Letters

Mushroom Poisoning

The article "The destroying angel: A story of a search for an antidote" (News and Comment, 16 Aug., p. 600) by Barbara J. Culliton gives an interesting and informative account of the drug thioctic acid and its use in the treatment of poisoning by *Amanita phalloides* in North America. Culliton's report presents the problem of mushroom poisoning and the seriousness of poisoning by *Amanita phalloides* and related species. Each year in North America several people die from eating mushrooms of the *A. phalloides* group; in Europe the number of fatalities is greater.

In North America at least four species of *Amanita—A. bisporigera, A. phalloides, A. verna,* and *A. virosa*—have been involved in serious or fatal poisonings. Early in this century, several species of *Amanita* were sometimes classified mistakenly as *A. phalloides.* A few, including *A. brunnescens,* do not contain the deadly toxins of the *A. phalloides* group and may not even be toxic. Instead of representing *A. phalloides,* the photo on the right in Culliton's article represents *A. brunnescens.* The photo on the left is most likely *A. virosa,* once considered a variety of *A. phalloides.* Recognition of particular species of *Amanita* is difficult, and all species of this genus should be avoided.

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Ely S. Parker

Harold N. Wiren's letter (16 Aug., p. 570) incorrectly gives the name of the Indian who made such good use of his RPI (Rensselaer Polytechnic Institute) engineering training as Ely Samuel Taylor. It was Ely Samuel Parker (*1*). His Indian name noted by Wiren—Donehogawa—was no ordinary one but is a name-title of 1 of the 50 Iroquois sachemships (chieftainships of the Iroquois Confederacy), 8 of which are held by the Seneca. He was given this name in his early 20's and, as the Iroquois say, was "raised up" as a sachem chief after the Seneca who had been known as Donehogawa died. Before that, Parker's Indian name was Hasanoanda.

Ely S. Parker was a man of uncommon talent and accomplishment. While still in his teens (and before attending RPI) he was serving as an interpreter and spokesman in his people's fight to retain the reservation that had been sold without their consent and was traveling to Albany and Washington to assist in this cause. He also was serving as Lewis H. Morgan's interpreter and collaborator in Morgan's Iroquois researches—a fact duly acknowledged by Morgan in the dedication to his now classic (and still the best single volume on these Indians) *League of the Ho-de-no-sau-nee or Iroquois* (*2*). (Later, his interest in anthropology and in Indians having been rekindled at a AAAS meeting, Morgan became active in the affairs of the AAAS and was elected its president in 1880, the first anthropologist to be so honored.)

Nevertheless, this should not diminish the pride Wiren says the engineering profession and RPI should take in "their early recognition of human resources and rights." Even Harvard's 17th-century educational affirmative action program—its Indian college—did not succeed in enrolling any student who so distinguished himself as did Ely Parker (*3*).

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References and Notes

1. The most comprehensive biography of Ely S. Parker is A. C. Parker, *The Life of General Ely S. Parker* (Publication 23, Buffalo Historical Society, Buffalo, N.Y., 1919).
2. L. H. Morgan, *League of the Ho-de-no-sau-nee or Iroquois* (Sage and Brother, Rochester, N.Y., 1851).
3. S. E. Morison, *Builders of the Bay Colony* (Houghton Mifflin, Boston, 1964), p. 303. In fairness to Harvard, however, it should be noted that Caleb Cheeshateaumuck (class of 1665), the only Indian student during the early decades of Harvard's existence to complete a course of study there, died not long after receiving his bachelor's degree.

Recombinant DNA Molecules

As a working microbiologist for almost 40 years, I find it inconceivable that fear of factors inadequately known or understood should be the basis for inhibiting free investigation. Are the members of the Berg committee (Letters, 26 July, p. 303; News and Comment, 26 July, p. 332) sure that recombinants of DNA molecules do not occur in nature? Isn't it possible that, for each recombinant that is formed or will be formed, there may be an inhibitory or lethal or immune factor which will limit the potential hazard? In fact, it may well be that such inhibitory systems already exist, but can only be shown by displaying the proper recombinant.

In this day and age, when scientists should be concerned with the means for improving the conditions of life, all studies should be utilized for the potential benefit that can result. Recombinants that can fix nitrogen from the air in the roots of major crops are sorely needed throughout the world; microbial by-products to aid seed germination and plant growth and special antibiotics to discourage root rotting fungi are equally needed, as are recombinants to function as methane or other producers of fuel from our ever-increasing garbage and sewage wastes.

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"Wasted" Water

Crops use tremendous amounts of water—much more than is needed for metabolism, translocation, and cooling. Green plants seem to be forced to "waste" water in order to obtain carbon dioxide, because the same mechanism which allows carbon dioxide to enter the plant also allows water vapor to escape. Various plants have numerous ways of cutting their water losses, but always at the cost of reduction in photosynthesis.

Does this have to be true? Couldn't we cover the leaf or the stomatal openings with a material that is highly permeable to carbon dioxide but relatively impermeable to water? This might present mechanical or physiological problems, but probably not insoluble ones. However, no such differentially per-

meable material seems to exist. I have tested many plastics and several other materials, but have found nothing that is significantly more permeable to carbon dioxide than to water at normal plant growth temperatures.

If anyone has reason to believe that some material (plastic, metal, liquid, or interface) is significantly more permeable to carbon dioxide than to water vapor, I would appreciate being told of it.

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References and Unreferences

In a graduate seminar in atmospheric science, I require that my students write a research paper. Having found that these student papers can stand improvement, I hand out, at the beginning of the semester, a list of "do's and don't's" for paper writing. One of the "don't's" is, "Don't use a reference unless you've seen the original paper. Are you going to swallow whole what Mason, or Blanchard, or Vonnegut said about Sigurdur Schlockinghausen's memorable 1932 paper that appeared in the *Journal of Irreproducible Results*? Better not, unless you wish to view the world through the particular brand of colored glasses each of them unknowingly wears. Try your own brand; you might find other colors more to your choosing."

Alas, I forgot to tell my students the most important "don't" of all—"Even if you've seen the original paper, don't copy the reference to it from somebody else's paper. It's probably wrong." For years I've been aware of this, but sometimes under the pressure of compiling a small mountain of references, I'd sneak into my reference list one or two I had read but for which I had forgotten to jot down the title, journal, page spread, and so forth. In an attempt to save time, I copied the references from someone else's list. When will I ever learn that this usually ends in disaster? Let me illustrate. A while ago I had occasion to reference a paper which I had read in 1970 in the Swedish geophysical journal *Tellus*. I couldn't find the reference in my card index, but I knew it was cited by the authors of six or seven papers in the 21 September 1972 issue of the *Journal of Geophysical Research,* which I just happened to have on my desk. Quickly I flipped the pages and found an author who cited the *Tellus* paper. He said it had appeared in volume 21, 1970, pages 451–461. I had no reason to doubt the accuracy of this citation, but just to be on the safe side I decided to check this against the reference list of another author who cited the paper. After all, there's safety in numbers. To my surprise the second author said the paper appeared, not in volume 21, but in volume 22. Otherwise he agreed with the first author. So who was correct? Obviously I needed a third author and a third reference list. In a few minutes I found him. He said the second author was correct regarding the volume number, but that both were wrong about the page spread. The paper actually ends on page 462.

Fast losing faith, I turned to a fourth author, and this time, I thought I had hit the jackpot. The fourth author was none other than the author of the *Tellus* paper in question, and, since authors are very careful when citing their own papers, I assumed that he would cite his own paper in impeccable form. He said everyone was wrong—the paper starts on page 541, not page 451. But he said nothing about the page spread. So on I went through the 21 September issue of the *Journal of Geophysical Research.* I found a fifth author who said the proper *Tellus* volume was neither 21 nor 22 but 12. With five authors behind me who could not agree on the citation, I went to the last remaining author who used the reference, expecting by this time a sixth version. But no, he agreed with the second author. The proper volume is 22, and the page spread is 451–461. Authors 2 and 6 are right. I confirmed this at the library.

Do you think I've made this all up? Check the aforementioned journal. But please, no secretaries, no Xerox copies. Go to the library yourself and see what I saw. As for me, I am now a firm believer in the moral of one of Thurber's stories (*1*), "There is no safety in numbers, or in anything else."

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References

1. J. Thurber, *Fables for Our Time* (Harper & Row, New York, 1939).