seem likely to afford important aid in working out the stratigraphy, and also to throw new light on the relationships between the faunæ of the old and new worlds in Carboniferous time. Many of the American species, though described and figured, still await publication. In the meanwhile, it is satisfactory to find that, in spite of the war, the insects of the European coal-measures are being fully described and well figured by Mr. Herbert Bolton, of the Bristol Museum. The apparent poverty of England in fossil insects of Paleozoic age may prove illusory, due merely to lack of interest in their discovery and identification. At all events, Mr. Bolton is making known a number of new types, his latest paper on this subject[2] containing accounts of new species, and a very fine new genus, for which he unfortunately uses the preoccupied name *Palaeomantis*. In another paper[3] Mr. Bolton gives an account of some insects from the coal measures of France, describing among other things the singular new genus *Megagnatha,* which appears to have long slender jaws, recalling those of the modern *Corydalis* male. The name *Megagnathus* has been applied to a genus of beetles, but it is to be hoped that no one will think it necessary to alter the name of Mr. Bolton's genus. Those who propose to change names in zoology, merely because they are thus similar, can hardly understand what confusion would result from the universal application of such a method. We all agree that absolute homonyms can not be permitted to stand, but a difference in the ending of the word is abundantly sufficient to prevent confusion. In connection with the evolution of cockroach types in America and Europe, particular attention must be called to Mr. Bolton's *Neomylacris lerichei,* which certainly has a strongly American facies. I should call it *Promylacris lerichei,* referring it to a genus described from Mazon Creek, Illinois.

2 "On some Insects from the British Coal Measures," *Quart. Jour. Geol. Society,* Vol. 72 (1916), pp. 43–62.

3 "The Mark Stirrup Collection of Fossil Insects from the Coal Measures of Commentary (Allier), Central France," *Mem. Manchester Lit. and Phil. Society,* May 11, 1917.

While the European Paleozoic insects are thus being elucidated, very welcome information reaches us from Australia, of the discovery of a rich fauna of Mesozoic insects.[4] It appears that the specimens represent a Triassic fauna, and consequently tend to complete our ideas of insect phylogeny, filling in a gap which has hitherto existed in the record. One of the Neuropteroid faunas is referred by Mr. Tillyard to a new order, Protomecoptera, having the surface of the wings finely reticulated. In modern *Panorpa* traces of the primitive reticulation can be seen in a good oblique light, and it really seems unnecessary to recognize more than a superfamily (Archipanorpoidea).

T. D. A. Cockerell
University of Colorado

SPECIAL ARTICLES

THE EVOLUTION OF BACTERIA

Dr. I. J. Kligler has recently contributed a paper on "The evolution and relationship of the great groups of bacteria."[1] The conclusions reached are so surprising and so in conflict with commonly held opinions that the contribution is well worthy of careful scrutiny to determine whether or not the premises are reasonable, and the deductions logical. It appears to the writer that in several instances Dr. Kligler has failed to prove his points and that his conclusions, at least when based upon the premises and the reasoning used, are open to serious question.

Probably no fault can be found in the introductory statement that the bacteria are "among the most primitive of living forms." It should be remarked, however, that this does not prove that the present living bacteria are any of them identical with, or even closely related to, the original types of bacteria which appeared upon earth. The next statement can not be accepted quite so readily. The author says:

4 R. J. Tillyard, "Mesozoic Insects of Queensland," *Proc. Linn. Soc. New South Wales,* July 11, 1917. Also publication 253, Queensland Geological Survey (1916).

1 *Journal of Bacteriology,* March, 1917.

The ability of some types to subsist on simple inorganic substances (CH_4, NH_3 and CO_2) without the aid of sunshine, and the sensitiveness of all bacteria to the action of sunlight suggest their existence on this planet prior to the appearance of plant life or the penetration of the rays of the sun through the volcanic vapors.

We may perhaps agree that an organism which can develop without organic compounds of any kind, utilizing inorganic compounds exclusively, would probably be primitive. There is no proof, however, that the original bacteria were any more sensitive to the sun's rays than are plant cells in general at the present day. It is not improbable that the purple sulphur forms are among the most primitive of our modern bacteria, and these, as is well demonstrated by the work of Molisch and others, grow only in the presence of light, and the motile forms show a very marked positive phototaxis. Most animal cells are equally sensitive to sunlight with most bacteria, but this does not argue that these animal cells are primitive. Finally, the assumption that life must have originated on earth while the earth was still bathed in volcanic vapors, and the earth's surface was dark, has little to substantiate it. It is quite as probable that we must account for the development of life upon earth on the basis of Chamberlain's accretion theory as on the theory of a once molten globe. It is not argued that either hypothesis of earth origin must be accepted, but Dr. Kligler's assumption is rather an insecure basis for the erection of his complex superstructure.

It is rather difficult to follow Dr. Kligler's reasoning through the succeeding paragraph. The thesis to be proved apparently is: "The intimate dependence of both plants and animals on bacteria and their activities tends to strengthen the conviction that these microorganisms must have preceded the others." The statement may well be accurate, but the examples adduced are not in all cases fortunate. For example: "Plants can not subsist without nitrates" is misleading, for many of the higher plants, fungi, yeasts, algæ, etc., utilize ammonia quite as well as nitrates, in some cases utilizing ammonia in preference. To state that in arid regions where plant life is absent bacteria are also absent is simply to affirm that both bacteria and higher plants require water for their growth, or are killed by an excess of salts in the soil rather than that the higher plants are dependent upon the bacteria. The author's examples of the necessity of bacteria for the growth of animals are likewise not convincing as proof.

Attention is next called to the fact that with the bacteria evolutionary changes may be physiological as well as morphological, and the point is made that these "adaptive modifications" should be traced.

The statement that "Bacteria need only minute amounts of nitrogenous food but require a relatively enormous quantity of energy-yielding (carbon) compounds" is doubtless true for many of the fermentative types, but there seems to be no more reason for this assertion for primitive bacteria than for higher plants, or the fungi. Then it is argued since most bacteria, at least the saprophytes, can utilize nitrogen either as NH_3, or NO_3 and show the most diverse ability to use various carbohydrates, then the first path of early evolution must have been increasing ability to use a variety of carbohydrates, and only later, when a higher scale of development is reached do we find increasing power to utilize complex nitrogenous compounds. No evidence is adduced that the primitive organisms were furnished with abundance of carbohydrates before they had opportunity to attack considerable quantities of protein or other complex nitrogenous substances. It should be emphasized that carbohydrates in the sense used and in quantity needed would probably be produced only by higher plants. But all plants contain protoplasm, and in their decay organisms have access to proteins as well as to carbohydrates. In spite of the array of supporting evidence, it seems that there is no adequate proof that utilization of carbohydrates is any more primitive a function than is proteolysis.

The point is made that there are four principal types of primitive oxidizing bacteria, those which oxidize C, N, S and Fe, then follows the statement:

It seems fairly certain from the important part played by carbon compounds in the vital activities of our common bacteria, especially as a source of energy, that the carbon oxidizers are the forerunners of the bacteria of to-day.

The author then concludes that the most primitive types must be those capable of oxidizing methane, followed by those which oxidize carbon monoxid, and, most astounding, those capable "of utilizing CO_2." A careful scrutiny of the text will lead to no conclusion but that "utilizing" is here used to indicate "oxidizing." This inference is strengthened by a second statement even more surprising.

Since the ammonia and nitrate oxidizers (or nitrifiers) also assimilate large amounts of carbon-dioxide (Jensen) they would seem to fall in line along with the organisms capable of *obtaining their energy from carbon dioxide*.[2]

By what mystic process an organism may secure energy from CO_2 is not explained. The assumption that carbon-oxidizing forms are most primitive is scarcely proved. In fact, study of the oligocarbophilous organisms, that is, forms which can utilize CO_2 in the building up of food or protoplasm indicates that the more primitive of the modern types may be among the nitrifying and the sulphur bacteria. An analysis of the conditions upon the earth in early times as postulated by the author and elaborated by Osborn would seem to indicate that they would be even more favorable to the development of ammonia or sulphur oxidizers than for methane oxidizers. It should be emphasized that all organisms of types not using organic carbon must have some source of energy which will enable the protoplasm to take up CO_2, replace the oxygen in part with hydrogen and build up complex organic compounds. The energy for this transformation might have come from the oxidation of ammonia or sulphur. Certainly ground waters laden with hydrogen sulphide must have reached the surface of the primitive earth much as they do to-day. Furthermore, the consistent development of the pigment bacteriopurpurin by most members of the modern large group of sulphur bacteria, together with the marked phototaxis of this group, might be interpreted as evidence that the sun's rays had some influence almost from the beginning in the explanation of energy source in CO_2 assimilation. In other words, early coordination of photosynthesis with chemosynthesis can not be ignored as a possibility.

It may be noted further that to discuss primitive bacteria as capable of utilizing formic acid, acetic acid and alcohol is somewhat anachronistic. These substances are the results of the growth or fermentative power of organisms which stand higher in the scale of evolution. Where could the primitive organisms find these complex compounds to work on?

The author next calls attention to the fact that certain organisms changed their habit of life from that of obligate aerobes, using atmospheric oxygen, to that of facultative anaerobes, "utilizing combined oxygen for intracellular combustion." The statement is made that the "prototrophic denitrifying bacteria are most probably the progenitors of this group." The statement requires some analysis. One would infer that prototrophic denitrifiers are common and well known. As a matter of fact, the denitrifying organisms are almost without exception anything but prototrophic. When one wishes to demonstrate denitrification in the laboratory it is customary to add a suitable organism to an aqueous solution of nitrate and organic carbon compounds. Under anaerobic conditions the oxygen is removed in whole or in part from the nitrogen, and used for oxidizing the carbon compounds present. The only exception to this rule known to the writer is the peculiar organism described by Beijerinck which will grow in the presence of free sulphur, nitrate and carbon dioxide in the absence of atmospheric oxygen, oxidizing the sulphur and using the energy thus gained apparently in the assimilation of CO_2. It would seem that such a form would be much more closely related to the *Thiobacteria* than to the other denitrifiers. To make his point, Dr. Kligler should cite some examples of "prototrophic denitrifiers."

[2] *Italics not in original.*

If descriptions of such organisms are nonexistent in the literature is it necessary to assume that they have not been discovered or that they once existed but have disappeared? Is it so evident that

> The line of descent from the prototrophic denitrifiers is entirely clear.

The statement is made that next in series following the prototrophic denitrifiers probably came the *aerogenes* type of organism, one indication of relationship being that it can live "in simple inorganic media and under certain conditions, even to fix atmospheric nitrogen." The present writer has been unable to find any evidence that the *aerogenes* group can ever grow in a medium devoid of organic matter. It is true that inorganic nitrogen compounds may be used, but this, as was above indicated, has no particular significance, as many bacteria, yeasts, molds, algæ and even flowering plants have the same power. Any close relationship of *aerogenes* to a prototrophic form certainly has not been proved. However, the author's claim for close relationship between *aerogenes* and *coli* and through the series to the typhoid, dysentery and possibly even to the hemorrhagic septicemia groups has much to commend it, and is a generally accepted hypothesis at present among bacteriologists.

It is possible that the relationship assumed to exist between *B. proteus* and the spore-bearing rods is a real one, but the structural modification incident to the development of the power of endospore production is so great that mere association of organisms in putrefactive processes and common property of producing proteolytic enzymes does not prove close affinity. It should be borne constantly in mind that proteolytic enzymes are commonly produced by cells. Every cell has protoplasm consisting largely of protein. Cells in general after death undergo autolysis. Every cell then contains an autolytic proteolytic enzyme. The step to the production of an extracellular proteolytic enzyme would therefore seem not to be a difficult one to make, and one which may have been made independently by different groups. It might be mentioned, however, that Kligler has ignored one point of morphology which would tend to show relationship better than proteolysis, this being the diffuse arrangement of the flagella in both groups. Apparently organisms showing peritrichous flagella constitute a group rather distinct from the forms with polar flagella.

The type of pigment produced, and the general cultural and physiological characters would seem to argue quite as close a relationship between the *Rhodococcus* and *Micrococcus* and the rods producing red or yellow pigment, as between the latter and the spore-bearers or *B. proteus*. Many more resemblances than differences will be found, for example, between *Rhodococcus roseus* and the organism usually known as *Bacillus prodigiosus*.

The effort to derive the gram positive streptococci from the aerogenes types seems rather strained. Differences are more marked than resemblances. The products of fermentation are very distinct in the two groups. This fact together with decided differences in morphology, relationship to oxygen and cultural characters, counterbalance the presence of capsules of similar composition, ability to ferment inulin (in fact a variable character in *aerogenes*) and localization in the same organs of the animal body. However, it should be granted that the author's emphasis on relationship between the streptococci and the aciduric bacilli is probably well placed.

Kligler's characterization of *Azotobacter* as a form not only fixing atmospheric nitrogen but oxidizing nitrogen to nitrates is apparently not well founded. It may be noted that there is an apparent tendency to consider *Azotobacter* prototrophic, an assumption which is quite unwarranted. The statement "the *Azotobacter* can assimilate free nitrogen more readily if glucose and a small amount of ammonia are supplied" is misleading. Apparently *Azotobacter* is wholly incapable of growing, certainly incapable of fixing atmospheric nitrogen, without an abundance of soluble carbohydrate food. By oxidation of this carbohydrate, energy is secured for the fixation of sufficient atmospheric nitrogen for the needs of the cell, but there is apparently no nitrogen changed into the form of nitrates.

Furthermore it should be emphasized that this organism has very high fermentative powers, producing large amounts of CO_2 and H_2O.

That the organisms of the nodules of the legumes are closely related to *Azotobacter* is not improbable, but that there is any close relationship between *Rhizobium* and the acid-fast bacteria and the *Actinomycetes* is not so clear. It is true that the latter contention has been supported by several writers, but the fact that *Rhizobium* produces nodules on the plant roots and the tubercle bacillus causes tubercles to develop in animal tissues is no more of an argument for their inter-relationship than to claim that the nematodes producing galls on plant roots are related to the tubercle bacillus or to the *Rhizobium* for the same reason. The differences between the motile (polar flagellate) gram negative, *Rhizobium,* fixing atmospheric nitrogen, and the acid-fast gram positive, non-motile tubercle bacillus incapable of fixing nitrogen are very marked and tend to outweigh the remote resemblance of the branched bacteroids to branched tubercle bacilli.

The relationship indicated between the tubercle bacillus and the *Actinomycetes* is not at all improbable, in fact, intermediate forms have been described.

The use by the author of the name *Sporothrix* for a group of bacteria is unfortunate, and will tend to confusion.

It is surprising to find the *Micrococci* and *Staphylococci* at one extreme and *Streptococci* at the other of the classificatory scheme that is worked out. The concept is unusual, to say the least, and is scarcely supported by adequate proof to be convincing.

It would seem that the family tree of the bacteria suggested by Dr. Kligler is based upon many misconceptions and misinterpretations and can scarcely be accepted without much more adequate proof. However, there is much in the article to provoke thought and discussion, and if this is accomplished and some conclusions eventually reached, the effort put forth can scarcely be said to have been in vain.

R. E. Buchanan

The Bacteriological Laboratories,
Iowa State College

THE AMERICAN MATHEMATICAL SOCIETY

The one hundred and ninety-sixth regular meeting of the society was held at Columbia University on Saturday, February 23, extending through the usual morning and afternoon sessions. Seventeen members were in attendance. Professor H. S. White occupied the chair. The following were elected to membership: Miss M. F. Chadburne, Smith College; Mr. Mervyn Davis, Equitable Life Insurance Company of Iowa; Mr. T. C. Fry, Western Electric Company; Dr. J. E. McAtee, University of Illinois; Dr. Norbert Wiener, Albany, N. Y. Four applications for membership were received.

The following papers were read at this meeting:

J. F. Ritt: "Proof of the multiplication formula for determinants by means of linear differential equations."

Olive C. Hazlett: "On vector covariants."

P. R. Rider: "On the problem of the calculus of variations in n dimensions."

A. R. Schweitzer: "On the iterative properties of an abstract group."

A. R. Schweitzer: "On certain articles on functional equations."

A. R. Schweitzer: "On iterative function equations."

J. R. Kline: "A new proof of a theorem due to Schoenflies."

R. L. Moore: "A sufficient condition that a system of arcs should constitute a surface."

J. L. Walsh: "On the location of the roots of the Jacobian of two binary forms, and of the derivative of a rational function."

O. E. Glenn: "Covariant expansion of a modular form."

J. F. Ritt: "Polynomials with a common iterate."

L. P. Eisenhart: "Transformations of applicable conjugate nets of curves or surfaces."

S. E. Slocum: "The romantic aspect of numbers."

The San Francisco Section of the society will hold its semi-annual meeting at Stanford University on April 6. The Chicago Section will meet at the University of Chicago on April 12–13; this meeting will include a symposium on divergent series and modern theories of summability. The regular New York meeting will be held at Columbia University on April 27.

F. N. Cole,
Secretary