On the Thermal State of the Earth's Mantle

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Abstract. An apparent paradox is discussed, arising from the contrast between an inferred constant mantle viscosity profile and theoretical and experimental rheological flow laws, which predict a mantle viscosity function varying strongly as a function of both temperature and pressure. One can explain the paradox by a particular choice of material parameters, but then mantle temperatures (computed adiabatically) are too low; increasing the temperature by inserting compensatory thermal boundary layers is considered to be dynamically unfeasible, again because of the flow law. We consider this an impasse, and to resolve it, we suggest that old dogmas concerning boundary layers and adiabats need to be critically re-examined, to understand their basis. When this is done, we find that the observed constant viscosity is, in effect, *demanded* by the interplay of the rheology with the convective process, the mantle temperature is not necessarily adiabatic, and some form of layering effect may be expected, although the ideas presented here are virtually independent of the precise dynamical style of the convective motion. A consequence of these results is that explanations and extrapolations taken from constant-viscosity convection models are, a priori, unjustifiable. (Specifically, a constant viscosity mantle is a fundamental consequence of the state of flow together with the fluid parameters and rheology: it is not a passive coincidence, which may *then* be used to deduce the flow state, etc.)

Key words: Mantle temperatures – Mantle convection – Rheology

Introduction

Fifteen years after the pioneering theoretical paper of Turcotte and Oxburgh (1967), our understanding of the nature of the convective flow in the Earth's mantle is in a somewhat confusing state. Although both theoretical (Stocker and Ashby, 1973) and experimental (Goetze, 1978) work on mantle-type minerals show that the effective viscosity is a strongly varying function of both temperature and pressure (which is why the lithosphere is 'rigid', i.e. does not deform significantly on convective time scales of interest (at least away from trenches)), there have been relatively few really serious attempts at modelling the mantle as a fluid with such a viscosity, and where this has been done (Parmentier and Turcotte, 1978; Schmeling and Jacoby, 1981; Kopitzke, 1979; Christensen, 1982), various stop-gap measures have to be introduced in order to simulate the real earth. Particularly, the vital issue of subduction of the lithosphere at oceanic trenches has either been treated kinematically, or simply ignored; this has serious consequences since it is precisely the subduction which causes the active style of convection in the mantle: without subduction, numerical models have a tendency to exhibit a rigid, passive, conducting lid (Daly, 1980; Schmeling, 1980; Schmeling and Jacoby, 1981) as they (obviously) should. Turcotte et al. (1977) propose an elastic transverse buckling of the lithosphere to explain subduction, and if we follow this idea, one should really study viscoelastic convection of the mantle, a proposal which has not been previously considered, except in the recent paper by Ivins et al. (1982).

On the other hand, many workers have used models of high-Rayleigh-number, constant-viscosity convection to study various features of interest (McKenzie et al., 1974; Hewitt et al., 1981; Olson and Corcos, 1980; Elsasser et al., 1979): a recent paper in this style is that of Jarvis and Peltier (1982). One can argue variously about the merits of such models: on the one hand, the inference of a relatively constant sub-lithospheric viscosity (Cathles, 1975; Peltier, 1980) supports their usefulness, but this inference is something which itself needs to be explained, and can lead to apparently circular arguments (Elsasser et al., 1979), or misconceived analogies (Hewitt et al., 1981).

In any case, difficulties remain with the formulation of the problem. Recent geochemical evidence (De Paolo, 1981) supports circumstantially the notion that there is a chemical layering of the mantle across the transition zone. This is plausibly explained by a convective layering, which may also be consistent with the cessation of deep earthquakes at ~700 km, and the existence of a seismic discontinuity there. While none of these observations has irrefutable implications, they are self-consistent, and there is no countering evidence in support of whole-mantle convection, other than dynamical reasoning (Schubert and Spohn, 1981) (again on the basis of constant viscosity convection). It would be worthwhile seeing if these dynamic objections stand up in the context of a temperature and pressure dependent rheology.

This paper will not be overtly concerned with the specific issue of a layered convection, but will address directly the issue of a constant viscosity distribution in the mantle, and study how it may be reconciled with the expected rheology of the real Earth. In so doing, we shall infer some characteristics of the style of convection, and will thus be led to a new proposal for the mantle geotherm.

On the face of it, constant viscosity observations from post-glacial uplift (Cathles, 1975; Peltier, 1980) or polar wander (Yuen et al., 1982) do not appear likely if the effective viscosity (Goetze, 1978) based on dislocation creep, is of the form

$$\eta = \frac{A}{\tau^2} \exp\left[\frac{E^* + pV^*}{RT}\right]; \qquad (1.1)$$

here, τ is the second stress invariant, T is temperature, p is pressure, E^* is activation energy, V^* is activation volume, and R is the gas constant. For any acceptable temperature in the mantle, one finds $E^*/RT \gg 1$ (e.g. ~ 30) using laboratory measurements of E^*, and the exponent is far and away the most important part of determining the viscosity. In particular, isoviscous temperatures are those for which the exponent is approximately constant, i.e. $T \propto E^* + pV^*$; for constant E^* and V^*, this would imply that isoviscous temperature profiles are essentially linear with pressure, and hence depth. With commonly quoted values $E^* \sim 125$ kcal/mode, $V^* \sim 11$ cm^3/mole, such isoviscous profiles have temperature gradients $dT_{iso}/dz \sim 0.7$ K km^{-1}: here z is the depth coordinate. On the other hand, the internal temperatures (i.e. away from boundary layers) of a vigorously convecting fluid should be approximately adiabatically related (see further discussion of this below), that is $dT_{ad}/dz \approx \alpha g T_{ad}/c_p$. Using values $c_p \sim 0.27$ cal gm^{-1} K^{-1}, $g \sim 10^3$ cm s^{-2}, $\alpha \sim 3 \times 10^{-5}$ K^{-1}, $T_{ad} \sim 2,000$ K (a mean value) we obtain $dT_{ad}/dz \sim 0.5$ K km^{-1} as a reasonable estimate. These fairly roughly calculated values show that adiabatic and isoviscous temperature profiles have gradients differing by probably no more than a factor of two. Nevertheless, a difference of 0.2 K km^{-1} in the mean temperature gradient across the mantle would imply a difference of these two profile temperatures at the core-mantle boundary of 600 K, and in view of the largeness of E^*/RT, this could correspond to a viscosity differing from that in the asthenosphere by at least four orders of magnitude: the viscosity law is very sensitive to changes in temperature.

This discrepancy led Weertman (1978) to suggest that transient creep processes in the mantle might explain the apparent viscosity, even though the long term behaviour would be governed by thermally activated steady state processes. Indeed, extrapolation of experimental results of Goetze and Brace (1972) to mantle strain rates $\sim 10^{-15}$ s^{-1} would suggest a time-scale for transient creep of $\sim 10,000$ years, just that over which post-glacial rebound occurs. Peltier et al. (1980) examined the data for transient rheologies, and did not find any need to include such effects. In any case, transient creep should also be thermally activated, and Weertman's motivation would disappear.

O'Connell (1977) and Sammis et al. (1977) have proposed that other factors may contribute to the resolution of the isoviscosity problem. It is plausible (see also Karato, 1981) that the activation volume V^* decreases by up to a factor of about 4 as p increases through the mantle. Such a decrease is able to offset the slower than isoviscous increase in temperature due to adiabaticity, and it becomes possible to accommodate the observations with plausible Earth parameters. On the other hand Jaoul et al. (1981) show that E^* for forsterite increases with temperature: this would act in the opposite direction to decreasing V^*. Further, if one appeals to variation of V^* with p, consistency demands adoption of the best constraints available on thermodynamic parameters. From results of Dziewonski and Anderson (1981), Anderson and Sumino (1980) and Stacey (1977), we can infer that the thermal expansion coefficient α decreases (a factor of about 3) with pressure in the mantle. If we adopt these various results, one can compute an adiabatic temperature which (given the largeness of E^*/RT) to all intents and purposes gives a relatively uniform mantle viscosity (see Table 1); obviously by adjustment of dK/dp, dV^*/dp, etc., one could tune the viscosity to be absolutely constant. However, more importantly, the resultant adiabatic temperature only reaches a temperature of $\sim 2,400$ K at the base, about 800 K less than the constrained value due to extrapolation of the FeS solidus to core pressures (Stacey, 1977). One could again remedy this by fiddling with parameters until both adiabatic and constant viscosity temperatures overlap: this is a philosophically unsatisfactory and academically unsound procedure, amounting to no more than curve-fitting, and the remedy should lie elsewhere. In fact, Jeanloz and Richter (1979) noted the same difficulty and suggested that the temperature deficit could be made up by one or more thermal boundary layers at the base of the mantle, and at the transition zone (670 km) if layered convection occurs: they do not consider that temperature jumps at phase changes can amount to more than about 100 K in total.

This proposal sounds reasonable provided one accepts boundary layers as inevitable, and indeed, this is exactly what constant viscosity convection leads us to expect. However, it is not a priori obvious in what manner boundary layers, in which the temperature jumps $\gtrsim 500$ K, can and will occur in a temperature-dependent viscous fluid: in fact, more recent consideration of this proposition suggests that boundary layers in convection of such fluids become asymmetric, the bulk of the temperature jump occurring across the cold, upper thermal boundary layer: this is evident in numerical studies (Schmeling, 1980), experimental studies (Nataf and Richter, 1982), and may be understood in terms of a marginal stability criterion of boundary layers (Howard, 1966); one can easily compute the order of magnitude temperature jump that basal boundary layers can sustain, from linear stability results of Schubert et al. (1969): when $E^*/RT \gg 1$ (as for the mantle), this temperature jump is $O[RT_b^2/(E^* + pV^*)]$, where T_b is the basal temperature (see also Fowler, 1982b; Morris, 1980); when $T_b \sim 3,000$ K, this is ~ 100 K at the core-mantle boundary, with reasonable estimates for E^* and V^*. Thus one should not expect a temperature jump of ~ 800 K in the D'' layer: this would entail

Table 1. Values of α, p, ρ, g, V^*, T_{ad}, η and K computed using the formulae $K = K_0 + K_1 p$, $\alpha = \gamma \rho c_p / K$, $\rho = \rho_0 [1 - \alpha(T - T_0)]$ $\exp\left[\int_0^p dp/K(p)\right]$, $V^* = V_0^*/(K/K_0)$, $\eta = (A/\tau^2)\exp[E^* + pV^*]/RT]$. The lithostatic pressure p and gravity g are calculated after solving Poisson's equation for the gravitational potential. For the results here, we have used $\rho_0 = 3.8\,\mathrm{gm\,cm}^{-3}$, $T_0 = 273\,\mathrm{K}$, $K_0 = 131\,\mathrm{GPa}$ (1 GPa $= 10^4$ bar), $K_1 = 4.0$, $\gamma = 1.0$ (Gruneisen parameter), $\tau = 10\,\mathrm{bars}$, $A = 1/70\,\mathrm{bar}^3\,\mathrm{s}$, $c_p = 1.26\,\mathrm{kJ\,kg}^{-1}\,\mathrm{K}^{-1}$, $V_0^* = 11\,\mathrm{cm}^3/\mathrm{mol}$, $E^* = 122\,\mathrm{kcal/mol}$, $R = 8.3\,\mathrm{J\,mol}^{-1}\mathrm{K}^{-1}$, and the adiabatic temperature is determined from $dT_{ad}/dp = \gamma T_{ad}/K$, where also $T_{ad}(0) = 1{,}550\,\mathrm{K}$ here. These values are either taken from commonly quoted values ($V^*, E^* \dots$) or induced from Earth models such as that of Dziewonski and Anderson (1981). With the chosen values of K_0, K_1, γ, ρ_0, we see that p, ρ, g and K look pretty much like their inferred values from the above Earth model, V^* decreases in line with inferences of Sammis et al. (1977), and α decreases as expected. The computed adiabatic temperature fails to approach the constrained value of $\gtrsim 3{,}000\,\mathrm{K}$ at the core mantle boundary, whereas the viscosity is *relatively* uniform over the whole sub-lithospheric mantle, though still varying by a factor of fifty or so. We emphasise that the point of this table is not to predict anything precisely (viscosity or temperature) but rather to show the qualitative results which may *reasonably* be expected

depth (km)	α (K^{-1})	p (k bar)	ρ (gm cm^{-3})	g (ms^{-2})	V^* (cm^3 mole^{-1})	T_{ad} (K)	η (poise)	K (GPa)
100.0	0.32E−04	36.1	3.7	9.81	9.9	1,591.1	0.15E+21	145.5
200.0	0.30E−04	73.3	3.8	9.81	9.0	1,630.3	0.52E+21	160.3
300.0	0.28E−04	111.3	3.9	9.81	8.2	1,667.7	0.13E+22	175.5
400.0	0.26E−04	150.3	4.0	9.80	7.5	1,703.5	0.24E+22	191.1
500.0	0.25E−04	190.0	4.1	9.79	7.0	1,737.9	0.37E+22	207.0
600.0	0.24E−04	230.6	4.2	9.78	6.5	1,771.0	0.49E+22	223.2
700.0	0.22E−04	271.9	4.3	9.77	6.0	1,802.9	0.59E+22	239.8
800.0	0.21E−04	313.9	4.3	9.75	5.6	1,833.7	0.64E+22	256.6
900.0	0.20E−04	356.6	4.4	9.74	5.3	1,863.5	0.65E+22	273.7
1,000.0	0.19E−04	400.0	4.5	9.72	5.0	1,892.4	0.64E+22	291.0
1,100.0	0.19E−04	444.0	4.6	9.72	4.7	1,920.3	0.58E+22	308.6
1,200.0	0.18E−04	488.6	4.6	9.70	4.4	1,947.5	0.52E+22	326.4
1,300.0	0.17E−04	533.8	4.7	9.68	4.2	1,973.9	0.45E+22	344.5
1,400.0	0.17E−04	579.6	4.8	9.67	4.0	1,999.7	0.39E+22	362.8
1,500.0	0.16E−04	625.9	4.8	9.66	3.8	2,024.7	0.32E+22	381.4
1,600.0	0.15E−04	672.8	4.9	9.66	3.6	2,049.2	0.27E+22	400.1
1,700.0	0.15E−04	720.3	4.9	9.65	3.4	2,073.1	0.22E+22	419.1
1,800.0	0.14E−04	768.3	5.0	9.66	3.3	2,096.4	0.18E+22	438.3
1,900.0	0.14E−04	816.9	5.1	9.66	3.1	2,119.3	0.14E+22	457.8
2,000.0	0.14E−04	866.1	5.1	9.67	3.0	2,141.7	0.11E+22	477.5
2,100.0	0.13E−04	916.0	5.2	9.69	2.9	2,163.7	0.92E+21	497.4
2,200.0	0.13E−04	966.4	5.2	9.71	2.8	2,185.3	0.73E+21	517.6
2,300.0	0.12E−04	1,017.5	5.3	9.75	2.7	2,206.6	0.59E+21	538.0
2,400.0	0.12E−04	1,069.3	5.3	9.79	2.6	2,227.6	0.47E+21	558.7
2,500.0	0.12E−04	1,121.9	5.4	9.84	2.5	2,248.2	0.37E+21	579.8
2,600.0	0.11E−04	1,175.3	5.4	9.90	2.4	2,268.7	0.30E+21	601.1
2,700.0	0.11E−04	1,229.5	5.5	9.98	2.3	2,288.9	0.24E+21	622.8
2,800.0	0.11E−04	1,284.8	5.5	10.07	2.2	2,308.9	0.19E+21	644.9
2,900.0	0.11E−04	1,341.1	5.6	10.18	2.2	2,328.8	0.15E+21	667.4

a viscosity *decrease* with depth on the order of a factor 10^3, which I suggest would be wildly unstable (Howard, 1966; Schubert et al., 1969). Alternatively, as Jeanloz and Richter (1979) suggest, one could have two layered convection cells, so that the temperature jump necessary could be accommodated by smaller boundary layers at the base of the lower and upper mantle. However, one would still have temperature jumps $\sim 400\,\mathrm{K}$, and consequently the lower mantle would have a viscosity say 10^2 times that of the upper mantle. According to rotation and uplift data (Peltier, 1980; Yuen et al., 1982) this is not possible. We think that Jeanloz and Richter found a real problem, but that their solution is not feasible – the problem remains. Note that we are not saying that a geochemical *layering* is impossible: simply that large internal or basal temperature jumps are improbable, for *dynamical reasons*.

If we accept the premises (constant sub-asthenospheric viscosity and thermally activated creep), ap-

parently we still have a dilemma on our hands. One can always escape by choosing not to believe either the reasoning or the results, but it is also worth pursuing the logic of the argument, to see if it is truly self-consistent. The impasse we have is that a feasible adiabatic temperature for an internal (i.e. away from boundary layers) isoviscous flow is too low for the core-mantle boundary constraint ($T_b \gtrsim 3{,}000\,\mathrm{K}$). The implication is simply that the internal temperature variation with depth is faster than adiabatic. The description of the temperature of convecting fluids in terms of boundary layers and adiabats is usually taken for granted, but it may be worthwhile to examine these concepts in a little more detail, to understand under what circumstances such a description is appropriate, since there is a tendency in the geophysical literature to misconstrue the mathematical significance of boundary layers, and to put them in freely whenever there is a temperature deficit to be made up.

Boundary Layers and Adiabats

For the purposes of this section, we require only an energy equation

$$\rho c_p \frac{dT}{dt} - \alpha T \frac{dp}{dt} = kV^2 T, \tag{2.1}$$

in which T is temperature, p is pressure, ρ is density, c_p is specific heat, α is thermal expansion coefficient, k is thermal conductivity, and $d/dt = \mathbf{q} \cdot V$ in the steady state. We can also define the Grüneisen parameter

$$\gamma = \alpha K / \rho c_p, \tag{2.2}$$

where K is the (adiabatic or isothermal) bulk modulus, and can be indirectly estimated from seismic and mean density data. The equation of state may then be written (Bullen, 1975)

$$\rho = \rho(p, T) \quad \text{and} \quad \frac{\partial \rho}{\partial p} = \frac{\rho}{K}, \quad \frac{\partial \rho}{\partial T} = -\alpha\rho, \tag{2.3}$$

and $K = K(p)$, $\alpha = \alpha(p)$ (dependence on T is weak). We will consider a viscosity in the form

$$\eta = \eta_0 \exp\left[\frac{E^* + pV^*}{RT}\right], \tag{2.4}$$

where really $\eta_0 = A/\tau^2$ (Goetze, 1978), or perhaps $\eta_0 = A/\tau^2 + B$ to include diffusion creep, but we suppress dependence on τ to emphasise the strong exponential variation of η with T and p.

We must scale the variables; thus we write

$$T = T_a \theta, \quad p = \rho_0 g d p^*, \quad x = l x^*, \quad y = d y^*,$$
$$t = (l/U) t^*, \quad u = U u^*, \quad v = (d U/l) v^*, \tag{2.5}$$

where g is gravity, $\mathbf{q} = (u, v)$ is the velocity, T_a is the sub-lithospheric temperature (this will be made more specific in due course), d and l are vertical and horizontal scale heights, and U is a horizontal velocity scale. Adoption of (2.5) then leads to the following dimensionless equations, where we drop asterisks on the variables:

$$\frac{d\theta}{dt} - D\theta \frac{dp}{dt} = \delta^2 V^2 \theta \equiv \delta^2 [\theta_{yy} + v^2 \theta_{xx}],$$

$$\eta = \eta_a \exp\left[\frac{1 + \mu p - \theta}{\varepsilon\theta}\right], \tag{2.6}$$

where $\eta_a = \eta_0 \exp(1/\varepsilon)$ is (roughly) the sub-lithospheric viscosity ($\theta = 1$, $p \approx 0$). In (2.6) there are five dimensionless parameters

$$v = d/l, \tag{2.7}$$

$$\delta^2 = \frac{\kappa l}{U d^2} \tag{2.8}$$

(κ is the thermal diffusivity $k/\rho c_p$),

$$D = \alpha g d/c_p, \tag{2.9}$$

$$\mu = \rho_0 g \, d V^*/E^*, \tag{2.10}$$

and

$$\varepsilon = R T_a/E^*. \tag{2.11}$$

If we are being realistic, then all these are functions of p and T: particularly $D = D(p)$, $\mu = \mu(p)$ (both decreasing). If we define a stress scale

$$[\tau] = \eta_a U/d, \tag{2.12}$$

and non-dimensionalise the stress components with $[\tau]$, the dimensionless constitutive law may be written

$$\tau_2 = \eta^*(u_y + v^2 v_x),$$
$$\tau_1 = 2v\eta^* u_x, \tag{2.13}$$

where η^* is the dimensionless viscosity $= \eta/\eta_a$.

We have some comments. We are now going to choose 'representative' values of the various parameters, and examine the analytical implications of these choices for the possible temperature field. Our discussion will use *asymptotic* orders of magnitude for the various quantities δ, etc., and to this (mathematical) extent, such discussion will be reasonably general. However, in attempting to make an application to one particular planet at one particular epoch (the present Earth), we are naturally forced to consider whether the *quantitative* values of δ, etc., that we adopt can be construed as having a realistic bearing on this one sample.

Being biased in favour of the views promoted here, it is natural that the scales so chosen will happen to give a satisfactory explanation of some observed features of the mantle. However, one should be aware that, even where the scaling analysis is *qualitatively* valid, precise numerical estimates depend on numerical evaluation of fixed 'order one' constants, whose actual magnitude can vary substantially.

As an example, consider the well-known boundary layer analysis (Turcotte and Oxburgh, 1967) of isoviscous convection at high Rayleigh number, Ra. Scaling alone (for stress free boundary conditions) suggests that the Nusselt number $Nu \sim Ra^{1/3}$ as $Ra \to \infty$, whereas experiments and analysis give the more accurate $Nu \sim 0.1 Ra^{1/3}$: here 0.1 (an order of magnitude) arises as a coefficient of "$O(1)$". In fact, one can understand the appearance of such a factor by writing $Nu \sim (Ra/Ra_c)^{1/3}$, where Ra_c is the critical value at the onset of convection; this form is suggested by Howard's (1966) marginal stability argument, where $Ra_c = 27\pi^4/4 \approx 657$ is "$O(1)$"! Thus, one should beware of treating approximate scaling results in an exact manner.

One might argue as a result of the caveat above that these kind of arguments are of little use, but the success of such techniques in many areas of applied mathematics belies this statement, and in any case the main thrust of such arguments is in suggesting the possible (or plausible) *structure* of solutions: precise quantitative results require a detailed numerical solution.

One might also then ask for such a numerical solution, but this inverts the argument: one can hardly give a numerically parameterised result (e.g. the factor 0.1 referred to above) without a previous analysis of the boundary layer structure.

As an illustration of these comments, consider the parameter δ^2 in (2.8). Below we shall find $\delta^2 \sim 10^{-2}$, and this will fit nicely with the inferred viscosity of the mantle. However, if δ^2 is much smaller (say, $<10^{-3}$), then this nice correspondence becomes less tenable. Now the obvious choice of $d = l = 3{,}000$ km for the whole mantle and $U = 10$ cm yr^{-1} leads to $\delta^2 \sim 10^{-4}$, too low for our needs. However, the same parameters with a viscosity $\eta_m \sim 10^{22}$ Poise define a stress scale $[\tau] = \eta_m U/d \sim 10$ bars, much less than the negative buoyancy in a down-going slab would suggest [~ 500 bars]. Alternatively, if we supposed layered convection can occur, then a more appropriate choice of d is 700 km, so that then $\delta^2 \sim 10^{-3}$, and the 10 bar figure (modified by d but also (probably) by η_a) becomes more recognisable as an inferred sub-lithospheric stress. But if shallow convection is considered, then more typically $l \sim 7{,}000$ km than 700 km, and $\delta^2 \sim 10^{-2}$! The important point to note is that in any case $\delta^2 \ll 1$, and that the subsequent discussion of this paper relies only on *asymptotic* estimates $1 \gg \delta^2 \gg \exp[-1/\varepsilon]$, which are in any case qualitatively applicable. Since a result of this analysis will be that the effective depth of vigorous convection is generally less than that of the whole mantle (see also Fowler, 1982a), we will simply anticipate this below in choosing d.

To proceed, however: in principle, scaling of the entire system of equations (not all given here) should rationally be done in terms only of the actual inputs to the model (basal temperature, depth, etc.) (Fowler, 1982b). This is not our present concern, and we simply use observation to choose

$$U \sim 5 \text{ cm yr}^{-1}; \tag{2.14a}$$

as may already have been suspected, we will define

$$d = 700 \text{ km}, \tag{2.14b}$$

as representative of layered mantle convection (or if one likes, the viscous scale height which might control the dynamics: Schubert et al., 1969). The length scale of surface plates is taken as

$$l = 8{,}000 \text{ km}, \tag{2.14c}$$

and then other standard (quoted) values ($\kappa \sim 10^{-2}$ cm s^{-1}, $R \sim 8.3$ J mole^{-1} K^{-1}, $T_a \sim 1{,}500$ K, $E^* \sim 122$ kcal mole^{-1}, $\alpha \sim 3 \times 10^{-5}$ K^{-1}, $g \sim 10$ m s^{-2}, $c_p \sim 0.27$ cal gm^{-1} K^{-1}, $\rho_0 \sim 3.5$ gm cm^{-3}, $V^* \sim 11$ cm^3 mole^{-1}) lead to *typical* values

$$\delta^2 \sim 10^{-2}, \quad D \sim 0.2, \quad \mu \sim 0.5, \quad \varepsilon \sim 1/40; \tag{2.15}$$

particularly

$$D, \mu \lesssim O(1), \quad \varepsilon, \delta^2 \ll 1. \tag{2.16}$$

The ensuing discussion will now proceed precisely on the basis of the asymptotic limits implied by (2.15), i.e. $\varepsilon, \delta \to 0$; $D, \mu \lesssim O(1)$. It should be noted that these arguments apply regardless of whether $\delta^2 \sim 10^{-2}$ or 10^{-4}, for example; it is only in the specific application to the Earth that any quantitative selection is indicated.

Let us first consider the energy equation (2.6) in the light of these estimates. *Since the scales are specifically chosen so that the dimensionless variables are $O(1)$*, we are led to neglect $\delta^2 \nabla^2 \theta$ in (2.6) on the basis that $\delta^2 \ll 1$, and that if $\theta \sim 1$, then we should expect $|\nabla \theta|, |\nabla^2 \theta|$, etc. to be $O(1)$ unless this leads to inconsistency. In the case that this is valid, i.e. if $|\mathbf{q}| \sim 1$ *and* $|\nabla^2 \theta| \sim 1$, then

$$\mathbf{q} \cdot \nabla \theta - D \mathbf{q} \cdot \nabla p \approx 0 \tag{2.17}$$

(at least in a steady state) and we have the adiabatic temperature profiles

$$\theta = \exp \left[\int_0^p D(p)\,dp \right] \tag{2.18}$$

(we can choose $\theta = 1$ at $p = 0$ by choice of T_a). This is the basis for the assumed adiabatic thermal profile.

The neglect of $\delta^2 \nabla^2 \theta$ in (2.6) constitutes a removal of the highest derivatives of the equation, and a consequent reduction of order. As a result of this, the reduced (hyperbolic) equation (2.17) *cannot* generally satisfy all the prescribed boundary conditions (on top and bottom), and at least one must be ignored (we *could* choose θ_0 to satisfy one of them). As a result of this, our original assumption that $\delta^2 \nabla^2 \theta$ is always small must break down, and this implies that $|\nabla \theta|$ becomes large over a length scale small compared to one, i.e. θ changes rapidly in a thin *boundary layer*. Naturally, since the inconsistency is due to an inability to satisfy boundary conditions, one looks first for such layers near boundaries, but it should be emphasised that (since the problem is non-linear) nothing precludes internal layers or multiply-layered solutions (Kevorkian and Cole, 1981) except possibly our hope that these are less likely, physically, to occur. Furthermore, the occurrence of symmetric upper and lower boundary layers in constant viscosity convection, heated from below (Turcotte and Oxburgh, 1967) is a consequence of the symmetry of the problem, and two boundary layers are not a necessary result of small δ, e.g. when internal heating drives the flow (Peltier, 1981). To re-iterate: one expects boundary layers to occur when $\delta \ll 1$ because of the singular nature of the approximation (2.17), but where and how many these are depends on the particular situation; basal boundary layers, particularly, are not always inevitable, especially in variable viscosity flows. Arguments about 'getting the heat out of the core' are also not germane, since it is quite feasible to have a singular layer in which $\partial \theta / \partial y$ jumps by $O(1)$, but θ jumps only by a small amount (e.g. $O(\delta)$): consider $\theta = \delta \exp[-y/\delta]$ near $y = 0$.

Let us now suppose we are away from any boundary layers or thermal plumes, so that we expect the adiabatic temperature (2.18) to be valid. Substitution of this $\theta = \theta_{ad}$ into (2.7), and recalling the dimensionless viscosity $\eta^* = \eta/\eta_a$, yields a dimensionless adiabatic viscosity

$$\eta_{ad} = \exp \left[\frac{1 + \mu p - \theta_{ad}(p)}{\varepsilon \theta_{ad}(p)} \right]. \tag{2.19}$$

We recall from Table 1 that it is feasible to have functional forms for $\mu(p)$ and $D(p)$ which make η_{ad} nearly constant; however, note that any slight deviation from

these isoviscous forms will lead to a substantial variation of η_{ad} *because of the fact that* $\varepsilon \ll 1$. For example, a variation in $[1 + \mu p - \theta_{ad}]/\theta_{ad}$ of $O(\varepsilon \ln(1/\delta))$ will cause a variation of $O(1/\delta)$ in the viscosity. In an asymptotic sense, it is clear that as $\varepsilon, \delta \to 0$, very small departures of θ_{ad} from the isoviscous profile θ_{iso} will cause large variations in η^*: yet the observational data apparently constrains η^* to less than a factor of ten within the lower mantle. It is therefore unrealistic and unreasonable to suppose that μ and D are just precisely such that $\eta_{ad} = $ constant, rather it is more pertinent and generic to suppose that even if η_{ad} does not vary substantially (i.e. many orders of magnitude), even small departures of θ_{ad} from θ_{iso} will ensure that η^* will vary by perhaps one or two orders of magnitude. We will return to a detailed discussion of the Earth below but for the moment let us suppose that η^* does so vary.

Now consider the effect of a varying η^* on the velocity. Stress and velocity are related in order of magnitude by

$$\tau \sim \eta^* u_y \qquad (2.20)$$

(we will not write full equations as our intention is to discuss scales, although with our choice of d and l, $v \sim 0.1$ and (2.20) accurately represents (2.13)). In order to try and understand how $|\mathbf{q}|$ may vary with η, we must first understand how stress is produced in convection. For this purpose, consider the Turcotte-Oxburgh boundary layer theory of convection (1967). In this situation, shear stress is generated by thermal buoyancy in the thermal plumes at the cell boundaries. Away from these plumes, the interior (isoviscous) fluid experiences a net torque which drives the convection cell. The stress (actually vorticity★) is distributed (by Laplace's equation★) through the cell interior, and there are no internal stress boundary layers. The flow is then determined (by Poisson's equation★), and there are similarly no large velocity gradients. In the absence of large velocity and stress gradients, (2.20) then implies that

$$|\mathbf{q}| \sim \tau/\eta^* \sim 1/\eta^* \qquad (2.21)$$

(N.B. we do *not* mean $|\mathbf{q}| \propto 1/\eta^*$).

In other words: if η^* increases, $|\mathbf{q}|$ decreases. We claim to have deduced this, but it is in fact an obvious physical proposition. However, (as was the case with the magnitude of δ^2) it could be rather important for the *quantitative* application of our theory that (2.21) holds rather than, say $|\mathbf{q}| \sim 1/\eta^{*\frac{1}{2}}$. In the present context, we cannot hope to prove (2.21) preferentially, but we can perhaps give some further indication of why it may obtain.

There are only two obvious ways in which (2.21) does not follow from (2.20), in the interior of the convection cell. One is that there are internal stress concentrations. As already outlined, this seems implausible and unlikely. Alternatively, since only strain-rates tend to zero as $\eta^* \to \infty$, one could have a state of 'quasi-rigid' motion, which would be, most likely, a parallel return flow. The trouble with this is that one would then naturally expect stresses in the upwelling plume of

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★ In this Turcotte-Oxburgh isoviscous case

comparable magnitude to those in the slab in order to obtain the necessary strain rate to alter the direction of flow from horizontal to vertical: however, it is part of the later argument of this paper that large thermal anomalies at the base of the convecting zone are precluded by virtue of the strong temperature dependence of the viscosity (they would be highly unstable). Consequently, stress in the ascending plume should be an order of magnitude smaller than that in the descending slab. It would seem that the more plausible alternative is a quasi-stagnant zone, as described. So let us now suppose that $|\mathbf{q}|$ decreases as η^* increases, as in (2.21).

If we remain on an adiabat, we can thus expect $|\mathbf{q}|$ to fluctuate. But now re-examine (2.6):

$$\mathbf{q} \cdot \nabla \theta - D\theta \mathbf{q} \cdot \nabla p = \delta^2 \nabla^2 \theta. \qquad (2.22)$$

The adiabatic temperature was derived from (2.22) on the basis that $|\delta^2 \nabla^2 \theta| \ll 1$. This is motivated by $\delta^2 \ll 1$, and is valid provided $|\nabla^2 \theta| \lesssim O(1)$ (we are away from boundary layers, i.e. in the interior) *and* $|\mathbf{q}| \sim O(1)$: specifically $|\mathbf{q}| \gg O(\delta^2)$. But if $|\mathbf{q}|$ decreases to $O(\delta^2)$, the adiabatic assumption is not valid, and we are no longer warranted in assuming an adiabatic temperature. We explore the consequences of this statement below. For the moment we observe that if the adiabatic gradient is 0.5 K km^{-1}, the isoviscous gradient is 0.7 K km^{-1}, and if we take $T \sim 2,000$ K, representative of upper mantle conditions, then we can expect non-adiabaticity when $\theta_{ad} - \theta_{iso} \sim 2\varepsilon \ln(1/\delta)$, corresponding to a temperature difference ~ 260 K, which at 0.2 K km^{-1} divergence rate occurs at a depth of $1,300$ km, certainly well within the mantle. Actually, this is only a rough order of magnitude, and a glance at Table 1 shows an increase of $\sim 40 (\sim O(1/\delta^2)?)$ at a depth of ~ 700 km. We note the coincidence, if it is that, that this depth is in the middle of the transition zone, and is commonly taken as the proposed chemically induced barrier separating lower and upper mantle convection (Jeanloz and Richter, 1979).

A Non-Adiabatic Temperature in the Lower Mantle

Let us now examine the depth dependence of the temperature in the mantle in some detail. The discussion essentially centres on sub-oceanic mantle, so that the lithosphere participates actively. Consequently, there is a boundary layer at the surface of dimensionless thickness $O(\delta)$, over which the temperature jumps by $O(1)$. Hence

$$|\partial \theta/\partial n|_{p=0} \sim O\left(\frac{1}{\delta}\right), \qquad (3.1)$$

i.e. the Nusselt number $Nu \sim 1/\delta$. The viscosity decreases by $\exp[O(1/\varepsilon)]$ through the lithosphere, attaining a minimum just outside the lithosphere in the asthenosphere (it attains a minimum since by assumption $d\theta_{ad}/dp < d\theta_{iso}/dp$, at least near $p=0$). For the sake of argument, we suppose this minimum is $\sim 10^{20}$ P (Cathles, 1975): the precise figure is *irrelevant*.

We now consider isoviscous profiles corresponding to this minimum viscosity, and two more, bigger by $O(1/\delta^2)$ and $O(1/\delta^4)$. Specifically, these are for viscos-
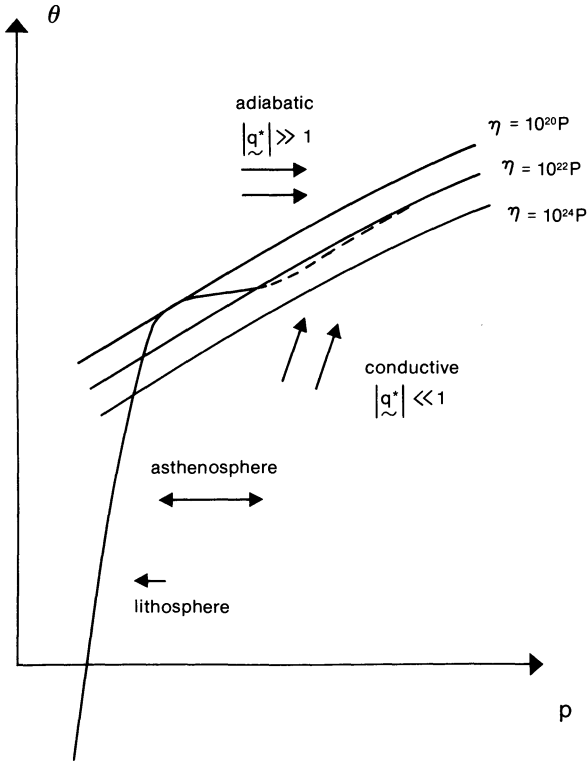
Fig. 1. Control of the temperature by the viscosity. Below the 'asthenosphere' the temperature becomes non-adiabatic and follows approximately the dotted (isoviscous) profile, thereby implying an approximately constant viscosity *and* a more reasonable geotherm in conformation with constraints at the core-mantle boundary

ities $\sim 10^{22}$ P and $\sim 10^{24}$ P, if $\delta \sim 10^{-1}$. Again, these are for illustrative purposes only (see Fig. 1). Choose $\eta_a = 10^{20}$ P, so the three profiles correspond to $\eta^* \sim 1$, $\eta^* \sim 1/\delta^2$, $\eta^* \sim 1/\delta^4$. Then in the asthenosphere, $\eta^* \sim 1$, so $|\mathbf{q}| \sim 1$ and $\theta \approx \theta_{ad}$. We make the (constitutive) assumption that (for small p, at least) $d\theta_{ad}/dp < d\theta_{iso}/dp$. Then as p increases, η^* increases till θ intersects θ_{iso} for $\eta^* \sim 1/\delta^2$. At this point $|\mathbf{q}| \sim \delta^2$, and putting $\mathbf{q} = \delta^2 \mathbf{q}^*$, we have

$$\mathbf{q}^* \cdot \nabla \theta - D\theta \mathbf{q}^* \cdot \nabla p = \nabla^2 \theta, \tag{3.2}$$

$|\mathbf{q}^*| \sim 1$ (if $\tau \sim 1$) and the temperature is not necessarily adiabatic: the question is, what is it? To answer this, we need to solve (3.2), together with the other equations: this would not be easy. However, we can very easily say what the solution is *not*. Essentially $\eta \sim 10^{20}$ P represents a barrier above which $|\mathbf{q}^*| \gg 1$ ($\gtrsim O(1/\delta^2)$), and hence the temperature there would be adiabatic. Equivalently $\eta \sim 10^{24}$ P represents a barrier below which $|\mathbf{q}^*| \ll 1$ ($\lesssim O(\delta^2)$), and hence in that case, one could ignore the *left* hand side of (3.2) and have a purely conductive profile. Since the heat flux through stagnant zones must equal the heat flux through the surface, one would have the result

$$\frac{\partial \theta}{\partial p} \approx \frac{\partial \theta_{cond}}{\partial p} \sim O\left(\frac{1}{\delta}\right) \tag{3.3}$$

from (3.1). If we make the constitutive assumption that

$$\frac{\partial \theta_{ad}}{\partial p} < \frac{\partial \theta_{iso}}{\partial p} < \frac{\partial \theta_{cond}}{\partial p}, \tag{3.4}$$

then thermal profiles above the $\eta \sim 10^{20}$ P barrier tend to pass below it, while those below the $\eta \sim 10^{24}$ P barrier tend to pass above it, as shown in Fig. 1. Since (3.4) is realistic for the mantle (e.g. $\partial \theta_{ad}/\partial p \sim 0.5$ K km^{-1}, $\partial \theta_{iso}/\partial p \sim 0.7$ K km^{-1}, $\partial \theta_{cond}/\partial p \sim 1.2$ K km^{-1}), assumption of (3.4) leads to the conclusion that the temperature will *self-regulate* itself so that the viscosity lies in a narrow band centred on (in our case) 10^{22} P. We could in fact reasonably restrict this band to be 10^{21} P $< \eta < 10^{23}$ P *below* the asthenosphere. The low viscosity zone $\eta \sim 10^{20}$ P is essential so that there is a viscosity contrast of $O(1/\delta^2)$ across it. The importance of the smallness of ε now becomes manifest. If $\varepsilon \ll 1$, then the width of the θ-band is $O(\varepsilon \ln(1/\delta))$, and θ is therefore quite well-constrained by this restriction.

Since $\theta \approx \theta_{iso}$ below the asthenosphere, we are quite well able to satisfy basal temperature constraints for reasonable V^*, E^*. In fact, using η determined by (1.1) with the same values as Table 1 for A, τ and E^*, we find that (with $\eta = 10^{22}$ P) for $V^* = 4.5$ cm^3 mole^{-1}, $T_b = 2,900$ K, for $V^* = 5.4$ cm^3 mole^{-1}, $T_b = 3,200$ K, fairly reasonable given uncertainties in E^*, V^* at these depths, as well as ignoring increases due to phase changes and a *small* (in temperature jump) basal thermal boundary layer.

The above idea, that the rheology controls the temperature, is not new, as Tozer (1967, 1972, 1977) has proposed a similar idea; however, apparently (Tozer, 1967) the idea in that form assumed that the interior viscosity was constant (in space), and then stated that this assumed constant viscosity controlled the basal temperature. This essentially deals with the case of an isothermal interior (no adiabatic heating, $D = 0$) and a temperature dependent viscosity (no activation volume, $\mu = 0$). Here we deal with the control of temperature *within the convecting cell* by a non-adiabatic temperature, altogether a different notion: the present discussion would also, for example, be applicable to the case $D = 0$, $\mu \neq 0$.

Although the reasoning leading to Fig. 1 seems elementary and logical, there is one disquieting feature: if the temperature is closely constrained by the rheology, what does the temperature equation solve for? To examine this question we need a simple *model equation* which exhibits the same essential physical process which is of concern. To this end, consider the second order ordinary differential equation

$$V[\theta_z - D\theta] = \theta_{zz}, \tag{3.5}$$

where

$$V = f\left[\frac{\theta - \bar{\theta}(z)}{\varepsilon}\right], \qquad \varepsilon \ll 1; \tag{3.6}$$

here z is depth, and we choose boundary conditions such as

$$\theta(0) = \theta_0 \in (0, 1), \qquad \theta(1) = 1. \tag{3.7}$$

The velocity V is chosen as in (3.6) for the reason that when $\theta < \bar{\theta}(z)$ (the isoviscous profile of relevance) then

we expect V to decrease (η^* increases) and if $\theta > \bar{\theta}$, V increases; we can think of f as $= 1/\eta^*$, and a measure of fluidity. For example, we might choose $f(\phi) \propto \exp[\phi]$.

Previous discussion in this section suggests the following solution for θ; firstly $\theta \approx \bar{\theta}(z)$ (for large enough z); assuming the adiabat is more slowly increasing than the isoviscous profile $\bar{\theta}$ (as we suppose), then (3.5) implies

$$V = \frac{\bar{\theta}_{zz}}{\bar{\theta}_z - D\bar{\theta}} = \bar{V}(z), \qquad (3.8)$$

a given function. Consider first $\bar{V} \neq 0$, i.e. $\bar{\theta}$ is *not* linear in z; then $\bar{V} \sim O(1)$ (generically), and (3.6) gives the first correction to θ as

$$\theta = \bar{\theta}(z) + \varepsilon f^{-1}[\bar{V}(z)]; \qquad (3.9)$$

further terms may be found similarly. Secondly, suppose $\bar{V} = 0$; this corresponds to $\bar{\theta} = 1 + \mu z$, μ constant (i.e. V^* constant), and is at first sight puzzling, since by assumption a conductive profile is steeper than an isoviscous one. The resolution, of course, is that $\theta - \bar{\theta}$ is not zero precisely: in fact, choosing

$$f(0) = \varepsilon, \qquad (3.10)$$

(as can always be done by shifting $\bar{\theta}$ up or down slightly, e.g. by $\varepsilon \ln(1/\varepsilon)$ if $f(\phi) \propto \exp(\phi)$), putting

$$f(\phi) = \varepsilon g(\phi), \qquad g \sim O(1), \qquad (3.11)$$

and writing

$$\theta = \bar{\theta}(z) + \varepsilon \phi, \qquad (3.12)$$

we find that (3.6), (3.5) and 3.11) give

$$g(\phi)[h(z) + O(\varepsilon)] = \phi_{zz}, \qquad (3.13)$$

where

$$h(z) = \bar{\theta}_z - D\bar{\theta} > 0 \qquad \text{by assumption,}$$

$g > 0$ is monotonically increasing (e.g. $g = e^\phi$). (3.14)

We thus solve

$$\phi_{zz} = g(\phi) h(z), \qquad (3.15)$$

together with boundary conditions at $z = 1$ and $z = c$ on ϕ, where $z = c$ is where the adiabatic asthenospheric solution first intersects the isoviscous profile $\bar{\theta}$. It is easy to prove (almost by inspection) that (3.15) is a well-posed two-point boundary value problem, so that again (3.12) provides the correction to $\bar{\theta}$. Thus in either case, the net effect of the energy equation is to calculate at leading order the $O(\varepsilon)$ correction to the isoviscous temperature profile. If we (as seems reasonable) transmit this back to the real problem, we should infer that $\theta - \theta_{iso} = O(\varepsilon)$ below the asthenosphere, and thus that viscosity variations in the lower mantle truly are no more than $O(1)$, as observed.

Discussion

In this paper we suggest that the temperature profile in the mantle regulates itself in such a way as to produce
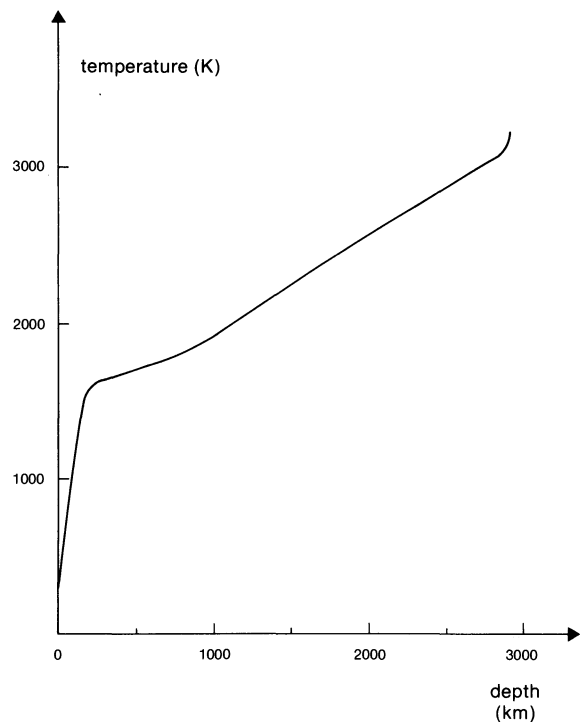

Fig. 2. One possible geotherm for the mantle. This is intended to be schematic, since (for example) it would depend numerically on a full solution of the governing equations, but more importantly on the precise form of V^* and E^* as functions of p and T. We have illustrated a lithospheric thermal boundary layer, a vigorous low viscosity asthenosphere/upper mantle, a less vigorous isoviscous lower mantle, and a small basal thermal boundary layer, in which θ jumps by (probably) $O(\varepsilon)$ in a small region. This basal layer 'gets the heat out of the core'

a relatively constant lower mantle viscosity, and that this temperature is essentially isoviscous, i.e. non-adiabatic, in the lower mantle. A typical geotherm of this type is shown in Fig. 2. Such a geotherm is computed on the basis of fairly elementary reasoning, nevertheless we emphasise that the temperature in the lower mantle may be non-adiabatic, since we propose that the convective velocity there is small ($\lesssim O(\text{mm/yr})$) due to the strong temperature (and pressure) dependence of viscosity. This geotherm seems to fit geophysical constraints better than other (adiabatic) models (e.g. O'Connell and Hager, 1980), and additionally *predicts* a mantle viscosity which is relatively constant *below* the asthenosphere, and a low viscosity zone (asthenosphere) between the lithosphere and lower mantle, in which the viscosity decreases by (say) a factor of 50. Our reasoning is based on a simple scaling analysis of the energy equation, coupled with consideration of a realistic viscosity law.

In the course of our analysis, we are led to reconsider the rôle played by boundary layers and adiabats in vigorous, thermoviscous convection. In supposing that a combination of such thermal profiles can provide a complete understanding of the geotherm is, we suggest, misleading, and in fact, ignoring the effect of a strongly temperature and pressure dependent rheology may lead to gross misconceptions of the nature of mantle convection. We cannot emphasise this too

strongly, since, apart from suggestions of Tozer (1972; 1977), and a variety of numerical computations (Kopitzke, 1979; Schmeling and Jacoby, 1981), much weight has been placed on conclusions of constant viscosity calculations, with only minimal considerations being given to the real rheology (Peltier, 1980).

Part of the concept of this paper has been that Jeanloz and Richter's (1979) argument concerning basal or internal boundary layers is unrealistic since temperature jumps much greater than, say, 200 K across such layers are not feasible, as such jumps entail a viscosity decrease of at least an order of magnitude, which would be expected to be highly unstable. If this is correct, then an explanation of the seismic D'' layer at the base of the mantle in terms of such a comparatively small temperature jump is necessary. Decisive estimates of this jump do not seem available: O'Connell and Hager (1980) quote 300 K, which would be at the upper end of our allowable range.

We have not discussed either phase changes or chemical discontinuities in the mantle. This omission is partly intentional, since the aim is to lay extremely heavy emphasis on the dynamical effect of a varying rheology on the convective style of the mantle, and we actually do not imagine that phase changes, at least, will have a major influence on this. Chemical discontinuities have been invoked (De Paolo, 1981; Anderson, 1982a, b) to explain heterogeneous geochemistry in varying kinds of surface rocks, the idea being that a layered upper and lower mantle would convect separately, consistently with this viewpoint. We do not wish to enter the layering argument here, beyond pointing out firstly, that Peltier (1980) has already observed that increasing the dissipation number D in a constant viscosity fluid can lead to layered convection (he omitted to extend his observation to the mantle), and secondly, the style of convection implied by our discussion already has a 'layering' implicit in it. We have a 'rigid' lithosphere, a 'fluid' asthenosphere (e.g. $\eta \sim 10^{20}$ P, $|\mathbf{u}| \sim 10$ cm yr^{-1}) and a 'sludgy' lower mantle (e.g. $\eta \sim 10^{22}$ P, $|\mathbf{u}| \sim 0.1$ cm yr^{-1}). Whether the circulatory pattern is actually segregated is unclear, but certainly, a direct implication would be that the sampling of lower mantle material at the surface will be comparatively rare. Of course, this is an approximate discussion, and an examination of the origin of thermal plumes/upwellings is required before any specific opinion is stated. We simply wish to point out that variable-viscosity dynamics appears to carry with it a self-imposed style of 'layering' so that arguments about vigorous whole mantle convection (Davies, 1977; O'Connell, 1977) need to be critically re-examined.

In conclusion, some currently held 'truths' concerning mantle convection should be re-evaluated in the light of the implications of a strongly temperature and pressure dependent rheology.

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