Geophys. J. Int. (2007) **171,** 1404–1405 doi: 10.1111/j.1365-246X.2007.03637.x

Comment on 'Consequences of progressive eclogitization on crustal exhumation, a mechanical study' by H. Raimbourg, L. Jolivet and Y. Leroy

Neil S. Mancktelow

Geologisches Institut, ETH-Zürich, CH-8092 Zürich, Switzerland. E-mail: mancktelow@erdw.ethz.ch

Accepted 2007 September 25. Received 2007 September 25; in original form 2007 March 27

SUMMARY

Two simple end-member models of a subduction channel have been proposed in the literature: (i) the 'pressure-imposed' model for which the pressure within the channel is assumed to be lithostatic, the channel walls have negligible strength with respect to lateral pressure gradients, and the channel geometry therefore varies with time and (ii) the 'geometry-imposed' model of constant channel geometry, rigid walls and resultant lateral variation in pressure. Neither of these models is realistic, but they provide lower and upper bounds to potential pressure distributions in natural subduction zones. The critical parameter is the relative strength of the confining plates, reflected in the effective viscosity ratio between the channel fill and the walls. The assertion that the 'geometry-imposed' model is internally inconsistent is incorrect—it merely represents one bound to possible behaviour and a bound that may be approached for realistic values of the effective viscosity for weak channel fill (e.g. unconsolidated ocean-floor sediments) and relatively cold and strong subducting and overriding lithospheric plates.

Key words: Ultra-high pressure metamorphism; Subduction zone processes; Rheology: crust and lithosphere.

COMMENT

Raimbourg *et al.* (2007) recently published a paper in this journal where they consider a subduction channel model with walls of negligible strength with regard to normal stresses perpendicular to the walls but effectively rigid with regard to shear stresses parallel to the walls. In this end-member model, the pressure in the channel is always taken to be lithostatic and the channel geometry changes with the flux of material in the channel. This 'pressure-imposed' model is similar in its basic assumptions to that proposed by Shreve & Cloos (1986). In Mancktelow (1995), I considered the opposite end-member, namely a subduction channel of constant geometry and thus rigid walls. In this 'geometry-imposed' model, significant non-lithostatic pressures are generated for a wide range of predefined channel geometries, viscosities and thicknesses of incoming material riding on the subducted plate, and convergence rates.

Clearly the natural case lies somewhere between these two models, which represent upper and lower bounds. The channel walls must have sufficient strength for the overall subduction zone geometry to be maintained for tens of millions of years, typically with a moderately dipping Benioff zone defined by the distribution of earthquakes (Jarrard 1986). Indeed, the occurrence of large earthquakes within both the upper and lower plates (Shimamoto 1985; Magee & Zoback 1993) establishes that there is at least transient strength in the confining channel walls, capable of sustaining stresses up to the yield envelope for brittle failure. However, as discussed in some detail in Mancktelow (1995), it is clear that the overpressures predicted by the constant geometry model represent an upper bound to potential values and that the values developed in nature will be limited by the actual strength of the (non-rigid) walls. Effectively rigid walls could also move apart to increase the channel width and reduce overpressure values.

Raimbourg *et al.* (2007) propose that the transition between the two end-member models is determined by a parameter $\lambda = (\frac{h_0}{L})^3 \frac{\eta_{\mathrm{wall}}}{\eta_{\mathrm{channel}}}$, with h_0 the average width and L the length of the channel. The viscosity is η, and in their discussion they set $\eta_{\mathrm{wall}} = \eta_{\mathrm{mantle}}$ and $\eta_{\mathrm{channel}} = \eta_{\mathrm{crust}}$. They provide no details on the development of this result but refer instead to another unpublished manuscript (Raimbourg & Kimura 2006). However, they note that for a value of $\lambda \ll 1$ deformation of the channel geometry cannot be neglected whereas for $\lambda \gg 1$ the channel can be considered as effectively rigid. They conclude that, for the channel geometry they consider, an effectively rigid model is appropriate for $\frac{\eta_{\mathrm{wall}}}{\eta_{\mathrm{channel}}} > 10^4$. This is in broad agreement with results from numerical modelling of a viscous channel with viscous walls, which establishes that (i) there is a gradual transition between the two models, (ii) significant overpressures are certainly possible for ratios $>10^4$ and (iii) that the walls are effectively rigid for ratios $>10^6$ (Mancktelow 2007).

However, I must take strong exception to their statement on p. 385 that '(iii) Using the parameters used by Mancktelow (1995) yields λ ≪ 1, in contradiction with the assumption the author made that the channel is rigid!'. Rigid means exactly that—the material is undeformable and therefore $\eta_{\text{wall}} = \infty$, which implies that $\lambda = \infty$. No other result could be possible, because the original model assumed *a priori* a constant channel geometry with rigid walls. It is not correct to claim that the constant geometry, rigid wall model is internally inconsistent. This model simply provides an upper bound to potential non-lithostatic pressure distributions in a convergent subduction channel, just as the assumption of fully lithostatic distribution represents a lower bound.

In Mancktelow (1995), the average thickness of the channel is of order 1 km and the length of the convergent part of the channel is of order 100 km. It follows that the factor $(\frac{h_0}{L})^3$ in the expression of Raimbourg *et al.* (2007) is, in this case, of order 10^{-6}. For λ to be of order 1, the viscosity ratio would therefore need to be 10^6. This is again similar to the result of Mancktelow (2007)—the channel walls can be taken as effectively rigid for viscosity ratios of 10^6 or greater. However, a marked overpressure effect can still be generated for relatively strong walls as this ratio is gradually decreased, and significant overpressures may still be attained for ratios on the order of 10^4 (depending also of course on the width and convergence angle of the channel).

In natural subduction zones, there will always be some non-lithostatic component, but the critical question remains whether the magnitudes and gradients developed are large enough to have any significant influence on tectonic processes. Raimbourg *et al.* (2007) argue that non-lithostatic pressures in a subduction channel will be insignificant. However, their own analysis establishes that, for the specific channel geometry they consider, a ratio of $\frac{\eta_{\text{wall}}}{\eta_{\text{channel}}} > 10^4$ would be sufficient for the upper bound, fixed-geometry model to be appropriate. A channel fill viscosity of $\leq 10^{19}$ Pa s (e.g. for incoming unconsolidated ocean-floor sediments containing isolated basaltic blocks, as considered in Mancktelow 1995) confined between walls of relatively cold lithospheric mantle, with a viscosity on the order of 10^{23} Pa s, is a conceivable first-order model. Estimates for the effective viscosity of the asthenospheric mantle below the lithosphere are typically in the range of (3–5) × 10^{20} Pa s (Lambeck *et al.* 1996; Steffen & Kaufmann 2005) and the experimental observations of Funiciello *et al.* (2007) suggest 'that a lithosphere/upper mantle viscosity contrast of about 300 is necessary to obtain realistic trench/subducting plate velocity ratio as well as the variability of subduction styles recognized in nature'. Taken together, these two observations imply that the subducting and overriding lithospheric plates could indeed have an effective viscosity on the order of 10^{23} Pa s, even without considering the effects of the reduced geothermal gradient associated with a subduction zone. The basic criterion for the local development of significant non-lithostatic pressures, namely an effective viscosity ratio between the channel fill and the walls of 10^4 or more, is therefore not unrealistic and could well be attained in natural subduction zones.

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