him for accumulation demand the utmost concentration of effort. What will be the result of this intense strain, and the effects of such a time limit on what the average man hopes to accomplish, can only be ascertained by future observation. But the situation is inevitable, and the young man is wise who heeds for his future the counsel and experience of his elders.

It would seem that these various influences have an important bearing on the present and future conditions of practical education in determining what the student may expect who seeks a suitable preparation for the profession that will absorb his best energies and that shall constitute his life-work. It is of less consequence where he is educated, provided he attains the mental poise and attitude that enable him to grasp fundamental truths and to apply them correctly in the accomplishment of great undertakings.

C. F. MABERY
CASE SCHOOL OF APPLIED SCIENCE

SECTION K—PHYSIOLOGY AND EXPERIMENTAL MEDICINE

SUMMARY OF THE PROCEEDINGS

THERE were three meetings of the section during convocation week.

The first session was convened on Thursday, December 27, at 2:15 P.M., at the College of Physicians and Surgeons, in the presence of an unusually large audience. At this meeting the officers for the year 1907–8 were elected; the retiring chairman, Professor William T. Sedgwick, delivered the annual address; and a symposium was held on the subject of 'Protozoa as Factors in Disease.'

The second session was held on Friday, December 28, at 10 A.M., at the Rockefeller Institute for Medical Research, in affiliation with the Society of American Bacteriologists. Twelve papers were presented.

The third session was held on Saturday, December 29, at 10 A.M., at the Rockefeller Institute for Medical Research, in affiliation with the American Physiological Society. Sixteen communications were offered.

EXECUTIVE PROCEEDINGS

The following officers were elected for 1907–8:

Vice-president and chairman of the Section—Ludvig Hektoen.

Secretary—William J. Gies.

Sectional committee—Simon Flexner, vice-president, 1906–'07; Ludvig Hektoen, vice-president, 1907–'08; William J. Gies, secretary, 1905–'08; Charles S. Minot (one year); J. McK. Cattell (two years); Frederick G. Novy (three years); Graham Lusk (four years); Jacques Loeb (five years).

Member of the Council—S. J. Meltzer.

Member of the General Committee—Edward K. Dunham.

SCIENTIFIC PROCEEDINGS

Program of the First Session, December 27, 1906

Vice-presidential address—'The Expansion of Physiology': William T. Sedgwick. (Published in SCIENCE, this volume, page 332.)

Symposium on Protozoa as Factors in Disease:

Introductory remarks by the chairman: Simon Flexner.

'The Protozoa from the Standpoint of the General Naturalist': Edmund B. Wilson.

'Some General Principles in connection with Protozoa as Factors in Disease': C. W. Stiles.

'The Protozoan Species': Gary N. Calkins.

'The Morphological Diagnosis of Pathogenic Protozoa': James Ewing.

'Immunity against Trypanosomes': F. G. Novy.

General discussion by William H. Welch, Henry B. Ward and James Carroll.

The Protozoa from the Standpoint of the General Naturalist: Edmund B. Wilson.

The zoologist who is asked to open a discussion on pathogenic protozoa with a few remarks on these animals as they appear from the standpoint of the general naturalist must approach his task with somewhat mixed feelings. There is hardly a field in zoology more interesting or fuller of suggestion; but neither has any other been oftener traversed or more widely exploited. Indeed the subject has been so often discussed, its importance is so well understood, that I am tempted to begin and end with the celebrated remark of Colonel Ingham's double that "there has been so much said, and on the whole so well said, that I will not further occupy the time." And yet there are certain aspects of the subject to which one may again and again return without loss of interest, which are a perennial spring of new ideas and new research. First and foremost among these is the fundamental analogy pointed out by Virchow and Haeckel between the animal body and an organized social state. The conception that the multicellular body is a 'cell-state'—a community of cooperating elementary organisms that are individually comparable to protozoa—made a deep and lasting mark on all morphology, physiology and pathology. Apparently there is no end to the fruits that it has produced, continues to yield, and seems likely to bring forth hereafter. The conception is no doubt an inadequate one. There are some, perhaps many, biological processes that can not adequately or profitably be considered from this point of view alone. Especially in the field of growth and development there are processes that are better treated as the action of a single and indivisible physiological unit than as a resultant of cooperating cell-activities. But, whatever its limitation may be, the conception of the 'cell state' remains one of the most brilliant, interesting and fruitful of the fundamental generalizations of biology. It is part of the air we breathe in every biological laboratory from the day we first sit down to the microscope, and in one way or other it pervades the whole tissue of our work. As such it needs no analysis at my hands.

But there are certain applications of this conception to some of the broader questions of our science on which for a moment I may appropriately dwell. For instance, our conception of heredity, on which the whole modern theory of evolution turns, has been profoundly affected by the phenomena of reproduction in the protozoa; and the same is true of the whole constellation of problems relating to sexuality, the duration of life, old age, and the renewal of vitality by fertilization, all of which are in close relation to the problems of heredity. A great number of the modern researches on these questions can be recognized as fruits of the celebrated comparison, drawn by Bütschli thirty years ago, between the life-cycle of a protozoan race and that of a multicellular animal. Fertilization of the egg is analogous to the conjugation of the protozoa; cleavage and development to the successive divisions of the ex-conjugants and their descendants; maturity, decline and old age in the multicellular organism to the physiological balance and the ensuing gradual failure of vitality after prolonged vegetative reproduction in the protozoa; the stimulus to development given by fertilization is comparable to the renewal of vitality that follows conjugation in the protozoa. This comparison has directly or indirectly stimulated a multitude of important and interesting researches on the fertilization of the egg, on artificial parthenogenesis, on the chemical renewal of vitality in the protozoa, on the stages of growth and decline, on the causes of old age and natural death, and many others. There is not one of these questions that can be adequately considered apart from the phenomena exhibited by the protozoa. It is my impression that we are more likely to solve these problems, in so far as they can be solved, by studies on the protozoa than by investigation on the higher forms. Some of the researches in this field have definitely brought into the arena of scientific experiment and discussion the old dream of the alchemists of the artificial prolongation of life, perhaps of human life. If this is no longer a fantastic vision of pseudo-science but has won a place among the legitimate subjects of scientific inquiry, it is in no small measure owing to investigations on the protozoa.

If we turn for a moment to the study of heredity and evolution, here again it was in considerable measure a comparison of the life histories of protozoa with those of metazoa that necessitated a readjustment of many views that had long been held, almost without question, as applicable to the higher forms of life. Considerations on reproduction and the duration of life in protozoa formed the starting point of Weismann's inquiries on heredity that resulted in a total denial by him of the inheritance of acquired characters in higher organisms—a supposed factor that formed an important part of Darwin's general theory and was the very corner-stone of Herbert Spencer's philosophy of organic nature. Whether Weismann was right remains to be seen. Naturalists are still in disagreement as to whether acquired characters are inherited or not. But there can be no doubt that Nussbaum and Weismann altered our whole point of view by fixing attention on the fact that heredity is effected in metazoa, as in protozoa, by cell-division; that just as protozoa inherit the characteristics of the mother cell because directly derived from it by division, so the body of the higher animal inherits not from the body of the parent but from the egg, because derived from it by division. This simple fact, while it places the basis of heredity in its true light, enormously increases the difficulty of accepting the inheritance of acquired characteristics of the body. That such an inheritance of acquired characters is difficult to conceive does not, of course, prove that it is not a fact; but the arguments urged by Weismann were of such force as to demonstrate the imperative need of a reexamination of the whole question. The illuminating view of heredity brought forward by Nussbaum and Weismann, primarily suggested, I repeat, by the mode of reproduction in protozoa, has made a deep mark on biological research, and has led to reinvestigation of many conclusions that had naïvely been taken for granted, with no realizing sense of the formidable difficulties that they involved, or the weakness of the evidence on which they had been based. Thus the study of the protozoa has had its effect on every part of biology that is concerned with questions of historical descent; and I do not think it forced to say that these creatures may have something to teach us in every department of thought into which evolutionary considerations enter.

It was Weismann's view that the protozoa are endowed with never-ending life. From the physical point of view, this conclusion was not altogether well founded, at least in the sense in which he meant it; but metaphorically speaking it was true. They certainly possess a vitality that seems without limit as a power to animate scientific research, to invade new fields of discovery. Their latest achievement is an irruption into the domain of pathology, and the inroads they have already made are sufficiently attested by the choice of the subject of this discussion. I feel myself here on treacherous ground and will venture no prediction as to whither the new subject of medical protozoology may lead us. But evidently the bacteria must look to their laurels with so formidable a competitor in the field. It is within the bounds of possibility that the discovery of the causal relation between protozoa and disease may one day rank with the greatest of those that general biology owes to the study of these simple animals.

Some General Principles in connection with Protozoa as Factors in Disease: C. W. Stiles.

The speaker first directed attention to the difficulties of classification and the necessity for extensive academic work on protozoology independent of the subject of applied protozoology. Because of incomplete knowledge of the protozoa, we must expect to meet with extensive differences of opinion in reference to numerous species or alleged species, for some years to come. These differences in interpretation are at present inevitable and should be accepted as having a right to existence; they do not necessarily indicate any special prejudice or unreasonableness on the part of contending authors. Zoologists are at present not in a position to concisely define the protozoa as a systematic unit, nor can they yet define sharply the different groups from one another. We must, therefore, have patience at present and receive with open minds, although with some reserve, many interpretations that are published. He referred to the two biologic rules he had enunciated in 1901, to the effect that diseases which are conveyed mechanically by arthropods (as facultative carriers) may be due to either plant or to animal parasites, but chiefly to the former, while diseases which are conveyed biologically by arthropods (as obligatory carriers) are, so far as known, due to animal parasites. In discussing the relation of Rocky Mountain fever, African tick fever and chicken spirillosis, to these same rules, he held that we must await further study in the case of Rocky Mountain spotted fever, while in the case of tick fever and chicken spirillosis it has not yet been proved that the ticks are biological (obligatory) carriers. In chicken spirillosis the disease has, according to literature, been conveyed through feeding the feces of a sick animal to a well animal, hence in nature, this disease does not seem biologically dependent on the ticks. As for African fever, there are certain indications, though as yet no proof, that this can be conveyed as a wound disease.

Passing to yellow fever, the speaker thought that despite certain opinions to the contrary, the present indications remain in favor of a protozoal rather than a protophytic origin.

The Protozoan Species: Gary N. Calkins.

Formerly it was the custom to base protozoan species on the structure of the single cell. This made it very easy to make new species, for any novice with a microscope might see known organisms new to him and describe them as new species thus burdening the literature with names and pseudonyms. On this former basis there were so few points of structure to base species upon that they were difficult to accurately define.

A species in zoology is an abstract conception of a group of animals based upon similarity in structure, habits and mode of life and power of reproducing amongst themselves offspring identical with themselves.

In the single individual of a species we usually recognize more or less clearly defined variations in vitality and sequence of changes which we designate as youth, adolescence and old age, the whole constituting a life history.

In the protozoan species, according to the old custom in taxonomy, these criteria of species can not be maintained. The individual, in the first place, has no life history to speak of; it is formed by division of a cell; it lives only a few hours as an independent cell and then disappears, its substance going into two or many new cells. There is no indication of the changes in vitality characteristic of individuals of metazoa, that is, no trace of periods of youth, adolescence and old age while natural death of the individual is unknown. Furthermore, in such individuals the criterion of offspring which exactly resemble the parents is not observed, for cells may be formed in the course of the several divisions which do not at all resemble the parent cell and which seen independently would warrant interpretation as a new species. Thus in *Tetramitus* or *Cercomonas* the perfectly rigid contour of the ordinary 'individual' is very different from the amœboid 'individual' which is ultimately formed. On the basis of such variable 'individuals' the protozoan species must be of questionable taxonomic value.

A new basis for the conception of protozoan species was given by Schaudinn in 1900. Always interested more particularly in the life history of protozoa, he gave from time to time more or less complete accounts of the life cycle of different forms, *e. g.*, *Calcituba, Leydenia, Polystomella, Paramœba, Trichosphærium,* etc., while in this year he founded *Coccidium schubergi* on the basis of the complete life cycle and gave us a model which later students of the group have tried to follow.

The new method of taxonomic research which Schaudinn started has resulted in a far more profound knowledge of protozoan species. A number of supposedly different varieties, species and even genera have been found to be only stages in some life cycle. For example, microsphæric and macrosphæric shells of foraminifera are now known to be only stages in the life history of the individual foraminiferon, and in my own experience some of the commonest forms of microscopic life are found to be curiously related. Thus an organism which formerly any student of the protozoa would have described as a species of the genus *Pelomyxa,* is found to be only a stage in the life history of *Amœba;* and the supposedly different species of *Paramecium*—*caudatum* and *aurelia*—are found by continuous culture in their natural habitat to be one and the same species.

Other examples might be given to show how the 'individual' in the old sense varies from time to time and thus becomes a most unstable subject of protozoan species. For this reason, I have urged that the old idea of the protozoan 'individual' be discarded and the life cycle substituted, and I would recognize as the individual, not the single cell but the entire aggregate of cells that are formed from the time of one conjugation up to natural death of the protoplasm resulting from it, or until syngamous union of that protoplasm with similar substance from another individual.

In such an individual we recognize periods of varying vitality which have the same sequence as in metazoa, and we can find characteristic features which indicate the period of youth, of adolescence or old age as in a metazoon. The variations in vitality may be represented by a more or less regular curve in which these periods are clearly marked out.

It is particularly important that species limits should be clearly defined among the pathogenic protozoa. Here as yet, however, there are very few that are based upon the full life cycle, most of them indeed are on the basis of a few individual cells. For example, Woodcock in his review of the hæmoflagellates enumerates no less than forty-eight species of *Trypanosoma* and adds two additional ones as questionable. I do not wish to be too severe, but I imagine it would be nearer the truth had the account been of two species and forty-eight questionable ones. In this group species are based for the most part on unmeasurable differences in structure; often indeed no such differences even can be made out—but species are defined on strictly physiological characters, such as life in different hosts, inability to live on certain culture media, different reactions towards immune sera, etc., none of which are sufficiently characteristic of species, although they may well indicate specific differences.

In very few cases has the life history of the trypanosome been made out, but the work of Schaudinn, Keysselitz and Prowazek indicates that we have to do with digenetic forms, while Novy's conclusions are the reverse. It seems to me that the culture method can not be relied on absolutely in the testing of protozoan species; to cultivate parasitic protozoa on media is to cultivate them in one phase only of their life history, and since the most important phases have never been seen in the cultivated forms it is probable that the period of youth of the individuals is the best for cultivation. Inability to live in the same medium is no test of a good species. There are many species of protozoa which live both in salt and in fresh water; either if transferred suddenly to the other medium would die, but either could be gradually trained to live in the other medium. So it is, as I believe, with *Trypanosoma,* and the multiplication of species here is, to my mind, only evidence of our ignorance of their life history.

What is true of *Trypanosoma* is even more evident in the case of *Spirochæta.* Here, every month or so, some student of the group describes a new species of *Spirochæta* before we have found out even whether the genus to which it is ascribed belongs to the Schizomycetes or to the Mastigophora. Again the variations in structure of the various spirochætes are so great that if they were all true we should have to make new generic names to hold them. *S. obermeieri,* for example, has the chromatoid granules of a bacillus, it reproduces by transverse division, and is, in its morphological characters much more like the spirilla type than like the flagellate forms. *S. plicatilis,* on the other hand, has longitudinal division, a periplast membrane and nucleus similar to that of the male trypanosome, according to Prowazek. *S. pallida,* finally, has flagella, and reproduces by longitudinal division according to Schaudinn, and has a single nucleus according to Schaudinn, Herzheimer, Krzyztalowicz, Siedlecki and Forrest, and trypanosome phases according to Siedlecki.

Out of this heterogeneous collection it would seem to me that *S. obermeieri* might well be a spirillum while the rest that I have mentioned may be *Spirochæta.* Whatever they are, it is quite evident that we are here in territory which lies between the two divisions of schizomycetes and mastigophora and to tell whether a given *Spirochæta* is a plant or an animal is only an academic matter after all. What we really want to know is the life history of the individual and the form changes through which it passes. When these are ascertained the genera, families, orders and classes, or kingdoms, will take care of themselves.

The Morphological Diagnosis of Pathogenic Protozoa: James Ewing.

A review of protozoan morphology shows that all these organisms at some stages of development assume very characteristic forms which leave no doubt that they belong to a living structure. This is especially true of the recognized pathogenic protozoa of man and the higher animals. Hence some zoologists have taken the ground that when a protozoon is encountered under the microscope its characteristic forms can be recognized at a glance. Nevertheless, there has been a constant endeavor to force the acceptance, as protozoa, of various specific intracellular bodies found in cancer, rabies, the exanthemata, and certain related diseases of lower animals, although the structure of these bodies is not clearly protozoan. The danger of accepting these doubtful bodies as protozoa lies in the fact that not only in these diseases, but in others of known bacterial origin (diphtheria, glanders), there are peculiar intracellular degenerative products, more or less specific of each disease and organ, all of which closely resemble some forms of protozoa. Hence in the absence of entirely characteristic trophozoites, sporocysts, or spores, it is unsafe to regard any of the bodies as protozoa. The morphological study of the cell inclusions in cancer, variola, scarlatina, measles, rabies, clavellé, contagious epithelioma of birds, etc., has not led to a definite result in any of them.

In the absence of fully characteristic protozoan forms, the circumstantial or collateral evidence becomes of decisive importance. This collateral evidence in cancer is practically conclusive against the protozoan nature of the disease, and of the various cancer parasites. In the exanthemata it does not seem favorable to the protozoan nature of the bodies described as parasites, nor to the protozoan theory of the origin of these diseases. In rabies there are somewhat more definite indications pointing to the protozoan nature of the disease and of the Negri bodies, but this evidence is still inconclusive. The study of diseases of unknown etiology exclusively from the protozoon standpoint is to be deprecated. There are some grounds for supposing that the above diseases, except cancer, may be due to invisible but not necessarily sub-microscopic organisms or even to peculiar actions of well-known bacteria. The virus in the exanthemata, and probably in rabies, is perhaps connected with the specific cellular products of these diseases, but is itself of a nature not understood, and yet unparaleled among known protozoa.

Immunity against Trypanosomes: F. G. NOVY.

It is an established fact that rats which have recovered from an infection with *T. Lewisi* are immune to subsequent inoculation with that species of flagellate. The same holds true for cattle, sheep, goats, etc., that have recovered from the infection caused by the pathogenic trypanosomes, such as nagana, surra, dourine, etc. This condition of active immunity is seemingly possible only in those species of animals that are relatively insusceptible, for with really susceptible species the infection is always fatal.

Heretofore all experiments on artificial immunity against trypanosomes have been made on animals that have recovered from the effects of the parasite which has been living and multiplying in the blood-vessels of that animal. Now that cultures of some of these organisms, as for example *T. lewisi* of the rat and *T. brucei* of nagana, are possible it was desirable to ascertain whether or not they could be used to immunize against the virulent organisms. It may be said, in passing, that cultures of both of these trypanosomes, even after they have passed through a hundred generations or subcultures in the course of two years, do not become attenuated by such prolonged consecutive passage but readily infect susceptible animals.

We have shown, however, that cultures of *T. brucei* can be attenuated by exposure for about two days at 34° C. By repeated injections of cultures thus treated, attempts have been made to immunize rats and guinea pigs against *T. brucei*, but thus far these have been but partially successful. That is to say, there has been at most a survival of a few days of the treated as compared with the untreated animals. The failure to immunize with such cultures is attributable in part to the excessive susceptibility, of the animals employed, to infection with *T. brucei*, and in part to the existence of a negative phase following the injections. It is desirable to repeat these experiments with less susceptible animals.

In view of the fact that rats invariably recover, some soon, others late, from infection with *T. lewisi* and the further fact that rich cultures of this organism are readily obtainable, it is evident that this species is well adapted for studies on immunity. Up to the present time it has not been satisfactorily shown that trypanosomes elaborate toxins or that they confer immunity by means of soluble or intracellular products. The latter problem was approached by means of plasmolyzed cultures. To effect solution of the trypanosomal cells the cultures were taken up in distilled water and dialyzed in collodium sacs. Usually after one or two hours of such dialysis in distilled water the trypanosomes completely disappear and the intracellular matter apparently passes into solution.

By means of such plasmolyzed cultures it has been shown that rats which receive three or more injections, on alternate days, on subsequent inoculation with a minimal infective dose of fresh trypanosomal blood from a rat do not become infected, whereas controls are positive. With such solutions it is possible to hyperimmunize rats so that 0.5 c.c. of the immune rat blood protects against a simultaneous and separate injection of the infective blood.

Protection is seemingly obtained against *T. lewisi* by simultaneous and separate injection of the infective blood and plasmolyzed culture, followed twenty-four hours later by a second injection of the latter. Repeated injections of too large a quantity of the plasmolyzed culture and at too short an interval leads to a negative phase, the presence of which is indicated by the unusually early appearance of trypanosomes in the blood after inoculation with the virus.

Inasmuch as it may be said that the plasmolyzed material does not represent a true solution, a series of experiments were made with the filtered (Berkefeld) plasmolyzed liquid. While these experiments go to show that immunity can probably be induced by such filtered soluble products they are not as decisive as they should be and for that reason will have to be repeated. The chief reason for this uncertain result is the rather frequent failure of the control rats to develop infection. Although young rats (50–80 grams) were used to guard against previous infection with trypanosomes it is certain that a large percentage of the rats, as purchased on the market, have acquired an immunity against *T. lewisi.* That the immunity encountered is really acquired and not natural is shown by the fact that we have many times isolated *T. lewisi,* by means of the cultivation method, from rats which on repeated examination were found to be free from parasites and hence were supposed to be normal.

Program of the Second Session, December 28, 1906

(Joint meeting of Section K and the Society of American Bacteriologists.)

On the Biology of Diplococcus intracellularis: SIMON FLEXNER.

The Stability of Tetanus Toxin: M. J. ROSENAU.

Some Observations on the Blood of Horses: J. J. KINYOUN.

The Alleged Rôle of Intestinal Worms as Inoculating Agents in Typhoid Fever: C. W. STILES.

The Absorption of the Third Serum Component: W. H. MANWARING.

The So-called Physical Chemistry of Hemolytic Serum: W. H. MANWARING.

On the Chemical Inactivation and Regeneration of Complement: HIDEYO NOGUCHI.

On the Electric Charge carried by Toxins, Antitoxins and Agglutinins: C. W. FIELD.

An Improved Technic for Tuberculo-opsonic Preparations: A. P. OHLMACHER.

Some Suggestions concerning the Terminology of Opsonic Theory and Practise: A. P. OHLMACHER.

The Generic Characters of the Coccaceæ: C. E. A. WINSLOW and Miss A. F. ROGERS.

Actinomyces of the Oral Cavity: D. H. BERGEY.

Program of the Third Session, December 29, 1906

(Joint meeting of Section K and the American Physiological Society.)

The Functions of the Ear of the Dancing Mouse: R. M. YERKES.

The Effect of Section of One Vagus upon the Secondary Peristalsis of the Esophagus: S. J. MELTZER and JOHN AUER.

On the Alleged Adaptation of the Salivary Glands to Diet: F. P. UNDERHILL and L. B. MENDEL.

Adaptation of Saliva to Diet: C. H. NEILSON.

The Effect of Phosphorus Starvation on Aspergillus niger: WALDEMAR KOCH and H. S. REED.

New Chemical Facts about Tendon and Compound Proteins: WILLIAM J. GIES.

A Further Study of Peptolysis: W. N. BERG and WILLIAM J. GIES.

The Action of Blood Serum and Tissue Extracts on the Coagulation of the Blood: LEO LOEB.

Some Observations on the Esophagus after Bilateral Vagotomy: W. B. CANNON.

Concerning the Pharmacological Action of Salicylic Acid: L. B. STOOKEY and M. MORRIS.

A Nuclein Metabolism Experiment on a Dog with an Eck Fistula: P. A. LEVENE and J. E. SWEET.

Protein Analysis: P. A. LEVENE, W. A. BEATTY, D. R. MACLAURIN and C. H. RUILLER.

A Demonstration of Normal Gastric Peristalsis in the Rabbit: JOHN AUER.

Peristalsis of the Rabbit's Cecum (with demonstration): JOHN AUER and S. J. MELTZER.

Preservation of Blood Vessels in Cold Storage: ALEXIS CARREL.

Demonstration of Failure of Regeneration of the Cervical Ganglion Twenty-six Months after its Removal: S. J. MELTZER.

WILLIAM J. GIES,
Secretary

SCIENTIFIC BOOKS

THE TERRACES OF THE MARYLAND COASTAL PLAIN

Maryland Geological Survey. Pleiocene and Pleistocene. Baltimore, 1906. Pp. 291, plates and maps.