Aside from the faulty use of these and other indicators, the authors are guilty of additional loose reasoning in reaching conclusions that are not warranted from the data. Using a much smaller sample of industrial nations ($N = 15$ to 19), the authors discover that there are fewer significant correlations between energy usage and indicators of life-style for this small sample than there are for all nations. Little wonder that it is difficult to reach a .05 level of significance with such a small sample when all of the previous analysis has been based on an N three times as large. Furthermore, the variance in energy consumption among these highly industrialized countries is not that great. The United States is by far the largest per capita consumer and the rest of these countries cluster remarkably close together, significantly restricting possibilities for significant findings. The authors ignore climatological differences and differing patterns of industrial and agricultural development that could readily account for any variance in energy usage, especially given comparatively small differences within this sample.

In summary, the authors make a point that surely needs to be made, that energy consumption and quality of life are not linearly related, but they make their argument so poorly that it loses its effect. Suffice it to say that they offer no proof that "so long as America's per capita energy consumption does not go below that of other developed nations, we can sustain a reduction in energy use without long-term deterioration of our indicators of health and health care, of education and culture, and of general satisfaction." Making this argument cogently requires much more data than the authors present. Surely social scientists are capable of bringing more exacting scholarship to bear on this very important question.

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Kruskal's "most serious" point about the inaccuracy of small-sample estimates of population parameters is certainly correct, but it is irrelevant here. Our "samples" are nearly complete populations of countries, so our "estimates" of correlation coefficients are nearly perfect.

He is again correct in pointing out that correlation coefficients are appropriate only for linear relationships. We assure him that we did do graphical checks for linearity, but it would have been cumbersome to present 224 graphs with the article, as he suggests.

We agree that it is not rigorous to apply significance tests to complete populations, however such tests are commonly used in this manner as a traditional criterion for distinguishing "small" from "large" correlation coefficients. (For sample sizes ranging from 15 to 55, as in our case, the boundary between "small" and "large" correlations ranges from $r = .24$ to .42, but it was usually about .3.) In retrospect, we would have been wiser to avoid significance tests, as they muddied the water, but the issue is a very minor one since all of the correlations deleted as "insignificant" from our table 2 are obviously small. For nations with developed market economies, which were the focus of our analysis, only 3 of the 70 "insignificant" correlation coefficients have an absolute magnitude exceeding .35; about half do not even exceed .20. It must surely be clear that, among developed market economy countries, most noneconomic indicators of life-style show low correlation with energy consumption. That, of course, was our main conclusion.

Fienberg vociferously objects to our treatment of extreme values for Canada and the United States, apparently ignoring our rationale (in our reference 6). He suggests that our treatment improperly reduces the correlation coefficients. In fact, the treatment raises as many correlations as it reduces.

We feel that our original article provides an adequate discussion of additional points raised by Pirages.

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Green Thumbs

The hybrid *Chlorella*-animal cell as proposed by N. Nashed (Letters, 27 Dec. 1974, p. 1159) is a very interesting idea, and I hope that someone will be able to pursue this potentially valuable resource. Further information dealing with similar types of naturally occurring relationships has been reviewed for the algae endosymbionts of the Mollusca (*1*). In addition, Nass (*2*) successfully incorporated higher plant chloroplasts into mouse fibroblast cells for short periods of time. Using the information obtained from these studies, I would like to carry the Nashed proposal one step further: eliminate the middleman, that is, the farmer that would grow the hybrid cells, by incorporating chloroplasts or algae cells directly into the skin of humans. This relationship would be much more efficient.

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Hemoglobin in Humans

Thomas H. Maugh II (Research News, 17 Jan., p. 154) indicates that an average-sized human might contain about 1.5 kilograms of hemoglobin. This appears to be in error by a factor of about 2. An average man might contain about 15.5 grams of hemoglobin per 100 milliliters of blood; an average woman has about 12 percent less. It has been known since 1856, when Bischoff (*1*) determined the amount of blood in a condemned criminal, by weighing him before and after decapitation (by guillotine) and washing out residual blood, that total blood mass constitutes approximately 7.7 percent of the total weight. Thus a man weighing 70 kilograms would have about 5.4 liters of blood and 837 grams of circulating hemoglobin. South American miners adapted to living at 5,340 meters (17,500 feet) above sea level may have only 22.9 grams of hemoglobin per 100 milliliters of blood (*2*), which would give 1.24 kilograms (about 20 percent less than the figure given by Maugh), but these miners hardly represent the average. Mountaineers acclimatized to 5,790 meters in the Himalayas contain an average of 20.2 grams of hemoglobin per 100 milliliters of blood (*3*), but only in individuals with pathological polycythemia are values higher than these recorded.

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