Letters

Atmospheric Sulfur

Hanley's letter (22 Oct., p. 360) seems to lament the loss to crops of atmospheric sulfur if low-sulfur gas and fuel oil are used. He neglects to point out, as have Bormann and Likens (*1*), that atmospheric sulfates, which originate largely from industrial activities, are a major cause of the leaching of nutrient ions from the soils of forest ecosystems. The "million of tons of sulfur that are released into the atmosphere annually," even when considered by themselves, would not appear to be an unmitigated blessing.

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Reference

1. F. H. Bormann and G. E. Likens, *Sci. Amer.* **223**, 92 (October 1970).

Kinetic Energy: Saltation

I am not sure whether W. F. Tanner's report "Net kinetic energy in littoral transport" (18 June, p. 1231) is meant to be taken literally. Let me point out a few incongruities:

1) Kinetic energy is a measure of a body's capacity to perform work by virtue of its possessing mass and velocity. The work done cannot be estimated by inserting the mass moved and its average velocity over a long period of time into the kinetic energy equation. Even if the motion were steady, this would simply be a measure of the kinetic energy imparted, not expended. In the case of unsteady or intermittent motion, this method of calculation is even more erroneous.

2) The figures quoted in the report indicate that the maximum "energy" estimated for a single cell (length between 5 and 25 kilometers) was approximately 3×10^6 ergs over a period of 68 years. According to my calculations, this is approximately the amount of energy required to lift a mass of 1 kilogram through a height of 3 centimeters. I imagine it might be expended by an energetic crab in the course of a few days.

In the same issue of *Science*, the same writer states in a review of Graf's *Hydraulics of Sediment Transport* (p. 1228) that "saltation is unimportant in water, a fact overlooked by many a textbook writer." This may be true if saltation is defined as the special type of impact saltation first described by Bagnold with reference to wind action on sand. However, both the American Society of Civil Engineers' *Nomenclature for Hydraulics* (*1*) and the American Geological Institute's *Dictionary of Geological Terms* (*2*) define saltation in a more general way, consonant with its Latin root, to mean simply an intermittent jumping of sediment. This is a very common and important form of stream-bed movement—so much so that the derivation of H. A. Einstein's bed-load function depends on a certain assumption about the average length of jump.

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References

1. *Nomenclature for Hydraulics* (Manual on Engineering Practice No. 43, American Society of Civil Engineers, New York, 1962).
2. American Geological Institute, *Dictionary of Geological Terms* (Doubleday, New York, 1962).

The total wave energy for a cell that was studied along the Gulf of Mexico coast of Florida (over a period of about 70 years) was estimated to be about 5×10^{22} ergs. Surf energy was estimated to be about 16 percent of this, or 8×10^{21} ergs. The rest of the energy was used in frictional losses and sediment shuffling. Most of the sediment shuffling, however, was bidirectional (that is, along the coast in one direction, then along the coast in the opposite direction); it is, therefore, not pertinent to the net unidirectional transport of sand, which is important in beach erosion.

Three different methods of estimating the net unidirectional work done in the movement of sediment produce a result of roughly 10^{19} ergs for the study period. From these estimates it can be seen that about 2 percent of 1 percent of the total wave energy in the system was used in net unidirectional transport of sand. The figures in my report represent reasonably precise long-term measurements, not estimates. They are many orders of magnitude lower than the available estimates because they do not include repeated grain accelerations, which cannot be measured in a net sense. The measurements in my report were carefully labeled in terms of net kinetic energy.

Both definitions of saltation that are cited by Neill have been in common use and describe two quite different processes; I prefer to use the term to mean the carefully defined systematic process outlined by Bagnold, rather than the older, less specific concept, which implies (for many persons) an impact-ejection mechanism under water.

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New Science

Pearlman's letter (25 June, p. 1293) perpetrates a popular misconception that scientists seem determined not to correct, which is that only applied research is relevant. On the contrary, to solve our technological problems we need new science, and this can come only from basic research.

As an engineer, I cannot understand why scientists visualize our technological future only in terms of prodigious engineering developments based on yesterday's science, with the implicit assumption that science will do nothing in the future that is relevant to our needs. The subject of fusion power is an excellent example. The engineering problems that must be solved in order to achieve fusion power are formidable indeed, and the most optimistic forecasts do not see their solution in less than 20 years. In all the discussions I have seen on this subject, no scientist has mentioned the possibility that the next 20 years will bring new scientific knowledge that will enable us to convert mass into energy without having to conjure up and contain a major catastrophe.

Since scientists are so free with their predictions about what the engineers will be doing, I don't mind telling scientists that I think they will develop useful new knowledge; astronomy, par-

ticle physics, solid-state physics, and who knows what else are relevant to our needs in this respect and are therefore deserving of public support. Nothing is more primitive than our means of generating power, either by building a fire, or, even worse, by starting a fission reaction; no part of our technology is more ripe for a scientific revolution.

New science can change our lives in highly desirable ways. There have been many notable examples in the past, and it does not make sense to behave as though it would never happen again.

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Diet and Stomach Cancer

In his interesting report "Talc-treated rice and Japanese stomach cancer" (17 Sept., p. 1141), R. R. Merliss advances data indicating that asbestos fiber contained in talc added to rice as a flavor fixative is the carcinogenic agent that causes a high incidence of stomach cancer in Japan. He suggests that the diets of Finland, Iceland, and Chile, where the incidence is similarly high, should be scanned for asbestos contaminants.

Being familiar with the dietary habits of the Scandinavian countries, particularly Finland, I believe that the high incidence of stomach cancer there is connected with methods of fish preservation. Great quantities of strongly salted fish, *smoked* fish, and *smoked* cereal powders ("talkkuna") are consumed in these countries. I am not aware that any asbestos fibers find their way into foods in Finland and Iceland.

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I presume that the talc content which Merliss found was for rice that had been dry-ashed from a raw state. However, no self-respecting Japanese woman serves raw rice. She rinses it many times in both cold and tepid water until the rinse becomes clear. At this stage she adds a final measure of water and either directly cooks it or uses a rice cooker. The thorough rinsing preparations certainly must remove a great deal of the asbestos-contaminated talc fibers, possibly lowering the content to an insignificant amount.

May I suggest that a similar study be made in which thoroughly rinsed rice, and then cooked rice, are used. Different amounts of talc and asbestos may be found.

The connection between rice eating and stomach cancer, however, may actually exist. Boiled white rice is bland, and in any country where rice is a staple something has been developed to add to its flavor. In India it is curries, in Korea and Thailand, hot and garlicky spices; the Japanese use soy sauce, pickles, and some commercial gravies, to say nothing of seaweed (nori). I would look for the cause of Japanese stomach cancer in (i) soy sauce, which is used in tremendous quantities, and in which there is ample opportunity for fungi to grow and produce aflatoxins; (ii) the wide variety of pickles and pickled vegetables that are eaten with the rice; (iii) seaweed, which is extensively consumed by the Japanese; and (iv) the method of rice storage, which could result in the growth of fungi and the production of aflatoxins in the rice.

These may or may not be blind alleys, but they are more reasonable than the assumption that the Japanese eat raw rice.

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Merliss's arguments and conclusions seem deceptively self-evident. However, there are two major flaws in his theory. First, most of the rice sold in Japan does not have a talc coating. No Japanese home is far from a rice shop, and much of the rice is never stored but moves quickly from the field to the consumer. In most instances the bran is removed just prior to sale in the shops. Second, if talc is used, the method of preparation used by the Japanese involves prolonged washing prior to cooking (instructions on bags of glucose and talc-covered rice packed in California by the Japan Food Corporation clearly indicate that the rice should be washed before using). It would therefore seem that the amount of talc ingested by the Japanese is negligible and thus not likely to be related to the high prevalence of stomach cancer in Japan.

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The talc used to coat the rice that Merliss analyzed may have been California talc, which contains about 20 to 40 percent tremolite (calcium magnesium silicate), a form of asbestos.

However, talc is suitable for human consumption, provided that it is nearly all hydrous magnesium silicate and contains only traces of calcium silicate. California talc, or talc of a similar quality, should not be used to dust foods or in medicines.

Baby-care products also contain large quantities of talc. The amount of talc used in the care of a baby can be estimated at 2 ounces per week per baby. Although only a small portion of this quantity is inhaled or swallowed by the baby, we should not underestimate the amount. The frequency of respiration is very high. An additional warning should be made against the use in baby-care products of talc with a high calcium silicate content.

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Miller and Smith make the point that washing would remove the asbestos-contaminated talc from talc-treated rice. I have not found this to be true. Examination on 19 October 1971 of rice that was cooked and canned in Japan and sold here in a Japanese market showed 1.1×10^4 asbestos-form fibers per gram of desiccated cooked rice. Apparently washing and cooking reduces the amount of asbestos-form fibers in rice but does not free it of these fibers. Miller says that most rice in Japan moves directly from the field to the consumer. It is hard to believe that this is so in metropolitan areas, and it is obviously not the case with the Japanese canned rice I examined. In all circumstances the incidence of stomach cancer in Japan varies from area to area, and differences in the nature of the rice might be one of the factors influencing this variation.

Bjorksten writes that he does not know of talc additives to food in Finland, a country where the incidence of stomach cancer is high. The problem is complex. Talc is usually considered so innocuous that specific inquiry or examination must be made to determine its presence as a food additive. Firm information about talc additives is needed from Finland, Chile, and Iceland.

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