Comments and Communications

Toward a More Convenient Method for Expressing the Concentration of Biological Fluids

This is to call attention to a method of designating biochemical concentrations which is both erroneous and anachronistic. The designation in point is the term milligram percent. This ''unit'' is not comprehensible *a priori* and is inadequate for expressing present concepts in enzymology, pharmacology, and biochemistry. Since modern clinical medicine is becoming more and more dependent on these fundamental subjects, it follows that medicine, too, will begin to find this term inadequate.

In clinical biochemistry it has become customary to express the concentration of the more common inorganic ions in terms of milliequivalents per liter. This practice is commendable, and it is to be hoped that the custom will be extended to include iron, iodine, sulfur, phosphorus, and the rest of the ions currently determined.

With the precedent already established, it now becomes adventitious to extend the same principle to organic compounds and express their concentration in terms of millimols per liter. The enzymologists are currently following this practice, and even some pharmacologists are beginning to envision the action of certain drugs on the basis of molecular action. Competitive inhibition illustrates the wisdom of such thought.

In tracing the course of a metabolite through an organism one can easily calculate ratios between successive products and form a rough idea as to lability or metabolic pool size. This could then form a point of departure for more rigorous mathematical treatment. For example, the decomposition of 1 millimol per liter of acetoacetic acid could give rise to 1 millimol per liter of acetone. In corresponding units 10.2 mg% of acetoacetic acid would give 5.8 mg% of acetone. Any resemblance in the latter case is, of course, purely coincidental.

The term ''normal'' as applied to a chemical solution is a rather unfortunate choice of words, and it has been suggested by the present author (*J. chem. Educ.*, 1947, **24**, 200) that it be replaced by the term ''equant,'' abbreviated E. The word, equivalent, could then be abbreviated Eq, and a 1-E solution would contain 1 Eq/l. Such terminology would suffice for most solutions of inorganic ions.

Organic compounds in solution could then be described in terms of molarity. Thus, a solution could be called molar (M), millimolar (mM), or micromolar (μM). Unfortunately, the abbreviation mM has been used by some authors to mean millimols. A more consistent designation would be: Mols, mMols, and μMols.

One of the chief disadvantages of changing from milligram percent to millimolarity is the unfamiliarity with physiological levels in the new form. However, a few studies using the new expressions will quickly establish familiar landmarks. Since it is almost inevitable that the older terminology will become more and more inconvenient, immediate adoption of the newer expressions will shorten the period of confusion.

The recalculation of older data is quite simple, since, to convert from milligram percent to millimolarity, one merely divides by 1/10 the molecular weight. For the reverse transformation one multiplies by 1/10 the molecular weight. If the mM range is too large or too small, one can pass to the μM or M ranges, an advantage not enjoyed by the dubious expression, milligram percent. When calculating dilutions, one cannot deprecate the ease of handling concentrations in terms of molarities.

Habit will, of course, prejudice clinicians and old-line biochemists against the acceptance of the units here suggested. Chemists and investigators of metabolic problems should be quick to see the advantage of the ''new'' system. Should the staid editors of scientific journals eventually succumb to this heresy, the transition would be complete.

CHARLES BISHOP

University of Buffalo Medical School

Bird Navigation in Homing and in Migration

The recent paper by Griffin and Hock (*Science*, April 2, pp. 347–349) provides valuable confirmation of the hypothesis earlier set forth in detail by Griffin (*Quart. Rev. Biol.*, 1944, **19**, 15–31) that homing of birds can be explained, at least in large part, by random searching until familiar territory is reached. It is therefore unfortunate that they have confused the issue by speaking of navigation in migration as if it were clearly the same phenomenon as that displayed in homing.

Actually, there is little or no evidence that these two forms of navigation have the same basis, and, as Rowan (*Science*, August 24, 1945, pp. 210–211) has indicated, there is abundant evidence that they are entirely unrelated phenomena. Homing, if we accept Griffin's considerable body of evidence, is an acquired skill operating through what Griffin terms topographical memory. That it is gradually developed through prolonged experience is well recognized, at least for the racing pigeon. On the other hand, migration (as the term is commonly used, referring to a regular seasonal movement between breeding and wintering grounds that are far removed from each in latitude) appears in many birds to be a strictly inherited tendency. Rowan cites ample cases in support of this view.

This distinction is of great importance in any attempt to assess the endurance and long-range flying speed of birds. If flight direction in migration is instinctive, we may expect many such flights to be completed much more expeditiously than most of the homing flights cited by Griffin. Many flights are geared to the progress of the season or the abundance of food; but when flights are made over unattractive country or over water, we may expect them to be direct within the limits of the navigational mechanism. The data on such flights are fragmentary, for it is seldom that the exact times of departure and arrival can be determined; but many long oceanic flights seem to be made at speeds considerably above the trifling rates generally found in homing tests, which may be construed as added support for the distinction between homing and migration.

Rowan's (*Trans. roy. Soc. Canad.*, 3rd Ser., Sec. V, 1946, 123–125) test with American crows from Edmonton, released 720 miles to the east, at Portage la Prairie, and Rüppell's (*J. Orn. Lpz.*, 1944, **92**, 106–132) later work in Europe with the hooded crow indicate that in these species the inherited tendency is to fly a particular compass course or series of courses and not merely to fly to a particular point. It is unfortunate that in this particular test of Rowan's the returns were so low. If they had been more numerous, the test might have finally disposed of the hypothesis that migrating birds orient themselves by sensitivity to the earth's magnetic field; for the angle that Edmonton and Portage subtend to the magnetic north pole is quite large, and any orientation of the birds dependent on the magnetic field should presumably have been appreciably different at the two places. Ising's (*Arch. Math., Astron., Fys.*, 1946, **32A**, No. 18) suggestion that the navigational mechanism in migration may be explained by sensitivity to the effects of the Coriolis force on the semicircular canals of the inner ear has at least a sound theoretical basis, but Thorpe and Wilkinson (*Nature, Lond.*, 1946, **158**, 903–904) have shown that there are serious practical difficulties to be overcome before such an explanation can be accepted.

It is likely that the lack of an acceptable explanation of migratory navigation has tempted some workers to ascribe it to reaction to the prevailing wind or to visible aids such as landmarks or the direction of the sun. The inherited tendency in many migrants is certainly remarkable, though perhaps not more so than the habit of building a particular form of nest, but it seems beyond belief that it should enable an unaccompanied young bird to make use of landmarks; and the crow experiments cited above show that this explanation is inapplicable to these birds. Long periods of unbroken overcast over many areas of ocean and the extreme variations in wind speed and direction over short periods make it inconceivable that these factors could provide the basis for the astonishingly precise landfalls made by numbers of oceanic birds in particular. It is possible that no single mechanism will be found to be generally applicable, but I feel that Ising's hypothesis merits the most careful examination because it seems to be the only one capable of explaining a large number of cases. In the meantime, much may be learned from the observational approach. If some transoceanic migrant could be followed by airplane or radar from the start of its flight, we would at least learn whether it starts on course and would accumulate sorely needed information on the speeds maintained on such flights. The golden plover, in its autumn migration, flies from Nova Scotia to Brazil, often apparently without sighting land; and it is possible that it may continue on across the Brazilian jungle without a halt until it reaches open country. If some of the spectacular migration flights such as this could be followed for even the first few hundred miles, we would add greatly to our inadequate knowledge of them.

D. B. O. Savile

497 Golden Avenue, Ottawa, Ontario, Canada

Physical Basis of Bird Navigation

A paper by H. L. Yeagley (*J. appl. Phys.*, 1947, **18**, 1035) on the above subject describing experiments with homing pigeons is a stimulating and provocative contribution to a little understood phenomenon of nature. It has already given rise to three rejoinders by physicists (J. Slepian. *J. appl. Phys.*, 1948, **19**, 306; R. H. Varian. *J. appl. Phys.*, 1948, **19**, 306; L. Davis. *J. appl. Phys.*, 1948, **19**, 307).

I write mainly on the question as to whether birds are sensitive to magnetic fields and to their gradients. During the war the sweeping of magnetic mines called for the setting up of intense magnetic fields over a considerable area. Elementary calculations show that these fields far exceed in magnitude those which would be met with under ordinary conditions. The same is true of the gradients of the fields. Moreover, these fields extend to considerable distances below and above the surface of the sea. The complete absence of any effect on fish and on birds was apparent to everyone who had to deal with minesweeping.

If birds were guided in their navigation by geomagnetic phenomena, it would be expected that their behavior would be affected when they flew within several hundred yards of minesweepers. Yet nothing of the sort has so far been observed with such migratory birds as herring gulls and ducks or with nonmigratory birds. In many of the sweeping areas birds were rare, even though sullage was thrown out. When present, birds appeared to be supremely indifferent to magnetic fields, even at the sudden beginning of magnetic pulsing. A sweeper might pass close to a group of gulls or ducks sitting quietly on the water, yet they would completely ignore any surprise which man might provide except for food. Again, a flight of ducks might pass over the sweep with no sign of tailspin!

The hypothesis that electromotive forces set up in the body of a bird by flying through a magnetic field would excite some sensory mechanism which would help the bird in navigation does not seem plausible in view of the great variation of electric fields existing in the atmosphere. Again, the effects of the earth's rotation as manifested by the Coriolis force are small compared with gravity but still large enough to be given consideration. However, the requirements of level flying on the part of the bird seem to be excessive if the earth's rotation is to play a part in bird navigation.

The only other explanation within the ken of physicists seems to be along the lines of such things as the elevation of the sun, the stars, and the use of prevailing winds. The eyesight of birds is known to be greater than that