him, inevitably and improperly, on the ostensible ground that the matter is not yet proved and therefore should not be supported. If it had been proved, we should have been asking for money for some other purpose.

A few medical men have been interested in our point of view. We thank them for this and we hope that we may retain their sympathy in the stormy days that are to come.

The medical profession, as a whole, is hostile to us, due to the attitude of those who should be the leaders. So far as we know, not a single medical school or hospital has shown any active, intelligent interest in our work. Two deans of medical schools have been good enough to make clear to us what the attitude of the medical profession is. We have confirmed the following views independently:

(1) Nothing good along lines of research involving living tissue can come from chemists.

(2) Our line of reasoning is foreign to the medical mind and the authorities in the medical profession consequently consider our work and conclusions so unsound that it would be a waste of time to check either.

(3) Since our experimental work is bad by hypothesis, one hundred or even five hundred cases would not be convincing, because one hundred or five hundred experiments done badly have no cumulative value.

(4) Since our conclusions are unsound by definition, any doctor confirming our findings proves thereby that he is incompetent to do that type of research.

At Beaufort, N. C., Miss Koehring has shown that treating a starfish with ether or chloroform causes a reversible agglomeration of some of the proteins in the walls of the stomach. Though this experiment can presumably be confirmed by anybody who is interested, the experimental results carry no weight in medical science against an *obiter dictum*. The theory of Claude Bernard on anesthesia is to be considered wrong; not because it is wrong but because we have shown it to be a first-class working hypothesis.

Pauli's work along similar lines to ours has stood uncriticized for about thirty years, but is now automatically and officially worthless because it confirms our results.

Our answer to the medical profession is simple. It is up to them to clean house. From now on it is a fight to the finish between the medical profession and ourselves. There can be only one outcome to this contest. The medical profession will lose. The medical profession—or their unwise leaders—can and probably will retard progress; but they can not prevent progress completely.

We challenge the medical profession to run fair tests of our treatment, with all experimental details released, on certain forms of alcoholism, insomnia and sciatica. These are selected because even a medical man should get good results the first time. The medical profession does not dare make these tests, because the results would show that we are right. The medical profession can not admit that they do not dare to make these tests, because that would prove that the medical leaders are wrong. The medical profession can not treat us with dignified contempt, because that is confession in view of the fact that this is not a commercial venture.

If all data are released of any tests that are run, we can and will expose the faulty technique or the misunderstanding, which will probably occur in the future as it has in the past. Individual medical men have said that it would take twenty years to test our views properly. That is nonsense so far as we are concerned. If sufficient material is available, tests satisfactory to us can be made in a few months. There will come a time when the intelligent medical men—and there are such—will resent the false position into which they have been led by following the priests of Baal.

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A PASTEURELLA-LIKE MICROORGANISM IN THE BRAINS OF HORSES SUFFERING FROM SO-CALLED CORNSTALK DISEASE

STUDIES at the Laboratory of Animal Pathology and Hygiene of the Illinois Agricultural Experiment Station on the etiology of an acute encephalitic disease of horses, referred to as cornstalk disease, cerebro-spinal meningitis, staggers, blind staggers, meningitis, forage poisoning, etc., which occurred in Illinois during the fall of 1934, have given consideration to the presence of filterable agents, pathogenic molds and bacteria, as well as toxic chemical substances. For the reason that such investigations require long periods of time for their completion, preliminary observations on the bacterial flora of the brains of affected horses are being reported, in part, at this time.

Animal inoculations of the brain tissue suspensions in saline of two horses, together with the inoculation of five mixed cultures made from the brains of horses, yielded pure cultures of a pathogen possessing the characters of the pasteurella group. The seven horses supplying material for these studies originated on seven different farms in three counties. In six of the seven brains, visible areas of degeneration were encountered in the cerebrum. Pasteurellosis infection has long been recognized as an etiologic factor in so-called cornstalk disease of cattle, but so far as the

writer has been able to determine, pasteurella equiseptica-like strains have not heretofore been isolated from the brains of horses suffering from so-called cornstalk disease.

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A FRESH WATER SPONGE FROM SOUTHERN CALIFORNIA

FRESH-WATER sponges are rare in California, largely because of the scarcity of permanent streams. This is especially true of so-called Southern California, south of the San Gabriel or Sierra Madre mountains. It appears, in fact, that hitherto no fresh-water sponge has ever been reported in this part of the state. On October 13, 1934, a student, Mr. Donald Nelson, found and a few days later brought to my attention such a sponge, *Asteromeyenia plumosa* (Weltner) Annandale. This is a rare species, originally described from Kinney County, Texas, and having as its only other reported locality Shreveport, Louisiana. The two Southern California specimens were each about the size of the palm of a man's hand, growing in a cement weir box which is part of an irrigation system, near Fullerton (just southeast of Los Angeles). The source of water is the Santa Ana River, which runs deep in winter, but is often dry in the summer. The specimen collected was well provided with gemmules and is typical to the most minute degree of the species as previously described.

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SCIENTIFIC BOOKS

ELECTROLYTES

Electrolytes. By HANS FALKENHAGEN, professor in the University of Köln. Translated by R. P. Bell, fellow of Balliol College, Oxford. Royal 8vo, pp. 346. $9.50. Oxford University Press.

THE motif of this comprehensive monograph is "not only to give the most important theoretical principles in the domain of electrolytes, but also to give the reader some idea of methods of experimental investigation and the reliable experimental results obtained." This statement obviously implies the notable advances made by Debye and his followers, but readers interested in topics closely allied to electrolytes will find the English translation extremely valuable.

Although the present book is for the greater part simply a translation of the 1932 German edition, it has been revised in consultation with the author to bring it into line with the experimental and theoretical advances of the past two years. The added topics embrace: theoretical and experimental work on transport numbers, Onsager's treatment of the dissociation field effect, the extension by Fuoss and Kraus of Bjerrum's theory of ion-association and finally an appendix by R. H. Fowler illuminating R. H. Gurney's application of quantum mechanics to electrode processes.

The author does not assume that, since the basic subject-matter is a time-honored one, all readers will be prepared to comprehend the intricacies of such specialized topics as those just enumerated. To this end, he devotes the first six chapters to an elementary and well-organized presentation of the problems of the equilibrium state and the irreversible process of conductance in a way which should prove helpful to one who is approaching the subject for the first time.

The thermodynamic treatment follows closely the classical methods of Planck, modified, of course, to embrace the activity concept of G. N. Lewis. Although disciples of the American and Danish schools of physical chemistry would doubtless prefer a more concise and less labored development, nevertheless the thermodynamics is eminently sound and consistent. In chapters 7 to 10, the principles of the Debye-Milner theory are developed pictorially, then mathematically and finally tested in their limiting forms as explanations of the solubility influences of ions upon ions, the salting-out effect of ions upon neutral molecules, heats of dilution and dependence of conductance upon concentration (Onsager's theory), viscosity, frequency and field strength (Wien effect).

The author has made notable contributions, in collaboration with Debye, on the intricate problem of the frequency and field strength effects, and hence is well qualified to present the subject. Although there is now available a wealth of experimental data supporting the theory in its numerous aspects, the author selects examples which not only substantiate his case, but give appropriate credit to pioneer workers in the field.

The title of Chapter 11, "More Concentrated Solutions," may prove somewhat disappointing in that one who has not been dealing with the subject might expect that the concentrated solutions of industrial importance are to be discussed. As a matter of fact, the term refers primarily to that all too dilute range of concentrations for which it is necessary to consider the ion-size parameter "a" as a correction to the limiting laws—to account for the specific effects of individual electrolytes. The Hückel formula which is based upon the assumption of specific linear decrease of dielectric constant with concentration and which reproduces the experimental results of really concentrated solutions (0.1 to 4 M) is dismissed as little