SUMERIA

Selections from

The Third Element of the Blood

(Out of Print)

by *Antoine Bechamp*

This document consists of the following sections:

Chapters 1-8 are not included here. (They contain the details of Bechamp's experiments; the Author's Postface is initially more interesting, in that it is a summary of Bechamp's conclusions. If you want to cut to the chase, look there.)

PREFACE

This book is the last work by a man who should today be regarded as one of the founders of modern medicine and biology and who deserves a place as one of the giants of the history of science. History, however, is written by the winners; the career of Antoine Bechamp, and the manner in which both he and his work have been written out of history, bear witness to the truth of this statement.

During his long and distinguished career as an academic and a researcher in 19th century France, Antoine Bechamp was widely known as both a teacher and an innovator. His work was widely documented in scientific circles, and few made as much use of this fact as the now famous Louis Pasteur, who set about plagiarising and distorting Bechamp's ideas and discoveries, and in doing so gained for himself an undeserved and unwarranted place in the history of medical science.

There have been several excellent books written (mainly in the early decades of this century), which explain in detail the plagiarisms and accompanying injustices which Pasteur and others inflicted on Bechamp.

This present text, The Third Element of the Blood, is the injured party's own exposition of his position and his defence of it. It is a reworked translation of the last major work written by

Professor Bechamp, and as such it describes the culmination of his life's work, and shows clearly the importance that his work should have with regard to contemporary medicine and science.

This book contains, in great 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 Bechamp and those who worked with him to be without foundation.

The reader should be aware when reading The Third Element of the Blood that in formulating his microzymian theory of biological organisation, Bechamp 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. The Professor worked until a few weeks before his death; even if he were working now, he would no doubt still regard his work as unfinished.

It is no accident but rather a vindication of the truth of Bechamp's theories that many researchers over the course of the twentieth century have arrived at hypotheses and conclusions in various disciplines that concur with the microzymian model.

In the United States during the 1920s and 30s, Royal Rife's microscope revealed processes of life which would have made a great deal of sense to Bechamp. The medical establishment, however, was confounded 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 interests of a system which supports itself by maintaining a high level of sickness amongst humanity, and keeping health at a safe and lucrative distance.

To maintain the profits of the drug companies and the authority of the medical establishment, no price is too great, and by the time Rife died, his work was all but forgotten.

Another process, this time a more recent one called CanCell, is experiencing the same fate at the same hands. Using techniques which are very much a refinement and development of Rife's use of frequencies, Ed Sopcak has developed a process which has been tested and vindicated by the American FDA, who are now doing their best to bury it. Again, the danger of this technology seems to be that it works, against Aids and cancer as well as other diseases, and it is simple and cheap. For years, Sopcak has given CanCell away without charge to anyone who asked for it. Today he can no longer do so because of a groundless court injunction.

Many of the "alternative" ideas of medicine and biology that are currently under attack in various parts of the world would have no argument with the views of Bechamp. And if science had granted to Bechamp the position in history and the influence upon scientific thought that it instead allowed to fall into the hands of the charlatan and opportunist Louis Pasteur, modern science would in turn have no argument with those ideas which today are being suppressed at every turn, whether by law, propaganda, discrimination, or, as in the latest development, armed raids, confiscation of equipment, and the jailing of researchers and health practicioners.

One such unfortunate is Basil Wainright, an American responsible for a process known as polyatomic apheresis, an advanced form of oxygen therapy which has proven itself to be effective enough against Aids and cancer for it to be worth banning. At the time of writing, he has spent three years in prison without being charged with any offence, his medications for Parkinson's disease have been tampered with, and clinics using polyatomic apheresis have been raided and closed.

Similar stories could be told concerning many other products and practices, including Essiac and other herbal therapies, which should have been greeted with open arms, but have instead been marginalised by the establishment.

Among the many characteristics that these processes and theories have in common is the fact that the Germ Theory, that great and fallacious iconoclasm that Pasteur and his legions have cursed modern medical thinking with, plays no part in them. There is no hunt for the responsible bug, no expensive and complicated treatment for the sole cause of a disease.

The Germ Theory is convenient because it provides what every simplistic view of a problem seeks before all else: a culprit, an invisible hare for the hounds to chase in their costly research labs, universities, hospitals, and drug factories. The fact that the hare can never be caught is the perfect guarantee that their race will never finish, their demands for funding will never cease, and their ability to generate profits for the drug and chemical corporations will continue to grow.

There is no single cause of disease. The ancients thought this, Bechamp proved it and was written out of history for his trouble, and now the same thing is being done to those whose work, consciously or otherwise, carries on from where Bechamp's left off. The relevance of his work to the dilemmas that beset modern medical science remains as yet unrealised.

This book is being republished with the intention of being one small element in the movement that will correct that situation.

The original English edition of 1912, translated from the French by Dr M. Leverson, has until now been available only as a facsimile reproduction.

This new edition has been reset, in a new layout that it is hoped will make the content much 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.

** End of Publisher's Preface **

1. TRANSLATOR'S PREFACE

On the 16th October, 1816, at Bassing, in the department of Bas-Rhein, (France, since ceded to Germany), was born a child by whose name the nineteenth century will come to be known, as are the centuries of Copernicus, of Galileo and of Newton by their several names. Antoine Bechamp, 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 Bechamp and his collaborators. The American physician made his visit to Paris for the purpose of becoming personally acquainted with Professor Bechamp, who, as his family stated, had looked forward with eager anticipation to such a visit.

The translator had long previously submitted to the professor an extensive summary of his physiological and biological discoveries, and by him 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 Bechamp 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 Bechamp, especially as, in my opinion, it would not be easy to carry out among them that "conspiracy of silence" by means of which the discoveries of Bechamp had been buried in favour of distorted plagiarisms of his labours which had been productive of abortions, such as the Microbian or Germ Theory of disease, "the greatest scientific silliness of the age," as it has been correctly styled by the Professor.

To this suggestion Professor Bechamp 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 of pathology, discovered by him, and authorized me freely to publish either summaries or translations into English, as I might deem most advisable.

In pursuance 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 Bechamp.

The subject of the work is described by its title, but it is well to remind the medical 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 was dead, though his evil work still lives.

One of the discoveries of Bechamp was the formation of urea by the oxidation of albuminoid matters. The fact, novel at the time, was hotly disputed, but is now definitely settled in accordance with Bechamp'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 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 fractionized into numerous defined species albuminoid matters described theretofore 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 received chemical theory, he demonstrated that the alcoholic fermentation of beer-yeast was of the same order as the phenomena which characterize the regular performance of an act of animal life - digestion.

In 1856 he showed that moulds 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.

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.

These moulds, under the microscope, are seen to be formed by a collection of molecular granulations which Bechamp named microzymas. Not found in pure calcic carbonate, they are found in geological calcareous strata, and Bechamp 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.

Bechamp 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 Bechamp, 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 Bechamp 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, when his "operations were successful and his patients lived."

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

Bechamp 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 Bechamp's discoveries, be to-day 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.

Bechamp 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 pebrine, which is due to a parasite; 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 Bechamp'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 labors and discoveries of Bechamp, of which the work now translated was the crowning glory.

The present work describes the latest of all the admirable biological discoveries of the Professor Bechamp. 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; New Researches upon the Albuminoids, also complete, etc., etc.

The study of these and of the other discoveries of Professor Bechamp 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 suffered itself to be led astray into the microbian theory of disease, which, as before mentioned, was declared by Bechamp to be the "greatest scientific silliness of the age."

Ainsi Soit-il!

Montague R. Leverson London, 1911

** End of the Translator's Preface **

2. AUTHOR'S PREFACE, Part 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 lately related that Cleanthus, a philosopher, three centuries before our era, wished to prosecute Aristarchus for blasphemy, for having believed that the earth moved, and dared to say the sun was the immovable center of the universe; that two thousand years later, human reason having remained stationary, the wish of Cleanthus was realized; Galileo having been accused of blasphemy and impiety for having, like Copernicus and following Aristarchus, maintained the same truth; "a tribunal dreaded by all, 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" upon a given by experiment alone.

It is interesting to know if 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 labor disinterestedly for the triumph of the truth, if, in short, those who, for the large public, are as authoritative judges of the value of the discoveries of others, have become less intolerant, or, at least, more impartial, less prompt to pronounce against opinions which they do not share, less anxious to deny facts than to test them. And if the lesson has not been learned, it is not less interesting to examine if it is "human reason" which must be held responsible; if it be not rather "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 century now closing (the 19th), is well adapted to show that human nature has not changed since the time of Cleanthus, and that there always exist persons 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 as the crown to a collection of works upon ferments and fermentation, upon spontaneous generation, upon albuminoid substances, upon organization, upon 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, of physiological, of 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 organisation, which has led to the discovery of the true nature of blood by that of its third anatomical element, and, at least, 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 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."

That is very well and very 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 Aristotlelian 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 admitted. Charles Bonnet himself said that organization was the most excellent modification of matter; nevertheless that learned naturalist and philosopher attempted to opposed 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, member of the Royal Society of London. On the other hand, to sustain Needham, Buffon invented the hypotheses 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.

Fermentations and ferments were very simply explained. Macquer in 1772, in agreement with the savants, regarded it as certain that vegetable and animal matters, abstracted from living organisms, under certain conditions of the presence of water, of contact, at least momentarily, of the air and of temperature, become altered of themselves, 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, 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 various proportions which carbon, hydrogen, oxygen, 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 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 that of 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 and that of 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, of water and of 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 be 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:

sugar = carbonic acid + 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 nitrogenized body, and 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 labors 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, whereof 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."

What marvelous and novel language! It is true that, a chemist rallying to the new theory, Fourcroy, like Bichat, was a physician.

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 of 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 (say), about 1856.

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

Among the nitrogenized proximate principles a number of them were distinguished as soluble or insoluble, also uncrystallizable, 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 process of 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 nitrogenized animal matters.

Now after the discovery of gluten, of vegetable albumen, nitrogenized quaternaries like beer yeast, it was admitted that they were the ferment of vinous fermentation; then generalizing 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 about 1836, Cagniard de Latour, resuming the study of beer yeast 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. In effect it is not because of his having regarded beer yeast as organized 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! Yeast ceased to be regarded as the reagent that Lavoisier had admitted as able to disturb the equilibrium of the simple bodies which constituted sugar.

Also, soon after wards, 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 who feed, and that, for the orderly maintenance of the life of the yeast, there was needed, as for animals, besides sugar, nitrogenized albuminoid matter.

In Germany, Schwann pronounce for 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 several others.

But the conception of Cagniard de Latour did not prevail, nor especially the interpretation of Turpin and of 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 favor either of spontaneous generation or of the production of these living products by the germs of the air.

The discovery of diastase and of synapse, soluble and nitrogenized quaternaries like yeast, was held to legitimize the refusal to consider yeast as acting because it was organized and living. Now these substances being 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, you see 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 of 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 admitted that vegetable and animal matters altered of themselves under the conditions specified by Macquer, but also the proximate principles extracted from them, even cane sugar, the aque ous 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. 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 polarization to the right deviated it to the left after the alteration. 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, 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 admitted 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 of 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 mad various experiments, commencing in 1855 and continued them 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: 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 colorless moulds, generally myceliennated, and the solution becomes completely inverted in the course of time, while the liquor reddens litmus paper, that is to say, 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 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 demonstrated to me clearly 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 nitrogenized 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 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 nitrogenized, the moulds, when heated with caustic potash, set free an abundance of ammonia.

But these moulds being nitrogenized could not be born of the cane sugar, which I have proven to be exempt from nitrogen. Now 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 then remained to explain the birth of the organized productions than the old hypothesis of germs; pretended 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 favorable medium for their development"; a development during which the new organism, making use of the materials present, effects the synthesis of the nitrogenized and non-nitrogenized 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 whereof 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 galleon 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 to 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:

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.

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

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

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.

Developed moulds invert the cane sugar, even when the solution has first been creosoted; that is to say, 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.

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

And 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; then 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 in virtue of its physiological functions of development and nutrition.

Thence 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 nitrogenized, 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 nitrogenized substance of plants; which up to that time 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 there produced 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 assimilation, dissimulation, and excretion of the products dissimulated.

Without doubt these views were in conformity with the conceptions of Canard 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, others that it acted in so far as it was nourished, by an action of extalyic contact, an occult cause, that it effected the decomposition of sugar in the same manner as 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 of 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; that consequently, if in the mixture of sugared water and of 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 therein 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, of itself, 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. At last, 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 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. Now, one day, some starch made of potato facula had some chalk added to it to prevent it turning sour and was left in an oven at 4 to 45 degrees C.(= 104 to 113 degrees F.). I expected to find the starch with the same consistency as before; on the contrary, it was liquefied. "The germs of the air," said I. I repeated the experiment, creosoting the boiling starch and added some of the same chalk; again liquification! Much astonished I repeated the experiment, replacing the chalk with pure artificial calcic carbonate; this time the creosoted starch was not liquified, 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, wherein I stated that some of the calcareous earthy 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, so that the product of the liquefaction being thrown upon a filter, if the operation is performed on a sufficiently large quantity, permits 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.

This observation suggested the making upon yeast 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 organised 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 being 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, 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 leukocytes 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.

I will revert later, with details, to this work, which also 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 simple because, although M.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. Bechamp; 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."

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 J.B.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 is peopled with a great number of microscopic organisms long since recognized 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."

We demonstrated two facts equally essential, viz., that the buccal microzymas of man liquify and saccharify the starch of fecula with rare energy; that the parotidian saliva of the dog or horse can also liquify 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 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 possessing, to the same degree, the power of saccharifying the starch of fecula; that is that Bernard, contrary to the opinion of Longet and of Mialhe, held the salivary diastase, according to the ideas of Liebig, to be an animal matter in a condition of alteration, etc.

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 preliminaries of the 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 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 of the ideas of Cagniard de Latour and of Schwann regarding the nature of beer yeast.

   Proof that the ferments are not the fruits of spontaneous generation.

   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 organic matters reduced to the condition of definite proximate principles (that is to say, of the organic matter of the chemists, which is not living) from natural organic matters, such as they exist in animals and plants; that is to say, of the organic matter of physiologists and of anatomists which is reputed living or as having lived. The proximate principles are naturally unalterable, 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 like 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, viz.: 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. Bechamp upon the microzymas of the same animal at its various ages and my own, especially those upon milk, upon eggs and upon the blood, have led me to consider the microzymas not only as being living ferments 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:

On the one hand, all these researches showed me these microzymas functioning like anatomical elements endowed with physiological and chemical activity in all the organs and humors 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.

On the other hand, 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, in the third place, 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 oxidized, were transformed into water, carbonic acid, nitrogen, etc.; that is to say, restored to the mineral condition, so that of the natural organic matters and of their tissues and cellules there remained only the microzymas. And these microzymas, proceeding from the bacteria which the anatomical element microzymas had become, were identical, morphologically and functionally, with those of the chalk, of the calcareous rocks, of the alluviums, of the waters, of arable or cultivated earths, or the dusts of the streets and of 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; they are there the agents of the changes which are ascertained to take place there, whether or not they undergo the vibrionien evolution; changes which go on to the destruction of the tissues and of 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 capable of again undergoing vibrionien evolution.

Whence, 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.

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. Now tissue and cellular physiology being established in accordance with the prevision of Estor, it should result therefrom that tissue and cellular pathology are in reality microzymian pathology. In disease the cellules have been seen to change, 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 the 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 be 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, what used to be called germs of diseases and now 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 the 3rd May, 1870, upon Les Microzymas, la Pathologie et la therapeutique. It will help to establish a 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 Medicale 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, recognised the spontaneous alterability of natural organic matters; and at length Spallanzane, 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 for 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, 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 a theory founded upon the nature of things, at the base whereof there is 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 genial conceptions of Bichat, of Dumas, 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.

** End of the Author's Preface, Part 1 **

3. AUTHOR'S PREFACE, Part 2

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

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,ÿbut 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 said 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."

"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 lacticÿacid 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, withoutÿneglecting the formation of lactic acid, but extending it from annite 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, 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;ÿthat 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 ÿof 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 yeastÿand the alcoholic fermentation; in the solution to which chalk as 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,ÿheld 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.

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 sugarÿferments 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."

He knew also that Berthelot had supposed that the reduction of the sugarÿinto 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 Dr. 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 whichÿare developed in its aqueous solution, and especially by the act 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."

The other, by Pasteur, is very different from that. There was set free,ÿin 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.

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,ÿsaturated 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."

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 calcicÿcarbonate (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 productionÿof 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 e 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 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 elsewhereÿon 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 Estorÿand 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. Bechamp and of ourselves remain unanswered.".

Pasteur said no more, and abandoning "the new road" he pretended to haveÿopened (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 acts 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,ÿhad 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 has said, "is the greatest derangement of the mind."

The strangest part of the business is that it was believed, and that the was able to make the Academies his accomplices. 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!"

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 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.

** End of Author's Preface, Part 2 **

4. INTRODUCTORY AND HISTORICAL

The explanation of the fact of the coagulation of the blood, rightly regarded as spontaneous, has been sought for by physiologists, by physicians and by chemists, but without satisfactory result. The detailed history of the attempts at explanation would only demonstrate the uselessness of the preconceived hypotheses and systems on which they rest. Among all these hypotheses, only one deserves attention, precisely the one which the latest investigators have neglected to consider or to verify. The history of the conception of this hypothesis is of great interest.

From time immemorial, it was known that shed blood soon becomes a concrete mass, red, of a consistency more or less soft and called a clot; the phenomenon was otherwise compared to the coagulation of a homogeneous liquid.

It was not until the 18th century that Haller (in the supplement to the article "Blood," in the Encyclopaedia of Diderot), after correcting some errors of Leuwenhoeck concerning blood globules, asserted absolutely that they were essential elements of the blood, existing only in the red part, and, said he, "perhaps also in milk."

But he recognised that "the figure of the blood globules is constant and that they are not merely a collection of fatty grains ... but circumscribed, bounded and solid."

Haller also first placed the spontaneous coagulation of the blood on its true ground (tracing the theory back to Aristotle):

"An element of the blood, generally so regarded by the ancients, especially by Aristotle, are the fibres which the scholiasts regarded as the foundation of the coagulable matter of the blood; these fibres have been seen in the clot-cake which the blood, left to itself, never fails to form, and which seems to be really a sort of network made of small membranes, which can be separated from the fluid part and can then be plainly seen."

But Haller did not admit that the fibres were really an element of the blood. He said:

"If the authors wish us to understand that these fibres are in the blood as are the globules, they are certainly in error."

In support of his view he cited Borelli, the mathematician, who had been the first to refuse to admit "the fibres among the elements of the blood, as also Boerhave and other great men who have followed him," adding further:

"If the authors wish to say that under certain circumstances fibres and flakes are born in the blood, he did not object thereto, "but observed that these fibres and flakes seemed to have their birth rather in the lymph than in the red particles of the blood. In short, according to Haller, the blood contained nothing solid and figured but the globules in a liquid called lymph, adding that he had recommended as a good way of rendering the globules visible the addition of certain salts to the blood which increased the fluidity and the colour; "nitre being of all salts that which gives the best colour to the blood."

Haller,who derived the fibres of the clot from the lymph of the blood, was the precursor of the savants, who, like him, saw in the blood only globules in suspension in a liquid where everything else was supposed to be in a state of perfect solution.

The circumstances of the formation of the clot, its shape depending on that of the vessel in which it was formed, its progressive contraction and expulsion of the yellow serosity, thence called the serum, were all observed with attentive curiosity. The blood having finished its contraction, the washings in water which dissolved its colouring matter furnished the white matter which was called the fibrous portion of the blood, and, after the reform of chemical nomenclature, fibrin. The fibrin was finally isolated from the blood by whipping, before it coagulated. The great German physiologist, J. Muller, believed with Haller: he wrote:

"By liquor of the blood (liquor, lympha sanguinis) we mean the colourless liquid, such as exists before coagulation, in which the blood globules swim . . . it contains all that is really dissolved in the blood. At the moment of coagulation, the liquor separates itself from the fibrin which had before been dissolved"; and from his microscopic observations upon frog's blood, he thought that his researches "proved that besides the albumen, the fibrin was dissolved in the liquor of the blood."

H.H. Schultze gave the name of plasma to the lymph of Haller, which Muller had called liquor saunguinis.

The conclusion of J. Muller was the more circumspect, seeing that it was a refutation or contradiction of another mode of considering it, already published. W. Hewson had expressed two views, one of which had agreed with that of Muller; the other was original. According to the former, the fibrin exists in the blood in a state of solution; according to the other, it exists in it in suspension in a state of fine granulations; he further admitted that the globules did not contain fibrin.

Milne-Edwards accepted the second opinion of Hew son, maintaining that the fibrin did not exist in solution in the blood, but in a finely divided state as a solid, under the form of fine granulations, which after the blood has been shed and left at rest, united together in the form of the fibres of the clot, or by whipping, to form fibrin.

Dumas, who, with Prevost of Geneva, had first admitted the globular origin of the fibrin to explain coagulation, afterwards accepted, to a certain extent, the opinion of Milne-Edwards.

It is important to explain the point of view of such a genius. He said:

"None of the properties of fibrin give us the means of explaining the state in which it exists in the blood. It has not been possible to bring back the fibrin to this condition by any known process. In fact, the blood contains the fibrin, both liquid and spontaneously coagulable ... Everything leads to the belief that this fibrin of the blood is not in solution in it, but that it exists there in a finely divided state, which it maintains so long as the liquid is in motion, but which, in the liquid at rest, stops all of a sudden as a consequence of the disposition of the fibrin particles to unite in a fibrous and membranous network."

Later he modified this view as follows:

"The blood contains a quantity of spontaneously coagulable fibrin in suspension, or in a state so closely approaching solution, that it seems to be really dissolved in it; it is found there in a peculiar flowing state, analogous to that presented by starch mixed with water in the aqueous solution of starch."

But neither of the views of Hewson, nor that of Milne- Edwards, nor that of the illustrious Dumas regarding the individual state of the fibrin in the blood, which, as will be seen, were the nearest to the truth, received much consideration, and were soon lost to view. Physiologists reverted more and more to the view of Haller adopted by J. Muller and by Schultze. The word "plasma" prevailed over lymph, and it was held that everything except the globules were in a state of complete solution in the blood. They came at last to believe that the blood did not contain fibrin even in a state of solution.

In short, the fibrin which was called the "corps de delit" of the coagulation of the blood was imagined in turn to be the same substance as albumen; and it was further imagined that:

- the albumen of the blood was none other than the fibrin combined with the alkali of the blood, only the part not so combined being coagulable;

- that the plasma contained plasmine, which, when out of the vessels, transformed itself by spontaneous decomposition into concrete fibrin and into dissolved fibrin, called also metalbumen;

- that the fibrin does not exist either in the blood or in the plasma;

but that they contain, in solution, substances called fibrinogen and fibrinoplastin, respectively, which, outside of the vessels, under the influence of a ferment, produced the fibrin with an elimination of alkali, etc.

Chemists agreeing with Thenard came to look upon fibrin as an isolated animal matter, that is to say, a "proximate principle," according to the definition of Chevreul. Glenard, who paid a great deal of attention to the phenomenon of the coagulation of the blood and its causes, wrote upon the subject of fibrin, as follows: "Science has not yet been able to establish the constitution of fibrin, of the "corps de delit" of coagulation; it is not known whether it be derived from albumen, or should be regarded as one of its stages; and the formula of this substance varies with each chemist; it is not known whether it is superfluous (recrementitious) matter, or a product of excretion, a nutriment or an organic waste.

It is, therefore, a legitimate conclusion that after a century of hypothesis on hypothesis, we have gotten back to the point where Haller had left the question. Having neglected the conception of Milne-Edwards and of Dumas, as well as some researches which seemed to be approximately a verification thereof, it is not a matter for surprise that scientists who understand neither the real nature of fibrin nor its origin, had recourse to occult causes for the explanation of the phenomenon of coagulation.

The celebrated English Surgeon, Hunter, thought that:

"blood coagulated by virtue of an impression, that is to say, that its fluidity being inopportune or no longer necessary in its state of rest after issuing from the vessels, it coagulates in reply to the indispensable customs of solidity"; also he said that "the blood possesses in itself the force, by virtue whereof it acts in conformity with the stimulus of necessity, a necessity which is derived from the position in which it finds itself."

And Hunter wrote in the time of Haller.

Long after, Henle, having said that the cause of the coagulation of the blood directly after circulation ceased was unknown, added:

"Coagulation is often regarded as the last act of life, as the death of the blood."

This point of view, which was not that of Henle, has been lately revived and fitted into the system signified by the word plasma. In short, the following propositions can be collected from a work full of interesting observations on the coagulation of the blood:

"The blood is endowed with a life of its own."

"Coagulation is a synonym for the death of the blood ."

"By the fact of spontaneous coagulation, the plasma loses its chief property, that of living, and from the state of an organised humor becomes an inert aggregate of proximate principles."

"Coagulation then is the disorganisation of the plasma."

"It is the fact of this organisation which struggles for some minutes against the fatal influence upon the shed blood of contact with foreign bodies."

Right here, before going further, will be the place to seek for the substance beneath the mask of words.

It is true that the author of the above propositions did not, like Hunter, invoke "an impression" or `the indispensable customs of solidity" nor "the stimulus of necessity" to explain the phenomenon of the spontaneous coagulation of the blood, but has he escaped the shoals of ``occult causes ?"

It is true that blood as it issues from a living body is alive. But is it not an "explanation" by the occult causes to say that the blood coagulates because it dies?

But if the chief property of the plasma, an organised humor, is to live, is not the struggle of its organisation against the fatal influence of contact, the loss of its life, also an "explanation" by occult causes?

Also the plasma being an aqueous liquid, in which the materials composing it cannot be other than proximate principles, are by the hypothesis and by definition in a state of perfect solution, is it not an explanation by occult causes to say that the cause of its spontaneous coagulation is its disorganisation, etc.?

And what is the value of explanations by occult causes? Here is the answer given to this question by Newton: "To say that each species of things is endowed with a specific occult quality, by means whereof it has a certain power of action, and can produce sensible effects, is to say nothing at all."

Nevertheless, if in 1875 the author (M. Glenard) was reduced to the extremity of seeking an explanation of the phenomenon in considerations outside of anatomy, physiology and chemistry, it was because the then state of science did not offer anything more satisfactory. There are to be found in the transactions of the Academy of Sciences of the same year, attempts at explanation which compared the so called coagulation of milk to that of the blood.

Still later, M. Frey, returning to the methods of Muller and of Haller, said:

"Studied from the anatomical point of view the blood offers for our consideration a transparent colourless liquid, the plasma or liquor sanguinis, wherein float two kinds of cellular elements, the coloured cellules or red globules and the colourless cellules or lymphatic globules."

And as regards the fibrin, he says:

"It is not known under what form it exists in the liquids of the organism before coagulation, and it is generally supposed to be a derivative of albumen."

That amounts to saying that the red globules and the leukocytes are the only figured elements of the blood, and that the plasma holds the materials composing it in perfect solution, as Muller thought he had demonstrated for his liquor sanguinis, these materials being reducible from the organic point of view to albumen.

Further, Frey so thoroughly believed this that he said:

"The rapid nutritive exchanges which are produced in the nutrient liquid of the organism hinder the formation of fibrin during life."

All of which amounts to saying that at the moment of shedding the blood does not contain fibrin.

And here it may be observed that neither Haller nor Muller had any prejudices on the subject of the innate nature of the lymph or liquor of the blood. On the other hand, when plasma is made a synonym for "liquor sanguinis" the question is prejudged, for the synonym plasma is attached to a particular conception of organisation and of life, in conformity to the system which asserts that: "Life is a special form of the activity of matter," a system which differs greatly from the doctrines of Bichat, according to which life is not attached directly to matter, but to anatomical elements limited as to their form and structure. On this I shall insist further in explaining anatomically and physiologically the spontaneous coagulation of the blood.

But several years before GLenard and Frey wrote, Bechamp and Estor had demonstrated that the blood contains, besides the two species of globules, a third figured element, clearly determined in form and properties, by means whereof the phenomenon of coagulation could be explained without any recourse to occult causes.

In his thesis Glenard referred to our researches in these words:

"For reasons which we shall not fail to develop in a later work, we suppress a chapter having for title,`Theory of Bechamp and Estor on the Microzymas.' "

I do not know whether Glenard has anywhere developed his reasons for suppressing the above entitled chapter from his thesis. For my part I had the great sorrow of not being able to continue and complete with Estor the work we had commenced together. A separation which occurred in 1876, and then the so premature death of Estor, deprived me of my eminent collaborator and devoted friend; I had to pursue alone the complete solution of the problem. My latest researches have been carried on in the laboratory which M. Friedel provided me with at the Sorbonne.

The partial results of my researches have been described in notes which have appeared in various magazines; the last, in 1895, was in the form of a communication to the Congress of the French Association for the Advancement of Science held at Bordeaux; but several portions, and that especially which is the crown and keystone of the work, remained unpublished until the appearance of the present work.

The discovery of the third figured element of the blood was not made during the investigation of the phenomenon of the spontaneous coagulation of the blood; but Estor and I applied it according to the ideas then prevailing to the production of fibrin after phlebotomy, to explain the formation of the clot. When I resumed my study of fibrin from the point of view of blood-coagulation, I had already solved the problem of the coagulation of milk in a sense very different from received ideas, and this was long before the publication of the thesis of Glenard, who said:

"Not only are we ignorant of the first cause of coagulation, but we do not even know its proximate cause; we do not know whether this change in the state of the blood is a physical or chemical phenomenon; whether it is a crystallisation or a precipitation."

Unless I am much mistaken, that implies that the author doubted even what Haller, and, later, Muller, Hewson, Milne-Edwards and Dumas held as certain, viz., that the formation of the clot had the fibrin for its direct and near cause. As to the assertion that coagulation is a variation of the state of the blood, etc.; it proves that its author did not know either the anatomical or chemical constitution of the blood any more than that of milk.

In our note of 1869, the microzymas of the blood were expressly mentioned as being the first cause of the production of fibrin and the proximate cause of coagulation. My new researches further demonstrated that the presence of the microzymas and that of the fibrin in the blood are correlative, the one presupposing the other; it was only necessary to explain this correlation to verify, while completing the conception of Milne-Edwards developed by Dumas.

These new researches were allied to others, both older and newer, regarding the determination of the causes of the changes, reputed spontaneous, of organic matters, even of proximate principles in general, and specially of natural vegetable and animal matters, viz.:

1. The question of the origin of ferments and the physiological theory of fermentation.

2. The resolving in the negative the problem of the supposed spontaneous generation of ferments.

3. The origin of urea in the organism during the act of respiration.

4. The chemical constitution of albuminoid matters and the demonstration of the definite specificity of their chemical molecules.

5. The true theory of organisation according to the doctrine of Bichat.

It is thus seen that the complete solution of the problem concerning the spontaneous coagulation of the blood necessitated the previous solution of several other problems very difficult of solution; they are given here nearly in their chronological order.

1. The nature of fibrin, isolated from the clot, or obtained by whipping.

2. The real specific individuality of the albuminoid proximate principles.

3. The state of the fibrin in the blood at the moment of shedding.

4. The real structure of the red globules of the blood.

5. The real constitution of the blood at the moment of shedding.

6. The real chemical and physiological meaning of the coagulation of shed blood.

These will be the captions of the following chapters.

After the developments which are to follow, it will be possible to understand that what is called the phenomenon of the spontaneous coagulation of blood is not at all a coagulation of the blood itself, but of that of a portion of its third anatomical element.

It will then clearly appear, that that which is improperly called a coagulation is only the first phase of a much more complete alteration of the blood, involving the destruction of its blood globules and other changes, even that of its red colouring matter; and further, that this spontaneous alteration of the blood is but a special case of a very general phenomenon, that of the spontaneous alterability of all animal matter, solid or humoral, abstracted from an animal, whether living or dead; an alterability, physiologically spontaneous, necessary, drawing with it the destruction even of the cellular anatomical elements themselves, as the consequence of phenomena of fermentations of a special kind, whereof the microzymas of these matters are the principal agents.

End of the Author's Introduction

5. AUTHOR'S POSTFACE

This postface consists of a note read by Professor Bechamp before the Academy of Medicine on the 3rd of May, 1870. It establishes an important date in the history of science during the last three decades of the last century. The microbian doctrines were not yet imagined; nor were they, till several years after, as a result of the plagiarizing of the microzymian theory.

THE MICROZYMAS, PATHOLOGY AND THERAPEUTICS.

Chauffard has recently published an important work on the treatment of smallpox by carbolic acid. His conclusions interest me greatly, and I desired to make the matter clear to the Academy. In a note which appeared in the Transactions of the Academy of Sciences (Vol LXVI, p.366), I said, in reference to a note of Chauveau on the molecular granulations of the vaccine virus:

> "The transition from the study and meaning of the molecular granulations which are born or act in certain fermentations and which I have named microzymas, to the study and meaning of those which exist normally in all the tissues of organized beings, and also in the cellules of those tissues, was natural." My satisfaction, then, was extreme when I saw Chauveau enter upon this path, and, from another point of view, confirm the observations made in the laboratory of the chemist.
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> [I said] "from another point of view"; I was wrong, because from the physiological point of view where I had placed myself and whence I studied what is called fermentation, the experiments of Chauveau, on the molecular granulations of the vaccine virus, are closely connected with mine. I place the molecular granulations in solutions of simple organic matters; Chauveau in the organic and organized matters of living beings."

From a time long ago certain diseases have been compared to fermentations. We may go back to Stahl and Willis and probably still earlier for this, though that is not important, for, as was remarked by Babinet, "Antiquity has told everything; when it told truly, it was simply a wonderful accident, and it proved nothing."

My researches upon fermentations and ferments, particularly upon molecular granulations, date back some fifteen years, and those which Professor Estor and I conducted for the purpose of generalizing my earlier observations have led to this result: that the animal is reducible to the microzyma. But the microzyma, whatever its origin, is a ferment; it is organized, it is living, capable of multiplying, of becoming diseased and of communicating disease.

All microzymas are ferments of the same order - that is to say, they are organisms, able to produce alcohol, acetic acid, lactic acid and butyric acid.

In a state of health the microzymas of the organism act harmoniously, and our life is, in every meaning of the word, a regular fermentation. In a state of disease, the microzymas do not act harmoniously, and the fermentation is disturbed; the microzymas have either changed their function or are placed in an abnormal situation by some modification of the medium. This was what I tried to make clear by a positive example of a kind which would leave no room for misunderstanding either the extent or the bearings of the conclusion.

The harmonious function of a bird's egg is to produce a bird. During incubation the chemical acts which are accomplished within it result in the transformation of the materials of the yolk and the white into the various chemical compounds which will form the various organs of the complete animal.

While these chemical acts are being accomplished, no gases other than the normal gases of respiration are set free. But, if that which will be the embryo is abstracted from the egg, it contains nothing organized but the microzymas. That which will be the embryo is itself, at first, only a collection of microzymas. From the chemical point of view, everything within the egg is the work of the microzymas.

What will happen if in the egg we proceed to mix up those elements within the egg which were not destined to be mixed together? Donne said and demonstrated that the egg becomes putrid. I am of the same opinion, but this has to be explained.

If, as was done by Donne, everything in the egg is mixed up by violent shaking, there is soon observed an escape of carbonic acid gas, hydrogen and a trace of sulphuretted hydrogen. When the escape of gas has ceased, the contents of the egg, from being alkaline as it was before the mixture, have become acid; the odor is disagreeable, but gamey only, distinct from the horrible odor of rotten eggs, which are alkaline.

If we then examine what has happened to the materials of the egg, the albuminoid substances and fatty matters are found to be unchanged. The sugar and glucogenic matters have disappeared, and in their place we find alcohol, acetic acid and butyric acid. What has then taken place has not been a putrefaction, but a distinctly characterized fermentation. The violent agitation has not killed anything which was organized within the egg; only the order of its contents has been disturbed.

The microzymas have been thrown into media which was not intended for them; those of the white into the yolk, and vice versa. Having been forced to take their nourishment from materials not intended for them, they have reacted in a new manner, but without any change in their nature or appearance.

I could multiply such examples and show that the same microzyma, free or enclosed in a cellule, acts in the former condition as a lactic or butyric ferment, in the latter as an alcoholic ferment. I have reported the example of what happens in the egg because in this instance nothing foreign intervenes; fundamentally, the egg is an animal in posse.

But the microzymas may be regarded from another point of view. Not only are they individually ferments, but they are also able to produce bacteria.

This ability, alike for all, does not manifest itself equally for all under the same conditions. This amounts to saying that in each natural group of beings, and also within each centre of activity within each organism, the microzymas possess a certain specificity.

What I mean is that the microzymas of dogs, sheep, birds, etc, and those of the liver, the pancreas, or the blood, for instance, although morphologically identical in appearance, and even identical in certain aspects chemically, are nevertheless different. What is remarkable is that the bacterium derived from the microzyma possesses the same function as that microzyma; it is a ferment of the same order.

Not only is the microzyma a builder of the bacterium, but it is also a builder of the cellule; but in this new condition its functions may be entirely changed. The microzymas which are butyric ferment, and which produce bacteria which are also butyric ferments, may produce cellules which are alcoholic ferments.

Finally, the microzyma may become diseased and may communicate the diseased condition.

The first time that my attention was called to this subject was in relation to my studies of the diseases of the silk worm. On examining the eggs of a nursery in which there were many morts-flats, I was struck with the presence in these eggs of molecular granulations, motile like the others, but more abundant, of which a large number seemed united in 2, 3, and 4 grains, like the chaplets of microzymas.

I asked myself if there might not be a relation of cause and effect. All the eggs which presented this characteristic yielded morts-flats, and those worms which did not die produced butterflies which in turn produced eggs possessing this same character. Finally, when the disease was at its worst, the animal and sometimes the eggs contained bacteria. There is then for the silk-worm a characteristic which enables one to say, ab ovo, that the caterpillar which will be born of this egg will be afflicted with a certain disease.

I have not yet had an opportunity to study the different viruses from this point of view, but there can be little doubt that those of smallpox and syphilis contain specific microzymas, i.e. they transport the disease of the individual from which they originate. These two examples have led to the proposal of the specificity of certain diseases called infectious. I do not contradict this. Nevertheless, when we see that smallpox and syphilis are inoculable upon certain animals, and that anthrax is not communicable to dogs nor yet to birds, it is certainly right to ask why.

Notwithstanding many remarkable works, nothing is more obscure than the cause which presides over the development of diseases and their communicability. But what we can affirm is that when we are sick, it is we who suffer, and that the suffering is a cruel reality. This is because the cause of our diseased condition is always within ourselves. External causes contribute to the development of the affliction and hence of the disease only because they have brought about some material modification of the medium in which live the ultimate particles of the organized matter which constitutes us, namely, the microzymas.

These external causes, by a succession of changes brought about, and depending on a crowd of variables, bring about correlatively a further change, which then bears precisely upon the physiological and chemical status of the microzymas.

The tendency of the most recent researches is to show that miasms, like viruses, contain living microscopic organisms, something analogous to microzymas and bacteria, which proliferate in the blood or tissues of the animal and make it sick. I do not believe that things happen in that manner.

Every phenomenon having a cause, I admit the existence of organized particles in miasms, but I do not believe in their proliferation in the organism, a proliferation which has nowhere been proven, up to the present time, and which many experiments positively contradict.

Two authors, for instance, who agree in regarding the malignant pustule as a fermentation and who also agree that the blood of an animal attacked by a disease can communicate it to another animal of the same species, agree no longer when they endeavor to explain what they observe. For Davaine, the virulence of the carbuncular blood is due to the species of bacterium to which he gave the name of bacteridium. For Sanson, this virulence is due to a specific putrid change in the blood. According to him, the bacteria have nothing to do with it. Often they are not to be found in it; nothing organized can even be seen. He even doubts that the bacteria are animals, or plants, or even living beings. And the author remarks - and this time truly - that putrid albuminoid matter, although containing bacteria and even bacteridia, cannot communicate anthrax, even to an animal susceptible to it.

What does all this mean - if neither bacteria nor the products of the putrefaction of albuminoid matters communicate anthrax?

I will try to explain these contradictions.

Davaine made an experiment which I regard as a very important one upon this question. He inoculated some very parenchymatous plants with some putrid matter of plants, in which bacterium termo or something similar to it was present.

In an opuntia and in an aloe, he said, the bacteria propagated while preserving their primitive characters. Inoculated from these plants upon another aloe, they gave birth to long filaments divided into 2, 3, or 4 articles or segments. These long filaments, being innoculated upon a new species of aloe, produced corpuscles in a fine powder. Lastly the long bacteria, inoculated upon the species of opuntia and of aloe, which were the subjects of the first inoculations, reproduced the bacterium termo.

These facts cannot be disputed. The authority of Davaine guarantees them, but their interpretation seems to me to be open to question.

On the other hand, when I examined the frozen parts of several species of plants (belonging to various families), in which previous to the congellation there had been no lesion whatever, I always found bacteria of several kinds, not to say species, according to the specific nature of the frozen plant, and in the healthy parts, adjacent to the latter, there was not a trace of bacteria; nothing but normal microzymas. This proves that bacteria can develop in plants without inoculation, just as they can develop and even exist normally in man throughout the entire length of the digestive tract.

I would then explain the experiment of Davaine by saying that by the wound and the introduction into this wound of certain bacteria and of the liquids which saturate them, this savant produced a lesion and a change of medium which permitted the normal microzymas of the inoculated plants to evolve according to their own individual aptitudes, and there was no proliferation of the inoculated bacterium.

It is the same with animals. It is not the inoculated organisms which multiply, but their presence and the liquid which saturates them causes a change in the surrounding medium which enables the normal microzymas of the organism to evolve in a diseased manner, either reaching or not reaching the state of a bacterium. The disease is not the consequence of the new mode of being of the normal microzymas; the fever which ensues is only the result of this new method of functioning and of the effort of the organism to rid itself of the products of an abnormal fermentation and disassimilation, while inducing a return of the diseased microzymas to the physiological condition.

This theory, which is founded upon facts ascertained by indisputable experiments, explains, among other things, why the blood of carbuncular sheep containing bacteridia inoculated upon dogs or birds does not induce the appearance of bacteridia and the development of the carbuncular disease, as Davaine has shown.

But is there any difference in the purely chemical materials of the blood of a dog, a bird and a sheep? They contain the same albuminoid and other matters, the same salts, the same fatty bodies, and under other conditions, the microzymas found there certainly evolve into bacteria. The only difference which exists, as is proved by the experiment itself, must be in the histological elements of the blood of these animals and in their unequal susceptability. If then the bacteridae inoculated upon birds and dogs do not multiply as they should have done, it is certainly not that the chemical medium is different; and if anthrax does not result from the inoculation, it is because the microzymas of these animals are unfit to evolve morbidly under the influence of the medium which promotes the introduction of morbid materials.

To sum up, the microzymas are organized ferments, and they can under favorable circumstances produce bacteria. Under other circumstances they become builders of cellules. All organisms, ab ovo, are created by them. In short, the cellule, the bacterium itself, can rebecome a microzyma, and thus the microzymas are seen to be the beginning and end of all organization. If that is true, we ought to encounter them wherever organized beings have lived; and the fact is that I have found them in all the calcareous rocks from the oolithic to the most recent; the dusts of our streets swarm with them, and there as everywhere they are ferments of the same order. Not all of them are morbid. If they were, we would be living under a constant menace; but there may be morbid ones among them.

What relation is there between the above and the work which I recalled at the commencement? The relation is the following:

It is now a long time, from the beginning of my researches upon ferments at a time when nobody occupied themselves with the question of spontaneous generation, since I demonstrated (in opposition to generally received ideas) that creosote (and phenic acid, for at this time this acid was sold as creosote, especially in France), in a non-coagulating dose, did not impede a fermentation that had already commenced.

I showed that in the same dose, these agents prevented the appearance of organized ferments in the most fermentiscible mixtures. And I gave as explanation for this the fact that they opposed the germination or hatching of the germs of microphytes or microzair ferments which the air might bring to the mixtures, thus confirming an old experiment of Humer, recalled by Chevreul, and precisely proved by that savant, i.e. that the vapors of the essence of turpentine, in a confined space, hindered the germination of seeds and caused the destruction of those which had begun to germinate. I also demonstrated that the same doses of these agents did not hinder fresh muscle from acting on fecula starch, to liquify and to cause it to ferment, nor finally the appearance of bacteria in the mixture.

I concluded from this that the muscle must contain ferments already developed, living, and active of themselves, since creosote did not prevent the fermentation from beginning. This observation was the point of departure for the researches that Estor and I undertook upon the evolution of the microzymas of the higher organisms into bacteria. In ending my earliest observations, in 1866, I advised the use of creosote and phenic acid in the rearing of silk worms, for the purpose of preventing the birth of the vibrating corpuscle, which is the vegetable parasite of the pebrine.

At the same period Dr. Masse, starting from the same point of view, employed the same agent to dry up the fecundity of the spores of the microsporon mentagrophytes of parasitic sycosis.

In 1868 my friend, Dr. Pecholier, inspired by similar ideas, published his researches regarding the treatment of typhoid fever by creosote; he proposed to prevent the appearance and multiplication of the typhoid ferment. Later Gaube published a work in confirmation of that of Dr. Pecholier. The same year Calvert reported the experiments made at Mauritius by Drs. Barrault and Jessier on the application of phenic acid in the treatment of typhoid and intermittent fevers.

The above is the connection of the ideas and origin of the employment of creosote and carbolic acid in therapeutics. The theory of this employment is as follows:

Creosote dries up the fecundity of the germs which produce disease, in conformity with the principles enunciated by me in 1857. The following experiment, while maintaining the principle, gives it a wider meaning and places it in connection with the first parts of this discourse.

Beer yeast is a complete organism, though reduced to the state of a simple cellule. As an alcoholic ferment, in a sugared medium it preserves indefinitely its cellular form. But under other conditions, things happen differently. Beer yeast, it has been said, does not cause starch to ferment; that is an error; it causes it to ferment, but in a different manner to sugar, that is all. If it is introduced into starch of fecula, with some very pure calcic-carbonate (not from the calcareous rocks), the whole being creosoted to hinder the influence of germs of the air, the starch will be liquified, a fermentation will be set up and the yeast disappears by degrees and is finally replaced by an innumerable quantity of superb bacteria. The fermentation is acetic, lactic and butyric instead of being alcoholic.

It may be said that it was the bacteria which were the ferments; granted, but observe that these bacteria are the issue of the beer yeast, of its microzymas. That settled, in other experiments, the same quantities of yeast, calcic carbonate and starch being employed, and double and triple the quantity of creosote, the starch was still liquified, and the fermentation proceeded, but the globules of yeast were not destroyed, and the bacteria did not appear. The yeast was not killed; the creosote when used in greater amounts has only prevented the evolution of the microzymas into bacteria.

Creosote, which resists the blossoming of the germs of microphytes and microzoairs in fermentiscible media, preventing thus the commencement of fermentation, does not hinder a fermentation already commenced and where there exist already adult organisms. But in certain doses it is a moderating agent which, according to the experiments just mentioned, regulates the function of the cellule and its microzymas, which it prevents evolving into bacteria.

The explanation of the role of carbolic acid and creosote in therapeutics is easy to understand, if account is taken of the researches which have permitted this hasty resume to be made. These agents do not hinder the physiological functioning of the histological elements of the organism, but they arrest the morbid evolution of the microzymas, the too rapid destruction of the cellules, and tend, doubtless by modifying the medium, to bring back into harmony the functioning of the deviated microzymas.

This recalls unavoidably the agents used in the old therapeutic devices which our ancestors employed; camphor, essences, musk, etc. It is true that it was empirically that they fulfilled the indications which, after many deviations, we now supply like them, but instead we use new methods which rely on experimental and positive data.6

And, in conclusion, I beg the permission of the Academy to repeat here something which Professor Estor and I said in a recent work upon this subject:

"After death (leaving here the domain of pathology to enter into that of the physiology of the species), it is essential that matter be restored to its primitive condition, for it has only been lent for a time to the living organized being. In recent years an extravagant role has been assigned to the airborn germs; the air may bring them, it is true, but it is not necessary that it should do so."

The microzymas, whether in the state of bacteria or not, are sufficient to assure by putrefaction the circulation of matter.

The living being, filled with microzymas, carries in itself the elements essential for life, disease, death and destruction. And that this variety in results may not too much surprise us, the processes are the same. Our cellules, it is a matter of constant observation, are being continually destroyed by means of a fermentation very analogous to that which follows death. Penetrating into the heart of these phenomena we might really say, were it not for the offensiveness of the expression, that we are constantly rotting.

** End of the Author's Postface **

[Antoine Bechamp][Sumeria]