Comments and Communications

The American Naturalist

The American Naturalist, a journal established in 1867, became, beginning with its 85th volume in January 1951, the journal of the American Society of Naturalists, which has now assumed full editorial control. The society has designated L. C. Dunn, of Columbia University, as managing editor, and an editorial board is in process of formation. At present it consists of G. E. Hutchinson, Yale University; Thomas Park, University of Chicago; G. L. Stebbins, Jr., University of California; and Conway Zirkle, University of Pennsylvania. Jaques Cattell will continue as publisher, and the journal will be issued six times a year by the Science Press, of Lancaster, Pennsylvania.

The policy of the journal will be to continue its service to general biologists and naturalists through the publication of general essays, addresses, and symposia of biological societies which in the opinion of the editorial board contribute substantially to the purpose of the society in "correlating the various biological sciences."

It will continue to publish papers reporting new research, giving preference to those in which the chief interest inheres in theoretical interpretation and synthesis. It will make a special effort to publish promptly brief reports of new research, comments, and criticisms of material published in *The American Naturalist* or elsewhere, especially when these can be put in the form of concise letters to the editor. It will welcome such papers from naturalists in any country. Intending contributors are invited to consult the current number of the *Naturalist* (January 1951) for style and form to be used in preparation of manuscripts. Manuscripts and editorial correspondence should be addressed to: Editor, The American Naturalist, Box 2, Schermerhorn Hall, Columbia University, New York 27.

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Intracellular Fat Deposits in the Kidney

THE recent paper by Dallemagne, Gerebtzoff, and Philippot (*Science,* **112,** 143 [1950]), regarding fat deposits in the kidney of the dog, may have called forth some comment from workers in the field.

The presence of fat in the renal tubular epithelium of the dog has been known for many years. The best recent description that I know is the work of Foote and Grafflin (*Anat. Rec.,* **72,** 169 [1938]), who studied teased preparations of the kidney.

Any statement regarding toxic effects upon the canine kidney manifested by changes in fat content would have to demonstrate significantly larger amounts than are present normally, or would have to demonstrate the presence of fat in a location other than the proximal convoluted tubules.

The photomicrograph shown by the authors (*op. cit.*) does appear to demonstrate more fat than one usually sees in the normal animal, but the diet of the animals would have to be carefully controlled in order to be certain that this is not a normal variation.

Their concluding statement, "This specific intracellular fat deposit in the kidney is bound to the still unelucidated biochemical lesion induced by hexachlorocyclohexane," is not justified.

The problem can be further elucidated by the use of other experimental animals, but in the light of present knowledge, the presence of fat in the dog's kidney must be regarded as other than "specific."

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Measurements in Colloidal Systems

A BRIEF article under a similar title, by Jenny *et al.,* in the August 11 issue of SCIENCE raises questions of great importance in colloid chemistry. The customary electrochemical approach is abandoned by these authors. Starting from a theoretical consideration of the liquid junction potential arising between a salt bridge and a colloidal system, they proceed to discard entirely Donnan potentials between colloidal systems and true solutions in contact with them, and finally even to deny that analytical evidence for a Donnan situation exists in their experiments.

This is indeed a radical departure. Must chemists abandon the whole of Donnan theory? Is the evidence for it so feeble that it can be overturned by raising questions as to the elimination of liquid junction potentials by a saturated KCl bridge? The answer is obvious. Something must be wrong with these authors' interpretation of their own experiments. A detailed consideration of the latter is thus called for. Fortunately the article, although highly condensed, is sufficiently informative for this purpose. The pivot of discussion turns on potentiometric and analytical observations in a cell of the type

Sat. Calomel Sat. KCl	K^+ Colloid + KCl	KCl	Sat.Calomel Sat. KCl
	Y		

According to Donnan theory, the chemical potential of the molecular species KCl should, at equilibrium, be the same on both sides of the membrane, or boundary, *Y*. Since, however, the K colloid is not diffusible, this condition can only be met when the activities of chloride ions on the two sides are different. This difference in activity can then be used to calculate, by the Nernst equation, a boundary potential. Numerous experiments in the literature have shown that this calculated potential agrees very well with the measured total potential of the above cell.

In the work here under consideration the statement is explicitly made that the chloride ion concentrations were the same on both sides of Y, yet potential differences were recorded. The key to this anomaly may be found in the following sentence: "Even after long standing, the Cl concentration of solution removed by rapid filtration from the right-hand compartment was identical with that of the solution in the left-hand compartment." Here the authors reveal a fundamental misunderstanding of the requirements of the Donnan situation. The Donnan theory is based on the postulate that the ionizing colloid is constrained in some way from attaining uniform distribution throughout the whole system. The nature of the constraint is immaterial. It can be furnished by a membrane or a filter, or by centrifugal or gravity fields of force, or by internal forces within a gel. What the authors do in performing a filtration in order to sample the colloid + salt system is simply to provide a different constraint. Therefore, what they get by filtration in the early stages will, of necessity, be identical with the solution that came to equilibrium across a membrane.

Their experimental result, therefore, is in precise agreement with Donnan principles. In order to test the matter in which they are interested—namely, whether the chloride ion concentrations on both sides of the membrane are in accord with the observed difference of potential—they should proceed differently. A sample of the colloid + salt system should be removed, the total chloride determined, and allowance made for the volume of solid phase present. In this way a valid chloride ion concentration may be obtained. This is the accepted procedure, and the authors have gravely misled themselves by departing from it.

There is thus absolutely no warrant for the rejection of Donnan theory, and we may now proceed to examine their other experiments with this in mind. The results presented in Table 1 of their paper, derived from a consideration of the cell

$$\begin{array}{l|l|l|l|l} \text{Sat. Calomel} & \text{KCl} & \text{Cation exchanger} & \text{KCl} & \text{Sat. Calomel}\\ \text{Sat. KCl} & A_2 & +\,\text{KCl} & A_1 & \text{Sat. KCl}\\ & X & & Y & \end{array}$$

are extremely interesting. At the outset they assume that the KCl in contact with the cation exchanger is, on the one side, at the same concentration as A_1, although the Donnan equilibrium precludes this. They now make a distinction between the two boundaries X and Y, stating that they believe a potential to exist at X, but not at Y. The present writer fails to see any fundamental difference between X and Y in this respect. When the KCl on one side becomes highly concentrated, then we believe that the corresponding potential becomes small.

The measurements represent, of course, the algebraic sum of the two potentials. This cell is, in fact, similar to one utilized by Mukherjee and Marshall (paper presented at the Colloid Symposium, June 1950) in testing the membrane functions of gels and pastes, based on theoretical considerations discussed earlier by Marshall (*1*). Through use of this approach, a plug of ionizing gel can be regarded as equivalent to a charged membrane. Below a certain value, measurements made using different cation activities on the two sides will show agreement with the ordinary Nernst equation; above this value the considerations brought forward by Meyer and Sievers (using Donnan principles) become pertinent (*2*). Actually the results in Table 1 are exactly as one would expect from this. The exchange material ion-X gives good selective membrane characteristics as regards cations up to an activity of 0.005, possibly somewhat higher, since the cell which employs 0.044 KCl against 0.0046 gives a potential within 4 mv of the Nernst value.

This raises the question as to the validity of the equation utilized in calculating transport numbers across a single liquid junction between a colloidal system and a salt solution. Since no details of the assumptions or the integration are given, little can be said, except to point out that Meyer and Sievers showed that, by the employment of Donnan principles, the relevant transport numbers could be calculated. Thus they opened up an approach which, when we deal with highly concentrated and highly ionized colloidal systems, may enable us to correct the observed results for the potential existing at the junction with the KCl bridge. So far this has not been done, but it should not be lost sight of, now that concentrated systems of highly ionized exchange resins are available for experiment. The main value of the paper under consideration is that it draws attention to this need. With a saturated KCl bridge, so long as the ionic activity of the cation associated with the colloid remains below 0.1, it is not likely to amount to more than a trivial correction.

One further comment regarding potentiometric measurements with glass membrane electrodes on highly concentrated systems is pertinent. The results, as Dole (*3*) has pointed out, depend both on the pH difference on the two sides of the glass and also on any difference in the chemical potential of the water. In very stiff pastes or gels the latter factor may well be of importance.

In conclusion, therefore, the experiments conducted by Jenny *et al.* in no way warrant the rejection of the Donnan principles or the further conclusions arising from this rejection.

References

1. Marshall, C. E. *J. Phys. Coll. Chem.*, **52**, 1284 (1948).
2. Meyer, K. H., and Sievers, J. F. *Helv. Chem. Acta,* **19**, 649, 665, 987 (1936).
3. Dole, M. *The Glass Electrode.* New York: Wiley (1941).

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