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SOME PROBLEMS IN FERMENTATION.[1]

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THE present has several times been spoken of as the age of electricity and microbes and the description is certainly not inapt with respect to the first when one considers the numerous and rapidly increasing applications of the current in almost every sphere of human interest. We are accustomed to think of these applications of the current as mostly mechanical, but a little consideration will show that in the field of electrochemistry the developments are not the least remarkable. In the field of microbe action the advances have been scarcely less startling, though perhaps less readily recognized. From Galvani to Faraday, and from Faraday to Hertz or Kelvin, Bell or Marconi, is a wide range, but the progress from Leuwenhöck to Pasteur and from Pasteur to Koch or Hansen, Buchner or Warington is equally marked.

The conception of fermentation changes as due to the actions of micro-organisms, or enzymes produced by them, has been a gradual development and even to-day is not clearly defined. It is not yet possible to say whether certain reactions are of enzymic

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origin or are brought about as a direct life function of the cell itself. For my purpose, however, this distinction is of little importance as I wish to include in the list of fermentation reactions all those produced by micro-organisms either directly or indirectly. While the latest tendency, as illustrated, for example, by the monographs of Green and Oppenheimer, is to limit the use of the term fermentation to describe the action of the so-called enzymes, I shall employ it here in the older sense and include even the action of anaërobic bacteria, as is indeed still the custom with several writers of prominence.

The distinguishing feature of our discussions of fermentation phenomena to-day is perhaps the quantitative direction they have taken. In the older mycology the thing of greatest importance apparently was morphology and the relation of species. In a few cases only was it thought worth while to consider function and function quantitatively. With the gradual evolution of the science of mycology it has come to be recognized that the most interesting thing about these minute micro-organisms is, not what they are, but what they will do, and how much they will do. This brings the whole matter within the ken and interest of the chemist who now begins to recognize in bacteria most valuable agents of chemical change, new tools or instruments at his disposal.

It is true that a certain amount of this knowledge we have long possessed. The behavior and uses of certain yeasts and of the vinegar ferments have been known in a way for thousands of years, but even here our most important knowledge is of very recent origin, and in addition it is just beginning to dawn upon chemists that what the yeasts have done many other species may be made to do. That is to say, a multitude of these micro-organisms may be brought into the service of man and compelled to do work and to do it quantitatively. This leads us then to a consideration of some of the problems ahead in this field. There are, of course, many directions in which this ferment activity may be exercised, but I shall attempt to talk about a few of these only. The most important of the applications perhaps, and the one which is now attracting the most attention, I cannot touch upon at all. I mean the question of the production of toxins and anti-toxins and the relation of these bodies to modern medicine. The study of fermentation in this direction has attracted a host of workers and constitutes a domain apart and in itself. I wish to confine my remarks to a discussion of problems less attractive and less difficult perhaps, but which are, at the same time, somewhat more tangible for us as chemists.

It is interesting to recognize that the most inviting and important of these problems are concerned with the chemistry of nitrogen, with the fixation and transformations of the element which we think of as the most characteristic in connection with things living. From earliest times it has been known that certain organic wastes possess a great value in agriculture as fertilizers, but the mode of their action remained long without explanation. Indeed, the behavior of such substances was often capricious and beyond calculation, since sometimes the effect realized was not beneficial, as expected, but quite the reverse.

It gradually began to appear, therefore, that these organic substances, urea, hippuric acid and the nitrogenous compounds of feces being the most important, might not in themselves be valuable or useful as plant food, but that under certain conditions, not understood, something could be formed from them that really might be consumed by the growing plant. How this change was brought about was a great mystery. The solution of the mystery came slowly and was aided by observations from other directions. For a hundred years or more the production of niter in artificial beds or plantations was a common process. The demands for saltpeter for many purposes, especially for the pickling of meat and the manufacture of gunpowder, stimulated investigation and experiment greatly in this line of work, and many of the conditions favoring the nitrification of household waste and excreta, and stable drainage were empirically established. In the saltpeter plantations, as they existed especially in southern France, heaps of organic waste with spent lime or ashes, or certain kinds of calcareous soils were prepared and allowed to stand, and protected at first from light and air as far as possible. Later the air was admitted and the heaps, on saltpeter hills, were moistened from time to time with urine or stable drainage. This treatment was continued for a longer or shorter period and then the hills were allowed to stand at rest, sometimes two or three years before being leached.

A century ago most of the niter consumed in Europe came from India, where it was collected as an efflorescence on soils of thickly inhabited places. In the 1804 edition of that valuable old work, Thomson's chemistry, this statement occurs: "No phenomenon has excited the attention of chemical philosophers more than the continual reproduction of niter in certain places after it had been extracted from them. Prodigious quantities of this salt are necessary for the purposes of war; and as Nature has not laid up great magazines of it, as she has of some other salts, this annual reproduction is the only source from which it can be procured. It became therefore of the utmost consequence, if possible, to discover the means which Nature employed in forming it in order to enable us to imitate her processes by art, or at least to facilitate and accelerate them at pleasure. Numerous attempts accordingly have been made to explain and to imitate these processes." Following this passage Thomson discusses the theories then held about the formation of niter, which have no value for us at the present time. The practice, however, became important. In consequence of the almost complete blockade of continental ports during the years of the Napoleonic wars, the niter industry, like that of beet sugar, was developed and soon made the continent in a measure independent of India. These niter plantations continued to furnish much of the supply until the discovery of the great beds of soda niter of South America. Then they were gradually abandoned because of the lack of competing power. But the experience gained had its scientific value, which later, in the hands of Schlösing, Müntz, Warington and others, was of great help in building up our modern conceptions of the work of anaërobic and aërobic bacteria and the cause of nitrification. Of the theory of nitrification I do not intend to speak, as in substance it is well known to all here. But in practice we are yet far from hearing the last word. Two applications of importance in agriculture and in the arts have long had an interest for me. The old niter plantations were abandoned through commercial compulsion. Will they ever be revived? It is a well known fact that the supply of Chile saltpeter is not inexhaustible and that already the end of the output is in sight. It is not likely that the deposits found in the western part of this country will be sufficient for the world's needs through many years. In 1869 the output of sodium nitrate was 120,000 tons; in 1873, at the time of the Vienna exposition, when the development of the industry was supposed to have reached a maximum, the yield was 280,000 tons annually, but in 1883 it had grown to

580,000, in 1890 to over 1,000,000, and at the date of the last Paris exposition to over 1,200,000 tons. The life of the beds is no longer a question of hundreds, but, it may be, of tens of years. We are told, however, that the electric nitrification of the atmospheric nitrogen, as carried out at Niagara Falls and elsewhere, will supply the need when felt. This may be possible, but on the other hand, we may be brought back to the nitrification of animal waste again, now that the nature of nitrogen oxidation by bacteria is so well established. At the present day it appears far cheaper to throw away the nitrogen of excreta and kitchen refuse than to try to save it. Because of the almost universal water conveyance of sewage, with its consequent great dilution, we are told that its use in agriculture is rarely economical and its use for other purposes never. This is probably true as far as ordinary dilute sewage is concerned. But that sewage should be so dilute is merely a wasteful convenience, not a necessity. There are, besides, many localities in which the disposal of sewage in streams is impossible, or for sanitary reasons, undesirable, even after so-called purification. For such conditions chemical bacteriology should offer a remedy. The excreted nitrogen and waste from an urban population of 20,000,000 would, if oxidized, supply the world's need of nitrates. The dissolved nitrogen in the Chicago drainage canal, if saved and oxidized, would go far towards supplying the need of the United States for nitrates. The old saltpeter plantations were remunerative because of the relatively high price of niter. May it not be found possible to again make such undertakings remunerative through increased knowledge of the nature of nitrification and scientific skill in control? This is a problem of sufficient importance to attract the ability of the wisest. It is, of course, not likely that the niter bed, as it was formerly known, will ever be brought into use again. But I mean that some outgrowth of the old principle may be developed in which relatively pure cultures, properly managed, will take the place of the haphazard air or soil inoculation from which the results were at best uncertain and always far below the theoretical expectation. Probably we are not yet near the time when a successful solution of the problem could be hoped for. Indeed, it was not long ago that bacteriologists were quite ignorant of methods by which the nitrifying bacteria could be cultivated and, although to grow them on a large scale would still be considered a difficult matter, many of the essential conditions are now known, which is a long step in advance. The general explanations of denitrification with consequent loss of nitrogen, which long puzzled the agricultural bacteriologists, are now pretty well understood. This knowledge is fundamental and important in any solution of the problem in hand.

The second problem, like the first, has to do with organic nitrogen, but this time with a destructive loss, not a utilization. While I believe that it is true that a part of our nitrogen waste may be saved and utilized, it is also true that another and perhaps larger part can not be brought under this treatment. Under existing conditions an enormous amount of nitrogen as excreted matter, household and industrial waste finds its way into rivers and lakes to become by its bacteria-supporting power a menace to public health. The natural destruction of this is always accomplished sooner or later, but often too late for practical needs. How to hasten this destruction is certainly one of the greatest problems of modern times. All here are doubtless familiar with the attempts made by chemical means alone to destroy or render harmless this polluting material, the accumulation of all civilized communities. Oxidation with hypochlorites or permanganates, precipitation with lime, iron and alum salts, treatment with electricity and other disinfecting agents have all been tried and found to be practically weak when applied on the large scale and costly works constructed were abandoned while yet quite new. Meanwhile a mass of information was accumulating, referred to above, which showed that plants, not chemicals, must be the real agents to dispose of sewage, and not the large plants of the fields, but the minutest forms seen under the highest microscopic power. When this fact was recognized, it was found to depend on the further most remarkable fact that sewage always contains within itself many of the agents for its own alteration or oxidation. In speaking of the destruction of organic matter in water the term oxidation has been long used and with various meanings, but as now employed to describe the gradual disappearance of excretory and other products it includes a series of changes through which plant organisms, working together or in proper order, convert the most complex substances, like the proteins, into bodies as simple as ammonia and nitric acid. Such a series of reactions is somewhat analogous to the conversion of starch into acetic acid.

First we have here the action of enzymes, then inversion by an organism or an enzyme, then fermentation by a yeast or its enzyme, and finally a new fermentation by the organisms in mother of vinegar to yield acetic acid. These operations may be so conducted as to furnish a continuous yield of acid, as shown in the working of the malt vinegar factories, where in the final stages a weak alcohol flows into the top of a large vat containing shavings, covered with the slimy ferment and emerges below as the oxidation product or acid.

In the modern treatment of sewage we have the cesspool or septic tank, in which preliminary reactions like the familiar saccharification of starch are accomplished, and finally we have the large filtration tanks or beds, such as have been experimented with and developed in Massachusetts and later in England, from which an effluent of nitrates flows very much as in the vinegar factories the shavings-filled tank produces an effluent of weak acetic acid. In one case as in the other, minute plants are the agents of the change accomplished.

But it is not of this artificial or filter bed treatment of sewage that I am thinking. I have in mind the old question of the self-purification of sewage in streams. The problem is still new. Before the days of bacteriology this question was discussed in all its known phases by sanitarians. The extreme opinions of Pettenkofer on the one side were opposed by the extreme opinions of the elder Frankland and the Rivers Pollution Commission on the other. Pettenkofer's remarkable statements regarding the apparent purification of the Isar are still often quoted by the advocates of the rapid self-purification theory, while the dictum of the Rivers Pollution Commission, that no river in England is long enough to permit of complete self-purification, is still presented to us as weighty authority on the other side.

For nearly twenty years I have been much interested in this problem, my interest growing out of the conditions which practically obtain in the city of Chicago. The constant defilement of the lake by sewers led to attempts to turn the sewer flow in the opposite direction. The watershed separating the lake region from the Mississippi valley is very low and for a long distance is parallel to and but a few miles west of Lake Michigan. In 1848 a shallow canal was opened, connecting the lake with the Illinois river. This was known as the Illinois and Michigan

Canal, and the water in it was partly supplied by pumping from the Chicago river at the eastern terminus of the canal. In 1866 it was decided to widen and deepen this canal so as to secure a constant gravity flow. This work was completed in 1871, and the gravity flow from Lake Michigan through the Chicago river to the Illinois and Mississippi amounted to about 33,000 cubic feet per minute. But for some reason, mainly on account of fluctuations in the lake level, the flow was somewhat uncertain and the canal gradually accumulated much sediment which diminished appreciably its capacity. In 1884 pumps were put into operation again to maintain a constant flow of about 36,000 cubic feet per minute from Lake Michigan toward the southwest through the Chicago river, which received most of the city sewage. This canal was operated until January 17th, 1900, when the large Drainage Canal was opened. The old canal which was kept in service from 1884 to 1900 carried through its length of 33 miles a fairly concentrated sewage, as sewage is produced in this country. The flow was great enough to prevent appreciable sedimentation, from which it follows that unusual opportunity was offered to study the changes taking place spontaneously in the organic matter of the water. Such a study became almost imperative from the growing conviction that sooner or later the whole of the sewage would have to be diverted from Lake Michigan and sent down a large canal. The effect of this upon the water of the Illinois and possibly the Mississippi called for careful investigation. After several preliminary studies, I began such an investigation under the direction of the State Board of Health of Illinois, in 1886, and carried it through several months, including with the summer, part of the spring and autumn. The results obtained were most remarkable. In the flow of 29 miles, from Bridgeport, the pumping station, to Lockport, free ammonia was reduced from 26.56 parts per million to 12.73 parts; albuminoid ammonia from 1.63 to 0.75 parts per million, and oxygen consumption, by the Kubel process, from 26.2 to 11 parts per million. These figures indicate a loss of over 50 per cent. of the contaminating matters. In the following flow of four miles without dilution, from Lockport to Joliet, over one-third of the remaining contamination disappeared. In the winter following, 1886-87, further tests were made, showing a decrease of about 32 per cent. in free ammonia, 40 in albuminoid and 50 in oxygen consumption between the same points, Bridgeport and Lockport. These very important results, which were afterwards confirmed, were not accepted without criticism when published. One eminent writer referred to them as valuable if true, but stated that they made too great a claim on the faith of the reader. Still more remarkable results, however, but of a different character, were obtained in the same canal thirteen years later. In the meantime our notions of self-purification had undergone some change; the meaning of oxidation in water, with the effects of temperature, were more clearly understood, and therefore, when I began a long series of tests in the summer of 1899, which, like that of 1886, was a dry season, I expected to find in the canal results very similar to those of the earlier investigation, but I was disappointed; an apparent increase instead of a decrease in organic contamination was found on tabulating the data in the autumn. From Bridgeport to Lockport free ammonia increased apparently from 19.84 to 19.85, albuminoid ammonia from 3.22 to 3.37 and oxygen consumption from 26.5 to 27.90 parts per million. I mention these figures in detail because some important conclusions may be drawn from them. The flow of the water in the two seasons was practically the same. The initial contaminations were of the same general character and not greatly different in amount, but for some reason the bacterial scavengers were much more active in one season than in the other. Why this was we cannot now say, but the difference in fermentation activity was probably due to some slightly different conditions in the chemical character of the water which further investigations might have disclosed. A difference in acidity or in the amount of gas-house refuse present may have had something to do with the results. At present we are concerned with the fact only, not the cause. The experiments showed in 1886 that an enormous amount of the organic matter could be quickly destroyed in an open stream by bacterial agency. The interesting question is, can this fermentation activity in so large a volume of water be artificially controlled?

Assuming the conditions known, will it be possible to secure from sewer water like that of 1899 the results observed in 1886? The canal water joins with the water of several streams and produces finally the Illinois river. In 1886 the improvement begun in the canal was continued in the river, which, at Peoria, 157 miles from Chicago, furnished water, clear, bright and practically potable. The free ammonia had sunk to 0.027, the albuminoid ammonia to 0.194 and the oxygen consumption to 4.81. In 1899 the free ammonia at Peoria was over 30 times as large as in 1886, the albuminoid ammonia over three times and the oxygen consumption nearly twice as large.

Another very interesting illustration of what bacteria can do is furnished by the same Illinois river. Peoria is the home of enormous distilling industries and at times the amount of grain mashed has been as high as 25,000 to 40,000 bushels daily. The starch only of the grain is utilized in the fermentation; the waste, or distillery slop, making up about 8.5 pounds to the bushel, and largely nitrogenous, is partly saved by drying in some distilleries. In others it is fed to cattle. Before the introduction of the drying process, and when cattle were not being fed, the whole of this organic waste of 100 to 160 tons daily passed directly into the river where it created, a mile below the city, a contamination almost beyond description. When the slop was fed to cattle, the contamination from the daily flushing of the sheds, though apparently greater, was really less, since the nitrogen was now left largely as hippuric acid and more easily disposed of than when thrown into the water as undigested protein. The current in the river below Peoria in the years when I first observed these conditions was very slow; through much of the time a thick scum formed over the water and under this an enormously active fermentation went on day after day as regularly as in any septic tank I ever heard of. A few miles below Peoria the town of Pekin furnished more filth from similar distilleries and cattle sheds, but this, too, speedily went into fermentation to disappear in the same way. I say disappear, because this was certainly the fate of most of the organic refuse. The escape of gas bubbles from the surface of the water, at times very marked, gave some clue to the nature and extent of the processes going on. At Havanna, 36 miles below the Peoria distilleries, the river became clear again and in the winter furnished much ice for ordinary purposes. Sanitarians would naturally object to the use of such ice for household needs, but the information I secured did not show any definitely bad results, and this, remember, from a stream which received the drainage from many towns and cities above, and which at its source was made up of Chicago sewage. It is possible that a marked decrease in pathogenic bacteria followed the rapid destruction of organic matter in the Peoria-Pekin stretch which I have compared to a great open septic tank.

These two illustrations of the magnitude of bacterial action, the work in the canal from Chicago to Joliet, and the work in the Illinois river below Peoria, are sufficient for my purpose. They suggest the query which has come to me many times in the years in which this sewage problem has been growing in importance. To what extent may these fermentations be controlled by man without resort to artificial septic tanks, contact beds or filter beds? In the canal, in 1886, at least 40 tons of nitrogen daily was under process of change, and at least two-thirds of this was profoundly altered in its form of combination within a period of twenty-four to forty-eight hours. To have maintained the same rate of destruction through other years would have required a corresponding bacterial activity, depending on a greater number of organisms, or on different kinds of organisms. To accomplish such a modification in the bacterial work as this, seems perhaps beyond our powers. But is it actually? It was not many years ago that almost complete ignorance regarding the nature of alcoholic fermentation reigned, and it is practically within ten years that the enzymic character of the yeast ferment was demonstrated and the possibilities and advantages of pure cultures in the brewing industries shown. The experience gained here will not be without use in the fields of bacterial fermentation.

In addition to this advance in knowledge of the older fermentation processes we have in recent years a remarkable widening in the theory and practice of using bacterial organisms in an entirely different direction, which promises much for the future. In agriculture, after it was demonstrated that certain crops draw their supply of nitrogen from the air, it remained for a time a question as to how or through what agency this is done. The classic researches of Hellriegel and Wilfarth, Schloesing, Jr., and others, showed finally the important function of the root nodules with their bacterial contents in this work, and then it remained to apply this knowledge to practical agriculture. A few years ago, as you know, there was put on the market in Germany a bacterial fertilizer known as nitragin, which was essentially a pure culture of the nodule organisms supposed to be most active in aiding nitrogen assimilation. It is true that experiments with nitragin were not always successful. But that they were successful at all, that in many cases where nitragin was employed the crop yield was enormously increased, is sufficient to show the inherent possibilities in this large-scale use of bacteria. When apparently failure followed inoculation of the soil with nitragin, it is likely that all the necessary conditions were not well understood. In a long series of investigations carried out by the Department of Agriculture of this country, it appears that most of the practical difficulties in the way of producing active pure cultures of this kind at reasonable cost have indeed been overcome, and that results of the highest value to agriculture may now be expected.

With such experience before us I believe it is not too much to hope for that some day some one will undertake the scientific inoculation of streams as a means of hastening or completing the destruction of organic matter. Indeed, a suggestion essentially in this line was thrown out by Dupre sixteen or seventeen years ago, but it attracted no attention, as too little was then known of the conditions or extent of bacterial action. It may be urged that the difficulties in the way of such a scheme are insurmountable, that it will never be possible to properly control all the necessary conditions. I admit at once that the task will not be an easy one. If it were simple, there would be no problem in it and I am talking about problems, and difficult ones at that. I believe the importance of this problem is even greater than its difficulty and that it will be solved by a series of investigations in which chemistry and bacteriology will each take a part. Some of the existing conditions which might seem to point to certain failure for such an undertaking are known. The element of time, for example, is most important and the flow of a river, it will be argued, can not be retarded long enough to give the bacteria time to work. That is doubtless true of some rivers, but not of all, and besides we are coming to the point of admitting that the flow of many of the tributaries of our large rivers will have to be checked if we are to escape the disastrous floods of the last few years. The storage reservoirs, made primarily to hold up floods, may become purification reservoirs also. The question of temperature is also important and a difficult one. The higher the summer temperature, the more rapid the rate of natural oxidation, is a fact long observed by those interested in the question of self-purification. What will be done in the winter time is, I confess, a difficult problem, but that considerable purification may take place even in the coldest weather is true, as shown by the experiments referred to, made in the winter of 1886-87 and the spring of 1889. The recognition of the plant organisms which are the most active at a low temperature, is of the utmost importance in such a discussion, and how to cultivate and distribute them is a problem like that now being worked out by the Department of Agriculture with the nodule bacteria, and possibly of no greater difficulty as the conditions become more fully known. It may even be found that the inoculation of a river will prove a simpler matter than the inoculation of a farm.

The question of the fate of micro-organisms in water has long been a much debated one and to the sanitarian is of the highest importance. That certain pathogenic forms, the typhoid bacillus for example, are able to live for some time in water is unfortunately too true. The limits to their existence in time and the conditions of their destruction are yet, however, to be thoroughly investigated. It is a well-known fact that most pathogenic organisms are destroyed in certain sewage oxidation beds, and that their existence in presence of large numbers of saprophytes, such as abound in the rivers, is in a marked degree limited, is also true. The fate of the saprophytes and other nonpathogenic forms is similar; with destruction of their food supply through their own action, extinction follows. An interesting experiment bearing on this point originated in this city (St. Louis) in an attempt to determine whether or not sewage bacteria could reach St. Louis from the Chicago drainage canal. A few miles west from Chicago 107 barrels of a concentrated nearly pure culture of Bacillus prodigiosus, a species easily recognized, was thrown into the canal and some time later the organisms were sought for at the mouth of the Illinois river and in the Mississippi between that point and St. Louis. The search was practically fruitless, since after collecting and plating out over 5,000 samples, taken through a period of several weeks near the mouth of the river and above, but five prodigiosus or prodigiosus-like colonies were found, and of these, because of a previous inoculation, there is some doubt that they came from far up the river. According to the statements of the authors of the experiments, the concentration of the original extract was not less than 1,000,000,000 to the cubic centimeter, or about 1.6×10^{16} for the whole volume.

The total volume of the canal water flowing through four hours and into which these bacteria were discharged, was approximately 1.6×10^{12} cc., giving an initial concentration of 10,000 or over, to the cubic centimeter. Supposing this initially inoculated volume to pass down the Illinois river unchanged, except by the normal dilution, and the bacteria number to remain unaltered, we should expect to find at some period at the mouth of the Illinois a concentration of prodigiosus colonies measured by 2,500 to 5,000 in the cubic centimeter. Allowing now for probable dilution, because of the irregular flow, a concentration of 250 to 500 per cubic centimeter, at least, should have been found through a period of several days. But, as intimated above, an extended search through weeks showed only an occasional prodigiosus of doubtful origin. The necessary conclusion follows that the vast majority of the organisms were destroyed in the upper portion of the river and indeed near the point where they were thrown in, since some hundreds of tests made at Joliet and Peoria were absolutely negative. Such experiments are expensive, but of the highest importance in the study of problems like those suggested, and although this one failed of its object, proving rather the reverse of what was expected, it must still be looked upon as of great value scientifically, as indicating the very rapid destruction of micro-organisms in water under certain conditions. This particular organism, like the typhoid bacillus, is not a natural water bacterium and in the struggle for existence it must perish early. In the same river it has been found that the indol-producing bacteria disappear very rapidly as the complex protein derivatives on which they thrive are used up, and if our methods of chemical analysis were more refined, I believe it might be possible to connect certain micro forms with the presence of certain organic substances. Here there is a field for much investigation.

In another class of reactions also we have to do with ferment changes which, until comparatively recently, were supposed to be essentially chemical. I refer to the production of aroma or flavor in a large number of substances employed as food. It was not many years ago that all bacteria were considered harmful and of them it was said, as of the Indian, there are no good bacteria but dead ones. To know that any article was full of bacteria, or was essentially a product of bacterial action, was enough to condemn it; but that time is past. There are plenty of good bacteria, and even the much maligned bacillus coli communis has found a champion. Only two or three years ago a German physician, in all seriousness, proposed and used a pure culture of certain coli as a cure for constipation. The idea was not bad. The thing is "purely vegetable" and so far removed from the taint of ordinary materia medica that even a Christian Scientist might find favor in it.

Recent literature has made us all familiar with the part played by bacteria in the ripening of cream and of cheese, and a part of the success of the butter and cheese industries seems to depend largely on the proper control of bacterial fermentation. In the development of certain flavors the changes secured are chemical, but the agents at work are certainly the plant cells or their enzymes. Ordinary market milk, in good condition too, we are told, often contains from 3,000,000 to 100,000,000 of bacteria per cubic centimeter, and even this number must be reinforced by the addition or development of others to properly ripen cream. Pure cultures of certain organisms have been introduced within the last few years for the use of butter-makers. That some species are much more active than others in the development of butter flavors, has been recognized by butterine manufacturers who, by the addition of a small amount of highly ripened milk to the oleo product, have been able to produce a mixture of marked butter flavor.

Our knowledge in all these lines is as yet somewhat empirical, however; the active species have never been properly identified and the chemical nature of the valuable and characteristic products formed in small amount is not known. Even the conditions of the long known lactic acid fermentation, the reaction of which has been represented in our chemical books by very simple equations, are just beginning to receive the right kind of study from chemists trained also in bacteriology. Lactic acid can be formed in quantity, not merely by one, but possibly by hundreds of species of bacteria and the purity and the quality of the yield are influenced in many ways. Various alcohols and volatile acids and gases are usually produced at the same time and it is just here that the greatest technical possibilities are apparent. Investigation in this line is not important to the milk industries alone, but also in many other directions, and especially in the theory of bread-making. We read in our books of the value of yeast in making alcohol and in leavening bread, yet in most cases it is likely that the pure yeast cell is the least active of the agents in the leavening and flavoring of bread. Able bacteriologists have been concerning themselves with this problem in the last few years, and many observations of value have been made. For years back it has been my custom to ask of students from the country information as to the methods employed at home in the leavening of bread, and as I now recall the answers received, it is clear that in most cases the ordinary yeast fermentation could have had but little to do with the result. Even when some form of compressed yeast or dry yeast was used, it is likely that the final effect was secured through some kind of a symbiosus in which both yeast and bacteria were concerned. It is a curious and well-known fact that from the same flour breads of many flavors are obtained; the differences are doubtless due in a large measure to variations in the active bacterial flora present. What is true of bread is, to a less degree, perhaps, true of many articles of food; the aid of ferments is somewhere necessary to their perfect preparation. It is a sorry admission that most of our knowledge in this wide field is of the empiric order and the outcome of the daily labor of the most ignorant. To the scientific man the question of kitchen chemistry appears too trivial to claim his mature attention. But is it? Is not the problem here of sufficient difficulty and importance to make it worthy of the attention of the most highly educated? The problems in the preparation of foods, involving some of the most intricate questions in bacteriology and chemistry, merit certainly far more attention than has been given them. In these days of cooks' and bakers' strikes and the growing independence and unreliability of the housemaid, may we not be forced to the consideration of the food preparation problem from an entirely different standpoint? The physician has his field of usefulness in curing the ills which are often the result of errors in food preparation. Is there not room for another class of professional men whose more important duty it will be to teach and direct the preparation of food in such a way that it will attract and not make sick?

The question of the intelligent use of ferments is presented to us from still other directions, and in some fields, as in the production of beers and wines from pure cultures, most remarkable results have already been obtained. These developments are so well known that they call for no further mention. The action of ferments is important also in the curing of tobacco and in the singular modern methods of storing green fodders as illustrated in the silo pits. Citric and tartaric acids are already made to some extent by artificial fermentation processes, but the industries are new and the stages of difficulty not yet passed. I believe the fermentation of cellulose on the large scale will some day attract attention. The hydrolysis of cellulose, and subsequent change into products which are useless, however, has long been observed, but it remains to turn this fermentation into directions important from the standpoint of the food problem, or the production of alcohol. Consider the enormous amount of cellulose wasted to-day in the form of sawdust. While the new Classen alcohol process will undoubtedly save a part of this, a method of saving the large mass is still to be discovered.

In the development of these and similar ideas we recognize the importance of the new composite science of chemical mycology, which should aim to give form to the great mass of empirically established data, and from these as a groundwork to build up a system in which definite results may be expected and also accounted for. In working out these problems the chemist must learn to use his bacteria as he would new reagents in research, and later they will become the important agencies through which he may be able to work out, on a large scale, chemical changes with the same certainty that he expects in converting sugar into alcohol. The great problems of bacteriology involved in the utilization of waste, the preparation of foods and even the preparation of sera for the prevention of disease are, in the end, essentially chemical problems and must be solved by chemists trained especially for work in this new direction. For the future there is much in store, and the chemical study of the bacteria, which are among the smallest of organized things, will rival in interest and possibly outweigh in importance the study of the atom and corpuscle, the smallest of lifeless things.