CHAPTER XII

A Plagiarism Frustrated

A marked contrast between Béchamp and Pasteur lay in the fact that the former demanded a logical sequence between his views, while the latter was content to put forward views that were seemingly contradictory one to another. For instance, according to him the body is nothing more than an inert mass, a mere chemical complex, which, while in a state of health, he maintained to be immune against the invasion of foreign organisms.[1] He seems never to have realised that this belief contradicts the germ-theory of disease originally put forward by Kircher and Raspail, which he and Davaine had been so quick in adopting. How can foreign organisms originate disease in a body when, according to Pasteur, they cannot find entry into the self-same body until *after* disease has set in? Anyone with a sense of humour would have noticed an amusing discrepancy in such a contention, but though Pasteur's admirers have acclaimed him as a wit, a sense of the ludicrous is seldom a strong point with anyone who takes himself as seriously as Pasteur did or as seriously as his followers take their admiration of him.

On the 29th June, 1863, he read a Memoir on the subject of putrefaction[2] before the Academy of Science.

In this he said:[3] "Let a piece of meat be wrapped up completely in a linen cloth soaked in alcohol" (here he copied Béchamp in an earlier experiment) "and placed in a closed receptacle (with or without air matters not) in order to obstruct the evaporation of the alcohol. There will be no putrefaction, neither in the interior, because no vibrios are there, nor on the outside, because the vapours of the alcohol prevent the development of germs on the surface; but I observed that the meat became *tainted* in a pronounced degree if small in quantity, and *gangrenous* if the meat were in considerable mass."

Pasteur's object was to show that there were no inherent living

[1] *"Le corps des animaux est fermé, dans les cas ordinaires, à l'introduction des germes des êtres inférieurs." Comptes Rendus de l'Académie des Sciences* 56, p. 1193.
[2] *ibid.*, pp. 1189-1194.
[3] *ibid.*, p. 1194.

elements in meat, that if external life, the germs of the air, were quite excluded there would be no bacterial development from inner organisms. These were the days in which, having enthusiastically adopted Béchamp's ideas of the important parts played by the atmospheric hosts, he denied equally vociferously any inherent living elements in animal and vegetable bodies.

Béchamp, knowing how his own skill with the microscope outstripped that of all his contemporaries, excused Pasteur for not having been able to detect the minute organisms in the depth of the fleshy substance. But he maintained that Pasteur's own acknowledgment of the tainted or gangrenous state of the meat should have been sufficient to have convinced him of the reality of a chemical change and its correlative necessity—a causative agent. Béchamp claimed that Pasteur's own experiments, while attempting to deny, on the contrary, proved the truth of the microzymian contentions.

For instance, again, in an experiment on boiled milk, Pasteur observed a smell resembling tallow and noted the separation of the fatty matter in the form of clots. If there were nothing living in the milk, how could he account for the change in its odour and explain the cause of the clotting?

Thus it is impossible to set aside the marked contrast between Béchamp and Pasteur in regard to their attention to any phenomenon, since by the former nothing was ever ignored, while the latter constantly passed over most contradictory evidence. In spite of, for example, all the marked changes in milk, Pasteur was content to describe it as unalterable, except through access of germs of the air, and nothing else than a solution of mineral salts, of milk-sugar and of casein in which were suspended particles of fat, in short, that it was a mere emulsion which did not contain any living bodies capable of causing any change in its composition. For years Béchamp studied milk, and it was not till a much later date that he finally satisfied himself as to all its scientific complexities.

We find that just as in 1857 Pasteur's sponteparist views were entirely opposed to Béchamp's, so through the 'sixties of the nineteenth century, Pasteur completely ignored Béchamp's teaching in regard to the microzymas, or microsomes, of the cells and the fermentative changes due to these inherent living elements. Having realised the germs of the air, he seemed blind to the germs of the body, and ignored Béchamp's prodigious work when the latter differentiated by experiment the varying degrees of heat

required to destroy the microzymas of milk, chalk, etc. Finally, it seems as though Pasteur must have been convinced against his will by Béchamp's conclusions in regard to the diseases of silk-worms, and his disparagement of the latter was no doubt provoked by his consciousness of a dangerous rivalry. At the end of 1868, laid low on a bed of sickness, who can tell what thoughts passed through his mind in regard to the views of the man who had so enlightened him on the subject of air-borne organisms and their part in fermentation; the man who had so incontestably proved the causes of the diseases of silk-worms that his own scientific reputation had been seriously threatened—the man, in short, who would never be his disciple?

Anyway, when Pasteur rose from his sick-bed, semi-paralysed, dragging one leg, the Prussian hordes for a time interrupted the even tenor of French life and national distress annihilated minor controversies. Who shall say if he thought these catastrophic events likely to have a lethal effect on the memories of his contemporaries? Be that as it may, in the year 1872 Pasteur suddenly sprang a surprise upon the Academy of Science.

For a moment we must recapitulate. It will be remembered that as early as 1862 Béchamp took up the study of vinous fermentation and the results of his experiments were published in 1864, when he stated clearly that from the outside of the grape comes the mould that causes must to ferment and that the stalks and leaves of vines bear organisms that may produce a fermentation injurious to the vintage. He showed here his extensive view of fermentative phenomena. Not only did he understand the part played by air-borne organisms and the rôle of indwelling cellular elements, but he was also able to point to organisms found on external surfaces. Subsequently, from the year 1869 to 1872, two other experimenters, Lechartier and Bellamy, bore out his views by demonstrating that the intracellular elements of fruits ferment and furnish alcohol when protected from air, the fermentation being in relation to the vegetative activity.

While this solid work was quietly progressing, Pasteur on his part was gaining great public attention. We have seen how at the start he was fortified with the Emperor's blessing, and he dedicated to Napoleon III the book for which he was given the grand prize medal of the exhibition of 1867. Indeed, to receive it he made a special pilgrimage to Paris, where, as his biographer naïvely suggests,[1] "his presence was not absolutely necessary."

[1] *Life of Pasteur*, by René Vallery-Radot, p. 141.

One would have imagined that after so much worldly success he would have been ready to give credit where credit was due in regard to views diametrically opposed to his incessant invocation of atmospheric germs in sole explanation of fermentative phenomena. But we fear that even his admirers must admit that to give place to others was scarcely a habit of Pasteur's; that is, not unless the others acknowledged him to be the sun, when he, in return, was ready to shed lustre on them as his satellites. Had Béchamp first bowed the knee to him, he might have been ready to accord a meed of praise to the Professor; but as the latter outstripped and criticised him the two were always at variance, even on points where their views might have been assimilated.

Pasteur, as we have already said, sprang a surprise upon the Academy in 1872, a year memorable for the incessant work undertaken by the School of Montpellier.

To take merely the end of the year, we find on the 7th October, 1872, an extract read before the Academy from a Note of Béchamp's, entitled "Upon the Action of Borax in the Phenomena of Fermentation."[1] This was of considerable interest at that time and answered certain questions raised by M. Dumas.

On the 21st October, 1872, Professor Béchamp and Professor Estor presented a joint Memoir, "On the Function of the Microzymas during Embryonic Development." [2] This was one of the many highly important treatises upon striking discoveries and the experiments that substantiated them.

On the 28th October, 1872, Béchamp read a Memoir entitled "Researches upon the Physiological Theory of Alcoholic Fermentation by Beer-Yeast."[3]

On the 11th November of the same year he read a Memoir on "Researches upon the Function and Transformation of Moulds."[4]

Some idea of his incessant toil may be gleaned from the mere titles of these records of his untiring energy. We can, therefore, picture his astonishment and natural chagrin when he was roused from his arduous researches by Pasteur's appropriation of views that he had put forward years previously.

First of all, on the 7th October, 1872, Pasteur described to the Academy "Some New Experiments Showing that the Yeast-Germ that Produces Wine Comes from Outside the Grape."[5]

[1] *Comptes Rendus de l'Académie des Sciences* 75, pp. 837-839.
[2] *C. R.* 75, pp. 962-966.
[3] *C. R.* 75, pp. 1036-1040.
[4] *C. R.* 75, p. 1199.
[5] *C. R.* 75, p. 781.

Here was Béchamp's discovery, published in 1854!

This was too much even for the subservient Members of the Academy! M. Fremy interrupted, with the object of exposing the insufficiency of Pasteur's conclusions.

On the invitation of M. Dumas, Pasteur renewed his Address to the Academy, under the title of "New Facts to Assist to a Knowledge of the Theory of Fermentations, properly so-called."[1]

Here Pasteur made the statement in which he claimed "to separate the chemical phenomena of fermentations from a crowd of others and particularly from the acts of ordinary life," in which, of course, nutrition and digestion must be paramount. Here we clearly see that as late as 1872, while theorising upon fermentation, he had no real conception of the process, no clear understanding of it as a function of nourishment and elimination on the part of living organisms. How little foundation is shown for the statement made later by his disciple, M. Roux: "The medical work of Pasteur commences with the study of fermentation."

Proceeding with his address, Pasteur claimed to have shown that fermentation is a necessary consequence of the manifestation of life when that life is accomplished outside of direct combustion due to free oxygen. Then he continued: "One perceives as a consequence of this theory that every being, every organ, every cell that lives or continues its life without the help of the oxygen of the air, or uses it in an insufficient degree for the whole of the phenomena of its proper nutrition, must possess the character of a ferment for the matter that serves as a source of heat, wholly or in part. This matter seems necessarily to contain carbon and oxygen, since, as I have shown, it serves as food to the ferment. . . . I now bring to this new theory, which I have already several times proposed, though timidly, since the year 1861, the support of new facts which I hope will this time compel conviction." After a description of experiments mere copies of those undertaken by others, he wound up triumphantly: "I already foresee by the results of my efforts that a new path will be opened to physiology and medical pathology."

The only timidity apparent is the wariness with which Pasteur put forward a conviction that "every being, every organ, every cell must possess the character of a ferment." Such teaching was entirely opposed to the theories he had formulated since 1861, and really seems to have been nothing less than a cautious

[1] *Comptes Rendus* 75, p. 784.

attempt to plagiarise Béchamp's microzymian doctrine. As we have seen, Béchamp, though maintaining that the grape, like other living things, contains within itself minute organisms, microzymas, capable of producing fermentation, yet ascribed that particular fermentation known as vinous to a more powerful force than these, namely, organisms found on the surface of the grape, possibly air-borne. Therefore, if Pasteur were accused of plagiarising Béchamp's microzymian ideas, he had only to deny the accusation by pointing out that the provocative cause of vinous fermentation came from outside the grape; though here again he was only following Béchamp. The Reports of the Academy of Science show us how well the clever diplomatist made use of these safeguards.

M. Fremy was quick to return to the contest. In a Note upon the Generation of Ferments,[1] he said: "I find in this Communication of M. Pasteur a fact that seems to me a striking confirmation of the theory that I maintain and which entirely overturns that of my learned *confrère*. M. Pasteur, wishing to show that certain organisms, such as the alcoholic ferment, can develop and live without oxygen, asserts that the grape, placed in pure carbonic acid, can after a certain time ferment and produce alcohol and carbonic acid. How can this observation agree with the theory of M. Pasteur according to which ferments are produced only by germs existing in the air? Is it not clear that if a fruit ferments in carbonic acid, consequently under conditions in whic[h] it can receive nothing from the air, it must be that the ferments are produced directly under the influence of the organisation within the interior of the cells themselves and that their generation is not due to germs that exist in the air? More than ever, then, I reject this theory of M. Pasteur that derives all fermentations from germs of ferments, which, though never demonstrated, are yet said by him to exist in the air; and I maintain that the phenomena due to atmospheric spores must not be confused with those produced by the actual ferments begotten by the organisation."

M. Pasteur replied: "M. Fremy seems not to have understood me. I have carefully studied the interior of fruit used in experiments, and I assert that there were not developed either cells of yeast or *any organised ferment whatever.*"

The argument between the two continued and grew heated; till Pasteur, losing his temper, accused M. Fremy of making

[1] *Comptes Rendus* 75, p. 790.

himself the champion of German science; though at the same time he expressed regret at overstepping the bounds of courtesy.

After some more argument M. Fremy accepted Pasteur's apology; though he hoped he would not repeat such an offensive observation as that about the Germans, for then, as again afterwards at the time of the World Wars, there was naturally such a prejudice against everything Teutonic that not even German science could be excepted.

M. Fremy then went on further to criticise Pasteur's contentions:[1] "Our *confrère* imagines that he will issue victorious from the discussion that I sustain against him, if the exactness of the facts that he presents be not contested. M. Pasteur deceives himself strangely as to the actual basis of the discussion. It relates not only to the determination of certain experimental facts, but also to their interpretation."

Pasteur, tentatively trying to put forward Béchamp's microzymian views, was now faced by M. Fremy with his actual theories of the past decade. M. Fremy tried to entangle him in them and at the same time expose the shallowness of the theory of air-borne germs as the explanation of *all* vital phenomena. To defend it, Pasteur was obliged, as M. Fremy pointed out, to account for each kind of fermentation as the work of a special organism. Then again, if fermentations were only produced by atmospheric germs, they could not take place when air has been purified by rain, or on mountain heights, which Pasteur himself had described as free from such organisms. And yet it was indisputable that fermentations are produced everywhere, even after rain and upon the highest mouuntains.

"If the air," said M. Fremy, "contained, as asserted by M. Pasteur, all the germs of ferments, a sweetened liquid capable of developing ferments should ferment and present all the successive changes experienced by milk or barley-meal—a thing that never happens."

M. Fremy persisted that it was established that organised bodies, like moulds, elaborate ferments; and that though Pasteur had always declared fermentation to result from the action of atmospheric corpuscles, he, M. Fremy, had long since demonstrated that when the seeds of barley are left in sweetened water a fermentation is produced in the interior—an intracellular fermentation, carbon dioxide being eliminated from the cells. Fremy claimed that this intracellular fermentation gave the final blow

[1] *Comptes Rendus* 75, pp. 1059, 1060.

to Pasteur's theory, and he derided Pasteur for declaring the production of alcohol within the cells not to be fermentation because of the absence in the fruit juices of specific beer-yeast. He pointed out that actual ferments are secreted inside organisms, instancing pepsin, secreted by the digestive apparatus, and diastase, produced during the germination of barley. He showed that in these cases the ferments themselves are not visible, but only the organs that secrete them; and that though known ferments, such as yeast, are not found in intracellular fermentations, that is no proof that fermentation does not occur.

He contended that "a fermentation is defined not by the ferment that causes it, but by the products that characterise it. I give the name of alcoholic fermentation to every organic modification that in decomposing sugar produces chiefly carbon dioxide and alcohol. The lactic fermentation is characterised by the transformation of sugar or dextrin into lactic acid. The diastasic ferment is that which changes starch first into dextrin and then into glucose. It is thus that, in my idea, fermentation must be defined. If, as desired by M. Pasteur, one rests the definition of ferments upon the description of the forms that the ferments may take, serious errors are likely to arise."

Finally he wound up: "In conclusion, I wish to refute a sort of accusation often reproduced in the communications of M. Pasteur. Our *confrère* accuses me of being almost alone in maintaining the opinions I have above developed. I do not know that M. Pasteur is justified in saying that all *savants* share his opinions upon the generation and mode of action of ferments. I know a certain number of *savants* of full competence in these matters, Members of the Academy and others, who do not agree with M. Pasteur."

In the course of the controversy M. Fremy distinctly showed that he did not rest his opposition to M. Pasteur on the accuracy or inaccuracy of his experiments, but upon the conclusions drawn from them, which he considered to be incorrect. Pasteur artfully refused to consider the subject from this point of view, and called for a Commission of Members of the Academy to judge of the accuracy of his experiments without regard to his interpretation of results! M. Fremy pointed out that to do this would be to beg the real question at issue,[1] and the matter ended in the two men continuing to slap at each other, Pasteur trying to make capital

[1] *Comptes Rendus* 75, pp. 1063-1065.

out of the fact that Fremy saw no use in the suggested Commission.

Pasteur also fell foul of the botanist, M. Trecul, in regard to a Note that had not been read aloud at the Session of the Academy on the 11th November.[1] At the Session held on the 18th November, Trecul expressed regret that Pasteur had seen fit to add this Note, which is of considerable importance, being tantamount to a complete confession that about four months previously he began to have doubts in regard to the transformation of the cells of the organism he called *mycodermi vini* into yeast cells, and now was prepared to deny M. Trecul's belief in a transformation of cells.

He condescendingly warned him: "Let M. Trecul appreciate the difficulty of rigorous conclusions in these delicate studies."

To which M. Trecul retorted:[2] "There is no need to caution me as to the causes of error that may present themselves in the course of such experiments. I pointed them out in 1868 and in 1871 in four different Communications and have since written lengthily upon them." He added: "M. Pasteur said in the Communication of the 7th October and in his reply to M. Fremy of the 28th of the same month, first, that the cells of grapes and of other fruits placed in carbonic acid immediately form alcohol; second, that there is no appearance of yeast in their interior; third, that it is only in rare and exceptional cases that cells of yeast can penetrate from the outside to the inside."

M. Trecul found these statements confusing in view of another made by Pasteur:[3] "In the gooseberry, fruit of quite another nature to grapes and apples, it often happened to me to observe the presence of the small yeast of acid fruits."

"How," said M. Trecul, "can this penetration of the beer-yeast take place into the interior of fruits that have intact surfaces?"

It is not altogether surprising that such contrary statements on this and other subjects should have driven Trecul to complain of Pasteur's mode of argument,[4] which he said consisted of contradicting himself, altering the sense of words, and then accusing his opponent of the alteration. Trecul himself experienced "many examples of the contradictions of our *confrère,* who has nearly always two opposite opinions on every question, which he invokes according to circumstances."[5]

[1] *Comptes Rendus* 75, p. 1168.
[2] *C.R.* 75, p. 1219.
[3] *C. R.* 75, p. 983.
[4] *C. R.* 88, p. 249.
[5] *Le Transformism Médical, par M. Grasset,* p. 136.

But while many realised that Pasteur could not support his new without giving the lie to his old theories, none could understand as clearly as the workers of Montpellier his tentative effort to capture Béchamp's teaching and put it forward, dressed in new words, as his own scientific offspring. This was too much for the Professor's patience, and on the 18th November, 1872, we find a Note presented by him to the Academy on[1] "Observations Relating to some Communications recently made by M. Pasteur and especially upon the Subject 'The Yeast that Makes the Wine Comes from the Exterior of the Grape.' "

In this Memoir Béchamp referred to his early experiments on vinous fermentation which had been published in 1864. He added: "M. Pasteur has discovered what was already known; he has simply confirmed my work; in 1872 he has reached the conclusion arrived at by me eight years before, namely, that the ferment that causes the must to ferment is a mould that comes from the outside of the grape; I went further: in 1864 I established that the stalks of the grape and the leaves of the vine bear ferments capable of causing both sugar and must to ferment, and further, that the ferments borne on the leaves and stalks are sometimes of a kind to injure the vintage."

Béchamp now also took the opportunity of bringing before the Academy the conclusions of a note presented by him previously on the 15th February, 1872. This had been omitted, ostensibly on account of its length, but the need for its publication was now apparent, and its previous omission illustrates in a small degree the annoyance to which he was continually subjected. But it was not until the Session of the Academy on the 2nd December, 1872, that the Professor dealt with the deeper significance of Pasteur's newly expressed views. In his Memoir entitled[2] "Second Observation on some Recent Communications by M. Pasteur, notably on the Theory of Alcoholic Fermentation," Béchamp commenced with a restrained and dignified protest:

"Under the title 'New Facts to Forward the Knowledge of the Theory of Fermentations, Properly So-called,' M. Pasteur has published a Note, the perusal of which has interested me all the more in that I have found many ideas in it that have been familiar to me for a long time. My deep respect for the Academy and consideration for my own dignity impose upon me the obligation of presenting some observations on this communication, otherwise

[1] *Comptes Rendus* 75, pp. 1284-1287.
[2] *Comptes Rendus* 75, p. 1519.

people who are not in touch with the question might believe that I had imposed on the public by attributing to myself facts and ideas that are not mine."

He went on to show by dates and by quotations from numerous works that he had been the first to establish two essential points: First—That organised and living ferments could be generated in media deprived of albuminoid matter; Second—That the phenomena of fermentation by organised or "figured" ferments are essentially acts of nutrition.

One single fact surely deals the death-stroke to the claim that Pasteur initiated a true understanding of fermentation, and that is that in his earlier experiments—those of 1857, for instance, and again in 1860—he employed proteid matters and thus showed that he had missed the whole point of Béchamp's great discovery that organised living ferments could arise in media totally devoid of anything albuminoid. The life at large in the atmosphere could only be demonstrated by its invasion of a purely chemical medium entirely free from the suspicion of any organised living elements. This solitary fact gives evidence that Pasteur did not then understand the real significance of Béchamp's demonstration.

The latter now went on to describe the physiological theory of fermentation as proved by his past experiments: "For me alcoholic and other fermentations by organised ferments are not fermentations in the proper sense of the term; they are acts of nutrition, that is to say, of digestion, of assimilation and of excretion.

"Yeast transforms first of all, outside of itself, cane-sugar into glucose by means of a substance that it contains fully formed in its organism and which I have named zymase: it then absorbs this glucose and nourishes itself on it: it assimilates, multiplies, increases and excretes. It assimilates, that is to say, a portion of the modified fermentible matter becomes momentarily or definitely a part of its being and serves towards its growth and its life. It excretes, that is to say, it expels the parts used by its tissues under the form of compounds that are the products of fermentation.

"M. Pasteur objected that acetic acid, the constant formation of which I had demonstrated in alcoholic fermentation, had its source not in the sugar, but in the yeast. To this question on the origin of the products of fermentation, which so greatly occupied M. Pasteur and his disciples, I made answer: They ought, according to the theory, to come from the yeast in the same way that

urea comes from us, that is to say, from the materials that at first composed our organism. In the same way that the sugar which M. Claude Bernard saw being formed in the liver comes from the liver and not directly from food, so alcohol comes from yeast. This is what I call the physiological theory of fermentation. Since 1864 all my efforts have been directed to the development of this theory: I developed it at a Conference held at Montpellier and at another held at Lyons. The more I insisted on it the more it was attacked. Attacked by whom? We shall see."

Béchamp then went on to show that it had been M. Pasteur and his pupil M. Duclaux who had been the chief opponents of this teaching. He quoted M. Duclaux as having said: "M. Béchamp has not observed that there might be two quite distinct sources from which they (the volatile acids of fermentation) might proceed, namely, the sugar and the yeast." He also again quoted M. Duclaux's extraordinary misconception of digestion as exposed by his statement: "When one sees in an alcoholic fermentation a given weight of sugar transformed into alcohol by a weight of yeast a hundred or a thousand times smaller it is very difficult to believe that this sugar ever made part of the material of the yeast and that it (the alcohol) is something like a product of excretion."

This misconception Béchamp showed to be now echoed by M. Pasteur in the Memoir under discussion, in which the latter stated: "That which separates the chemical phenomena of fermentation from a crowd of others, and particularly from the acts of ordinary life, is the fact of the decomposition of a weight of fermentative matter greater than the weight of the ferment in action."

The Professor repeated the explanation he had given in 1867 in answer to such crude objections. He had then shown that they could only have been made by those ignorant of physiological processes and had put forward the simile of a centenarian, weighing 60 kilogrammes, who, in addition to other food, could have consumed something like the equivalent of 20,000 kilogrammes of urea. "Thus," Béchamp concluded, "it is impossible to admit that M. Pasteur has founded the physiological theory of fermentation regarded as a phenomenon of nutrition. That *savant* and his disciples have taken the opposite view. I ask the Academy to permit me to record this conversion of M. Pasteur."

So far, Professor Béchamp had ignored Pasteur's final attempt at plagiarism; but now, at the same Session of the Academy, on

the 2nd December, together with Professor Estor, he presented a joint Note entitled "Observations upon the Communication made by M. Pasteur the 7th October, 1872."[1]

Nothing can surpass the dignity with which the two great workers dealt with the subject.

"M. Pasteur," they said, "at the Academy on the 7th October last, announced new experiments on the rôle of cells in general, considered as agents of fermentation in certain circumstances. The principal conclusions of his Communication are as follows:

"1. All beings are ferments in certain conditions of their life, for there are none in which the action of free oxygen may not be momentarily suspended.

"2. The cell does not die at the same time as the being or organ of which it forms a part.

"3. M. Pasteur foresees, from results already obtained, that a new path is opened to medical physiology and pathology."

Béchamp and Estor showed that, for a long time past, it was they who had taught that every being, or rather every organ in such a being and every collection of cells in such an organ, could play the part of ferments, and it was they who had shown the minute cellular particles that are the agents of fermentative activity. It was Béchamp who had demonstrated that the egg "contains nothing organised except microzymas; everything in the egg, from the chemical point of view, will be necessary for the work of the microzymas; if in this egg its ordered procedure should be disturbed by a violent shaking, what happens? The albuminoid substances and the bodies of fat remain unchanged, the sugar and the glucogen disappear, and in their place are found alcohol, acetic acid and butyric acid; a perfectly characterised fermentation has taken place there. That is the work of the microzymas, the minute ferments, which are the agents and the cause of all the observed phenomena. And when the bird's egg has accomplished its function, which is to produce a bird, have the microzymas disappeared? No; they may be traced in all the histological elements; they pre-exist—one finds them again during the functioning and the life of the elements; one will find them yet again after death; it is by them that the tissues are made alive. The part of organised beings essentially active and living, according to the physiologists, is the granular protoplasm. We went a step farther and said it is the granulations of the protoplasm, and though for their perception a sort of spiritual insight

[1] *Comptes Rendus* 75, p. 1523.

is required, we have based our conclusions upon experimental proofs of the most varied and positive nature. Bichat looked upon the tissues as the elements of the bodies of the higher animals. With the help of the microscope very definite particles, cells, were discovered, and were regarded in their turn as elementary parts, as the last term of the analysis, as a sort of living molecule. We have said in our turn: The cell is an aggregate of a number of minute beings, having an independent life, a separate natural history. Of this natural history we have made a complete description. We have seen the microzymas of animal cells associate two by two, or in larger numbers, and lengthen into bacteria. . . . We have studied the function of these microphytic ferments in physiology, in pathology and after death. We have first determined their importance in the function of secretions and shown that this functioning is, after all, only a special mode of nutrition. We have considered them as builders of cells. . . . We have also announced the importance of microzymas in pathology: 'In typhoid fever,' we said in 1869, 'in gangrene, in anthrax, the presence of bacteria has been established in the tissues and in the blood, and there has been a strong disposition to look upon this as a fact of ordinary parasitism. It is evident, after what we have said, that instead of maintaining that the disorder has for source and cause the introduction into the organism of foreign germs with their consequent action, it should instead be affirmed that it is only a matter of a deviation from the normal functioning of microzymas, indicated by the change effected in their form.' (*Congrès Médical de Montpellier, 1869. Montpellier Médical, Janvier, 1870.*) . . . All modern works on contagion and viruses are baseless outside the doctrine of the microzymas. After death, we said again at the Medical Congress of Montpellier in 1869, it is necessary for matter to return to its primitive state, for it has only been lent for a time to the organised living being. In these latter days an excessive rôle has been ascribed to germs carried by the air; the air may bring them, true enough, but they are not essential. The microzymas in their bacterial stage are sufficient to assure, by putrefaction, the circulation of matter. We have thus demonstrated for a long time not only that cells can behave as ferments, but also which are the parts in them that undertake this rôle. The cell, it is said, does not die at the same time as the being or the organ of which it forms a part. This proposition is badly expressed. The cell dies fast enough, if one considers as such the external envelope or even the nucleus. It is known that it is

impossible to study histology on a corpse, so capable is it of varied fermentations; a few hours after death it is sometimes impossible to find a single epithelial cell intact. What should be stated is that the whole cell does not die; this we have demonstrated for a long time by rearing the parts in them that survive. M. Pasteur foresees that a new path will be opened in physiology. In 1869 we wrote as an epitome of all our preceding work: 'The living being, teeming with microzymas, carries in himself with these microphytic ferments the essential elements of life, of disease, of death and of complete destruction.' This new path we have not only foreseen, but have actually opened many years ago and have persistently pursued it."

In face of this restrained but damning protest, Pasteur could not keep silent. So we find that on the 9th December he presented to the Academy "Observations on the Subject of Three Notes Communicated at the Last Session by Messrs. Béchamp and Estor."[1]

"I have read with attention," he said, "these Notes or claims of priority. I find in them only appreciations, the truth of which I believe I am authorised to dispute, and some theories, the responsibility for which I leave to their authors. Later, and at my leisure, I will justify this judgment."

But apparently the leisure was never accorded him. Pasteur relapsed into silence.

No "justification of his judgment" being forthcoming, Professor Béchamp and Professor Estor sent up the following Note on 30th December, 1872:[2] "We beg the Academy to permit us to place on record that the observations inserted in the name of M. Béchamp and of ourselves, on pages 1284, 1519 and 1523 of the present volume of the *Comptes Rendus,* remain unanswered."

The facts indeed seem unanswerable. The famous chemist who had gained the ear of the public, that exceedingly credulous organ, and had put forward as his own so much of Béchamp's teaching, was now completely checked in his attempted incursion into the microzymian doctrine. Here he had to cry a halt and content himself with his own assertion that "fermentation is life without air, without oxygen." To this, applying his own approved test of time, we find his admirers regretfully acknowledging the deficiencies of his explanation.

"It would be out of place here," say his biographers, Professor

[1] *Comptes Rendus* 75, p. 1573.
[2] *Comptes Rendus* 75, p. 1831.

and Mrs. Frankland,[1] "to discuss the criticisms which at the present day are being actively carried on; one of the principal objections to the acceptance of Pasteur's views being the omission of all consideration of the element of time in estimating the fermentative power of yeast. . . . Within the present year (1897) the discovery has been made by E. Büchner that a soluble principle giving rise to the alcoholic fermentation of sugar may be extracted from yeast cells, and for which the name of *zymase* is proposed. This important discovery should throw a new light on the theory of fermentation, as it will soon be possible to attack the problem in a new and much more decisive manner. Thus it is presumably very improbable that the action of this soluble zymase is influenced by the presence or absence of air. . . ."

Thus the test of time makes answer to the pronouncements of Pasteur! And if his exponents would only study the old records of the French Academy of Science, as well as the panegyrics of a dutiful son-in-law,[2] not only might their point of view undergo a change, but they would be spared the blunder of attributing to Büchner at the end of the nineteenth century a discovery made by Professor Antoine Béchamp little more than midway through that "Wonderful Century"!

[1] *Pasteur,* by Professor and Mrs. Frankland, chap. IX.
[2] M. René Vallery-Radot.

Who First Discovered the Cause of Vinous Fermentation—

BÉCHAMP or PASTEUR?

BÉCHAMP	PASTEUR
1864 10 October	1872 7 October
Communication to the Academy of Science[3] on "The Origin of Vinous Fermentation." An account of experiments that prove vinous fermentation to be due to organisms on the skin of grapes and also found on the leaves and other parts of the vine, so that diseased vines may affect the quality of the fermentation and the wines that result from it.	Communication to the Academy of Science[4] on "New Experiments to Demonstrate that the yeast-germ that makes wine comes from the exterior of grapes."

[3] *Comptes Rendus* 59, p. 626.
[4] *Comptes Rendus* 74, p. 781..

Corollary

That Béchamp's discovery antedated Pasteur's by eight years and that his explanation was considerably fuller.

Did Pasteur come to acknowledge Béchamp's contention that there is fermentation apart from the action of air-borne organisms, but fail to substantiate any claim to this discovery?

1872

BÉCHAMP & ESTOR

2 December

Communication to the Academy of Science[1] on "Observations upon M. Pasteur's Note of the 7th October." It was shown that it was they who for many years past had taught that every being, or rather every organ in such a being and every collection of cells in such an organ, could play the part of ferments by means of the minute cellular particles, the fermentative agents.

The new path to physiology they had not only foreseen, but had opened up and persistently pursued for many years.

30 December

A Note to the Academy of Science[2] asking for the fact to be recorded that their observations on M. Pasteur's Communication remain unanswered.

[1] *Comptes Rendus* 75, p. 1523.
[2] *C. R.* 75, p. 1831.

PASTEUR

7 October

Communication to the Academy of Science[3] that "Every being, every organ, every cell that lives without the help of oxygen must possess the character of a ferment."

The opening foreseen of "a new path to physiology and medical pathology."

9 December

Expressed to the Academy of Science,[4] hope to be able later, at his leisure, to dispute the Communication of Messrs. Béchamp and Estor.

[3] *Comptes Rendus* 75, p. 785.
[4] *C. R.* 75, p. 1573.