Available online at www.sciencedirect.com
SCIENCE DIRECT®
Tectonophysics 361 (2003) 321–326
TECTONOPHYSICS
www.elsevier.com/locate/tecto

Discussion

The upper crust is softer than dry quartzite

E.B. Burov*

Laboratory of Tectonics, University of Paris VI, Case 129, T26-16, 4 Pl. Jussieu, 75252 Paris, France

Accepted 15 October 2002

The note “How soft is the crust?” by Ranalli (this volume) (hereafter G.R.) challenges some of the rheological choices of “Burov and coworkers” (hereafter B&C). This note could also be named “How soft is the dry quartzite?” since the author’s criticism is focused on the value of the quartzite creep parameter *A* from Brace and Kohlsted (1980) (hereafter B&K). A retrospective look at B&K’s quartzite rheology reveals, indeed, that it falls out of the range (too soft) of recently revised experimental parameters. In this context, we agree that B&K should be excluded from the reference list on *dry quartzite*. However, this neither adds to the solution of the major problem “How soft is the crust?” nor proves that B&K’s or B&C’s rheology is not good for the real upper crust, which is clearly not made of pure quartzite.

We can neither prove nor disprove any statements on laboratory data provided by other authors, but we can immediately state that the major thrust of the note by G.R. is based on somewhat erroneous stress/depth/temperature estimates. This may invalidate the essential part of the criticism including that on possible errors in B&K and, certainly, on the consequences of application of B&K’s rheology for geotectonic models.

1. Comparison of stress predictions, based on Eq. (5), with Fig. 6 from B&K and with G.R.’s and B&C’s yield stress envelops (YSEs) is not trivial, as suggested by G.R. This comparison may be questioned if we first analyse Fig. 4 from B&K, on which Fig. 6 is based. The stress shown in this figure is total horizontal stress (i.e. confining pressure included) and not differential stress used in Eq. (5). If we correct the total stress in Fig. 4 for depth-dependent lithostatic pressure, we will obtain significantly smaller strength values than those appearing in Figs. 5–6. These values appear to match the predictions of Eq. (5) assuming $A = 5 \times 10^{+6}$ MPa^{-n} s^{-1} and not $A = 5 \times 10^{-6}$ MPa^{-n} s^{-1}. In particular, Fig. 4 predicts a differential stress of less than 50 MPa at 19–20-km depth and not 100 MPa derived by G.R from Fig. 6. We cannot guess how Figs. 5–6 were obtained but they may contradict with Fig. 4. It may actually happen that the exponent of *A* swapped sign between these figures, or different data sets were used (B&K refer to two raw data sources). In any case, we previously (and quite naturally) did not pay attention to Figs. 4–6 derived on the basis of *a priory* wrong geotherm $T = 350\ \mathrm{K} + 15z$ (z is depth in kilometer) used in B&K. This geotherm predicts $T \approx +90$ °C at the surface of the Earth and is thus simply wrong, at least for the crust. This is a well-known error, in difference with the newly presumed error in the *A* parameter.

As to general consequences, G.R.’s comparison of estimates of the depth to 100-MPa strength at a fixed strain rate may be misleading. In a real physical system “a softer” power law rheology would automatically develop a higher strain rate than a stronger rheology. The associated stresses at the same depth will hardly differ by more than 10%. Moreover, the upper part of the crust is dominated by brittle failure

---

* *E-mail address:* evgenii.burov@lgs.jussieu.fr (E.B. Burov).

doi:10.1016/S0040-1951(02)00608-X

(especially in extensional regime), and it might be useless to discuss the peculiarities of flow laws in this domain. Finally, the very choice of a stress value of 100 MPa appears to be completely misleading because a number of data sources suggest that the upper crust may not sustain stresses in excess of 50–80 MPa (e.g. Chester, 1995; Bos and Spiers, 2002).

2. G.R. attempts to demonstrate that B&K's A parameter is physically senseless because it would infer a ridiculously low strength near the surface. With this goal, G.R. uses B&K's incorrect over-hot geotherm which predicts +90 °C at the surface of the earth. This geotherm comes from Goetze and Evans (1979), see reference in B&K. Yet, Goetze and Evans used it for an all-olivine oceanic lithosphere, in which creep is activated at a great depth and high temperature so that ± 100 °C temperature variation near the surface is of no importance. This geotherm is severely wrong, however, for the continental crust, which may be ductile above 10–15-km depth, at temperatures nearing 300 °C (Bos and Spiers, 2002). The correct geotherm would be $T \approx 273\ \mathrm{K}+(10 \div 15)z$. First (incorrect), geotherm of G.R. predicts a stress of 100 MPa for B&C's rheology at a ridiculous 1-km depth. Second (correct), geotherm predicts same stress at 8–10-km depth, which is near G.R.'s own value for wet quartzite. Fig. 3 from Burov and Diament (1995) or Fig. 7 (right, wet granite) from Carter and Tsenn (1987) suggest similar depths for 100-MPa strength.

Of course, the fact that G.R. uses a wrong geotherm does not negate the problem with the value of the A parameter. Yet, it negates G.R.'s point on the "obvious" physical senselessness of this value; the *YSE*s generated using B&K's A and correct geotherms just fall in the range of the upper crustal *YSE*s published in a recent literature (e.g. Bos and Spiers, 2002).

3. It should be mentioned that contrary to what may be understood from G.R.'s note, B&C tested and used highly different rheological laws for the upper crust: Kirby and Kronenberg (1987b) and Ranalli (1995) in Burov and Diament (1996); Carter and Tsenn (1987) and Kronenberg and Tullis (1984) in Burov and Cloetingh (1997); Ranalli and Murphy (1987) in Burov et al. (1998) and Gerbault et al. (1999) and so on. In a paper by Burov et al. (1998) dealing with the Canadian Craton, we check different rheologies against the geological scale data (lithospheric reaction to buried crustal loads). In particular, we verified both B&K's weakest quartzite rheology and the hardest quartzite rheology from Ranalli and Murphy (1987) and Ranalli (1995). We concluded that B&K's rheology is definitely not good for Cratons, whereas Ranalli's rheology suits well. In other cases (young lithosphere, young rifts and emplacement of magma chambers), we could state just the opposite (G.R.'s rheology would be impossible). In the general case of a "middle-aged" lithosphere, the application of different laws would not reveal important differences, in particular because: (1) the upper crust is predominantly controlled by brittle failure; (2) the rheology laws are so sensitive to temperature and fluids that a slight variation in the background geotherm, thermal conductivity or fluid content may easily turn hard dry quartzite from Kirby and Kronenbeg (1987b) into something like soft calcite from Kohlsted et al. (1995). Furthermore, in power law fluids, flow stress weakly depends on the strain rate but strongly depends on temperature, that is on activation energy Q. For example, a simple increase of Q by a factor of 2 converts B&K's "quartzite" into hard olivine or clinopiroxene. In most cases, behaviors predicted by Ranalli's (1997) quartz flow laws can be turned into those predicted by B&K by a small adjustment of the poorly constrained concentration of radiogenic elements that are abundant in the first 10–15 km of the upper crust. The internal heat production, not accounted in laboratory experiments, may also influence the long-term creep mechanisms (softening).

4. As a minor point, we would like to note that we contacted one of the authors of B&K (Kohlsted, 2002, personal communication) who suggests that the parameter $A = 5 \times 10^{6}\ \mathrm{MPa}^{+n}\ \mathrm{s}^{-1}$ from Eq. (5) may be correct, but also does not exclude the possibility of printing errors. It is noteworthy that in difference from the famous case of the paper of Kirby and Kronenberg (1987a), which was followed by immediate contestations and corrections (Kirby and Kronenberg, 1987b), B&K's A values were never contested, including some major review studies such as Kohlsted et al. (1995).

Conclusion 1. "Burov and coworkers" most probably did not make mistakes presumed by G.R.: (1) Eq. (5) from B&K does not contain immediately detectable errors; (2) we did not blindly used this equation but made various checks. We agree, however, that the parameters from B&K are not representative for dry laboratory quartzite. The modellers who are interested in quartzite rheology should avoid them. Nevertheless, on the basis of the arguments provided in point 3 above, as well as on the basis of recent experimental studies on polyphase crustal rocks, we are rather confident that the "weak" rheology used by B&C better represents the geological time scale behaviour of the real upper crustal rocks such as granites. The true question is "what level of confidence can be attributed to the extrapolations of the laboratory data to geological scales?" We believe that this level is lower than that of the rheological laws derived from direct geotectonic scale observations and models.

We could finish our reply at this point. However, the note by G.R. invokes some points of general interest that must be discussed as well.

5. The title of the note by G.R. "How soft is the crust? " targets a great geotectonic problem. However, the following text reveals that it is about a minor question: "How soft is the laboratory quartzite?" This particular question may be of restricted interest to geodynamic modellers. A priori there is no demonstrable link between the experimental parameters of quartzites and geological time scale behaviour of the upper crust, except that both contain abundant amounts of quartz.

6. Some elements of the answer to the question "How soft is the crust? " can be found in the landmark paper by Kohlsted et al. (1995) who point that "flow strengths now predicted from laboratory data probably overestimate the actual rock strength, perhaps by a significant amount". Citing this paper, G.R. only recalls the methodological part of the conclusions on the greatly improved robustness of the experimental data (in terms of data scatter, but not of the applicability). But Kohlsted et al. (1995) also admit that these new achievements add little to the solution of the fundamental problem of the credibility of extrapolation of the experimental data to the real earth. These authors doubt the validity of substitution of real polyphase crustal rocks with monophase samples used in the experiments, and emphasise that there is still no reliable data on flow properties of real rocks. In dislocation creep mode, the weakest phase would probably dominate rock behaviour. Yet, for the upper crustal rocks (first 10 km), the weak phase that can accommodate deformation is not necessarily quartzite. Very weak phases such as albite or micas can participate as well. Depending on microstructure and composition, polyphase quartz-rich rocks may be both significantly weaker, in case of upper crustal rocks (Bos and Spiers, 2002), or stronger, in case of lower crustal rocks (Ji et al., 2000), than quartz. In case of predominating diffusion creep, polyphase materials can be far much weaker than their single-phase constituents.

A number of rheological studies point out that under the upper crustal conditions, additional "non-Bayerlee" strain rate-dependent frictional mechanisms may be activated simultaneously with the ductile creep. This leads to a ductile-like constitutive law for the brittle regime resulting in very weak behaviour. According to these studies (e.g. Chester, 1995), upper crustal strength may be limited to 50 MPa reached at 6–15-km depth. Direct observations of crustal rebound (Bills et al., 1994) indicate that the strength of the upper crust may be strongly reduced below 3-km depth, with estimated viscosity as low as 10^{23} Pa s^{-1} that suggests stress levels below 50 MPa. Many known natural examples show that creep can start (even in quartz) at 5–6-km depth (Patterson, 2002, personal communication). The rheology used by B&C is about four times stronger than most of these estimates.

7. The most extreme response to the question "How soft is the crust (and the lithosphere)?" belongs to D. McKenzie and to the "Cambridge school" as a whole (e.g. Maggi et al, 2000), who suggest that all the strength of the continents is concentrated in *brittle* layers and that all crustal and mantle rocks would have a *zero ductile strength* at geological time scales. We do not share these extreme views, yet, not because they disagree with the predictions of laboratory data (e.g. Ranalli, 1995, 1997), but because these estimates are not supported by the wealth of observational data on geological time–space scale deformation. However, this manifestation of total disregard of rock mechanics is highly instructive, since it reflects the growing disbelief in the utility of laboratory data for geodynamic problems.

8. Compared to these major doubts, the message by G.R. appears to be slightly out of focus; the softest of

"upper crustal" rheology ever used by B&C is *only* three times weaker than that of dry quartzite from Ranalli (1997) but three to four times stronger than that suggested by Chester (1995), Bills et al. (1994) or Bos and Spiers (2002). The upper crustal part of the yield strength envelope (YSE contour for strain rate of 10^{-14} s^{-1}) based on this rheology (Burov and Diament, 1995, Fig. 3c) is just an exact replica of the YSE for wet rheologies from Carter and Tsenn (1987) (Fig. 7 right) or from Fig. 9 (right) of Kohlsted et al. (1995). Burov and Diament's (1995) YSE has a BDT depth of 8 km for high geothermal gradient whereas that from Carter and Tsenn (1987) is even shallower (BDT depth = 7 km).

9. The question that tectonic modellers need to resolve is not "how soft is quartzite" but "which rheology represents the long-term behaviour of the upper crust?" It was never shown, but always doubted, that quartzite is a good candidate here. Modellers have to account for all sensible *data*. Yet, here, there is a common trap to avoid: experimental parameters can be considered as *data* only for conditions similar to experiments. The creep parameters are not data when applied to problems involving geological time–space scale deformation. Their use implies huge, formally invalid extrapolations (10 orders of magnitude) and physically questionable assumptions. Most experimental data are obtained for uniaxial strain and performed on monophase rock samples, often for a single pressure value and for a limited temperature range. Direct application of the flow law parameters also implies another immediate error: the uniaxial experimental flow laws operate with total differential stress, whereas mechanical modellers directly substitute it with 2D or 3D deviatoric stress components. Only this forced substitution may impose corrections of the assumed rock strength by 50%, i.e. require a softer rheology law. The other main problem with the extrapolation of the experimental data relates to the possibility that there is a transition to another deformation mechanism with a different flow law that is the experimental regime. The best candidate for this type of behaviour is diffusion creep or pressure solution, yet it will require a lower activation energy than in B&K (Drury, 2002, personal communication).

Conclusion 2. At geological time scales, the implied creep parameters should not be considered as data but as an integral part of geotectonic models. In this case, the experimental flow parameters from Ranalli (1995, 1997) present upper bounds of the assumed rheological submodel. As any other model, these laws have to be varied, tested and validated using geological time scale observations in each particular case (e.g. Cloetingh and Burov, 1996). For example, B&C systematically verified if the assumed rheology fits the observed equivalent elastic thickness (EET), predicts correct fault spacing or deformational wavelengths and so on. The rheology laws that we can derive or constrain from large-scale observations (response of the lithosphere to loading, BDT depths, seismic data, etc.) are certainly "more valid" for geological scale problems than those derived from short-term laboratory experiments.

10. B&C deal with upper crusts composed of polyphase rocks such as *wet granites and gneisses*. Strength of near surface wet granites may be orders of magnitude lower than that of pure quartzite. Small concentration of soft metamorphic issues (e.g. micas, exponential flow parameter $n>18$!) or of other weak constituents such as albite may reduce the overall resistance to some ridiculously low values (Gueydan, 2001).

11. Errors in rheology papers are quite common (e.g. the famous example of Kirby and Kronenberg, 1987a). This circumstance should additionally motivate us to constrain and re-parameterize rheological laws using geological scale data. In most existing tectonic models, de facto variation of the input rheology is already a routine but somewhat hidden procedure usually accomplished through variation of the temperature profile. Contrary to tectonic models, mantle convection models commonly consider rheology as one of the variable input parameters. The Rayleigh numbers are typically varied from 10^5 to 10^{11}, which corresponds to a large integral variation of rheology, density and thermal properties. In a paper by Cloetingh and Burov (1996) cited by G.R., we discuss the ways of constraining rheology using geological scale observations and physical models. We suggest (see also Burov and Diament, 1992) that only YSE slopes can be used with confidence. This parameter is basically sufficient to constrain the effective flow law using observed brittle–ductile-transition (BDT) depth, equivalent elastic thickness (EET) and mechanical thickness of the layer.

12. Just like a computer Trojan, the *silicosis* has infected the tectonic community to a degree that manuscripts are routinely *rejected* if the authors use something else than "standard" quartzite. However, the main reason for the "quartz domination" is mostly historical or pragmatic. In the 1980s, quartzite was chosen to represent the upper crust because there were only a few data on real upper crustal rocks. The common explanation, however, tells that "quartz is both the most abundant and weakest element of crustal rocks and thus should control their strength". This is not always true. Quartz is not necessarily the weakest phase of the upper crustal rocks. Also, the strength of a polyphase rock is largely controlled by the efficiency of interphase bonds, by its microstructure and by the way it is loaded (normal stress, shear, etc.). B&K and Kohlsted et al. (1995) state that real earth rocks have to be significantly "softer" than the experimental estimates because: (1) quartzite (monophase rock) is stronger, probably by orders of magnitude, than polyphase rocks representative for the uppermost crust; (2) first several kilometers of the upper crust are not necessarily made of relatively strong granites but of weak metamorphic sedimentary rock; (3) fluids and pore pressure reduce rock strength by several times. This especially applies to polyphase rocks; (4) uniaxial strain is not what happens to real rock, which has a much smaller resistance to torque and shear; (5) there are intermediate weak (semi-brittle, brittle–ductile) deformation mechanisms that often take place instead of creep or frictional sliding.

13. Even though *laboratory* data on real rocks are far much less reliable than *that* of quartzite, many modellers *outside the tectonic community* consider very weak rheologies for the upper crust (e.g. Bittner and Schmeling, 1995; Weinberg and Podladchikov, 1994). Indeed, many upper crustal scale phenomena such as observed faults spacing (controlled by BDT) or emplacement of magma bodies would be physically impossible if the host rock was as hard as quartzite. However, strong regional variations in the upper crustal composition suggested for different tectonic provinces should result in wild variations in ductile strength. The rheology laws thus have to be validated in each particular case, and no rheological "standardisation" can be envisaged.

Conclusion 3. Unconditional application of "standardized" quartzite rheologies to the upper crust may lead to a dreadful deadlock. Although it is not excluded that B&K rheology is not representative for dry laboratory quartzite, this or close weak rheology appears to be reasonable for most real situations involving young or "middle-aged" lithospheres. The rheology of the crustal rocks should be constrained in the same way as it was partly constrained for deeper levels (mantle and asthenosphere). Long-term mechanical properties must be scaled on the basis of the direct observations of long-term/large-scale deformation such as seismic data, reaction of the lithosphere to known geological loads, geodetic data, post-glacial rebound data and so on. Greatest precautions should be taken specifically in relation to experimental data for "upper crustal" rocks. These laboratory data should basically serve as a "first guess" for thermomechanical models. Criticism for using this or another rheological law might be counterproductive until it is not based on geological scale evidences.

Acknowledgements

S. Cloetingh and M.R. Drury are deeply thanked for the very important contribution to the final version of this comment.

References

Bills, B.G., Currey, D., Marshall, G.A., 1994. Viscosity estimates for the crust and upper mantle from patterns of lacustrine shoreline deformation in the Eastern Great Basin. J. Geophys. Res. 99 (B11), 22059–22086.

Bittner, D., Schmeling, H., 1995. Numerical modelling of melting processes and induced diapirism in the lower crust. Geophys. J. Int. 123, 59–70.

Bos, B., Spiers, C.J., 2002. Frictional-viscous flow in phyllosilicate-bearing fault rock: microphysical model and implications for crustal strength profiles. J. Geophys. Res. 107 (B2), 101,029.

Brace, W.F., Kohlsted, D.L., 1980. Limits on lithospheric stresses imposed by laboratory experiments. J. Geophys. Res. 85, 6248–6252.

Burov, E.B., Cloetingh, S., 1997. Erosion and rift dynamics: new thermomechanical aspects of post-rift evolution of extensional basins. Earth Planet. Sci. Lett. 150, 7–26.

Burov, E.B., Diament, M., 1992. Flexure of the continental lithosphere with multilayered rheology. Geophys. J. Int. 109, 449–468.

Burov, E.B., Diament, M., 1995. The effective elastic thickness (Te) of the continental lithosphere: what does it mean? J. Geophys. Res. 100, 3905–3927.

Burov, E.B., Diament, M., 1996. Isostasy, equivalent elastic thickness, and inelastic rheology of continents and ocean? Geology 24, 419–422.

Burov, E.B., Mareschal, J.-C., Jaupart, C., 1998. Large scale crustal inhomogeneities and lithospheric strength in cratons. Earth Planet. Sci. Lett. 164, 205–219.

Carter, N.L., Tsenn, M.C., 1987. Flow properties of continental lithosphere. Tectonophysics 136, 27–63.

Chester, F.M., 1995. A rheologic model for wet crust applied to strike-slip faults. J. Geophys. Res. 100 (B7), 13,033–13,044.

Cloetingh, S., Burov, E., 1996. Thermomechanical structure of European continental lithosphere: constraints from rheological profiles and EET estimates. Geophys. J. Int. 124, 685–723.

Gerbault, M., Burov, E.B., Poliakov, A., Dagnieres, M., 1999. Do faults trigger folding in the lithosphere? Geophys. Res. Lett. 26 (2), 271–274.

Gueydan, F., 2001. "Les bandes de cisaillement de la transition fragile–ductile de la croûte continentale en extension. du terrain à la modélisation" PhD thesis, University of Paris VI.

Ji, S., Wirth, R., Rybacki, E., Jiang, Z., 2000. High-temperature plastic deformation of quartz-plagioclase multilayers by layer-normal compression. J. Geophys. Res. 105 (B7), 16651–16664.

Kirby, S.H., Kronenberg, A.K., 1987a. Rheology of the lithosphere: selected topics. Rev. Geophys. 25, 1219–1244.

Kirby, S.H., Kronenberg, A.K., 1987b. Correction to: rheology of the lithosphere: selected topics. Rev. Geophys. 25, 1680–1681.

Kohlsted, D.L., Evans, B., Mackwell, S.J., 1995. Strength of the lithosphere: consratites imposed by laboratory experiments. J. Geophys. Res. 100, 17587–17602.

Kronenberg, A.K., Tullis, J., 1984. Flow strength of quartz aggregates: grain size and pressure effects due to hydrolic weakening. J. Geophys. Res. 89, 4281–4297.

Maggi, A., Jackson, J., McKenzie, D., Pristley, K., 2000. Earthquake focal depth, effective elastic thickness and the strength of the continental lithosphere. Geology, 495–498 (June).

Ranalli, G., 1995. Rheology of the Earth, 2nd ed. Chapman & Hall, London. 413 pp.

Ranalli, G., 1997. Rheology of the lithosphere in space and time. In: Burg, J.-P., Ford, M. (Eds.), Orogeny through Time. Geol. Soc. Londn. Sp. Publ., vol. 121, pp. 19–37.

Ranalli, G., How soft is the crust? Tectonophysics 361, 319–320, this volume.

Ranalli, G., Murphy, D.C., 1987. Rheological stratification of the lithosphere. Tectonophysics 132, 281–295.

Weinberg, R.F., Podladchikov, Y., 1994. Diapiric ascent of magmas through power law crust and mantle. J. Geophys. Res. 99, 9543–9559.