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# Reply

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At the outset, let me note that Dr. Christensen is willing to accept the *idea* of a superadiabatic lower mantle, and, since the expression of the idea was the main point of my paper, I might let the issue rest there. However, Dr. Christensen claims that even if the idea could *in principle* be valid, it is inapplicable to the Earth, as the numbers do not come out right. To an extent, this contention must remain a matter of interpretation, since my argument is (necessarily, at this stage) only a semi-quantitative one. On the other hand, I actually think that each of the four arguments he advances against my conclusions is wrong and insupportable, and so I will here try to elaborate why I think this to be so.

## (1) The Depth Scale

At issue is the choice of an "upper mantle" depth scale $d_u$(=700 km, say) rather than a "lower mantle" scale $d_l$(=3,000 km, say). The choice of $d_u$=700 km (or 500 km or 1,000 km) is actually not made because it "[leads] to the desired result", but because of other considerations (Fowler, 1982a, b) which suggested that an upper mantle vigorous convection was more likely; but in fact, it is clear from the argument in my paper (Fowler, 1983), and its conclusion, that the natural depth scale turns out to be $d_u$, where $d_u$ is the depth at which the 'switch' from adiabatic to isoviscous is supposed to take place.

However, the clearest rebuttal may be to do as Dr. Christensen asks, and scale the vertical coordinate with $d_l \approx 3{,}000$ km. With reference to Fowler (1983), we will denote the corresponding $\delta=\delta_l$, etc. Now let us proceed with the argument of the paper. Since $\delta_l \lll \delta_u$ ($\delta_u$ computed using a putative $d_u$), we still have an adiabatic sub-lithospheric temperature $\theta \approx \theta_{ad}$ (given by (2.18) of Fowler, 1983), and corresponding $\eta \approx \eta_{ad}$. Accepting (for the moment) that $|\mathbf{q}| \sim 1/\eta$ (see point (2) below), then the transition depth from adiabatic to isoviscous will be at a *dimensionless* (with $d_l$) depth such that

$$\eta_{ad}=\exp\left[\frac{1+\mu p-\theta_{ad}}{\varepsilon\theta_{ad}}\right] \sim \frac{1}{\delta_l^2}, \tag{1}$$

where

$$\theta_{ad}=\exp\left[\int_0^p D(p)\,dp\right] \approx 1+Dp \tag{2}$$

for constant $D$ and small enough $p$ (this does not affect the argument); this can be adequately approximated by

$$(\mu-D)\,p \sim 2\varepsilon \ln\frac{1}{\delta_l}, \tag{3}$$

for $\varepsilon$ small enough (as is being assumed, i.e. $\varepsilon \to 0$). If we assume rather that $|\mathbf{q}| \sim 1/\eta^b$ (e.g. $b \leqq 1$), then (3) is

$$(\mu-D)\,p \sim \frac{2}{b}\varepsilon \ln\frac{1}{\delta_l}. \tag{4}$$

It is instructive to write (4) dimensionally, denoting the value of the depth determined by (4) as $d_u$. Then (2.5), (2.8), (2.9) and (2.10) of Fowler (1983), together with a lithostatic pressure, lead to

$$\frac{d_u}{d_l} \sim \frac{2\varepsilon}{b}\left[\frac{d_l}{d_1}\right]^{-1} \ln\left[\frac{d_l}{d_2}\right], \tag{5}$$

where

$$d_1=\left[\frac{\rho_0 g V^*}{E^*}-\frac{\alpha g}{c_p}\right]^{-1},$$
$$d_2=(\kappa l/U)^{\frac{1}{2}}; \tag{6}$$

for the Earth, we compute $d_1 \approx 2{,}300$ km, $d_2 \approx 50$ km, with usual parameters. If we assume (as the problem suggests) $d_l \gg d_2$, $d_l \sim d_1$, $\varepsilon \ll 1$, then (5) implies that firstly $d_u < d_l$, and in addition for *larger* $d_l$, $d_u/d_l$ actually *decreases*. This is true for mantle-type values of $\varepsilon \lesssim 1/20$, $d_l \approx d_1$, $d_l/d_2 \approx 60$, and remains so even if we take $b \approx 1/2$, as recommended by Dr. Christensen (see point (2)). But if $d_u < d_l$, in particular if $d_u \ll d_l$, then since this will imply a restriction of vigorous convection to a shallow depth range $\lesssim d_u$, it means that $d_u$ *is* the correct scale for isolating the adiabatic-isoviscous transition.

The subsequent argument in my paper is essentially a *reductio ad absurdum*, and I cannot offhand see any logical fault in it. Dr. Christensen's further comment is that if $|\mathbf{q}| \sim \delta^2$ at the transition zone (this is $\delta_u^2$), then advection must dominate in the lower mantle, since *its* relevant (dimensionless) depth scale is $y \sim \delta_u/\delta_l$ (compared to the upper mantle): i.e., in the lower mantle one should write

$$y=(\delta_u/\delta_l)\,y^*, \qquad \mathbf{q}=\delta_u^2\mathbf{q}^*, \tag{7}$$

leading to

$$\mathbf{q}^* \cdot \nabla\theta \ldots = (\delta_l/\delta_u)^2 \nabla^2 \theta, \tag{8}$$

so that adiabaticity is still assumed (since $\delta_l \ll \delta_u$); however this is not the appropriate conclusion: rather, one concludes that (7) is inappropriate for the lower mantle, and some other scaling is relevant. My argument is that once $|\mathbf{q}|$ *gets* to $O(\delta_u^2)$, adiabaticity is no longer guaranteed, and the rest of the argument consists of looking at Fig. 1: I do not offer any precise scaling appropriate to the lower mantle. This was why I considered the *model* problem (3.5), in order to try and understand what the equations could mean in an isoviscous régime.

### (2) Velocity-Viscosity Relation

The point at issue here is whether $|\mathbf{q}| \sim 1/\eta^b$ as $\eta$ increases, with $b=1$ (my assumption), or $b<1$. The outcome does not affect the argument, but only its quantitative interpretation, particularly if one feels constrained by independent mantle viscosity estimates (e.g. Cathles, 1975). Dr. Christensen contends that my 'arguments to defend this relation [that $b=1$] are not conclusive'. On the contrary, I hardly need to defend it, since it is explicitly written in the constitutive relation, particularly in its lubrication theory form (as is *a posteriori* relevant here, for shallow aspect ratio flows). Rather, if $b<1$, one would need sound numerical evidence to establish any other *asymptotic* value of $b$, and even then some convolved theoretical argument to explain it.

But such numerical evidence as there is does not lend itself to any *quantitative* relation about a purported value of $b$, beyond the general premise that $|\mathbf{q}|$ generally decreases with $\eta$. McKenzie's (1977) results treat a highest viscosity contrast of 110, in an order-one aspect ratio; Torrance and Turcotte's (1971) results are also for an order-one aspect ratio, free slip boundary conditions; Davies' (1977) model is of *marginally* unstable *linear* modes in a *piecewise discontinuous* viscous model. I might accept qualitative indications from these papers (e.g. the appearance of stagnant zones in Torrance and Turcotte's results): but it is hardly realistic to take any quantitative conclusions from them. I might even quote Davies (1977) in my own favour: although a viscosity contrast of $10^4$ gives very little velocity contrast, a contrast of $10^5$ gives a totally stagnant 'lower mantle'! But the result is simply not quantitatively relevant in the present context. Concerning Dr. Christensen's own recently obtained results, I am unable to comment; in view of his other adduced 'evidence', however, I am for the moment inclined to remain skeptical.

As for stress and vorticity, the stress generated by thermal plumes in high Rayleigh number constant viscosity convection is actually equal to the (viscosity times the) vorticity (Roberts, 1979). I am well aware what stress and vorticity are. Dr. Christensen suggests internal stress concentrations "may in fact occur". Since internal (in a steady state) means away from plumes (upwellings, downwellings) and thermal (top and bottom) boundary layers, I am enthusiastic to hear how he is going to do this.

### (3) Asthenospheric Viscosity

The choice of a controlling function for asthenospheric viscosity is actually made on the basis of a separate analysis (Fowler, 1982b), but in the present context it is clear that it is the appropriate self-consistent choice of viscosity scale. I do not say that the plate *velocity* is controlled by the asthenospheric viscosity (see Eq. (2.14a) and preceding discussion), but in view of the purported nature of the flow, one needs substantial return flow in the upper mantle. This requires the balance (2.12) or (2.20), so that one can have $\int_0^{d_u} u\,dz \approx 0$, thus conserving mass. The *shear rate* is controlled by the asthenospheric viscosity. The plate-driving analyses of Forsyth and Uyeda (1975) and Davies (1978) are *kinematic* discussions of plate forces, with nothing to say about sub-lithospheric dynamics; they are not of relevance in the present context.

### (4) Lower Mantle Instability

At issue here is whether the proposed super-adiabatic lower mantle is unstable, not in a normal dynamic sense (e.g. boundary layer plumes, or oscillatory instabilities which do not alter the *basic* structure), but in a catastrophic sense, in the same way (and analogously to the situation) that a conductive temperature profile is catastrophically unstable when the Rayleigh number is large.

Firstly, the Rayleigh number criterion applies to *linear* instability of a motionless, conducting state. Since this does not pertain here, one had better discuss the use of large 'Rayleigh' number somewhat adeptly. This, Dr. Christensen acknowledges. Nevertheless, the use of linear stability theory to analyse a slowly varying basic state can be justified (in principle, by the method of multiple scales: Kevorkian and Cole, 1981) provided the growth or decay time of instability is much faster than the slowly varying time scale: this is called the frozen time hypothesis, and has been used in convection studies by Robinson (1976), amongst others. Unfortunately, Dr. Christensen's linear stability results do not concern the problem at hand: (i) he assumes a constant viscosity *fluid* (exactly what I do not have); (ii) his basic state is one of *no* motion (also exactly what I do not have).

Nevertheless, suppose that in some sense, the result is correct; that is, an infinitesimal disturbance to the lower mantle starts to grow very fast. Let us imagine that this disturbance consists of an initial upward movement of a parcel of fluid which is slightly lighter than its surroundings. If the instability is *fast*, then the parcel's temperature changes adiabatically. Correspondingly, its viscosity changes 'adiabatically', and consultation of Fig. 1 in Fowler (1983) reveals that its viscosity increases rapidly and exponentially, whereas the driving stress (the excess buoyancy compared to its surroundings) increases slowly and linearly. Viscosity wins, and the parcel velocity becomes quickly limited. In fact it becomes limited to a value such that the viscosity stays roughly constant. In other words, I am suggesting that any purported rapid linear instability that *might* occur is effectively limited at small finite amplitude by the temperature dependence of viscosity, and in fact that this amplitude is just that which I in any case propose that the lower mantle should have. A similar case of effective stability appears in the glaciological context (Fowler and Larson, 1980), where it is computed explicitly.

The discussion above is certainly no proof, but it does at least consider the problem in its correct physical context. I would not claim that the lower mantle *is* necessarily stable, but Dr. Christensen's claim that this argument 'defeats [my] thesis' is an inflated one.

Much of my viewpoint, as expressed above, is based on the notion that the ideas are self-consistent, and 'hang together'. To a geophysicist, this may seem a circular way of reasoning, but anybody who is at all familiar with the methods of singular perturbation theory will recognise this self-consistency as the hallmark of the procedure, and a sign that it 'works'. I would not guarantee that the proposed geotherm is necessarily of the correct form, and there are certainly problems (the 'how do you get the heat out of the core?' question may be one), but I haven't been able to find anything better, which is itself not in contradiction to the assumed equations describing the flow.

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