presentations and the work load he imposes on his students; a shortage of one can be compensated by an increase of the other.

Because the ratings were obtained after the students had received their final grades, it might be argued that the statistical associations are an artifact of the students' reactions to their grades, such as a desire to "get even" with instructors who give them poor grades. This "retaliation hypothesis" can be tested in the data in two different ways:

Students who received low grades might have been more likely to return the mail questionnaire and also more likely to give their instructor low ratings. But in fact, the average nonregressed exam grade for the 222 students in Introductory Calculus who returned the rating form was 84.2 and for the 81 who did not was 72.6. This difference is opposite to that predicted by the "retaliation hypothesis" and is highly reliable ($t = 5.16$, d.f. $= 301$, $P < .001$). Similar data were observed in Multidimensional Calculus: an average grade of 73.8 for the 132 students who responded and 63.3 for the 56 nonresponders ($t = 3.93$, d.f. $= 186$, $P < .001$).

Second, if grades are causally related to ratings independent of the teacher's performance, there should be a high positive correlation within each class between a student's final exam grade and his ratings of the instructor. In the present study these correlations have been calculated for the two factors showing the highest interrater agreement, work load and teacher's presentation. They range from $-.33$ to $+.43$ with an average value of $-.02$; five are positive and eight are negative. These outcomes are not consistent with the hypothesis in question; there is no evidence for a strong positive relationship between final exam grades and the ratings when the effect of the different instructors is removed.

A reasonable explanation for the differences between my results and those of Rodin and Rodin can be formulated by considering the differences in our methodologies. The negative relationship they observed may be a unique outcome which was highly dependent on the principal lecturer's teaching style and the way this style affected the performance of his teaching assistants. Second, the Rodins' rating measure required the students to make a global judgment about teaching performance whereas my questions focused on more discrete aspects of teaching and on observable behaviors. I believe that the very strong relationships in my study resulted from a successful effort to categorize student ratings in terms of specific factors and thus to be able to separate more useful from less useful ratings. Further research with separate factors might make it possible to identify the important aspects of teaching in particular fields.

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5. Supported by the Center for the Teaching Professions, Northwestern University, which is funded by the Kellogg Foundation. I thank B. Claude Mathis, director of the center, for helpful suggestions and encouragement.

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Origin of Mitochondria

In relation to the recent comments between Uzzell and Spolsky (*1*) and Raff and Mahler (*2*), I wish to propose (*3*) that the primitive phagocyte in which bacterial ancestors of mitochondria allegedly settled some 1.5 billion years ago was actually an aerobic cell that relied on peroxisomes instead of on a phosphorylating electron transport chain for its respiratory metabolism. This hypothesis was formulated mainly in an effort to retrace the evolutionary history of the peroxisome, a particle which certain facts suggest may have been of much greater metabolic importance in early eukaryotes than it is in many plant and animal cells today. Acquisition of the more efficient mitochondria was put forward as an explanation of the evolutionary decline of the peroxisome. By the same token, possession of a primitive respiratory system would have made acquisition of mitochondria advantageous even in an aerobic cell. Thus the objection that "the aerobic nature of the ancestral protoeukaryotic cell would make the acquisition of an aerobic symbiont unnecessary" (*2*) loses much of its pertinence.

According to my hypothesis, both the endosymbiont and its host are pictured as originating from primitive aerobic bacteria endowed with a peroxisomal type of respiration ($O_2 \rightarrow H_2O_2 \rightarrow 2\ H_2O$), one evolutionary line leading to the development of a respiratory chain and of coupled phosphorylating systems, the other to the acquisition of phagocytosis and intracellular digestion, proliferation of intracellular membranes, and an increase in cell size. It will be noted that these two evolutionary lines correspond to two distinct, and possibly mutually incompatible, differentiations of the cell membrane.

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Internal Gravity Wave–Mean Wind Interaction

Bekofske and Liu (*1*) have demonstrated that the interaction of a vertically propagating internal gravity wave (IGW) with the background wind shear near a critical level (where the mean wind speed equals the phase speed of the IGW) can increase the background wind shear sufficiently to satisfy the criterion for Kelvin-Helmholtz instability. Breaking of the resulting Kelvin-Helmholtz waves would then be expected to produce clear air turbulence (CAT). This mechanism is indeed a plausible source for some CAT. However, the idea is not a new one (*2*).

Numerical calculations quite similar to those in (*1*) have previously been reported by Lindzen and Holton (*3*) in their study of the quasi-biennial oscillation in the mean zonal wind in the equatorial stratosphere. Lindzen

and Holton, however, considered a multichromatic wave source so that it was unnecessary for them to invoke eddy viscosity to smear out the critical level singularity which occurs for a monochromatic wave. The depth scale for the critical layer in (*3*) was determined by the spectral distribution of the IGW's rather than by viscosity as in (*1*). Since atmospheric IGW's are generally multichromatic, Lindzen and Holton's model is likely the more realistic. Moreover, the assertion by Bekofske and Liu that a decrease of the Richardson number at the critical level allows a larger portion of the wave momentum to pass through without being absorbed is incorrect. It is shown in (*4*) that if the upward flux of momentum due to the IGW is a constant A far below the critical level, then an amount of momentum given by

$$A\{1 + \exp[-2\pi(\mathrm{Ri} - 0.25)^{1/2}]\}$$

is absorbed at the critical level. Here Ri is the Richardson number. Thus, the momentum absorption actually increases as the Richardson number decreases. The reason for this behavior is that the momentum flux changes sign at the critical level so that even though a larger fraction of wave energy passes through the critical level as the Richardson number decreases, the momentum flux convergence nevertheless must increase. Bekofske and Liu's contrary conclusion apparently arises from their failure to properly normalize the IGW momentum flux to remain constant (independent of Ri) far below the critical level. It should be added that at small values of Ri all analyses are somewhat questionable, but the assertion of Bekofske and Liu about momentum passage is impossible.

We do agree that a quasi-steady balance should eventually occur in which the mean wind shear is limited to the value for which $\mathrm{Ri} \simeq 0.25$ over the depth of the critical layer. However, this balance should occur not from decreased absorption of the IGW momentum flux but rather from increased momentum diffusion due to the turbulence produced by breaking Kelvin-Helmholtz waves.

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Dioptrics of the Periphery of the Eye

Leibowitz *et al.* (*1*) described an improvement in peripheral motion detection with correction of refractive error of the peripheral dioptrics of the eye. Retinoscopy was used on three subjects to determine the refractive error to eccentricities of 80°. When these errors were corrected they found a decrease in the motion threshold and in individual differences. They interpreted these data as implying that motion perception in the periphery was dioptrically rather than retinally limited, and they stated that the relative degradation of off-axis viewing is less for motion than for resolution and that as dioptric variables had been eliminated, the behavioral data and the relevant neurological substrate could be compared.

Their findings are at variance with those expected theoretically for light entering the optical system of the eye at an oblique angle (*2*, *3*). Taking a typical example, LeGrand (*2*) calculated that at 50° in the temporal field 7.52, diopters of astigmatism would be induced. At that same eccentricity Ferree *et al.* (*4*), using a refractometer, recorded an average astigmatism of 4.82 diopters for their type A (12 of 18 subjects) and 1.90 diopters for their type B. Leibowitz *et al.* (*1*) found a 0.50-diopter change in astigmatism at 50°. The theoretical data and both types described by Ferree *et al.* have progressively increasing amounts of astigmatism. Ferree *et al.* also recorded a change in axis of 90° in 16 of their 18 subjects. Thus, although Leibowitz *et al.* agreed with Ferree *et al.* in there being large refractive errors and wide individual differences, they did not demonstrate the increase in the amount of astigmatism or change in axis expected.

We repeated measurements of the refractive error by using retinoscopy, refractometry, and a subjective method on three subjects. We were unable to record repeatable values beyond 50° with retinoscopy and 60° with the refractometer and subjective methods. With the refractometer the mean refractive error was similar to that of type A in Ferree *et al.* There was a shift toward hyperopia with retinoscopy, as expected with the eye at a known and constant focus, and due to the error inherent in retinoscopy (*5*). The mean results for the three subjects were (*6*) $+2.58/-3.08 \times 90$, $+1.50/-2.92 \times 90$, and $+2.08/-3.50 \times 90$ for retinoscopy, refractometry, and the subjective method, respectively, at 50° eccentricity and when adjusted to give zero refractive error along the visual axis.

In the report by Leibowitz *et al.*, it would seem that either their subjects did not conform to the norm of theory and other experimenters, or insufficient attention was paid to the determination of the expected large astigmatism by the retinoscopist. Perhaps the marked improvement in the motion threshold obtained after correction might be attributed to training if testing without correction were always done before testing with correction. It is somewhat difficult to conceive that for subject TI of their study, at 50°, a refractive change of $0.13/-0.50 \times 15$ would affect the results to the extent shown. And these results are of the same order of magnitude as those obtained with larger changes, for example $+4.50/-0.50 \times 30$ for subject KS at 50°.

Thus, it is difficult to draw a conclusion linking behavioral data and the relevant neurological substrate on the assumption that optics have been eliminated.

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6. The notation $+2.58/-3.08 \times 90$ is an optical prescription which means $+2.58$ diopters spherical power and -3.08 diopters cylindrical power (astigmatism) at axis 90°.

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