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Letter to the Editor

Comments on "On the Thermal State of the Earth's Mantle" by A.C. Fowler

U. Christensen
Institut für Meteorologie und Geophysik, Feldbergstrasse 47, 6000 Frankfurt 1,
and Max-Planck-Institut für Chemie, Saarstrasse 23, 6500 Mainz, Federal Republic of Germany

In this paper A.C. Fowler deals with convective systems with strongly temperature- and pressure-dependent viscosity and an isoviscous gradient which is steeper than the adiabatic temperature gradient, as it is thought to apply to the Earth's mantle. His suggestion is that below a certain depth the temperature gradient switches from adiabatic to essentially isoviscous and superadiabatic. This seems to be a reasonable possibility in a general context provided the convective system has sufficient depth-extent. However, the proposition that it happens at the boundary between upper and lower mantle in the Earth and requires a lower mantle viscosity of about 10^{22} Poise is untenable. In the following I shall indicate several of the weak points and supply an additional argument why the conclusion is wrong.

(1) Although the author discusses this point at some length it is not clear how he can choose a depth range of $d = 700$ km for calculating $\delta^2 = \kappa \cdot l / U \cdot d^2$ (Eq. 2.8), while he wants to find out about the convective state of the whole or lower mantle. The reason that the obvious choice $d = 3000$ km does not lead to the desired result is not very convincing. If some form of layered convection is expected due to chemical or viscosity stratification or any other reason one might apply $d = 700$ km, but only to draw conclusions concerning the properties of convection in the upper layer. The ratio of $|q|$ versus δ^2 (Eq. 2.22) measures the relative importance of advective versus conductive heat transport, which depends critically on the length scale over which this transport has to take place. When it is taken for granted that at 700 km depth in the mantle the (scaled) velocity has dropped to that value of δ^2 which is calculated from $d = 700$ km, then advection is still the dominant process for scales in excess of this value, i.e. for whole or lower mantle dimensions. Concerning the thermal state of the *whole* mantle the 'obvious choice' for d is the only appropriate one, irrespective of the convective style which eventually emerges.

(2) Essential to the whole analysis is the assumption 2.21, namely that the local velocity is inversely proportional to the local viscosity in different parts of the convective system – at least on an order-of-magnitude scale. This appears reasonable at first but it does not seem to hold for convection. The author's arguments in defence of this relation are not conclusive. Stress and vorticity are confused. These quantities behave very differently – especially in a variable viscosity fluid. Unlike vorticity stress is not distributed via a Poisson's equation and internal stress concentrations (thought to be improbable by Fowler) may in fact occur. If any relation like 2.21 makes sense, one might try with a more relaxed formulation

$$|q| \sim 1/\eta^{\beta}$$

where β has to be determined from numerical or laboratory experiments with variable viscosity. No special emphasis has been laid on this point in the various publications, however, a re-analysis of the available data suggests that the dependence is rather weak. For convection with layered viscosity at the marginal stability state (Davies, 1977) a velocity difference of one order of magnitude relates to a viscosity contrast of 10^4. In a model with temperature-dependent viscosity (McKenzie, 1977) the typical difference in velocity is at most a factor of two between regions with a viscosity contrast of more than an order of magnitude. In Torrance and Turcotte's model (1971) with p,T-controlled rheology a change of one order of magnitude in velocity seems to relate to two orders of magnitude in viscosity (as far as can be deduced from the packing of stream lines in their figures), suggesting $\beta \simeq 0.5$. In a re-examination of a variety of models with variable and partially non-Newtonian rheology (Christensen, 1983) regarding this point, I find the range for β to be 0.3–0.6. Although a comprehensive study of the q versus η dependence is missing, it seems that Fowler's assumption of β equal 1 cannot be the general rule. A lower value of β would imply that larger viscosity and temperature variations can occur within the flow.

(3) In order to arrive at $\eta = O(10^{22}\mathrm{P})$ for the lower mantle (when accepting the rest of the analysis) one has to start out with an asthenospheric minimum viscosity of 10^{20} Poise as that value which controls the plate- or scaling-velocity of 5 cm/a. The analysis of plate-driving forces shows that there is hardly any viscous drag at the bottom of a plate (e.g. Forsyth and Uyeda, 1975; Davies, 1978). This implies that the asthenospheric viscosity is fairly irrelevant for the velocity of the plate, which is thought to be controlled by localized forces like 'trench resistance'. The asthenospheric viscosity is therefore only a lower bound to that value which can be related to the chosen scaling velocity. The higher this value is the higher also becomes the lower mantle viscosity.

(4) Fowler's proposed model has a lower mantle which is superadiabatic by about 0.2 K/km and has a viscosity of $O(10^{22}\mathrm{P})$. With these and some other standard values ($\alpha = 1\text{–}3 \cdot 10^{-5}\,\mathrm{K}^{-1}$, $\kappa = 1\text{–}2 \cdot 10^{-6}\,\mathrm{m}^2\,\mathrm{s}^{-1}$, $d = 2000$ km) one calculates the local Rayleigh-number for the subsystem of the lower mantle

$$Ra=\frac{\alpha g \rho d^4 \partial T/\partial z}{\kappa \eta}$$

to be of the order of 10^6, suggesting that this layering is highly unstable. It is true that supercritical Rayleigh-number alone is not sufficient since the lower mantle is embedded into the larger circulation which might suppress local instability – although this seems hard to believe with the local Rayleigh-number being 1000 times supercritical. I suggest, as a reasonable stability criterion, a comparison of the characteristic time for the large-scale flow to pass once through the lower mantle with the (*e*-folding) growth-time of a disturbance. The former is estimated with the velocity of 1 mm/a assumed by the author to be in excess of 1 Ga, while the latter can be estimated from linearized theory for an aspect-ratio-one disturbance in a constant-viscosity fluid with stress free boundaries:

$$\tau=\frac{d^2}{\kappa}\frac{4\pi^2}{(Ra-Ra_c)}.$$

Using the previous data one arrives at τ less than 10 Ma. Even when it is taken into account that the viscosity in the lower mantle is not entirely constant but is bounded by 10^{21} and 10^{23}Poise and that the upper bound rather than the average might control the growth time, τ is still much lower than 1 Ga. Therefore it seems impossible that the proposed temperature- and viscosity distribution exists for any sufficient length of time (after having once been established). This defeats Fowler's thesis independently from the other criticism.

Both the more realistic lower value for δ^2 (point 1 of this discussion) and the weak dependence of velocity on local viscosity (point 2) widen the range of permissible viscosity and temperature variations in a convecting mantle. I recently showed (Christensen 1983) that convection in a power-law fluid with stress exponent 3 can be represented by Newtonian convection with activation energy and volume reduced by a factor of one half or less. If this applies to the mantle, the band of permissible temperatures around an isoviscous profile has to be doubled once more. In this light it appears very doubtful that the breakdown of the convective regime proposed by Fowler ever occurs in an essentially adiabatic mantle above the core mantle boundary.

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