# Letters

## Waste Heat Management

Hetrick and Seale, in their letter (26 Jan.), appear puzzled by the fact that waste heat presents a problem for nuclear power plants but not for fossil-fueled plants. Fossil-fueled plants, while not immune from the second law of thermodynamics, currently operate on a more efficient thermodynamic cycle and therefore reject less waste heat for each kilowatt-hour of electrical energy produced, about 6000 BTU versus 8500 BTU for nuclear plants. Furthermore, in order to effect economical operation, nuclear power plants tend to be several times larger in electrical power output, and this aggravates the problem of waste heat management.

The writers present the choice for the future as between "thermal pollution" and a shortage of electric power. I believe that we have a variety of technical means at hand to get rid of the waste heat, and that imaginative constructive uses of the waste heat are also possible: climate control, desalting of water, heating buildings, heating irrigation water, aquiculture, and so forth. Thus, the real choice is between thermal pollution, when the waste heat is simply dumped into a river or estuary, as against paying a surcharge of under 5 percent, or much lower—depending on ingenuity—for restoring the water to its original temperature state.

As the need for electric power increases, the problem of waste heat management will become a major one. To give an idea of the dimension of the problem, by 1985 about one-quarter of the total run-off of the United States will be used for the cooling of electric power plants. We might as well face up to the problem now and solve it while it is still manageable, and avoid serious ecological consequences.

S. FRED SINGER
*Office of the Secretary,*
*U.S. Department of the Interior,*
*Washington, D.C. 20240*

## Heredity and Environment

It is not my purpose to debate with either Shockley or the National Academy of Sciences on the desirability of increasing research on the relative significance of heredity and environment on human performance ("Racial studies: Academy states position on call for new research," 17 Nov., p. 892). Rather I would emphasize that mankind can be studied without getting into loaded questions which can be destructively exploited by racists. Let us start with the assumption that "states of health or disease are the expressions of the success or failure experienced by the organism in its efforts to respond adaptively to environmental challenges" (*1*, p. xvii). Let us also include in the concept of diseases those forms of social pathology or deviation that tend to cause genetic deaths in adolescent or post-adolescent humans (homophilia, hard narcotics addiction, and compulsive criminal behavior are examples). Biological models based on diseases where host and pathogen show marked mutual adaptation tend to demonstrate that the relative contribution of heredity and environment is a meaningless question, and I rather suspect that most adaptation failure syndromes will turn out to be comparable. One expects surviving populations to produce genetically inferior individuals as a result of genetic recombination, but their presence is no evidence that the population generating them is inferior. In fact, it is generally also meaningless to apply researcher value judgments to surviving populations, races, or cultures, a principle that anthropologists seem to know well. In the proper study of human adaptation there are two classes of observer position which must be kept reconciled; one class deals with individuals and the other with populations. The easiest way to distinguish the two positions is to borrow Lawrence Slobotkin's quasi-teleological idea that evolutionary processes are analogous to a game.

One observer is interested in seeing how far the game analogy can be pushed. Further, he is interested, for example, in new formulations of the genetic load hypothesis and in vigorously testing with animal models such hypotheses as the Sacher-Trucco theory of mortality, and Slobodkin's idea that there is an evolutionary advantage in having a relatively larger number of less healthy animals than having a smaller number of more healthy ones. His measures are in terms of morbidities, mortalities, genetic deaths and near deaths, and population physiology, and they allow no room for measures of good and bad.

The other observer is trying to bias the game in favor of a good outcome. This is the traditional position of the physician, the social worker, and the teacher. There is a hierarchy of goods of which generally the most important is that of the individual who is the object of the professional attention. Good and bad heredity and environments only have meaning insofar as the individual concerned and his advisers perceive them to be such.

Behind all this the fundamental question really is, can man participate intelligently in his own evolution without destroying the truly humane values in society? The answer is an unqualified yes if it turns out that both classical and social pathology are to a large degree the results of evolutionary pressures. But we probably will never know since obviously "the intellectual atmosphere in the scientific establishment is becoming increasingly incompatible with the study of biomedical problems that involve more complex living systems" (*1*, p. 445).

J. H. WHITLOCK
*New York State Veterinary College*
and *Division of Biological Sciences,*
*Cornell University, Ithaca 14850*

### Reference

1. R. J. Dubos, *Man Adapting* (Yale Univ. Press, New Haven, Conn., 1965).

## Copper Deposit at Glacier Peak

Whether or not the copper deposit in the wilderness near Glacier Peak should be mined is a question which should be judged by attempting to weigh the very different values involved,

that is, by determining if the mineral deposits are of a quality and extent sufficient to affect the national economy as distinguished from those which are not important nationally, no matter how valuable they may be to the owners.

Is the deposit at Glacier Peak expected to contribute, say 10 percent, to United States copper production or less than, say, 1 percent? If the first is true, then the scenic and other public values should be compromised; if the second is true, then the scenic values should prevail. Opinions, of course, will differ about the potential value that should be accepted as the cutoff, but such a formula enables each of us (with our built-in bias) to arrive at a weighted decision.

Under this approach, if scenic and other public values are to be compromised, it is because the deposit is believed to be extensive and as such can be expected to absorb unusual expense in minimizing damage to the scenery and other land use.

CHAS. B. HUNT
*Department of Geography,*
*Johns Hopkins University,*
*Baltimore, Maryland 21218*

## Reprints: The Unfunded Author

Parkes's discussion of his "Reprint philosophy" (Letters, 8 Sept.) demonstrates that the "grant-supported" and the "grantless" scientists are in rather different situations. But are reprints alone affected in an institution which is "small and underfunded"?

Being grantless and in an underfunded institution (the vast majority of European institutions are underfunded!), I know the problems well and my previous experience in very good U.S. institutions provides me with a scale of comparison. My collection of reprints supplements our underfunded library. For that reason, I treasure them more than as a mere personal convenience. Many times, in response to my request for a single reprint, I have received several others which were published in journals not being received in our library. Also, I have established excellent contacts by writing to certain scientists who request reprints. This is not incompatible with providing reprints in reply to all requests, a system which Parkes says he follows, as long as he has a reasonable supply. But writing one or two hundred letters to ascertain if the requests are justified is not only tiring but expensive. There is really no basic disagreement between Parkes's and my viewpoints, but we react in slightly different ways to the same nonideal conditions.

MIGUEL MOTA
*Department of Genetics,*
*Estação Agronómica Nacional,*
*Oeiras, Portugal*

## Biological Research on Aging

In regard to the discussion on how to improve the present feeble effort in research on aging (Letters, 15 Dec.), I suggest there is increasing need for research at the molecular level, which should start by enhancing opportunities for researchers on aging to use molecular biology equipment, such as the analytical ultracentrifuge, the electron microscope, and others. This can be achieved by providing space for researchers on aging in laboratories of molecular biology, or by extending the existing institutes of molecular biology, or by extending the existing institutes of molecular biology or institutes of developmental biology to "Molecular Biology Institute(s) of Development and Aging." Such steps would provide an increased opportunity for rigorous testing of the various hypotheses on the origin of aging and would increase exchange of ideas between molecular biologists and researchers on aging. In the past 10 or 12 years excellent studies have been done in several leading laboratories of this country on developmental changes at the molecular level. These represent outstanding contributions to the elucidation of aging processes throughout the life span, even though the expression "aging" rarely appears in any of their publications. However, to a researcher on aging and senescence, it is immediately evident that about 10 percent additional effort, and possibly less, might have increased the value of these studies for the understanding of aging processes by a factor of 10 at least. This relative shortcoming could have been avoided if these researchers in molecular biology of developmental processes had been exposed to concepts on aging processes through contacts and discussions with researchers on aging.

JOSEF P. HRACHOVEC
*School of Public Health,*
*University of California,*
*Los Angeles 90024*

## Matthew, Mark, or Luke Effect

Merton's article, "The Matthew effect in science" (5 Jan., p. 56), itself demonstrates the importance of being "first author." The title effect is described in nearly identical words in all three of the synoptic gospels (Matthew 13:12 and 25:29, Mark 4:25, Luke 8:18 and 19:26), and since Mark unquestionably published first, it would be more in accord with scientific practice to have named it the "Mark Effect." Moreover, it is obvious (if you have a red-letter edition, as well as a concordance) that all three gospel writers are really quoting the words of Christ, so that it would be still more logical to call it the Jesus effect. . . .

CHARLES D. GEILKER
*Department of Astronomy,*
*Case Western Reserve University,*
*East Cleveland, Ohio 44112*

Merton quotes and explicates Scripture:

> "For unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath."
>
> Put in less stately language, the Matthew effect consists in the accruing of greater increments of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark.

Objective consideration of style preference among a small but carefully selected subset of the literate scientific readership would tend to indicate that the language of the gospels in the King James version demonstrates a comprehensibility and clarity not inconsiderably in excess of that exhibited by the less stately jargon of Professor Merton. I hope he is not planning to translate the rest of the Bible.

KENNETH MANLY
*74 Kirkland Street,*
*Cambridge, Massachusetts 02138*

Merton may be interested in still another area where the Matthew effect operates with particular precision.

A study was made of the correlations that may exist between the characteristics of the managers of large research and development projects and the success of the project (*1*). As it turns out, an understanding of the Matthew effect would have eliminated the need for the study.

It was found, of course, that there was no correlation at all between a