typically plastic traits in natural human populations, I concluded that meaningful heritability estimates cannot now be obtained for such traits.

Page argues that assuming genotype and environment to be correlated is tantamount to granting the hereditarian thesis that "the higher SES groups are, already, innately smarter than the lower SES groups." To pinpoint the fallacy in this argument, consider a phenotypically plastic trait that is easier to define and measure than intelligence: proficiency in the game of squash. Few people will deny that this proficiency is correlated with genetic factors (for example, genes specifying a predilection for strenuous forms of exercise). It is also undeniable that the general level of proficiency at squash is substantially greater among students and graduates of Ivy League colleges than among students and graduates of the Big Ten. Page and the authors whose views he cites with approval would, I hope, reject a genetic explanation for this systematic difference. Why, then, do they persist in interpreting systematic behavioral differences between social, economic, and racial groups as evidence for systematic genetic differences?

I thank Kruskal for his clarification.

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The Lower "Petrologic Geotherm": A Transitory State

In a recent conceptual advance, MacGregor and Basu (*1*) have presented a "petrologic model" of the geotherm in the upper 200 km of the earth. This work is important for at least three reasons: (i) it is based on concepts different from those previously used in modeling thermal structure in the earth and so provides an independent check on these concepts; (ii) it is in general agreement with the earlier models for the upper 140 km (although with interesting changes of detail), thus deepening our understanding of this region; and (iii) it reveals a new feature (a steepening of the geotherm) below 140 km beneath continents, thus inaugurating a new discussion of the thermal structure in this region. Although MacGregor and Basu are aware that this feature cannot reflect the steady state, and although two transient mechanisms (*2, 3*) are mentioned, the experimental data are interpreted as a petrologic *model* of the geotherm, applicable to a typical subcontinental tectonic setting, and generalizable (with suitable evolutionary modification) to other such areas. My purpose in this technical comment is to argue, on very simple grounds, that the "geotherm" represented by the lower part of the petrologic model must represent an extremely unusual state of the asthenosphere and cannot represent any steady evolutionary development applicable to other times or places, especially to the "typical" state of the upper mantle.

The conservation of energy, in the case of heat flow in one dimension, is expressed as

$$\rho\, C_P \frac{dT}{dt} = \frac{dF}{dz} + Q \qquad (1)$$

where ρ is the density, C_P is the specific heat, T is the temperature at depth z and time t, F is the vertical heat flow, and Q is any heat source density. Equation 1 is valid for all materials (for example, inhomogeneous plastics) if F is suitably defined (below). The one-dimensional case is sufficient for this problem, as the indicated (*1*) horizontal temperature gradients are less than the vertical gradients by more than an order of magnitude. The heat flow is given by

$$F(t,z) = k_s \frac{dT}{dz} + F_{conv}$$

where the conductive term (the first term on the right) depends upon the thermal gradient and k_s, the thermal conductivity in the stationary state. The convective contribution F_{conv} is indicated only symbolically; it is always positive or zero. Hence

$$F(t,z) \geqslant k_s \frac{dT}{dz} \qquad (2)$$

in both liquids and solids. It has been shown (*4*) that k_s increases slowly in the upper 400 km, being always greater than 3×10^5 centimeter-gram-second units.

One can apply these equations in an elementary way to a temperature distribution showing upward curvature, as in the "petrologic model" (*1*). Neglecting possible heat sources Q for the moment, Eq. 2 substituted in Eq. 1 yields

$$\frac{dT}{dt} > \frac{k_s}{\bar{\rho}\, C_P} \frac{\Delta(dT/dz)}{\Delta z} \qquad (3)$$

Using a change of gradient $\Delta(dT/dz)$ of 16°C per kilometer over a depth interval Δz of 30 km [suggested by the data (*1*)] in Eq. 3 yields a minimum value $dT/dt > 10^{-5}$ °C per year. This indicates that temperature excesses (over the extrapolated lithospheric geotherm) of the order of 100°C would decay away (by heating of the lithosphere) in a maximum of (10 to 20) $\times$ 10^6 years and possibly sooner; that is, if the petrologic model geotherm does represent true paleotemperatures just prior to surface emplacement (some 100×10^6 years ago) of the corresponding ultramafic rocks, that thermal structure has long since smoothed itself out. Conversely, it could not have existed for more than a few million years prior to the emplacement event without conductively heating up the lithosphere and removing the inflection. Hence it must be considered an extraordinary situation, not part of a steady evolutionary development, and not generalizable to other areas in similar tectonic settings (for example, similar distances from spreading centers). In effect, it constitutes petrographic evidence of a transient (or mobile) anomalously hot spot in the mantle rather than a representative geotherm.

The neglected source term Q does not affect this conclusion. The time scale for the conduction of heat into the lithosphere does not depend on the source of the heat, be it convectively transported from below or internally generated by radioactivity or viscous dissipation. Only a negative heat source (a heat sink) at the top of the asthenosphere could maintain a concave-upward geotherm for significant times. The only heat sinks available are endothermic chemical reactions, such as melting or dehydration, and descending diapirs. Considering first the endothermic reactions, the reaction rate required to maintain an inflection in the geotherm is easily calculable. It is more instructive, however, to estimate the steady-state rate of accumulation of reaction products, since this quantity is independent of Δz, the interval of upward curvature of $T(z)$. This production rate is easily shown to be

$$p > \frac{k_s}{L} \Delta(dT/dz) \qquad (4)$$

where L is the latent heat of the reaction. Using L = 100 cal/g (for the melting of forsterite), one calculates p > 0.3 g/year per square centimeter of horizontal area, corresponding to a column of reaction products growing continuously at about 1 mm/year. This rate is sufficient to produce a molten layer 10 km thick in only 10×10^6 years. Dehydration reactions would yield product columns growing at comparable or even greater rates. Such production rates are, of course, unacceptable; the conclusion remains that a concave-upward geotherm could be of only an extremely transient nature. An effective heat sink caused by a descending diapir is also transient, a priori.

Hence, the lower part of the paleogeotherm inferred from petrological data must relate to a transitory tectonic event, immediately preceding surface emplacement [for example, see (*2*)], and not representative of any steady evolutionary development in southern Africa or any other Precambrian shield. The frequency of such events may be estimated by the frequency of occurrence of kimberlite pipes, that is, a few events per 10^6 km^2 per 10^7 years. It is not surprising that these events should fail to have been reflected in earlier thermal models.

Alternatively, it seems reasonable that the partition of aluminum and calcium between coexisting pyroxenes might depend upon shear strain, as well as upon pressure and temperature, through distortion of the crystal field. If so, then the pressures and temperatures (*1*) for the asthenosphere would be in doubt. However, I am not aware of any work which establishes the existence or importance of such an effect. Further discussion of petrologically determined temperatures is contained in a recent review (*5*) of a special session of the American Geophysical Union.

Finally, a long-lived *downward* inflection of the geotherm at the top of the asthenosphere is easily understood as a sudden increase in F_{conv} (corresponding to the onset of convection) with a corresponding decrease in dT/dz, maintaining a near-constant total flux. Similarly, an upward inflection at the bottom of the convecting zone (*6*) is a reflection of the disappearance of F_{conv} upon passage from "fluid" to solid, with an increase of the thermal gradient necessary for the near-equality of heat flow.

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Erythrocytes in Human Muscular Dystrophy

The observations of Miale *et al.* (*1*) and Matheson and Howland (*2*) appear to represent a conflict with respect to the adequacy of scanning electron microscopic analyses of erythrocytes in detection of heterozygote carriers of Duchenne muscular dystrophy. Miale *et al.* stated that they were unable to detect heterozygotes in their study; Matheson and Howland (*3*) replied that an inadequate sample was employed and the methods of cell preparation may be critical. The latter cited contributions from our laboratory, presented in part at the Third International Congress on Muscle Diseases, as evidence in favor of the diagnostic value of scanning electron microscopy (SEM). However, the citation was somewhat misleading.

We agree that the methods of preparation are critical. It is apparent from the work of Miale *et al.* as well as our own that Matheson and Howland's method does not give easily reproducible results. The data we presented at the congress do not exactly confirm their results. With rigidly defined conditions in which the cells are unwashed and fixed immediately in solutions controlled with respect to pH, ionic strength, and osmolarity, we were able to demonstrate stomatocytic changes in Duchenne carrier erythrocytes but unable to demonstrate the echinocytic changes described by Matheson and Howland. Furthermore, the stomatocytic shapes were not specific for Duchenne dystrophy or the carrier state, but were noted in other muscular disorders including myotonic muscular dystrophy, limb-girdle dystrophy, and myotonic congenita, as well as in a person with no clinical evidence of muscle disease (*4*).

Thus, despite our findings of altered erythrocyte morphology in Duchenne carrier states, we cannot agree with Matheson and Howland that SEM can be used to detect the carrier state. The changes observed by SEM, which are obviously in vitro artifacts produced by the fixation procedure, are insufficient to establish or confirm the diagnosis, although they do implicate a subtle membrane defect. Other data support the expression of the metabolic defect in membranes of many different tissues, especially in myotonic muscular dystrophy (*5*). When combinations of these metabolic alterations are used in conjunction with the clinical state, the detection of carriers is made more definitive. We agree that the use of erythrocytes as a model is an important approach to the study of various muscular dystrophies that have widespread expression in membranes from different tissues. However, we do not believe that any single parameter is sufficiently specific at the present time to be used as a diagnostic tool.

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