Letters

Units of Energy

I read with a great deal of interest the article by Chapman, Tyrrell, and Mount (17 Nov. 1972, p. 703) on electricity demand growth and the reports by Allen Hammond on energy (Research News, 8 Dec. 1972, p. 1079, and 15 Dec. 1972, p. 1186). The editors of *Science* are to be commended for consistently bringing forward for public discussion so much provocative material concerning energy.

Discussion of world energy needs, and of world ecological problems in general, necessarily brings together specialists from a multiplicity of disciplines. The exchange of information would be greatly facilitated if editors would recognize the desirability of expressing data in metric units, specifically in units of the International System. Such strange aberrations as the Tkwh and quadrillion (!) British thermal units have no place in a modern scientific journal.

Today the only internationally acceptable unit of energy is the joule. The practice of using different units to measure mechanical energy, electrical energy, and thermal energy is obsolete, and both the British thermal unit ($\approx$ 1055 joules) and the calorie ($\approx$ 4.187 joules) should be avoided in technical writing. Likewise, there is only one acceptable unit for power or heat transfer, the watt (or joule per second).

When energy and power are expressed in joules and watts respectively, many hidden relationships immediately become obvious. The "trillion kilowatt hours" becomes 3.6×10^{18} joules, while a quadrillion British thermal units becomes 1.055×10^{18} joules. The so-called "heat rate" of 10,508 Btu per kilowatt-hour becomes 3.08 (joules per joule), which is the reciprocal of the thermal efficiency for the generation of electricity, 32 percent.

Hammond's comments about the thermal efficacy of heat jumps become truly dramatic when consistent units are used. For example, the efficacy of room air conditioners, which in his words "ranges from 4.7 to 12.2 Btu of cooling per watt-hour of electricity," can better be expressed as a cooling efficacy of 1.4 to 3.6. This implies that 100 watts of electrical power buys you from 140 to 360 watts of cooling, which would be a very good buy indeed if there were no shortage of primary energy.

There are those who will argue that both the Btu and the kilowatt-hour are so deeply entrenched that it is impossible to communicate without them. Such arguments do not appear valid today. With their transition to the metric system even the British have abandoned the British thermal unit, and the Institute of Electrical and Electronics Engineers has gone on record as saying that "the kilowatt-hour should eventually be replaced by the megajoule in most applications."

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Regulating Marine Transplantation

Louis D. Druehl's plea for international regulation of long-range marine transplantation projects (Letters, 5 Jan., p. 12) mentions that the several biologists with whom he discussed the subject at the Seventh International Seaweed Symposium in Sapporo, Japan, were unanimously opposed to such projects. Not all marine scientists are of this opinion. For example, I was approached at the symposium by several of the leading Japanese algal culturists with a request to send them gametophytes of our Pacific coast giant kelp (*Macrocystis pyrifera*). A nation such as Japan might derive enormous benefits from *Macrocystis*. The Japanese are second to none in their aquicultural skills and abilities to regulate natural populations in their coastal waters. I cannot imagine more favorable conditions for a carefully controlled experiment involving a long-range transplantation of great potential benefit. Several American scientists, however, objected vociferously, and the Japanese politely withdrew their request.

As Druehl notes, control of transplanted species or adventitious species accompanying the transplant is difficult if the organisms have pelagic stages. This element of uncertainty cannot be eliminated by any committee or commission. A regulatory body could assure itself that experiments were conducted by competent scientists. Serious efforts involving long-range transplantations are usually expensive; hence they are typically funded only if competent people are involved. The Japanese request at Sapporo for *Macrocystis* gametophytes involved probably the most experienced scientists in the world, yet it was not immune to vigorous criticism.

A regulatory group could not guarantee that adventitious introductions would not accompany some transplants. Very few marine organisms can presently be isolated in pure culture. In my opinion, adverse effects of adventitious introductions are sometimes exaggerated. For example, Druehl fears that *Sargassum muticum* will displace *Zostera marina* in shallow habitats. Where I have examined *S. muticum* patches in southern California bays, the alga requires a solid substrate for attachment. *Zostera marina* requires a sedimentary bottom for its roots. In Newport Bay, California, the two species coexist without apparent competition. *Sargassum* foliage creates a somewhat different habitat, possibly enhancing diversity.

If the regulatory body proposed by Druehl consisted of a majority of conservation-minded scientists, their principal actions would probably be prohibition, not regulation. Their function could more simply be carried out by laws. If the commission was weighted with progressive-minded scientists, would we add anything to the careful reviews and deliberations that precede today's aquicultural projects?

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Objectivity of the Peer Review System

Nicholas Wade's report on the peer review system for awarding National Institutes of Health (NIH) and National Science Foundation (NSF) grants (News and Comment, 12 Jan., p. 158)

was interesting and, in view of considerable folklore perpetuating (or perpetrating) contrary views, reassuring. It would seem difficult, under the system as he describes it, for an individual or small group to influence, for nonscientific reasons, a study section's vote on any particular application.

This does not, however, deal with the issue of very general scientific biases which the group as a whole may share. Wade's comment that NIH and NSF staffs apparently feel that "they have to go to good places to get good people" (both as members of study sections and as successful applicants for grants) clearly implies a consensus on what constitutes good scientific work. While it is possible to specify general criteria for "good" science, it is also true that, beyond a certain point, decisions must rest upon subtle qualitative judgments about the direction of the field as a whole—the importance of certain questions as well as the formulation of a particular problem. It is in this complex and difficult area that the value and danger of the present review system may lie. A group of intellectually like-minded scientists may be able, through grants awarded on the basis of what they regard as "scientific merit," to support work conducted within a particular intellectual fromework, until the paradigm either proves fruitful to most workers in the field or it does not. In any event, such a system is valuable in that the approach currently in fashion will get a fair trial; funds will not be distributed in such a way that no cohesive intellectual framework is thoroughly tested. The danger, however, is that the "peers" in the study sections may, for various reasons, become isolated and insulated from a recognition that the approach they support may have more promising alternatives. This is particularly true in the social sciences, and lies behind the criticism that the present reviewing system is too inbred. If women and other minority-group scientists were in a position to determine, by allocation of funds, the direction of research in certain fields (psychology, nonmolecular biology, and sociology come immediately to mind), the fields would be different. Not necessarily better, perhaps, but different.

A middle-of-the-road solution to the various problems presented by the peer review system would be to limit, absolutely, appointments to study sections to one 4-year term. The study sections should—at least in the social sciences—be representative of workers in the field in terms of sex, race, and perhaps region of the country. A consideration of these "nonscientific" factors in forming study sections is not new; I am suggesting that they be considered differently—to include, rather than to exclude. Suggestions for changing criteria for this or that (be it membership on a study section or tenure in a university) are frequently labeled as "lowering the standards"; do we really have the best possible system so that any change is automatically for the worse?

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Mental Health Care

The Nader study on mental health centers (News and Comment, 4 Aug. 1972, p. 413) contains many excellent and telling criticisms, but it is weakened by serious omissions. Nowhere do the authors recognize the existence of presently incurable schizophrenia patients who occupy the majority of beds in mental hospitals. There is no treatment for these patients, rich or poor, Caucasian or otherwise; their care is chiefly a custodial problem, because they cannot look after themselves. The quality of that custodial care is a measure of what the community is willing to spend. Wealthy families can afford better hotels, but whether the American society will in the future be willing to spend enough to improve the housing for less affluent schizophrenics is an open question. Historically, it has not been willing to do so. If society does desire to spend more money on the schizophrenic, the question also arises of whether it should be spent for better hotels or for more research on treatment.

The Nader report does not consider