Letters to the Editor

Concerning Rains of Fishes

At the risk of seeming a very obdurate Sadducee I am afraid that I must persist in my incredulity concerning rains of fishes (*Science*, 1946, 103, 693). That many people have professed to believe in rains of fishes I am well aware. That many people have observed fish on the ground after a heavy rain may be. But no trained observer has yet seen quantities of fish coming down out of the sky.

Dr. Gudger says that these remarkable downfalls have been "scientifically attested." But what does he mean by this? He refers to "scientific men of high standing and veracity" who "did not witness the falls" yet professed to believe in them. But profession of belief does not in itself constitute evidence, however honored the professor. *No* man's "printed word" is "accepted today by scientific men" as conclusive proof of anything. It is the very essence of science that it should not be.

Dr. Gudger's evidence is all hearsay, and that often at second and third hand. His fullest account is James Prinsep's account of a Mr. Cameron's communication of the alleged depositions of some Indian farmers. Two of these witnesses state that some of the fish were "stinking and headless," and the one who gave the fullest account said that after the fish had struck him on the head he "looked at heaven" and "saw like a flock of birds flying up." The correspondent from Louisiana only states in effect that some fish had been found in boats that had been swamped in a high wind. What more likely?

The very nature of waterspouts, unfortunately, renders exact observation of them almost impossible, and hence little is really known about them. But such accounts as there are (as, for example, in Wenstrom's *Weather*. Boston: Houghton-Mifflin, 1942. Pp. 323–328) suggest that the cone is composed of spray or mist *drawn down from the cloud* and that such water as is drawn up from the surface of the ocean or lake or river by the vacuum—the only part that might possibly carry fish—would not be carried far across the land and could not possibly be mistaken for rain. Milman, in his *Meteorology* (New York: Macmillan, 1929. P. 342), says that "stories of large quantities of water being carried up from the sea into the clouds is pure myth."

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Why Is Taxonomy Ill-supported?

Fosberg and Diehl, in "Present status of foreign herbaria and museums" (*Science*, 1946, 103, 282), make several observations which are important to biological science and with which most botanists will heartily agree. I wish to comment on only one phase of the problem, mentioned by them in these words: "Systematic botany has traditionally been ill-supported." This is true, and it will continue to be true, and deservedly so, as long as taxonomists maintain present principles and practices. Taxonomy is the only branch of science where this is true.

Rewards are bestowed, in taxonomy, for creating names and not for knowledge of the organism named. Some persons have renamed scores, hundreds, and probably even thousands of plants which they never saw and could not possibly recognize if they did see them, but the names so applied must be acknowledged under taxonomic rules, either as valid or as synonyms. No stigma or penalty whatsoever is attached to the creation of innumerable synonyms.

Permanent rights are accorded to the creator of a name, without regard for the general welfare of scientists. This is true whether the name be valid or one of the many kinds of synonyms; whether it was based on knowledge or ignorance; whether it represented an addition to, or merely confusion of, the existing knowledge; and wholly without regard to the inconvenience caused to tens of thousands of other workers. When a man wishes to patent an idea, under United States law, he must submit it to a jury of experts (U. S. Patent Office). If they determine that it is sufficiently new and different to warrant recognition (patent), he has to pay a sum of money to obtain such recognition. After all that, he gets exclusive rights for only 17 years—not forever.

Taxonomy has no required standards of preparation or fitness, yet its products may help or harm many. Anyone who wishes may name organisms, and the names must be recognized forever, either as valid or as synonyms. Schoolteachers must be trained and must pass examinations in order to teach even the three R's in the rural schools. Auto drivers, electricians, plumbers, and public accountants must be examined by experts and must obtain a license before they may operate. Dentists, doctors, lawyers, and ministers must be examined and certified before they can ply their trades. But anyone may apply Latin names to organisms, and the names automatically have permanent status in one or the other of two categories. The products of an ignorant, careless, or dishonest tyro may cause untold and continuing labor to many others much more able.

Taxonomy is the only science that openly appeals to, and openly rewards, the innate selfishness of man by guaranteeing the permanent association of his name with the organism-name he coined, whether it be of value or not. Chemists, physicists, geologists, mathematicians, physicians, and philosophers make discoveries and develop theories of immense importance to humanity, but we are under no compulsion to attach their names when referring to such discoveries and theories. Sometimes we do, in the case of a dozen out of tens of thousands (Boyle, Darwin, Einstein, LaPlace, Newton), but no rule compels it. What is the value of a single plant

name, out of a million or more, as compared with one of these great products of the human mind?

These principles and practices of taxonomy have created another field of labor, synonymy, fully as difficult, more far-reaching, and even more expensive, as taxonomy, and we are but at the beginning. Some now devote their time to the study of names and never learn about plants. All real taxonomists are compelled to spend an increasing percentage of their time in the study of names, leaving less and less of their energy for the study of plants. Which are the most important to humanity, names or plants? Which are the most important to science?

All of this has been done under a fetish known as stabilizing nomenclature. Fosberg and Diehl refer to systematic botany's contribution in studies to "stabilize nomenclature." I will defy any person to compare the successive manuals of botany issued in the last 100 years and produce any indication whatsoever that nomenclature is being stabilized. If the permanence of patent (priority) rights to a name never had been acknowledged in taxonomy, we would have had a stable nomenclature long since. As it is, all workers in botany have to learn a new set of names for most plants every 25 or 30 years. This not only is maddening, but absolutely unnecessary.

Suppose that we applied such a rule in the realms of chemistry, economics, geology, cosmogony, mathematics, philosophy, physiology, theology, etc., and had to cite the name of the original promulgator of an idea every time we mentioned it. Suppose that we always were finding (as we are) that someone just a little earlier had evolved what might be claimed to be the same idea. Suppose that we had to append the name of the architect or builder every time we mentioned a great structure or addressed a letter to a given house or office building. If we did, we would be approaching the futility of the situation to which the stupidity and unrealism of taxonomists have brought that science.

If the taxonomic world really believed in, or desired, stability of nomenclature, they would have worked to achieve it long ago through limiting prior rights to a specified term of years or through conserving well-known and widely used specific names when they had been in such use for 100, 50, or 25 years. This would prevent the present disastrous absurdity of letting a few-months priority displace names well-known and widely used for 140 years.

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Cancer Research and Benefit to Patients

It cannot be otherwise but that the tenacity with which the practitioners of applied or practical research claim omnipotence is matched by the pertinacity with which the practitioners of academic or pastime research claim holiness. While the former clothe their activity with the semantics of utility, their basic motive is, of course, the acquisition of material things. And while the latter clothe their activity with the semantics of increasing knowledge, their basic motives are, of course, self-amusement and fame. Such camouflage is necessary, we humans being what we are. But the conflict between the motives of the two groups is in considerable part responsible for what Prof. George Shull so aptly calls "the historical phenomenon" of the "long interval which" elapses "between the making of a fundamental discovery and the general understanding of its importance and full realization of its benefits" (*Science*, 1946, **103**, 547).

Nowhere today is this delay more unhappily evident than in the field of cancer research. The accumulated data of Rous, Shope, Coley, Bittner, Strong, Andervont, Green, Greene, Williams, Taylor, Furth, Twombly, Cowdry, Diller, Bawden, Pirie, Stanley, Wycoff, Kunitz, and others indicate beyond peradventure the path for getting at something of practical benefit to the cancer patient of the future other than surgery and radium.

The demonstration that mother's milk may contain a transmissible agent productive of malignant growth—call it a virus or what you will—is evidence enough of an autogenously produced chemical compound type which is responsible for that distortion of ordered cell growth which results in malignancy and death of the individual. Yet where is the proposal that mothers with family histories of cancer should be warned against breast-feeding their infants? One such I have seen, but this was so buried in the literature that its excavation is impossible.

And where is the proposal that workers acquainted with this principle of transmissible agent should get together as a team, pool their respective experiences, knowledges, and ideas to undertake a concerted, coordinated, cooperative, organized attempt at isolation, identification, and investigation of the offending chemical compound—not separate and alone as they are now doing, but under one roof, in daily contact with each other, exchanging results, information, and ideas to the sole end of bringing as early as possible something of practical benefit to the cancer patient?

It is said that the academic scientists are too egoistic to work together, too set in their opinions, too unwilling to consider alternatives, too determined on fame. If this is so, they should take a leaf from their confreres in industrial research. Here, too, there are able scientists, working together in both applied and fundamental research to bring about results of practical utility. And our best brains in physics worked together in organized cooperation to produce the greatest destructive agent known to man. So why can't the same be done constructively to produce deflation of the second greatest scourge of human living, namely, cancer? Surely logic, reason, common sense, and the call of humanity make such procedure much to be desired.

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