initially contains an amount of an electrolyte which is greater than what will correspond to the maximum in the cataphoretic speed-concentration curve of that colloid with the particular electrolyte, the charge on the colloid when it is subjected to dialysis will first increase and then decrease. On the other hand, if the amount of the electrolyte initially present is equal to or less than what will correspond to the maximum in the cataphoretic speed-concentration curve, the charge on the colloid will continuously decrease with the progress of dialysis.

The changes which will be produced in the charge on the particles of a colloidal solution with the progress of dialysis are not so simple as many colloid chemists seem to imagine. It is clear that results of viscosity and stability as determined by the coagulating concentration of an electrolyte cannot always be utilized for getting an idea about the charge on the colloidal particles, for whether there is any relationship between charge and viscosity and charge and stability will depend upon the amount of the stabilising agent initially present in the sol besides other factors like hydration, etc. Under the circumstances it is difficult to understand how far one should consider as satisfactory the interpretations of the results of coagulation of colloids by electrolytes whenever inferences have been drawn from those results about charge on the colloidal particles ; also one cannot get an idea of the extent to which preferential adsorption of either the stabilising or of the coagulating ion takes place from viscosity and flocculation value determinations. It is, therefore, necessary that simultaneous measurements of charge, viscosity, flocculation value, etc., of every colloidal solution containing varying amounts of the peptising agent (this can be done by subjecting the colloidal solution to dialysis for different periods) should be made in order to get a clear idea about the relationship between charge and other properties. In our Laboratory we are investigating various colloidal solutions from these points of view.

Present Position of the Problem of Spike Disease.*

By M. Sreenivasaya.

TWO schools of thought have influenced the study of Spike Disease both in the field and in the laboratory, since its discovery by McCarthy in the year 1899. Exponents of the physiological school believed that the characteristic symptoms induced in sandal are due to the imposition of an unfavourable environment, brought about by drought, fire, deprival or death of host plants, unbalanced sap circulation, unfavourable host plants and other purely physiological causes. In 1917, Dr. Coleman lent brilliant experimental support to the "infectious theory" of spike disease by the experimental disease transmissions he was able to effect by cleft grafting, the scion for the operation being derived from a diseased plant. This achievement marks a definite stage in the history of spike investigation.

Cleft grafting is a difficult technique; the percentage of success is small, even in the hands of the expert: the operation involved the cutting back of the stock and this gave the plant "a severe physiological shock," in the words of the exponents of the physiological theory, who tried to explain away the most important and definite result achieved by Dr. Coleman.

It is true that his experiments were conducted on stocks growing under natural conditions; it is also true that the host plants nourishing the operated stocks were not determined. Other methods of transmission by budding and sap injection had failed. It was at this stage, that the problem was taken up by the Indian Institute of Science in the year 1927, the Government of Madras and the Commission of Coorg having generously agreed to finance the scheme proposed by Dr. Norris.

Culture of sandal plants in pots in association with known species of hosts marked the next important stage in the progress of Spike Disease investigation. This achievement simple as it appears at the moment, helped to remove the reproach inherent to experiments conducted under un-controlled, natural conditions where many unknown and non-determinable factors operate. All experiments conducted at the Institute have been done with pot cultured sandal plants, whose nourishing host, age and physiological condition are all definitely known.

The development and perfection of an easy and an effectively reproducible artificial disease transmission is an important step indispensable to the progress of the investigation; new methods of disease transmission, extremely simple and elegant involving no "physiological shock" to the operated stock, have now been evolved; the weight of the infective material has been reduced to a few milligrams of diseased tissue. This method which has lent itself to quantitative control, has been of immense value in evaluating the relative resistance or comparative suscepti-

* Abstract of a lecture delivered at Coimbatore, under the auspices of the Society of Biological Chemists, India, on 8th October 1932.

bility of individual sandal plants growing under different conditions of environment; the technique has also been employed in determining the resistance offered by a composite environment to artificial infection.

The simplest and the readiest way of diagnosing spike is through the external symptoms; but the method often fails even in the case of the experienced observer. This is how the exponents of the physiological theory have been misled and have mistaken these symptoms generally produced through drought, fire, etc., as those of genuine spike. Symptoms produced through physiological causes are not communicable to other healthy plants through grafting while those of genuine spike are readily transmissible. Communicability of the symptom from one plant to another is the criterion on which infectious nature of spike disease has been firmly established.

Relative immunity can be imparted to the sandal plants by nourishing them with certain types of host plants, generally non-leguminous. *Pongamia glabra*, *Cajanus indicus* and *Acacias*, generally those which favour a rapid growth of the parasite, render it particularly susceptible to disease. The observation is borne out not only by ecological survey of the diseased and healthy areas, but also in the regeneration plots where only those associated with leguminous hosts have succumbed to the disease.

Mr. Dover of the Forest Research Institute, Dehra Dun, supplemented the lecture with an account of the entomological work which is at present mainly directed to prove whether insects are vectors or not.

Letters to the Editor.

Nomenclature of Shell Layers.

MAY I crave the courtesy of the columns of the *Current Science* to reply to a criticism of the terminology for the various shell-layers as used in my recent Memoir on *Pila* in the *Indian Zoological Memoirs'* series, by Dr. C. Amirthalingam in his note entitled "Correlation of Sex and Shell Structure in a Mollusc—*Trochus niloticus* Linn," published in your September issue? In his first footnote Dr. Amirthalingam remarks, "The terms used here are those generally accepted by English-speaking authors; those used in a Memoir on *Pila* (*The Indian Zoological Memoirs*) differ from this terminology." Leaving out of consideration the use of the structural names, nacreous and prismatic layers, the main differences in the terms employed by Dr. Amirthalingam and myself are in reference to the use of the terms Ostracum and Hypostracum. On a reference to the literature it will be seen that the terms ostracum and hypostracum were introduced into literature by Thiele[1] in 1892. The term hypostracum was used by Thiele in a loose sense for the innermost layer of the shell. In shells of *Chiton* he designated the entire innermost layer as the hypostracum, but in the account of *Patella* he restricted this name to the shell-layer in the region of attachment of the columellar muscle. Later in the same paper when describing the shell of *Gibbula magus*,—a Trachid,—he designated the innermost shell-layers of the upper whorls of the shell, which are in no way connected with the columellar muscle, as the hypostracum. This confusion in the use of the term hypostracum was pointed out by Simroth[2] who concluded "Es ist whol klar, dass man diese locale, unbedeutende Schicht kaum mit Thiele (s.o.) als Hypostracum deuten kann. Wenn man den Ausdruck festhalten will, dann kann bloss die Perlmutterschicht, sowiet sie von der ganzen Mantelfläche abgeschieden wird, bezw. die innere vierte Schicht von *Buccinum* als Hypostracum gelten." The usage of the term hypostracum in the original confused sense of Thiele is followed in an English text-book by Pelseneer,[3] the famous Belgian malacologist and not an "English-speaking" worker, while I have used it in the modified sense of Simroth, as has been done by other workers like Robert[4] in Trochidæ and Bergenhayn[5] for Mollusca in general.

I have also to enter here a protest against the introduction of the new term—gonidial twist—which is apparently meant to replace

[1] Thiele, J., *Zeitschr. wiss. Zool.*, **55**, 220, 1892.

[2] Simroth, H., *Gastropoda-prosobranchia in Bronn's Tierreichs-Mollusca*, **3** (2), 232, 1899.

[3] Pelseneer, P., *Lankester's Treatise of Zoology*, Pt. V, *Mollusca*, p. 4, 1906.

[4] Robert, A., *Zool. Descriptive*, II, p. 382, 1900.

[5] Bergenhayn, J. R. M., *Kungl. Svensk.-Akad. Handl.*, (3) **9**, No. 3, p. 1, 1930.