journal of Geophysical Research, 88, 10449-10477, 1983.

Marques, F.O., Mandal, N., Ghosh, S., and Ranalli, G.: Channel flow, tectonic overpressure, and exhumation of high-pressure rocks in the Greater Himalayas, Solid Earth, in press.

Penniston-Dorland, S. C., Kohn, M. J., and Manning, C. E.: The global range of subduction zone thermal structures from exhumed blueschists and eclogites: Rocks are hotter than models, Earth and Planetary Science Letters, 428, 243-254, doi:10.1016/j.epsl.2015.07.031, 2015.

Spiegelman, M., and McKenzie, D.: Simple 2-D models for melt extraction at mid-ocean ridges and island arcs, Earth and Planetary Science Letters, 83, 137-152, 1987.

Watts, A. B., and Zhong, S.: Observations of flexure and the rheology of oceanic lithosphere, Geophysical Journal International, 142, 855-875, 2000.