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Fermentation:— A few Points of different Theories briefly discussed.

Although aware of the fact that this entire subject was still a matter of the liviest controversy, I indulged a hope that by taking a sort of general survey of the whole field which has been occupied by so many of the most eminent chemists and physiologists of the day, one might derive some advantage in the way of forming correct conclusions, even over those who have made extensive original experimentation, and have built theories upon the exclusive grounds within the limit of their own research.

In making a review of the various chains of evidence which have been presented in support of so many different theories, one cannot fail to become apprized of the fact, that altogether too exclusive study has been given to that particular form of fermentation generally known as the alcoholic or vinous modification, and until the subject has been studied in a more general sense, it is easy to perceive the difficulty with which any

one view can establish itself, or become anything like a settled conviction in the minds of scientific men.

It has been generally admitted that the phenomena attending the various kinds, or species of fermentation, should all be referred to one general origin, or exciting cause, and the very signal failure to demonstrate what this agency really is, so plainly and evidently as to bring it into universal acceptance as truth, is probably due to the fact just mentioned, that there has been by far too little study bestowed upon some of the varieties of fermentation, as compared with that given to the alcoholic variety.

Again, either as a result of the egotism of human nature, or of careless oversight on the part of those who have been first and foremost in the ranks of those making the most unrestricted investigations into this branch of research, experimentalists have omitted in great measure to link the results of their own observations with those of others, which combination of experimental testimony would probably hasten the general adoption of one or other of the conflicting theories, and that most closely allied to the correct one.

But this connection of results of experiments, all of which may have been conducted in a most

reliable manner, and may yet argue for little and prove nothing when taken alone, but indicate much when taken into consideration conjointly with each other, seems to be left for him to make, who comes to a contemplation of the subject with a mind wholly unaffected by any anxiety as to a gain or loss of public reputation, according as the one or other view becomes popular, and who sits down for a quiet resumé of the whole theme.

The object of this paper is chiefly to present a few items of fact and probability, in such a connection, and relation to each other, as to suggest some ideas which may have hitherto escaped general notice.

The demand for an explanation of the origin and maintenance of fermentation, which shall be general in its application to the various forms which this mysterious process assumes, is so evident, that one of any other nature can hardly be tolerated. That which has been advanced by Liebig appears, however imperfect it may be in some of its bearings, to have at least this one meritorious element of general applicability, and seems to be the only theory yet brought forward, which to any great degree

possesses it.

Although much evidence has been collected, and is still in progress of deduction from the researches of other eminent scientific men than Leibig and Pasteur, yet the larger share of the present conflict exists between the theories of these two individuals. The standpoints which these chemists take, are so familiar to all who are enough interested in the progress of the scientific world to read its current periodicals, that a statement of their views is hardly needed in order that one may refer to them with the utmost familiarity and always be understood; and yet for purposes of definiteness, it may be well, even here, to insert a brief statement of the substance of the theory urged by each of these two chemists, who appear to have taken the lead in this particular debate.

Leibig maintains that the various substances denominated as ferments are ever in a state of mutation and consequent molecular activity, and that when placed in contact with a substance of somewhat complex chemical composition, and, as a consequence, one whose elements are held together by a rather weak chemical affinity, all of which conditions are satisfied in most bodies capable

of undergoing fermentation, they can, by a transmission of the motion of the atoms in their own ingredients to those of the adjacent medium, whose atomic structure may be regarded as being in a state of rather unstable equilibrium, induce a similar activity and mutation, attended by the usual visible phenomena of fermentation.

Pasteur argues, that the chemical process of fermentation is essentially a phenomenon correlative to a process of vitality; that fermentation always commences and ends with this life process, and that it depends for its duration upon the organization, development, and multiplication of certain organic bodies constituting the ferment.

These two different conceptions seem to be but outgrowths of those formed by earlier investigators, among whom, ~~the names~~ a few may be briefly mentioned.

The names of Willis and Stahl should, perhaps, appear first on the list of those who have contributed evidence which has to any extent influenced the ideas of to-day, in this research;

and in the views entertained by them may be discerned the germs of Liebig's theory, and even something more than the mere germ. Indeed, it is markworthy that the purport of their doctrine should not have lost its foothold during the many years of discussion, much of which has been in direct opposition to it, but that it should have ever maintained its position among the influential propositions urged by the long line of experimenters, until at last it has found its development in Liebig's theory.

Mac Bride and Cavendish followed up the advancement of these first propositions with courses of experimentation, Cavendish appearing to be the first one to estimate the amount of Carbonic acid gas, liberated by the process of fermentation operating upon a known quantity of Sugar.

Later, Wiegleb and Lavoisier brought forward hypotheses somewhat opposed to those supported by the former theorists, chiefly differing therefrom concerning the question, whether the alcohol, which had been observed to be in a free state after fermentation of saccharine liquors, originally existed as a combined ingredient in the un-

fermented liquor, and by the process of fermentation was merely separated out from the same, or was created by that process. These two investigators were inclined to favor the former view.

Lavoisier also thought that by fermentation, the sugar was wholly decomposed into alcohol and carbonic acid. The imperfect analyses, however, by which he arrived at these results, furnish us with a very good illustration of the crudeness which was characteristic of the researches of like delicate nature in those times. The results of Lavoisier's analyses were afterwards, to a certain extent, corrected by Gay-Lussac, who, however, made only a few experiments of any importance, in connection with this research. Perhaps the most important experiment which he ever carried out in this relation, was one, demonstrating that access of air or of oxygen was necessary for the commencement of the fermenting process of grape-juice.

It was Fabbroni who first directed the attention of chemists to the substances them-

selves, which possess the power of exciting fermentation, when in contact with other bodies; and soon afterward, Thénard pointed out the fact, that in alcoholic fermentation, a substance always separates out, which possesses the same power of inducing fermentation when added to a fresh sugar solution, as that belonging to the well known beer-yeast.

After this period, we read of a long line of research, most all of which seems to lead in one direction, and to be, in fact, the very groundwork of Pasteur's recent, most elaborate course of experiment, and to form an intimate connection with his wide-spread, but not uniformly accepted theory.

Although microscopical investigations had, as early as about the year 1680, been applied to beer- and wine-ferment, and through this means it had been discovered that these substances consisted of little globules, yet the use of the microscope was not very generally applied to this research, and when such manipulation was resorted to, it was with very little effect until the

later inquests of this character by Cagniard de Latour, Kützing, Schwann, Turpin, Quevenne, Mitscherlich, and others, followed by a set of experiments by Schwann, of a nature not altogether microscopic, and familiar to nearly every one, the results of which were in great measure corroborated by Schultze, Wee, Helmholtz, Schröder and Dusch, although Schröder and Dusch found some cases which were apparently exceptional to the general line of evidence.

But alas for the practical inquiries into the reasons of things, this use of the microscope seems likely to run away with some of its devotees, so that one is apt to get very vague and mysterious answers, in response to a request for an explanation of the cause of various kinds of fermentation; as, for example, when demand is made for a demonstration of the original cause of alcoholic, or of acetous fermentation, one is obliged to content himself with the respective replies: "Torvula Cerevisiae", "Mycoderma Aceti". Whatever ideas such replies may convey in the abstract, they signify nothing at all

in the way of definite answers to the questions, but, on the contrary, give rise to a whole train of interrogations in sequence. Such answers partake nothing of the nature of an explanation.

The views entertained by Berzelius, while antagonistic to the ideas of Cagniard de Latour and of Schwann, do not coincide with those of Liebig. To him the process of fermentation was one to be explained on the ground of "Contact Theory" or "Catalysis".

There is great interest in connection with this view, and in some instances of fermentation there is much to uphold it; especially does it seem that acetous fermentation might be explained in this way, through the peculiar working of the "Mycoderma Aceti", and other substances, by which the alcohol may be first changed to aldehyde, and then to acetic acid. But further reference to this subject may be made presently.

The experimental investigations into this branch of research, made by the French chemist, Pasteur, are of a very searching, delicate nature, and the whole course is a very elaborate one.

His "memoir upon the organized corpuscles which exist in the atmosphere" is full of accounts of experiments of a nature well calculated to inspire confidence; but whatever influence this subject may have had upon the doctrines of fermentation, its effectiveness is at least rendered weaker by the very recent, rigorous course of experimentation, applied to the question of "spontaneous generation", undertaken by H. Charlton Bastian.

All of Pasteur's reports are made with a great deal of detail, and are of a very pleasing, winning nature, and it may be that it is partially due to this fact, that their sentiment has won so much of popularity. But his statements are not always consistent, and in some instances they so contradict each other, that even a mere amateur in scientific reasoning, would hardly fail to notice this weakness.

Some very sharp criticisms made by Liebig, and others, upon the discrepancy of different arguments urged by Pasteur, which had come to my notice, and for some of which I could scarcely believe there was warrantable ground,

caused me to look over some of Pasteur's research with more careful scrutiny, and, somewhat to my disappointment, I not only found enough to justify their complaints, but I could not fail to remark some conflicting statements, which, strangely enough, seem to have escaped attention. The almost numberless experiments which Pasteur has made are invaluable, and it is surprising that one who is competent to originate and carry on such brilliant manipulation, the merit of which must be evident to every one, should so lack the power of argument. While one feels the utmost confidence in relying upon the correctness of his experimentation in most cases, yet one feels that he is left to himself to build up a resultant theory, so unsatisfying are the connections of cause and effect which this eminently skilful experimentalist makes.

On the other hand, while it is always easy to follow a heightened discussion to which Leibig may give utterance, and which always appears consistent in all its parts, yet his statements of experiments are not nearly as satisfying as those

made by Pasteur, and do not inspire one with an equal degree of confidence. Indeed, in some of his references to the figures of other men's researches, his statements of, and mathematical deductions from the same are given out in a most neglectful manner.

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Beer-yeast or alcoholic ferment has been shown, by the microscope, to consist of globules, the general characteristics of which are familiar to everyone, and need not be further spoken of here, than to mention that the fluid contents of these cells, in which are suspended the little granules, are largely made up of a substance which has been very generally compared by all chemists to protein, and not unfrequently to casein, both of which substances always contain a certain amount of sulphur. The most trustworthy analyses always render an amount of sulphur as a constant ingredient of alcoholic ferment.

In one of Pasteur's researches, he dissolved pure sugar in distilled water, to which

he added a little tartrate of amonia, and also a very small quantity (not more than 0.080 grm.) of yeast ash, and an almost imponderable trace of yeast. Fermentation ensued, and the yeast increased very considerably in bulk and weight. As Sulphur is an essential constituent of yeast, and its presence consequently necessary for the physiological development of the yeast, it would appear, according to Pasteur's own theory that the yeast ought not to have increased because there was no sulphur in the original mixture.†

While Pasteur holds to the idea that fermentation is a process dependent for its origin and progression upon a life process of the ferment which nourishes itself upon the elements contained in the fermentable substance, he does not hesitate to bring forward certain statements, which, at least, have the appearance of being in open contradiction to this view; and in some cases, as in the one just cited, he gets an increase of growth and weight of the ferment, in a medium lacking the elements of the ferment itself, and, on

† Analyses of yeast-ash give no sulphur.

the other hand, in no way does he satisfactorily explain, how so much more of sugar is decomposed, in other cases, than the amount which is required to furnish all the material which can come from the sugar for the nourishment and development of the ferment formed. In one instance, where he has recorded the amount of sugar employed, all of which was decomposed by the process of fermentation, he gives the weight of the yeast after fermentation, and the percentage of cellulose contained in it, which, when calculated to sugar, gives a quantity more than a hundred times smaller than the actual amount of sugar decomposed. How can this surplus of sugar, decomposed into alcohol, carbonic acid, Succinic acid etc. be concerned in the life process of the yeast, or in any way dependent upon it?

Again does Pasteur appear inconsistent, where he endeavors to account for a slight difference in the increase of weight in the yeast, during fermentation in two different cases, the

one process taking place in a medium containing no albuminous substances, the other in a fluid which contained such matter; the fermentation which took place in the medium containing no albuminoïds, or other substance which would be considered as fit for the nourishment, and propagation of the ferment-cells, was attended by the greater increase of weight in the yeast. Now, this very fact would appear to be contrary to his theory, but how strange it seems, that his explanation should be, when judged in strict conformity to his theory, as contradictory to his experiment, as this latter is to his hypothesis. It is urged by him, that the slight difference in the two results is due to the probable fact that "the globules which are formed in a medium rich in nitrogenous food, have more activity and can consequently decompose more of sugar, than those which take birth in one, poor in mineral, or nitrogenous aliments."

This species of ferment partaking more decidedly of the plant, than of animal nature,

any "activity" can only be demonstrated by growth, and gradual increase in weight. If the decomposition of sugar in the process of fermentation, depends upon the development of the yeast-cells, the more sugar decomposed in any one case, by so much the more should these cells be propagated, and add to their weight. But it was in a medium rich in nitrogenous matter, and in which the globules have more activity, according to Pasteur, and can decompose more sugar, than those which take birth in one, poor in such food, where, in actual experiment, there was a smaller increase in the weight of the yeast than in the other case. It is easy to perceive the idea which Pasteur means to convey in his explanation, and one might, perhaps, read it over several times without noticing its inconsistency with his entire theory of fermentation. But one becomes conscious of the inefficiency of the elucidation, when a reference is made to a statement of his doctrine, and when it is remembered that according to it, fermentation is only taking place when "increase and development"

of the cells are taking place simultaneously, and that only according to the activity and rapidity with which this development takes place, will there be a greater or a less amount of sugar decomposed.

Liebig argues, and with, at least, apparent reason, that if fermentation is a process dependent for its origin and maintenance upon the nourishment, growth, and development of the ferment-cells, yeast ought not to excite fermentation in a pure sugar solution, which is wanting in the most important constituents of the yeast-cells, i.e. nitrogenous matter. This criticism which Liebig has made upon Pasteur's theory, has never been fully satisfied, although Pasteur has advanced some arguments in reply, which, however, do tend in a measure to demonstrate that in accordance with his theory, such a result, the possibility of which Liebig questions, might take place.

It will be remembered, that, in the instance previously cited, Pasteur observed a greater increase in weight of the ferment, in a pure sugar solution, than in one containing albuminoïds ~~with~~

with the sugar. It is likely that in some of these experiments there has been a wrong interpretation of their results. In one case, after fermentation had ensued, the yeast cells were weighed, and there was found to be an increase in weight, to the amount of about half a grammie. After filtering off the yeast-cells, the residue obtained by evaporating the fermented liquor to dryness, was treated with alcohol and ether to remove succinic acid and glycerine, and the insoluble residue (insoluble in alcohol and ether) is denominated by Pasteur, as the "Soluble part" of the yeast, or the "extractive matter". This nitrogenous substance, he says, has been supplied to the liquor by the ferment-cells during fermentation, and should be added to the weight of the globules of yeast remaining in the liquor after fermentation, in order to bring up the total weight of the ferment to the true amount.

It is probably true that some nitrogenous matter is always ceded to the fluid by the yeast during fermentation, but that all of this residue has been supplied by the globules, his own statistics render improbable.

In the experiment under consideration, this "soluble part" of the yeast was found, after fermentation, to amount to a quantity which was only about sixty-five mgrm. less than the original weight of yeast employed. This would indicate, of itself, that the greater part of this soluble matter cannot have been supplied by the yeast, for if it were derived from that source, the weight of the globules of yeast existing in the liquor, after fermentation, ought to be less than the weight of the yeast employed; but Pasteur states, to the contrary, that there was a greater weight. There is need of further proof, then, than this experiment furnishes, in order to demonstrate clearly that there is more development of yeast in pure sugar solution, than in one containing also nitrogenous matter; and taking this into consideration with his inconsistent explanation of the matter which has been previously discussed, one is led into a very distrustful state concerning the correctness of any such view.

Further, how improbable does it appear that such could be the case, if one considers

for a moment only, what takes place if the yeast globules, by a process of natural growth, develope in two media, the one consisting only of a solution of sugar, the other of sugar and albuminoïds. If in both cases development takes place, then in both cases must there be an assimilation of substances, derived from some source.

Laying aside the consideration of ingredients of the yeast, ~~xxxx~~ which seem to be of doubtful necessity for its growth, or rather, which are contained in it only in relatively small quantities, which it is perfectly fair for one to do for the sake of argument, if only both cases be treated alike in this respect, let us see what conclusions may be drawn.

Considering, then, only the main ingredients of the yeast, in the case of the solution of pure sugar, all the matter which can be assimilated from the medium for the development of the ferment-cells is cellulose. This may, indeed, assist in the formation of new cells, but the nitrogenous substances which constitute so large a

portion of the globules must be, and according to Pasteur are derived from the mother-cells for the further assistance in the creation of the new globules. But it is otherwise in the case of the medium containing albuminoïds besides the sugar, where, as Pasteur admits, the cells do not grow at the expense of the nitrogenized matters of the old cells, which cede these matters to the liquid in contact with them, constituting the so called soluble part of the yeast, but derive both cellulose and azotized matters from the "soil" in which the yeast is first sown. In the first case, then, we have of yeast, after fermentation is completed, only the original weight employed plus the weight of the cellulose derived from the sugar. In the second case, we have the original weight of yeast employed (considering the "soluble part") plus the weight of the cellulose derived from the sugar, plus the weight of azotized matter derived from the albuminoïds.

Payen and Quevenne both maintain, that after fermentation of pure sugar solution, the weight of the yeast will be found to be less than the weight of that employed, before the process of

decomposition had set in. It is likely that this is an error in the opposite direction, for they probably omitted to weigh any of the "soluble matter", some of which should, undoubtedly, be added to the weight of the remaining yeast-cells, but not all of it, as Pasteur erroneously considered.

So that while it appears possible for yeast to excite fermentation in solutions of pure sugar, it is not easy to understand how there can be an increase of weight in the yeast, after fermentation has subsided, in such a case, over that where albuminoïds are present.

Pasteur's explanation of the fermentation of the yeast-cells by themselves, which readily takes place at a proper temperature, and with other conditions of like nature favorable, is altogether inadmissible. But I will now call attention to a few considerations of a somewhat different nature.

Many conjectures have been formed as to the origin of the difference in kind, which exists between fermentations. Some argue that the variety of fermentation which shall ensue, depends upon the condition of the medium in which the

globules of yeast are sown, i.e. upon its acidity, neutrality, alkalinity etc.; others think that it depends upon the nature of the yeast globules themselves.

It is probable that it depends upon all of these conditions.

It has been found that yeast water, (water containing the soluble part of the yeast) when added to a solution of sugar, causes this latter to ferment. In such cases it not unfrequently happens that both alcoholic and lactous fermentations occur at once. Also the one or the other variety may be made to predominate, accordingly as water of fresh or of altered yeast is employed. When water of altered yeast is used, lactous fermentation is more apt to take place.

This fact, which was observed by Pasteur shows that there is a degree of reason in the view taken by Fremy and Boutron, that yeast is, at different stages of its decomposition, capable of acting as a different ferment, thus: at one stage it is better fitted to excite alcoholic fermentation; at another period it partakes more of the nature of lactous ferment.

In applying Liebig's hypothesis to lactic fermentation, this same conception has been urged by its originators as a sort of modification, and it has been to a great extent adopted by the adherents to Liebig's theory.

Another fact, taken into consideration with the observation made by Pasteur, adds to the probability of the correctness of some such view as that adopted by Fremy and Boutron. When lactous fermentation takes place simultaneously with alcoholic fermentation, it usually sets in long after the alcoholic variety, when, probably, the yeast has reached the same stage in its transformation as the altered yeast, the soluble part of which, Pasteur found so favorable to lactous fermentation.

That the character of the medium, in which the yeast is placed, exerts an important influence in deciding the variety of fermentation, is a well established fact. Even when the yeast may be in the proper state to excite alcoholic fermentation, so far as its own individual condition is concerned,

yet, if the medium be alkaline, this will be enough, in most cases, to give to the nature of the fermentation a new turn; perhaps by changing the nature of the yeast globules themselves.

Alcoholic fermentation takes place best in a medium which is neutral. Alkalinity generally arrests it, acidity retards it; and undoubtedly, the acidity always produced during alcoholic fermentation tends to protract the process. But there are practical difficulties in the way of keeping the medium neutral, as it should be kept theoretically, so as to favor alcoholic fermentation. First, neutrality tends to promote lactous fermentation. Pasteur adds a second difficulty. According to him, a neutral state of of the medium, under ordinary circumstances, is extremely disadvantageous to alcoholic fermentation, because it is favorable to the development of animalcula, which feed upon the soluble part of the alcoholic ferment, and so destroy its identity, and power of acting as such.

There can be no doubt but that a neutral medium is more favorable to the reception of infusoria than one which is only slightly acid. But

it is very singular that there should not exist in the air (from which, Pasteur thinks, these organisms are communicated to the fermenting liquor) or elsewhere, other beings, of a nature just enough different from those which live upon the alcoholic ferment, to enable them to find some comfort in globules of lactous ferment, and so destroy it also. Indeed, this idea is a pure assumption on Pasteur's part, and I have never been able to obtain any attempted proof of his, as to its veracity.

It seems much more probable that the difference in nature of the fermentations, is dependent upon chemico-physical conditions, and changes of the ferment, and fermenting medium, rather than upon any of a chemico-physiological nature.

It should be stated that Pasteur was the first one to obtain, in a free state, the lactous ferment, which, according to him, consists of very much smaller globules than those which characterize alcoholic ferment. Until this discovery of a ferment proper to lactous fermentation, and which identifies itself with it, every phenomenon which had

been brought to light, concerning this one variety of fermentation process, had strongly favored Liebig's hypothesis. Although this revelation of Pasteur's agrees with, and in a measure supports his own theory, it is not contradictory to that of Liebig, which may be applied equally well to this particular ferment as to alcoholic ferment, only, some of the very positive evidence which formerly existed against Pasteur's view, and favoring Liebig's, is altogether annulled.

There are some very strong reasons taken in connection with the foregoing, for regarding this so called lactous ferment as only a modification of alcoholic ferment.

That acetous fermentation should be caused by a physiological process seems, perhaps, more improbable than that such a process should be the origin of the varieties of fermentation hitherto considered. Yet, in this case also, Pasteur urges the application of his theory. Acetous fermentation is, according to Pasteur dependent upon the development of the fungoid, Myco-derma Aceti. But this substance has been found by Thompson

to excite alcoholic fermentation, when in contact with a solution of sugar, under certain circumstances, with the simultaneous creation of succinic acid.

Pasteur also maintains that phosphates and alkalies are essential constituents of the Mycoderma Aceti, but Mulder has found that after ignition, there is not a ponderable amount of ash left. So at the outset, we find there is great conflict between the statements of the identifying characteristics of this peculiar fungoïd.

Withers, the formation of acetic acid out of alcoholic liquors has been regarded in the light of an oxydation process, usually attended by the intermediate conversion of the alcohol into aldehyde. As yet, there is no well founded reason why this view should be superseded by any other.

We know of the oxidizing influence, which certain varieties of vegetable matter, in a state of decay, exert upon other substances with which they are put in contact. If a small quantity of some kinds of vegetable mould be introduced into a glass ves-

sel, and the vessel be filled with a mixture of hydrogen and oxygen, in the proportion of two volumes of the former gas to one of the latter, gradually the hydrogen becomes completely oxidized to water.

It is also well known, that ammonia, by a process of oxidation in presence of fixed, salifiable bases in various soils containing much humus, gives rise to nitric acid. This fact has been well demonstrated by Kühlmann, and, to establish a further analogy between this kind of oxidation process, and that applied to the conversion of alcoholic liquors into acetic acid through the intermediate state of aldehyde, it may be well to mention in this connection, that carefully regulated experiments performed by Vandin, demonstrate clearly, that this oxidation of the nitrogen of the amonia to nitric acid, is attended by a well defined, intermediate stage of the process, that of conversion into nitrous acid. Such a process takes place best in the shade, and, of course, free access of air is necessary.

Schönbein has proved that many substances, among which, mention may be made of Turpentine-oil, ether, and bitter-almond oil, after

having been brought thoroughly into contact with air, by being repeatedly shaken up with fresh portions of air, possess for a long time the power of oxidizing other substances with which they are placed in contact; for instance, sulphurous acid may, oftentimes, be converted into Sulphuric acid by this means. Finely divided platinum, under proper conditions, converts alcohol into aldehyde and acetic acid.

In all these cases, oxidation is brought about, not by direct taking on of the free oxygen of the air, or other mixture containing it in the free state, by the substance which becomes oxidized, but through the intervention, or mediation of some substance in contact with the oxidizable body, which possesses the power of absorbing and condensing within its pores, and upon its surface, the free oxygen. This power is apparently possessed by decaying wood, and a great variety of vegetable matters, and whenever the fungoïd, Mycoderma Aceti, takes any part in the conversion of alcohol into acetic acid, it is fully reasonable to suppose that it is through the power to condense

upon its surface, the oxygen of the atmosphere, and to give it up to the hydrogen of the alcohol, thus rendering its action precisely analogous to that of the other substances, which have just been considered.

In the process known as the quick method of making vinegar, where the dilute alcoholic liquor is made to trickle over beech-chips†, arranged in an open tube, or in a tun, through which a current of air is constantly circulated, there is no development of Mycoderma aceti visible, and bits of the wood which have been thus employed for years have been examined microscopically, and no such organism has been found. On the other hand, where fermented beer-wash is employed, containing nitrogenous and phosphatic matter, the development of the Mycoderma aceti has been found to actually retard the process, and is a great source of annoyance to the manufacturers, as it clogs up the passages and chinks, through which the air must circulate.

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† Sometimes pieces of charcoal are used.

Unlike the ferments which have been mentioned, this substance never works as ferment except in continous contact with air. This fact alone would be strong evidence in favor of casting it in the same category with the other substances mentioned, which possess the power of condensing the oxygen of the air upon their surfaces, so far as its influence in bringing about the oxidation of the alcohol is concerned.

Further, the analyses of the air after the transformation of the alcohol has been effected, in cases where this fungoid has been developed, have shown that the oxygen consumed was derived from the air.

The theory of Berzelius, which has been shared also by Mitscherlich, appears to be supported by this variety of fermentation, which seems to be so intimately connected with the so called "Catalytic Force", as well as in its application to a few other varieties, which, however, have been less studied.

In point of fact, Liebig's theory embraces that of Berzelius and Mitscherlich, but advances a step farther, and so covers more ground, thus becoming of wider application.

The "Catalytic Force" is truly a theory of contact, but a contact theory in its widest

sense may comprehend a consideration of other working forces than that one, which has come to be recognized by the technical name "Catalysis".

It has been remarked in the earlier part of this article, that the mere statement, that fermentation is a result of a physiological development of different species of organized bodies, is no explanation of the matter. but, aside from this consideration, the whole subject, concerning the characteristics of these organized bodies of plant and animal nature, is made up of conflicting statements, and is beset at every point with doubt and uncertainty. I have already cited one illustration of this point, with regard to the elements entering into the composition of the Mycoderma Aceti. The following serves the same purpose:

Pasteur attributes butyrous fermentation to the working of an infusorium, which, he states, is killed by access of air; but it has been shown by other eminent experimentalists, that contact with air is favorable to butyrous fermentation.

There are many processes, so closely allied, in outward characteristics to the more common kinds of fermentation, that, until they have been further studied should be regarded as true fermentations.

It is a fact familiar to chemists, that a solution of salacin becomes, through the action of beer-yeast, changed into saligenin and salicylic acid.

By a substitution of emulsin for the beer-yeast, the other conditions remaining the same, salicin is transformed into saligenin and glucose. In the one case the process of transformation is attended by a physiological development, in the other case, no such event ensues. But the two cases bear too much resemblance to each other to admit of any distinction to the effect that one is an instance of fermentation, and that the other is not. This same neutral principle of sweet and bitter almonds, also acts as a ferment upon the amygdalin of the latter, in presence of water, converting it into hydride of benzoyl, hydrocyanic acid, and glucose. The application of Pasteur's theory fails utterly in such instances as these.

In some fermentation processes, there is, indeed, a physiological development; and in such cases it appears to be, in some way, intimately connected with the process of fermentation. In what way, however, has never been explained by any one.

Of the question suggests itself at this point: what conclusion may be drawn from the foregoing, as to which of the existing theories is most probably the correct one?, a glance at the heading of this paper will be remindful of the fact, that any demonstration in answer to such query has not been its object.

the universal

A difficulty in the way of ~~Liebig's theory~~ ~~be-~~
acceptation of Liebig's theory,
~~coming~~ ~~universally accepted~~, is, that from the very
nature of the hypothesis, it hardly admits of any
experimental proof. All the evidence concerning it
must be built upon analogies. It is wholly differ-
ent in this respect with Pasteur's argument,
which involves questions of nourishment, and
the consequently essential elements in both ferment,
and fermenting medium, all of which considerations
admit of experimental demonstration.

The theory which can be shown to possess
the broadest range of applicability, must unques-
tionably be the one which shall become the pre-
vailing view in the scientific world.

In the present state of research upon
fermentation, Liebigs hypothesis seems to meet this
requirement more fully than any other; and it is
my belief that, as still farther progress shall be made
in our knowledge of the fermentation processes hitherto
but little studied, his theory, perhaps somewhat modified
by himself and others, will gain an ever firmer
foothold.

W. Fred. Merrill.

Aug. 30th 1870.