THE BLOOD AND ITS THIRD ELEMENT

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ANTOINE BECHAMP

A DISTANT MIRROR

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EDITOR'S PREFACE

This book is the last work by Professor Antoine Béchamp, a man who should, by rights, be regarded today as one of the founders of modern medicine and biology. History, however, is written by the winners, and too often has little to do with the truth. The career of Antoine Béchamp, and the manner in which both he and his work have been written out of history, are evidence of this.

During his long career as an academic and researcher in nineteenth century France, Béchamp was widely known and respected as both a teacher and a researcher. As a leading academic, his work was well documented in scientific circles. Few made as much use of this fact as Louis Pasteur, who based much of his work on plagiarising and distorting Béchamp's research. In doing so, Pasteur secured for himself an undeserved place in the history of medical science.

There have been several excellent books written, mainly in the early decades of the twentieth century, which explain in detail the plagiarisms and injustices which Pasteur and his allies inflicted on Béchamp. Among these are *Pasteur Exposed* (previously published as *Béchamp or Pasteur?*) by Ethel Douglas Hume, and *The Dream and Lie of Louis Pasteur* (previously *Pasteur, Plagiarist, Imposter*) by R. Pearson.

The Blood and its Third Element is Béchamp's explanation of his position, and his defence of it against Pasteur's mischief. It was his last major work, and as such it embodies the culmination of his life's researches.

This book contains, in detail, the elements of the microzymian theory of the organization of living organisms and organic materials. It has immediate and far reaching relevance to the fields of immunology, bacteriology, and cellular biology; and it shows that more than 100 years ago, the germ, or microbian, theory of disease was demonstrated by Béchamp to be without foundation.

The reader should be aware when reading *The Blood and its Third Element* that in formulating his microzymian theory of biological organisation, Béchamp in no way sought to establish it as the last word on the subjects of disease, its transmission, general physiology, or indeed the organisation of living matter itself. Béchamp worked continuously until a few weeks before his death; and if he were working now, he would no doubt still regard his work as unfinished, and subject to revision and development.

It is no accident, but rather a vindication of Béchamp's theories, that many researchers over the course of the twentieth century and up to the present have arrived at conclusions in various disciplines that support the microzymian model.

In the United States during the 1920s and '30s, Royal Rife's microscope revealed processes of life which confused many of Rife's contemporaries, but which would have made perfect sense to Béchamp. The medical establishment, however, was disturbed by the implications of Rife's discoveries, especially so when he began curing diseases, including cancer, with electromagnetic frequencies. Rife and his discoveries were soon consigned to that special anonymity which is reserved for those who threaten the status quo. To maintain the profits of the drug companies and the authority of the medical establishment, no expense or effort is too great, and by the time Rife died, his work was all but forgotten. The authorities confiscated and destroyed all of his equipment and writing that they could get their hands on. Fortunately, in recent years, interest in his work has revived, as a search on the internet will demonstrate.

Contemporary researchers whose work connects with that of Béchamp include Gaston Naessens (*www.cerbe.com*), whose 'somatids' are without doubt what Béchamp described as 'microzymas'. Naessens has gone further than Béchamp, though, aided by his revolutionary microscope technology, and has identified the various stages of the somatid life cycle.

Just recently, Dr. Philippa Uwins of the Centre for Microscopy and Microanalysis at the University of Queensland in Australia has been making headlines with her work documenting the existence of 'nanobes', which she describes as involving the "morphological and microstructural characterisation of novel nano-organisms".

One can't help but think that Béchamp, Rife, Naessens and Dr Uwins are all talking about the same thing.

There is no single cause of disease. The ancients thought this, Béchamp proved it and was written out of history for his trouble. The relevance of his work to the dilemmas that plague modern medical science remains as yet unrealized.

Fortunately, though, there are streams of modern research such as the ones mentioned previously that are heading in the right direction, even though they are encountering resistance and cynicism. This book is being republished in the hope that the information it contains can contribute to that research.

This new edition has been reset, in a new layout that will hopefully make the content more accessible. Wherever it has been possible without altering the intent of the author, archaic or ambiguous use of English has been brought up to date.

The footnotes are either Professor Béchamp's or Montague Leverson's. Where they belong to Leverson, they are enclosed by square brackets.

When the letters *C.R.* appear in a footnote, they denote the *Comptes Rendus* (trans. *transactions*) of the various French academies cited in the text.

D. L. Major

TRANSLATOR'S PREFACE

On October 16th, 1816, at Bassing, in the department of Bas-Rhein, was born a child by whose name the nineteenth century will come to be known, as are the centuries of Copernicus, Galileo and Newton by their names.

Antoine Béchamp, the babe of 1816, died on the 15th April, 1908, fourteen days after he was first visited by an aged American physician between whom and himself a correspondence had passed for several years on the subject of the researches and wonderful discoveries of Professor Béchamp and his collaborators. The American physician made his visit to Paris for the purpose of becoming personally acquainted with the Professor, who, as his family stated, had looked forward with eager anticipation to such a visit.

The translator had long previously submitted an extensive summary of the professor's physiological and biological discoveries, by whom it was revised and approved.

This was intended to be introduced as a special chapter in an extensive work on inoculations and their relations to pathology, upon which the translator of this work had been engaged, almost exclusively, for some fourteen years.

But in the lengthy and nearly daily interviews between Professor Béchamp and myself, which, as just shown, closely preceded the former's death, I suggested that instead of such summary it would be better to place before the English speaking peoples an exact translation into their language of some, at least, of the more important discoveries of Professor Béchamp—especially as, in my opinion, it would not be easy to carry out among them the conspiracy of silence by means of which his discoveries had been buried in favour of distorted plagiarisms of his labours which had been productive of such abortions as the microbian or germ theory of disease, "the greatest scientific silliness of the age," as it has been correctly styled by Professor Béchamp.

To this suggestion Professor Béchamp gave hearty assent, and told me to proceed exactly as I might think best for the promulgation of the great truths of biology, physiology, and pathology discovered by him, and authorised me to publish freely either summaries or translations into English, as I might deem most advisable.

As a result of this authorisation, the present volume is published, and is intended to introduce to peoples of the English tongue the last of the great discoveries of Professor Béchamp.

The subject of the work is described by its title, but it is well to remind the medical world and to inform the lay public that the problem of the coagulation of the blood, so beautifully solved in this volume, has until now been an enigma and opprobrium to biologists, physiologists and pathologists.

The professor was in his 85th year at the time of the publication of the work here translated. To the best of the translator's knowledge it has not yet been plagiarised, and is the only one of the Professor's more important discoveries which has not been so treated; but at the date of its publication the arch plagiarist (Pasteur) was dead, though his evil work still lives.

One of the discoveries of Béchamp was the formation of urea by the oxidation of albuminoid matters.[1] The fact, novel at the time, was hotly disputed, but is now definitely settled in accordance with Béchamp's view. His memoir described in detail the experimental demonstration of a physiological hypothesis of the origin of the urea of the organism, which had previously been supposed to proceed from the destruction of nitrogenous matters.

By a long series of exact experiments, he demonstrated clearly the specificity of the albuminoid matters and he fractionised into numerous defined species albuminoid matters which had until then been described as constituting a single definite compound.

He introduced new yet simple processes of experimentation of great value, which enabled him to publish a list of definite compounds and to isolate a series of soluble ferments to which he gave the name of *zymases*. To obscure his discoveries, the name of *diastases* has often been given to these ferments, but that of *zymas* must be restored. He also showed the importance of these soluble products (the zymases) which are secreted by living organisms.

He was thus led to the study of fermentations. Contrary to the then generally accepted chemical theory, he demonstrated that the alcoholic fermentation of beer yeast was of the same order as the phenomena which characterise the regular performance of an act of animal life—digestion.

In 1856, he showed that moulds[2] transformed cane sugar into invert sugar (glucose) in the same manner as does the inverting ferment secreted by beer yeast. The development of these moulds is aided by certain salts, impeded by others, but without moulds there is no transformation.

He showed that a sugar solution treated with precipitated calcic carbonate does not undergo inversion when care is taken to prevent the access to it of external germs, whose presence in the air was originally demonstrated by him.[3] If to such a solution the calcareous rock of Mendon or Sens be added instead of pure calcic carbonate, moulds appear and the inversion takes place.[4]

These moulds, under the microscope, are seen to be formed by a collection of molecular granulations which Béchamp named *microzymas*. Not found in pure calcic carbonate, they are found in geological calcareous strata, and Béchamp established that they were living beings capable of inverting sugar, and some of them to make it ferment. He also showed that these granulations under certain conditions *evolved into bacteria.*

To enable these discoveries to be appropriated by another, the name *microbe* was later applied to them, and this term is better known than that of microzyma; but the latter name must be restored, and the word microbe must be erased from the language of science into which it has introduced an overwhelming confusion. It is also an etymological solecism.[5]

Béchamp denied spontaneous generation, while Pasteur continued to believe it. Later he, too, denied spontaneous generation, but he did not understand his own experiments, and they are of no value against the arguments of the sponteparist Pouchet, which could be answered only by the microzymian theory. So, too, Pasteur never understood either the process of digestion nor that of fermentation, both of which processes were explained by Béchamp, and by a curious imbroglio (was it intentional?) both of these discoveries have been ascribed to Pasteur.

That Lister did, as he said, most probably derive his knowledge of antisepsis (which Béchamp had discovered) from Pasteur is rendered probable by the following peculiar facts.

In the earlier antiseptic operations of Lister, the patients died in great numbers, so that it came to be a gruesome sort of medical joke to say that "the operation was successful, but the patient died." But Lister was a surgeon of great skill and observation, and he gradually reduced his employment of antiseptic material to the necessary and not too large dose, so that his operations "were successful and his patients lived."

Had he learned his technique from the discoverer of antisepsis, Béchamp, he would have saved his earlier patients; but deriving it second hand from a savant (sic) who did not understand the principle he was plagiarising,[6] Lister had to acquire his subsequent knowledge of the proper technique through his practice, i.e. at the cost of his earlier patients.

Béchamp carried further the aphorism of Virchow—*Omnis cellula e cellula*—which the state of microscopical art and science at that time had not enabled the latter to achieve. Not the cellule but the microzyma must, thanks to Béchamp's discoveries, be today regarded as the unit of life, for the cellules are themselves transient and are built up by the microzymas, which, physiologically, are imperishable, as he has clearly demonstrated.

Béchamp studied the diseases of the silk worm then (1866) ravaging the southern provinces of France and soon discovered that there were two of them—one, the pébrine, which is due to a parasite;[7] the other, the flacherie, which is constitutional.

A month later, Pasteur, in a report to the Academy of his first silkworm campaign, *denied* the parasite, saying of Béchamp's observation, "that is an error." Yet in his second report, he adopted it, as though it were his own discovery!

The foregoing is but a very imperfect list of the labours and discoveries of Antoine Béchamp, of which the work here translated was the crowning glory.

The present work describes the latest of all the admirable biological discoveries of the Professor Béchamp. It is proposed to follow it up with a translation of *The Theory of the Microzymas and the Microbian System* now in course of translation; and *The Microzymas,* the translation whereof is completed. Other works will, it is hoped, follow, viz.: *The*

Great Medical Problems, the first part of which is ready for the printer, *Vinous Fermentation,* translation complete; and *New Researches upon the Albuminoids,* also complete.

The study of these and of the other discoveries of Professor Béchamp will produce a new departure and a sound basis for the sciences of biology, of physiology and of pathology, today floating in chaotic uncertainty and confusion; and will, it is hoped, bring the medical profession back to the right path of investigation and of practice from which it has been led astray into the microbian theory of disease, which, as before mentioned, was declared by Béchamp to be the "greatest scientific silliness of the age."

Montague R. Leverson
London, 1911

NOTES

A few notes have been appended by the translator; these are distinguished from the author's by being enclosed in square brackets—[ ] and by the phrase —*Trans.* which appears at the end of the note.

The letters *C.R.* are used for the words *Comptes Rendus* (trans. *transactions*) of the various French academies cited in the text.

AUTHOR'S PREFACE / 1

There is nothing but what ought to be.

—*Galileo*

Nothing is created, nothing is lost.
—*Lavoisier*

Nothing is the prey of death:
all things are the prey of life.

—*The author*

An historian of the founders of modern astronomy recently related that the philosopher Cleanthus, three millennia before our era, wished to prosecute Aristarchus for blasphemy, for having believed that the earth moved, and having dared to say that the sun was the immovable centre of the universe. Two thousand years later, human reason having remained stationary, the wish of Cleanthus was realized. Galileo was accused of blasphemy and impiety for having, like Copernicus and following Aristarchus, maintained the same truth; a tribunal condemned his writings, and forced him to a recantation which his conscience denied.

The following is the judgement of the historian upon this event:

> " Never perhaps has the generous detestation of the public conscience for intolerance shone forth more strongly than around the name of Galileo.
>
> The narrative of his misfortunes, exaggerated like a holy legend, has affirmed, while avenging him, the triumph of the truths for which he suffered; the scandal of his condemnation will forever vex in their pride those who would oppose force to reason; and the righteous severity of opinion will preserve its inconvenient remembrance as an eternal reproach thrown in their teeth to confound them."

The "righteous severity of the judgement" which preserves the inconvenient memory of the sufferings of Galileo, it is well to mention, is that of the scholarly and learned members of Academies whereof the author forms part. It is agreed; yes, intolerance is odious and hateful, the situation of Galileo was particularly horrible. He was forced to go to church and pronounce with a loud voice the abjuration dictated to him.

> " I, Galileo, in the seventieth year of my age, on my knees before your Eminences, having before my eyes the holy gospels, which I touch with my own hands, I abjure, I curse, I detest, the error and the heresy of the movement of the earth."

There is no more atrocious torture than this brutal violence against the conscience of a man. It is the greatest abuse of force and pride when we know that it was the priests of Jesus Christ who perpetrated it.

The theologians of the holy office were not competent to judge the astronomer Galileo, yet they in their ignorance undertook to proscribe an opinion which differed from their own as being erroneous and contrary to the holy Scriptures, which, said the Popes, "were dictated by the mouth of God himself." In truth what did they know about it? Assuredly it is distressing to observe how long human reason can remain at the same point.

It is then interesting to know whether the lesson taught by the condemnation of Galileo has been properly learned, and if three centuries later "the righteous severity of the judgement against those who would still resist the power of reason" would be able to protect those who labour disinterestedly for the triumph of the truth. Have those who, for the large public, are the authoritative judges of the value of the discoveries of others become less intolerant, or at least more impartial, less prompt to pronounce against opinions which they do not share, and less anxious to deny facts than to test them?

And if the lesson has not been learned, it is not less interesting to ask whether it is "human reason" which must be held responsible; if it might not instead be "pettifogging" ratiocination, the abuse of reasoning warped by passion and too often by personal interest which overcomes private conscience and leads the public astray.

The history of a discussion wherein chemistry and physiology closely united were interested, which agitated the second half of the 19th century, is well adapted to show that human nature has not changed since the time of Cleanthus, and that there always exist people ready to associate themselves together to contradict or insult the unfortunate wretch who has devised some new theory, based upon unsuspected facts, which would compel them to reform their arguments and abandon their prejudices.

This work upon the blood, which I present at last to the learned public, is the crown to a collection of works upon ferments and fermentation, spontaneous generation, albuminoid substances, organization, physiology and general pathology which I have pursued without relaxation since 1854, at the same time with other researches of pure chemistry more or less directly related to them, and, it must be added, in the midst of a thousand difficulties raised up by relentless opponents from all sides, especially whence I least expected them.

To solve some very delicate problems I had to create new methods of research and of physiological, chemical and anatomical analysis. Ever since 1857 these researches have been directed by a precise design to a determined end: the enunciation of a new doctrine regarding *organization and life.*

It led to the microzymian theory of the living organization, which has led to the discovery of the true nature of blood by that of its third anatomical element, and, at last, to a rational, natural explanation of the phenomenon called its spontaneous coagulation.

But the microzymian theory, which is to biology what the Lavoisierian theory of matter is to chemistry, and which is founded on the discovery of the microzymas, living organisms of an unsuspected category, has been attacked in its principle, by denying the very existence of the microzymas.

Since this was so, if the assertion that the microzymian theory of the living organization gives to biology a base as solid as does the Lavoisierian theory to chemistry be deemed imprudent, well, I choose to commit this imprudence, and to be imprudent to the end, and to struggle against a current of opinion which is the more violent, as will be seen, the more it is artificial.

It was the boldest of those who deny the fact of the existence of the microzymas who wrote:

> "Whenever it can be done, it is useful to point out the connection of new facts with earlier facts of the same order. Nothing is more satisfying to the mind than to be able to follow a discovery from its origin to its latest development."[1]

That is very well and fine, the more so that the author took good care not to follow this wise precept; let us ascend then to the sources.

Two centuries after Galileo, we were still in the Aristotelian hypothesis regarding matter, but reinforced by the alchemical hypothesis of *transmutation* and the Stahlian one of *phlogiston.* It was readily conceded that matter could *of itself* become living matter, animated, such as it is in plants and animals; thus it was that spontaneous generation was still generally accepted.

Charles Bonnet himself said that organization was *the most excellent modification of matter*; nevertheless that learned naturalist and philosopher attempted to oppose spontaneous generation by imagining in turn the hypothesis of encapsulation and that of pre-existing germs universally diffused, whereof Spallanzani made use to refute the experiments and conclusions of the sponteparist Needham.

On the other hand, to sustain Needham, Buffon invented the hypothesis of *organic molecules,* not less universally diffused, whose substance, distinct from common matter, called raw matter, helped to explain the growth of plants and animals, as well as spontaneous generation.[2]

Fermentations and ferments were very simply explained. Macquer, in 1772, regarded it as certain that vegetable and animal matters, abstracted from living organisms, under certain conditions of the presence of water and of contact, at least momentarily, with the air and of temperature, become altered of themselves, and ferment, becoming putrid in producing the ferment.

And according to the same principles it was said that water could transmute itself into earth, the earth into a poplar, and that the blood begets itself by the transmutation of flesh into the flowing liquor.

Such in a few words was the condition of science upon these questions before the advent of Lavoisier. In the Lavoisierian theory there is no matter other than that of simple bodies, which are heavy, indestructible by the means at our disposal, and always reappearing the same, not withstanding all the vicissitudes of their various combinations among themselves and the changes of states or allotropic modifications they might undergo. No transmutations and no phlogistication to explain the phenomena.

In this theory, matter is only mineral, simple bodies being essentially mineral. There is no living or animal matter, no matter essentially organic.

That which, long after the time of Lavoisier, chemists have called organic matters are only innumerable combinations in the various proportions which carbon, hydrogen, oxygen, and nitrogen can form, often with other simple bodies at the same time—sulphur, phosphorus, iron, etc, carbon being always present, so that what is called organic matter in modern chemistry is only various combinations of carbon with the simple bodies mentioned.

In fact, Lavoisier, after his demonstration that water did not become transmuted into earth, nor earth into plants, asserted clearly that plants draw their food from the air, as was verified later. He even asserted that animals obtained the materials for their nutrition from plants, thus demonstrating that plants effected the synthesis of the substance without which animals could not exist. Even respiration was only a common phenomenon of oxidation.

The substance of plants and animals being only combinations of carbon with hydrogen and oxygen, with the addition of nitrogen for animals, it is very interesting to recall shortly what Lavoisier thought of the putrefaction of these substances and of fermentation.

Like everybody, he knew that the juice of grapes or apples enters into fermentation of itself to produce wine or cider, and he wrote the following equation:

grape = *must* = *carbonic acid* + *alcohol*

To demonstrate this, he reduced the experiment to the employment of sugar, which he called a vegetable oxide, and of water and a ferment. The following is his account of the experiment:

> " To ferment sugar, it must first be dissolved in about four parts of water. But water and sugar, no matter what proportions be employed, will not ferment alone, and equilibrium will persist between the principles

> (the simple bodies) of this combination if it is not broken by some means.
>
> A little yeast is sufficient to produce this effect and to give the first movement to the fermentation; it then continues of itself to the end. The effects of vinous fermentation reduced themselves to separating the sugar into two portions, to oxygenize the one at the expense of the other to produce carbonic acid of it; to deoxygenize the other in favour of the former to make alcohol of it; so that if it were possible to recombine the alcohol and carbonic acid, the sugar would be reformed."

It is thus clear that Lavoisier instead of the equation regarding the *must* might have written thus:

$$\textit{sugar} = \textit{carbonic acid} + \textit{alcohol}$$

Lavoisier intended to give elsewhere an account of the effects of yeast and of ferments in general, which he was prevented from doing. But it can be seen from his *Treatise upon Elementary Chemistry,* published in 1788, that he had established that yeast is a quarternary nitrogenised body, and that that which remained of it at the end of the fermentation contained less nitrogen, and that besides the alcohol, a little acetic acid was formed. Lavoisier also found that after distillation there remained a fixed residue representing about 4% of the sugar. We shall see later the importance of these remarks.

It might thereafter have been anticipated that Lavoisier should explain the phenomena of the putrid fermentation of vegetable and animal substances "as operating by virtue of very complicated affinities" between the constituted principles of these substances (the simple bodies), which in this operation cease to be in equilibrium so as to be constituted into other compounds.

Bichat, who died in 1802 at the age of 31, had been much struck by the results of the labours of Lavoisier. He could not accept a living matter constituted of pure chemical compounds whereof the simple elements are the constituent principles. He imagined, then, that the only living things in a living being are the organs composed of the tissues, of which he distinguished twenty-one as elementary anatomical elements, as the elementary bodies are chemical elements. Such was the first influence of the Lavoisierian theory upon physiological anatomy; it was thus that in 1806 in the third edition of his *Philosophie Chimique*, Fourcroy said:

> " Only the tissue of living plants, only their vegetating organs, can form the matters extracted from them, and no instrument of art can imitate the compositions which are prepared in the organized machines of plants."

Let us bear in mind that Bichat had been led by the Lavoisierian theory of matter to lay down a new principle of physiology. As Galileo had laid down the metaphysical principle "nothing is but what ought to be", Dumas drew from the chapter on fermentation of Lavoisier's treatise the following principle, which is also a necessary one: *"nothing is created, nothing is lost."*

We have above rapidly sketched the state of the relations of chemistry and physiology as well as the state of the subject of fermentations at the beginning of the nineteenth century; we will now see what they were at the commencement of the second half of that century, in about 1856.

The chemists, thanks to direct analytical methods which were more and more perfected, had isolated a great number of incomplex compounds, acids, alkaloids, neutral or having diverse functions, from vegetable and animal substances. Those incomplex compounds were more and more exactly specified under the name of *proximate principles* of plants and of animals, nitrogenised ternaries and quarternaries.

Among the nitrogenised proximate principles, a number of them were distinguished as soluble or insoluble, and also uncrystallisable, such as the albumin of the white of egg and of the serum of blood, caseum (later called casein) of milk, the fibrin of the blood and that of the muscles, the gelatine of the bones, the gluten of wheat, the albumin of the juices of plants, etc. In time, the similarity of their composition and of certain of their common properties with the albumin of the white of egg led to these matters being formed into the groups of the albuminoid matters.

Lavoisier knew these albuminoid matters only in so far as they were nitrogenised animal matters.

Now after the discovery of gluten, of vegetable albumen, and nitrogenised quarternaries like beer yeast, it was admitted that they were the ferment of vinous fermentation. Then, generalising, it came to be thought that albumin, the albuminoids in general, became or were directly the ferment, while the ternary proximate principles, such as cane sugar, grape sugar, milk sugar, the other sugars, amylaceous matter, inulin, gum, mannite, etc, were called fermentescible matter.

Matters had reached this point when in about 1836, Cagniard de Latour, resuming the study of beer yeast[3] and of its multiplication during the fermentation which produces beer, regarded it as organized and living, decomposing the sugar into alcohol and carbonic acid by an effect of its vegetation.

That was a conception as original as that of Bichat. It is not because of his having regarded beer yeast as organised and its multiplication during fermentation as a multiplication by vegetation that the conception of Cagniard de Latour is original; it is because he admitted that the fermentation of the sugar operated by an effect of this vegetation, that is to say, owing to a physiological act.

That was an absolutely new point of view; beer yeast, the only isolated ferment known, ceased to be regarded as a precipitate of albuminoid matter which had become insoluble, and was henceforth looked upon as a living being! Consequently yeast ceased to be regarded as the reagent that Lavoisier had said was able to disturb the equilibrium of the simple bodies which constituted sugar.

Also, soon afterwards, Turpin, the botanist, interpreted the effect of the vegetation of Cagniard by saying that the globule of yeast was a cellule which decomposed sugar in nourishing itself. Dumas went further, and asserted that the ferments, the yeast, behaved as do animals when feeding, and that, for the orderly maintenance of the life of the yeast, there was needed, as for animals, nitrogenised albuminoid matter as well as sugar.

In Germany, Schwann supported the opinion of Cagniard de Latour while broadening the question; he supposed that no animal or vegetable substance altered of itself and that every phenomenon of fermentation presupposed a living ferment. To prove this, he experimented as Spallanzani had done—improving upon his method in order to demonstrate that the infusoria or ferments had their origin in the germs of the air. The experiments of Schwann were confirmed by others.

But the conception of Cagniard de Latour did not prevail, nor especially the interpretation of Turpin and Dumas. It was not denied that infusoria or moulds existed in the mixtures in a state of alteration, but it *was* denied that they were the agents of the fermentation; this would begin of itself and the altered matter was regarded as evidence in favour of either spontaneous generation or the production of these living products by the germs of the air.

The discovery of diastase and synapse, soluble and nitrogenised quarternaries like yeast, was held to legitimize the refusal to consider yeast as acting because it was organized and living.

Now because these substances were reagents of rare power for transforming certain fermentescible matters in aqueous solution, the transformations were called fermentation, and these reagents were called ferments; and it was said that it is not because they are organized and living that the ferments act to effect the phenomena of fermentation.

Then the opponents of the doctrine of Cagniard de Latour and Schwann, with regard to fermentations and the relations of chemistry to physiology, triumphed so completely that opinions reverted to the point maintained in 1788. The principle of Bichat's doctrine was lost to view; not only was it proposed that vegetable and animal matters altered of themselves under the conditions specified by Macquer, but so too the proximate principles extracted from them, even cane sugar, the aqueous solution whereof Lavoisier had declared to be unalterable.

In short, the old hypothesis of germs of the air, which Schwann had revived, was completely lost to view.

Nothing is better fitted to convince one that the human soul during the second half of the 19th century has remained the same as it was in the times of Galileo and of the inquisition than to reflect upon the sequel of the history I have just sketched out.[4]

I will now describe the fundamental experiment, the results whereof have completely changed the aspect of science with regard to the relations of chemistry and physiology with fermentation, such as they were still imagined to be at the end of the year 1857, after the theory of Cagniard de Latour in relation to yeast had been rejected.

In 1854, it was conceded that cane sugar dissolved in water altered of itself and became transformed into what is called invert sugar, because the solution which deviated the plane of polarisation to the right before the alteration deviated it to the left afterwards. The inverted sugar was also called grape sugar. The phenomenon of this alteration was called *inversion*.

With reference to other researches I resolved to verify the fact, and in the month of May, 1854, I left to themselves in a closed flask, in the presence of a small volume of air, at ordinary temperature and in a diffused light, some aqueous solutions of pure cane sugar. After several months, I found that the sugar solutions in pure distilled water were partly inverted.

At the beginning of 1855 I published the observation as a verification of the fact, but I mentioned at the same time the presence of a mould in the inverting liquor. It is not an unusual thing to see moulds appear in aqueous solutions of the most diverse substances.

That was why, in the then state of science and given the contradictory assertions regarding the experiments of Schwann, I would not assert anything beyond the fact. I noted merely that in the solutions to which I had added chloride of calcium, or chloride of zinc, the inversion had not taken place and no mould had appeared. To find an explanation of these differences I made various experiments, commencing in 1855 and continuing to the month of December, 1857.

Among these experiments, all accordant with one another, I select two, because, reducing the problem to its simplest expression, they leave no room for doubt concerning the legitimacy of the conclusions I deduced from them.

The first conclusion was that the solution of cane sugar in distilled water remains indefinitely unchanged when, having been boiled, it is preserved in an absolutely full closed vase.

The second was that the same solution, whether boiled or not, left in a closed vessel in the presence of a limited volume of air permits the appearance of colourless moulds, generally myceliennated, and the solution becomes completely inverted in the course of time, while the liquor reddens litmus paper, that is to say, it becomes acid. To prove that the volume of air left in the closed flask has nothing to do with the inversion, it suffices to add beforehand a small quantity of creosote[5] or a trace of sublimate of mercury to ensure that the liquid shall not become acid, or mouldy, and that the sugar will remain unchanged.

These two experiments clearly demonstrated to me that the presence of the air was essential for the inversion to take place and for the moulds to be born, and at the same time that the volume of air left present could not operate the inversion.

It was then necessarily the developed moulds which were the agents of the phenomena observed. But myceliennated moulds are true microscopic plants, and consequently organized and living. I proved that they were nitrogenised and that, introduced into creosoted sugar water, they inverted the cane sugar much more rapidly than during their development. Nevertheless these moulds being insoluble, I asked myself: *how do they do it?* And I supposed that it was by an agent analogous to diastase and also thanks to the acid formed; but I have since demonstrated that it was indeed chiefly by means of a soluble ferment which they contain and which they secrete. And the presence of this soluble ferment, and consequently of an albuminoid matter, explained to me how, being nitrogenised, the moulds, when heated with caustic potash, set free an abundance of ammonia.

But these moulds being nitrogenised could not be born of the cane sugar, which I have proven to be exempt from nitrogen. Besides this sugar there was nothing present but distilled water, the mineral substance of the glass, and no other nitrogen than that of the air left in the closed flask; now (thanks to a little creosote or mercuric chloride) the experiment itself showed that these materials could not unite of themselves, by synthesis, to produce the substance of the moulds. Nothing remained to explain the birth of the organized productions other than the old hypothesis of germs; which allowed me no rest until I had discovered their origin and nature.

While waiting to specify them, I admitted that under the conditions of the experiment "germs brought by the air found in the sugared solution a favourable medium for their development";[6] a development during which the new organism, making use of the materials present, effects the synthesis of the nitrogenised and non-nitrogenised materials of its substance.

Under the conditions of the experiment such as I have reported, where there are no other mineral matters than those of the glass, the crop of organized production is necessarily very small, and the inversion as well as the transformations which follow it are very slow.

The addition of certain salts or of creosote hinders the inversion by preventing the development of the germs, either by rendering the medium sterile or by acting directly upon the former.

But the addition of certain other purely mineral salts, even of arsenious acid, had the effect of increasing the harvest and of singularly hastening the inversion and the other phenomena of fermentation which follow it, for if the reaction is prolonged, the acid of which I have spoken above is found to be acetic acid, with, in certain cases, lactic acid, and alcohol in all cases; but to determine the production of this last the mould must be allowed to act for several years. It was thus that I was able to establish that the study made in 1857 was really a phenomenon of fermentation, for the manifestation of which it had not been necessary to employ albuminoid matter, but which, on the contrary, was produced from these matters.

In its simplicity, the experiment was of the same order for physiological chemistry as had been the observation of Galileo with regard to the lamp, hung by a long cord, which oscillated slowly before the altar of the cathedral of Pisa. From that oscillation it was learned that it always beat the same measure, that the duration of the oscillation is independent of its amplitude, and Huyghens discovered the law of the pendulum's oscillation by connecting it with the Galilean principle of falling bodies. The consequences which have sprung from the above experiment have not been less fruitful; some day doubtless there will come a genius like that of Huyghens to extend them and increase their fruitfulness; meanwhile the following are some which I have been able to deduce from it, either in 1857 or subsequently while continuing to experiment. The chief and essential facts of the memoir of 1857 are the following.

1) Cane sugar, a proximate principle, in watery solution, is naturally unalterable even in contact with a limited volume of air, when the solution has been previously creosoted.

2) The solution of cane sugar in contact with a limited volume of air permits the appearance of moulds and the sugar is altered, first of all becoming inverted.

3) If the solution has first had creosote added to it, moulds do not appear and the sugar is not altered.

4) The fact that moulds develop in sugared water, in contact with a small limited quantity of air, forms the verification of the hypothesis of atmospheric germs; in no other way can that fact be explained.

5) Developed moulds invert the cane sugar, even when the solution has first been creosoted, i.e. the creosote which hinders the moulds from being born does not prevent them, when born, from acting. Moulds, being insoluble by reason of their being organized, effect the inversion by means of an agent analogous to diastase; that is to say, by means of a soluble ferment.

6) The totality of the phenomena of the non-spontaneous alteration of cane sugar and the production of an acid and of alcohol prove it to be a fermentation both of moulds and of ferments.[7]

These facts, studied more attentively, showed clearly, contrary to what had before been believed, that albuminoid matter was not necessary for the birth of these ferments; and also that the soluble ferments were not the products of the alteration of some albuminoid matter, since the mould produced at once the albuminoid matter and the soluble ferment by virtue of its physiological functions of development and nutrition.

Thus it resulted that the soluble ferment was allied to the insoluble by the relation of product to producer; the soluble ferment being unable to exist without the figured ferment, which is necessarily insoluble.

Further, as the soluble ferment and the albuminoid matter, being nitrogenised, could only be formed by obtaining the nitrogen from the limited volume of air left in the flasks, it was at the same time demonstrated that the free nitrogen of the air could help directly in the synthesis of the nitrogenised substance of plants. Up to that time this had been a disputed question.

Thenceforward it became evident that since the synthesis of the materials of the substance of moulds, of ferments, is necessarily produced by intussusception within the organism of these moulds, it must necessarily be that all the products of fermentation are produced there and that they are secreted therein as was secreted the soluble ferment which inverted the cane sugar.

Hence I became assured that that which is called fermentation is, in reality, the phenomenon of nutrition; i.e. the assimilation, dissimulation, and excretion of the products dissimulated.

Without doubt, these views were in conformity with the conceptions of Cagniard de Latour, even to those of Schwann and to the more precise view of Turpin and especially of Dumas; but in complete disagreement with those of their opponents, Liebig and his followers, some of whom denied that yeast was living, and held it to be nitrogenous matter in a state of decomposition, and others that it acted in so far as it was nourished, by an action of *extalyic contact,* an occult cause, and that it effected the decomposition of sugar in the same manner as did platinum that of oxygenated water.

We must then demonstrate that that which was true of the moulds was so in the same sense as in the case of beer yeast and of the ferment of the lees of wine; that is to say, that the cellules of these ferments invert cane sugar under the same conditions, in spite of the creosote, and *before* any other phenomenon of transformation is produced. It is found, in effect, that the yeast contains the soluble ferment which inverts, as the mould also contains it.

Nevertheless, the opponents of the conception of Cagniard de Latour and Schwann could always object that if the creosote prevents the cane sugar from being altered, it would not be the same in the case of a mixture containing albuminoid matter, and that consequently, if in the mixture of sugared water and beer yeast, the cane sugar was inverted, it was because beer yeast, an albuminoid substance, continued to be altered in spite of the creosote.

I replied by demonstrating that under the same conditions as the cane sugar all the true proximate principles, including soluble and insoluble albuminoids, even the most complex mixtures of proximate principles, remained unchanged, nothing organized appearing in them—provided that in the cases wherein cane sugar is present, the inverting soluble ferment does not exist among these proximate principles, because creosote does not prevent double ferments from reacting.

Two contemporary experiments of that fact greatly impressed me.

The first relates to milk. Everybody except Dumas regarded milk as an emulsion, as a pure mixture of proximate principles. Now, it is known that, like blood, it alters and clots after it is drawn, as Macquer said in the last century (the 18th).

This furnished an opportunity to verify the fact of the unchangeableness of mixtures of proximate principles when creosoted.

The milk of a cow was then creosoted while being drawn, by receiving it into vessels washed with boiling creosoted water divided into three portions; one of which was left with a limited volume of air present; a second was left without any, and in the third the air was expelled by a current of carbonic acid gas. To my very great surprise, the milk altered, became sour and clotted, almost as quickly as if no creosote had been added. And lastly, which surprised me most of all, shortly after the coagulation was completed, there was a crowd of bacteria in every part of the clot.

The second experiment relates to the chalk which chemists employed, as calcic carbonate, in their experiments even upon fermentation, and which, like them, I employed to preserve the neutrality of the media.

One day, some starch made of potato fecula had some chalk added to it to prevent it turning sour and was left in an oven at 4° to 45°C (104° to 113°F). I expected to find the starch with the same consistency as before; on the contrary, it was liquefied. "The germs of the air," I said.

I repeated the experiment, creosoting the boiling starch and added some of the same chalk; again liquefication! Much astonished, I repeated the experiment, replacing the chalk with pure artificial calcic carbonate; this time the creosoted starch was not liquefied, and I preserved it in this state for ten years.

These two experiments, in their simplicity, were of the same order, equally fundamental as that of the inversion of sugar by moulds, but they embarrassed me much more.

It was not until after other researches and after having varied and controlled them that I placed them before the learned societies of Montpellier (1863) and informed Dumas of them in a letter which he thought fit to publish,[8] in which I stated that some of the calcareous earths and milk contained living beings already developed.

And here are three other experiments, not less fundamental, which verify the first three:

1) I had ascertained that in the fermentation of cane sugar by moulds born of atmospheric germs, in a watery solution of sugar, acetic acid is produced; why is it not also produced in fermentation by beer yeast? And I shall prove that there is, in fact, produced at the same time only a very small quantity of acids homologous to acetic acid.

2) Beer yeast inverting cane sugar as do moulds, I tried to isolate from the yeast the soluble ferment it produces, as one can readily obtain as much beer yeast as may be required. I will say here how I proceeded to isolate it directly. Brewery yeast, pure, washed and drained, was treated with powdered cane sugar in suitable quantity. The mixture of the two bodies became liquefied and the sugar was entirely dissolved. The product of the liquefaction was thrown upon a filter. If the operation is performed on a sufficiently large quantity, there results the flowing off of an abundant limpid liquid before any indication of fermentation is manifested.

   The filtered liquid, being treated with alcohol, furnishes (as does an infusion of sprouted barley to precipitate its diastase) a rather considerable white precipitate, whereof the part soluble in water is the required soluble ferment. There could be no further doubt; this soluble ferment forms part of the very substance of the content of the cellule of the yeast. I gave it the name first of *zymas,* and later that of *zythozymas.*

3) The cellule of yeast, being a living organism, ought, being insoluble, to possess a vital resistance and should permit only such things to issue from its being as were disassimilated in it.

   Now, in effect, pure yeast, subjected to a methodical washing with distilled water, yields to it at first scarcely anything, only a trace of zythozymas and phosphoric acid. But there comes a time when it yields enormously, then less and less, until it has lost nearly 92% of its substance, preserving its form with its tegument distended with water.

   The observation suggested a comparison with the famous experiment of Chossat upon starving dogs. To compel the yeast to

> dwell in pure water would be to deprive it of nourishment; to submit it to a regimen of starvation would force it to devour itself. Pure yeast, steeped in creosoted distilled water, absolutely protected from air, disengages pure carbonic acid for a long time, producing alcohol, acetic acid, etc, and at the same time other compounds which it does not make when nourished upon sugar. It exhausts itself thus enormously, remains whole a long time, its tegument preserving its form and, having eliminated its content almost wholly, inverts cane sugar to the end. I thus demonstrated that notwithstanding the creosote, the yeast alters of itself, as does the milk.

The spontaneous alteration of milk and that of yeast seemed to me indisputable proof that neither milk nor yeast was a mixture of proximate principles, but that both of them contain, inherently, the living organized agent which is the cause of their spontaneous alteration, or that consequently, if the chalk liquefies fecula starch, it is because it contains that which can produce the necessary soluble ferment.

It was the experiment of starving the yeast which enabled me to complete the demonstration that the phenomenon called the fermentation of cane sugar by yeast was the digestion of the sugar by the zymas, the absorption of the digested (invert) sugar *by* the cellule, the decomposition of this sugar *in* the cellule being the result of the complex phenomenon of *assimilation,* followed necessarily by disassimilation and of elimination. The products eliminated were carbonic acid, alcohol, acetic acid, etc, the same as with man the products of disassimilation—urea, etc.—come from man and reunite in part in urine.

While I was thus experimenting to develop the consequences of the memoir of 1857 and discovered the *zythozymas* in the yeast, I also discovered *anthozymas* in flowers, *morozymas* in the white mulberry, and the *nefrozymas* of the kidneys in the urine as a product of the function of the kidneys, in order to demonstrate that as the moulds form and secrete their soluble ferment, plants and animals form theirs in their organs, and I shall demonstrate besides that the leucocytes of pus even produce a zymas in the pus.

The phenomenon called fermentation is then the phenomenon of nutrition, which is being accomplished in the ferment, in the cellule of the yeast, in the same manner as the phenomenon of nutrition is accomplished in the animal, and following the same mechanism by the same means. This was the fundamental idea of my memoir *Upon Fermentations by Organized Ferments* which dates from 1864.[9]

I will revert later, with details, to this work, which is fundamental. I mention it now only as a verification of the conception of Dumas of which mention has before been made; it was in that work that for the first time the word *zymas* is employed to designate the soluble ferment which yeast contains performed, distinguishing the soluble ferments as agents of a different order from the figured ferments and effecting transformations also of a different order.

For the history one should read, in the *Jahresbericht* of Heinrich Will for 1864, how this was regarded as new in Germany and was favourably appreciated.

It is difficult, however, to realize the resistance which was offered from many sources to the demonstration that the phenomenon of fermentation is a phenomenon of nutrition accomplishing itself in the ferment. It was simply because although Virchow had held that the cellules were living in a living organism, the conception of Bichat was more and more regarded as unacceptable and the hypothesis of the cellularists as unfounded.

Alfred Estor, who was interested in my researches, in giving an account of them in 1865, expressed himself as follows:

> "It is easy to perceive the tendencies of M. Béchamp; each cellule lives like a globule of yeast; each cellule should modify by use the materials of nutrition which surround it, and the general history of the phenomena of nutrition teaches us that these modifications are due to ferments. We know what emotion has welcomed the admirable works of Virchow upon cellular pathology; in the remarkable researches of the Montpellier professor there is to be found nothing less than the foundations of a cellular physiology."[10]

Seven years had passed since the publication of the memoir upon the inversion of cane sugar by moulds, when Estor delivered this judgement and when I wrote to Dumas the letter upon living agents which, in the milk, effect its spontaneous alteration and which, in the chalk, effect the liquefaction and fermentation of fecula starch. The year following I first named the microzymas in the *Comptes Rendus* of the Academy of Sciences to designate the ferments of the chalk.

It has been know since the time of Leuwenhoeck (17th century) that human saliva contains a great number of microscopic organisms long since recognised as vibrioniens, but which in a cleanly kept mouth I have found to be chiefly microzymas.

I supposed that, even as the "little bodies" inverted cane sugar in the experiments of 1857, these microzymas might be those which produced the salivary diastase of Miathe in the saliva. I interested Estor and Camille Saintpiere in this question, and in 1867 we addressed a note to the Academy, having this title: *On the Role of the Microscopic Organisms of the Mouth in Digestion in General, and Particularly in the Formation of the Salivary Diastase.* The note was sent for examination to a commission composed of Louget and Robin, who made no report, and the note was mentioned in the *Compte Rendu* in the following terms:

> "The conclusion of this work is that it is not by an alteration that the parotidian saliva becomes able to digest fecula, but by means of a zymas which the organisms of Leuwenhoeck secrete there, while nourishing themselves upon its materials." [11]

We demonstrated two facts, equally essential; that the buccal microzymas of man liquefy and saccharify the starch of fecula with rare energy; that the parotidian saliva of the dog or horse can also liquefy starch, but does not saccharify it, while such as has stayed upon the buccal organisms soon becomes as saccharifying as human saliva.

The short note inserted by the commissioners shows that they had no idea of a zymas produced as a function of a cellule, of a vibrionien, or of a microzyma, nor even of an organ. Here is an indisputable proof thereof: the pancreas was known and it was called an intestinal salivary gland.

Now Bernard and Berthelot, studying the pancreatic juice and isolating from it the soluble substance called *pancreatin,* never thought for a moment to compare it to the salivary diastase, although it possessed, to the same degree, the power of saccharifying the starch of fecula; that is, Bernard, contrary to the opinion of Longet and of

Mialhe, held that salivary diastase, according to the ideas of Liebig, was an animal matter in a condition of alteration.

The microzymas being discovered, the general demonstration was made that the soluble ferments were substances produced by a living organism, mould, yeast, geological microzyma, diverse flowers, a fruit, the kidneys, and the buccal microzymas. But these were only the preliminary researches, whereof the totality have, since 1867, enabled the microzymian theory of the living organism to be formulated.

After our joint experiment upon the buccal microzymas, I showed Estor an experiment in which a piece of muscle placed in fecula starch, after having liquefied it and commenced to make it ferment, caused bacteria to appear in it as they appeared in soured and clotted milk. He then became my collaborator in proving that that which was true of milk and meat was also true for all the parts of an animal. There has resulted from this, thanks to other collaborations and other researches subsequent to 1870, the microzymian theory of the living organism, the construction whereof is completed by the present work.

The new theory rests upon a collection of fundamental and new facts which may by ranged under the following heads:

1) *The verification of the old hypothesis of atmospheric germs and the ideas of Cagniard de Latour and Schwann regarding the nature of beer yeast.*
   i) Proof that the ferments are not the fruits of spontaneous generation.
   ii) Demonstration that the soluble ferments or zymas are not the products of some change of an albuminoid matter, but the physiological products of a living organism; in short, that the relation of a mould, of beer yeast or of a cellule and of a microzyma with the zymases is that of producer to a product.

2) *The distinguishing of chemical, i.e. not living, organic matters reduced to the condition of definite proximate principles from natural organic matters, such as they exist in animals and plants.*

   The proximate principles are naturally unalterable; they do not ferment even when (being creosoted) they are left in contact with a limited quantity of ordinary air, in water at a physiological temperature. On the other hand, *natural organic matters,* under the same conditions or absolutely protected from atmospheric germs, invariably alter and ferment.

3) *Demonstration that natural organic matters are spontaneously alterable, because they necessarily and inherently contain the agents of their spontaneous alteration.*

   That is, productions similar to those which I called "little bodies" in certain experiments upon sugared water, and "the living beings already developed," in the letter of 1865 to Dumas, and to which I gave the name of microzymas the following year, as being the smallest of ferments, often so small that they could only be seen under the strongest enlargements of the immersion objectives of Nachet, but which I had discovered to be the most powerful of ferments.

What does this similitude of form and of function mean? What was there in common between a microzyma proceeding from a germ of the air, a microzyma of the chalk, a microzyma of the milk, and those of natural organic matters?

Ever since 1870 all my efforts have been directed to its discovery. My joint researches with Estor, later those of Baltus, upon the source of pus; those of J. Béchamp upon the microzymas of the same animal at its various ages and my own, especially those upon milk, eggs and the blood, have led me to consider the microzymas not only as being living ferments and producers of zymases, like the moulds born in sugared water, but as belonging to a category of unsuspected living beings without analogy, whose origin is the same.

In fact, *first*, all these researches showed me these microzymas functioning like anatomical elements endowed with physiological and chemical activity in all the organs and humours of living organisms in a perfect state of health, preserved there morphologically alike and functionally different, *ab ovo et semine,* in all the tissues and cellules of the diverse anatomical systems, down to the anatomical element which I have called *microzymian molecular granulation.* And especially, they showed me that the cellule is not the simple vital unit that Virchow believed, because the cellule itself has microzymas as anatomical elements.

Secondly, the experiment showed me that in parts subtracted from the living animal, the microzymas, being no longer in their normal conditions of existence, produced therein chemical alterations, called fermentations, which inevitably led to tissue disorganizations, to the destruction of the cellules and to the setting free of their microzymas, which then, changing in form and function, could become vibrioniens by evolution, which they did whenever the conditions for this evolution were realized.

And, *thirdly,* I established that the vibrios, the bacteria which the anatomical microzymian elements had become, destroyed themselves, and that, with the aid of the oxygen of the air, under the conditions which I had realized, they were at last reduced to microzymas while the matters of the alteration, being oxidised, were transformed into water, carbonic acid, nitrogen, etc, i.e. they were restored to the mineral condition, so that of the natural organic matters and of their tissues and cellules there remained only *the microzymas.*

These microzymas, proceeding from the bacteria which the anatomical element microzymas had become, were identical, morphologically and functionally, with those of chalk, calcareous rocks, alluviums, water, arable or cultivated earths, or the dusts of the streets and the air. From these experiments, I argued that the microzymas of the chalk, etc, were the microzymas of the bacteria which the anatomical element microzymas of the living beings of the geological epochs had become!

We then have to consider:

1) The microzymas in their function as anatomical elements in the living and healthy organism; there they are the physiological and chemical agents of the transformations which take place during the process of nutrition.

2) Microzymas in natural organic matter abstracted from the living animal, or in the cadaver; there they are the agents of the changes which are ascertained to take place there, whether or not they undergo the vibrionien evolution—changes which lead to the destruction of the tissues and the cellules.

3) The microzymas of the bacteria which result from this evolution, which are essentially ferments productive of lactic acid, acetic acid, alcohol, etc, with sugar and fecula starch; these microzymas are also producers of zymases and are capable of again undergoing vibrionien evolution.

The microzymas being the anatomical elements of the organized being from its first lineaments in the ovule which will become the egg, I am able to assert that *the microzyma is at the commencement of all organization.* And the microzymas of the destroyed bacteria being also living, it follows that these microzymas are *the living end of all organization.* The microzymas are surely then living beings of a special category without analogue.

But that is not all. Estor and I demonstrated that in a condition of disease, the microzymas which have become morbid determine in the organism special changes, dependent upon the nature of the anatomical system, which lead alike to the disorganization of the tissues, to the destruction of the cellules and to their vibrionien evolution during life, so that the microzymas, living agents of all organization, are also the agents of disease and death under the influences which nosologists specify.

Finally, they are the agents of total destruction when the oxygen of the air intervenes. Like the indestructible atom or element in the Lavoisierian theory of matter, the microzymas, too, are physiologically imperishable.

From the experimental fact that the microzymas of the chalk and dusts of the air are only microzymas from bacteria which proceeded from the vibrionien evolution of the anatomical element microzymas, it follows that that which I have called *germs* in my verification of the old hypothesis of *germs of the air* are *not* pre-existent in the air, in the earth and in the waters, but are the *living remains* of organisms which have disappeared and been destroyed.[12]

The facts of the microzymian theory have legitimatized the genial conception of Bichat; that the only thing living in an organism is what he regarded as elementary tissues. Later, among cellularists, Virchow, following Gaudichaut, held that the cellule was the simple anatomical element from which proceeded the whole of a living being; but it is in vain that he contended that it is the vital unit, living *per se,* because every cellule, even that of beer yeast, *is transitory,* destroying itself spontaneously.

It is the microzyma which enables us to specify precisely wherein a tissue, a cellule is living; living *per se*—that is to say, autonomically, it is in truth the simple vital unit.

But the conception had none the less as a consequence the assertion that, in disease, it is the elementary tissues or the cellules which are affected.

Tissue and cellular physiology now being established in accordance with the prevision of Estor, it should result from this that tissue and cellular pathology are in reality microzymian pathology.

In disease, the cellules have been seen to change, to be altered and destroyed, and these facts have been noted. But if the cellule were the vital unit living *per se,* it would know neither destruction nor death, but only change. If then the cellule can be destroyed and die, while the microzyma can only change, it is because the microzyma is really living *per se*, and physiologically imperishable even in its own evolutions, for, physiologically, *nothing is the prey of death;* on the contrary, experience daily proves that everything is the prey of life, that is to say, of what can be nourished and can consume.

From the beginning of our researches, Estor and I have established the presence of microzymas in the vaccine matter, in syphilitic pus as in ordinary pus, and I have shown in pus (even laudable) the presence of a zymas. In diseases there is, then, a morbid evolution of some anatomical element which corresponds to a vicious functioning and to vibrionien evolution.

It is thus that in anthrax the morbid microzymas of the blood become the bacteria of Davaine, and the blood globules experience such remarkable changes. But even as the microzymas may become morbid, they may cease to be so. For instance, there is a leading observation of Davaine upon the non-transmissibility of anthrax even by inoculation; if the animal is in process of putrefaction, its blood can no longer communicate anthrax.

From this observation of Davaine, I draw the conclusion that normal air never contains morbid microzymas, or what used to be called germs of diseases and are now called microbes; maintaining, in accord with the old medical aphorism that *diseases are born of us and in us,* that no one has ever been able to communicate a characteristic disease of the nosological class (anthrax, smallpox, typhoid fever, cholera, plague, tuberculosis, hydrophobia, syphilis, etc.) by taking the germ in the air, but necessarily from a patient, at some particular moment. And within the limit of my own studies upon the silkworms I distinguished with care the parasitic diseases whereof the agent came from outside, such as the muscardine and the pebine, from constitutional diseases, such as the flacherie, which is microzymian.

I give in the postscript of this work the communication which I made to the Academy of Medicine on the 3rd May, 1870, upon *Les Microzymas, la Pathologie et la Therapeutique.* It will help to establish the date, and will show that the theory was then nearly complete. It was not inserted in the Bulletin of the Academy, but an able physician, who gave an account of it in the *Union Médicale* of Paris, remarked that had it come from Germany it would have been received with acclamation. But there was not at that time any question about the medical doctrines of Pasteur and I did not then have to defend the microzymas against the denials of that savant; it was otherwise some years later.

The foregoing exposition shows clearly the connection of the new facts of the microzymian theory with certain earlier facts of the same kind, ascending to Bichat and Macquer, who, in agreement with the science anterior to Lavoisier, recognized the spontaneous alterability of natural organic matters; and at length Spallanzani, who, to explain certain apparitions of organized beings ascribed to spontaneous generation, invoked the germs of the air. It has enabled me further to follow the connection of the successive discoveries of special facts which, since 1854, the commencement of these researches, have resulted in the discovery of the microzymas and to the demonstration that the blood is a flowing tissue.

It is important to remark that the microzymian theory is in no way the product of a system or of a conception *a priori,* nor is it the consequence of a desire to demonstrate that the conception of Bichat and the cellular theory are conformable to nature. In fact, it has had for a point of departure the solution of a problem of pure chemistry and the necessity of discovering the role of the moulds in the inversion of a solution of cane sugar exposed to the air. Then, from induction to induction, applying unceasingly the method of Lavoisier, and from the attentive study of the properties of the lowest organism, I ascended to the highest summits of physiological chemistry and of pathology to discover wherein vital organization consists.

But so fertile is this theory founded upon the nature of things, and which has as its base no gratuitous hypothesis, that after it had led me to discover the source of the zymases, the physiological theory of fermentations, the nature of what were called the germs of the air, it enabled me to understand what was true in the ideas of Bichat, Dumas, and in the cellular pathology of Virchow and what profound truths there are in the aphorisms of the old physicians.

The microzymian theory of the living organism is true because it agrees at the same time with these conceptions and with the three aphorisms which I have chosen as the epigraph to this first part of my preface.

Nothing is but what ought to be.

Nothing is created; nothing is lost.

Nothing is the prey of death;
all things are the prey of life.

NOTES

1. L. Pasteur, *Annales de Chimie et de Physique*, 3rd S. vol LVIII, p.371, note.
2. It is wrong to suppose that the word organic, in *organic* molecules, had the same meaning as in *organic matters* of modern chemists; this is so little the truth that Buffon admitted *organic* molecules to explain the crystallisation of marine salt or of others, purely mineral.
3. A study already made by Desmazieres, who regarded the globule of beer yeast as an infusoria under the name of *Mycoderma cerevisiae,* but which Turpin called a plant under the generic name of *Torula* and Kutzing under that of *Cryptococcus.*
4. [The translator bespeaks a painstaking attention by men of science, by philosophers and by philanthropists to the rest of this narrative; and to keep in mind the constant boastings by literature, by the press, and by men held as most eminent in science of our superiority over our fathers. Can that superiority be proved to exist elsewhere than in the arts of murder, and what pertains thereto?—*Trans.*]
5. [Here is the discovery and source of all that is true in the theory and practice of antisepsis; but it has been carried to absurd extremes by the dominant faction in medicine.—*Trans.*]
6. *Annales de Chimie et de Physique*, 3rd S., Vol. LIV, p.18 (1858).
7. I have called by the name of moulds the totality of the productions which have appeared in different experiments which I have diversified. Generally these moulds remained in the state of colourless mycelium, even in solutions to which arsenious acid and certain salts had been added. In others, the completely developed mould was green or gray and rarely red. In some experiments there were actual cellules, different both from yeast globules and from the ferment of the lees of wine. Generally, at the beginning of the experiment, there was a slight deposit before the appearance of the mycelian tubes; in some cases, the inversions were effected by the "isolated little bodies," *little bodies* which I had not known how to classify, but which I held to be organized and living because, like the mycelian moulds, they effected the inversion of the sugar even in a creosoted solution.
8. Letter to Dumas, *Annales de Chimie et de Physique*, 3rd S., Vol. VI, p.148 (1865).
9. *C.R*, vol. LVIII, p.601 (4 April, 1864).
10. Montpellier, *Le messager du Midi (*1865).
11. *C.R.*, Vol. LXIV, p.696 (1867).
12. [The "experimental fact" referred to in the text (the very highest form of all evidence which can be supplied by science) cuts away the entire fabric of the microbian theory of disease from its very foundation. Never having been other than a baseless *guess* on the part of Pasteur and of his followers, it was fittingly designated by Béchamp as "the greatest scientific silliness of the age." It and the other "experimental facts" learnedly elaborated by Professor Béchamp and his collaborators make patent the absurdity of all pretended prophylactics against disease save one, and casts all rational minds back to the one sure and only

protection—sound hygiene!

We are mocked by quarantines, vaccines, inoculations and other devices for "conveying" the products of labour into the pockets of official doctors. We are gulled by them to the full extent of our willingness to be gulled. The opponents of a truly rational medicine are many and powerful, as evidenced by the suppression for more than a generation of Béchamp's admirable discoveries beneath a conspiracy of silence, and these opponents of the art of healing are entrenched in nearly all medical schools, in richly endowed research institutes, in expensive manufactories of animal poisons for poisoning men and animals (under the ignorant belief that they are benefiting us), and in all medical officialdom! -*Trans.*]

AUTHOR'S PREFACE / 2

"The greatest disorder of the intellect is to
believe things because one wishes that they were so."

– *L. Pasteur*[1]

To understand how man's intelligence, arrested at the same stage that it was in the days of Aristarchus, could come to proscribe the microzymian theory of the living organization as it had proscribed the theory of the movement of the earth, it is necessary to know something of the prejudices with which man's intelligence in these latter days has been imbued.

The Lavoisierian theory of matter suggested to Bichat the idea that in organized beings, life is not connected merely with chemical compounds, but also with anatomical elements personally and autonomically living. This caused Fourcroy to say that plants are organized machines which formed the matters extracted from them, matters which Chevreul will call *definite proximate principles,* and which no instrument of art is able to imitate. Gerhardt in 1849 will say of them that they are the work of the *vital force.* It was in vain that Bethelot, therein recalling Lavoisier, will think to prove that the proximate principles are chemical compounds such as those whose synthesis he effected; all the legitimate consequences of the conception of Bichat were disregarded, even the notion that the cellule is personally living, and it was maintained that:

> "The proximate principles of plants and animals are bodies, definite or not, generally very complex, gaseous, liquid, or solid, constituting organized substance by reciprocal solution, viz.: the humours, and by special combination, the anatomical elements."[2]

'Reciprocal solution' and 'special combination'; vague expressions used to conceal a preconceived system, thanks to which it was only necessary to consider the proximate principles in a living organism as purely chemical matter.

The autonomous nature of the anatomical elements in the tissues being thus set aside, it was declared that the protoplasm of the botanist Hugo Mohl was *living, organized matter* (although not morphologically determined, that is to say, not structured), whence the entire organism would proceed. It was thus that a liquid, in which all the proximate principles were supposed to be in a state of perfect solution, such as was called *plasma* in the blood, was called organized, living, and could die.

This was going back beyond the hypothesis of organic molecules of Buffon to the old hypothesis of matter living by its nature and to that of an organization which would be only the most excellent modification of matter such as it was imagined to be in the epoch of phlogiston.

That is where science stood in 1857; seeing in animal membranes and tissues only nitrogenised matter. Let us consider the consequences of this mode of view.

In 1839, Fremy found that certain animal membranes could produce lactic acid with the sugar of milk, which Scheele had discovered in the whey of soured and clotted milk. Thereupon lactic fermentations were produced by treating solutions of the sugars with all sorts of animal membranes and tissues, with cream cheese or gluten, and at the same time with chalk used to saturate the lactic acid as it was produced.

Berthelot resumed these experiments from another point of view, without neglecting the formation of lactic acid, but extending it from mannite sugar to allied substances, even to glycerine. The memoir wherein, in 1857, the author explained the results of his researches is entitled *Sur la Fermentation Alcoholique,*[3] for it happened that in some cases the quantity of alcohol formed was greater than that of the lactic acid and other products which accompany them.

But whatever name may be given to the phenomenon, lactic or alcoholic fermentation, that which resulted from the experiments of Berthelot was that:

> "The cause of fermentation seems to reside in its chemical nature; that is to say, in the composition and not in the form of the nitrogenous bodies (cream

> cheese, yolk of egg, muscle, pancreas, liver, kidney, spleen, testicle, bladder, small and large intestines, lung, brain, hairy skin, blood, dried fibrin, dried yeast, gluten, gelatine) fit to play the part of a ferment, and in the successive changes which their composition undergoes."

On the whole, he was of opinion that:

> "The sugared body and the nitrogenised body are decomposed at the same time, exerting upon one another a reciprocal influence."

In short, it was spontaneous fermentation of materials in the presence of one another.

As to the chalk employed for calcic carbonate, it was supposed to be absolutely needed only in certain cases, for example for the fermentation of mannite; further, the calcic carbonate, besides maintaining the neutrality of the medium, had for its role:

> "to direct in a certain determined sense the decomposition of the nitrogenised body which provokes the fermentation."

So far as an explanation of the phenomenon went, Berthelot seemed to relate it to the saccharification of fecula by diastase, the decomposition of amygdalin by synaptase, called fermentation, or even the etherification of alcohol by sulphuric acid; in short, to connect it, as did Mitscherlich and Berzelius, with an action called *catalytic contact.*

Berthelot did not fail to have established by Robin, Montagne, and Dujardin, the disorganization of the tissues and the development of particular living beings (mucors and vibrios or bacteria). He does not explain their source, makes no mention of the molecular granulations, but, he asserts, "this development is in no way necessary to the success of my experiments."

I have endeavoured to give an idea of the very important work of Berthelot because it constitutes the greatest effort in opposition to the opinion of Cagniard de Latour. But from the same experiments, entirely contrary conclusions ought to be drawn.

In fact, the following year Pasteur, in a memoir upon lactic fermentation[4] of sugar, under the conditions of Berthelot's experiment, placed himself on the side of Schwann and asserted that the development of special living beings was the sole cause of the fermentations pointed out, but without paying any more attention to the molecular granulations that Berthelot had done, he had the merit to distinguish among the particular living beings that which he named *lactic yeast,* and which he regarded as being to lactic fermentation what beer yeast is to the alcoholic.

But of the development of these beings, especially of the lactic and alcoholic yeasts, what according to him, was the cause? He had the choice between two hypotheses; that of the germs of the air with Spallanzani and Schwann, and that of spontaneous generation; he chose the second, asserting that these beings were born spontaneously of the albuminoid matter of the nitrogenised matters. To prove this he made the two following experiments which are important to remember:

> "The lactic yeast is born spontaneously with as much facility as beer yeast wherever the conditions are favourable.
>
> Let there be, for example, first, water of sweetened yeast without addition, and, second, the same with the addition of chalk.
>
> In the clear solution of the first we have beer yeast and the alcoholic fermentation; in the solution to which chalk has been added it is lactic yeast and lactic fermentation which will be developed. The yeasts are born spontaneously of the albuminoid matter furnished by the soluble part of the yeast; the beer yeast because the water of the yeast is acid, the lactic yeast because the chalk makes the yeast neutral."

We can say then that Pasteur and Berthelot have proposed, each in his own way, the spontaneous alteration of nitrogenised matter under the conditions specified by Macquer, but while this alteration resulted in the spontaneous generation of the ferments according to Pasteur, Berthelot did not express his views upon the origin of the living beings developed.

As to the manner in which the lactic yeast acted, how did Pasteur understand it? Cagniard de Latour had said that the fermentation of the sugar was an effect of the vegetation of the yeast; Pasteur said of the lactic yeast that "its chemical action is correlative of its development and of its organization", which, though in other words, is the same thing and may be classed as an explanation by catalytic contact.

I have insisted thus strongly upon this earlier work of Pasteur upon fermentations for two reasons:

First, to firmly establish how vain had been the efforts of Schwann to establish the idea that there can be no spontaneous alteration of organic matters by fermentation without the presence of special living beings, and that in conformity with the hypothesis of the germs, these living beings were not the product of spontaneous generation.

Secondly, to show how in 1858 Pasteur, having remained a sponteparist with regard to these living beings and as to beer yeast and lactic yeast, held that these organic matters were spontaneously alterable. We shall see how some years later Pasteur will 'discover' all of a sudden that ferments are never born spontaneously, but always from these atmospheric germs which he had neglected; he will even 'discover' that albuminoid matter is not necessary for it. He will next pretend to demonstrate that without these germs all organic matter, without exception, even an entire cadaver, will remain unchanged indefinitely. First it will be useful to know certain parts and certain conclusions of his memoir upon the alcoholic fermentation of cane sugar by beer yeast in the year 1860.[5]

From this work it is first to be remembered that Pasteur in it again asserts the spontaneous generation of beer yeast and then the fact, absolutely new, that glycerine is among the products of fermentation, the same as in wine of vinous fermentation. He also discovered in it succinic acid, which had been long before discovered in it by Schmidt.

With regard to the chemical action of the cellule of beer yeast, it is equally correlative with its development and organization. He was, in fact, so certain that the yeast took no other part in the phenomenon that he laboured hard to prove that all the products of fermentation came from the sugar, which would be a physiological heresy if fermentation is a phenomenon of nutrition which is accomplished within the ferment.

It is thus that upon the interesting question of whether the cane sugar ferments directly, or if it is first inverted (as was the opinion of Dubrunfaut in agreement with the remark of Dumas, who had shown that for the equation of fermentation the concurrence of water with the cane sugar is necessary), Pasteur pronounced for direct fermentation, asserting that the inversion was consecutive to the formation of succinic acid.

Nevertheless he knew that I had demonstrated the inversion of the sugar by organized productions which are born in sugared water exposed to the air. None the less he wrote the following, which is typical: “I do not think that there is any special power in the globules of yeast to transform cane sugar into grape sugar.”[6]

He knew also that Berthelot had supposed that the reduction of the sugar into alcohol and carbonic acid was to be compared to the reduction of amygdaline by synaptase. He knew that Dumas had clearly stated that yeast, like an animal, could not be nourished only upon sugar; that for its normal life an appropriate albuminoid matter was needed. If he did nothing to elucidate these important questions it was because he was obsessed with the preconception that there is nothing in common between the organization and life of a cellule of yeast and that of an animal cellule. This was because he regarded it as certain that the ferments are living beings apart by destination, and that fermentations are individual phenomena. He asserted that a special ferment corresponds to each fermentation.

This state of mind and a remark suggested to Pasteur an experiment which Doctor E. Roux, wonderstruck, called an *“experiment a la Pasteur.”*

This memorable experiment had for its object the multiplication, that is to say, the vegetation with reproduction, of beer yeast in a sugared medium without the addition of some appropriate albuminoid matter. The remark which made him attempt it was as follows:

Pasteur had been greatly impressed by the results of my experiments regarding the inversion of cane sugar by the various productions which are developed in its aqueous solution, and especially by the fact that the addition of certain non-ammoniacal mineral salts had the effect of increasing the harvest of these productions while causing them to vary. Now the nitrogen necessary for the synthesis of the albuminoid matters of these moulds could only have been that of the air left in the flasks in contact with these sweetened liquors.

Pasteur repeated the experiments and was convinced not only that true ferments of many species were developed without the employment of albuminoid matters, but that these ferments had formed these matters by synthesis. Then he who had asserted that the ferments were spontaneously born from the albuminoid matters of the sugared media had to amend his former opinion.

Assuredly, no more than I, could Pasteur have seen the beer yeast appear under the conditions in which the experiments had been reduced to their simplest expression, in order to make more strikingly plain the evidence that there could be no question there of spontaneous generation.

He thought he would succeed better by adding to a solution of candied sugar the right tartrate of ammonia and for mineral salts the ashes of the yeast itself; he succeeded no better, then he added to the same mixture a lot of yeast, in the hope that the tartrate of ammonia and the sugar would form by copulation an albuminoid matter which would help the multiplication of the globules of yeast. There are two versions of the results of the experiments.

One, that of Roux, more or less agreeing with or imitated from an earlier one of Pasteur, is the following: "Pasteur," he said, "had seen carbonic acid set free, the yeast augmented ... he observed that *all the sugar had disappeared,* transferred into alcohol, carbonic acid, etc."[7]

The other, by Pasteur,[8] is very different from that. There was set free, in fact, carbonic acid, but in microscopic globules; some sugar had disappeared, but out of ten grams, 5.5 grams had not fermented: there was some alcohol, but only a very small quantity, sensible but not sufficient to weigh, etc. What then had become of the sugar that had disappeared? It had become lactic acid, which had furnished "an abundant crystallisation of lactate of lime"; in short, the fermentation instead of being alcoholic had been lactic!

Now for the explanation of the facts according to the microzymian theory:

Pasteur, having continued to neglect the hypothesis of germs, found that the situation of the beer yeast being extra-physiological, its globules had proliferated at the expense of the reserve of their content, so that the time soon arrived when these were *exhausted,* the new after the old, while infusoria and lactic yeast overspread the liquor. "The infusoria disappeared and the lactic yeast multiplied," said Pasteur. About a month later, the lactic yeast 'continuing to increase', the ferments were collected and weighed.

Pasteur gave his results as being "of the most rigorous exactness." I, however, assert that under the conditions of his experiment, the quantity of yeast collected *must* have been less than that of the yeast sown. Now, reflecting upon what he thought was an increase of the yeast and this production of lactic yeast, he has given this experiment "as illuminating with a new day the phenomena of fermentation."

This declaration is applicable to my experiments of the memoir of 1857, which are really demonstrative and which Pasteur has attempted to ascribe to himself while imitating after repeating them. In fact it was a plagiarism to the detriment of science.[9]

To complete the exposition of the state of the question in 1860, here is an experiment of Berthelot in the sense of mine. The author made a solution of gelatine, of glucose and of bicarbonate of potash, saturated it with carbonic acid, filtered it while warm in a still which he filled completely and left to itself. At the end of a greater or less time (some weeks) gas was set free and a good deal of alcohol was formed. At the same time a slight, insoluble deposit was formed "composed of an enormous number of molecular granulations, much smaller than beer yeast and very different in appearance."[10]

Berthelot did not ascribe any role to these molecular granulations, and believing that he had performed the experiment "protected from contact with air", he asserted, as in 1857, that the presence of calcic carbonate (the chalk) or of any alkaline bicarbonate directs the decomposition of the nitrogenised body (in this instance, the gelatine) in a certain manner which sets up the fermentation by regulating the steps of the phenomena. In short, Berthelot had not yet distinguished between the calcareous rocks (the chalk) and pure calcic carbonate, exactly like Pasteur in this matter, and did not yet believe that atmospheric germs had anything to do with the appearance of the molecular granulations. In short, he naturally believed that the lactic yeast of Pasteur was also constituted of molecular granulations, and that there was nothing to show that it was organized and living; as was the opinion of Pasteur, who, in 1858, stated that he had argued "*on the hypothesis* that the new yeast was organized and living."

This, then, was the state of knowledge in 1860, and even much later. It was not known, although it already stood out from the facts of my memoir of 1857, and which the microzymian theory has since confirmed, that that which characterizes the fact of a living organization is not essentially, as the naturalists of the schools still believe, the establishment of the existence of some organ or structure, nor is it the presence of movement more or less spontaneous or voluntary in any living being whatever, or such as a microzyma, molecular granulation or lactic yeast, or such as a vibrionien. Rather, living organization is characterized by the property of producing and secreting zymases, each according to its nature or species; and the production of the chemico-physiological phenomena of transformation called fermentation, which are acts of nutrition, that is to say, of digestion, followed by absorption, assimilation, disassimilation, and so forth, and finally, the ability to reproduce itself if all conditions dependent upon nutrition are fulfilled.

This is what Pasteur could not understand when he alleged in 1860 that the fermentation of cane sugar by beer yeast was correlative to the multiplication of the yeast, which is as great a physiological heresy as to imagine that an animal could be nourished upon sugar alone.

But soon after, Pasteur, who had not yet explicitly invoked the germs in explanation of the alterations of organic matters and the production of the alterations of organic matters and the production of ferments, would explain by them what he had before explained by spontaneous generation; in short, he held my verification of the hypothesis to be so rigorously correct that in 1862 he published a memoir against spontaneous generation, wherein the alteration of all organic matters was explained as Schwann had done, by applying his method as improved by Claude Bernard.

That was his second plagiarism.

His experiments in the memoir of 1852 had been made with the organic substances treated, cooked, for the purpose of killing the germs which the air might have deposited upon them. In 1863 he repeated them upon blood and flesh, not cooked, for the purpose of proving that they did not contain germs capable of becoming vibrios, and that, without atmospheric germs, they would be unalterable. Not being able to heat flesh in the same manner as blood, he applied my method, substituting alcohol in the place of creosote. That was a third plagiarism. But he could not see the vibrioniens which, in spite of the antiseptic agent, were developed in the depths of the flesh, and he concluded that neither the blood nor muscle became putrid because the germs of the air were absent from them. And he regarded as proven that there was nothing living in the blood or in the flesh, and that all animal matters, without the germs of the air, would remain indefinitely unchanged.

While Pasteur thus experimented, I continued to develop the consequences of my memoir of 1857. I demonstrated especially that not only were the atmospheric germs unnecessary for vinous fermentation, but that they were injurious, and that the grape carried normally, upon itself, the cellules of the ferments of the lees; not only the germs but the fully developed ferment. This was in 1864.

At last, in 1865, I announced to Dumas the fact of the existence in the milk, and in the chalk, of the agent which is the cause of the spontaneous alteration of the former and of that which enables the second to act as lactic ferment, agents to which in the following year I gave the name of microzymas.

Pasteur, who had been named a member of the commission[11] upon my memoir upon the ferment of the chalk, said not a word, and I continued with Estor the study of the microzymas of the higher organisms up to applications to pathology, as may be seen in the postface. This was in 1870.

In 1872 Pasteur attempted his boldest plagiarism; he discovered all of a sudden, eight years after my discovery thereof (I will state elsewhere on what occasion), that the ferment of vinous fermentation exists naturally upon the grape. In this connection he discovered also that plant and animal matters contain normally the things which cause them to alter spontaneously; that their cellules, without the atmospheric germs, are ferments. In other words, he repudiated his experiments and conclusions of 1862. He announced that his *'new discoveries'* would mark an epoch in general physiology; and he asserted that he had thrown a great light upon the phenomena of fermentation and had "opened a new path to physiology and medical pathology."

This was too much: up till that time I had treated the man with consideration; but now he must be properly exposed. First I, then Estor and I together, protested energetically. Our protests were inserted literally by Dumas and by Elie de Beaumont; the complete text can be read in the *Comptes Rendus*, Vol. LXXV, pp1284, 1519, 1523 and 1831. Pasteur replied by a subterfuge, to which we replied as follows: "We request the Academy to permit us to record that the observations inserted in the names of M. Béchamp and of ourselves remain unanswered".

Pasteur said no more, and abandoning "the new road" he pretended to have opened (a road which we showed we had not only opened but had sturdily traversed) he retraced his steps. Then, while since 1858 he had not disputed the meaning of any of the results, of any of the facts upon which the microzymian theory rests, results and facts which he knew to be exact and the discovery whereof he tried to ascribe to himself; then, I say, it was that he undertook in 1876 to explain them all by the atmospheric germs as he had "explained" them, in 1862, by spontaneous generation.

He first evoked his experiment upon the blood in 1863, and, doubtless because Estor and I, after the discovery of the microzymas of the fibrin, had not thought it worth criticizing, he qualified it as *famous(!)*, using it to deny even the existence of the microzymas. He then canvassed for approvers to maintain that uncooked milk, like the blood, is unalterable when preserved from contact with the natural air; that without atmospheric germs there would be neither fermentation nor disease, because there would be neither ferments nor *microbes*; for Pasteur, in spite of the inaccuracy of the etymology, had adopted this word with which to designate the micro-organisms.

In short, Pasteur, who understood what he was about in this matter, ended by causing belief that things were as he wished they were, which as he himself had said, "is the greatest derangement of the mind."

The strangest part of the business is that it was believed, and that he was able to make the Academies his accomplices.[12] It is true that he had at the same time organized the conspiracy of silence around the works related to the microzymian theory—so thoroughly, that one day, after a discussion during which I had attacked the principles of the microbian doctrines and had defended the microzymian theory, Cornil maintained that the discoveries of Pasteur had been verified in every country and that I was alone against all the would; to which I replied:

> "It is not because everybody thinks so that it is true. I have demonstrated in an already old communication that the protoplasmic system, false in its principles, is false also in its consequences. It is so likewise with the microbian doctrines. For the dignity of science and of human reason it is time that they were abandoned!" [13]

The discussion did not rest there. I will narrate the rest, which is most instructive, in *The History of the Microbian Doctrines*, to show the sort of respect which Pasteur had for truth.

It is true we have not been treated as was Galileo by the Inquisition, but Estor, painfully afflicted, wrote me this, which constitutes a grave witness against the spirit of these times:

> "We can publish letters from members of the Institute begging us in the name of our personal interest to proceed no further in the road opened (by us) ... but let them be convinced that energetic protests[14] will be directed wherever one may hope to find associated science and honesty."

That honourable and conscientious savant died of grief!

The microzymian theory has experienced in our days, as was the case formerly, the fate of all new truths which go counter to the habits, the passions, and the interests of those in power.

It is because man's reason, that is to say, that part of it which has become vacillating, without ballast, hypocritical and pharasaical, has remained the same as it was in the days of Aristarchus, of Socrates, of Galileo. It is that part of mankind which allows the plagiarist to calumniate and to vilify the victim whose work he has plagiarised.

NOTES

1. *C.R.*, Vol. LXXX, p.91 (1875).
2. *Dictionnaire de Médecine,* Littré and Robin, articles *Immédiat* and *Organique.*
3. *Annales de Chimie et de Physique,* 3rd S., Vol. 2, p.322.
4. *Annales de Chimie et de Physique*, 3rd S., Vol. LII, p.404.
5. *Annales de Chimie et de Physique*, 3rd S., Vol. LVIII, p.323.
6. *Loc. cit*., p.357. In relation to this, an observation is necessary. Some persons, not well informed, ascribed to Berthelot the discovery of the property of beer yeast to invert cane sugar. That savant had nothing to do with it. The following is the truth. In 1840, Mitscherlich discovered that the clear liquor obtained by leaving beer yeast to drain upon a filter possesses the property of converting cane sugar into uncrystallisable sugar, whereas the globules of the ferment, well washed with pure water, *are entirely deprived of this property.* And Berzelius added: "*The formation of the uncrystallisable sugar is not due to the globules of the ferment, but to a soluble matter in the water with which they are mixed*." Now, in 1860, Berthelot had simply confirmed the fact and isolated the soluble matter, whereof Berzelius spoke, but had not demonstrated that there was a special property of transformation of the cane sugar in the globules. That is what I demonstrated after having discovered that the moulds born in the sugared water without albuminoid matter possess individually the inverting power, and it was that which was needed to prove that the soluble ferment was not a product of the change. See *Les Microzymas,* pp.45-47, and *Memoir sur les Matieres Albuminoides,* p.352, for the complete history of the zythozymas.
7. *Revue Rose,* Vol. X, 4th S., p.834 (1898).
8. *Annales de Chimie et de Physique,* 3rd S., Vol. LVIII, pp.383-392 (1860).
9. (From p.42) Roux, evidently for the purpose of causing people to believe in the priority of Pasteur in this matter, has stated that the experiment was made in 1856, anterior to the publication of my memoir, while it really was of the 10th December, 1858, several months subsequent to the publication of the memoir, wherein Pasteur asserted that the ferments were the results of spontaneous generation from albuminoid matters (*prennent spontanément naissance des matieres albuminodies*), posterior by a year to the deposit of my memoir with the Academy of Sciences, published by extracts in the first *Compte Rendu* of 1858, and *in extenso* in the *Annales de Chimie et de Physique* in September of the same year. It was in the same spirit, that before that time, Roux had the audacity to write that "the medical work of Pasteur began with the study of fermentations" (*Agenda du chemiste* for 1896); this was an absolute untruth, for seven years later Pasteur had not yet attained to an elementary understanding about them; Roux either did not go to the original documents or he was anxious to contribute to the legend which attributes to Pasteur the discovery of the facts of the microzymian theory. That legend is a falsehood.

   [A further "illumination" is thrown upon this subject, so discreditable to science and its professed masters during the last quarter of the nineteenth century, in *Les Grands Problemes Médicaus*, Paris, 1905, pp12-13. The statements wherein can be verified by anyone possessed of a moderate knowledge of physiological chemistry, who will take the trouble to read, and to study, Section III of Pasteur's memoir to be found in the *Annalse de Chimie et de Physique,* 3rd Series, Vol. LVIII, pp381. On such study being made, the experiment there pretended to have been made will be seen to be A FAKE, purely and simply.—*Trans.*]

10. *Chimie Organique Fondée sur la Synthese*

11. [When a memoir is presented to the Academy which seems to be of more than usual importance, a commission is named composed of members reputed skilled in like studies to examine and report upon the memoir. It was of such a commission on the memoir of Béchamp upon the chalk ferments whereof Pasteur was appointed a member.—*Trans.*]

12. The following is typical in this respect. Pasteur had treated Fremy shamefully, because he had maintained that cream cheese produced lactic fermentation of itself. I said to him: "But show then to the Academy the microzymas of the milk and of the cream, which are the lactic ferment of Pasteur and you will confound him." "Ah," said he, "I should never dare to pronounce the word *microzyma* at the Academy." To such an extent indeed had Pasteur cunningly manoeuvered!

13. *Bulletin de l'Académie de Médecine,* 2nd S., Vol. XV, p.379 (1886).

14. [The translation of this work, and its publication, is one of the first of those *protests* which Estor foretold. It is hoped that it will mark the turning point of the followers of science from the wisdom of the "philosophers of Lilliput," in which so many of them have been wallowing—and, what is worse, training students of biology, physiology, pathology and medicine to mistake follies for wisdom! -*Trans.*]

AVANT PROPOS

The object of this work is the solution of a problem of the first order; to show the real nature of the blood, and to demonstrate the character of its organization. It has, besides, a secondary purpose; the solution of a problem long ago stated, but never solved—the cause of its coagulation, correctly regarded as spontaneous, after it has issued from the blood vessels.

The conclusion arrived at is that the blood is a flowing tissue, spontaneously alterable in the same manner as are all other tissues withdrawn from the animal, coagulation of the blood being only the first phase of its spontaneous change.

It would be too tedious to give even a summary of what had been written upon the blood before the discovery by Harvey and that of the blood globules; I will merely observe here that both before as well as after these memorable discoveries, the blood has been almost exclusively called a liquid by those physiologists who specially studied it. This will appear abundantly from the historical introduction, especially with regard to the attempts at explanation of the phenomena known as spontaneous coagulation.