following morning, and shortly thereafter my barn went intensely paramagnetic (kiddophilic and mammaphobic). It took me 10 days to crawl out from under the crowd. Meanwhile the press forged two versions of the story—the humorous side captioned by such headlines "Pied Piper Sends 'em Back," "Science at Work." "Researcher Avenged with Mouse-ola,'" "One Jolly Hickory Dickory Doc," "Of Mice and Mangun," "Doc Makes Town Crawl," and "Mangun, Merry Mouse Man of Mendham." No one thought to call me the Pied Pipetter. Except for the original story in the Newark *News*, all versions edited out the *only* in my original statement and used the statement out of context.

Some papers ignored the tongue-in-cheek vein of the original story and quoted me as saying I was taking revenge against the township. I have never made any such statement. Retribution is proceeding sanely by due democratic process.

I have learned the hard way that research is looked upon by some as "just a way to make a buck." In part this may be due to the confusion existing in the mind of the public as to the distinction between basic scientific research, applied research and technology, and manufacturing laboratories. Fear and distrust of the scientist were also abundantly evident in the attitude of a few of the objectors, who had some almost amusing Frankensteinian qualms.

Well, it's been fun and most educational to operate a laboratory on an isolated farm. At the same time I have obtained a lot of very interesting data on enzymes and analgesics. However, I would reccommend that anyone setting up a private research laboratory seriously consider inventing a new name for it—for instance, a "knowledge studio."

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The Chinese Chestnut

I noted with interest the photograph of Chinese chestnut nuts, bur, and leaves on the cover of the 25 March issue of *Science* [**131** (1960)].

I have a slight criticism to make about your statement about the Chinese chestnut. You say that widespread planting of the Chinese chestnut "is bringing back the chestnut to the American scene." It is true that we again have nut-producing trees of the chestnut growing in this country, but the Chinese chestnut is very different in its habit of growth from the American chestnut, and it will never replace the native tree. The American chestnut was one of our greatest timber species. It was a tall, stout-growing tree whose wood found many uses, particularly for telephone poles. The Chinese chestnut is almost a shrub in comparison. It is a small-growing tree of unimpressive potentialities as a timber tree. It will never find its way into our native forests. Its best use is as an orchard or lawn tree.

Incidentally, the poetic phrase "Under the spreading chestnut tree" referred to the horse chestnut, a beautiful tree which fortunately is still with us.

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Robert Rodale's statement that the Chinese chestnut will never replace the American species as a timber tree is correct. In our description of the cover illustration we did not have the space to point out that "bringing back the chestnut to the American scene" referred to the production of nuts and not to timber. However, Rodale is somewhat misleading when he says the Chinese chestnut is "almost a shrub" in comparison with the American. We know of Chinese chestnut trees that have a trunk more than 2 feet in diameter and are more than 50 feet tall. It is true that the tree does not generally have a straight central trunk, and the top is usually spreading and rounded.

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More on Stochastic Models

This note is concerned with a criticism of some of the remarks made by N. E. Manos in his recent letter [*Science* **131**, 1400 (1960)]. Although Manos did not give an indication of what he meant by the much abused term *deterministic*, I assume that he meant it in the sense of entailing a necessary logical relation between the members of a class of prescribed characteristics. The latter is in keeping with E. Nagel's definition of *deterministic* [*Phil. and Phenomenolog. Research* **20**, 291 (1960)].

By way of equilibrating Manos' statement to the effect that many investigators in the physical sciences reject any research which is not deterministic, I wish to point out that much of contemporary philosophy, physics, and electrical engineering is "process-minded"; this includes stochastic processes. Surely, quantum mechanics with its expanding domains of intellectual inquiry and its materialistic yield of the transistor cannot be said to be unrealistic. The statistical model pulled together enough relevant facts long enough so that a human mind could make a significant prediction. The same may be said for the model of communications called "information theory." Further fuel may be added to the fire when we consider D. Bohm's remark, "we may say that the processes taking place in nature may have been found to satisfy laws that are more general than those of causality. For these processes may also satisfy laws of chance. . . ." [*Causality and Chance in Modern Physics* (Van Nostrand, Princeton, N.J., 1957), p. 3]. Probably it is accurate to say that an exclusive use of only determinism or only statistics will make understanding of a scientific endeavor more difficult. Perhaps this is a useful principle of dualism in the interpretation of physics.

Finally, as to Manos' comment, "if the world is basically deterministic," I am of the opinion that we should view the world with any model which is capable of exercising our brains, with some resultant esthetic pleasure, and which shows signs of allowing us to reap material rewards.

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Terrestrial Ostracodes

Ostracodes were recently described at a scientific convention, to a wife who was not a biologist, as "microscopic clams each with a shrimp inside." For almost 200 years they have been known as living and as fossil aquatic bivalved crustaceans that inhabit fresh, brackish, and marine waters.

Menzel [*Arch. Hydrobiol. Planktonk.* **11**, 478 (1916)] described ostracodes from wet moss, but the first known terrestrial species, *Mesocypris terrestris* Harding, 1953 [*Ann. Natal Museum* **12**, 359 (1953)] was described from ordinary damp forest humus obtained at an altitude of 500 feet in the Knysna forest, South Africa. The discovery of these terrestrial forms was incidental to the processing of soil samples in a Berlese funnel in order to collect myriapods and small arachnids. In a later paper, Harding [*Bull. Natl. Inst. Sci. India No. 7* (1955), pp. 104–106] suggested that the water in the form of vapor in a humid atmosphere is sufficient to maintain the terrestrial ostracodes.

Chapman [*Nature (Paris), No. 4706* (1960), p. 121] recorded the presence of ostracodes of the same genus in New Zealand from six localities that range in elevation from 800 to 3200 feet and from a variety of environment, such as ordinary damp litter from podocarp-broadleaf forest, subalpine forest, and beech litter. She reported that the animals can be extracted alive by slowly drying out litter in a Berlese funnel, with a water-jacket maintained at a temperature of 40°C and with the end leading into water. They can be kept in the laboratory on moist filter paper and fed on humus. The ostracodes have survived total immersion in water in the laboratory for at least a week and can withstand drying out by closing the valves. The length of the ostracodes is recorded as from 0.56 to 1.04 mm.

There is a possibility that terrestrial ostracodes may be present also in the Northern Hemisphere. A record of terrestrial ostracodes in North America would be of interest to both zoologists and geologists. Because the presence of fossil ostracodes has hitherto been taken as evidence of an aquatic environment, data on the frequency, distribution, and ecology of terrestrial forms would have some bearing on paleoecological interpretations.

Researchers who examine extracts from Berlese funnels are asked to watch for possible ostracodes. I will be interested to hear of any terrestrial ostracodes found.

I. G. SOHN

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Oxygen Diffusion

Scholander has recently described an experiment which demonstrated that the presence of hemoglobin in a wet membrane increased the diffusion rate of oxygen as much as eightfold over the rate through the same membrane when it contained only water [*Science* **131**, 585 (1960)]. Actually, Scholander measured only the change in ratio of the diffusion rates of oxygen and nitrogen when hemoglobin was added to water. He interpreted his data as evidence for an increased diffusion rate resulting from the presence of hemoglobin.

Although Scholander's experimental data are not subject to argument, there is some question concerning his interpretation of these data. He interprets his data to mean that there is an increase in oxygen diffusion rate through a liquid containing hemoglobin compared with the same liquid free of hemoglobin. He then proceeds to develop a hypothesis to explain this increase in oxygen transport.

In his experiment Scholander measured the resistance to diffusional flow through three resistances in series. At the upper surface there was an air-liquid interface; in the filter paper there was liquid; at the bottom surface there was a liquid-water vapor interface. The experiment showed that the *sum* of the three resistances decreased for oxygen diffusion (compared with nitrogen) when hemoglobin was added to the liquid in the filter paper. The experiment does not permit one to say that the oxygen-nitrogen diffusion ratio has increased in the liquid. It may well be that the increase is at one of the interfaces. The problem of diffusion through a liquid-gas interface is not easily treated. A detailed description of the difficulties is given by R. W. Schrage in *A Theoretical Study of Interface Mass Transfer* (Columbia University Press, New York, 1953).

A modification of Scholander's procedure would permit measurement of the diffusional resistance in the membrane alone. This could be done by filling the upper chamber with distilled water saturated with oxygen and nitrogen at a given pressure. The lower chamber would be filled with deaerated distilled water. In this system there would be no phase interface. Samples of the water in the lower chamber could be analyzed for oxygen and nitrogen content as a function of time. If the concentrations in the upper and lower chambers did not change much, then it would be evident that the boundary conditions are fairly simple, and the diffusion coefficient of the membrane and its liquid could be calculated without trouble. If the results of this experiment agreed with previous results, then indeed there would be evidence of a change in the diffusional resistance of water to oxygen when hemoglobin is added. It is equally likely, however, that the diffusional resistance at the liquid-air or liquid-vacuum interface would be changed.

If the hemoglobin tended to diffuse away from the filter paper, the filter-paper unit could perhaps be made in the form of a sandwich in which only the center paper was impregnated with hemoglobin.

To examine the changes at the interfaces, two additional experiments could be performed. In the first, the upper chamber would be filled with water containing oxygen and nitrogen in solution. The lower chamber would contain an inert gas, such as helium or argon, at a pressure sufficient to prevent bulk flow of the water from the upper chamber. By this experiment one can find changes in the diffusional flow resistance at the lower interface. In second experiment, the lower chamber would be filled with deaerated distilled water while the upper chamber would contain oxygen and nitrogen in the gas phase.

The experiments outlined above are all of the steady-state type. It may be interesting to compare the steady-state diffusion coefficients with the coefficients obtained from nonsteady-state measurements. Scholander's apparatus could be used to do a "time-lag" measurement described by Barrer [*J. Phys. Chem.* **57**, 35 (1953)], from which the diffusion coefficient can be calculated. Ideally, the steady-state and nonsteady-state diffusion coefficients are the same. Differences between the coefficients often shed light on the diffusion mechanism.

In the experiments I have outlined above, it may be necessary to analyze for oxygen and nitrogen directly. I believe this could be done by gas chromatography.

IRVING FATT

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I agree that it would be of great interest to devise an experiment which would avoid the complication of possible interphase effects, and in fact a great deal of thought was given to this problem. Unfortunately, hemoglobin molecules diffuse too fast through a membrane to make promising a system such as that proposed by Fatt, and experiments on intact red cells seemed to be the nearest practicable approach. When smeared as a layer on the under side of the finest grade Millipore membrane, this layer showed the same enhanced oxygen transport as a hemoglobin solution, and it was assumed that the entrance into and exit from the cells took place through a hemoglobin-free interphase—that is, through the cell membrane plus whatever saline solution covered the cells.

Even if we were to assume that hemoglobin molecules in solution constitute part of the interphase, it would seem difficult to account quantitatively for the observed enhancement of oxygen transport. At a low pressure this may exceed diffusion by a factor of 8, and because we are dealing with a steady-state transport, each layer of the membrane transmits the same amount of oxygen. If we were to treat the system as diffusive flow through three resistances in series, as suggested by Fatt, we might, for the sake of argument, consider a surface layer of 1-micron thickness to have zero resistance. This would still leave 148 microns of solution to be traversed by diffusion, and it would seem that a specific interphase effect, if indeed there is such an effect, could hardly give enhancement of more than a few percent.

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