LECTURES

ON

THE BLOOD;

Presented by D. C. Peters

AND ON

THE CHANGES

WHICH

IT UNDERGOES DURING DISEASE.

DELIVERED AT

THE COLLEGE OF FRANCE IN 1837-8.

BY F. MAGENDIE, M.D.

MEMBER OF THE INSTITUTE OF FRANCE, AND OF THE ROYAL ACADEMY OF MEDICINE, ETC., ETC.

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PREFACE.

The Lectures of M. Magendie "On the Blood; and on the Changes which it undergoes during Disease,"* now presented to the readers of the 'Library,' will be found to be the fullest description hitherto given of the properties, and physical and vital modifications of this fluid. It would be inaccurate to say, that the reign of the humoral pathology is restored; but the well-read and observing physician may safely allege, that it is impossible for us to take cognisance of the reciprocal action of the organs, in the play of their functions, on each other, or of the operation of either medicinal or noxious agents on the animal economy, without admitting the frequent changes in the fluids under all these circumstances. As there is no tissue which does not consist of both fluids and solids, so is there no organic change accomplished without a change in their relative proportion or position, and no impression of any moment made on the one which does not directly affect the other of these two classes of vital elements.

Foremost in the list of the fluids which are both the results and supporters of vitality, is the blood. Into the cavities and vessels destined for its transmission and distribution are poured the products of absorption, whether nutritive or others, in order to be subsequently mixed up with it. From the blood are supplied the materials for growth and reparation of all the organs; and from it, also, are given out those other matters, the retention of which would be injurious, or positively destructive, to the animal economy. Although the blood is protected, in the functions of chymosis and chylosis, against the introduction, through the lacteals, of substances which have not undergone the requisite chemical and vital elaboration, it is not by any means equally guarded in the direction of the venous capillaries, which take up, often without selection, from the stomach and bowels, water holding various saline and, on occasions, even poisonous matters in solution. The lymphatics, also, whose proper function is to absorb the fluids in their physiological state from the serous and cellular systems, and the molecules of the solids from all parts of the body, display every now and then a vicious appetency, by their imbibing pus and other matters, the result of morbid action, and transmitting them into the general sanguineous current.

Even in the functions destined for the distribution and purification of the blood, there may be such irregularities as to seriously affect this fluid, either in the proportion of its gaseous bases and saline elements, or of its proximate principles. The lungs may fail to discharge the requisite proportion of carbonic acid, and to absorb that of oxygen,—or they may imbibe, from their extended surface, delete-

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rious air and positively poisonous gases, which will deteriorate the quality of the blood, and alter its constituent proportions. So, also, the kidneys and the skin—the other two great depurating organs—by their failing to discharge their appropriate functions, exert a powerful influence on the blood.

Dependent directly for its vitality on the continued motion imparted to it by the circulatory apparatus, and mediately through this latter on the function of innervation, the blood is liable to be changed in various ways by those causes which act with any force on the nervous system, and on the heart and the capillaries.

With a knowledge of these premises, there is, therefore, little reason for wonder that the blood, after excessive waste, should be less fitted to supply its fibrinous element to the muscles, and albuminous to the membranes; or that, after obstructed pulmonary or cutaneous transpiration, or renal secretion, it should contain, together with its appropriate constituent principles, others effete and deleterious, which, on being carried to the organs and tissues, in the round of the circulation, disorder and pervert their functions; and which, by being mixed up with the proper nutritive particles deposited in them, cause a laxity of structure, and even actual disintegration.

M. Magendie, in the following Lectures, has enlarged on these points, and illustrated and enforced them by many and various experiments and observations. The influence reciprocally of the organs on the blood, and of the blood on the organs; the altered proportion of its elements as cause and effect of disturbance or lesion of the solids; the effects on the body of losses of blood, and of the transfusion of this fluid, and of the manner in which it is affected by heat and cold and different gases, are set forth by the lecturer with his accustomed zeal and ability. The changes of the blood in diseases, viz., in inflammation, cholera, chlorosis, peritonitis, typhus, albuminous nephritis, &c.; the effects of the injection of water into the veins in hydrophobia, and of saline fluids in cholera, are likewise detailed. From the effects produced by the mixture of putrid matters with the blood, inferences are drawn by M. Magendie in illustration of the influence of miasmata, and of the symptoms in typhous and yellow fevers, and plague. Numerous experiments are detailed of the changes caused by various therapeutical agents on the blood, and explanations offered of their *modus operandi*, including the action of mineral waters on this fluid.

The Lectures of M. Magendie in connexion with those of Dr. Clutterbuck on Blood-letting, which latter have been also introduced into the 'Library' of the current year, will supply an amount of positive knowledge of the properties and different states of the blood in diseases, and of the indications for its abstraction, and the effects of this measure, which will be sought for, elsewhere, in vain. It should be added, that both these series of lectures have been made thus readily accessible to the profession in the United States through the medium and means alone of the 'Library.'

J. B.

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LECTURE I.

Mode of studying medicine.—Defects of pathological anatomy, as at present studied.—Necessity of searching for the cause of disease.—Importance of an examination of the properties of the blood.—Mode of production of pneumonia, fever, &c.—Pathology of phthisis exemplified.—True method of advancing the science of medicine explained.

GENTLEMEN:—I always hail, with lively satisfaction, the period of our annual meeting; but upon the present occasion I feel even more than ordinary pleasure, for the recollection of the important and unexpected results, which we were fortunate enough to obtain last year, justifies me in the hope that our future researches will be equally productive, and a new impulse thereby given to the study of medicine. It is surely not going too far to assert, that the manner in which we explained the origin and *mechanism*, if I may so speak, of various diseases, is of a nature to clear up, in no contemptible degree, the mysteries of pathology, of that science which cannot be said even yet to exist; for I set but little value on the minute examination of the traces left by disease on our organs, though that pursuit has been pompously styled pathological anatomy. Are you not in truth convinced,—you who have many a time ascertained the fact for yourselves,—that the lesions found at our autopsies are frequently produced after death, and that consequently the plan hitherto followed in such inquiries is fallacious, and can only lead to vague information and error? It is not a little remarkable that, at a period when the *positive* is sought in every quarter, the study of a science so important to humanity as medicine should be almost the only one characterised by uncertainty and chance; be it ours to strike out a new path. The *effects* of diseases have long been studied; let us search for their *causes*, study them one by one, and this done we shall, perhaps, be enabled successfully to modify their injurious effects. This, believe me, should be the end of all our efforts. The task is no holiday one, it is true, for nothing is so difficult to eradicate as absurd notions generally adopted, and there are many such that corrupt the existing theory and practice of our art. But, Gentlemen, the more difficult the more honourable the attempt; and even if we do no more than put our successors in the right way one day to complete the work, we will feel satisfied that we have ourselves been no unworthy labourers in the field.

You have often heard me raise my voice against the defective character of the existing system of medical study. Like me, too, you have no doubt been struck with the trifling good that study confers on society. But could it be otherwise when there is scarcely a sound idea on physiology abroad? when anatomy is learnt in a hurry and forgot with still greater speed; when those sciences, which though called *accessory* are no less of first-rate importance in a plan of medical education intended to prove practically useful, namely, physics, chemistry, natural history, mathematics, &c., are generally neglected! To illustrate the importance of physics, I need only remind you that to its laws are to be referred a crowd of phenomena, incorrectly styled *vital.* You will recollect, among other things, how, by the help of some of the principles of hydraulics and mechanics, we succeeded in simplyfying the study of the circulation, which formerly required for its explanation such a complex train of chimerical hypotheses. Compare the results of the experiments we have made in common with what you find in books, and you will see how widely the imagination may lead the understanding astray. Beware, Gentlemen, of the fanciful creations of writers, no matter with what ingenuity they are dressed up. Devote yourselves, on the contrary, to experimental study,—*see, touch for yourselves,*—take no one's word for anything,—mistrust yourselves, mistrust me, and you will manage to steer clear of the whimsical conceptions brought forward to explain, some way or other, the frequently inexplicable phenomena of organisation.

I am anxious to recall your attention to the experiments we made last session on the blood. You learned through them the influence that fluid exercises on our organs. You saw me produce at will, in animals, the majority of the striking phenomena determined by the most terrible diseases, for the relief of which art is powerless.

You saw me give rise, at my pleasure, to pneumonia, scurvy, yellow fever, typhoid fever, &c., not to mention a number of other affections which, so to speak, I called into being before you. And here, Gentlemen, we will especially employ the aid of organic chemistry. Feeble though its light may be, we will strive, by its help, to discover the intimate composition of the blood in the normal state; the changes it undergoes, the impressions it receives, and the disorders it conveys into the economy when variously modified. The paramount importance of such inquiries must be admitted by all; for I would ask, wherein lies the difference between the medical practitioner and the nurse at the sick bedside? Suppose them engaged with, for example, a case of variola. In the course of his studies the former has attended clinical practice, and has learned the symptoms and terminations of that disease; he knows, admirably well, that it is ushered in by certain general phenomena, which are followed by a peculiar eruption of a certain duration; that the pustules formed dry, and that desquamation closes the scene. Very true; but do you imagine that the nurse, provided she be habituated to her calling, does not know all that quite as well as he? Will he be able to tell me a whit more correctly than she, why the case of variola before him will prove confluent or benign? or why the skin, suddenly assuming a purple colour, the sufferer is carried off in a few hours? No; the most skilful and experienced practitioner knows nothing of all this: then he stands the ignorant and too often powerless spectator of such modifications as those of the primary disease. All he can do is to order certain remedies which, if necessary, the nurse could prescribe equally well. Is this the sort of superiority to which it is the province of the physician to aspire? That in the present condition of the science it is the sole one, cannot be denied. Let it be our business, Gentlemen, to cast away the humiliating part our predecessors have left us to play. But how is this to be done, you will ask? I answer, by creating *experimental medicine*, which, aided by our present extended knowledge of various sciences, will doubtless reveal the mechanism of morbid changes, and thereby render it possible for us to attack vigorously, to modify, nay, to prevent the causes of those changes. Such is the manner in which I view the aim and plan of our studies; nor do I believe that a more rational system of inquiry, or one so likely to prove fertile in results has at any period been laid down.

Our former experiments on the nature of the blood are daily confirmed by cadaveric examination. A woman labouring under the prodrome of acute disease, was recently brought to my wards at the Hôtel-Dieu: I diagnosticated variola. The same day, though the orthodox treatment was duly employed, she suddenly grew worse; the surface assumed a purple hue, and death supervened in thirty hours.

I have already spoken to you of this terrible complication of small-pox. But in this particular case the symptoms were so rapid in their course, and death so unexpectedly sudden, that several of the people about the hospital were terrified, and declared the patient had perished of the plague. I was naturally curious to compare the phenomena disclosed in the dissection of this woman with those produced in the artificial diseases I am in the habit of creating in your presence. The result was, that I found the resemblance between them so perfect, that one might have fancied the patient's death caused by the very means I employ in my experiments on animals. In pneumonia, again, you have a new proof of the evident part the blood plays in the production of morbid disorders. Writers, even from the earliest times, have described that affection. You are well acquainted with its prodromata, its periods, its termination. But what is its first cause? what actually goes forward in the lung in order that in one instance we shall find engorgement, in another *red* or *grey hepatisation?* These terms strike you as ridiculous, and excite your smiles. I am, however, compelled to use them in order to speak the language of the schools which, loaded as it is with false and grotesque comparisons, cries aloud for reformation. But to return to the pneumonia. The parenchyma of the lung is, in some cases, hardened, softened in others, and is rendered totally unfit for carrying on the functions of respiration and circulation. I have frequently explained to you the mechanism of these changes. At present I shall merely, in a summary manner, say that, through a chain of chemico-physiological phenomena, now in great part understood, the blood becomes extravasated in the labyrinth-like pulmonary canals, coagulates, becomes solid, and produces the various disorders you have many a time noted. These disorders, which are always nearly identical, cannot be accounted for otherwise than as I now explain them. I attach great importance to this point, because a multitude of pathological cases are referrible to it. It is thus we produce, at will, what is called gastro-enteritis and typhoid fever; the redness and other changes in the intestinal mucous membrane, every characteristic, in short, of those diseases, is imitated with strict accuracy.

These positions acquire new importance from my having been enabled, since last session, to make some very curious applications of them. Take, as an example, malignant pustule. That frightful disease, in which inflammation is said to play so important a part, coincided in the case of a subject, who died a short while past of it at the Hôtel-Dieu, with non-coagulability of the blood. All my researches have, indeed, invariably demonstrated that the property of clotting possessed by that fluid is an indispensable condition for the free and regular continuance of its movement in the capillary system; and it is some modification or other of this condition that causes the frequency of death or diseases attended with the appearance of petechiæ, such as the plague, typhus, variola, scurvy, &c. Our attention will, therefore, be directed to the investigation of the causes that may act mediately or immediately on that property. It will require but little thought on your part to form a conception of the deep interest attached to such inquiries, not only as respects pathology, but the well-being of humanity. We are already acquainted with a great number of solid, liquid, and gaseous substances that destroy or weaken the coagulability of the blood; we will now endeavour to discover such as increase its energy, which I do not believe to be impossible.

Be persuaded of this, Gentlemen, that the truly scientific mode of studying medicine lies almost wholly in investigating the manner in which morbid changes are produced. We have already succeeded in elucidating it in the case of several diseases, and beyond a doubt we shall be enabled to increase their number. Look at pulmonary phthisis for example, there is an affection which you see day after day cutting off individuals of every age, of every sex, and of every rank, yet none has been more carefully studied on the old plan, none has proved a more fruitful source of dogma and disquisition. Eminent observers have described all its phenomena, even to the minutest details. But what is all this description but so much natural history; will it throw any light on the treatment of the affection? Not a particle: but we hope for enlightenment in this respect; we must learn the *cause* of this disease; nor need we despair of discovering it. Perhaps tuberculous matter may be detected in the blood, and, as a further step, the means of destroying it, or preventing its formation, ascertained. Hear a fact well suited to give confidence to our hopes. We know, in the first place, that tuberculous may be accurately distinguished from purulent matter under the microscope. Now, within the last few days, in examining the body of a woman who died of phthisis, I found among the pillars of the right ventricle of the heart certain bodies resembling a sort of fibrinous sac, and containing a fluid like the pus of a phlegmonous abscess. I carried the part home and satisfied myself, by microscopical examination, that the supposed pus was, in reality, tuberculous matter; it is clear, therefore, that it existed in the blood, but whether its presence there was the cause or the effect of the phthisis, is a question for future investigation, of which we shall not lose sight; and this important fact does not stand alone. The practice of the Hôtel Dieu daily furnishes similar ones, which give new strength to my persuasion that we shall find in the blood the cause of a host of diseases, and in every instance a fresh source of instruction.

The poverty of the science as regards all these fundamental questions perhaps surprises you; but it will cease to do so if you reflect for a moment on the scientific character of the profession. Medical men may be divided into two classes; those forming the first give up all study the moment they leave off attending lectures; they quite conscientiously believe, and frequently succeed in persuading others, that they understand every disease, and can cure every variety of suffering; these gentry occasionally realise a handsome fortune, but I must say they do so at the expense of science and of the interests of their fellow-men. Their numbers explain to my mind marvellously well the discredit into which medicine seems to have fallen at the present day, and the high favour lavished by the public on quacks of every grade. The second class of practitioners continue, it is *true*, to follow clinical pursuits with zeal, but some among them, misled by scholastic errors, retard rather than accelerate the progress of the science. Against one of those errors I have waged war ever since I took upon me the duties of a teacher,—a fallacy which is the more injurious because it seems supported by some specious argument, which, nevertheless, I shall not now discuss; I allude to the assertion that there is nothing in common between the phenomena of living bodies and those of inert matter; not that I wish to ascribe to the latter more than belongs to it, but my devotion to the cause of truth induces me to bring forward everything that may in any wise facilitate its discovery. In illustration I may remind you of your having seen how certain phenomena, as those of imbibition, of permeability to gases, &c., take place in organised and living membranes exactly in the same manner as in inert bodies.

It were much to be desired that persons engaged in medical research would, on all practicable occasions, make use of instruments fitted to estimate the strict value of the results obtained. The advancement of all the other sciences has been effected through the gradually increasing perfection of their means of observation; why should it not be the same with medicine? In several of our experiments last year we had occasion to employ an ingenious instrument invented by M. Poiseuille for ascertaining the precise pressure of the blood, and the force of impulsion of the heart. The results were scarcely credible, and have overturned all theories that might have been previously held on the subject. Among other facts M. Poiseuille has established, by means of the hemodynamometer (for so the instrument is called), that the static force is equally energetic in two animals exceedingly disproportioned in size—a horse and a rabbit. I myself made an experiment with the instrument on two dogs of equal size and strength; I injected the greater part of the blood of one into the veins of the other, and found that the variation in the pressure amounted only to a few milimetres. Now, no matter how suprising these results may be, they are incontestable, and, indeed, the solution of what at first appeared inexplicable was thereby rendered extremely easy. Here it is: any notable diminution of the quantity of the circulating fluid increases the number and force of the heart's contractions, while a superabundance of blood produces a contrary effect. Hence it may be laid down as a principle, that the number and force of the heart's pulsations are in the inverse ratio of the quantity and pressure of the bood; this increase in the frequency and power of the heart's contractions, when the organ is nearly deprived of blood, may be compared to the acceleration of the movements of a watch caused by shortening its balance. These facts show how much might be said on the effects of blood-letting, and what a novel view might be taken of the theory of that operation. At a future period we will reconsider this subject; for the present, suffice it to say that the removal of blood by venesection inevitably modifies the contractility of the heart.

Every question brought before you will be examined in its physical, chemical, mechanical, and vital bearings, for life is the result of these different modes of action. I propose devoting a considerable number of lectures to the study of the blood, for I am acquainted with no subject more deserving of attention. We will study it not only in chemical apparatuses, but in the living animal and in the human subject labouring under disease; we will endeavour especially to ascertain by what series of transformations its renewal is effected through the chyle, the materials of which, when examined separately, are found to bear no analogy to the proper constituents of the blood. Our attention will also be directed to the globules of the blood, which certain physiologists classify as animals of the tribe infusoria. We are already acquainted with their form, and the manner in which their movements are effected. We shall pursue our inquiries relative to them by examining the modifications, if any, to which they are subject. M. Turpin has lately asserted that the globules of the milk belong to the vegetable kingdom. It will be worth our while to determine whether or not any close relation may be shown to exist between these globules and those of the chyle or blood.

LECTURE II.

Emptiness of medical theories.—Necessity of determining the nature of phenomena.—Physical nature of phenomena called vital.—Union of solids, liquids, and gasses necessary to life.—Effects of the movement of the blood; materials which furnish the blood.—Fluids more necessary than solids; illustrative example.—Reparative effect of respiration.—Action of blood-letting on the composition of the blood.—Change of the blood during epidemics.

GENTLEMEN:—Such of you as are acquainted with my former researches on the blood, will not be surprised at my devoting another course of lectures to that important subject. Your clinical studies in the hospitals, and more especially the events of your own practice, must have supplied you with abundant evidence of the emptiness of past and present medical theories. Nor can the important truth, I feel convinced, have escaped you, that in ultimate analysis the majority of questions bearing on the causes and mechanism of disease, are more or less closely connected with the study of the chemical, physical, and vital properties of the blood. These considerations appear to me to justify the novel character I propose to give my lectures this year. I will take care to exemplify the doctrines laid down by a series of experiments, and as these will be made in public, I shall have an opportunity of showing how investigations of the kind should be conducted.

I should observe, *in limine*, that a point of the very highest importance is to determine accurately the nature of the phenomena we wish to fathom. Physiology has, in this point of view, made some progress, and I shall, I trust, be pardoned for believing that to my labours that advancement is in some measure to be ascribed. My experiments have proved that various actions, habitually called the effects of *vitality*, are purely *physical*. The clear demonstration of this important fact horrified, in its day, not a few of the avowed partizans of the marvellous; but people grow accustomed to it now. As to you, Gentlemen, I flatter myself that the experiments performed in your presence have made a deep impression on your minds, and that they will exercise no mean influence on your entire medical career. I am far from classing you with those who scorn the voice of experiment and observation, and obstinately persist in regulating living nature by laws concocted in their own imaginations.

I do not deny that there is some talent shown in the production of the dreamy theories so much in vogue, and I know further that they are of a kind to acquire popularity and make the reputation, nay even ensure the fortune of those who profess them. I am, however, far from envying success of this stamp, especially when I observe, day by day, the fatal consequences of error whether brilliant or dull. It will therefore be our first care when we set about the study of any question, to demonstrate, not by fine phrases, but by proofs furnished by experiment, the character of the phenomena we propose to investigate. In cases where a *vital* action is the subject of inquiry, we shall in all likelihood have only a number of facts to lay before you, for in that branch of physiology generalisation is as yet impossible, and we are still confined to facts alone. I shall in no instance attempt to give you an hypothesis when the true explanation is beyond my grasp; I will candidly avow my ignorance rather than run the risk of leading you into error.

When we review the different orders of organised beings, not even excepting vegetables,—which, whether right or wrong, are considered as a sort of transition-kingdom, established by nature between inert and living matter—we discover that the indispensable condition, the *sine quâ non*, of life consists in the union of solids, liquids, and gases. The combination of these three forms of matter is found at both extremities of the scale of living existence. Gaseous matter is generally associated with liquids, and for the present we will set aside its study, to recur to it at length, I trust, at a future period. Among organic beings some are almost wholly composed of solids, others of fluids. In some instances both are intimately combined, in others they appear simply mixed, or only in contact. However, it is certain that the liquid prepares and forms the solid, and that it maintains in the latter such conditions as are necessary for existence. The mechanism employed by nature in effecting the movement of the fluids through even the most delicate parts of organised bodies calls for our unqualified admiration. That mechanism long furnished materials for the most absurd theories; and he who first caught a glimpse of the truth—the immortal Harvey—lived in consequence the remainder of his days a butt for the persecutions of the theorists of his time. It is now better understood, and I described it to you at length last session. The facts then laid before you showed the palpable error of the notions heretofore admitted on the subject. You learned, too, the importance of this movement of the liquids through the frame; you saw that if these stand still, various disorders directly follow in the various organs; the functions are no longer carried on, and life itself ceases. But why is it necessary that the blood—observe, Gentlemen, by the way, that the word *blood* is so vague as to be totally unfit to designate a fluid of such complexity—why is it necessary, I say, that the blood should be in a constant state of movement in living bodies? Nature, it is certain, does nothing without motives, and we find in every direction proofs of her inexhaustible resources, and profound wisdom. Listen, then, to the facts bearing on this question. The existence of certain relations between its constituent parts is necessary for the support of the blood's vitality; its disappearance, its reproduction, its transformation, are also requisite for the same end. Death or disease is the inevitable result of the absence of any one of these conditions, while their perfect association constitutes and maintains the state of health. If too much of the fluid escape from its reservoirs the play of the organs is impeded. If any periodical evacuation ceases, disorder similarly follows. But if the blood receive new materials in one way, while it loses the old proportionally by another, an equilibrium of composition and decomposition is established, constituting its normal state. At another time we will analyse its intimate properties; for the present I will simply state in a few words its mode of reproduction, and the nature of its action on the organs.

The chief source from which its materials are derived is the food, under which head I include drink. In these materials are found the three constituent principles of living beings,—liquids, solids, and gases. The nutritive and sensorial functions regulate the phenomena by which one is converted into the other. Fluids, however, seem to be more indispensable for its reproduction than solids, which explains the agonising character of the sensation of thirst. Men have succeeded in enduring total abstinence from solid food; but the torments of thirst—of the instinct that forces us to furnish the blood with its most necessary material—have invariably proved too great for human tolerance. A curious circumstance exemplifying this recently occurred in my wards at the Hôtel Dieu. A woman, who was either an impostor or a lunatic, gave out that she had received a mission from the Deity to reform mankind: she was to fast 365 hours, doubtless to render herself more worthy of the great work with which she had been entrusted. For several days she refused all nourishment, and I became anxious to ascertain how far she would carry her melancholy juggle, which was beginning to make some noise among the devotees of the metropolis. She was separated from the other patients and confined in a solitary room, and every precaution taken to prevent her obtaining food without our knowledge. However, as I had no wish that the poor creature should fall a victim to her obstinacy, some food, previously weighed with care, was placed at her disposal. The result was that during the whole time of her seclusion which lasted, I believe, eleven days, she resisted the calls of hunger; she did not touch a morsel of the solid food set before her. But she was unable to support the anguish induced by privation of water. She drank daily; and with a refinement of craft, made up the deficit in the bottles supplied her with her urine.

The necessity of drink for the reproduction of blood is thus established; let us inquire into the effects of abstinence from solid aliment. If we deprive an animal, either by degrees or suddenly, of its food, and examine its blood some time after, we find certain modifications produced in that fluid by the alteration of regimen. The proportion of its serum is increased; its coagulability lessened; at the same time the animal's strength rapidly decreases, purulent ophthalmia supervenes, and is followed by other affections dependant on the morbid condition of the blood. Hunger, then, it appears, is nothing more than the instinct that commands the animal under penalty of disease and death, to supply materials for the renewal of its blood.

Again, in respiration we have another important reparative process of the blood. The importance of any apparatus of organs may be estimated by the disorders following an interruption of its functions, and you are aware of the distressing character of the sensation of suffocation. I need not for the present bring to your recollection the numerous causes of disorder in asphyxia; but as regards the effect, here is what takes place; the blood, meeting with some obstacle or other which materially interferes with its freedom of contact with the atmospheric gases, does not combine with them in sufficient quantity, and in proportion to the modification produced thereby, ceases to convey to the brain the normal degree of excitement. If this state continue, asphyxia, a state of apparent death, follows. I would ask, then, what is the sensation of suffocation unless the instinct that commands us not to remain an instant without bringing atmospheric air into contact with our blood, so as to furnish it with certain elements indispensable for the maintenance of its vitality?

You perceive by what I have just said on food, drink, and atmospheric air, that in the blood reside certain conditions absolutely requisite for the support of existence. Now these conditions are always more or less modified in the various affections of the economy, and it is hence of major importance that the practitioner thoroughly study them. None will, I imagine, contest these truths; but far from fearing their refutation I court any attempt of the kind. No opposition would ever succeed in preventing me from striving to fathom the indubitable fact, that *every notable departure from the healthy state of the blood manifests itself almost always by physical modifications of the organs.* And this fundamental position is so true that the simple inspection of the solids indicates in many cases the nature of the modifications undergone by the blood.

But, Gentlemen, I must step a little out of my way in these generalities, to mention a point of the very greatest consequence in practice. You are already acquainted with a great number of causes that modify the blood and induce disease; but you are scarcely, perhaps, prepared for the announcement, that by means of a therapeutical agent, holding the first rank amongst the fashionable remedies of the day, I produce the very same alterations in the blood, and, as their result, the very same disorders in the economy. This may, perhaps, strike you as a random assertion, but my words are not lightly spoken. I have within my reach the guarantee of their veracity; experiment shall confirm them. I assert, then, loudly, and fear not to affirm it, that BLOOD-LETTING induces, both in the blood itself, and in our tissues, certain modifications and pathological phenomena, which resemble to a certain extent those we have seen developed in animals deprived of atmospheric oxygen, of drink and of solid food. You shall have the material proof of the fact. Here are three glasses containing blood drawn from a dog on three different occasions, at intervals of two days. The animal was in good health, and I took care to supply him abundantly with nourishment. In the first glass you see that the serum and clot are in just proportion to each other. The latter, which is perfectly coagulated, forms about four-fifths of the entire mass. This specimen of blood consequently appears to possess the desirable qualities. Now turn your attention to the second glass. The animal was still well fed when its contents were drawn, and yet you perceive an evident increase in the quantity of serum; the clot forms at the most only two-thirds of the whole. But here, in the produce of the third venesection, although the animal's diet remained unchanged, we find a still greater difference. Not only is the proportion of serum more considerable, but its colour is changed. It has acquired a reddish-yellow tinge, owing to the commencing solution of the globular substance.

Do not suppose, however, that I would recommend the practice of blood-letting to be completely discarded. Far from this, I am persuaded that in a certain number of cases it may be useful; but between using and abusing there is a vast difference, too frequently, I am sorry to say, lost sight of. I will continue to bleed this animal from time to time; but I can tell you beforehand, from the result of similar experiments, that the alteration in the properties of its blood will entail that of its organs, and finally death. The lung, for example, will become affected with engorgement, œdema, pneumonia, and the entire train of what people are pleased to call inflammatory phenomena; and mark the extraordinary fact, that this inflammation will have been produced by the very agent which is daily used to combat it.

But to pursue this subject, let us consider *variola*, for no matter what specialty there be in its phenomena, to the present subject it really belongs. For my part, I do not believe that disease is ever developed without some modification or other existing in the blood, and I will give you a proof of this. I have at present a woman under my care in mild small-pox; I had two or three ounces of blood taken from her, and here they are. You perceive that this blood does not in the least resemble that of the patient I spoke of to you in my last lecture, who died so suddenly of the form of the disease called *variola nigra*. In the present instance, there is a tolerably consistent clot, whence I infer that the patient's life is not in danger; I will take care, however, to let you know the event. Still, Gentlemen, I must beg you to observe that when I plunge a piece of reddened litmus paper into the serum, it is immediately turned blue. This shows the serum to be very strongly alkaline, more so I think than in the normal state. Now we have often had occasion to observe, that an excess of alkalinity in the blood interferes not only with the freedom of its passage through the capillary vessels. When thus modified it penetrates their walls by imbibition, as extravasated into the surrounding tissues, and, among other effects, produces in the mucous membranes the disorders long known under the name of inflammation.

Here is some blood taken from another patient. You remark its saffron tint; the yellow colouring matter of the serum, which, by the way, has been well analysed and described by M. Chevreul, is in excess. In the peculiar modification of this specimen you may recognize the condition that produces jaundice.

You have another example of the influence of modified blood on the organs in œdema of the lungs. You are aware that I can, by injecting a certain quantity of carbonate of soda into the veins of an animal, produce that affection, with its pathognomonic signs at the outset, and its cadaveric lesions, when it causes death. When the blood becomes surcharged with an alkaline principle its serous matter increases in quantity, escapes from its vessels among the lobular ramifications of the lung, distends, bursts them, and carrying with it the coagulable part in a semi-liquid state, collects into irregular masses. Pathologists, who are always ready to show their sagacity in comparisons, compare the blood in this state to currant jelly (*gelée de groseille*). The semi-liquid clots I have described, to which this fantastic term is applied, are almost invariably formed of fibrinate of soda or potassa. Now, on comparing these disorders caused by spontaneous excess of alkalinity in the blood, with those we ourselves produce by injecting carbonate of soda into the veins, or by a frequent repetition of blood-letting, we find the same symptoms, and in every respect similar results. Nevertheless, according to the theorists of the day, bleeding is an almost infallible remedy, a sort of panacea, in such affections. You will allow, Gentlemen, on the contrary, that in such cases venesection is had recourse to without our knowing very well why; for unless we set up for out-and-out homœpathists, it is scarcely reasonable to employ as a remedy an agent which would itself cause the disease.

It is well for you to know, Gentlemen, that I do not, as is the ordinary habit of clinical professors, pick out the subjects who form the subject of my observations. I am in this respect guided by chance alone. In cases when the nature of the complaint under which any of my patients labour is doubtful, I prescribe a small *trial bleeding*. The blood drawn is received into small glass vessels; these are brought here at the hour of lecture, and in the majority of cases my first examination of their contents will take place in your presence. Here, then, is some blood taken from a woman with an enormous enlargement of the spleen. She has for several years been subject to violent attacks of intermittent fever, which it has been found impossible to check. It appears to me to be in a morbid state; the serum, which usually forms one-fifth, here constitutes more than one-half of the whole. That this woman's fever is in some way or other connected with the great proportional quantity of serum in her blood, does not admit of a moment's doubt.

Facts of this kind are perhaps at present difficult for you to comprehend; but when we shall have advanced further in our study of the fluid under consideration, and become acquainted with the vast number of its constituent parts, each of which has its special properties, or, if I may so speak, *functions*, you will readily understand how it may be modified in numerous ways that at present escape you.

These considerations are not applicable to the human species alone. The nutritive liquid has invariably struck me as having undergone a morbid change in animals attacked by epizooties. I am a member of a committee of inquiry appointed by government to investigate a disease that yearly robs the state of several thousand horses,—I allude to acute glanders, an affection characterised by an abundant sanious discharge from the nostrils. It is currently believed to be contagious, and in consequence the moment an animal is declared glandered it is consigned to the slaughter-house. My opinion is not yet made up on this important question. We are told that the blood of glandered animals does not coagulate; and yet, as you may see here, the blood of the horse is very rich in fibrin. Eight days ago I injected some of this blood into a healthy animal. So far no anormal phenomenon has followed the operation. I shall not omit to let you know the result of the experiment, as it seems to me calculated to throw some light on the question of contagion.

Besides the external causes that act on the blood we must admit the nervous influence. Several diseases of the pulmonary organs, such as angina pectoris, and certain forms of asthma, appear to me connected with disordered innervation; for it appears from experiment that the section of the nervus vagus, though it does not interrupt the act of respiration, seriously affects the capillary circulation, or that indispensable association of the air and blood of which we have spoken. I divided this nerve in an animal; pulmonary œdema followed, and it died. Compare this specimen of its blood with that of a woman who also died of œdema of the lungs; the analogy is so strong that you can detect no appreciable difference between them. In the first instance we know its cause, in the second we do not; but the material lesion is the same in both.

If, Gentlemen, the facts we collect and the experiments we perform, go to confirm my conjecture, that every modification of the blood is represented by one or more organic changes, it will become necessary without delay to revise the existing medical doctrines, and establish a new therapeutical code. Time will show.

LECTURE III.

True method of studying medicine; disputes about nothing.—Pathology of typhus fever; effects mistaken for causes.—Constituents and properties of the Blood; its power of passing through minute tubes; Ruysch's mode of injecting.—Lecanu's analysis of the blood.—Necessity of the presence of fibrin.—Pathology of rheumatism.—Use of acetate of ammonia.

Gentlemen:—The considerations into which we entered at our two former meetings, and the remarkable facts then brought forward, must, if I mistake not, justify in your minds the selection I have made of a subject for the year's course. I am not deceived as to the difficulty of our undertaking; I know that, on the one hand, our steps must be slow, simply from the novelty of our plan, and that we must, on the other, be prepared to contend against the ill will of those with whose favourite ideas our researches may interfere; but neither of these obstacles will deter us from prosecuting our inquiries. The facts we observe will be submitted to the examination of all, and we will steadily adhere to our old plan of attacking *things*, and making no allusion to *persons*.

I will never attempt to decide beforehand what shall be the results of a new experiment, unless it be to show the futility of such anticipations. I will affirm that alone, of which I can produce the incontestable proofs; for I had rather remain stationary for ever, with truth on my side, than advance a single step beyond the limits of reality. My aversion to every species of system is well known; consequently, you will not infer, from my drawing your attention, in a special manner, to the blood, that my object is to revive the system of humorism, and to enter the lists with the partisans of solidism. No, Gentlemen; yet I am far from despising the pages of medical history, wherein figure those obsolete dogmas.

They will serve as salutary guides, by pointing out, when we incline to go astray, the limits that cannot be passed without falling into error. At a period when accurate ideas on physics were unknown, when chemistry did not exist, when the science of the equilibrium and movement of bodies had but just been thought of, and the study of human anatomy seems to have been, as it were, proscribed, Galen was certainly excusable, for adopting the doctrine of humorism, in his eagerness to explain every thing. Much may well be pardoned in a man who, by his unaided efforts, caught a glimpse of one of the most brilliant discoveries of the seventeenth century, the circulation of the blood. At other periods, too, when a new species of inquiry disclosed the vast multiplicity of changes induced by disease in our organs, and in the texture of our tissues, the invention of solidism might be forgiven; but in the present age, with its greatly increased scientific attainments, it would be worse than absurd to espouse either doctrine, although very respectable writers have supported, and still support, one or other of them.

Yet there are questions quite as absurd as those of humorism and solidism, agitated by the present race of medical men. Is it not truly distressing to witness the acrimony with which men dispute on the *essentiality of fevers*, on *the necessity of an organic change, existing or not, in all diseases*, for example? For my own part, I would not waste my time in taking part of such controversies. Is it not as clear as the day, Gentlemen, that to entitle us to decide on the essentiality, or non-essentiality, of fever, we ought first to be acquainted with every phenomenon that takes place, from its outset to its close? But as such acquaintance is, in the present state of the science, utterly beyond our reach, it is impossible for us to discuss rationally and thoroughly the question alluded to. For an example of the error into which people fell in this way, let us take the fever called typhoid. In this disease the mesenteric glands are found considerably enlarged, the intestinal mucous membrane, and especially the follicles, ulcerated, besides many other disorders. Well, they ascribe the malady to these lesions, but they are wrong. These are the *consequences* of the disease, the *anatomical proofs* that it has existed, but they are not its starting point; and it is manifest that with our present knowledge, and with the present mode of studying pathology, that starting point will never be discovered.

What is to be done, then, in this our state of ignorance? Must we stand calm spectators of the ravages of that fatal disease, and of the scenes of mourning and desolation it causes? No, Gentlemen, man has been granted plenary power over the lower animals. Among them there are some resembling him strongly in organisation and functions; and though I deplore the necessity to which we are reduced, the hope of benefiting our fellow-creatures justifies us in experimenting on those animals for the purpose of ascertaining, if possible, the origin and causes of our own disorders. We have already succeeded in producing in them symptoms and lesions precisely similar to those occurring in several human maladies; it is surely reasonable to hope that by perseverance we may be enabled to make out their intimate cause, and learn how to subvert its action.

This is the point of view in which disease should henceforth be studied at the bedside. Would that I could see both students and practitioners convinced of one great truth, namely that the plan hitherto pursued in the study of medicine has been too narrow and confined ever to lead to those happy results that dignify an age, by improving the condition of humanity. Yes, I affirm, that a medical man, unused to guide his practice by the light derivable from chemistry and physics, and who has never made a study of the difficult art of performing experiments on the living animal,—and there are many such,—frequently sees in a series of patients nothing but so many individuals enduring more or less pain—the dying and the convalescent. The utmost limit to which his knowledge goes scarcely enables him to point out among them those who will die and those who will recover. A crowd of phenomena, to which the sciences supply the key, escape him; for, as he does not dream of their existence, of course he does not endeavour to detect them.

So long as I live, Gentlemen, I will labour to expose the absurdity of a set of ideas, circulated, as it would seem, mainly to damp the spirit of inquiry natural to young and ardent minds. The baneful influence of the ideas to which I allude, on the advancement of our art, is notorious. People say, why should I trouble myself to seek the cause of this or that fact, when I can so conveniently explain it by a word? That word, I need hardly say, is *vitality.* True, they cannot define what they mean by this vitality, nor even prove its existence in many cases; but no matter, for all that, it affords materials for chapters without end, and it is infinitely more diverting to invent idle tales about physiology, than to devote one's time to difficult and laborious research. Young men who commence their medical career under the influence of such doctrines as these, learn, as the sum-total of their acquirements, to phrase a few prescriptions neatly, and enter into practice without a notion of the difficulty attending the serious study of medicine.

For my part, I declare loudly that I look on these ideas about vitality, and the rest of it, as nothing more than a cloak for ignorance and laziness; and the outcry raised against experimental inquiry, as the mere result of that hypocritical zeal, which invariably entrenches itself behind pretended considerations of morality and social order. You will, undoubtedly, agree with me, that he who desires to study to good purpose the science of Nature, ought not to be deterred by such arguments as those referred to, from pursuing the path of experiment.

Having thus *legitimised* our plan of study, we will continue our investigations on the subject with which we were engaged at our last meeting. We then established, as you will recollect, that an animal was a compound of solid and liquid parts variously combined. From the class of insects to that of crustacea, the fluids move in a particular manner, which is, as yet, far from being perfectly understood. When examined with the microscope these fluids are observed to hold in suspension certain solid corpuscula, which have been improperly termed *globules;* for that term suggests the idea of a round, globular body, whereas as we shall see, they do not in general affect that form. Passing through the different stages of organisation, we arrive at the class mammalia ; here we find the blood, properly so called, a liquid in which a considerable number of globules are seen to swim. If these globules are modified in their form or size, they lose the power of passing through the capillary vessels; the circulation, consequently, ceases in the various organs, and from its cessation spring the greatest disorders.

Here, then, is one of the properties of the blood ; it is composed of a liquid holding globules of determinate form and dimensions in suspension. I only rapidly enumerate these facts now, for, as you are aware, I devoted several lectures to them in former courses.

There is a great difference between studying the blood during life, and while yet in its vessels, and when removed from those vessels; in the latter case we may ascertain, it is true, its different material properties, such as its specific weight, its degree of consistence, its smell, its colour, and the changes it undergoes from contact with the air, &c. ; but the majority of its important qualities, those necessary for life, disappear the moment it ceases to circulate in its natural tubes.

When blood is drawn from a vein, or artery, of a living animal, and left to itself, a series of chemical, physical, and vital actions immediately commences; it forms into a mass, coagulates, and separates into two distinct parts,—one liquid, called the *serum*, the other red and solid, known as the *clot*. This phenomenon of coagulation is not usually observed during life ; however, I met with a specimen of blood yesterday in a woman that had, probably, coagulated before death. Here is the uterus of the subject in question; you perceive, when I make an incision into it, that the cavities of its vessels are plugged with cylindrical masses of fibrin, while those of the neighbouring parts present no such phenomenon. It is rendered very probable, by a variety of circumstances, that this solidification took place during life ; the evidence of this case, however, is far from sufficient to convince me of the possibility of coagulation occurring during life.

Before the discovery of the microscope, and the successive improvements effected in that valuable instrument, the notions entertained on the composition of the blood, and on its movement through the infinitely small vessels it is destined to traverse, were exceedingly rude ; and even now, if we attempt to analyse mentally that marvellous circulation, it seems incomprehensible how it can take place through tubes of only the 1-80th or 1-100th of a millemètre in diameter. These vessels are called capillary, but the term is altogether deficient in exactness. Compared with such minute tubes a hair is a huge cylinder ; the diameter of a hair bears about the same proportion to that of a capillary vessel, as the trunk of a tree does to the fibrils of its roots; but our wonder is increased when we set about trying to imitate this circulation. It is with the greatest difficulty we can succeed in making water, or any analogous fluid, pass through glass tubes of 1-10th of a millemètre in diameter; if we use tubes of a smaller calibre than this it becomes almost impossible to drive a liquid through them, no matter what force be employed; and we meet with similar resistance in injecting water into the mesenteric artery of a frog, with a view to force its passage into the continuous vein. The fluid employed is, in great part, extravasated into the surrounding tissues, and but a small quantity reaches the desired point.

These facts prove, that in its capability of passing through tubes of such minute size, under the influence of a slight force, the blood possesses a very remarkable property. I am aware that the phenomenon does not give physiologists the most trifling embarrassment,—they explain it with all imaginable facility by the all-efficient vitality. Gentlemen, we know the value of these deceptions; but, be that as it will, this is a point of view under which, so far as I am aware, the blood has not yet been studied, and which is every way worthy your attention. My inquiries, up to the present time, go to prove, that the passage of the blood, from the arterial to the venous capillaries, is effected by means of the nice adaptation of its physical properties to the physico-vital endowments of the vessels. The difficulty of succeeding well in our anatomical injections, confirms this view of the matter. Why is it that they so frequently fail? Simply, because the fluid used bears no relation, in its physical properties, to the tenuity of the tubes through which we wish to drive it. In all probability, the astonishing perfection of Ruysch's injections was owing to his having discovered a combination of substances, which had some analogy, in properties, with the blood, and could, consequently, freely traverse the capillary vessels of the various organs.

I cannot allow the opportunity to pass, without expressing my disapprobation of the narrow-minded selfishness evinced by the anatomist just named, in carrying his secret with him to the grave, after he had turned it to the best pecuniary account during his life. The loss is one much to be regretted. Who knows but that, had we been acquainted with the composition of the fluid he employed, we might have, under various circumstances, been enabled to modify the blood beneficially for our patients, to restore it some of the properties it might have lost, or given it new ones? I am persuaded, that, with our present improved knowledge, considerable advantages might have been obtained, had we been acquainted with the nature of its component ingredients.

But to return; I should now have to prove, in justification of the theory by which I proposed to account for the free circulation of the blood, through the most delicate tubes of the living animal, that if a single one of the properties of that fluid be modified, its movement through the capillary system becomes impossible. But, as this has been already done fully, as I have shown you the mechanism of the various local lesions, produced by changes in the properties of the blood, such as extravasations, œdema, engorgement, inflammation, I shall not stop longer on this point at present But here is an experiment confirmative of what I was saying a moment ago, that is worthy your attention. In this vessel you see some fluid blood, which contains the normal quantity of serum, of globules, and of various salts. In an excellent Thesis by M. Lecanu, the number of distinct substances in the blood is estimated at twenty-five; they are as follow:—

Free oxygen.
Nitrogen.
Carbonic acid.
Extractive.
Phosphuretted fat.
Cholesterine.
Seroline.
Free oxalic acid.
Margaric acid.
Hydrochlorate of potass.
———————— soda.
———————— ammonia.
Sulphate of potass.

Carbonate of soda.
———————— lime.
———————— magnesia.
Lactate of soda.
Fatty acid salt.
Yellow colouring matter.
Albumen.
Water.
Fibrin, } The clot.
Hematosin, } The clot.
Albumen, } The clot.
Globules.

Now, it is very possible that the number may be really still greater; but, counting according to this analysis, there are only twenty-four in the specimen I show you. The absence of one of its normal constituents is not perceptible by any untoward sign; the sample before you appears perfectly identical with the blood that circulates in the living animal. Yet, notwithstanding this apparent similarity, its properties are different; for, if I reintroduce it by a vein, it will at first pass through the large vessels, but, on reaching the capillary system, its progress will be arrested, the series of phenomena I have so often described will succeed, and the animal soon perish of the disorders induced by the stoppage of the capillary circulation. Now, nothing has been added to this blood; I have simply removed from it one of its elements,—an element, too, that at the utmost forms no more than from 1-1000 to 2-1000ths of its volume. That element is fibrin, which, while in the vessels, is liquid, but when removed from them becomes solid; and hence it is to its *fibrin* the blood owes the extraordinary property it possesses of passing through the capillary system.

But this is not the only important fact affecting the fibrin; indeed, were we to take this alone into account, we should fall into a very serious error. Let us suppose an animal whose blood contains fibrin, as well as all its other constituent parts, in the normal proportions. If I inject into the veins of such an animal any substance possessing the property of combining chemically, of forming salts with the fibrin, such as fibrinate of soda, potassa, or ammonia, that fibrin will lose its coagulability. The change in the characters of the fibrin affects the blood generally; it ceases to be coagulable, and the usual consequence ensues. You perceive, therefore, that the blood may contain its just proportion of fibrin, and yet be unfit for circulation.

Observe, I beg of you, Gentlemen, the fundamental point in the theory of the blood deducible from the facts just described,—it is, that in order to support life it must be coagulable; if it loses that property existence is threatened, and ceases within a short while; and this is precisely what occurs in the greater number of destructive epidemics. They are specially connected with certain modified conditions of the blood, that cause it to stagnate in the pulmonary vessels. Such was the state of things in the epidemic—the "*grippe*" —by which we were lately visited.

Now, even at the present stage of our acquaintance with the properties of the blood, we are enabled to take a different view from that ordinarily held concerning the nature and origin of *local* and *general* diseases. In the former case the blood becomes obstructed in the pulmonary capillaries, and a local lesion of those organs,—either apoplexy, hemorrhage, or hepatisation—follows. In red hepatisation, however, the blood does not cease altogether to be coagulable; in fact, the compact, hard, resisting masses formed in the areolæ of the organ, during the course of pneumonia produced by any external cause, are nothing more than clots of blood. But in the *false pneumonia* of the "*grippe*," it is totally deprived of the property of solidifying, is effused into the parenchyma of the organs, and causes the blackish serous infiltrations met with in the victims of that epidemic.

But, let us admit that the altered circulating fluid has succeeded in getting through the pulmonary vessels; it passes to other organs, and in some of them, as, for example, in the intestinal mucous membrane, encounters capillary vessels of still greater tenuity than those of the lungs. The mechanical obstruction here produces redness and ulceration of the follicles; the organs of digestion lose their power of assimilation, and the whole economy suffers from the same shock. You will not, I trust, in the hope of discovering the source of these disorders, set to counting the number of capillary vessels obstructed, or measuring the number and dimensions of the ulcerations formed. No, you will treat with the proper neglect these ridiculous minutiæ; you will examine the blood, and, in its morbid changes, discover the cause of the disease.

Again, in acute rheumatism, as I once before explained to you, the painful parts become the seat of engorgement due to the stoppage and accumulation of the blood in its canals. The liquid stagnates, its temperature falls, and hence the sensation of cold felt by the patient, and which may, in some cases, be felt by a bystander on the application of the hand. The vulgar call this state a *cold*; medical men, an *inflammation;* and really, were I obliged to make a choice of one or other of these terms, I should prefer the former; for it expresses a fact, the latter only an hypothesis. But, whatever be the relative correctness of the expressions, I have for a long time been in the habit of treating my rheumatic patients with acetate of ammonia; and here are the grounds of my practice. It has been shown by M. Béniqué, and others, that acetate of ammonia, when mixed with various liquids, facilitates their passage through minute tubes. Hence, as it appears to me, its admixture with the blood must produce a similar effect on the passage of that fluid through the capillary system. I know not whether my theory be a just one; perhaps I am wrong; but this I am sure—and this is the most important point for patients—that recovery is quite as rapid under this mode of treatment as under various others, which exhaust the powers of the economy and prolong convalescence to an indefinite period. Further, of this also I can assure you, that I have never, either in my hospital or private practice, lost a single patient from acute rheumatism; and it is very remarkable that the patient himself, with a sort of enlightened instinct, surrounds the affected joints with flannel and other substances fitted to accumulate caloric in the parts, and even causes them to be gently rubbed to bring back the absent heat; the utility of these practices lies in their facilitating the capillary circulation.

All this is accurately true; nevertheless, did I assert it in too exclusive a manner, I should, in my turn, be guilty of an error. Nothing is so easy as to deceive oneself into the belief of the universal applicability of any important fact one may be fortunate enough to discover. Against such self-deception it is my duty to guard you. In the present instance I am far from maintaining that all diseases originate in altered conditions of the blood. Such an opinion would be grossly absurd. Our organs are liable to be influenced by a number of agents which directly modify, as it were, their texture. Thus, for example, intense cold causes the contraction of the pulmonary vesicles, and by this purely physical action the circulation is retarded, and may be temporarily stopped. It is not necessary for me to enumerate the series of phenomena that ensues.

On the other hand, if the temperature be too high the capacity of the vessels is increased, and not only is the circulation affected thereby, but the composition of the blood may be modified to such an extent as to produce affections analogous to those just alluded to. Here, then, are two phenomena, perfectly physical in their nature, to which a considerable number of local lesions are traceable.

Hence local diseases may originate either in an alteration of the blood, or of the tissues of the organs themselves. The distinction between these two sources of topical affections should never be lost sight of, especially in regulating the treatment.

There are other causes of local disorders wholly distinct from those I have mentioned. Thus, the chemical injuries effected in the stomach by the concentrated acids will never be confounded, by any one in his senses, with disorders induced by a morbid condition of the blood, no more than will the mischiefs caused by the violent drastics that certain individuals have the signal effrontery to extol as sovereign balms for every variety of complaint.

These cursory observations will suffice to show you that there certainly exist diseases brought on by a morbid condition of the fluids, as well as others by a primitive alteration of the solids; they will, besides, convince you that I am neither exclusively a humorist nor a solidist.

The causes that deprive the fibrin of the capability of clotting reside in the air, in miasmata, in our food and drink, and, in short, in every agent surrounding us that penetrates into the economy, no matter by what route. While engaged in investigating the nature of these causes, we will also study, in this point of view, the action of various medicines of which the frequent use may, as I conceive, entail serious evils. Among these medicines I will only, for the present, mention carbonate of soda, which is in common use as a reagent for the neutralization of the uric acid occasionally deposited in the kidneys, ureters, and bladder. I am now thoroughly persuaded that, even in the case of urinary calculus, the use of this salt, if carried beyond certain limits, may become decidedly hurtful. I found my opinion on these reasons. My experiments have demonstrated the property possessed by this body of liquefying the blood, by combining with its fibrin. Now, I have not a doubt but that its long-continued medicinal use produces similar results on the blood, causes infiltration of the lungs, and acts, in short, so long as persisted in, as an inexhaustible source of pneumonia; at least, I am authorised in this belief by what occurred to a friend of mine, one of the most celebrated men of the present day. He has been compelled to give up taking the carbonate by a succession of pneumonic attacks, that followed each other while he was in the habit of employing it.

LECTURE IV.

Facts in physics illustrative of the circulation of the blood.—Absurdity of medical terms.—Inflammation and irritation.—Coagulation of the blood.—Effects produced when the blood is rendered too fluid.—Action of œnanthic ether on the blood.—Post-mortem appearances in poisoning with this substance.—Viscidity of the blood; effects of this property.—Action and effects of blood-letting.—Changes produced by increasing the viscidity of the blood.—Globules of the blood; their importance in the circulation.

Gentlemen: at our first meeting I solicited your aid in the researches I have undertaken. Your assiduous attendance on these lectures, and the lively interest you evidently take in the novel questions I have brought before you, are therefore particularly gratifying. One of you has just given me some blood taken from a patient; this I will take care to have examined, and if they are worthy of notice, you shall be made acquainted with the results.

I derive much satisfaction from this proof of interest in my labours; I beg its author will accept my thanks, and, at the same time, I would entreat each of you to supply me, from time to time, with specimens of the bleedings you may have occasion to perform, accompanied with an account of the patients on whom they were practised.

For my part, from the 1st of January next, I will cause what I call trial bleedings to be regularly practised in my wards. Exact notes of these shall be kept, as well as of the previous and subsequent condition of the patients. By these means, at the close of the session, we shall have accumulated a large number of facts and observations, which will materially assist us in carrying out the plans we have laid down.

You remember, without doubt, the various facts we passed in review at our last meeting. Those facts showed you the importance of a close acquaintance with the state and composition of the blood generally, and with such morbid changes as it is liable to undergo. They proved, also, that those changes play an important part in the production of a host of diseases, the causes of which appeared an impenetrable mystery. We have but just entered on the study of the blood; and scarcely had we done so, when we were fortunate enough to discover a phenomenon that had previously escaped the notice of both natural philosophers and medical men. Yet that phenomenon is one indispensable for the persistence of life, inasmuch as without it the circulation of the blood must cease.

Heretofore the particular conditions, which enabled the blood to traverse tubes of scarcely a thousandth part of a millemètre, were unknown. In all probability physiologists considered the matter quite natural, and therefore troubled themselves little about it. We think differently, however, and shall, therefore, continue our inquiry into the properties that confer on the blood that marvellous power.

The questions we have so far treated of belong, in great measure, whatever may be said to the contrary, to the subjects of physics and hydraulics. You will find that, in elucidating them, we shall have no occasion to avail ourselves of the so-called contractility of the vessels, which, in reality, does not exist; and we shall be quite as independent of the species of *sensibility*, termed *insensible*, and of the entire collection of absurd terms by the help of which no few romances, doubtless very clever, but very empty too, have been got up about the capillary circulation.

There is a fact, in physics, remarkable for the excellent term of comparison which it serves to establish between the phenomena of the movement of the blood in our organs and the circulation of liquids in inert tubes. I allude to the enormous pressure which is required in order to make water pass through a tube of very small diameter, while the blood traverses with ease the infinitely more minute tubes that abound in our tissues. There must be some particular conditions to facilitate its passage. What proves their existence is, that if certain alterations are effected in the composition of the blood it stops, undergoes morbid changes, becomes extravasated and decomposed, and produces the various disorders which pathologists have vainly attempted to explain by the words *inflammation* and *irritation*. What sense, in truth, is there in applying the words inflammation to our organs? Do our tissues really take fire? I confess I know of no example of such a phenomenon. When the blood rushes to a part in abundance, a certain rise of temperature, no doubt, occasionally follows; but it only reaches a few degrees above the normal standard of the organ, and never exceeds that of the blood in the left ventricle. To cause a real inflammation, the elevation of heat should be carried infinitely higher; besides, in many cases, also called inflammatory, there is a notable fall of temperature. Some of you, perhaps, Gentlemen, accustomed to regard with deference the precepts of the schools, will look on this dislike to their incorrect momenclature as extravagant; but you will think otherwise if you reflect for a moment on the exact and perfectly defined character of the terms employed in other sciences. Thus, in physics, the instrument that measures the weight of the air is not called an hygrometer, but a barometer; and the same precision exists throughout that and all other sciences. Why should medicine, then, be alone without positive terms and technical phrases of precise and determinate signification? Again: What on earth is the meaning of the word *irritation?* An obstacle of some sort or other modifies the course of the blood in any given organ, and instead of simply stating that there is a modification produced by a mechanical impediment to the progress of that fluid, you tell us the part is *irritated*, and actually employ the very term the worst fitted to designate the disorder manifested in the circulation. How long, I should be glad to know, have our organs been proved susceptible of feeling passions—of becoming irritated—I had almost said of getting angry? The medical language of our schools is almost from one end to the other an incoherent metaphor. Figures and tropes have their merit, I allow; but their fit place is in a poem, or a romance, and the science we cultivate will, I trust, cease to be ranked with such performances.

I cannot be surprised that the study of the properties of the blood has been neglected, for such neglect is in harmony with the habitual conduct of medical men. Instead of seeking, by careful experiments, the elucidation of important problems, they waste their time and energy in futile disputes. The verbiage and subtlety of the bar seem to have spread to the members of our profession. Whoever talks loudest and longest usually triumphs, and, of course, settles the matter his own way: he does so often in defiance of truth; but what matters for that? he gets himself spoken of and makes a name. On the other hand, the man who does not possess that extraordinary gift of the tongue, by which white is made black and black white, remains unknown, vegetates, and passes for an ignoramus in the eyes of the public. I really cannot help occasionally falling into these digressions, I feel so strongly the degraded state of the science and the retrograde course that people strive to make it run. I am persuaded that I am doing the student a real service in warning him against the crowd of errors that are obstinately promulgated by medical writers and professors.

But to return. We ascertained that the first condition for the accomplishment of the circulation, was that the blood should have the property of forming into a mass when removed from its vessels and left to itself. This is the fundamental fact to which the majority of those we shall subsequently become acquainted with, will be found referrible. Meanwhile we will endeavour to discover the physiological or chemical circumstances, and the particular substances that influence that phenomenon. There are some that diminish, others that immeasurably increase its activity. Among the former we have already become acquainted with a certain number, such as the alkalies, employed, as I mentioned in my last lecture, for the saturation of uric acid in the kidneys and bladder. The gases we breathe, our food, &c., modify that property also; neither must we forget asphyxia by lightning and by carbonic acid, nor the action of hydrosulphate of ammonia, all of which liquefy the blood with more or less rapidity. Suffer me to remind you, too, of the details of an experiment I performed with œnanthic æther. That liquid, as its name indicates (οἶνος wine, and ἄνθος a flower), is considered by its discoverers, MM. Liebig and Pelouze, to be the principle that gives wine its peculiar aroma, or *bouquet.* In other words, the quality of one wine is superior to that of another. because it contains a larger proportion of œnanthic æther. I leave to the palates of the connoisseurs the experimental solution of one part of the question; but as to the physiological relations of the fluid, here is what I observed:—I caused a drachm of it, mixed with an equal quantity of distilled water, to be injected into the veins of a dog. The animal immediately fell down, with every symptom of drunkenness; but it was not alone in feeling its effects; the assistant who prepared the æthereal solution staggered as he brought it in; and the manner in which he introduced the injection confirmed my idea that the liquor took effect in the form of vapour. The dog remained motionless, became comatose, his respiration grew loud and stertorous, and he died in three-quarters of an hour. The post-mortem examination was made the following day:—On incising the muscles of the trunk, a quantity of fluid blood poured forth from the vessels; it seemed as if no clots had been formed in their interior. The lungs contracted on opening the chest, as in the normal state, nevertheless their physical properties were much altered: they presented the greater part of the signs of engorgement; among others, augmented weight and density; but there was no true hepatisation—a new proof that that particular condition of the pulmonary parenchyma is connected with the coagulating property the blood possesses while living. The heart and great vessels were filled with blood, which was more viscid than that of animals killed by the carbonate of soda, though its appearance was the same to the eye. The internal surface of the vessels was studded with brownish patches, produced by the imbibition of some of the elements and of the colouring matter of the blood : no fibrinous clots were to be found. The liver and spleen were evidently enlarged ; when cut into, a quantity of black viscous blood followed, in greater abundance than usual. The condition of the kidneys and other parenchymatous viscera was the same. The external surface of the stomach and intestines was streaked with reddish lines, radiating in various directions, and forming a rete by their interlacement; these were the capillary vessels distended with fluid blood. There were no extensive patches of extravasation, for death was so rapid that the liquids had no time to penetrate the walls of the intestines by imbibition. Now, let us suppose the body of an individual opened at one of our hospitals, who had been suddenly carried off by some disease of unknown nature (and the catalogue of such affections is a tolerably long one); and let us suppose lesions similar to those described to be discovered, would the observer hesitate for a moment to attribute them to inflammation? Certainly not; he would pronounce the case a beautiful example of gastro-enteritis. The most incredulous by-stander would be forced to yield to the conclusive evidence afforded by the condition of the mucous membrane. Who could have the face to utter a syllable of dissent when he saw that membrane swollen, red, arborised, and covered with a layer of viscid sanguinolent matter? Be that as it will, however, I found no local lesion in the subject of my experiment capable of explaining the severity of the symptoms and rapidity of death. The whole economy was affected ; non-coagulable blood circulated through the system, and, as the walls of the vessels were unable to prevent its imbibition, it deposited its materials in every direction. The first cause of any organic disorder that might have ensued was evidently to be found in the physical modification, the liquefaction of the blood. I have no doubt but that by following up this by similar experiments, we should come at some facts calculated to throw light on the physiological effects of wine. When habitual drunkards fall ill, the phenomena their complaints present are of a special character, and require peculiar treatment. As the sudden introduction of a little œnanthic æther into the veins of an animal destroys the coagulability of its blood, it is by no means impossible but that the prolonged abuse of wine may, in the end, entail similar modifications in the physical properties of that fluid. Much has been written on drunkenness, its effects, and the disorders it induces in the organic functions. The pathological anatomist has examined every organ in its turn in search of the peccant principle, but in vain; in spite of all that has been done, conjectures are all that we have got. Delirium tremens has been attributed to inflammation of the brain, of the cerebellum, of the meninges; but not a word has been said of the condition of the fluids. It will be my aim to investigate these different points thoroughly; they are of the last importance, for it is by examining the condition of the blood that we have learned the mechanism of the production of several of those terrible diseases that decimate the human species. Thus we have been enabled to explain the black vomiting of the yellow fever, that devastates the shores of America. In the first place it is known that the deleterious miasmata, rising from animal or vegetable matters in putrefaction, produce that terrible disorder; but its immediate cause, the middle change between the action of the miasmata and the appearance of the disease, had been unknown. We have now ascertained that that change is none other than the liquefaction of the blood, which, in consequence of its altered state, is effused into the abdominal viscera, disorganises them, causes their sphacelation, and gives rise to the pathognomonic dejections of that frightful disease.

Continuing our scrutiny into the properties of the blood, we soon discover another of its special characters. This is a peculiar viscousness. Now, on first thought, this would seem an obstacle to its passage through the ultimate tubes, but it is, in reality, an indispensable condition for its free circulation; to such a degree, indeed, is this true, that viscousness and normal state of the blood are two inseparable ideas. This property, again, is illustrated by experiments, made on inorganic tubes. If we endeavour to introduce water into a tube of extremely small diameter, that liquid, as I have already mentioned, will not enter it, no matter what force be employed; but if a certain quantity of any mucilaginous substance, such as gum, gelatin, or albumen, be added to it, the attempt at injection becomes successful immediately. This fact has been established by some ingenious researches of M. Poiseuille, the results of which are not yet published. It affords a fresh illustration of the utility of the study of physics in their application to physiology; it shows, too, what little regard should be paid to those arrogant inventors of vital laws, who not only themselves disdain the light shed by the phenomena of physics on physiological actions, but do all they can to prevent the rest of the world from availing themselves of it also.

In spite of whatever vitalists may, in their pompous style, urge to the contrary, I look upon it as incontestable, that the blood circulating in our organs, may be justly assimilated to the fluid in the experiment adverted to. If it lose its viscosity, its further passage becomes quite as impossible as that of the non-mucilaginous water; it stops at the entry of the capillary system, is extravasated into the surrounding tissues, and causes the disorders which those sage personages ascribe to irritation and inflammation. The prevalent mode of treating disease harmonises admirably with, and is quite as senseless as, this method of reasoning in pathology. The practitioner mixes, combines, and jumbles together vegetable, mineral, and animal substances; administers them right or wrong, without considering for a moment the cause of the disease, and without a single clear idea on the why and wherefore of his conduct. You may prove to him, as you will, that this or that substance is insignificant, useless, or even hurtful; little will he regard your expostulation. And why should he, when, by readiness in prescribing a monstrous farrago of drugs, he knows he shall acquire the reputation of being *profoundly versed in the materia medica, of being a man of immense resources, &c.?* Such, Gentlemen, has ever been empiricism.

We were speaking of the viscidity requisite for the circulation of the blood through our organs. Now, here is the blood of an individual who had an attack of hemoptysis, and was bled freely for it. You know well what I think of that remedy,—worse, perhaps, than the disease. Be that as it will, you may perceive that this blood is very slightly viscous; I, in consequence, presume that further mischief will occur. We shall see if my presumptions are realised. We have no instruments to measure the viscidity of fluids; we are, therefore, instead of having an accurate and rigorous estimate of that of the blood, reduced to simple conjectures. I consider that the discovery of any method of measuring it with exactness would be a most valuable acquisition. Meanwhile, we will do what we can to determine it with the areometer. There is further proof that a certain share of viscidity is an essential requisite for the healthy circulation of the blood. If you bleed an animal several times, and replace the blood withdrawn with water, exhalation and effusion into the cavity of the pleura will follow, and, subsequently, into the peritoneum. Now, you have done no more than diminish the viscidity of the blood, by adding a little water to it. But, on the other hand, if you try the converse experiment, and augment its viscidity beyond its natural term, the circulation ceases altogether, in consequence of the affinity between the molecules of the blood being rendered too great. The molecules adhere to the walls of the vessels, and impede the circulation, just as blocks of ice, stuck to the banks of canals, or rivers, interrupt the course of their streams. Hence there must be diseases originating in excessive viscousness of the blood.

You remember the experiments we made to exemplify this question. We added a viscid matter, innocuous in itself,—gum, for example,—to some water, and, after colouring the fluid, injected it into the jugular vein of an animal. So long as the injection traversed the large venous trunks no disorder was occasioned; but once it had arrived through the pulmonary artery, amongst the minute ramusculi of the lung, its degree of viscidity ceased to be in just proportion to the capacity of the tubes. The consequence was, that the circulation almost instantly stopped, and, as the encephalon no longer received the necessary excitement of arterial blood, its functions ceased, and the animal quickly perished. The autopsy was immediately made, and, on incising the pulmonary parenchyma perpendicularly to the direction of its principal vessels, we invariably found them stuffed with the substance injected. Let us admit, however, that this liquid had succeeded in making its way through the capillary system of the lung. You are aware that the diameter of the ultimate tubes varies almost in every organ, and that in some of them it is still smaller than in the lungs. Let us suppose, then, any substance that has passed through the pulmonary parenchyma with great difficulty, arriving at other capillaries of greater tenuity. It will, beyond question, be arrested in its course by this new obstacle; and its stagnation and subseqent effusion will produce, according to the nature of the parts with which it is in contact, various disorders, more or less analogous to those already described. In both cases the cause of the mischief will be the same in principle, namely, the obstacle the circulation encounters from the want of harmony between the globules of the liquid and the tubes to be traversed. It is in a case like this that a precise and even minute knowledge of anatomy would be useful in physiology; but as the former of those sciences is very superficially studied, we are invariably found wanting when the application of anything like profound acquaintance with it is required.

I mentioned gum as a substance that increases the viscidity of the blood; I may add that oil and starch, and all amylaceous matters generally possess the same property. Besides, similar modifications arise spontaneously in certain diseases. Thus, we sometimes meet with blood so extremely viscid, that it has very nearly the same consistence as, to use the language of pathologists, currant jelly. I showed you several cases of this kind, both natural and artificial; and you saw that the most perfect analogy always existed between the phenomena produced by the unknown causes of disease, and by my experiments. In every case in which you find the blood clotted in this manner, you may rest satisfied that the lungs have been the seat of some profound lesion. We have ascertained that the alkalies liquefy the blood; and it is similarly demonstrated that certain acids, sulphuric among others, increase its viscidity, by combining with, and solidifying, its fibrin. Nay, more, I have proved, as in every other case, by experiment, that prolonged inanition produces the same result; the blood loses its aqueous principle, and tends to solidification. We will repeat the experiments from which these important conclusions were derived. Experiments should, indeed, in all cases be repeated with care, for their execution is not such an easy matter as might be supposed, nor is all the world capable of performing them properly. Besides, we shall, perhaps, by their repetition, discover some new facts, which escaped us in their first performance. The density of the blood must, also, be taken into consideration in the theory of the circulation. This property has, it is true, been noted by observers; but no attempt has been made to investigate the results of the different modifications to which it is liable. We know that its density is somewhat greater than that of water; still this simple fact is of little importance; it tells us nothing positive on the degree of density necessary for the circulation. Here is another property—it were to be wished we could find out some accurate method of measuring.

But there are important facts, of another order, totally to be learned respecting the blood. When we examine that fluid, while yet circulating in the living animal, we are immediately struck with its heterogeneous appearance. It is seen to hold myriads of little particles in suspension, rolling on each other, and intermingling in a multitude of ways. These particles, called globules, are known to have determinate dimensions, and to affect particular forms, according to the class of animals in which they are observed. They are elliptical in fishes, reptiles, and birds; in the mammalia they have the appearance of a circular lens These globules deserve our close attention, for when they undergo certain changes they are unfitted for circulation in their tubes. However, it must be remarked, that, of all the elements of the blood, they undergo the fewest modifications in disease. As I before mentioned to you, we must be careful not to draw conclusions from one class of animals to another; to suppose, for example, that these bodies have characters identically the same in fishes, birds, and mammalia. Their real characters are, on the contrary, materially different. In the first-named class of vertebrata the globules are of two kinds, small and large, and have all of them a central nucleus; when washed and shaken in water, their outer investment is dissolved, but the nucleus remains. This is not what is observed in mammalia and birds. In order to obtain the globules in these classes, it is not enough to submit the clot to pressure; we must beat up the blood as it flows into the receiving vessels, just as we should if we wished to separate its fibrin. By these means the globules are precipitated, and by pouring off the supernatant liquid we obtain them in a perfectly isolated state. If examined under the microscope in this condition, they, too, are seen to consist of a sort of investment with a nucleus in the centre. But both investment and nucleus disappear by washing, and the water employed becomes red-coloured, like the serum, in some diseases. From this I conclude, that in mammiferous animals these corpuscula are not formed of an investing substance and nucleus analogous to those of fishes. Sanguineous globules are thus divisible into two classes; those with and those without a nucleus. The former belong to reptiles and fishes; the latter to mammalia and birds. The globules of other animals have not been sufficiently examined in this point of view, to admit of any opinion being formed respecting them. The doctrine I have just stated is, I am aware, at variance with that maintained by some physiologists. I give it to you, however, as the result of the experiments I have made on the subject; I will cease to hold it when it is proved fallacious. It has also been advanced, that the globules are composed of a sort of proper parenchyma. According to this view their superficies are formed of *hematosin;* the parenchyma itself of albumen; the central nucleus of fibrin; but the perfect solubility in water of the globules of mammalia and birds, makes me suspect the correctness of this account.

The point to which I am anxious, at present, more especially to call your attention, is the dimensions of these corpuscula. It has been asserted that their size varies considerably in the human subject, and that, under certain circumstances, their diameter does not exceed the 100th part, the 120th, or even of the 150th part of a millemètre. Their magnitude does vary; but I feel almost certain that their diameter never exceeds an eightieth of a millemètre. Now, this point of limitation to the size of the globules is extremely remarkable from the relation it bears to the diameter of the vessels in which they are destined to move. There is in this an admirable harmony between their volume and the capacity of the capillary vessels; so accurate is it, that I am persuaded, if we injected the blood of a fish, or reptile, into the veins of a mammiferous animal, various disorders in the organs would quickly supervene, from the simple reason, that the size of the globules of the former would be disproportioned to that of the vascular tubes of the latter. I shall have recourse to direct experiment for the confirmation, or the contrary, of this hypothesis. I will procure the blood of a sufficient number of frogs, and inject it into the veins of a guinea-pig, or some other of the mammalia. Meanwhile, there are two facts that give considerable weight to the opinion I have started. They are these,—when we add, to the blood, globules of the feculæ of corn, or potatoes, which are perfectly innocent in their properties, but vary from 1-20th to 1-10th of a millemètre in diameter, we invariably produce an obstruction of the capillary vessels, followed by the consequences with which you are well acquainted. If, on the other hand, we substitute for these globules others similar in nature, but of infinitely smaller size, the circulation continues uninterruptedly. Thus, I injected some of the fecula of the mirabilis jalapa, the granules of which are only 1-300th of a millemètre in diameter, into the veins of a dog; and though the experiment was repeated twice in the same animal, no notable disorders followed. Subsequently a larger quantity of the liquid, containing the fecula, was injected by another person, but as it had meanwhile lost almost all its water by evaporation, the solution had become very thick. The consequence was, that the animal was seized with intense dyspnœa, and died in a few hours; but its death was to be ascribed to the viscidity of the fluid, and not to the size of its granules. The dimensions of those little bodies were the same in the solution that caused death, as in that which had twice traversed the lungs without injuring the animal. But a stronger proof still of the justness of accounting thus for its death is, that the appearance of the lungs was precisely that observed in animals who die from an injection of oil, or any other viscous fluid. Hence, to recapitulate, in a few words, what I have been saying, if we view the globules as corpuscula suspended in the serum, it is fair to conclude that they might, without causing inconvenience, be smaller than they are; but that if their size were increased beyond a certain point, it would constitute an insurmountable obstacle to their passage through the terminal vascular tubes. Their mean ordinary diameter varies from the eightieth to the hundred and twentieth part of a millemètre. There are some considerably smaller, but, in all probability, they are of a different species from those of which we have been speaking; to ascertain this for certain, however, would be a very interesting as well as novel topic for inquiry.

If we turn from these purely physiological questions to the chemical history of the blood, we shall find in it many points quite as interesting as those we have rapidly reviewed. The difficulties encountered in the prosecution of that branch of inquiry are not less serious than those met with in investigating its physical properties. However, it is easy to establish one important fact, namely, that every anormal chemical modification of the blood is followed by morbid phenomena, of just as great gravity as those which have already afforded us subject for discussion.

I may here announce that my old pupil, M. Denis, of Commercy, has made some very curious researches on the chemical composition of the blood; among other important facts, it would appear to result from his labours, that the fibrin is nothing more than albumen combined with different salts. For the present I neither adopt nor reject this opinion,—I wait for the proofs of its correctness. M. Denis has presented a memoir on the subject to the Academy of Sciences. He proposes repeating his experiments in public at the Ecole Pratique, and I strenuously recommend you to attend his lectures.

Among other practical points of importance to which these general views have led us, we have ascertained that venesection modifies the relative proportion of the serum and clot. Here is the produce of the fourth and fifth bleedings, which were practised on the animal I showed you the other day. In the fourth the serum is to the clot as 55 : 45; in the fifth as 65 : 35. Now this is surely a very great difference; yet in this case the various abstractions of blood took place at intervals of two days, and the animal was fed well all the while, which prevents any very rapid alteration of the blood; and not only is the clot modified, but the serum also becomes whitish, and is pretty often found covered with a layer of opaline matter.

In fine, the nature of the blood, and of its different elements, is an important question in a therapeutical point of view. We can now maintain, with confidence, that it is not a matter of indifference whether we bleed little or much; whether we draw a small or a large quantity of blood in a very short space of time, or at distant intervals; whether we push the abstraction to syncope, as has been advised by many writers, or repeat it to a smaller extent, at several different times, with a variable interval of time between them, as learned practitioners of the present day recommend. We are also justified in proclaiming, that men who bleed without giving themselves the least uneasiness about the disorders that follow the removal of blood, both in that fluid itself, and as their consequence, in the various organs; who look on these disorders as curable by blood-letting, while they are, in reality, produced by it, act with most reprehensible blindness. In simple language they do mischief, where they imagine they are doing good; and in many an instance, on their doing that mischief or that good, the death or recovery of their patient depends.

LECTURE V.

Effects of carbonic acid on the blood of man and animals.--Modifications of the blood induced by temperature.—Experiments of Hales.—Illustrations.—Experiments of the lecturer.—Treatment of cholera.—Effects of injecting cold water into the veins.—Changes in the blood during chlorosis.—Obstacles to the study of the pathology of the blood.

Gentlemen:—We will commence our studies for to-day with a case of asphyxia, the details of which have been furnished me by M. James. You will find that the facts elicited by its examination afford a very curious confirmation of some of the principles we have already deduced from our experiments. The patient was a female suffocated by the fumes of charcoal; here is some of her blood, which, you observe, is quite fluid. I will not, for the present, examine into the cause of its fluidity; but the case gives us an opportunity of verifying in the human subject the mode in which such blood affects the condition of the organs. I have explained to you at length the serious consequences of non-coagulability. I have told you that, when so affected, the blood cannot traverse the capillary vessels, or, at least, that its circulation through them is materially modified; that it stagnates in the parenchyma of the organs, is extravasated, alters their appearance, and sometimes renders them utterly unfit for the performance of their several functions. For my part, whenever I meet with blood of this description, I feel certain that the lungs have been the seat either of engorgement, hepatisation, œdema, or apoplexy; and if its fluidity be still greater, I feel almost positive that there has been effusion into the cavity of the pleura. This shows you how important our investigations may become in a diagnostic point of view. By proceeding methodically, and not yielding to the numerous obstacles we encounter, we reap the satisfaction of gradually accumulating facts, the knowledge of which is calculated to advance the science of pathology. For example, it was formerly the habit to ascribe the various disorders I have just enumerated to a certain morbific matter borne along through our organs, and depositing in them the germ of irritation. This irritation was at first supposed to be slow in its progress, but subsequently to require full development, and give rise to an incomprehensible something termed inflammation. This latter was said to be complicated with the prostration or superexcitation of the vital properties, wherewith all the tissues of the enonomy had been bountifully supplied. But, Gentlemen, it has been shown to demonstration, that the empire of inflammation is even more widely extended than it was the habit to assume; and what is remarkable, it is I who, like a generous enemy, have conferred on it powers, of which even its very warmest partisans have deprived it. *They* limited its action to living organs; *I* have extended it to the tissues when they have ceased to live. Many and many a time have I proved, by experiment, that its most terrible symptoms develop themselves in parts *wholly inanimate*. See the absurd conclusions that are drawn, naturally enough, too, as far as the process of deduction goes, from the majority of the physiological and pathological doctrines of our time. Turn where they will our medical men find nothing but vitality, just as if the organisation of the human frame were not quite as worthy of our admiration, regulated by physical laws, as by a jumble of pretended vital properties. They will not allow a membrane to be a membrane; they refuse to believe that when a membrane is brought in contact with liquids, either imbibition or exhibition follows. No, that would be too simple; they must have exhalant vessels, and absorbent vessels, and they must endow these vessels with intelligence, and make this intelligence decide on the fitness of this or that substance to enter their cavity. Experimental inquiry gives the most striking refutation to all these reveries. But it has a hard task to effect the triumph of truth; so certain is it that a single fallacious idea, emitted by chance, may retard, for an indefinite period, the progress of the sciences. People are generally persuaded that physiology, as taught in the schools, has made extraordinary progress within the last thirty years; but a little reflection shows that unfortunately this is a complete mistake. What difference, in truth, is there between the archœus of Van Helmont, or the monstrous conceptions of Paracelsus, and the vital laws and properties admitted by our modern schools? I see no more than a change in words, the ideas remain unaltered; each man has done his best to show his talent, and make a name, by expounding these imaginary laws but nothing more; and, assuredly, they might have chosen a better way to ensure a solid reputation than by battling and disputing about words; it would have been a surer and more simple plan to avow their ignorance, and seek in silence the causes of such phenomena as seemed inexplicable. But let us return to our subject, and examine the lungs of this woman, whose blood we found in a liquid state; the organ is, as you perceive, of a blackish-red colour, and on pressing it between the fingers only very slight crepitation is detected, an evident proof of its imperfect impermeability to the air. The colour of these brown spots resemble that of blood deprived of all contact with atmospheric oxygen. When I cut into the parenchyma of the organ, you see that black blood is extravasated into it in greater or less quantities, according to the part we examine. There are, no doubt, here and there, a few points of more florid tint, but that is the result of insufflation, as I shall presently explain to you. Thus, in the same manner as the altered appearance of the lung might have led us to anticipate the condition in which we found the blood, so the modification of that fluid perfectly accounts for the state of the lung. Here you see exemplified the close relation that exists between the organs and the liquid that circulates through them. The blood, from a certain cause, ceased to come in contact with atmospheric air, it in consequence almost immediately lost one or more of its normal properties, and the lung, in its turn, suffered from the modified characters of the blood. Stagnation and infiltration took place, and it is probable that, had not the duration of the disease been so limited, the disorders induced would have been still more serious.

Previous to the lecture, and without knowing that this case was about to be communicated to me, I had asphyxiated an animal with carbonic acid gas. This is a fortunate chance; we will now avail ourselves of it, by comparing the two cases, so as to discover their points of difference and resemblance.

In this animal, the subject of my experiments, death supervened in a few instants; the blood will, therefore, I doubt not, be but little altered.

[M. Magendie here commenced the autopsy.]

Here is the lung; the blood contained in its vesicles has exactly the same tint in every part of the organ as that of the woman of whom we have been speaking. When insufflation is practised you see that its whole surface assumes almost instantaneously a florid red colour. The reason of this phenomenon is simple; the blood had stagnated for a short while only in the pulmonary areolæ; hence it has not altogether lost its essential properties; it still retains that of reddening perfectly under the influence of the oxygen of the air. On the other hand, insufflation had little effect on the organs of the human subject, whose death was more lingering; the florid hue produced in them was comparatively pale, and existed only in isolated patches. This fact will throw some light on the study of organic lesions, for the different conditions of the blood may actually point out *the age of the disease*. You observed that it was completely liquefied in the woman suffocated by the vapours of burning charcoal, and, in consequence, there was considerable sanguineous infiltration of the pulmonary tissue; hence, I presume, that the sufferings of the poor victim must have lasted several hours. In the animal, on the contrary, death occurred rapidly, and the lung neither presents the same disorders, nor the blood the same degree of fluidity. Here, if I mistake not, are clear and precise applications of the general results to which our researches have led. I had never before had an opportunity of observing, in so direct a manner, the action of carbonic acid on the coagulability of the blood. We now know that that gas liquefies the blood, not only the portion of it circulating through the lung, but its entire mass. We may legitimately go further than this; the phenomena of exhibition will account for a number of lesions in the other organs that are, beyond all question, primarily produced by the liquefaction of the blood. In my opinion, various morbid changes pointed out by pathologists as occurring in analogous cases, such as dark colour of the intestines, erosions of the mucous membranes, effusions, sanguinolent diarrhœa, &c., are, in ultimate analysis, traceable to the same cause. I would, therefore, strongly recommend you to turn your serious attention to facts of this character: examine them with severity and minuteness; if they are, as I believe, real, they both simplify the science and may lead to valuable discoveries capable of changing the face of pathology.

We may now change our subject, and turn to the consideration of other phenomena that have been but little studied, though they are quite as important as those with which we have hitherto been engaged. I allude to the different modifications induced in the blood by variations of temperature. These may be of such a kind as to cause a complete stoppage of the circulation, though that fluid have lost none of its chemical characters or normal properties. This fact has alone been established by experiment. Still, in respect of its connection with the generation of organic changes, medical men have paid it but little attention; they are so absorbed with irritation and inflammation that they think of nothing else, and yet the temperature of the blood deserves to be seriously examined in this point of view. A recapitulation of what is known on the subject cannot be otherwise than useful. Hales, an exceedingly clever natural philosopher, and a man of genius besides, made a great number of experiments on this subject. Their object was to prove that the movement of liquids through tubes was rapid in the ratio of their temperature: the greater their heat, the more swift their circulation. Here was one of his ingenious experiments: he fixed an injecting syringe to the mesenteric artery of an animal, after having first secured the vein of the same name in such a manner that he was enabled to collect and measure the exact quantity of liquid brought back by it. The anastomotic branches in the neighbourhood were also carefully tied. Matters having been thus arranged, Hales states that he ascertained the following facts:—First, cold water pushed into the artery returned by the vein in a given time, but I cannot at this moment recollect the exact figures. Secondly, the same quantity of liquid, of a medium temperature, passed 18 times quicker under the influence of the same pressure. Thirdly, when hot water was employed, the velocity of its passage was found to be 32 times greater than that of the lukewarm fluid. Hales has not mentioned the exact temperature of the liquids with which he experimented. If we suppose that there was a difference of 60 deg. Centigr. between the liquids he called cold and hot, we should find that the water flowed in the latter state with 576 times greater velocity than in the former. Every one is aware that during very hot weather the face is more highly coloured than at other times, and the movements of the heart more rapid. A greater quantity of blood is constantly passing through the vessels, and the cutaneous and pulmonary exhalations are very sensibly increased in quantity; hence the craving we feel for aqueous drinks, in order to restore to the blood the water it is constantly loosing. But it is on the capillary circulation, more especially, that the temperature of the blood exercises a very manifest influence. A voluminous artery is more easily traversed by a column of blood than a minute branch. In proportion as the diameter of the vessel decreases the obstacles increase. It is easy to estimate, comparatively, with glass tubes, the mechanical difficulties originating simply in the narrowness of the vascular canals to be traversed ; but we cannot with these form any accurate notion of the influence of temperature; and this because the variation of heat acts not only on the blood in the capillary vessels, but dilates the membranous walls of the latter, enlarges their cavity, and modifies the hydrodynamic phenomena. Pulsations are felt where they did not exist before; several globules now pass a-breast in the tubes, which scarcely allowed of the passage of one; in a word, the new state of things discloses itself for the investigation of the physiologist. The condition of persons labouring under an organic affection of the heart furnishes a good illustration of this. Cold, damp weather is sure to bring on fits of suffocation in such patients, as well as infiltration and œdema of the lower extremities. Let the atmospheric temperature undergo a trifling rise, and immediately the circulation becomes freer. The symptoms just named then disappear until the return of cold weather. The sufferers themselves are so perfectly conscious of all this that, when you prescribe for them, they tell you a little sunshine would do them infinitely more good than all your drugs. They are right.

When, again, the practitioner wishes to increase the activity of the circulation in an individual, and facilitate the passage of the blood through the whole vascular system, he orders a warm bath: its effects are soon felt through the entire frame. The respiration becomes accelerated, because as a greater quantity of blood arrives in the lungs within a given time, a greater quantity of oxygen is required; the tissues swell and the external surface grows red. Again, if the object be to diminish the activity of, for example, the cerebral circulation, and to increase that of distant parts, you make use of pediluvia. The phenomena ensuing are precisely the same as in the preceding case, except that they are simply local. Instead of acting on the entire vascular system, you limit the action of the elevated temperature to a few of its tubes. This, too, is an exclusively physical result. Let us take an example of the opposite kind. An individual puts his hands and feet in snow; the fingers and toes immediately become white, in consequence of the reflux of blood to the central parts of the limbs. The capillaries of the periphery of the body, when submitted to the action of sudden cold, become unfit for carrying on the circulation. Part of the fluid they contained passes into the veins; and as they no longer admit that which the arteries carry to them, they are really for a moment empty. M. Poiseuille has given the details of several experiments referring to this subject, in his essay on the capillary circulation. He has clearly shown that any notable fall of temperature in those exquisitely minute tubes arrests the course of the blood in them: the globules stagnate and remain immoveable; but when acted on by heat in this state, their oscillation recommences; they assemble in the centre of the vessels, glide over each other, and the circulation is once again in activity. The blood in the living vessels is, in truth, almost as directly influenced by the temperature of the atmosphere as the mercury in the barometer. But how, we may naturally ask, are we enabled to outlive intense frost? Why does not our blood cease to flow, in the same manner as the water of rivers? Because we are provided with an apparatus destined to keep that fluid in a state of constant and equable warmth. Respiration, independently of its other uses, raises the temperature of the blood by the chemical combination of oxygen with the molecules of that fluid, and by increasing the velocity of its course prevents its solidification. Although these facts are generally known, you will easily conceive that the science would gain materially by our having positive information on the exact amount of influence temperature exercises on the circulation. When we say that it renders it slower or more rapid, there is not enough of precision in our language. We should be able to tell how much slower or more rapid the movement of the blood becomes. With the view of attaining some such accuracy as this, I have planned some experiments, which I propose to perform to-day. First, however, I must give you an account of one I performed yesterday. I was anxious to determine with M. Poiseuille what degree of pressure the vessels support, according as the temperature of the tissues to which they are distributed is low or high. We employed the hæmodynamometer, substituting a solution of the subcarbonate of soda for the mercury, in order that the variations in the level of the liquid might be well marked. The specific weight of corbonate of soda being to that of mercury as 1 : 10, the slightest oscillation of the column was apparent. As the amount of pressure in the veins is much less than in the arteries, it is frequently advantageous, when experimenting on the former sort of vessel, to use this modification of the instrument. We proceeded in the following manner:—The tube was introduced into the femoral vein, with its point towards the capillaries; the paw and the rest of the limb had been immersed previously for a quarter of an hour in a frigorific mixture, composed of two parts of ice and one of common salt. We turned the cock of the instrument, and in 55 seconds the carbonate of soda rose from 0 to 670 millemètres. We then removed the apparatus, and replaced the frigorific mixture with compresses saturated with water at 50 deg. (Réaum.) At the end of half an hour we reintroduced the tube as before into the vein. On opening the cock we found that the subcarbonate took only 38 seconds to rise from 0 to the 670th millemètre of the scale.

The movement of the blood was, therefore, evidently rendered slower by the cold, quickened by the rise of temperature. We should have had still more strongly marked results, had we applied the instrument to a superficial vein, or polonged the action of ice or cold water. We made the experiment on a deep-seated vein, and acted with our artificial heat and cold on the peripheric circulation only. We will now vary our process. Here is an animal whose limb has been surrounded with a mixture of ice and salt for the last three-quarters of an hour. There must be already a very notable diminution in the progressive force of the blood; but of this we will assure ourselves by the hæmodynamometer charged with carbonate of soda. I now introduce the tube of the instrument into the vena saphena interna, with its point towards the capillary system; the column of saline solution stands at 0. I now turn the cock. You observe that the column rises, but so slowly that any motion in it is with difficulty discernible. The force now moving the blood is evidently much inferior to that in action under ordinary circumstances; not that the energy of the contractions of the left ventricle has diminished, but the mechanical obstacles are more numerous, and in consequence the liquid does not pass so freely from the capillary arteries to the vein. The experiment has now lasted eight minutes, and the upper surface of the column is at the 360th millemètre of the scale; yet the degree of cold is far from intense; a thermometer placed in the wound indicates a temperature of 15+0 (Réaum.).

[The experiment was continued to the 11th minute; the cock being then closed, the carbonate of soda was found to stand at the 620th millemètre.]

We will now perform the converse experiment. I remove the ice, and substitute for it a linseed-meal poultice, warm and thick, just such as would, in hospital phraseology, be called emollient.

[The animal was now left to itself, in order to give the poultice time to act.]

I now replace the tube in the saphena interna; we shall see what time the column of fluid will take to reach the height of 620 millemètres; you remember that in the last instance it took eleven minutes to reach that point. I turn the cock, and you observe that the column rises much more rapidly than in the former experiment. A minute has scarcely elapsed, and the scale already marks 150 millemètres. The pressure is, therefore, sensibly increased. The carbonate of soda continues to ascend in the tube, but its movement apparently becomes slower. Three minutes nearly have passed, and yet it has only reckoned 300 millemètres. I know not to what to ascribe its stoppage, but it certainly stands still at that point. The heart continues to beat, the respiration to go forward. Possibly a clot has formed in the instrument; of this we will assure ourselves. I withdraw the instrument, and turn the cock, yet you see that none of the saline solution escapes. I blow into the upper orifice of the tube, and, as I suspected would be the case, I have driven out a clot; the liquid now flows out freely; so that there was a mechanical obstacle to the ascent of the column of liquid. The experiment, therefore, though not terminated, is perfectly favourable to our doctrines; for, as far as it went, it confirmed the theory founded on physical laws, which I laid down.

From these and similar experiments it follows, that by raising the temperature of a limb we increase the force with which the blood moves in the veins; this fact did not escape the observation of medical men of all ages, and even of non-professional persons. What follows the instinctive action of rubbing the hands when numbed with cold? Disengagement of caloric, and, in consequence, increased velocity of the flow of the blood though the capillary system. The deepened colour of the tissues shows that the liquid moves in greater abundance, and with more facility. But a number of therapeutical principles rest upon these physical data. If you wish to determine a flow of blood to the skin, you prescribe *douches* and the vapour-bath. If a limb becomes cold from having had its principal artery tied, you surround it with bags filled with hot sand. I am well aware that these means are, for the most part, empirically employed. The effect obtained, what matters the cause, you will say? Much; it is repugnant to my reason, I had almost said to my pride, thus to accept *consequences* without inquiry, when it depends upon myself to ascertain their *cause*. I seek, as far as possible, to be both the hand that executes, and the intelligence that comprehends.

It was during the awful epidemic of 1832 that I felt most fully the utility of sound physiological knowledge in guiding therapeutics. In individuals seized with the cholera, the diminished energy of the circulation, and the blue and cold condition of the body, were prominent phenomena; to them, therefore, I gave my chief attention. On the arrival of the patient in my wards he was placed in a thoroughly-heated bed, his limbs rubbed with stimulating liniments, and bags of hot sand applied along the body; internally he was given hot drinks. While I endeavoured by these physical means, to raise the temperature of the blood, I also gave my attention to supporting the contractility of the heart, and for this purpose employed spirituous and aromatic draughts; and what we see in our experiments with the hæmodynamometer, I have many a time had occasion to verify in the human subject, namely, the re-establishment of the circulation, and in fortunate cases the recovery of health through the artificial restoration of animal heat. On the other hand, cold baths are recommended in cases where the indication is to soothe general excitement, characterised by excessive activity of the circulation. When the forehead burns, and the temporal arteries beat violently, we apply ice, or some frigorific fluid, to the part. When too much blood flows to the brain, we have recourse to the cold affusion; cold, therefore, skilfully managed, and applied opportunely, is an energetic therapeutical agent.

The treatment of fractured limbs, by constant irrigation with cold water, furnishes another example of this mode of action. The prolonged continuance of a low temperature diminishes the vascular pressure, prevents the blood from rushing in the natural quantity towards the seat of the lesion, and, in a word, prevents the phenomena called inflammatory from taking place. When we plunge into cold water, during very warm weather, a peculiar sort of constriction is felt at the thorax. This, again, is a purely physical phenomenon. Less blood traverses the capillaries, consequently it accumulates in greater quantities in the larger trunks; these, distended as they are, press on the neighbouring organs; hence the uncomfortable sensation of which the patient is conscious. It disappears when the equilibrium of temperature is restored throughout the sanguineous system; and so the sudden immersion of the whole body renders its effects much less sensible. Again, to take another instance, what do you suppose occurs, physiologically speaking, when an ice, or a draught of very cold water, is swallowed? We have not investigated with the instrument the effects of the introduction of a cold liquid into the veins; but I do not believe that I shall widely err in saying, that the pressure supported by the vessels must diminish. The drinking iced water, must depend on its rendering the circulation slower; but as all ingested fluids remain in the stomach some moments, before they are imbibed by the coats of the veins, their temperature must have risen by the time they enter the circulation. In order, therefore, to ascertain with accuracy the effects of directly cooling the blood, we will inject cold water into the veins of the animal. The instrument is, as you see, applied to the right carotid of a middle-sized dog. The jugular vein is laid bare, and a tube secured in its cavity covers the extremity of the injecting syringe. The temperature of the water we are about to inject is 3+0 (Réaum.). I almost fear the introduction of such very cold fluid may at once determine serious functional disorders. The syringe is capable of holding 100 cubic centimetres of liquid. The mercury now oscillates between

70—75 ; 65—70 millemètres.*

Let us see if the pressure will be diminished or increased in the artery; I presume it will be lessened, but the experiment has never been tried.

* The equivalent of the millemètre in English measure is 0.039370 inch.

1st injection : 70—85 ; 68—75 mill.
2d injection : 60—80 ; 75—85
3d injection : 65—80 ; 70—85
4th injection : 65—85 ; 70—80

It would appear, Gentlemen, that instead of diminishing, the pressure is slightly augmented. I certainly expected quite a different result. I continue the operation.

5th injection : 60—85; 65—85 mill.
6th injection : 70—80; 65—115

The last ascent of the column was caused by a violent effort the animal made at that instant; but it is clear that the pressure is not weakened by the action of cold water; the mercury mounted, on the contrary, a few millemètres after its introduction.

I should be curious to examine the temperature of the blood. We will remove the tube, and collect a little in a vessel. Here is a small quantity, and it marks, as you perceive, 31° Centigr.; hence its temperature is 7 or 8 degrees lower than the normal standard.

7th injection : 75—85; 70—85 mill.
8th injection : 65—80 ; 65—85
9th injection : 65—85; 70—80
10th injection : 60—75 ; 65—85

The mercury remains at the same level without any notable variation. My surprise at not seeing the column full is doubly great, inasmuch as, theoretically, I should have admitted two causes of diminution in the vascular pressure—the temperature of the liquid and its debilitating action. You perceive how the animal is swollen by the accumulation of the successive injections. The coats of the arteries are extremely tense; there is a sort of aqueous plethora.

[M. Magendie now injected fresh quantities of the water; but the mercury still oscillated between 65 and 85 millemètres.]

I have just injected for the eighteenth time, and yet the pressure is not sensibly modified. Nearly two litres* of cold water have passed into the circulation, which is more than enough to produce the effects I contemplated. The result of the experiment is, I freely confess, exactly the reverse of what I had expected. The hæmodynamometer has once again corrected my theories; it were to be wished, indeed, that every clinical or physiological professor had an instrument of the kind by his side, to ascertain the real solidity of his conjectures. Unfortunately there is no such instrument; but if there were, some time would, no doubt, elapse before it came into general use; few people would like to have so indiscreet a contradictor constantly beside them.

I will now beg to call your attention to the influence of the blood on the development of certain physiological phenomena, that have excited considerable interest among clinical observers since the

* The French litre is equal to 1.760 pints.

time of Lænnec. No one had, however, traced them to their true source; for the generality of medical men, unacquainted, as they are, with physics and chemistry, pay no regard to the community of character existing between the normal or anormal phenomena of our organisation, and those of physico-chemical science. Yet, if it be desirable that medicine should cease to be a mere trade, that it should take the foremost place among the departments of human knowledge, their habitual indifference must be got rid of, and they must learn to call to their aid every description of inquiry capable of widening the field of their information, and of smoothing the path to ultimate truth.

Well, let us suppose a chlorotic female admitted into a hospital. On examination we find that the skin is soft and flabby; that all the tissues present a pecular kind of whiteness, and that the patient feels constantly chilly. These phenomena prove the existence of an alteration in the blood, as well as that the capillary circulation is imperfectly performed. In order to remedy these organic disorders, the practitioner bleeds the patient, and gives preparations of iron, which according to the current belief, possess the property of recomposing the blood. But farther a particular sound is discovered on ausculting the heart and principal arteries. Now, this sound has been carefully described, and called *bruit de soufflet*, *bruit de diable*, *&c.*, but no attempt has been made to find out how it has been produced. I have lately proved, Gentlemen, that this sound depends on the nature of the blood; my experiments place the point beyond all doubt. Thus, when we examine into the audible effects of the sudden passage of water through inorganic tubes, we find that only a very slight rubbing sound (*bruit de frottement*) is produced; but if we add to the fluid either albumen, starch, or common salt, a sound is then emitted pretty closely resembling that heard in the arteries of chlorotic subjects; and what strengthens my conviction of the correctness of this purely physical explanation is that when the chlorotic state goes off the sounds disappear also. From this I necessarily conclude, that they depend on the composition of the blood, and not on any particular condition of vitality and vascularity, which has, in reality, no connection with them. I intend to repeat my experiments relative to these phenomena, for their importance cannot be doubted. Professor Bouillard has already made an application of this idea by seeking in various ways to estimate the relation existing between the state of the blood and the sounds of which we have been speaking. Among other attempts of the kind, he has measured the viscidity of the blood with the areometer, and has, I believe, ascertained that when that quality is developed beyond a certain degree, those sounds cease to be produced.

The blood is an extremely complex liquid, as may easily be learned, by casting an eye over the numerous treatises that have been written respecting it. As regards the chemical part of the question, a good deal has been done; but if we turn to its physiology and pathology, we find that the very foundation has yet to be laid; but even the few scattered facts heretofore noted, show the importance of its most trifling alterations in the production of various maladies. We are daily consulted by patients who complain, some that they have a "disease in their blood," others that their blood is too thick. Although there is neither science nor merit in these old woman's notions of our patients, still experience proves that there is something of truth at the bottom of them. It would almost appear, indeed, that medical men, persist, from mere pettishness, to deny the existence, and neglect the study of causes, which the most ignorant persons have divined, simply because they are uninfluenced by the prejudices and the disputes of the schools.

One of the obstacles which interferes most materially with the direct and thorough study of the blood, is the difficulty of examining it in its proper vessels. It is a very different thing to study it with the microscope, while yet within those tubes, or after it is removed from them. No matter what expedients you employ, during the short transit from the vascular tubes to your receiver, the component elements of the fluid are found to affect a new arrangement. The simplest proof of this, that when newly-drawn blood is examined, it is sometimes found wholly coagulated; at other times partly liquid; the clot may even float in the midst of serum: in a word, it presents itself under a variety of aspects; besides, it is impossible to look upon it as a perfectly identical fluid in the same individual, even during a few consecutive hours. It is unceasingly destroyed by excretion and exhalation; and as continually it is renewed by the chyle, by drinks, &c. So that, examining it in the midst of these continual movements of composition and decomposition, it is almost impossible to obtain it in a fit state for comparative study at different periods. The influence of drinks on the blood, especially of those that contain an excess of carbonic acid, is almost instantaneous. Scarcely have we swallowed a glass of Champagne when the blood is modified by venous absorption. In order to establish the reality of this modification I propose getting some one to drink a bottle of Champagne voluntarily, and a few moments after practising venesection, to the extent of one or two ounces. Beer produces very nearly the same effect. Scarcely has one swallowed a glass or two of that liquor, when the urine becomes increased in quantity to such a degree as to require immediate expulsion. These considerations are enough to show that the analysis of the blood is attended with difficulties of a peculiar kind, such as are not met with in the analytic examination of other liquids, such as mineral waters, for instance.

In consequence of this double movement of composition and decomposition in the blood, it becomes necessary to admit in it two principal series of elements. First, those really forming it, which we will call *constituent*; next, those that simply pass through it, which we may term *transitory*. The same distinction holds with regard to our food; some part of it remains as a formative element of the frame, the rest disappears by exhalation, especially that from the lungs. The blood, while circulating, is liquid; but what shows, beyond dispute, that it must not, for a moment, lose the property of coagulating is, that its solidification is necessary for the production and formation of our organs. When, therefore, it becomes unhealthily fluid, it is rendered unfit for nutrition.

I placed before you, a few days past, the list of substances detected in the blood by modern chemistry. You must have been struck with the variety of those materials,—gaseous, fluid, and solid. One of the most remarkable among them is the phosphuretted fatty matter, analogous to that of the nervous substance, and, like it, causing instant death, when re-introduced into the blood by injection. M. Pinel Grandchamp states, that the injection of half-an-ounce of this substance into the veins of a furious buffalo, proved almost instantaneously fatal. Now death, in this instance, is a purely mechanical result; this I demonstrated clearly last year. The nervous pulp acts solely by obstructing the minute capillary tubes; it is otherwise inoffensive.

Among the more novel substances detected of late in healthy blood are cholesterine, the well-known material of biliary calculi; seroline, a yellow and a blue-colouring matter. But there is one assertion of M. Lecanu which I am disposed to combat; that gentleman believes, and MM. Prevost and Dumas formerly supported the same opinion, that the fibrin forms part of the globules. It would be impossible to study the blood with any success in its physiological and pathological relations, if the fibrin were looked on as forming part of the globules. If we take the blood of an animal, and let it remain undisturbed, it becomes solid; if, on the contrary, we shake it, or whip it, as the butchers do, it will differ from other blood by our having, through the process alluded to, separated from it an elastic, whitish matter, which is nothing else than the fibrin; and if we examine it in this state, we find the globules exactly as they were, unchanged either in volume or in form.

It is not denied that, if the blood be left at rest, the globules will be found, on analysis, united to the fibrin; but, as when their association is mechanically interrupted, we still find the globules just as they were before, I cannot admit, until some new evidence be found in their favour, the ideas of the physiologists to whom I have adverted. There is another question involved in that just mooted, of still more delicate character, namely, What is the condition of the fibrin in the serum? Is it in solution or suspension? The latter appears to me the more probable of the two. I believe that the fibrin, instead of being a sort of precipitate in the blood, exists in that fluid in *the form of minute vascular arborisations*, forming, in a certain sort, the first degree of organisation. But the problem is by no means solved on account of this preference on my part.

LECTURE VI.

Effects of the injection of cold water into the veins.—Description of the hæmodynamometer.—Pressure of the blood equable throughout the system.—The arterial pressure nearly the same in different animals.—Causes of variation of pressure.—Action of different substances on the heart.—Exciting effects of coffee.—M. Poiseuille's theory.—Laws respecting the circulation.—Depressing action of digitalis.—The blood does not possess any innate motile force.

GENTLEMEN:—At our last meeting I attempted to verify, by experiment, the influence which I had, in theory, attributed to direct refrigeration of the blood on the movement of that liquid. Generalising the observation we had just made on the external application of ice, I injected cold water into the veins, under the impression that I should thereby diminish the pressure of the blood; but this, to my great astonishment, augmented instead of decreasing. Such a result was too directly contradictory of my views for me to rest satisfied of its correctness, without having recourse to further trials. I, therefore repeated the experiment this morning, and shall now describe the result. The mercurial hæmodynamometer was placed in the femoral artery of a middle-sized dog, and a tube adapted to the jugular vein, for the injection of water at +4° Centigr. The syringe contained 100 grammes. Before the introduction of the liquid the scale marked from 60 to 100 millemètres. As soon as the animal became perfectly calm we commenced. During the first five injections the level of the column of mercury underwent no notable variation; between the sixth and tenth it rose a few millemètres. We then introduced a thermometer into the cavity of the abdomen, and found the temperature +34° Centigr. The tissues traversed by the blood were, therefore, not very materially cooled, though a litre of cold water had already passed into the circulation. I now became desirous of trying, in the same animal, what effect the injection of hot water would produce. The oscillations of the mercury were carefully noted down; and here is a summary of what we observed. The column, which stood between 60 and 115 millemètres, fell, during three injections of water, at +45° Centigr. to

55—70; 65—80; 50—55 mill.

Towards the fourth, it rose to

80—90; 85—95 mill.

The fifth, sixth, and seventh, caused a still more rapid ascent. The scale marked

100—120; 115—140; 125—145 mill.

At the eighth the mercury suddenly redescended to

60—80: 55—75 mill.

I was at a loss to know to what I should ascribe this spontaneous diminution of the pressure, when I perceived that the animal had just had an abundant miction. The explanation of this phenomenon of vital statics will appear simple, if you reflect on the influence exercised by the parietes and viscera of the abdomen on the movement of the blood. The change in the state of the bladder, from fulness to vacuity, gives you the mechanical reason of it. The dog, enormously swollen by repeated injections, seemed now on the point of expiring. After the ninth the column had fallen to 30—45 millemètres. Finally, it descended at the tenth to 25 millemètres, and remained stationary at that point. The heart had ceased to contract. On opening the chest we found that organ very considerably dilated with fluid, as we had anticipated would be the case, from the dull sound elicited by percussion of the precordial region. I was fortunate enough, just then, to make an important remark. Having made an opening in the femoral vein, I pressed the walls of the left ventricle with my hand, and produced a jet of blood perfectly analogous to that observed in the arteries during life. The flow of blood was, at once, jerking and continuous. Such, indeed, was the resemblance of the two phenomena, that had I been guided simply by the character of the flow, without taking into consideration the colour of the liquid, I might easily have fancied that the animal was still living. The column which had stopped at 25 millemètres, fell now still farther to 17, 16, 15, 11 millemètres. There was, consequently, a certain amount of pressure in action even after death, due, no doubt, to the abundance of fluid contained in the vessels. This experiment agrees in result with that performed at our last meeting. I had predicted that the elevated temperature of the injected fluid would increase the pressure; the pressure decreased. I had predicted that a fall in the temperature of the liquid would diminish the pressure; the pressure increased. If we wish for accurate results respecting the influence of cold and heat, in its relations to the force with which the blood presses against the walls of the vessels, the first few injections only should be taken note of. The augmented pressure occurring at the end of an experiment, is to be ascribed not to the temperature of the injected liquid, but to its volume. Nearly two litres of distilled water had passed into the circulation.

It appears to me that I cannot, now that I am engaged with the hæmodynamometer, do better than briefly recapitulate, more particularly for the advantage of those among you who have attended my former courses, some of the more important facts it has served to bring to light. I will commence with a description of its structure. It has received its name from the purpose to which it is applied, namely, the measurement of the force that moves the blood (αιμα, blood, δυναμις, force, μετρον, a measure). It consists, as you perceive, of a glass tube presenting a horizontal branch, a descending vertical branch, and a third ascending branch; these are curved, so as to form a quarter of a circle and a semi-circle, at two different points.

A certain quantity of mercury is placed in the tube, and it is evident that when the instrument is in a vertical position, the upper surface of the mercury will be on the same level in both branches. But if the blood be allowed to enter into the horizontal branch by an orifice, which we suppose to communicate with the interior of a vessel, it will press on the surface of the mercury; the metal will, therefore, be depressed to a certain point in the descending branch, and will rise to the same extent in the ascending branch. The degree of depression and elevation is estimated by the two scales, graduated by millemètres, which are attached to the vertical branches of the instrument. When the apparatus is about to be used, the horizontal branch is filled with a saturated solution of the subcarbonate of soda, which M. Poiseuille found, after a variety of trials, the best material for preventing the coagulation of the blood. A small brass tube receives into a concavity the extremity of the horizontal branch of the glass tube, and is fixed to it with Spanish wax. At the other extremity of this little brass tube is a screw, intended to pass into another brass tube, one extremity of which presents a cavity of the same form as the nut of a screw; the other is free, intended for introduction into the interior of the vessels, and provided with a prominent rim. As the slightest inclination in the instrument would cause a variation in the height of the column of mercury, a leaden wire is adapted to it, to secure it in an accurately vertical position. When we wish to bring the instrument into communication with the blood, we lay bare an artery, seize it between the fingers, taking care first to pass a ligature round it, and make a longitudinal incision beyond the compressed point. The lips of the wound in the vessel are next laid hold of with a forceps, and separated from each other, so as to render the orifice as circular as possible. The tube is then introduced, and the artery tied below the rim of the instrument. The moment we cease to compress the vessels between the fingers, the blood passes from it into the tube, mixes with the subcarbonate of soda, and so transmits the force of its impulsion to the column of mercury.

M. Poiseuille has conferred a material benefit on the science, by devising this instrument for the estimation of the blood's pressure. You will readily fancy the numerous errors to which the observer was exposed, when endeavouring to calculate it by the tension of the walls of the vessels. Suppose we take two tubes, made of caoutchouc, one thin, the other thick; it is evident that if you press them between the fingers, you will receive from each a different sensation. If they are filled with liquid you will find it impossible to estimate the degree of pressure that each supports, by the resistance of their coats alone. It will require greater force to dilate the thick than the thin tube, and you will be exposed to the error of ascribing to the pressure of the liquid what in reality depends on the difference of their structure. The case is exactly the same with arteries as with caoutchouc tubes. It is evident, from the laws of hydrostatics, that the least pressure, acting on the extremity of the horizontal segment of the hæmodynamometer, will be felt through the entire apparatus; the column of mercury will rise in the ascending branch to the same extent that it falls in the descending one; and so the total force with which the blood moves in an artery will be represented by the weight of a cylinder of mercury, the base of which is a circle of the same diameter as that of the artery, while its height is equal to the difference between the points at which the two surfaces of the mercury stand. In order to attain still greater precision, we must deduct the pressure of the small column of subcarbonate of soda.

The first important proposition established by this instrument is, that the pressure acting within the arteries of any animal, is the same at every point of the entire system. Take an artery close to the heart, another at some point remote from that organ; the instrument applied to each of these vessels will be affected to exactly the same extent in both cases. M. Poiseuille made the experiment on the carotid, and on a muscular branch of the thigh in a horse. Notwithstanding the enormous difference of the two tubes, in respect of their diameter and distance from the heart, the displacement of the mercury was exactly the same in both instances. This equality of pressure through the entire arterial system is an extremely important fact, in a practical point of view. It shows that, if the practitioner desire to lessen the quantity of fluid in circulation, it is of little consequence what vessel he opens; for the equilibrium of pressure is simultaneously re-established in all the vascular tubes.

So much for the vascular system of the same animal; as there is only one agent of impulsion, there is certainly no apparent reason why the force with which the blood moves in any artery should be different from that by which it is influenced in the others. But if you apply two instruments, one to a horse, the other to a dog, does it not seem extremely probable, *à priori*, that the column of mercury will rise to a different height, under the influence of the arterial pressure, in the two animals? Must there not be the same difference between the degrees of pressure of the liquid as exists between the volume and energy of their respective hearts? Such was the very natural supposition of M. Poiseuille, when preparing to perform the experiment on large animals, such as the horse; and he had, in consequence, provided himself with a very long tube, capable of indicating a pressure of several atmospheres. But what was his astonishment when he saw the mercury rise only to the same height as in the case of the dog! He subsequently applied the apparatus to other animals, equally disproportionate in point of size and strength, and invariably with similar results. So that a heart, weighing three or four ounces, transmits the same amount of pressure to the walls of the vessels, as one weighing six or seven pounds. These are, no doubt, most curious facts; still they are comprehensible, for the question of which they involve the answer, is not to calculate the total force of the heart, but the surface of the column of blood displaced. It appears that in very small animals, such as rabbits, guinea-pigs, &c., the mercury rises in the tube to about the same height. There is one circumstance capable of exercising a slight influence on the exactitude of the results of these experiments, which requires to be mentioned. A certain quantity of blood becomes, of necessity, mixed with the subcarbonate of soda, and as this blood is removed from the total mass of fluid set in motion by the heart, the pressure exercised on the arterial coats must be thereby diminished. This trifling loss of blood is a matter of insignificance in large animals, but in small ones it must not be forgotten in our calculations.

Now, that we are acquainted with the method of estimating the pressure of the blood, let us inquire into the circumstances capable of causing variation in it. Here is a caoutchouc tube filled with liquid, though not to such an extent as to distend its walls. Occasionally circumstances occur in the living economy, wherein the arteries, though full of blood, are not dilated; but such are exceptional cases. I inject some more water into the tube; in proportion as the liquid enters, its walls are distended; when I draw the piston back the distension decreases. This is a very simple phenomenon. In the same manner it sometimes happens that the blood contained in the vessels becomes more abundant in quantity than natural, and consequently increases the pressure. It may be reduced by removing a certain quantity of fluid; and this is what you do, without reflecting on it, when you open an artery. Bleeding not only diminishes the pressure in the vessel opened with the lancet, but, at the same time, lessens that supported by the totality of the vascular system. We will now examine this pressure in the living animal. The circumstances that modify it are referrible to two principal causes, the mass of liquid in movement, and the force of impulsion. By augmenting or diminishing either of these mechanical influences, we produce a proportional increase or decrease in the pressure acting within the vessels. We must also take most accurate note of the movement of respiration. The animal, on which our experiment is about to be made, is, you perceive, perfectly quiet; besides, every precaution has been taken to prevent his movement from disturbing the apparatus. Two instruments have been applied; one to the left carotid the other to the femoral artery of the same side. The mercury stands at the same height in both divisions of the instruments, because the cocks are closed. I turn them, and immediately the oscillation of the columns commences. You have, without doubt, already remarked that the height of the mercury is less in respiration, greater in expiration, whence we must infer, that the force with which the blood moves in the arteries is diminished during inspiration, augmented during expiration. Observe, too, that every time the animal coughs the column rapidly ascends above its ordinary level. No one has ever thought of placing the action of coughing among the causes capable of accelerating the movement of the blood, and yet the hæmodynamometer shows clearly its great influence on the progression of that fluid. To this accidental cause are to be attributed those sudden elevations of 10, 13, 20, or 30 miliemètres; the heart has nothing to do with them. That organ is the constant agent of the circulation; but the respiratory movements exercise so powerful an influence in this way, that during deep expiration the force that moves the arterial blood becomes almost double as great as in the normal state. The struggles of the animal have ceased, and the respiratory movements succeed each other regularly; so that the ascents and descents of the column of mercury appear to oscillate to the same extent above and below a fixed point of the tube; the scale marks 100—110, 80—105 millemètres. The results given by the two instruments correspond exactly. In order to ascertain the precise amount of pressure we have only to take the mean of the extremes furnished by a number of experiments. From the identity of effects on the mercury in both instruments we learn that a molecule of blood moves with the same force in the carotid, as another in the femoral artery. Now that the equality of pressure in the two vessels is indisputably proved, we will apply the instrument to the elucidation of other points. This subject is, indeed, so new, that I am not aware of any person, except M. Poiseuille, having undertaken such inquiries; and yet the simple ascent of the mercury, by the impulse of the blood, resolves a problem, in the attempted solution of which Borelli and Keil vainly exhausted the science of figures and of algebraic calculus. This, by the way, is a new proof of the superiority of the experimental to all other methods of pursuing physiological research. We know that the liquid contained within the vessels cannot increase in quantity without those vessels suffering under augmented pressure. We can, therefore, in all probability, cause at will the elevation or descent of the column of mercury, by adding to, or subtracting from, the circulating fluid, given quantities of liquid. The volume of the latter will be the measure of the variations of pressure.

Now that the animal is again at rest, the column marks 80—105; that is, it oscillates over a space of 25 millemètres, which gives for the two columns 50 millemètres. I will now vary the experiment described this morning. I expose the jugular vein, and apply a ligature to its upper end, to prevent loss of blood. I introduce into the lower end the extremity of this syringe, filled with tepid water; it holds about a quarter of a litre. I push the piston gently, but the mercury remains at 80—100, 85—105 millemètres. Possibly the quantity of fluid injected is too trifling, relatively to the total mass of the blood; I will, therefore, push some more into the jugular. The column has distinctly fallen, and now marks only 35—50, 35—60, 30—45 millemètres; so that, far from causing an elevation of the level of the mercury, it produces a sensible fall. So true is it, Gentlemen, that soberness in conjecture is imperative on the experimentalist, unless he wish to obtain, with few exceptions, results at variance with his announcements. In this instance I was pre-occupied with the influence exercised by the volume of liquid in circulation on the arterial parietes, and forgot, for a moment, the principal agent of impulsion, the left heart. Our injection, it is true, augments the pressure, but the introduction of a notable proportion of water into the blood diminishes the energy of the heart. What we gain in one way is lost in another; and in five the mercury, instead of rising above, actually fell below its previous level. This experiment allows of a new and unexpected inference, namely, that the volume of liquid does not contribute so much as the energy of the heart's contractions to the arterial pressure. If you weaken, in any way, the contractile force of that organ, you will in vain increase the quantity of circulating liquid; the former influence will more than counterbalance the latter, and the mercury will fall, as you have just seen. As the animal is still in a fit condition, I will make a further experiment, by injecting an aqueous infusion of coffee into the jugular, in order to ascertain the action of that liquid on the force of the heart's impulse. It is generally known that coffee is an excitant, and that it increases the activity of the circulation; but as, so far as I am aware, it has never been directly introduced into the veins, I am curious to see its effects on arterial pressure. In theory it should accelerate the ventricular contractions, but I cannot say it will at the same time increase their energy. Of this we shall judge; the experiment, however, will not prove as conclusive as it might be, as the circulating fluid has already been diluted with water, which will render the presence of a new liquid less sensible. The scale still marks 30 — 45 millemètres; the slight elevations you observe from time to time, depend on the struggles of the animal. I now inject about two drachms of coffee into the jugular. You perceive that the respiration is already accelerated; the pulse is more frequent and stronger than before, and there is a state of general excitement. The mercury oscillates between 45—50, 40—50, 50—65, 70—75, 85—90, 60—90 millemètres. A notable rise has, therefore, taken place; the column now stands at 70—105, millemètres. I have no doubt but that the ascent would have been much more considerable had we made the experiment before the injection of the water. To complete our experiment, and render its results more applicable to the human subject, I will add a small quantity of brandy.* I must first mix it with half its volume of water; for, in the pure state, it would coagulate the blood and stop the circulation. Possibly this precaution is superfluous, for, as I had the cup of coffee and glass of brandy from an adjoining café, it is not impossible that before the latter reached us, it had already undergone the process of dilution. I fill this little syringe, which scarcely holds a drachm. The injection is now performed,

* M. Magendie alludes to the very general habit of his countrymen of taking a small quantity of brandy either with or after their coffee.

yet I do not observe any marked difference in the level of the mercury. The column which was at 60—95, varies now between 75—80, 70—80, 65—90 millemètres. I repeat the injection of an equal quantity of brandy; but the result is still insignificant. The alcoholic liquid has not, therefore, the same influence on the circulation as coffee; it would even appear that the mercury has fallen a few degrees. But these experiments need to be repeated before they acquire real scientific value. No conclusion can be drawn from a single observation, more especially when its results are not accurately defined, and when the subject to which they refer is then for the first time investigated. I will now bleed the animal, for the purpose both of relieving the disturbance we have produced in its functions, and of seeing what effects the removal of blood will entail in the arterial pressure. I open the jugular: very little blood flows, which is easily accounted for by the presence of the instrument in the corresponding artery. As the circulation is intercepted in the latter vessel, the vein receives no more blood. Besides, I tied the carotid on the other side a month ago. The same obstacle exists in the femoral artery; nevertheless I will open its attendant vein, for, from its large size, it will no doubt furnish more blood, and we shall not be obliged to make the dog undergo an additional operation. I prick it now with the point of a bistoury; the blood escapes but slowly, and in small quantity. The column, which stood at 75—90, now marks 75—85, 75—80, 70—85, 65—80 millemètres. The fall in the mercury is, therefore, very trifling; this, no doubt, depends on the flow of blood not being rapid enough to cause a momentary interruption of the equilibrium of pressure in the circulating system.

The general theorem established by M. Poiseuille is this:—*The total static force which moves the blood in an artery, is exactly in the direct ratio of the area of the circle of that artery, or in the direct ratio of the square of its diameter, no matter what be its position in the economy.* You perceive how we should proceed to ascertain the force of any given vessel, the aorta for example. All we should have to do would be to take the square of its diameter, which would give the surface of the circle; multiplying by the mass of the column of mercury, the base of which is represented by the surface of the circle, we should ascertain with mathematical precision the force of the left ventricle. It would, of course, be also necessary to take into account the variations caused by the movements of respiration, by the struggles of the animal, by the volume of fluid in circulation, and by the activity of the heart's contractions; for all of these modify the impulsion of the column of blood. An attentive examination of the pulse shows that that impulsion is not always of identical force.

In spite of the great information furnished by the hæmodynamometer, there are still several points in animal hydraulics which must, in the present condition of our resources, remain in obscurity

The velocity with which the blood moves has not yet been accurately determined; we are obliged to content ourselves with approximations. There is an instrument used for measuring the rapidity of the current of rivers, called *hasta reometrica*, by means of which the velocity of the stream may be calculated. This instrument is a sort of funnel, the wide extremity of which is turned towards the current, so as to let the water rush into it. A tube fitted to its narrow end marks the degree of ascent of the liquid, and furnishes the means of calculating the rapidity of its course. The column rises to a different height when the instrument is kept at the surface or at the bottom of the river; so that the different strata of fluid do not flow with the same velocity. It were much to be wished that we had some means of estimating the degree of rapidity of movement through the numerous divisions of the circulating system; more especially as we should then be enabled to allot to both the pressure and the velocity of the liquid, the influence that respectively belongs to them. You are aware that the pressure and velocity are not always exactly proportionate. The interest of these questions is not merely scientific; a number of useful precepts, bearing on the practice of medicine and surgery may be derived from them. When, for instance, you tie an artery, the pressure you thereby remove from one point of the system, is distributed through its entire extent. The vascular apparatus forms a chain, of which all the links are, in this respect, reciprocally vicarious. Thus, when you interrupt the circulation in the chief trunk of the lower extremity for the cure of popliteal aneurism, the skin almost immediately acquires a burning heat, the face becomes injected, the pulse beats with force, the patient complains of flushes of heat; in short, all the signs of superactivity of the circulation ensue. This train of symptoms cannot be ascribed to the simple fact of a painful operation having been undergone: undoubtedly the wound has something to do with it, but the chief source of the fever is to be found in the modification of the hydronamic phenomena of the circulation. The force brought into play by the contraction of the left ventricle struggles in vain against the resistance of the ligature; but that force is not uselessly expended, it is divided into as many partial forms as there are sanguineous tubes, and increased the energy of the pressure in every part of the arterial system. Hence the fulness of the pulse; hence the pulsation of which the patient is conscious; and which surgeons, who are generally more dexterous as operators, than able as physiologists, have been so embarrassed to explain. There are a number of cases wherein the circle of the circulation is contracted. When you amputate a leg or thigh, you diminish, by a fourth or a third, the extent of that circle; the pressure is, in consequence, increased as the tubes on which it acts are diminished in number. These mechanical notions will find their application in the precautions which it is expedient to take before and after operations of this nature. They prove that it is wise to disgorge the vessels artificially of some of their contents, in order to obviate the effects of the sudden augmentation of internal pressure. Under such circumstances blood-letting may be of great service; managed with intelligence and discretion it becomes a powerful auxiliary in therapeutics.

I will now show you that by diminishing the extent of surface over which the blood flows we increase its force of progressions. This I affirm boldly, for the experiment has already been made by M. Poiseuille. When I only hazard a conjecture, my tone will not be so firm. You have often heard me denounce the pretensions of those men who aim at anticipating the results of experiments. Trust me, Gentlemen, I will endeavour to join the example to the precept. I have now introduced the point of the instrument into the left carotid. The cock is closed, so that both columns remain stationary and on the same level. Now that the animal is tranquil, we will allow the blood to communicate with the interior of the tube. You perceive the ascending and descending movements of the mercury; the extreme degrees answer to forcible efforts of respiration. It is difficult to separate, even mentally, what belongs to each of the powers that concur in producing the arterial pressure. The volume of the liquid, the action of the heart, and that of the lungs, are so many mechanical agencies, whereof the several influences coalesce and are confounded together; according as one or other predominates, the height of the column undergoes notable variations. In the normal state respiration appears to exercise the chief influence, to such a degree that during very deep inspiration the force that moves the blood seems to be rendered almost null: in compensation it is nearly doubled in the corresponding expiration.—The scale marks 60—90 millemètres; if we exercised pressure on the opposite artery it is possible the column would rise to a greater height. However, this difference, if it exist, will not be very considerable; for, as the pressure is divided among all the arterial tubes, it can only be slightly marked in any single one. I now lay bare the right carotid, and surround it with a ligature which I can tighten or loosen at will. We shall, in this manner, be enabled to judge of the influence exercised on the total static force of the blood, by the interruption of its course in a point of the vascular system. I now tighten the ligature: the column oscillates between 75—100, 75—105 millemètres. I loosen the ligature, it falls to 75—95, 70—90 millemètres. I again tighten it: the mercury rises forthwith to 85—105, 80—105 millemètres. I now remove it, and the column descends to 75—90, 75—85 millemètres. Hence it is evident that the degree of pressure differs according as the right carotid is or is not permeable to the blood. The phenomenon would be still more marked, if the experiment were made on a larger vessel, such as the aorta. I shall not open the abdomen for the purpose of applying a ligature to that vessel; such an operation could so seriously affect the animal that no accurate conclusion would be arrived at. There is a more simple way of proceeding; one which has been successfully adopted in the uterine hemorrhage of parturient women; I mean compression of the aorta through the abdominal parietes. Accordingly I press in the direction of the course of the vessel, and am now certain that the passage of blood through it is interrupted, for no pulsation is to be felt in the femoral arteries. The tube marks 90—115, 95—120, 95—115, 100—120 millemètres. I cease to make any pressure, and the mercury falls at once to 80—100 millemètres. I felt convinced, indeed, from the result of the preceding experiment, that when the force moving the blood in the aorta is suddenly transmitted to one-half the arterial system, the pressure in that half must be notably increased. These are phenomena of which we have both the experimental and theoretical certainty.

As I am anxious to investigate thoroughly the effects of the introduction of water into the circulation, I will once again vary the experiments already made. When I injected the jugular vein, the liquid was obliged to pass through the right side of the heart, the lung, and the left side of the heart, before reaching the aorta. This rather circuitous route prevented us from ascertaining the direct effects of the injection. As the water had touched the fibres of the heart and diminished their contractile force, before mingling with the mass of the blood, it acted first as a debilitant; consequently the trifling increase in the volume of the liquid was insufficient to compensate for the loss of energy of the impelling agent. Instead of injecting by the veins I will now inject by the artery, and push the liquid against the normal course of the blood. The experiment is simple: I surround the right carotid with two ligatures,—one intended to tie the upper end of the vessel, the other to secure the lower end to the tube of the syringe. The injected fluid cannot reach the capillary divisions of the artery, because I shall drive it in a contrary direction; nor will it reach the interior of the left ventricle on account of the resistance of the sigmoid valves. It must, then, of necessity, mix with the blood of the artery, and be, with it, distributed through the general mass of vascular ramifications. The upper end of the carotid is now tied. Before puncturing the vessel I will press the lower end, so as to remove the blood in a portion of the cylinder from the influence of the heart. The incision is now made: the scale marks 80—100 millemètres. Now, watch carefully the variations in the height of the mercury, while I gently push the piston. If I pushed it abruptly or forcibly, we might ascribe to the action of the liquid *per se*, what, in reality, depended on the progressive movement I gave it. The syringe is now half empty, and the scale marks 100—115, 90—105, 100—125 millemètres. The moment I cease to press on the piston, you see that it rises from the pressure of the blood; each jerk of the piston corresponds to an arterial pulsation.

The syringe is now almost wholly filled with blood mixed with water, which accounts for the fall of the mercury to 75—90 millemètres. I push the piston again, and the column rises to 90—105 millemètres. I draw it back, and the scale marks 55—75, 60—70 millemètres. I reintroduce the blood, and we find the mercury at 75—85 millemètres. It is plain, therefore, that the pressure increases or diminishes in the direct ratio of the quantity of liquid. You remark that the elevation of the column of mercury is much less sensible at the close than at the commencement of the experiment. M. Poiseuille very judiciously observes, on this, that a certain quantity of water has now returned by the veins to the heart, and consequently weakened its force of contraction. However trifling the quantity of liquid may be, it must unquestionably diminish the energy of the muscular fibre. I now tie the artery through which I drove the injection. If I had not introduced a debilitating fluid into the circulation, I have no doubt that the mercury would now have risen about its normal level in consequence of my having tied the right carotid. The extent of the circulation being lessened, an increase of pressure within the vessels would necessarily follow. The diminished force of contractility of the heart can alone explain the weakened pressure of the blood, in the present instance.

It would be a very interesting subject for inquiry to determine with the hæmodynamometer, the action of the principal medicines employed in practice. Among the substances whose action on the heart is most firmly established, certainly ranks digitalis. When taken in certain doses it diminishes the frequency of the contractions of the ventricles to such an extent that I have known the pulse of some patients to fall, under its influence, to 12 or 15 in a minute. I do not believe its effects have ever been studied with the desirable degree of precision. I will inject a small quantity of tincture of digitalis into the jugular vein of this dog. In all probability the heart's pulsations will fall below their normal rhythm; at least in theory such a result may be anticipated. I must first ascertain the existing number of arterial pulsations: they are, I find, about 120 in a minute. The mercury oscillates between 70—95 millemètres; it has almost returned to its original level. I now incise the integuments in the direction of the jugular vein, so as to expose that vessel. You see that it is small, collapsed, and scarcely apparent. This is easily accounted for by the presence of ligatures on both carotids. As the blood is no longer carried to the head by those arteries, it cannot return to the heart by the corresponding veins. As the circulation is interrupted in the jugular, I should have foreseen that it is unfit to receive the injection; I shall, therefore, introduce it by another route. All membranes not covered with epidermis, absorb; we could, therefore, cause the liquid to enter the circulation by placing it in the abdominal cavity. In the dog the tunica vaginalis communicates with the peritoneum: this explains to you my object in dividing, as I now do, the skin of the scrotum, and introducing the point of this Anel's syringe. I inject about a drachm of the tincture of digitalis. We must now wait a few moments, for the liquid must be absorbed before it can manifest its effects. The pulse is already less frequent, it is now

90 in a minute. The mercury stands at 70—90, 70—95 millemètres. So that the diminished frequency of the heart's pulsation is unattended with either increase or decrease of the pressure. The pulse is now 84; the mercury oscillates between 75—100 and 70—90 millemètres. The action of the digitalis is manifest, for the arterial pulsations have fallen from 120 to 84. Possibly its effects would have been still more marked, if we had brought the liquid into contact with a membrane endowed with stronger powers of absorption. I inject half a drachm into the pleura. Unfortunately the animal begins to grow wearied by our experiments; its struggles disturb the circulation and render it impossible to appreciate accurately the action of the drug. The pulse now beats 100 in the minute. We cannot infer anything very important from this experiment. Its most remarkable feature was the diminished frequency of the beats of the heart unaccompanied by any variation in the arterial pressure.

In enumerating the most apparent causes of the static force of the blood, I did not include a faculty of spontaneous movement without the influence of any mechanical agency, which has been ascribed to that liquid. I regard the notion as an absurdity, fitted at best only to excite a smile. When extracted from its vessels the blood has no other vital or physical force than the force of inertia. It is like any other body composed of inert molecules,—in order to produce a movement in it there must be an external agent of impulsion. Enclose some blood in a knot of chicken's intestine, or in a coautchouc tube, and you will find that not the slightest spontaneous displacement will ever occur. In truth, Gentlemen, it requires to have lost one's eyes to maintain that the blood has a motor power inherent in its nature. Such an idea is a mere hallucination.

LECTURE VII.

Effects of pressure on the parietes of the blood-vessels.—Considerations on transfusion.—Action of pain in increasing the pressure.—Effects of galvanism.—Nature of the buffy coat of the blood.—Differences between the fluid in and out of the body.—The fibrin of the blood different from that of muscle.—Effects of bloodletting.

GENTLEMEN:—If you take a caoutchouc tube, and force into its cavity more liquid than is required to fill it accurately, its walls yield, and are dilated. In proportion as you add fluid, the distension increases; in other words, the interior pressure augments in the direct ratio of the volume of the injected liquid. Now, it appears to me quite natural, that the arteries should be subjected to the same physical law; nevertheless, this is a view which, I admit, stands in much need of confirmation. I have already attempted, in your presence, to increase the pressure, by adding to the mass of the blood; but the mercury, instead of rising in the trials I made, underwent a considerable fall. It seems probable that the opposition between the anticipated and real results of the experiment, is to be attributed to the nature of the liquid injected. The volume of the blood was, no doubt, rendered more considerable than before, but the energy of the heart's contractility was diminished by the presence of water. My injection had the same effect as the diluents used in practice, for the purpose of moderating the violence of fever; the augmentation of the aqueous part of the blood weakened the force of impulsion of the left ventricle, and the pressure in consequence became less than previously through the entire system. I propose resuming this experiment to-day, but in a modified form; instead of introducing water, or any other debilitating fluid, into the circulation, I will make use of blood drawn from an animal of the same species as that on which our experiment is made; in fact, we will practise transfusion; and it affords me some satisfaction to have an opportunity of performing that operation before you,—one which enjoyed, in former times, the highest degree of reputation, and which has now fallen into the most complete discredit. Nevertheless, circumstances may arise wherein it would be proper to have recourse to it: I have myself many times injected medicinal fluids directly into the veins of the living subject. At the period of its discovery the operation of transfusion was received with a degree of enthusiasm bordering on frenzy. Some imprudent essays, however, made on our own species, were followed by the most disastrous results, and then the new discovery, on which a multitude of flattering illusions had been founded, was abandoned with as much inconsiderateness as it had been practically adopted.

I am of opinion that there was too much hastiness shown, both in the acceptance and in the subsequent total rejection of this last resource of medicine. It would be indispensably necessary, before introducing the transfusion of blood into the regular practice of surgery, to submit its performance to strict rules, due attention being also paid to the physical properties and composition of the liquid employed. The globules have not the same volume, or form, in different animals; the degree of coagulability of their blood is not uniform. If the blood be introduced by one process the circulation continues free; if by another, its continuance will become physically impossible. Here, as in every other experimental essay, it is right to study the results of the operation in animals before we venture to apply it to man.

There are two principal methods of performing transfusion; in the one the blood is allowed to pass immediately from the vessels of the animal that supplies, into that of the animal intended to receive it; in the other it is first collected in a vase of some sort, and then injected. The latter method is more generally adopted than the former, because it is easier of execution, and allows the operator to measure the quantity of fluid transfused. In the present instance I intend employing it. I must, however, observe, that so great is the plasticity of the blood, especially in dogs, that the mere contact of the metal of the syringe suffices to destroy its fluidity, and, in consequence, to render it inapt for circulation. Still, as it is not our present object to investigate the modifications produced in the living hydronamic apparatus by the composition of the contained liquid, but simply by its volume, the circumstance adverted to will not seriously interfere with the results of our experiment; so that, instead of introducing the extremity of one vessel by a sort of invagination into the interior of another, I will use an intermediate instrument; I will also modify, to a slight extent, the ordinary mode of proceeding in another way. Instead of venous I will inject arterial blood, taken from an animal of the same species, into the vascular system of this dog. This will, I believe, be the first instance in which the influence of the volume of the circulating liquid on the interior pressure supported by the vessels, will have been directly examined. The contractility of the ventricular fibres ought, in theory, to remain unchanged; to undergo neither augmentation nor decrease of energy, because the blood conveyed to the heart will be neither more debilitating nor more exciting than habitually.

Here are two dogs, of about equal strength, only one is older than the other. The right carotid of both has been laid bare, and the instrument applied to each by M. Poiseuille himself. In order to avoid tiresome circumlocutions, in describing the experiment, we will call the dog from which we abstract the blood No. 1; and the animal into which we inject it No. 2. The jugular vein of the latter has been exposed, and a canula fitted to receive the point of a syringe introduced into its cavity. The columns of mercury stand unmoved, because the cocks have not yet been turned. I now turn them; the mercury in both oscillates pretty accurately between the same points. Thus, we have in

No. 1. 75—100; 75—100; 70—90; 65—95 millemètres.
No. 2. 70—105; 75—105; 65—100; 60—105 millemètres.

It appears, then, there is no very marked difference in the height to which the mercury rises in the two cases. If both animals breathed isochronously, in such manner that the movements of inspiration and respiration should exactly correspond, it is probable that both scales would mark the same degree. I had forgot to tell you that the left carotid of the dog No. 1, has been laid bare, and an opening made in it, for the purpose of receiving the mouth of the syringe. These preparations were made before the lecture, for they would have taken up too much of our time, had they been adjourned till the time of performing the experiment. There is another advantage gained thus,—the animals have had time to recover, to a great degree, from the state of agitation into which they were thrown; besides, the operation is so simple, that it is quite enough to have seen it performed once, in order to understand it fully.

I now aspire some of the blood contained in the carotid of No. 1, by raising the piston of the syringe. The latter fills; and, indeed, to produce this result, there is no necessity for any effort on my part; for such is the energy with which the heart drives the blood forward, that the piston is raised at each contraction of the ventricle. This might even serve as a means of estimating the force of impulsion of the blood, though, of course, the information thus obtained would be only vaguely approximative compared with the mathematical precision of the results obtained with M. Poiseuille's instrument. I, therefore, called your attention to this phenomenon rather as a curious occurrence, than as one susceptible of any important application. The syringe is now filled. The scale marks

No. 1. 60—85; 55—95; 70—80 mill.

I inject the blood into the jugular of the other dog; we have

No. 2. 70—100; 65—90; 70—90 mill.

There is no very marked difference in the height of the two columns. We have, so far, acted with too little liquid to produce any very notable effect. It would even appear that a slight fall, instead of a rise, has taken place in No. 2. I will repeat the injection. The capacity of the syringe I employ may be estimated at about four ounces. The blood had no time for coagulation, for it remained a few seconds only in the instrument, and the latter had been previously warmed to the temperature of the blood. The syringe is now filled a second time. The mercury oscillates between

No. 1. 70—100; 50—65; 60—75; 60—80 mill.

The fall is very slight. The column acted on by the heart and blood of the other animal, gives

No. 2. 65—95; 70—100; 75—95; 60—90 mill.

The contents of a third syringe are now transfused. The scales indicate

No. 1. 40—70; 60—85; 50—75 mill.
No. 2. 70—100; 75—100; 60—95 mill.

There is clearly some diminution of pressure in the dog supplying the blood; but it remains uniform in the receiving animal. I now fill the syringe for the fourth time, and inject its contents into the jugular. We find

No. 1. 60—80; 50—65; 33—55; 45—70; 25—50 mill.
No. 2. 75—90; 70—80; 70—90; 60—95; 70—95 mill.

Consequently, the increase and decrease of the mass of the blood are not proportional to the height of the columns of mercury. The pressure is sensibly diminished in the animal which we have rendered almost anemic; it is normal, or perhaps slighfly lowered, in the dog whose vascular system has been distended by our injections. These results are not literally such as I had announced that we should probably obtain; but we will recur to the experiment, which must be repeated before it can be considered to have furnished precise conclusions incapable of being controverted.

The degree of frequency of the pulse of the two animals is remarkable; that of the dog No. 2 beats only 72 a minute; that of No. 1 beats 150 in the same time. The frequent contractions of the heart, in the latter case, are to be explained by the constant necessity for a new supply of liquid felt by every part of the economy, and by the maintenance of the equilibrium of the currents of blood. The frequency and extreme smallness of the pulse, in cases of abundant loss of blood, have been noted by all observers; they are even very valuable signs of a certain internal hemorrhage. I shall now let the blood of the animal, from which we have already drawn so much, flow freely. A large opening is practised in the carotid. The mercury stands at

No. 1. 25—30; 25—27; 20—30; 23—25; 0—25 mill.

You perceive that in proportion as the blood flows the pressure diminishes. I need not explain to you why the jet of the liquid grows so feeble; it is perfectly natural that its force of progression should decrease in the direct ratio of the weakened energy with which the muscular fibre contracts. I now tie the vessel, in order to prevent the animal from perishing of hemorrhage. I must confess that, in spite of the positive information acquired through M. Poiseuille's instrument, I am at a loss what opinion to hold respecting the agency of the blood on the pressure of the walls of the arteries. What a few days past appeared to me decided, seems to me now far from possessing that character. The mercury, it is true, fell to a considerable extent in the dog which we deprived of almost all its blood; but it did so at the close of the experiment only, at a time when the vascular system had retracted, in virtue of its elasticity, and ceased to be dilated by the currents of blood.

Now, in the physiological state, the arteries are not only full but distended; we cannot, therefore, legitimately conclude that the same condition of things exists in that state as in the experiment I have made before you. As the physical conditions are not the same the phenomena produced must also differ. Although my theory has, perhaps, not been demonstrated to be wholly fallacious, yet I cannot conceal from myself that I took an erroneous view of the influence exercised by the increase of the mass of the blood. First, I injected water into the veins, and the mercury fell. I was surprised at this result, but, in reflecting on it, fancied I might justly ascribe the diminished arterial pressure to the debilitating action of the liquid injected. There might, I well knew, be some other cause, which I was unable to discover, and this was a point, I also knew, on which experiment alone could decide. I consequently made the necessary experiment, and from its result it would appear that, in truth, the volume of the blood has only a trifling influence on the force with which the column moved by the heart presses on the coats of the arteries. I dare not draw a premature inference from a single fact, but we have just seen that the gradual injection of more than a pound of blood did not increase the elevation of the mercury; far from this, towards the close of the experiment it oscillated below its normal level.

These questions, Gentlemen, are not canvassed in our most recent works on physiology; nay, more, they are not so much as mentioned in them. The authors of those books are too fully occupied in discussing the part to be assigned to the *vivifying fluids*, the *materials of reparation, assimilable principles, &c.*, to spare time for the consideration of hydraulic phenomena. In this matter, therefore, we have no authority to refer to, and must, for the time being, be contented with the evidence of our first essays. But do not suppose that a candidate for graduation would do well in giving proof of knowledge of this kind to his examiners; he would not be understood. No; but let him take the internal maxillary artery, for example, from its origin, follow it through all its flexuosities, name all its branches, and enumerate the anomalies to which it is subject, and he will be sure to receive unanimous praise. If he were asked to describe the functions of the fifth pair, it is true that he would be sorely embarrassed; but he can describe the course of the corda tympani with unerring precision, and institute most learned antitheses between the thick and short, and the long and slender branches. This factitious knowledge is just what is calculated to make a brilliant show at examinations. If it were even obtained by dissecting, something might be said in its favour; but, in the majority of cases, anatomy is studied by plates, as geography by maps; and, provided proof be given of a certain share of memory, the rest is of no consequence. But to return; you see, from the uncertainty we feel respecting the modifications induced in arterial pressure, by the volume of the blood, that all study on the matter has heretofore been neglected. Nevertheless, there is no question more intimately connected with the practice of medicine. Whether you prescribe diluents, or remove blood by venesection, you alter the mass of the living liquid in circulation, and hence produce, inevitably, certain mechanical results. We know that in many cases the pulse becomes less frequent and strong after one or more bleedings; but practitioners are contented with the simple knowledge of the fact, without looking for its explanation. I am of opinion that we shall never obtain really scientific notions on these points, until a totally new view has been taken of them. The modifications ensuing in the frame are the consequences of changes produced in the volume and composition of the liquids; hence the hydronamic phenomena of the body must be studied before the vital.

Before quitting this subject for the present, allow me to bring to your recollection some experiments, elucidating the influence of agencies, of quite a different character, on the pressure of the blood. In those experiments, which were made on the venous trunks, we saw that the arterial pressure is not modified by the physical condition of the tubes, or of the contained fluids alone. We ascertained that in the circulating system, as in the ensemble of systems, constituting the animal economy, vitality claims a share in the regulation of the organic functions. Every moral impression and strong sensation reacts on the heart, and alters the rhthymn and energy of its contractions. We all know that anger and fright, for example, betray themselves to the bystander by the sudden change of colour in the features. According as more or less blood flows towards the capillary system the face reddens, or grows pale; it is justly termed the mirror of the passions, because on it are reflected the modifications that occur in the vascular apparatus.

It had already been shown by M. Poiseuille, in the case of frogs, that any considerable pain increased the internal pressure in the vessels; I repeated the experiment, you will recollect, on a dog, in order that you might have ocular proof of the fact. The animal had already served us for some experiments, and, to save it the torture of a new operation, I made my observations on the eighth pair, which was bared by a wound exposing the carotid. The sensibility of the pneumogastric nerve is, you know, far from being exquisite. When I raised it on the director this fact appeared very distinctly, for the dog gave no evidence of feeling the contact of the instrument. Indeed, as I have already had occasion frequently to observe, the sensibility of that nerve is extremely variable. You will not, in all probability, find two species among animals, nay, two animals of the same species, in which the eighth pair possesses a perfectly identical share of sensibility. In some it is enough to touch the nerve to excite cries and convulsive movements; in others you may irritate, and even lacerate it, without the animal appearing to be conscious of what you are doing. It is a still more curious fact, that the sensibility of the two nerves differs in the same animal. You may cut the vagus nerve on one side, and no sign of suffering be manifested; whereas, if you divide its fellow, the section determines all the symptoms of acute pain. I know not on what this difference depends; but, in fine, the nerve under consideration must be esteemed to be endowed with only slight sensibility in the dog. I made the experiment under the idea that the varying level of the mercury would point out more accurately even than the movements of the animal itself, the degree of sensibility of the nerve.

The scale, previous to the experiment, marked from 20 to 22 millemètres; the respiration was regular, and performed with freedom. I compressed the nerve with a pair of forceps; no evidence of much feeling followed; the animal remained quiet. On increasing the pressure considerably, he struggled violently, and the mercury rose to 35, 45, 38, 40, 35 millemètres; but I was at a loss what share in the elevation I should ascribe to the contractile force of the heart, and what to the movements of expiration. It was certainly possible that the ascent of the column was more immediately caused by the play of the respiratory organs than by the increased impulsion of the heart. However, you know that pain borne in silence acts quite as powerfully on the circulation in men, as when accompanied with the most noisy demonstrations; frequently, indeed, stifled suffering deranges the functions of the heart to a greater extent than that which is clamorously expressed, without any attempt being made to struggle against the instinct that causes us to utter cries when suffering physically. Nothing of this kind was, of course to be apprehended in the case of the animal on which I experimented; he cried out when he felt pain, and ceased to do so when he ceased to suffer. His arteries, indeed, complicated the problem I was trying to solve, and prevented me from forming an accurate estimate of the energy of the ventricular fibre. On repeating the compression of the nerve the column rose, though the animal did not make any considerable struggle; now it stood at 30, 28, 35, 40, 37, 43 millemètres. The pulsation of the heart became frequent and tumultuous; the stethoscope, applied to the precordial region, transmitted distinct shocks to my ear, which were audible even by the persons surrounding the animal. The respiratory movements became accelerated, so that it would have been impossible to draw any rigorous conclusion from this experiment, had not its results been confirmed by a crowd of previous observations. I finished by dividing the nerve. At the instant the section was performed the animal moved slightly, and the mercury rose a few degrees. All then became calm, except that the respiration, as a matter of course, grew difficult, because the eighth pair presides over the function of the pulmonary apparatus. The surface of the column stood, with little variation, between 20 and 22 millemètres.

I followed up this experiment by inquiring into the influence of galvanism in this way. It is my wish, as far as possible, to perform perfectly new experiments, for by doing so both you and myself are equal gainers. Every one knows the sensation determined by the contact of the electric spark; but no one had thought of studying its effects on the general circulation, though it was plain these might furnish a curious subject for investigation. I drove two platina needles into the animal, one into the cervical region, the other into the middle of the thigh. The two poles of the battery were brought into communication with these needles, so as to produce an electric current through the tissues. The plan generally followed in practice is to apply the conducting wires to the surface of the integuments, and so induce a sudden commotion of the whole frame. In pathological cases I prefer electro-puncturation by means of needles placed in the course of the nerves; I am, in this way, more certain of the action of the electricity, especially as I can limit it at pleasure. In the instance I am speaking of I made use of only ten pairs, lest the battery should be too strongly charged. They were quite sufficient to produce as powerful an effect as was desirable. The moment I touched the needle with the wire, the animal gave a convulsive start, rapid as lightning, and the mercury rose at the same instant to 55 millemètres. The rapidity of the ascent was so great, that it could not possibly be ascribed solely to the efforts of the animal. Something must have acted instantaneously, and directly on the walls of the vessels.

We may conjecture the phenomenon to be explicable as follows :—Muscular action favours the passage of the blood into the veins, as is clearly shown by the acceleration of the flow of blood from an opened vein at the bend of the elbow, by the movement of the muscles of the forearm. Now, if the muscles exercised energetic pressure at the same moment on the entire venous system, the force of progression of the currents of blood would increase to an enormous extent. This is, I fancy, just what happened in my experiment. An electric current traversed the tissues, and the coats of the arteries and veins being pressed on all sides, pressed, in their turn, on the liquid they contained. All authors who have written on tetanus, have described the pulse to be tense, hard, and vibrating. I have no doubt but that these peculiarities depend on the pressure exercised by the blood on the walls of the vessels. To prove this experimentally, we should apply the hæmodynamometer to an animal poisoned with nux vomica.

An equally curious phenomenon, bearing on the present question, was some time past observed by M. Poiseuille, in the course of his researches. A dog had been tightly attached to a table, to serve for some experiments on the pressure of the vessels. The instrument was applied, and the mercurial column stood at rest in its interior. Suddenly the mercury rose several millemètres without the animals having made any considerable movement, or very deep inspiration. To what, think you, was this sudden ascent owing? To a sudden twinge of pain? No, Gentlemen, far from being caused by a disagreeable, it was produced by an erotic sensation. The fact is, that a bitch in heat was at that moment brought into the room, and her presence awakened in the animal the instinct of copulation. It was a moral impression, and not physical suffering, that, in this instance, modified the circulation of the blood.

But to return to our more immediate study of the properties of the blood in the human subject. There is at present, Gentlemen, in my wards at the Hôtel Dieu, a young female, aged nineteen, whose parents forced her, two years ago, to marry against her will. The consequence has been the development of violent hysterical symptoms. The paroxysms occur frequently in the course of the day, and while they last the patient suffers under frightful delirium, and the strangest hallucinations. She fancies she sees her mother approach to strike her; her countenance then expresses horror and affright; at one moment she appears to try to defend herself, at least some broken words seem to indicate this; at the next she turns suppliant, implores her tormentors to spare her, and, bursting into tears, throws herself into the arms of the persons about her. It would, I think, be impossible to render with more fidelity and expression the different sensations she experiences. The most consummate actress could not equal her in the varying play of the physiognomy, on which one may read in turn despair, dejection, and grief, so truthfully depicted, that one longs for the skill of the painter. This poor patient fancied that loss of blood would relieve her. Aware of the influence of the moral over the physical being, in affections of this kind, I yielded to her desire, rather to satisfy her, than from any conviction of the efficaciousness of the proposed remedy. Here is the blood extracted; the serum is to the clot as 37 : 26. The proportion of the former is evidently very considerable; and, in addition, the consistence of the clot is but slight. Both agree in showing the existence of a remarkable modification in the liquid. As I have already repeatedly told you, I have no intention of referring all diseases to the alteration of the blood. I am not a partisan of exclusive systems, and, therefore, I will not draw any inference, for the present, from the facts we now observe. I simply note them as appearing very curious; we shall, possibly, be glad to avail ourselves of them at a future period.

I now turn to a point which I consider of some importance, especially as a particular view respecting it has been strenuously urged in favour of the doctrine of inflammation. I allude to what has been termed the *buff* of the blood. It has been asserted (and among others, by Rasori, in a long work on phlogosis) that this is the inflammatory element *par excellence*, and that it is never wanting under certain given conditions, such as during the existence of pleurisy, pregnancy, &c. This notion, advanced perpetually, and defended in several thick volumes, appeared to me to deserve serious examination. We should, above all things, seek for truth; and, therefore, though I see nothing in the *buff* but so much fibrin, which, as it is lighter than the colouring matter of the blood, rises to the top and forms into a mass,—I would almost say organises itself on the upper surface of the liquid,—still I set about searching for this appearance in the assigned conditions. With this view I had four or five gravid women bled at different periods of pregnancy; three of them were affected with pleurisy, and yet in not a single instance did this *buff* show itself. Was the inflammatory element afraid to face me? The fact is, that I found no such thing, no matter how anxiously I searched for it. However, I should add that the modification in the coagulating process, which produces the buffy coat, depends on a variety of circumstances. Thus, although inflammation evidently exist, if the opening made in the vein be narrow, or if its parallelism with the wound of the integuments be imperfect, and the blood flows slowly; or if the receiving vessel does not present a wide, and, according to others, a narrow, surface to the air, the *buff* refuses to show itself, and remains concealed within the cells of the clot. However, I propose to continue vigorously my inquiry into this matter; for the service done the science by overthrowing fallacious theories, which interfere with its progress, is not inferior to that of making useful discoveries.

Here is another specimen of blood, drawn some days ago, which has caused me considerable embarrassment. It comes from a young Savoyard girl, aged twenty-three, recently arrived in Paris. She was admitted with some symptoms of the fever called entero-mesenteric, or typhoid, such as injection of the conjunctiva, general uneasiness, prostration, colic, and vomiting. Still the blood removed by a first and second venesection seemed of a normal character; at least, you may see that the modification, if any exist, is not appreciable. I therefore felt uncertain about the diagnosis, and, at all events, I think the disease will not prove dangerous. The blood shall also undergo a special examination, and the issue of the affection will show if I have been in error as regards the diagnosis.

You remember, I trust, the case of the female asphyxiated with the fumes of charcoal, whose lung we examined a few days past, and have not forgot the fluidity of her blood. I purposely return to her case, because a singular phenomenon was ascertained respecting it after our separation. I never before met with a similar peculiarity, not even in cholera patients. Her blood is notably acid; you perceive that it reddens litmus paper. I cannot be sure that this character proceeds from the entry of carbonic acid gas into the circulation, but venture on this explanation of its existence, as a plausible hypothesis, worthy of further examination. The exact analysis of the liquid might, perhaps, give us some positive information on the point. We will try, at all events.

In pursuing our study of the blood we must constantly keep in view one fundamental fact, namely, that the blood in the living animal is quite a different fluid from that extracted from the body. Medical observers and chemists have, in general, paid too little attention to this difference. By studying the liquid in the laboratory alone, they have increased the perplexity of many points, and in their statements almost all of them are at variance with each other. In truth, there is a vast difference between the fibrin while circulating with the blood, and when separated from that fluid. Besides, the blood presents itself in two very distinct conditions, even when removed from the body. In some cases it forms a compact mass, its superior surface being of a bright-red colour, the lower part blackish; in other instances it separates into two perfectly distinct parts,—one solid, called the clot; the other liquid, known as the serum. Although our means of investigation cannot be made applicable to the blood, except when removed from the body, nevertheless we will improve on those who have preceded us, by not confounding it with the fluid that traverses our organs. It is from this view of the subject, and on just grounds, that very meritorious observers, among others Professor Müller, of Berlin, have distinguished the *liquor sanguinis* that circulates in the vessels, from the liquid which separates from the blood when extracted from the body. Thus, when we examine such vessels of an animal under the microscope as are sufficiently transparent for the purpose, we distinctly perceive an infinite number of globules, borne along by a rapid movement, rolling and slipping over each other; and we see, besides, between the mass in motion, and the walls of the vessel, a space almost destitute of globules, and filled with a colourless transparent fluid. This fluid is the *liquor sanguinis*, which holds the globules in suspension during life. It is probable that, by examining the blood while performing its various offices, we should succeed in discovering some of the phenomena of its organic composition. Unfortunately such study is rendered almost impossible from the absence of favouring circumstances; we are reduced to very limited observations on small animals, in which some of the organs are transparent enough to allow the eye to follow the different movements, and catch the form of the globules under the microscope. Very young rats and mice, and bats, are almost the only mammiferous animals on which these experiments may be made with any success. At the present moment the rigour of the season has benumbed the latter animals, and I have not been able to procure any. It is impossible to conceive why the phenomena of the intimate composition of the blood are enveloped in such complete mystery: it would seem as though Nature took pleasure in hiding their essence from us, precisely because it is a matter of such paramount importance to comprehend it. The *liquor sanguinis* is, no doubt, in reality, serum, but serum which holds in suspension, or in solution the coagulable matter or fibrin of the blood. Fibrin, by the way, is an improper term, and ought to be replaced by *coagulin;* this is, possibly, not unobjectionable, but it would, at all events, prevent us from confounding under the same denomination two different substances, the fibrin of the blood, and the fibrin of muscle. Chemists fall into a palpable error in regarding these two principles as identical; and the proof that they are not so, in addition to the absence of resemblance in their physical properties, is, that compared in respect of their alimentary qualities, they differ widely. I have demonstrated, by direct experiment, that the fibrin of blood is but slightly nutritive, whereas that of muscle is extremely so. The serum, again, is the *liquor sanguinis* deprived of its fibrin; the latter becomes organised, differing herein from the albumen of the blood, which always forms into an amorphous mass under the action of heat and the acids. When removed from the vessels part of the serum solidifies, the rest remains liquid. Such is not the constitution of the circulating fluid. The study of the clot presents similar difficulties. In the living blood, as it might be called, there is a liquid holding particles in suspension; but if you receive some of this blood into a vase, a clot will form and the globules disappear, while within the vascular tubes they move in the midst of a viscid liquid, endowed with properties with which you are acquainted. In a vase solidification takes place: one of the elements of the liquid, the fibrin, becomes organised, and imprisons the globules in its cellular structure. In the living subject the serum contains the globules and fibrin in suspension; removed from the body that liquid bathes a clot composed of fibrin and globules.

Here, then, are points of material difference. Still, of the two parts, the serum and the clot, into which the blood separates when out of its natural tubes, examine the latter, and you will find that the liquid dissolves the globules. The fibrin will alone remain, and you will be astonished at its minute quantity, compared with the dimensions of the clot, and the enormous number of globules it contained. The fibrin, which might be termed the basis of the clot, is, in this isolated form, worthy of the most serious attention. It, alone, is at once the cause and the agent of the solidification of the blood. The latter is one of those phenomena placed on the confines of vitality and physical endowment, and one which might, in a certain sort, be compared to the symmetrical transformations undergone by matter called crystallization. However, these two kinds of organisation must not be confounded; if you examine one of these masses of parenchymatous fibrin under the microscope, you will detect a regular conformation in it resembling the forms assumed by organised matter, such as ramifications and areolæ, intermingling and anastomosing in an infinity of ways. The clot, then, the *insula* of the ancients, must not be looked on as inert mass, but as a fibrinous arborescent matter, forming the basis of a finely and delicately organised parenchyma, and differing essentially from the albumen, the solidification of which is the simple result of a chemical or physical agency. But the functions of the fibrin do not stop here. It is to be found again, with the same characters, in the coagulum that obliterates divided arteries or veins: it is to be traced in the formation of adhesions, of false membranes, and of cicatrices; and it is to be seen deposited in layers at the surface of solidified wounds. Under all these conditions it is organised; its arborisations are converted into pseudo-membranes, and these false membranes become canaliculated, forming vessels permeable, in their turn, by the liquid that produced them, and of which they originally helped to constitute the substance.

Much has been written on the cicatrisation of tissues divided by the cutting instrument. The growth and adhesion of granulations, the effusion of coagulable lymph, and the organisation of new vessels have all of them given rise to important researches. One thing remained to be done, namely, to examine the influence of the composition of the blood on the rapid or slow, complete or incomplete, formation of cicatrices. Now, the animals I had defibrinised were well adapted for experiments on this point. I chose a dog for the purpose, from whom I had successively removed several portions of fibrin (this by the way, is an experiment to which I shall call your attention at a future period); he was greatly reduced in strength; still he was not so much pulled down as to be enabled to survive an operation. I made a longitudinal incision through the skin, and some depth of muscle in the anterior and middle part of the neck. The blood that escaped from the divided vessels appeared more liquid than usual; it did not coagulate on the blade of the bistoury. I united the lips of the wound accurately by the twisted suture, and the animal was left to himself. He died yesterday evening, having survived the operation a few days only. Here is his body, which I now proceed to examine. We may, before beginning, indulge in a few conjectures, as we shall be enabled to verify them immediately. The main question is, did union by the first intention take place? Opinions might be divided on this point. For my part, I believe that such union was impossible, because the blood contained no fibrin, and had, therefore, lost its power of solidification. I express my opinion thus distinctly and unreservedly because, even if it be erroneous, it cannot leave any wrong impression on your minds, as the autopsy will at once correct it. I now shave away the hairs surrounding the wound. The blood, which had by its desiccation glued its lips together, masks their appearance, and prevents us from seeing if they have really united. However, if I may judge from what I can already perceive, there is no real adhesion. The tissues are discoloured, dry, and hardly at all swollen. In the situation of the points of suture, there are, no doubt, a few cellular bands; but these are rather isolated, false membranes, than an exact adhesion of the two surfaces of the solution of continuity. The cicatrisation of the wound, therefore, could not be accomplished; and everything seems to show that, if the fibrin had been completely withdrawn from the blood, there would not have been any points even of apparent union. You will often see wounds of bad character in the hospitals, which remain stationary for weeks or months, resisting all the efforts made to hasten their progress to a cure. We may dress them with stimulants, cover them with powdered bark, sprinkle them with the chlorides, but all in vain; no improvement follows. The case alters when, as is sometimes done, leeches are applied in the neighbourhood; the effects are then soon perceptible,—but what are they? The rapid extension of the ulcerated surface both in length and depth! But let the patient be placed in favourable hygienic condition; let a strengthening diet replace that to which he had been confined, and his strength returns; his face regains its colour; the wound assumes a vermilion hue, and advances quickly to cicatrisation. The consequence deducible from these facts is too evident to require me to expatiate on it. Do you not see that the therapeutical means employed were useless, so long as they were calculated to act on the solids alone, but that they at once became efficacious when the liquids had been modified? The explanation of this is simple. As the solids draw from the blood the materials proper for their cure, they continued in a diseased state, so long as that liquid was itself altered, and could not furnish those materials, From the moment its normal composition was restored to the blood by a suitable regimen, there was nothing to impede cicatrisation.

I made another somewhat similar experiment on this animal. I tied a large vessel with the intention of examining it after a certain time to see if coagulum was formed within it. It is not very uncommon to observe recurrence of hemorrhage after the fall of a ligature: and it seems possible that that serious accident might be prevented, if we had more accurate notions on the coagulability of the blood. I consequently applied a ligature to the right carotid; let us see if it is obliterated in such a manner as to be capable of resisting the heart's impulse. I cut away a piece of the artery, in order to slit up its walls, and examine its interior with greater ease. You see that there is no trace of coagulum; for I cannot give that name to a few granules which lie on, without adhering to, the internal membrane of the vessel. The ligature had already nearly cut through its walls; had the animal survived a few days longer secondary hemorrhage must have taken place.

This rapid view of the subject will allow you to form a notion of the importance of the study of this organisable material, which reconstitutes, reproduces by itself alone, all the tissues of the economy. We will push our researches respecting it as far as possible, and shall possibly obtain results, both of a physiologial and therapeutical kind, more precise in their character than those we now possess.

I have now to inform you of the present condition of some animals submitted to various experimental treatments. All these experiments refer to questions of the deepest interest, such as, among the rest, the relative proportion of the serum and clot. In the blood of a healthy and robust male the serosity may be estimated as forming a fifth or a fourth of the whole; it abounds with fibrin and globules. In women and children the serosity constitutes one-third of the mass. But these proportions vary according to the age, temperament, and species of nourishment of the individual, as well as a variety of other circumstances. My object is to determine what proportion between the two elements is incompatible with the continuance of life. This is an experimental problem completely. In the hope of solving it, I have placed different animals in circumstances fitted to give useful indications respecting it. We know that successive bleedings, closely following each other, augment the proportion of serosity, and induce various disorders, and finally death. I have submitted one animal to this species of treatment. The quantity of serum increases also, from the abundant use of drink; at least, this a notion generally admitted by medical men. But once again, we are reminded that we must not prejudge results; our prophecies are constantly exposed to be proved to be false. Here, for example, is an experiment well calculated to make us cautious in this respect. I have had two ounces of blood taken from the animal before me every day, and replaced by an equal quantity of distilled water. I fancied I should have had to present you a specimen of blood rich in serum, and containing a small proportion of clot. The reverse is, however, the truth: it contains, to all appearance, scarcely any serum. I presume, however, that the serum is really augmented in quantity, but that some cause or other prevents us from appreciating its increase. Perhaps it is retained in the substance of the clot. A third animal is also bled daily; he is given plenty to eat, but no drink. At the second bleeding the serum was found increased in quantity, and it had, besides, lost its limpid appearance. A fourth animal has abundance of drink, but no solid food; his blood contains as much serum as clot. Here is the product of the seventh venesection practised on the animal about which I have already more than once spoken. Although he eats and drinks as much as he likes, his health is materially affected; a notable change has taken place in his gait, habitudes, and temper. The mucous membranes have grown singularly pale,—a peculiarity which has been long noted by veterinary practitioners in appreciating morbid symptoms. When he is bled now, syncope follows. I have no doubt but that an affection of the lungs will soon come on, and speedily put an end to his existence.

Finally, here is the subject of an experiment of a different kind. I injected some frog's blood, the globules of which are ovoid, and provided with a central nucleus, into the veins of this little animal. I am anxious to see whether they will become transformed, so as to acquire the characters of the globules of mammiferous animals. The subject of the essay is, as you perceive, in good health, and does not appear in any way affected by this new species of transfusion.

LECTURE VIII.

Effects of increasing arterial and venous pressure.—Death produced by injection of venous blood.—Differences between the serum and liquor sanguinis.—Proportions of serum and clot in different affections.—Effects of bloodletting in increasing the quantity of serum.—Theories of inflammation.—Examination of blood drawn from patients at the Hotel Dieu.

Gentlemen:—I presumed that by transfusing blood into the veins of an animal I should cause an ascent of the column of mercury in the hæmodynamometer, exactly proportional to the increase in the mass of the blood. The result was not such as I had anticipated; the level of the column, on the contrary, remained almost stationary. This is really difficult to comprehend, and I see no means of accounting for it, except by supposing that, instead of causing a simple mechanical effect, we brought some vital phenomena into play in the vascular system, by our introduction of a liquid differing from that which ordinarily circulates through the frame. The blood of one dog is not precisely similar to that of another; there is, possibly, an individuality in the liquids of every animal, as there is in the external form of its body. The age, the size, the degree of strength, the kind of nourishment it habitually uses, gives to each individual special and distinctive characters. The moment the foreign fluid touched the muscular fibre it must have modified its contractile force, and hence, without doubt, arose the alteration in the energy with which the heart propelled the blood into the arterial system. The ulterior effects of transfusion on this dog have been the same as those observed whenever an injection of any species is made into the veins. The functions of the important organs were temporarily disordered. After the operation the animal fell ill, and he is not yet completely recovered. When transfusion was regularly practised on the human subject formidable symptoms were frequently observed to follow its performance. We possess some cases of the kind, related by contemporary authors, but we have no precise details on the nature of the lesions induced, and the kind of organs most frequently affected. Among other morbid phenomena, this dog presents a certain state of disease invariably found in animals whose blood undergoes a change of composition—I mean *purulent ophthalmia*. This morning, when I first saw him, the eyelids were glued together by a viscous humour of a dirty white colour; the conjunctiva was tumid, covered with a layer of false membrane, and appeared engorged from the effusion of puriform fluid into the areolæ of the subjacent cellular tissue. At present you can only see the redness of the ball of the eye, for the pus exhaled between the eyelids was removed before the lecture. I forgot to mention to my assistant that I intended to speak to you on this lesion of secretion, and, in making the animal's toilet, he has unwittingly removed what would have furnished further demonstration of the existence of ophthalmia. I have already had occasion to communicate to you my suspicions regarding the nature of the purulent form of that disease. If observations such as these recur frequently, and if the modifications in the composition of the blood entail, in every case, analogous lesions of the mucous membrane of the eye, we must, of necessity, admit that that *inflammation* is nothing more than the local expression of an alteration of the qualities of the blood.

But the fact is, that it is impossible, physically speaking, that the volume of fluid in circulation can do otherwise than exercise some sort of influence on arterial pressure. The experiments we have made on this point have not, it is true, led to very precise inferences, but I must repeat them before I can feel convinced of the reality of the results to which they apparently lead. Why should we not be able to produce in the living animal, in the present, as in so many other instances, the phenomena developed by disease in the human subject? It would be rash to expect anything of the kind, perhaps, if vital properties were concerned in the matter, but there are none but mechanical phenomena to be produced. The problem is complicated by the action of the chest, which alternately increases and diminishes the progressive force of the liquid. This difficulty I will now endeavour to get rid of by modifying our previous experiments in the following manner:—After having accurately noted the height of the mercury in the instrument, we will extract a given quantity of blood from an artery, and then immediately reinject it into the same vessel, taking care to leave the syringe in its place. By this precaution we shall be sure that the liquid contained in the syringe will not cease for a moment to communicate with the blood in circulation. After this we will repeat the experiment on another kind of vessel; blood extracted from a vein shall be reinjected instantly into its cavity; and we will, in like manner, take care that there shall be no interruption to the contact of the contents of the syringe with those of the blood-vessel to which it is applied. It is scarcely necessary for me to dwell on the advantages that will accrue from these modifications in the experiment. In the first place, we shall be better able to judge of the real influence of the volume of the liquid in circulation, as we shall act on the animal with his own blood; in the next, we shall be enabled to compare the results produced by the removal of a certain quantity of blood, when effected through the arteries or veins. The syringe I am about to employ holds three hundred cube centimeters, that is, three-fifths of a pound. The hæmodynamometer is applied to the femoral artery. Although the movements of respiration exercise a very distinct influence, even in this vessel, on the height of the mercury in the instrument, nevertheless they will not produce, by any means, so marked an effect on it as in the carotid artery. Independently, however, of the contraction and dilatation of the thorax, the femoral artery is exposed to the influence of a twofold action that modifies the course of the liquid during its passage through the ventral aorta. Here is what takes place: during inspiration the diaphragm descends and compresses the blood-vessels by pushing the abdominal viscera before it; during expiration the diaphragm reascends, and the exterior and lateral parietes of the abdomen press, in their turn, on the viscera, and thereby accelerate the course of the arterial blood; both movements of respiration have the effect of augmenting the pressure in the tubes that distribute the blood to the lower extremities. These phenomena are, perhaps, even more interesting to the surgeon than to the physiologist. If a patient utter cries, and struggle against the persons employed in holding him down, while under the knife of the operator, it is evident that such efforts will inevitably increase the amount of arterial pressure. It is the usual habit to direct the patient to give free utterance to his sufferings, instead of attempting to conceal them by silence; and there certainly is no objection to the patient's uttering cries, so long as the cutting instrument is employed on parts at a distance from the larger trunks; but suppose that a patient indulged in immoderate outcry, while a ligature was being applied to the femoral, for an aneurism of the popliteal artery, there would be considerable risk of the pressure suddenly increasing in the interior of the vessel, and thereby pushing its coats against the edge of the bistoury. In a case like this, instead of recommending the patient to cry out, we should do all we can to induce him to remain quiet, and avoid, as far as possible, contracting his muscles. I need not add that efforts, though made without noise, would be attended with similar ill consequences, for they would increase the progressive force of both arterial and venous currents. There are some subjects endowed with such exquisite sensibility that their muscles undergo a sort of convulsive contraction almost every instant during the course of an operation. These are points which must be taken into account, not only in regard of surgical manipulations, but also of our attempts to determine the physical conditions of the vessels. Just in the same manner the circulation will be found far from being in a normal state in an animal suffering from artificial tetanus, produced by the introduction of strychnia into its veins, for the pressure will be inevitably modified. Our present business, however, is to ascertain the influence of the volume of the liquid on the static force of the currents of blood, when the economy is in the physiological state. I turn the cock that separated the blood from the carbonate of soda; the mercury is now in motion; it oscillates between 65—75 mill.; it neither rises nor falls much beyond those two points; its movements present none of those extreme variations which we observed in our former experiments; the difference, in this instance, depends on the great distance of the vessel from the respiratory apparatus. I have applied two ligatures to the carotid; one is intended to prevent hemorrhage by the upper end; the other, to fix the lower end of the artery to the tube introduced into its cavity for the purpose of transmitting the blood to the body of the syringe. Everything is now arranged. You see that the blood pushes the piston up of itself, and enters the instrument. The scale marks 60—70, 55—65, 55—60, 50—55 mill. The syringe is half full. I now drive its contents back into the artery. We have 60—65, 60—70, 65—80, 60—70 mill. The mercury, therefore, ascends when we increase, and falls when we diminish the quantity of liquid. Now, that the latter is all reintroduced, the level of the mercury has returned to 65—75, our original starting point. I have now refilled the syringe by simply allowing the force of progression of the blood to drive the piston back. The column falls to 60—70, 50—55, 40—47, 35—42, 25—35, 20—27 mill. Three hundred cube centimeters of liquid are now extracted, and you see that the pressure is very much diminished. Unfortunately we do not know what quantity of blood remains in the body of the animal. The notions we possess on the total volume of liquid in circulation are too vague to admit of our drawing any exact inference from them. I reinject the blood: the mercury marks 25—35, 40—48, 50—55, 50—57, 65—75 mill. It has again reached its original level. Twice we have repeated the same experiment, and twice the height of the mercurial column increased and diminished in the direct ratio of the volume of the blood; no reasonable doubt can, therefore, be here entertained respecting the nature of the phenomena. I now tie the lower end of the vessel, and proceed to make the same experiment; by way of comparison, on the venous system. The jugular has been laid bare. We must, in the case of this vessel, take some precautions that would have been useless when we acted on the artery. You will readily understand the reason of this; if I simply introduced a tube into the vein, and aspired its contents by raising the piston, the coats of the vessel would collapse under the influence of atmospheric pressure, and form a sort of valve. I have, therefore, selected a tube for the present experiment sufficiently long to reach into the thorax, as far as the vena cava superior, or even as the right auricle; while introducing it I heard a slight sibilus; my assistant also detects a strange sound in the chest; it is probable that a little air entered into the tube during inspiration, and so reached the right cavities of the heart. This accident will, probably, not interfere materially with the progress of the experiment. The mercury marks, as before, 65—75 mill. I fill the syringe, but with much greater difficulty than from the carotid; I am obliged to raise the piston forcibly in order to get the blood into the body of the instrument, whereas, in the former experiment, the impulse of the heart was sufficient to produce that effect. The level of the column falls to 55—65, 45—50, 30—45, 25—30, 15—20 mill. You observe that the animal appears to suffer very much more than in the former instance; he tosses about, and seems oppressed by some extraordinary sensation. I now reintroduce the blood. I know not to what it is owing, but I certainly feel a resistance in pushing the piston, which I did not encounter when experimenting with arterial blood. The state of the animal grows more and more desperate; I fear, indeed, that he cannot survive many moments. Almost all the fluid has now been reinjected into the circulation, and yet the mercury stands at 15—20, 20—25, 13—17, 10—15, 10—12 mill.

There is scarcely any pressure, and the oscillations are hardly appreciable: they have now ceased completely. The scale marks 10 mill.; but this slight elevation is caused by the weight of the volume of subcarbonate of soda, which is nearly equivalent to ten millemètres of mercury. It is not likely that the sudden death of the animal is to be ascribed solely to the accidental entry of air into the veins; but we will satisfy ourselves on this point by the autopsy. I have opened the thorax; the pericardium is notably distended; the heart appears tolerably heavy, though lighter than if it contained nothing but blood. I now percuss the surface of the organ gently with the finger, and instead of a dull sound, a distinctly sonorous one is produced, which must of necessity depend on the presence of a certain quantity of air in its cavities. I open the right ventricle, and you see that it is filled with a frothy liquid; or, more correctly speaking, that fluid forms a stratum over an enormous clot occupying the central part of its cavity. The superior and inferior venæ cavæ are filled with coagulated blood; they are much distended. The clots may be traced to the first valves of the iliac and subclavian veins. The left cavities of the heart are empty; they contain neither air nor liquid. The animal's death was, therefore, partly owing to the passage of air into the veins, partly to the coagulation of the reinjected blood. The latter was evidently the chief agent in the production of the fatal result. I was, I confess, very far from expecting such a conclusion to our experiment as this; it is opposed to all that is known respecting the relative degrees of coagulability of the blood in the veins and arteries.

You see, Gentlemen, that we scarcely ever make an experiment without establishing some novel fact, or without being obliged to come to a different conclusion from that we had anticipated. Hypotheses are much more complaisant, for they lend themselves servilely to all our wishes; whereas fair and conscientious experiment is pitiless in this respect. If we desire to attain truth in our researches, we must be prepared for every sacrifice, even for that of our self-love. Be that as it will, however, you must be struck, as I am myself, with the result of the experiment just concluded. So long as the animal was in a fitting condition for experimentation the height of the mercury and the mass of the liquid were directly proportional; but this new result does not contradict the former ones; for, as I have often told you, one fact cannot disprove another. It is only hypotheses that are ever reciprocally destructive. All the inference we can draw in the present instance is, that the phenomena observed varied in two different animals. The difference arose, in all probability, from the dissimilarity of the processes in each case; from the nature of the vessel; the composition of the liquid; and possibly, too, from the vital conditions of one and the other animal. The dog just employed had for some time been on a diet of gelatin; and, as he was weak and reduced in flesh, and the quantity of blood in his frame less than in the normal state, it was to be expected that he would not have sufficient energy to react against the least debilitating influence. Every modification of the blood would, of necessity, affect the contractile force of the heart. Nevertheless, I am far from affirming that the manifest variations of arterial pressure are explicable in this manner; I am much more inclined to believe that the variance in our results is mainly to be ascribed to the influence of respiration, which is powerful in the carotid, and extremely feeble in the femoral artery. Under all circumstances, it is fortunate that we did not perform the experiment in the first instance in the manner we have just done. Had we at first seen the mercury fall and rise according as we took away or added fluid, we should have been quite contented with the result. As our theory would have been then substantiated, we should have conceived no doubt of its correctness. You are now thoroughly convinced that the question is not so simple a one as we had in the outset presumed.

One of the most curious phenomena we have just observed is, that the blood extracted from the arterial system remained unchanged in the body of the syringe during several minutes. How shall we account for its not having coagulated from the contact of the metallic syringe? There was a circumstance which, by its physical influence, probably aided the blood in retaining its fluidity. In order to give the experiment a greater degree of precision, I took the precaution of allowing the liquid contained in the instrument to communicate freely with that in the artery, so that the impulsion of the heart, the movements of respiration, &c. acted with their full force on the contents of the syringe. The latter were, therefore, kept in constant agitation by all the causes of movement that act on the circulation, and were placed in a very different condition from what they would have been had they been exposed to the open air, and kept motionless in a vase. The proof that the influence of the contraction of the left ventricle was as distinctly felt in the instrument as in the artery itself, is that, as you plainly saw, the piston gradually rose of its own accord, as it were, until the body of the syringe was completely filled. It is very possible that constant agitation prevented the liquid from becoming solid. But there is another point which must not be lost sight of: the animal had, for several weeks, been submitted to a particular kind of diet. He was fed, during that period, with gelatin; and it is, therefore, possible that the composition of his blood was modified, and its coagulability consequently lessened. These, however, are mere surmises, and I by no means desire to give them the weight of affirmations. The *why* of phenomena is, in physiology, often beyond our reach; meanwhile, the really important point is to establish facts. To these we must more especially devote ourselves in the present instance.

As the plasticity of arterial is greater than that of venous blood, we should infer that if the two liquids were placed in the same physical conditions the latter would not coagulate so long as the former remained liquid. There is certainly no fair objection to be urged against the inference; it is conformable with the notions generally admitted on this subject, notions shared by myself. Nevertheless, you saw that the blood extracted from the artery was reinjected without causing any disorder of the economy, whereas an equal quantity of venous blood reintroduced into the circulation, immediately caused fatal disturbance. Had I attempted, *à priori*, to determine the issue of these experiments, I should have said, as venous is less coagulable than arterial blood, it will retain the fluid form the longer of the two. Consequently, when I saw the animal perish, it did not enter into my head to attribute its death to the reinjection of venous blood. I had, it is true, encountered some resistance in pushing the piston, but it happened in my case just as it happens in that of every one whose mind is pre-occupied; I treated certain important circumstances as though they were of insignificance, because my attention was not directed to them.

What principally struck me was a strange sound in the chest, which I fancied arose from the introduction of air into the right side of the heart. The autopsy showed that I was correct in my supposition; but it disclosed another cause of death in the voluminous resisting coagulum that distended the left ventricle. The latter had nothing to contract on but a compact mass. The blood must have remained partially liquid in the syringe (inasmuch as I succeeded in reinjecting it), and become solid the instant it reached its natural tubes. This is really a very curious circumstance; must we, in explanation, admit, hypothetically, the action of some idiosyncrasy in the animal; or should we infer that a diet of gelatin renders the arterial less coagulable than the venous blood? These are questions which a single fact may allow us to propose, but assuredly not to answer. We must be content to await further observation.

Among the topics of which I treated in my last lecture, you will remember I adverted to the relative proportion of the serum and clot. I also drew a comparison between the *liquor sanguinis* and the serosity, and came to the conclusion that the former belongs exclusively, so to speak, to the living blood, while the latter is only discoverable in blood deprived of life. The one is beyond the reach of almost all our methods of analysis; the other has long supplied a material for examination for the organic chemist. I also said a word or two on the *clot*, *coagulum*, *insula*, *hepar*, as it has been severally termed; of its composition; the different states in which it exists, and the mode of organization that characterises it.

I invited your attention, more especially, to this proposition,—that the blood must possess certain other qualities, in addition to liquidity, to enable it to circulate through and support the life of our organs. Nay, more, I am enabled, by numerous and incontestable facts, to affirm, that the power of solidification possessed by the blood, when removed from its vessels, is nearly the same as that proper to it while still circulating in them.

These positions, which are the results of direct experiment, may appear of trifling importance to superficial minds; they are, notwithstanding, highly interesting. They are directly applicable, in a multitude of ways, to the practice of surgical operations and of medicine; and I hesitate not to declare, Gentlemen, no matter how sorely I shall wound our vanity thereby, that so great is our ignorance of the real nature of the physiological disorders called diseases, that it would, perhaps, be better to do nothing, and resign the complaint we are called on to treat, to the resources of nature, than to act, as we are too frequently compelled to do, without knowing the why or wherefore of our conduct, and at the obvious risk of hastening the end of the patient. This notion savours a little of Hippocratism. But, I would ask, did not the physician of Cos possess, in an eminent degree, the instinct, I may say the genius of his profession; and has the art of physic really made solid progress in all respects since the day of that gifted man?

I was anxious to ascertain if the constituents of the serum of the blood in our vases were the same as those of the *liquor sanguinis* that circulates in the body during life. We soon discovered that an old and serious error existed on this point. Berzelius was the first to entertain doubts of the identity of the serum and of the liquor of the blood, and to prove experimentally that there was an important distinction to be established between them. The *liquor sanguinis* is, as you are aware, composed of serosity and fibrin. The latter element appears to be that which gives the blood the property of nourishing and renewing our organs: it is in consequence of its possessing this property that certain physiologists, ambitious of rendering the truths of physiology popularly intelligible, called that fluid *running flesh*. Whatever be the correctness of this expression, it is certain that the part of the blood which forms into a mass out of the vessels, exists, while circulating, in the form of a liquid; and is held in suspension, or perhaps in solution, in the serum. After death this portion of the blood endowed with the property of solidification separates from the serosity, carrying the globules with it. There is, therefore, the clearest proof that the blood extracted from its natural tubes differs essentially from that in circulation; for, in the former case, the globules are contained in the meshes formed by the coagulable matter. We may now, therefore, form some conception of the mode in which the solidification is effected. It is neither by a chemical action, nor by a process analogous to that of crystallisation; there is something more than this in the formation of the net-work of the clot; there is a something in it which tends to live, if it be not really life itself. Again, I stated that the relative quantities of the serum and clot are subject to great variation. The colouring matter and the fibrin, which are brought together in the clot, have not hitherto been sufficiently studied separately; this we shall see is, notwithstanding, necessary to be done. Be that as it will, however, physiologists and practitioners have long since laid it down as a rule, that a fourth, or, in some instances, a third, of the whole consists of serosity. So long as the clot forms about the fourth of the entire mass, there is a species of equilibrium of composition, which coincides with the state of health. Nevertheless, very considerable modifications may, it is believed, exist in this respect in different individuals, in the two sexes, at different ages, under difference of regimen, &c.; these positions, however, call for new experimental verification by the improved modes of investigation of the present day. It would appear, more especially, that the proportion of serosity is greater in females than in males. This excess of serum was very remarkable in the young hysterical patient of whom I spoke to you in my last lecture: it is no less so in the specimen you see in this vase, furnished by a girl, aged twenty, affected with leucorrhœa. On the other hand, the relative quantity of coagulum is much greater in some individuals. In illustration of this here is some of the blood of another of my patients at the Hôtel Dieu, a woman, aged fifty-two. She has cancer of the uterus, and has also suffered from slight attacks of hæmoptysis. I had a trial-bleeding practised on this woman; and you will at once perceive how widely this blood differs from the other specimens that we have passed in review. It scarcely contains a tenth part of serosity. I would not affirm that there is any relation between her complaints and the state of her blood, but, I confess, I am inclined to take that view of her case. Apropos of this uncommon quantity of coagulum, I must guard you against trusting implicitly, in all instances, to the appearance of things in the vase, and supposing that there is no more serum than can be seen. I have verified the correctness of a notion of which I threw out a hint at our last meeting: under certain circumstances the fibrin, while in the act of solidifying, retains a considerable share of serosity within its areolæ. In such cases it is necessary to cut the mass into slices, which causes the serosity to ooze out on all sides.

I stated to you that repeated bloodletting caused a variation in the proportions of serum and clot; but I have ascertained a still more important fact. It is this: in every case of serious disease I have met with since commencing the researches with which we are at present engaged, those two elements have invariably presented some anomaly, in respect of their relative volume. Here is another example of this,—there is a woman at present in my wards furnishing a very curious subject for inquiry in a physilogical and pathological point of view. Her complaint is a cancer of the left parotid gland, which has encroached on a part of the temporal bone. The compression caused by the tumour has brought on an affection of the fifth, combined with a lesion of the seventh pair. There is complete loss of sensibility in that lateral half of the face, destruction of the power of vision in the corresponding eye, ulceration of the cornea, insensibility of the organ not only to light, but under the contact of foreign bodies, deviation of the mouth to the right, &c. The patient is in a state of advanced marasmus. I have no hope of saving her life; her death, indeed, appears imminent. I keep her in my wards, however, that she may be spared the torture of a useless operation, which she would possibly be induced to undergo if she left me. She shall, at least, die quietly. Her blood, some of which you see here, presented, when first drawn, only 10 parts of serum to 40 of clot; there are now, however, at least 12 parts of serosity. It is evident that the additional two parts were retained by the clot, and that they transuded subsequently in consequence of the contraction undergone by the coagulated mass.

This other specimen of blood comes from a girl twenty years of age. She usually menstruates ill, is of lymphatic temperament, and subject to leucorrhœa. You observe that the serosity is very abundant. The dresser who bled her informed me that twelve hours after the operation the hand, but not the arm, swelled considerably, though the bandage was not tightly applied, and the patient had remained perfectly quiet. This sort of spontaneous infiltration, which, by the way, lasted only twenty-four hours, is not to be wondered at in the least, in a person whose blood is of the kind you see. You understand perfectly the mechanism by which it is produced. Here is another fact bearing on the subject under consideration. A medical practitioner gave me some of the blood, drawn on three successive occasions, from a patient affected with pneumonia. Each of the bleedings was abundant. The two first were practised on the day of the patient's admission; the last on the fourth day of his sojourn at the hospital.

In the first there are 11 grammes of serosity and 50 of clot, which gives about 22 parts of serosity in 100. In the second there are 24 grammes of serum to the same proportion of coagulum; the relative quantity of serosity was, therefore, more than doubled. Finally, the third gives 34 of serum to 35 of clot; that is, 50 *per cent.* of the former. These augmentations of the serum, induced by bleeding, ought surely to have struck practitioners. I have full room for astonishment at their having excited so little attention, for it is a regular practice to have the blood of patients set aside for examination. At the end of twenty-four hours usually this examination is made. The clot is felt, turned up and down, and fingered in every direction; and all with a view to the discovery of an appearance of *buff*. If this *buff* cannot be found, the conclusion is, that the disease is not inflammatory, a conclusion well worthy of the process by which it is obtained. But if you intimate to the physician that the serum has, at the second bleeding, acquired double its previous proportional quantity, and that it has lost its normal transparence, and that the clot is soft and diffluent, he will answer that such things are not of the most trifling consequence. No, the really important point is to detect the inflammatory element, were it only the veriest morsel of it, and then to annihilate it by the antiphlogistic treatment. Such is the manner in which medicine is generally practised. When we consider, Gentlemen, that in spite of the plainest and most forcible facts, the majority of medical men persist in blindly following a regular routine that brings discredit on the art, we are surely justified in applying to them these words, which, in more than one view, recapitulate the history of men,—*they have eyes, that they may not see.*

A superabundance of serum in the blood is, in my mind, a positive contraindication to bloodletting; and I conceive that this fact will sooner or later be admitted as a fundamental position in the treatment of disease. It is fully established, that inopportune bleeding may singularly aggravate the condition of a patient, and even render his restoration to health a positive impossibility. I will here, again, have recourse to the experimental method, in the hope of discovering with what proportions of serum and clot the equilibrium, essential to the vitality, ceases to exist. I do not despair of success although I have encountered some obstacles in the investigation, with which you shall be made acquainted. Reasoning from analogy I supposed, that as bleeding alone increased the serosity, the increase would be rendered still greater by replacing the blood removed with water. I consequently made some experiments of this sort, and with the following singular result. Here is an animal who has been bled three times within the last eight days, to the extent of four ounces on each occasion. The moment the operation was finished, four ounces of distilled water, at the normal temperature of the blood, 31° Reaum., were injected into the veins. The animal has meanwhile had an abundant supply of nutritive food. The blood in this glass vessel belongs to him. You must join with me in feeling some astonishment at the nature of its contents. You, no doubt, expected to find a very large proportion of serum in them, whereas a few drops only are to be seen. Here is one of those contradictions to our theories, for which we should be prepared, when engaging in experimental inquiry.

We must not suffer such a mischance to discourage us; for my own part, I shall not so easily be induced to abandon my design. But let this example, of which you will meet many a repetition, impress on your minds one important truth; I mean that there are some experiments which appear to furnish their conclusions, *à priori*, in so direct and logical a manner, that to perform them looks like undertaking a superfluous task; yet, when these same experiments are tried, their results are found to be at complete variance with our anticipations. The reason of this is simple; we are ignorant without suspecting that we are so. Here, for example, I fancied I should increase the proportion of the serous elements of the blood, by adding fluid to it directly; quite a contrary effect has, in reality, been produced. However, I will slit open the clot to see if there be any serum interposed between its lamellæ; none is to be found there. It would appear, on first view, that this fact contradicts our former experiments; but this it does not by any means do. Remember—over again let me warn you of this—that one well-observed fact cannot overturn another of similar character; if such appear to be the case, rest assured that our intelligence is not sufficiently enlightened to comprehend their real relation. Under circumstances like these we should confine ourselves to registering the seemingly contradictory facts, and waiting patiently until further observation enabled us to get rid of the difficulty. This is what we will now do; we will note down, that repeated venesection increased the quantity of serum in a patient, while the same agency appears to have had the contrary effect on this dog. However, it is right to mention, that the circumstances are not precisely the same in both cases; in the instance of the animal there is, in the first place, a certain amount of forces in the organism, which tend to keep the blood within certain limits of composition. There is also a direct and most efficient cause why the introduction of water into the veins should not contribute to increase the quantity of serum in the blood; it is that, as my assistant this moment informs me, the animal has an abundant miction after each injection.

You are aware, Gentlemen, with what rapidity fluids, ingested into the stomach, are carried by the veins of that organ into the general circulation, and thence to the kidneys, which they stimulate to secretion. Thus, when we drink beer, Seltzer water, or Champagne, we are almost immediately seized with a desire to void our urine. There can be no doubt that, if we could retain those liquids longer in the circulation, a bleeding practised before this expulsion, would discover evidence of their presence in the blood; but as they give an extraordinary degree of activity to the urinary secretion, they are quickly removed from the body. To continue our parallel: when a patient is bled he is put on very low diet; the animal on which our experiment was made has, on the contrary, been given abundance of nutritious food, and, therefore, provided with the means of repairing the losses of his blood. A patient, treated on the antiphlogistic plan, is placed in a diametrically opposite condition; his blood is taken from him, and he is, at the same time, deprived of all nourishment; he has nothing wherewith to make up for the lost blood but ptisans, of which, to speak a truth, he is allowed no niggard supply. But as the restoration of the mass of the blood is indispensable, the lost elements must be got, as best they can, from these same ptisans; hence, without doubt, comes the increase of serosity. Still, it would be an interesting point, to learn the means and mechanism by which an animal is, in a case like the present, enabled to maintain the component parts of his blood in their normal proportions.

In spite of anything that may seem to indicate the contrary, it is perfectly true that any signal disproportion between the serum and clot, renders the blood unfit for the performance of its functions. A very curious case, supporting this doctrine, has been recently observed in my wards at the Hôtel Dieu. A female was, some time past, admitted under my care, with most violent uterine hemorrhage, which had existed for two days at the time of her admission. This was the consequence of an artificial miscarriage, induced by the use of those powerful drugs which certain women, whose moral turpitude is even greater than that of the unfortunate beings employing them, make a trade of vending. As I have since learned, it was not her first attempt in this way; she had already succeeded twice or thrice in producing abortion. Such practices would be less frequently had recourse to, if their terrible consequences were better known. I may here say that death is often the most desirable issue in such cases, for it puts an end to most atrocious suffering. In other instances incurable mental alienation, or abdominal neuralgia, that no remedy can soothe, follow. Various cases of this kind have lately come before me, and I distinctly ascertained that serious disorders of the functions of the brain were the occasional consequences of these criminal practices.

In the example to which I more particularly advert, there was, as I said, hemorrhage from the uterus. The general pallor of the subject was very remarkable, as well as the state of prostration and stupor under which she laboured. Her blood trickled away in diffluent clots of a peculiar odour; it was this, indeed, that turned my thoughts to the probability of a premature delivery having taken place; this, however, the patient employed all her remaining strength in pertinaciously denying. I had two ounces of blood taken from her; not on homœopathic principles, but to enable me to prognosticate the probable issue of the affection. Here is the blood; the disproportion of its elements is frightful; there is only 15 per cent. of coagulum. I affirm, that with such a quantity of serum in the blood, the capillary circulation cannot be regularly accomplished. I have before me a new proof of the fact, in the lung of an animal, submitted, as I announced to you, to successive bleedings. At the eighth, the blood was so profoundly altered in its qualities, that it was impossible to continue the experiment for the following reason:—Blood can only be removed from the body through an artery or a vein. In the case of the arteries a clot forms, after the operation is finished, which mechanically blocks up the cavity of the vessel; but if the blood have lost the property of clotting, of course no coagulum is produced. In the case of the veins the edges of the wound become glued together, and unite in such manner as to leave the cavity of the vessel free; but in the condition of the blood referred to no adhesion takes place. You may tie the artery, and heap ligature on ligature, but in vain; they cut through the vascular tunics, and the hemorrhage reappears in a more threatening manner than before. As the very simple cause of these deplorable occurrences was not formerly understood, they were quasi-explained by a term without the least real meaning; they were ascribed to the *hemorrhagic diathesis*. Instances of their occurrence in the human subject are not rare; and only last year an example of the kind happened in the wards of one of our most celebrated surgeons.

Suppose that a woman has hemorrhage after parturition; if her blood be non-coagulable, you may vainly employ compression of the abdominal aorta; as soon as you cease to compress, the hemorrhage reappears, and carries off the patient. You are aware what difficulty is encountered in the attempt to stop the flow of blood in some individuals, after the application of leeches, or the trifling operation of cupping: yet, in both cases, there are only small vessels in question. What difficulty may we not then anticipate, when tubes of considerable diameter, such as the ulnar, the radial, the brachial, or others of still larger calibre, are the subject of hemorrhage in persons of a similar constitution?

But to return to our animal,—he died of hemorrhage; and, according to my theory, we should find affection of the lung, engorgement, œdema, or, possibly, even true pneumonia; and, as I had supposed would be the case, we discover, on cutting into the organ, that serosity oozes from its substance. This is nothing more than the serum of the blood, extravasated into the vascular areolæ, because the latter had lost its just degree of coagulability. What can have deprived it of that property, unless it be the bleedings the animal underwent? Hence that operation was the real cause of the animal's death, as it was so frequently repeated as to render the blood almost or completely non-coagulable. But there is something very remarkable, in a pathological point of view, in the case of the woman of whom I have just been speaking, and whose blood gave only 15 per cent. of coagulum. At the end of forty-eight hours, during which every drug supposed to possess antihemorrhagic virtues was employed, among the rest secale cornutum, peritonitis supervened. You know that by this term is understood a disordered state of the secretion and exhalation of the serous membrane of the abdominal cavity. You find, after death, a viscous liquid, in the midst of which flocculi of albumen are seen to float, &c.

Now, can you fancy that the attack of peritonitis was the result of excitation, or irritation, suffered by the patient? She was, on the contrary, perfectly anemic, and in the most marked state of weakness; and peritonitis is so acute a disease, that it carried her off in less than four-and-twenty hours. Is no relation to be recognised between the liquidity and slight coagulability of the blood, and the affection of the peritoneum? But this is not all; if we turn from the abdomen to the lung, there too, we find a state of engorgement, in other words, serosity effused; in short, lesions perfectly analogous to those detected in the lung of the animal submitted to repeated bloodletting. I may, therefore, legitimately conjecture, that these diseases are to be referred to particular conditions of the blood. In peritonitis I find an effusion of serosity, and I know that there is serosity in the blood also. I also discover a substance solidified in the form of very thin lamellæ; may I not suppose that this is the fibrin of the blood escaped from its vessels, and become organised? This idea has, indeed, been already broached, and nothing has hitherto shown it to be fallacious. The relation that may possibly subsist between the composition of the blood and the development of peritonitis, especially in cases where it follows hemorrhage from abortion, and forms a most serious complication of puerperal fever, seems to me a point of the deepest importance, and worthy of our most anxious attention.

LECTURE IX.

Theories of bloodletting.—Compression of arteries and its effects.—Capillary vessels.—Discoveries of M. Poiseuille.—Circulation of blood in the capillary vessels.—Pressure of the fluid in veins.—Effects of variation in the quantity of the serum.—Theories of inflammation.—Increase of the cephalorachidian fluid.—Modes of examining the blood.—Proportions of serum and of clot.

Gentlemen:—We know, both by experiment and by theory, that the diminution of the extent of the circle traversed by the blood augments the internal pressure in the vessels. We have repeatedly satisfied ourselves that, by arresting the circulation in the right carotid, we cause a rise of the mercury in the instrument applied to the opposite artery; and that a fall ensues when we restore to the vessel its natural permeability. You saw compression of the aorta produce similar phenomena. When the static force of the liquid is annulled in any given point, the impulse of the heart really becomes, as it was fair to anticipate would be the case, more energetic in the portion of the arterial system still traversed by the blood; the moment we cease to compress the vessel, the equilibrium became re-established throughout the entire arterial system. This law of the recovery of the equilibrium of pressure is of fundamental importance in the study of the circulation. Practitioners, who are unacquainted with it, fancy, in many cases, that by following certain rules in the employment of bloodletting, they shall obtain extraordinary advantages, but they sadly deceive themselves. When, for instance, they have a case of apoplexy to treat, they fix on the temporal artery, in preference to any other vessel, whence to deplete the vascular system. As that vessel is the nearest to the seat of the lesion, argue these reasoners, it must hold the cerebral circulation more immediately under its dependance.

No one would have had hardihood enough to deny the excellent effects ascribed to bleeding practised in those points of *election*, as they are called, while the theory which led to their adoption seemed so logically established. I confess that I myself formerly shared in the belief professed by the majority of medical men on the point; but, if you reflect on the matter, you will see that such a notion is utterly devoid of foundation. If the circulation were formed of a series of rings, mutually independent of each other, we might rationally open one vessel rather than another, according to the site of the disorder we had to combat; but the chain formed by the arterial tubes is perfectly continuous throughout the frame. The pressure cannot diminish at one point without doing so, to the same extent, in every other. Whether you bleed from the temporal or the tibial artery, the effects will be mechanically the same in respect of the circulation of the brain. The preference given the former of these vessels is justified by its superficial position, which renders it easily accessible; but as regards the therapeutical influence of its division, it can lay claim to no real superiority. If you represent by five the diminution of pressure in the temporal, you must represent by five also that in the tibial artery. The hæmodynamometer applied to the two vessels would show the same fall in the mercury.

Some days ago I read the article "Saignée," in one of our medical dictionaries, enjoying the highest repute, but was, I confess, totally unable to comprehend the strange notions propounded in it on the subject of the movement of our liquids. What is to be understood physiologically, I would beseech you to tell me, by a *revulsive* or a *derivative* bleeding, on which the writer descants so learnedly? For my part, I knew it not before I met with the article in question, and am at a still greater loss, now that I have become acquainted with the various hypotheses, whereby an attempt is therein made to prove that the opening of one vein is preferable to that of another. If there be a sensation of weight or fulness in the head, we are told to open the saphena vein, because the blood will be directed thus from the head to the feet, and the excessive plenitude of the cerebral vessels removed.

Such are the precepts written and professed by our most distinguished practitioners; just as if vacuity of one portion, and repletion of another, could coexist in a system of tubes freely communicating with each other. Again, the question is seriously discussed, whether it is advisable to bleed from the foot in inflammation of the digestive tube. It is decided, at length, in the negative, on the ground that the blood, rushing in quantity towards the opened vein, would be obliged to pass by the intestines on its route, and would so increase the activity of the inflammatory element. Further, it is a common inquiry, whether in cases of pneumonia of the right lung we should bleed at the right side; and in cases where the left organ is affected, at the left? Opinions are still divided on this point; a good number of practitioners, however, will answer you in the affirmative, and regulate their practice accordingly. But there is another method of bloodletting, which is reserved for great and important occasions, and to which very honourable and conscientious men accord incontestable efficacy; I mean *cross* bleeding. Suppose a case in which a variety of therapeutical measures have proved unavailing, while the disease continues to gain ground; a case, in short, in which ordinary art is powerless. What is to be done under such circumstances? A consultation of medical celebrities is, of course, held, and upon what do you suppose the deliberation sometimes turns? Upon the propriety of opening a vein in the right arm, at the same time as another in the left foot!

I was actually, some while past, one of a consulting party, among whom this proposition led to a discussion worthy of taking rank with the richest scenes of comedy. I do not seek, Gentlemen, to excite your hilarity; the patient was a dying man, who had but a few moments to live. Who would have dreamed that in an age which judges with such severity the prejudices of our fathers, men could be found not only to tolerate, but actually to extol such superannuated practices? Is there, I would ask, such a very great difference between the employment of amulets, which have supplied such a capital butt for our gibes, and the confidence attributed to bleedings, the jets of which cross each other in the form of an X? The day will come, and may it not be far removed, when the profession will refuse to believe, that in the year of grace, 1837, conscientious practitioners of the capital of France, where found to countenance such monstrous absurdities. But, on the other hand, consider the excellent practical deductions to which a correct knowledge of the mechanism of the circulation has led some observant men. Among others, flowing from the experiments you have seen, is a method proposed by a distinguished obstetrician for the treatment of uterine hemorrhage following parturition. It occasionally happens, as you are well aware, after the separation of the placenta, that so abundant and obstinate discharge of blood takes place, that cold astringent injections, the application of topical refrigerents of all kinds, and, in a word, every plan recommended in such cases, utterly fail in stopping it. And, in truth, any one who has seen the size of the venous orifices, that pour the liquid into the interior of the uterus, can have no difficulty in comprehending the powerlessness of the processes so improperly termed hæmostatic. The point, then, was to prevent, in some way, the uterine vessels from receiving the blood propelled by the left ventricle; and this M. Baudelocque has effected by pressure of the aorta through the abdominal parietes. The laxity of those parietes is so great after the expulsion of the fœtus that they may easily be depressed either with the fingers, or by the help of an instrument, which is by far the more advisable plan.

The suspension of the flow of blood through so voluminous a vessel as the aorta has a double effect; it immediately arrests the hemorrhage on the one hand; on the other it diminishes the extent of the circulation, and, consequently, increases the pressure in the arteries situated above the compressed point. As the brain receives more blood it is evident that the cerebral functions will acquire fresh activity; and, accordingly, we find that the patients recover their consciousness, that the face reassumes its natural colour, and that the prostration gradually ceases.

M. Baudelocque has rendered a real service to humanity by the suggestion of this plan; and the recompense voted him by the Institute shows that we know how to estimate its importance. Again, it has been noticed, that one of the chief means of restoring an individual in a state of syncope to consciousness, is to place him in a horizontal position; in that attitude the circulation requires less effort than in the vertical position, which easily explains its favourable effects. Perhaps we might act more directly on the encephalon by compressing a large trunk, such as the brachial, the femoral, or even the aorta, in these subjects. According to the principles of hydrodynamics, the pressure of the cerebral vessels would become more powerful, and the nervous system consequently receive an increase of excitement. I have never tried this plan, but, from the result of our experiments, I can scarcely entertain any doubt of its efficacy.

We have so far studied the static force of the blood without endeavouring to form any separate estimate of the influence of the respiratory movements on arterial pressure. We might, for this purpose, in imitation of M. Poiseuille, open both sides of the chest extensively, by removing the sternum, and, by means of artificial respiration, keep the animal alive long enough to note the oscillations of the mercury in the hæmodynamometer. But I prefer slightly modifying his method of conducting the experiment; instead of putting a sudden stop to the play of the thorax, I will modify it gradually; we shall in this manner be enabled to follow all the phases of the phenomenon. The instrument is applied to the carotid, and the scale marks 75—105, 70—105. A tube has been fitted to the trachea, so that if it become necessary, we may practise artificial respiration.

M. Poiseuille suggests that it would be well to open and shut the cock alternately, which regulates the passage of the air into the trachea; I will act on his suggestion, as it will allow us to estimate the effects of forcible efforts of expiration. The mercury still stands at 75—105 mill. I close the cock, and we have 25—55, 15—50, 10—50, 10—55 mill. I open it, and the mercury marks 100—150, 95—160, 90—155 mill. Hence it appears that the column, which had fallen considerably from the effect of the obstacle to pulmonary expansion, rose the moment the animal had performed some full expirations. Now, that the chest expands and retracts freely, the mercury regains its former level. What we have next to do is to prevent one side of the chest from acting; for this purpose I open the left pleural cavity with the *pleura-borer*. The corresponding lung collapses, and, in order to keep the orifice open, I introduce a caoutchouc tube into it. The animal makes violent and fruitless efforts to breathe. The column rises to 90—125, 80—145, 90—160, 85—165 mill. The sudden ascent of the mercury is perfectly conformable to theory, and cannot, therefore, in anywise surprise you. I next proceed to open the other side of the chest, so as to put a total stop to the movements of respiration; and, with this view, now make a puncture through the walls of the cavity with the *pleura-borer*, an instrument I devised for the purpose.

The sibilus you have just heard shows that the air enters the cavity of the chest. Both lungs are now in a perfectly collapsed state. The scale gives 40—85, 50—90, 45—85 mill. The cessation of respiration consequently induces a notable diminution of arterial pressure. You must have remarked that an involuntary discharge of urine and fæces takes place in experiments, whenever the cerebral action of the animal is modified. This has happened in the present instance. A similar occurrence follows concussion or lesions of the encephalon in the human subject. The contraction of the bladder and rectum augment in proportion as the nervous influence diminishes. I will now perform insufflation through the trachea to replace the action of the thorax. The dog is not dead yet, though both his lungs have now been in a complete state of collapse, from the elastic contraction of their tissue. My assistant keeps the bellows in movement. The column continues to fall; it oscillates between 20—70, 20—60, 45—80, 30—50, 15—25 mill. The sounds of the heart are scarcely to be heard on applying the ear to the precordial region. The animal must inevitably perish, and he is too weak to admit of our pursuing the experiment further.

I have shown to demonstration, Gentlemen, that the elastic tubes known as the arteries, perform a merely passive part in the great function of the circulation; but their size is far from being as easily ascertained by our experimental investigations, as the nature of the part they play in that function. The trunks and branches of a certain size may be easily studied with the naked eye, but such is not the case with their ultimate ramusculi, which are of extraordinary tenuity. According to the latest microscopical researches, it would appear that the diameter of the capillary vessels may not exceed the 1-800th of a millemètre. There is possibly some exaggeration in this estimate; but all are agreed that the diameter of those exquisitely minute ducts does not exceed the 1-150th or the 1-200 of a millemètre. Now the blood is obliged to traverse myriads of canals of this diameter in order to pass from the arteries to the veins. This is a state of things which is not met with in any of our machines; it has, consequently, not been studied by natural philosophers. Physiologists, alone, have directed their attention to it. Unfortunately they have taken so erroneous a view of them, that if we desire to come at any accurate knowledge of these hydrodynamic phenomena we must begin by supposing (which is actually the case) the science to be in a complete state of barbarism as respects it. M. Poiseuille, and several other experimentalists, engaged in the inquiry with this impression, and accordingly their labours form a new era in the history of the circulation.

The first problem that presents itself for solution is this: is the heart's impulse propagated from the arteries to the capillaries? As those two classes of tubes are throughout continuous, it is extremely probable that such is the case; for, speaking in a mechanical point of view, it seems likely that the same force which acts at the origin of a system of ducts will continue its action to their termination. Reasoning is in favour of this opinion. I shall not recur to the proofs which may be adduced in its support, for I have frequently had occasion to expose them to you; and, besides, I am anxious at once to attack the experimental part of the question. Let it suffice for us to know that the thing is not only possible, but that it has in its favour all the chances of probability. It does not require to be a profound anatomist to ascertain that a free communication subsists between the arteries and veins, through the medium of the capillary vessels. Push an injection into the spleen, or kidney, by the arteries, and it will return by the veins. None will deny this fact. But the litigated point respects the force that causes the fluid to move through the innumerable intervening canals. Well, you will see that the heart acts in the living subject as the piston of this syringe on the dead body, and that they who refuse to acknowledge the reality of its action in this way are, in truth, refusing to admit the testimony of experimental observation. It is a curious point for inquiry, to determine in what manner the impulsion propagated from the arteries to the veins is modified by being spread over a larger space. Does it retain its original intensity? No, The same character? No. Then a new series of researches is called for. Although the fundamental phenomenon be literally the same, it is necessary to take into accurate account the peculiarities proper to each locality. The further we advance from the central organ of impulsion, the more we find the rapidity of the flow of the blood diminished. The jerking movement of the arterial blood is very evident in the large trunks, it becomes less so in the secondary tubes, and is totally changed into a uniform motion forwards in the capillary vessels. Once arrived in the veins, the column of liquid moves extremely slowly. The walls of these vessels are scarcely pressed on by the current that traverses them, and are collapsed in the ordinary state; it is for the purpose of causing an artificial distention of them, by the accumulation of a greater quantity of blood than usual in their cavity, that we apply a bandage round the arm, above the point where we intend puncturing the vessel. The slackened flow of the blood in the capillaries is a point which had not been fully explained previously to the latest experiments of M. Poiseuille. To that young physiologist we are indebted for some most interesting microscopical observations on the manner in which the liquids conduct themselves while traversing the capillary vessels.

Among the most important facts he has discovered, are the following. Whenever a liquid moves in a tube, a certain layer of it adheres to the walls of that tube and remains motionless. If the course of the blood be examined under the microscope in an artery which has coats sufficiently thin to admit of the passage of luminous rays, the rapidity of the movement of the globules is found to be greatest in the axis of the vessel. This rapidity diminishes gradually as we pass from the centre to the circumference of the vessel. There is a transparent space next to the internal tunic, which varies in breadth from the tenth to the eighth of the diameter of the tube, and is filled with the serum of the blood. But, as it may probably have occurred to some of you, if this fluid be transparent, how can the reality of its presence be ascertained? How can it be shown that there is no optical delusion connected with it? This is done through the movement of the globules. Some of these are occasionally detached from the central current, and, in consequence, brought nearer the peripheric motionless stratum of fluid, and, at the same instant, this movement becomes much less rapid. Such among them as are jostled by their neighbours, are dashed against the walls of the vessel, and become stationary. Hence a translucid liquid, holding them in suspension, really exists, and communicates its immobility to them. In a large vessel these different degrees of rapidity of the fluid molecules have hardly any influence on the movement of the general current. But if you suppose a tube of considerably less diameter, a greater relative quantity of the liquid will be motionless, and consequently the central column moves in a comparatively narrow area. When the calibre of the vessel is still smaller, the stratum adhering to its walls obstructs its interior almost completely, and a mere filament of fluid can with difficulty force itself a passage in the centre. Finally, when the degree of tenuity becomes extreme, the tube ceases almost wholly to be permeable to fluids.

I have already stated to you my conviction that if it were possible to find inorganic tubes of such exquisite delicacy to experiment on, we should not succeed in forcing distilled water into their interior; and yet the blood, a fluid remarkable for viscidity, and which holds myriads of insoluble globules in suspension, circulates freely, under the influence of a slight impulsion, through canals of this prodigious tenuity. Unfortunately, such is the perfection of the processes employed by Nature in this marvellous system of animal hydraulics, that we are better able to admire than to comprehend it. The composition of the liquids, as I have already said, plays a most important part in the facility with which its molecules traverse either inert or living tubes. Water containing acetate of ammonia, tartrate of antimony, and potass, albumen, &c., passes with greater ease than distilled water. Now, chemical analysis proves that the blood holds several salts in solution,—an important circumstance, which shows at once that that fluid may be expected, on physical principles, to conduct itself differently from a simply aqueous liquid. But, on the other hand, the viscidity of fluids is one of the most powerful obstacles to their passage through tubes of very narrow diameter. Olive oil injected into the veins causes rapid death by the obstruction of the capillary rete of the lungs. The same effect is produced by mucilaginous liquids, such as gum-water, syrup of dextrine, cerebral emulsion, &c. How, then, is it possible for the blood to traverse such infinitely minute canals? Must not its viscidity contribute to retard or even to arrest the circulation completely? Far from this, the very property which is, in the case of inert tubes, most unfavourable to their permeability, is an indispensable condition of the performance of the hydraulic phenomena of living tubes. The blood is even more viscous than those substances, which, when injected, cause immediate death. Deprive it of its viscidity, and you convert it into a fluid unfit to circulate in its natural canals. Must we, then, admit that there is an opposition between physical and vital laws in this instance? I shall leave you to decide for yourselves. If we could make artificial tubes possessed of the same properties as the arteries, with porous walls, and lined with a smooth, unctuous membrane, perfectly fitted for contact with the blood, we might then be entitled to draw such a conclusion, if the results were the same as they now are with our feeble imitations of the arteries, made of metal, glass, or caoutchouc. Between such instruments and the living vessels no just comparison can really be instituted. This is the knotty part of the question.

Observe, I beg of you, that I adduce in support of my assertions the authority of such facts only as you have yourselves been eye-witnesses of. You have seen me deprive the blood of its coagulability either by removing its fibrin, or by alkaline injections. You saw that morbid transudation followed; that the materials of the liquid were effused by exhibition. Hence the composition of the blood cannot be modified, without derangement of the circulation ensuing. Again, you saw me adapt a caoutchouc tube to an artery, and cause some blood to enter its cavity; the liquid became solid in its passage through it. Hence the nature of the walls cannot be changed without disorder occurring in the circulation. The nature of the intimate relation subsisting between the living tubes and the liquid that traverses them is a profound secret. We may modify the physical properties thus intimately bound up with the normal state of the circulation, but we shall strive in vain to endow dead matter with similar attributes: a mysterious and unknown something will always be found wanting in our imitations. It would, therefore, be a very serious error to apply all that is known concerning the passage of liquids in organic capillary tubes, to that of the blood in the living capillaries. There are common phenomena in each case, it is true; but there are others totally different. All must be taken into consideration if we expect to learn anything positive and non-conjectural on the subject.

The adherence of a motionless stratum to the walls of the capillary vessels, is a capital fact, inasmuch as it explains how the course of the blood becomes slackened in those tubes. In order to surmount this obstacle the left ventricle is obliged to dispense a part of its contractile force; but its power is far from being exhausted, for it extends its influence even to the veins, as I shall demonstrate to you. Circumstances exist under which the ventricular impulsion is quite as evident in the veins as in the arteries. Now, this impulsion cannot pass from the latter to the former without affecting the capillary system in its way. If the movement of the blood in the veins were due to the action of the capillaries alone, the motion of the liquid in them would be uniform, and not in harmony with the causes which increase the force of movement of the arterial blood. The degree of the heart's energy, the respiratory movements, the volume of the liquid, would all of them be without influence on the venous circulation. Now, we have the testimony of daily observation in evidence of the contrary. It is positively ascertained that whatever acts on the flow in the arteries, acts also on that in the veins. My experiments had already demonstrated the existence of a close relation between those two great systems of tubes, when M. Poiseuille, by his late researches, gave us a mathematical solution of these important questions, and proved the futility of the theories founded on an assumed action of the capillaries. Before investigating the causes which produce a variation of pressure in the veins, it will be necessary to ascertain the exact degree to which that pressure exists in the normal state. This we will endeavour to do with the hæmodynamometer. The left jugular vein has been laid bare for the purpose in this dog. The extremity of the tube is introduced into an opening made in the vessel: two ligatures are applied; one fixes the upper end of the vein firmly to the instrument; the other ties the inferior end so as to prevent reflux of liquid. Now that all is arranged, I proceed to turn the cock which separates the blood from the subcarbonate of soda. The rise of the mercury will indicate the amount of pressure to which the coats of the veins are subjected. The mercury now begins to oscillate, the scale marks 15, 20, 14, 18, 15, 17, 18 millemètres; so that, instead of remaining stationary, the column falls and rises alternately a few degrees. By watching the movements of the thorax, and following the pulsations of an artery with the finger, we soon ascertain that the greatest elevation of the mercury corresponds either to an expiration or a contraction of the left ventricle. You remember that blood moves in the arteries with greater force during expiration than inspiration: this single experiment would, therefore, suffice to prove the influence of the pressure of the arteries on that of the veins. If the blood, when once it reached the capillary system, were kept in activity by a motor power belonging to that system alone, it is evident that its movement would be invariably uniform. The animal begins to struggle violently, and accordingly we find that a sudden rise of the mercury to 35—40 mill. takes place, showing that some accidental agency has interfered with the ordinary progressive force of the blood. The respiration again grows calm, and it falls to 15—18 mill. Hence the blood in the veins is manifestly influenced by the mechanical agents which augment its force of impulsion in the arteries. Observe, too, that inasmuch as the ascent of the liquid is effected uninterruptedly, there must be some other cause of movement in addition to expiration, and the action of the left ventricle: that cause is to be found in the elastic reaction of the arterial tunics, consequent on their dilatation by each wave of blood propelled forwards by the heart. As for the proper action of the capillary vessels, be assured that such action exists not in nature, but solely in the imaginations of those physiologists who have described it.

I will next apply the instrument to the femoral vein near the crural arch: the extremity of the tube is directed as before towards the capillaries. The pressure is uniform, as you are well aware, in the arterial system generally, but it varies in each quarter, and, indeed, in each vessel of the venous system. I can consequently make no *à priori* affirmation respecting the amount of pressure which the femoral vein supports. The actual application of the hæmodynamometer alone can furnish an accurate estimate of it. The cock is now turned, and the mercury marks 50, 60, 50, 45, 50, 55, 58 mill. The pressure is, therefore, clearly, much more considerable in the femoral than in the jugular vein. There is another important point to be noticed here; although the animal struggles violently from time to time, yet the level of the column remains nearly stationary. You observe none of those sudden and rapid ascents to which I drew your attention in the former experiment. This proceeds from our acting on a vessel placed at a considerable distance from the chest. You already know that the influence of deep expiration is much more marked in the neighbourhood of, than at a distance from the pectoral cavity. I shall have occasion to return to the consideration of this phenomenon, which I simply notice for the present. I long ago established the fact of the increased force of progression of the venous blood during deep expiration. I opened a vein with a lancet, and at the same time induced great respiratory efforts on the part of the animal; I then distinctly saw that the jet of blood diminished or increased according as the air entered or was expelled from the chest. But, although the results of M. Poiseuille's experiments agree with those of my own, yet I have much pleasure in acknowledging the immense utility of the hæmodynamometer in the examination of these physical questions. Any instrument that gives the study of a phenomenon a character of precision is beyond a doubt an invaluable acquisition. We will continue these experiments at our next meeting.

The point in the history of the blood about which we are at present more immediately interested, is the effects induced in the economy by variations in the relative quantity of the serum. If we could succeed in determining, even in an approximative manner, the influence of those changes on disease, and on temperament, we might assuredly lay claim to the honour of having done some service to pathology. But, Gentlemen, this question is not one of those that may be solved in a single lecture, nor in two, nor in three; it requires to be examined in various points of view, and calls for the evidence of numerous facts of an accuracy that none can gainsay. When we shall have studied this branch of our subject, we will turn to the chemical composition of the serum and clot, which, it cannot be denied, still requires minute investigation, although it has already been laboriously studied. We have hitherto done no more than receive specimens of blood into vases, examine them, and the changes they undergo therein, compare, as well as we could, the proportions of their liquid and solid constituents, and inquire if the phenomena of life can continue to be accomplished with such and such quantities of serum and clot. You have already seen, by the serious modifications induced in the state of the economy by certain diversities in their proportions, that we are not without plausible motives for investing this question with the importance we have done. Let me recall to your minds the case of uterine hemorrhage of which I spoke to you in my last lecture, following abortion produced by criminal practices. You cannot have forgotten the consecutive symptoms, such as the intense peritonitis which supervened at the end of two days, accompanied with most violent pains and embarrassment of the respiratory system, and which terminated by death in less than four-and-twenty hours. This case is worthy of close consideration; for, setting aside the signs proper to that fatal affection, we may ask what is peritonitis? what are its mode of origin and first cause? In general the answer is, that it is an *inflammation* of the peritoneum, that the mode of vitality of that membrane is changed by *irritation;* that this *irritation* accumulates the blood in the capillary vessels, which were previously impermeable by it, and that both an increase and a modification of the materials of exhalation are consequently produced. When these different suppositions, and a variety of others, have been more or less carefully enumerated, it is presumed that all has been said; nevertheless, the real question has not been so much as glanced at. For my own part, though I admire the ingenuity of those pathologists, who have made peritonitis one of the inflammations *per eminentiam*, I cannot quite accede to the correctness of their opinion. We found in the blood of the woman to whose case I now especially refer, a most remarkable proportion between the serum and clot; there were 85 parts of serosity to 15 of coagulum. This single fact was in itself enough to suggest to our minds a very different explanation of the disorders under which she succumbed, from that afforded by the doctrine of inflammation; and we had no difficulty in establishing the existence of certain relations between this occurrence and the phenomena daily observed in our experiments. Among other things, you saw that there was a most striking resemblance between the lesions of the lungs in our patient, and those occurring in the pulmonary organs of the animal submitted to a series of successive bleedings. But I must avail myself of the present opportunity to point you out a physical phenomenon long mistaken by medical men for a pathological change. In consequence of the greater or less liquidity and diminished consistence of the blood in certain cases, it becomes infiltrated from the capillary vessels into the pulmonary cells, accumulates therein, and constitutes the effusions known under the name of *hypostatic pneumonia*, which is, in truth, a simple effect of gravitation. Accordingly, this condition is always met with in the most dependant parts; and inasmuch as the horizontal decubitus is that usually affected by patients, the postero-inferior parts of the lung are its ordinary seat, while the antero-superior portions are still healthy and crepitating. If we could open our dead bodies at an earlier period after death, we should not meet with infiltrations of this kind so frequently; a fact we have fully ascertained in the autopsies of animals destroyed in our experiments. Further, the patient in his last moments compresses the lung by his efforts to breathe, and the blood imbibed into the parenchyma of the organ traverses its external serous covering, and goes to form the collections of serum and of colouring matter, such as we found in the pleural cavities of the patient under consideration. We discovered in the abdomen of the same subject a yellowish liquid containing albuminous shreds, yellow colouring matter, and something having a certain analogy to the soft flabby clot formed in the blood removed by venesection. I examined this liquid under the microscope, and discovered a considerable quantity, not of globules, but of particles of a singular form held in suspension, and intermingled with filaments of various lengths. I presume that it was nothing more than blood modified as regards the mode of coagulation of its fibrin, which had transuded through the serous membrane. I detected no globules of pus; and you are aware that nothing is more easy than to distinguish purulent globules under the microscope, when one is at all in the habit of observing them,—so that most decidedly none existed. Here is a piece of intestine which I have had conveyed hither, in order to show you the false membranes. In this organ, as in the lung, we have a very curious phenomenon exemplified; I mean the separation and deposition of a liquid under precisely the same conditions. This, too, is a cadaveric phenomenon, analogous to that of which I have already spoken to you. We may now, I conceive, regard it as an established fact that the curious series of phenomena observed in this woman were developed by the superabundance of serum in her blood. You must acknowledge that I by no means twist the facts so as to make them subservient to my opinions, but that, on the contrary, the latter have actually been modified by the former.

I now proceed to lay before you a fact which will give you a just idea of the difficulties encountered in these investigations. The blood you see in this vase was furnished by a young girl who had a slight uterine hemorrhage after a miscarriage, which she denies having produced artificially; the blood has almost wholly formed into a solid mass, and presents a few drops only of serum. The same phenomenon, to which I have already adverted, occurs in this instance: the clot has imprisoned all, or great part, of the liquid in its cellular structure; it will subsequently undergo a sort of contraction, and then allow of the escape of a greater or less quantity of serum. The manner in which the coagulum forms, evidently exercises considerable influence on the visible proportion of serosity; hence, before forming any opinion on its real quantity, we must ascertain how solidification has proceeded. These reflections on the serum remind me that when I commenced my medical career, imbued with the prejudices of the schools, and just such a novice as men usually are when they give up attending lectures; when, too, like my brethren, I paid my tribute to scholastic dogmatism; that is, I believed in inflammation, irritation, and the rest of it, as in so many articles of faith; they remind me, I say, that, even at that early period, these questions excited my attention. You shall hear how I was led to their consideration. It was at the time I allude to an acknowledged point of doctrine, that the abundance of serosity acted on the blood by *modifying its tendency to inflammation*, in somewhat the same manner as water added to alcohol prevents it from *inflaming*. Here, Gentlemen, the word is used in its true signification. I had, as it happened, set about repeating the experiments of Sir Benjamin Brodie, now one of the first surgeons in England, on the ligature of the ductus choledochus. The animals on whom I practised the operation died, without exception, of peritonitis. With a view of preventing this disagreeable result I practised a copious bleeding before the experiment, fancying, in conformity with the notions then prevalent, that I should thereby infallibly put a stop to the development of inflammation,—the inflammation nevertheless appeared with even still greater intensity than before. Subsequently I injected water in the room of the blood withdrawn, but in every instance peritonitis supervened with greater violence than before, and proved rapidly fatal. At the present time, when more correct notions on pathology have replaced those of former days, it appears to me that the more the blood abounds in serosity the more probable it becomes that the consecutive exhalations of the serous membranes will be abundant; and, hence, that, to use the orthodox language, inflammation will be more violently developed. Now, this fact, alone, shows what fatal consequences may be the result of a fallacious theory, founded on an imperfect conception of the morbid phenomena occurring in the body. I do not hesitate to assert that the anti-inflammatory bleeding ordinarily practised before capital operations may frequently, according to the constitution of the individual undergoing them, help to determine the serious accidents observed to follow those operations. I recommend you strongly to take a note of this proposition, and to watch with attention the issue of cases in which such prophylaxis has been adopted. You will, no doubt, find exceptional cases, but in the majority I make no question you will have reason to admit the justness of this view. The whole forms a subject for inquiry, which, though of great interest, has not yet been examined by any one. I myself long upheld contrary opinions to those I now maintain, but I willingly sacrifice my vanity and acknowledge my error; if all were as ready to do so the progress of our science would be much more rapid. This recalls to my mind certain experiments of this kind that I made in the hospital on the human subject. Formerly, when an individual was seized with the terrific affection known under the name of hydrophobia, the mode of treatment adopted was as follows; and, indeed, these charitable plans were employed at no very distant period from our own:—The wretched sufferer was either stifled between two mattrasses, or bled from the four extremities, and allowed to perish of hemorrhage, or he was put into a sack and thrown, sack and all, into the river. These methods of treatment, originally adopted from ignorance, were continued up to our own time, from the most culpable indifference. You are aware, without doubt, how this horrible disorder is communicated, and are acquainted with its principal symptoms. The most characteristic of these is a spasmodic contraction of the constrictor muscles of the pharynx; the patient becomes furious at the sight of water, and even of any polished surface; a thick and abundant foam flows incessantly from his mouth, and if he were not held down he would rush on the bystanders to attack and bite them; he loses the power of recognising either friends or relations. I felt anxious to try whether I might not, by injecting water into the veins of one of these patients, succeed in soothing the nervous excitement under which he suffered; I was partly successful, for, after I had injected nearly two litres of water into the circulation, the patient became calm; he even asked for water, and actually drank,—a most remarkable circumstance; for, as its name indicates, the disease is characterised by an aversion, a horror of water. I was not fortunate enough to save his life ultimately, nevertheless I had the satisfaction—the most intense of my life while it lasted—of prolonging it for six or seven days, during which he lay in a state of perfect tranquillity; whereas either men or animals affected with rabies ordinarily perish in thirty-six or forty hours, in the most terrible anguish. My patient died, as I said, on the sixth or seventh day, and presented the curious phenomenon of articular dropsy in several joints, no doubt in consequence of the enormous quantity of water injected into the veins. This last fact is exceedingly important in respect of the present subject of our investigations; there is a very strong analogy between these hydrarthroses and the effusion that took place into the peritoneum and pleura of the patient of whom we have been speaking. I will make some experiments on this matter, and inform you of the results. While on the subject of serous effusion I may illustrate it still further by the case of a woman admitted some time past under my care at the Hôtel-Dieu. The most remarkable symptoms she presented were these:—Complete loss of intelligence: partial abolition of sensibility; the patient's countenance expressing slight suffering when the skin was pinched; movements obscure and rare; decubitus dorsal; no deviation of the face; stertorous breathing; pulse natural. These various symptoms had not made their appearance successively and at intervals; on the contrary, the patient had suddenly passed from a state of health to the most dangerous one I have just described. The diagnosis in this case was far from being easy; the suddenness of the attack, and the nature of the symptoms, immediately drew my attention to the encephalon; the absence of hemiplegia, and of other symptoms limited to one side of the body, made me reject the supposition of hemorrhage into one of the hemispheres into the brain; it might be that an effusion of blood had taken place into the ventricles, or into the pons varolii, or into some other part situate on the median line; the combination and general characters of the symptoms induced me to suspect the existence of another lesion. You are aware that a layer of liquid, situate between the pia mater and arachnoid membranes, surrounds the nervous centres. The ancients must have had some knowledge of this fluid, at least such a supposition is warranted by various terms employed by them in describing the anatomy of the brain, such as the *aquaduct* of Sylvius, the *pons* Varolii, the *valve* of Vieussens, &c. Cotugno, an Italian anatomist, noted its existence in the dead body, but modern observers had completely overlooked the fact, and considered the liquid either as a cadaveric phenomenon, or as a product of morbid exhalation. Such was long my own opinion also. It was while engaged in making experiments on the section of the roots of the spinal nerves, that I established, for the first time, the constant presence of a liquid round the nervous substance. I also proved, in contradiction to the ideas of Bichat, that the cerebral serosity was not contained in the cavity of the arachnoid, but in the tissue subjacent to that membrane. As the notions of the old observers were of the vaguest kind respecting this liquid, and as the physiologists of our own time paid not the slightest attention to it before my researches were made public, I think I am entitled to the credit of the discovery. But this is not the place to argue questions of priority; I have simply to state the uncontested fact, that a stratum of liquid exists under the arachnoid in the normal state, to which, from its situation, the name of *cephalo-rachidian* has been applied. If this liquid be suddenly exhaled in excessive quantity the brain will suffer; compression and a series of phenomena, the symptoms of *serous apoplexy, aqueous congestion, acute hydrocephalus*, &c., supervene. This was precisely the sort of alteration the brain of my patient presented. I opened the skull by a process which is, I believe, peculiar to myself, and consists in sawing through the bones of the cranium and the substance of the brain together; the saw I employ is deeper and longer than those in ordinary use, the blade is very thin, and the teeth remarkably sharp, so that the nervous substance is not at all injured. The ventricles were enormously dilated with the accumulated cephalo-rachidian fluid, and the consequent pressure on the brain had caused the symptoms of general paralysis; the nervous tissue itself was perfectly healthy. But to what shall we ascribe this superactivity of the serous secretion? Here we have to acknowledge our complete ignorance. Should we treat such an affection by bleeding? Experience has not yet decided the question, but theory would lead us to doubt the propriety of venesection. You are aware that bleeding diminishes the proportion of fibrin and of colouring matter of the blood, increases that of the serosity, and facilitates that of exhalation; opening a vein, therefore, seems to me a most likely plan to aggravate the violence of the symptoms.

The animal under experiment, Gentlemen, into whose veins distilled water was injected in room of the blood extracted, presented, you will recollect, a very small proportion of serum; the emission of urine, that took place immediately after the experiment, seemed a very likely cause of the absence of serosity; to this I was inclined to ascribe it; I was in error, however. Here is the same clot I showed you at my last lecture; it is now greatly changed; it has since then undergone considerable retraction, and allowed a tolerably large proportion of serum, which was contained in its substance, to escape. Further, the clot is soft and diffluent. This shows of what slight utility the mode of examining blood usually followed really is; such examination will give no clue to the nature of the disease; nay, more, it will often prove the source of serious error, when, as in the present instance, the serosity is retained in the coagulum. For my part, I will endeavour to find out some plan capable of showing, with precision, the quantity of serum that a clot may contain. I will first try compression, and see what effect it will produce; but as to continuing the bleedings, with injection of water, in the case of the dog, of whom I was just speaking, that is impossible, for the animal died while the operator was aspiring the blood from the jugular vein by raising the piston of a syringe. I presume that air passed into the cavities of the heart; we shall learn by the autopsy if this be really the case. In the meanwhile, Gentlemen, remark that it does not follow from the fact of instantaneous coagulation taking place that the blood possesses its normal characters, for if the clot be soft it is almost certain that it contains serum interposed between its meshes. In order to fully make out the characters of the clot we should wash it carefully, separate the globules and colouring matter from it, and so isolate the fibrin; then attentively examine the latter substance to satisfy ourselves that it does not possess the properties of what I have termed *pseudo-fibrin.* I have already explained, at some length, the points that distinguish the latter from true fibrin; I showed that it ordinarily appears, on first view, to form a considerable mass, but that its weight is much inferior to that of true fibrin; and that it is less elastic and tenacious, breaking on the slightest traction. A coagulum may, therefore, be very large, and yet contain nothing but *pseudo-fibrin,* which cannot exist in blood fit for circulation. To this most important subject I shall, however, call your attention at a more advanced period of the course. There is, besides, another way of increasing the proportion of the serum, namely, by injecting that liquid itself into the veins instead of distilled water. This experiment is a remarkable one; if the serum injected be from human blood the operation is followed by most serious consequences. In the animal you see on the table it has given rise to retraction of the limbs, and considerable derangement of the cerebral functions. Last year I performed the experiment also, and it caused puriform effusion into the joints. I will take care to ascertain, on the death of the animal, whether a similar phenomenon occurs in the present instance. The quantity of serum injected was ten ounces. If we introduce, instead of human serum, that of a dog, as I have done in the case of this other animal, an affection follows, attended with most acute pains, resembling those of rheumatism. The animal has grown *intactile,* if I may be allowed the expression,—to such a degree is its natural sensibility increased; the pulsations of the heart are remarkably accelerated; they are 150 in a minute. I will follow up this experiment; meanwhile I proceed to the autopsy of the animal with the soft pseudo-fibrinous clot. Just as I had anticipated, the lung is engorged at its posterior part, which is accounted for by the supine position in which the animal has laid since his death. I cut into the pericardium, which is apparently distended; no doubt there is air in the ventricles. Accordingly, on raising the wall of the right ventricle, I heard a sibilus, such as indicates the escape of gas; besides, the reddish froth, which you now see accumulated in its cavity, leaves no doubt as to the cause of death. It was evidently none other than the *accidental entry of air into the veins.* As regards the effects of bleeding the experiment must be commenced *de novo.*

LECTURE X.

Causes of movement of the blood in the veins.—Differences between the arterial and venous circulations.—Effects of ligature of arteries on the circulation in the brain.—Progressive force of the blood in the deep and superficial veins.—Examination of the blood in a syphilitic patient.—State of the fluid in Bright's disease.—Effects of the injection of serum.

Gentlemen :—Notwithstanding all that has been written on the movement of the blood in the veins, there are few questions in physiology more completely enveloped in error and uncertainty. Our knowledge on the subject is limited to mere hypothesis; the most remarkable poverty in facts is everywhere apparent. The favourite idea—that which takes the lead of all others—is, that the contraction of the capillary vessels causes the progression of the fluid in the veins. Bichat maintained that this was the sole cause of impulsion; the school of Beclard, somewhat less exclusive in its dogmas, admits that the force which sets the arterial blood in motion, acts conjointly with the capillary system in carrying on the venous circulation. On the other hand, my opinion is, that the capillaries have nothing to do with the matter, and that they must be completely stripped of the pretended functions with which they have been invested. Our business on this and similar occasions, is not to divine how Nature acts, but to ascertain, by accurate experiments, the processes she adopts.

The point we are at present more immediately engaged in proving is, that the movement of the blood in the veins is chiefly effected by the heart's action, and by the elasticity of the arteries, which is itself brought into play by the contractions of the left ventricle. I shall not lose sight of the accessory powers that aid in circulating the venous blood, among which the movements of respiration hold the first rank. It results from the experiments made in your presence at our last meeting, that the pressure supported by the veins is very much inferior to that sustained by the arteries. Now, the principle of movement, in both systems, resides in the heart; without it the circulation in either would be at an end. Why, then, it may be asked, are not the hydrodynamic phenomena of that function identical in every point of the vascular system? Because a variety of circumstances, many of which are already known to you, and the remainder of which I shall take care to elicit experimentally in due season, prevent such from being the case. If you take a view of the general disposition of the tubes through which the blood moves, you will be forcibly struck with the difference in the mode of distribution and anastomosis that obtains in the arterial and venous systems. While the blood is propelled by the heart into tubes of decreasing dimensions, it returns towards that organ by tubes constantly increasing in diameter. Now, it is impossible but that the passage of a column of liquid into either narrower or wider spaces than those it previously moved in, must augment or diminish, respectively, the rapidity of its flow. This assertion is founded on a well-known theorem in hydraulics. In the arteries the current is most rapid where it originates; in the veins, where it terminates. The arterial tunics are constantly distended by the blood; the coats of the veins are frequently collapsed. The former are dense and resisting, and react incessantly on the fluid that traverses them; the latter are thin and flabby, and possess only a slight share of elasticity. In the veins you find numerous valves destined to prevent the reflux of the blood; in the arteries no such contrivances are to be seen. The pressure is uniform throughout the arterial system; extremely various in the different parts of the venous. The physical properties of the circulating fluid differ in the two classes of vessels, as well as the character of its movement; in the one the current is rapid; in the other slow. If you open an artery the blood escapes in a jet, and with a jerking movement isochronous with the pulse; if you divide a vein, the jet, when such exists, is feeble and uniform in its flow. But the two departments of the vascular apparatus differ most widely in respect of the number and capacity of their component tubes. An artery is usually accompanied by one or two veins; these are of much larger calibre than the arterial vessel. But, independently of these *satellite* veins, there are others placed more superficially. Whenever an artery and a vein are found together, the predominance of the latter over the former, in point of capacity, may be easily proved. If you take into consideration, besides, the number of the subcutaneous veins, to which there are no corresponding arterial trunks, you will at once understand that the total capacity of the veins is vastly superior to that of the arteries. This superiority is chiefly apparent in those points of the frame wherein the circulation of the blood is accomplished with most difficulty. Again, the main use of the anastomoses between the superficial and deep divisions of the venous system is to facilitate the return of the blood to the central organ. The arteries, from their position, structure, and the direction of the currents that traverse them, are independent of the majority of causes that slacken the movement of the blood in the veins. I pass rapidly over these points, which are capable of very great development, because I presume you are familiar with them. But do not be mistaken as to their importance;—if we neglect the physical conditions of the vessels, the resistance of their walls, and the degree of pressure of the liquid, any hope of framing a rational theory of the circulation may be at once given up. Its hydraulic phenomena will become so many mysteries; any physiological explanations respecting it, so many absurdities.

Although the pressure exercised by the liquid in the interior of the venous tubes is feeble, we have clearly seen that it does exist to a certain extent. When we applied the instrument to the jugular the mercury rose several millimètres; when we transferred it to the femoral vein the ascent of the mercury was still more evident. The force of impulsion of the blood in the latter vessel appeared tolerably energetic; however it did not reach the amount existing in the arteries. In attempting to explain this diminution in the influence of the heart, we must not forget to take into account the obstacle to the movement of the blood produced in the capillaries by the adhesion of a motionless stratum to their walls. But although this phenomenon has a decided influence on the velocity of the venous blood, still it is far from being of such consequence as might be supposed. The greater capacity of the venous tubes, as compared with the arterial, is the chief cause of the diminished progressive force of the current within them. If you represent by *one* the capacity of an artery, and by *ten* that of the veins that succeed it, it is clear that each vein will only receive a tenth of the force that moves the blood in the artery. The impulse of the ventricle is divided, but not lost, in the venous system. This is so true that when we cause the entire mass of blood conveyed to a part by an artery to return by a single vein, the pressure is found to be very closely equal in the two vessels. We are indebted for the discovery of this curious fact to M. Poiseuille; you shall by-and-by have ocular demonstration of it. I propose, also, trying some experiments in the present lecture on the circulation of the brain and of the head in general. We will first apply the instrument to one of the external jugular veins; once we shall have ascertained the height to which the mercury rises under the influence of the current moving in it, we will compress the jugular of the opposite side, so as to force almost all the blood of the head to return by the vein connected with the tube. It is clear that the column must rise in consequence. The carotids have been laid bare, and a ligature thrown round them, in such manner that we shall be able to suspend and re-establish the circulation through them at pleasure. These experiments, like a variety of others we have performed, will, in all probability, have more than a mere physical interest. They may give us some information on certain pathological phenomena affecting the brain, that have been repeatedly observed, but never yet explained. Thus, among the consequences known occasionally to follow the ligature of the primitive carotid in the human subject, stands cerebral hemorrhage. A young girl in whom I tied that vessel on the left side, for an enormous fibro-osseous tumour seated in the superior maxillary region, was seized with hemiplegia of the right half of the body the sixth day after the operation. The paralysis diminished subsequently, but the intelligence of the patient remained impaired, This patient presented a very curious peculiarity dependant on the condition of the brain: before the operation she read very well, but since its performance she has lost the power of combining letters so as to form syllables; she is no longer able to read. No one ever entertained a suspicion that, by diminishing the volume of blood which flows to the brain in a given time, sanguineous extravasation into the substance of that organ would be promoted. Nevertheless, my case is not a solitary instance of such an occurrence; you will find seven or eight examples of hemorrhage induced by ligature of the carotid recorded by authors. Mr. Samuel Cooper has collected several in his "Surgical Dictionary." No satisfactory explanation of these cases can, I think, be given in the present state of our knowledge. Compression of the carotid arteries has lately been proposed as an excellent method of treating cerebral hemorrhage. I have never tried it myself, nor am I aware that any other person has done so either, or at least had to congratulate himself on its employment. It is evidently a mere theoretical conception; and however rational it may be in appearance, it is to be feared that experience would not give any evidence in its favour, as we find that apoplexy follows the ligature of the vessels in question. Perhaps we should, in this instance, as in a multitude of others, be employing, without suspecting it, a simply homœopathic remedy.

I told you we should apply the instrument to one of the jugular veins, and then suspend the course of the blood in the other. The hæmodynamometer has accordingly been fixed, and the mercury rises to 15, 17, 15, 16, 17, 16 millimètres. The blood in the jugular consequently presses with a force of 7 or 8 millimètres, as the weight of the subcarbonate of soda holds 10 millimètres of mercury in equilibrium. I now tighten the ligature applied to the other—the left—jugular. Almost all the blood in the head is thereby diverted to the opposite vein; a very trifling quantity will return by the internal jugular veins, which, you are aware, are extremely small in the dog. The column evidently rises; the scale marks 20, 25, 23, 26, 25 millimètres. I loosen the ligature, and the column falls to 15, 16, 15, 17, 16 millimètres. I will now tie the vein and leave the ligature round it. It will be very easy, if we think fit, to cut the ligature, and so re-establish the circulation in the vessel. The mercury rises in consequence, oscillating between 24, 23, 22, 23, 25 and 24 millimètres. The pressure has evidently increased, but the close vicinity of the chest prevents us from obtaining so well marked results as we should were the experiment made on a distant vessel, such as the femoral. It is well known that any cause which lessens the facility of the movement of the blood in the veins, increase the pressure in those vessels by the accumulation of a greater quantity of liquid in their cavity. A ligature placed round the arm will double or treble their size; the erect position, if long persisted in, will, as well as pregnancy, cause large trunks to become apparent where the smallest ramuscule was not previously perceptible. The arteries are, in consequence of the density and resistance of their tissue, infinitely less subject to these variations in point of size; nevertheless, they do undergo such changes. It is impossible to compute, with any accuracy, the respective capacities of the two systems of tubes. The mode of death enlarges or diminishes the diameter of the veins, according as those vessels contain more or less blood. Injecting them would lead to a very fallacious estimate of their relative magnitude; for the veins are exceedingly distensible, and their capacity may become enormous in comparison with what it was in the living subject.

You remember among the preparations I had brought here from the museum of the faculty, that of an infant in which the inferior vena cava filled the entire cavity of the abdomen. Yet I had given directions that the best preparations only should be selected, in order to give you, as far as possible, correct notions respecting the size and other characters of the vessels. I shall, for these reasons, make no attempt to estimate, even approximatively, the capacity of the veins. The point we are now anxious to ascertain is, whether the pressure does or does not augment in such parts of the system as are left free, while the circulation is suspended in others. The experiment we have made places the affirmative of the question beyond all doubt. Let us now see what will happen on compressing one of the carotids. By this, it is true, we shall diminish the quantity of the blood transmitted to the brain, but the pressure will be increased in the other arteries that participate in carrying on the cerebral circulation. I now compress the carotid, and the mercury gives 20, 22, 20, 21, 19 mill. I no longer interfere with that vessel, and we have, 21, 19, 20, 22, 21 mill. There is, therefore, no appreciable difference. The animal is quite calm, so that the respiratory movements cannot have any very marked influence on the results of the experiment. I now, instead of compressing the carotid with my fingers, tie that vessel. If you had not seen me tighten the ligature, you would not have known, by any effect produced on the mercurial column, that I had done so, for it stands between 20 and 22 mill. No notable increase of pressure occurs except during deep expirations. The sudden rise to 40, 45 mill., that has just taken place, is caused by some struggles on the part of the animal to get loose; these have now ceased, and you see that the column again oscillates between 20 and 22 millimètres. When the left carotid is compressed the vertebral arteries alone supply the brain with blood, and the pressure supported by the coats of the latter vessels will be materially augmented. Will this increase of pressure suffice to counterbalance the effects of the diminished volume of the liquid? The comparative diameter of the vertebral and carotid arteries renders this hardly a likely supposition, but we will try the experiment. You remember that we have already tied the left external jugular vein, and the right carotid artery. I compress the left carotid, and the instrument gives 15, 17, 14, 16, 15, 16 mill. I allow the blood to pass freely through it now, and the mercury rises to 20, 22, 19, 21, 20, 22 mill.; the column, therefore, evidently fell under the influence of the compression; but, to obtain additional certainty of the correctness of this result, we will repeat the experiment. The respiration is easy and gentle, and the animal very rarely makes any effort to disengage himself from the cords that bind him down; we may, therefore, feel secure as to the accuracy of our results. I again compress the vessel; we have 14, 16, 17, 15, 17, 15 mill., and on ceasing the pressure you see it rise to 21, 22, 20, 23, 19 mill. It is, therefore, incontestable that the compression of the left carotid determined a notable fall of the column. Of this experiment we shall hereafter have occasion to avail ourselves in the consideration of certain physiological phenomena connected with the cerebral circûlation. Now, it is certain that, as a general proposition, the pressure cannot increase in an artery without augmenting at the same time in the veins; but we must not forget the relations of the vessel on which we experiment, as they may modify the results obtained. The ligature of the carotid in the animal before me must have increased the pressure in the vertebral arteries; yet, notwithstanding this, the mercury fell in the tube applied to the jugular. The most plausible explanation of the difficulty seems to be this: when the circulation is suspended in the carotids, the blood is conveyed to the brain by the two vertebral arteries only,—vessels which are considerably smaller than the former pair. The quantity of blood carried to the brain by the vertebral arteries, under such circumstances, is no doubt greater than that usually traversing them, but it is less than that reaching the brain in the ordinary state of things.

Had I applied the instrument to the femoral vein, and then tied the carotid arteries, the mercury would undoubtedly have risen several millimètres. In the one case we modify the progressive force only of the liquid in the vessel subjected to experiment; in the other we modify its quantity. The increase of pressure effected in one way is lost in another; as the cause is complex the effect cannot be simple. But, if we even suppose the velocity of the liquid to be such as to supply the deficiency of space in the vessels, and if we grant, further, that the blood conveyed by the vertebral arteries equals in quantity that normally sent to the brain, we shall not even yet have the physical conditions necessary to cause any increase in arterial pressure, to be felt to the same amount in the veins. The reason of this is plain: the capillary vessels, whereby the communication between the veins and arteries is effected, form a vast and intricate rete, each cylinder of which bears, as I before said, about the same ratio to a vascular trunk as a fibril of the root of a tree to its trunk. So long as it is contained in the large trunks and their principal branches, the liquid moves without difficulty, but, when arrived in the capillaries, the obstacles to its passage become serious. The chief of these are the increase of capacity of the vessels themselves, and the motionless stratum of serum adhering to their walls. The narrowness of the cavity available for the circulation is so great that a single globule only can frequently pass through it; and that single globule requires to be altered in shape for adaptation to the area of the canal. Now, these circumstances exercise a very powerful influence on the degree of pressure supported by the veins.

I now proceed to ascertain, experimentally, the progressive force of the blood in a superficial vein of the extremities. The vena saphena interna is laid bare; its small size renders the introduction of the tube of the instrument a matter of some difficulty; however, it is now fixed. The animal is, as you perceive, both strong and restless, circumstances which may interfere with the success of our experiment. The mercury marks 20, 22, 19, 22, 20 millimètres, showing that the pressure is very feeble. I now sieze the animal's paw in my hand, so as to suspend the circulation in the deep vein, above the point where the tube is applied. The mercury rises to 25, 28, 30, 32 millimètres. I now no longer press the limb, and the column falls to 19, 21, 19, 20 millimètres. Nothing can be more simple than this; my hand acts on the limb of the animal as the bandage applied to the arm of a person about to be bled, a greater quantity of blood consequently accumulates in the veins, and the pressure is proportionally augmented. I next proceed to place the instrument in the femoral vein; the resistance of its coats shows that the pressure within it is greater than in the saphena. I have already had occasion to point out this difference in the properties of the subcutaneous and deep seated veins. The tube is now introduced with its point towards the capillaries. I remove the straps from the animal's limb in order to render the circulation perfectly free. The cock is now turned, and the scale marks 55, 60, 63, 58, 60 millimètres; the pressure is, therefore, very nearly arterial in force. This is an important fact; it explains why a wound of a deep is a much more serious injury than that of a superficial vein. The latter class of vessels are rarely the seat of dangerous hemorrhage; bleeding from the former may, on the contrary, be very abundant and difficult to stop. I say that this result is very important, and highly interesting to the surgeon and physiologist. I delight in associating the two names; to separate them designedly is, indeed, an insult to common sense; for the habit of operating on animals gives so much precision to the hand, that when any one so habituated is summoned to act on the human subject, instead of encountering a difficult task, he feels it to be a sort of manual recreation.

In order to estimate comparatively the force of progression of the arterial and venous columns of blood, it becomes necessary to cause the blood driven by the ventricle through a single artery, to return to the heart by a single vein. This has been done by M. Poiseuille; after having isolated the femoral vein and artery he suspended the circulation in the thigh by means of a ligature tightly applied to the limb; and he then ascertained that the amount of pressure was the same in the artery and vein. This experiment, as far as regards the operative part of it, is precisely the same that I performed long since for the purpose of demonstrating the mechanism of absorption; I even separated the thigh completely from the trunk. In the present case circular constriction of the limb is quite enough. The preparatives of the experiment are all arranged; the femoral vein and artery are laid bare, and both enclosed in a noose, which will serve to lift them up if necessary. You see that the mercury still oscillates between 55 and 60 millimètres, which indicates a very considerable degree of pressure for a vein, and is explained by the anatomy of the vascular system of the thigh. The venous trunk on which we act, carries back of itself alone almost all the blood conveyed by the femoral artery to the periphery of the limb. The remaining branches that concur in maintaining the venous circulation are either tegumentary or anastomotic, and of small calibre. I now obliterate the cavities of those branches by forcibly tightening the two ligatures. The scale marks in consequence 80, 85, 75, 85, 80 millimètres. This is almost exactly the amount of pressure indicated in the artery also; the pressure supported by the latter is, therefore, transmitted nearly undiminished to the vein. I loosen the ligature, and the mercury falls again to 62, 55, 58, 60, 58 millimètres, in other words, to its normal level.

I shall now interrupt the circulation in the artery; that of the limb will still be carried on to a certain extent, for I have laid bare the vessel below the origin of the profunda femoris. Besides, there are numerous anastomotic branches, which bring the arteries of the pelvis into communication with the muscular ramusculi of the thigh. I now compress the artery; the column falls from 62 millimètres, at which it stood, to 55, 50, 45, 42, 35, 33 millimètres. Now, that I have ceased to compress it, the mercury rises to 62, 60, 63 millimètres. I again compress, the instrument forthwith marks 55, 48, 45, 40, 36 millimètres; I let the vessel go, and we have 58, 60, 58, 62 millimètres. The venous and arterial circulations are, therefore, intimately connected; whatever acts on the one, acts at the same moment on the other. Any influence on the part of the capillaries is sought for in vain; powerful in books, it is powerless in nature; nay, further, it is shown, by observation, to slacken rather than to accelerate the progression of the blood. In truth, it is not the capillaries that are busy in the matter, but the imaginations of physiologists.

One of your number, Gentlemen, who is attached to the Venereal Hospital, has done me the favour to bring me a specimen of the blood of an individual affected with constitutional syphilis. Examined under the microscope it presented two classes of globules; those of one class were of ordinary form and size, those of the other much smaller. However trifling the importance of the latter may be, still their existence deserves to be noted; I would, therefore, beg of the gentleman who brought me this blood to procure me that of some other individual labouring under the same disease, in order that I may ascertain if the presence of this smaller species of globules in the blood of such subjects, be a constant or a merely fortuitous circumstance. The serum and clot were in this blood in the ratio of 11:30. The clot is soft and diffluent. I must here inform you that the method I fixed on for determining the relative quantities of serosity and coagulum (which consists, as I have already informed you, in submitting the clot to pressure), is fallacious in many cases; we must, therefore, endeavour to devise some plan more certain in its operation. Meanwhile I proceed to communicate to you the circumstances of a very interesting case. The blood in these two glass vessels has been taken from a man affected with Bright's disease,—a complaint characterised by a peculiar disorder in the functions of the kidneys.

The urine voided by such patients is remarkable for containing a considerable quantity of albumen, which may be coagulated by the addition of a few drops of nitric acid. But, before going further, let me point out another illustration, in addition to those already given, of the error into which we may fall, if we trust to inspection alone, as a means of ascertaining the proportional quantities of the serum and clot. The blood in one of these vessels contains 17 of fluid, 62 of solid elements; that in this other, which is of the same nature, in as far as it comes from the same individual, and was drawn at the same time, contains a large share of serosity. It is evident that the coagulum in the former case must contain much serum imprisoned in its substance. It seems to me we might acquire a more accurate notion on this question by first weighing the blood, and then gradually evaporating the serum. This operation would, however, take up at least twenty-four hours, and consequently would be too slow; for the future, therefore, I will take a slice of clot, of fixed size, and weigh it; and as the mass acted on will be small, its desiccation will, of course, be more speedily completed. We must not forget, however, that, by proceeding in this manner, we shall have the solid elements of the serum united with the clot. But to return to the characters of the blood in my patient labouring under *albuminous nephritis.* The ordinary globules of the liquid were easily discoverable under the microscope; but, in addition to these, I detected a multitude of little globules, or rather corpuscula, without any determinate shape, which I might almost affirm were formed of albumen. I then proceeded to a slight chemical examination of the serum, being especially anxious to learn if the serum of this blood would, as in ordinary cases, coagulate by the action of heat. It did form into a mass under the influence of that agent, but when coagulated it resembled pus much more strongly than healthy serum; besides this, an albuminiform liquid oozed from the clotted mass, which I was unable to succeed in coagulating. The solidified part, too, is less firm and coherent than usual; for, as you perceive, the glass rod sinks into it simply from its weight. This examination, imperfect though it be, seems to indicate the reality of a morbid alteration of the albumen of the serum in the disease under consideration; hence we need not be surprised at finding that element in the urinary secretion. It remains to be determined whether the albumen carried off with the urine has the same properties as that we have just examined. I will take the first opportunity of inquiring into this.

From the first moment I set about these researches on the blood I perceived that the simplest questions were far from being thoroughly understood. It has, no doubt, been long noticed that the blood is unusually aqueous, or contains an unusual share of serosity, in persons of lymphatic temperament, in chlorotic patients, and in those subject to passive hemorrhage; but the really important point to be established is the proportion in which the elements must be combined in order to give rise to those pathological conditions. This it is which we should strain all our energies to ascertain; the mind recoils, I admit, before the extent and variety of research that such an undertaking demands, and, so far as the trials we have ourselves made go, I must acknowledge that little has been discovered respecting the precise proportion of serum and coagulum which is incompatible with life. We constantly encounter the most unexpected obstacles. We commenced by receiving the blood into a graduated glass vessel, and noticing the respective heights of the serum and clot. I fancied this was an excellent plan of investigation, but I soon learnt I was in error, by finding that a very considerable mass of serosity might be invisible from its being contained in the substance of the coagulum. This morning we made trial of a new method, which has, much to my regret, proved equally unsatisfactory: I put two parts of water into a glass vessel, and added a third part of venous blood; the mixture coagulated completely, and a few drops only of serum subsequently transuded through it. Again, here is some blood taken from a woman, aged 77, affected with pneumonia. It is true that the disease is not very acute; no trace of serum is here visible, consequently, were we to decide by appearances, we should declare that this blood scarcely contains any serosity at all. Let not these difficulties discourage you, Gentlemen; every experiment we make, though it fail in the end proposed, teaches us something new; our time is never, therefore, wasted.

You remember that, at our last meeting, I injected ten ounces of human serum into the veins of an animal. He died forty-eight hours after the operation, and presented, on post-mortem examination, the following lesions:—His blood was evidently altered in properties by the serum injected; the traces of its disorganisation were so manifest, and its fluidity so remarkable, that on opening a vein in the neck after death, and hanging up the animal by the hind paws, the greater part of the blood contained in the vessels trickled away; the remainder contained a few imperfectly-formed coagula. It is probable that the fluidity of the animal's blood is to be explained by the alkaline character of the serum introduced; the lesions of the pulmonary organs were not very strongly marked, which accounts for death not having been instantaneous; but the most remarkable morbid condition discovered by the autopsy was, beyond question, that of the cephalo-rachidian fluid; it was, in the first place, reddish in colour, as though the globules of the blood were dissolved in it; in the next, it had formed into a sort of coagulum, and seemed of a totally different nature from the normal liquid. I am inclined to think that the albumen of the serum transuded through the capillaries of the pia mater, and so became extravasated into the sub-arachnoid cavity. The injection of simple water into the veins does not determine this gelatiniform appearance of the extravasated liquid. I remember that, in some experiments made a long while since, in which I injected water into the veins, I produced various disorders in the action of the nervous system, such as general trembling, involuntary movements, signs of coma, and various forms of tetanus. I further recollect that I detected disorganised blood under the inner lamina of the arachnoid in the animals on whom those essays were made. This lesion alone would, as it did in the case adverted to in my last lecture, suffice to produce the most serious disorder in the functions of the brain. Now, were I one of those who servilely follow the favourite doctrines of the hour, I might have presented this to you as a superb case of *meningitis;* but tell me, I pray you, should I have thereby thrown any new light on the facts we have observed? None, absolutely none; I infinitely prefer, therefore, stating to you the simple fact, that the blood became extravasated because it had lost those physical and chemical properties which endow it with the capability of circulating through the minute canals of our organs. If we recollect, too, that this animal presented the signs of a lesion of the nervous system, we may easily deduce the conclusion, that its state of prostration, and the frequent contractions of its limbs, were induced by the morbid condition of the cephalo-rachidian fluid; the animal's right eye was seriously affected, and had already commenced to suppurate. But there is another important lesion in this animal to which I wish to direct your attention; I mean the marked disease in the intestinal canal; I mean these patches in a commencing state of ulceration, which, had the disease been of longer duration, would have passed into the state of true ulcers; I mean, too, the tumefaction and engorgement of the mucous tissue, caused by the effusion of blood, and accompanied by all the phenomena which we see manifested in the diseases known by the name of typhoid affections. When a pathologist of the present day opens a body and finds the external surface of the intestine maculated with livid violet, or bluish patches, and on cutting into that intestine, ascertains that its internal tunic is strewed with reddish elevations, formed by swollen follicles in a state of ulceration, and by laminæ of a sort of albuminous matter, he feels perfectly satisfied; there is no need, he conceives, to push the examination any further, for these patches and these follicles are the actual seat of the disorder; the inflammation spontaneously developed in them explains, in the most perfect and simple manner, the whole train of phenomena observed. But, admitting that inflammation has something to do with them, let me ask him, what was the cause of that inflammation. To this he will not easily find an answer; on the contrary, he will be forced to confess that he has given a ridiculous name to a still more ridiculous idea,—an idea without a particle of solid foundation to rest upon, and one of which all the positive knowledge we possess tends to demonstrate the complete fallacy. What is explained by this word inflammation? He not only gives us no additional knowledge by its use, but actually throws a thicker veil than before over the nature of the case; he appears marvellously learned in the eyes of the vulgar, no doubt, but, in reality, he is ignorant of the essence of the disease, treats it according to erroneous theories, and, in consequence, frequently induces serious complications. Will he affirm that the intestinal lesions are primitive? He will fall, if he do, into an egregious error. Do you fancy that if we had put this animal to death before performing the experiment on him which led to the fatal result, we should have discovered the intestinal lesions we now see before us? Indubitably we should not. How, then, it remains to be inquired, was the affection produced? We acted on the mass of the blood by the injection of serum into the veins of the animal; that liquid was alkaline; it liquefied the blood, which, consequently, became unfit to circulate in the infinitely small vessels of the intestinal glands or follicles; its elements separated, the liquid part of them became extravasated by exhibition, and the solid portion distended and burst the delicate tubes it was unable to traverse. Is all this inflammation, or a train of the most simple physical phenomena? Can you wonder if I adopt the latter opinion, when it explains, *seriatim*, the lesions we observe, and the manner in which they were produced? The same explanation is applicable in the case of the mesenteric glands, which are only in a commencing state of alteration, because death occurred in forty-eight hours; they, nevertheless, present the incipient stage of sanguineous effusion brought about by the same cause. I know full well that the doctrine I have laid before you is utterly opposed to the ideas admitted by the majority of pathologists; but against the experiments I have performed, directly illustrating the question at issue, they can have no valid objection to urge. Those experiments show the emptiness of the classification of fevers into entero-mesenteric, ataxic, typhoid, &c., which were established at the expense of so much labour, and which, while they appeared to benefit the science, in reality increased its perplexity, by creating imaginary distinctions between phenomena originating in the same causes, and producing the same effects. What is, perhaps, most to be regretted is, that meritorious men should have engaged in laborious researches respecting these fevers, previously imbued with such notions as have rendered their labours, conscientious though they be, worse than useless.

But, Gentlemen, let me once again assure you that it is not my wish to refer every disease to a morbid condition of the blood. I disavow the doctrine, and protest, in the strongest manner, against such a view being taken of that which I really profess. You see that I allow myself to be led wholly by experience; I have no ambition to advance a single step beyond what it teaches us, persuaded as I am, that in adopting experience as my guide, I have fixed on the surest method of avoiding the errors, absurd in their essence and disastrous in their consequences, into which theorists so frequently fall. Such experiments as that we have this day discussed may become extremely useful in therapeutic applications. Thus, when the cholera raged in England, the practitioners of that country, finding their efforts to save their patients on ordinary principles invariably fail, fancied, like many others, that the disease consisted in a loss of the aqueous part of the blood, which they supposed was carried off with the vomited matters and alvine evacuations. Acting on this theoretical notion,* they attempted to remedy the flux of serosity by the injection of artificial serum into the veins, and actually announced their having obtained marked success by the plan. I had also, though with different motives, employed the same method in desperate cases: I used an artificial serum prepared by M. Persos, but I was not fortunate enough to save a single patient. I am, therefore, of opinion that, if the English medical men were successful, the favourable result must have depended on their patients not having been in so desperate a state as those on whom I practised injection of the liquid alluded to. But, whatever be the state of things as regards those facts, it is now clear that such essays on the human subject would no longer be allowable, because we have reason to apprehend similar results to those observed in the animal into whose veins we injected human serum. Here, then, is another fact of which no suspicion could have been entertained before our experiment; let me counsel you to meditate upon it seriously; it will serve to show how cautious we must be about admitting the propriety of applying any speculation, however specious, to the practice of our art. For my own part I shall always prefer, no matter how afflicting the spectacle may be, leaving a patient to the simple resources of nature, to employing any process, which though useful in appearance, might, in reality, produce the most baneful effects. The question under discussion, bears, as you perceive, on the most serious disorders to which the frame is liable, and is, therefore, of the highest order of interest. But, before coming at its solution (if, indeed, such an honour be really in reserve for us), that is, at a precise knowledge of the proportions of serum and clot which constitute normal blood, apt for life, we must make a comparative study of the albumen, fibrin, and other elements of that fluid. To these points I will direct your attention in my coming lectures.

* M. Magendie has here fallen into a grievous error. The proposal to inject a solution of saline matter into the veins was not founded upon *theoretical notions*, but precisely on his own principles of *experimental* research. Chemical investigations had demonstrated the absence of serum and salts in the blood; the absent materials were discovered in the dejections; hence the idea of restoring to the blood, by direct injection, the materials which it was rapidly losing, in consequence of the disease.—Ed. Lancet.

LECTURE XI.

Pressure of the blood in the vessels.—The sphygmometer.—Influence of the respiration on the venous circulation; it diminishes with the distance from the chest.—Modification of the walls alters the circulation.—Death by entry of air into the heart.—Precautions necessary during the application of ligatures.—Respiratory movements also influence the abdominal viscera.—Mechanism of respiration.—Illustrative experiments.—Attempts to determine the proportions of serum and clot.—Action of salt on the blood.—Albuminous nephritis.

GENTLEMEN:—I have told you that we were without any instrument for measuring the pressure of the bloodvessels in the human subject; there is, however, a little apparatus, constructed with quite different views, which is, to a certain extent, applicable to that purpose; I allude to the instrument called *sphygmometer* by its inventor; it consists of a straight glass tube, terminating, inferiorly, in a metallic infundibulum, the wide extremity of the latter being closed with a piece of gold-beater's skin; the tube is graduated by millimètres, and filled with mercury; a cock is also adapted to it, and by turning this the mercury is allowed to come in contact with, or kept away from, the membrane closing the apparatus. The mode of using the instrument is very simple; you choose a superficial artery, such as the radial, in the close vicinity of the carpus, and lay the instrument by its lower end on the skin, taking care to lean somewhat on it, as the pulsation of the vessel is thus transmitted more perfectly to the mercury. In order to show you this I apply the instrument to the radial artery of this gentleman: the mercury, you see, rises and falls regularly, each oscillation corresponding to an arterial pulsation; the rhythm of the pulse is thus, no doubt, accurately learned, but we cannot estimate, with exactness, its degree of force in this manner. When a deep expiration is made the column, it is true, rises several millimètres; but observe that I produce the same effect by leaning more heavily on the instrument; it is, consequently, impossible to assign to each cause of elevation —the pressure of the walls of the vessels by the blood, and the direct action of the tissues on which the instrument is laid—their respective degrees of influence on the movements of the mercury. The flexibility of the gold-beater's skin causes it to mould itself to the inequality of the skin; the least prominence on the surface causes a rise in the mercury, and this might be mistaken for the effect of increased arterial pressure. Perhaps the instrument might be made more accurate in its indications by changing the membrane now used in it for a caoutchouc one. However, so far as they can be trusted to, the experiments made with the sphygmometer confirm those made with M. Poiseuille's instrument. It is evident that the pressure diminishes during inspiration. This fact is perfectly apparent in ordinary respiration, but becomes much more marked when the chest expands or contracts energetically. In very deep inspiration the slackened movement of the arterial blood may be such as to neutralise, momentarily, the contractile force of the heart; the mercury then becomes almost motionless. It was, probably, in this manner that certain persons, as authors narrate, were enabled to produce the singular phenomenon of a stoppage of their circulation.

We will now examine the influence of respiration on the venous circulation with the hæmodynamometer. The chest represents a pump, which, by means of its dilatation, aspires the liquid contained in the veins; when its walls expand a tendency to a vacuum is produced, and the blood contained in the venæ cavæ rushes towards the right auricle; on the contrary, during expiration, those vessels are compressed; and the fluid they contain repelled from the chest. All this is extremely distinct in the jugular vein; its superficial position and proximity to the thorax render the flux and reflux of the liquid very manifest. If you lay that vessel bare you find that its coats swell or collapse, according as the chest contracts or dilates. In the one case the pressure of the internal air exceeds that of the atmosphere; in the other, the pressure of the atmosphere is the more considerable; the force of aspiration of the right auricle is also to be taken into account; but that force does not extend its influence beyond the thorax, and, consequently, cannot be compared to that of the chest during inspiration. Barry maintained that the principal motor force of the blood, from the origin of the veins to the heart, is none other than atmospheric pressure. This is a serious error; the thing is physically impossible. How is it possible for a force acting only on the walls of a tube to give a circular movement to a fluid contained within that tube; Fill a caoutchouc tube with water, and you will find that the pressure of the atmosphere will not cause any movement in it in one direction rather than in another; but if you introduce the point of a syringe into it, and set the piston in motion, then, indeed, displacement will ensue. The heart represents this agent of impulsion; it is its contractions that communicate their movement to the columns of blood in the arteries, capillaries, and veins. Atmospheric pressure exercises only an accessory influence; though only accessory, however, it is well worthy of being experimentally studied, and this I now proceed to do in imitation of M. Poiseuille: I introduce the hæmodynamometer, filled with subcarbonate of soda, into the left jugular vein, directing its points towards the heart; the point of the instrument is a centimètre distant from the chest; the scale marks zero, You observe that the moment I turned the cock the mercury oscillated, and that its rise corresponded to a movement of expiration; it fell to one of inspiration. Take exact note of the numbers I announce. The sign + indicates the height to which the mercury rises above zero; the sign — the point to which it falls below it. The animal breathes freely and we have

—75, + 50;—80, + 60 millimètres.

He struggles, and, in consequence, the column rises to

— 120 + 105; —100 + 110 millimètres. He is again quiet, and the mercury gives

— 30, + 55; —55, + 90 millimètres.

When the animal struggles violently a still greater variation in the height of the column takes place; it may fall or rise to 250 or 300 millimètres. A similar effect is produced by compressing the sides of the chest with the hand; this I now do, and we have

— 150, + 120; —130, + 145 millimètres.

By these experiments, and others made by M. Poiseuille, we are justified in concluding that during inspiration the blood rushes to the chest, and during expiration flows from it. These results are certainly not new, but the precision with which we are now able to estimate them by the hæmodynamometer is so. You will readily conceive that the presence or absence of valves in the veins on which the experiments may be made must modify the retrograde pressure of the venous blood. M. Poiseuille was astonished, on applying the instrument to the jugular of a dog, to find that the mercury remained below zero during expiration as well as inspiration. He dissected the vein, and traced the cause of this peculiarity to the anatomical structure of the vessel; there were valves at its point of union with the subclavian, so that, when the chest contracted, they prevented the reflux of the liquid. The proof that this was really the obstacle that opposed the ascent of the column is, that when the point of the instrument was placed beyond the valves, the subcarbonate of soda immediately rose to 60 or 80 millimètres.

If the rarefaction of the interior air produced by inspiration were, as it has been maintained, indispensably necessary to insure the movement of the venous blood, the progression of that fluid would cease the moment the pressure exercised on the lung became permanently superior to that of the atmosphere. Now, the contrary is proved to be the truth, by the following experiment, described in one of M. Poiseuille's memoirs:—Make a wide opening in both sides of the chest of a dog, and then practice artificial respiration with a bellows; the air pushed into the lung dilates its cells, and, subsequently, its entire substance. It is evident that the pressure of the air contained in the chest is superior to that of the circumambient air. When you give over the insufflation the pulmonary tissue retracts, by reason of its elasticity; the air it contains has even still a greater force of pressure than that exercised by the atmosphere on the venous system generally. In this experiment aspiration of the blood is at an end; the scale of the instrument, applied to the jugular vein, shows that the elevation of the mercury remains unaltered, and yet the circulation continues.

The further we remove from the chest, the more the influence of the respiratory movements on the course of the venous blood decreases. This is easily ascertained by placing the hæmodynamometer in different veins, as the axillary, brachial, saphena, &c. According as the distance from the thorax increases, the oscillations of the column become less and less distinct, and we at length reach a point where the subcarbonate of soda stands motionless, no matter what violent struggles the animal may make. We may obtain very different degrees of pressure, even in the same vein, by applying the tube far from, or near to, the thorax. Thus, if you act on the jugular at a distance of 15 centimètres from the chest, the column stands still, instead of oscillating above and below zero. The most violent efforts of respiration are scarcely appreciable by the scale; but if you come within 7 or 8 centimètres of the chest, by employing a longer tube, the influence of respiration becomes much more manifest; finally, introduce its point within the thorax, in the neighbourhood of, or even into the interior of the superior vena cava, and you obtain those extensive oscillations we have already had occasion to observe. If the parietes of the veins were inflexible, instead of being, as they are, flexible, the aspiration of the chest would not be confined to the trunks in its vicinity; it would extend to their radicles, were they ten metres long. The structure of the vascular tunics is such as to allow of alternations of dilatation and contraction, which at one moment increase their calibre, at the next block up their interior. Herein lies the real cause of the gradual diminution and total cessation of aspiration of the venous blood through the movements of breathing. This is clearly seen by laying bare a portion of the jugular of a dog; at the distance of a few centimètres from the chest the vein ceases to be pervious, in consequence of the application of its walls to each other by the pressure of the atmosphere during inspiration; the blood returning from the head is thus prevented from entering the chest. M. Poiseuille has very clearly explained this by the analogous case of an inert tube with moveable walls, filled with water, and having a syringe adapted to it; the elevation of the piston corresponds precisely in its effect to the dilatation of the chest,—a tendency to the formation of a vacuum is the result in both instances. Suppose the tube to be insusceptible of locomotion in a forward direction, a small quantity of liquid will, at first, enter the syringe, but the piston will soon cease to be moveable, because the same phenomenon occurs as in the jugular vein of a living animal; the walls of the tube, applied against each other by the atmospheric pressure, form a sort of valve, so that this pressure, which is the original cause of the entry of the liquid into the syringe, subsequently becomes an obstacle to its further admission; and do not fancy, that, by increasing the force of aspiration, you will surmount the resistance offered by the walls of the tubes: on the contrary, the more forcibly you raise the piston, the more intimate will their contact be; the atmosphere will press the walls of the tube together with a force increasing in the direct ratio of the diminution of the internal pressure; the obstacle to any further ascent will, at length, become insurmountable: by modifying the degree of resistance of the walls, we also modify those phenomena we have been considering; if you use a metallic instead of a flexible tube, the aspiration of the piston will be felt at a much greater distance, for the ascent of the liquid in the syringe will not be impeded by the collapse of the walls of the tube; such, too, would be the case with the veins, were their structure of such a kind as to enable them to resist atmospheric pressure effectually. The trachea never collapses so as to form a valvular obstruction in its interior; the cartilaginous rings that form its frame-work allow of the ingress and egress of the air with perfect freedom. Why, then, does not the venous blood move with equal facility in its vessels? Have we not seen that the chest, during dilatation, aspires both that liquid and atmospheric air? The difference exists solely in the physical properties of the bloodvessels and air-tubes: this is so true, that when the veins happen, from any cause, to have acquired such resistance as the trachea normally possesses, the air rushes into them during inspiration, and reaches the right ventricle. Too many examples are, unfortunately, on record, of the occurrence of fatal results through the accidental entry of air in this manner. I have published an essay on the subject, and I presume you remember the experiments I made before you, in the course of which I explained the best means of preventing or remedying this formidable occurrence. Here, again, physiological theory is no more than an application of the most simple laws of physics. Whenever a surgeon practises an operation close to the thorax he should take care to be provided with an instrument fitted for withdrawing any air that might accidentally enter the veins, and, through them, the right cavities of the heart. The chief symptoms of such entry are a peculiar sibilus, tumultuous pulsations in the chest, and spontaneous syncope. It is not the deleterious action of the air on the living tissues that causes death, but the stoppage of the circulation, which takes place in consequence of the ventricle having nothing to contract on but a spurious mass incapable of traversing the vessels. The sole means of saving the life of the patient consist in aspiring the froth accumulated in the right side of the heart, before too large a quantity has reached the lungs and left side of the heart. I say, too large a quantity, advisedly; for it is not true that a single bubble of air mixing with the blood will induce death. I am aware that this assertion makes somewhat light of the organic sensibility of the capillaries, concerning which such marvels are related; but the fact may be easily verified by any of you. Open the jugular of a dog, and inject air into it, taking the precaution to perform the operation extremely slowly; the animal will continue to live after receiving into its veins, not a single bubble, but an entire syringe full of that fluid. Next vary the experiment, by pushing the piston rapidly, and you will find that death follows instantaneously. The difference in the effects depends on the manner in which you cause the entry of the air. In the one case the air reaches the blood gradually, by successive bubbles: its presence in the small vessels constitutes no obstacle to the continuance of the pulmonary and general circulation. When, on the contrary, you suddenly force in a considerable volume of air, that fluid, dilated by the heat of the organs, distends the walls of the ventricle to such a degree that they cannot contract. The movement which subsequently takes place in the heart depends on contraction of the left ventricle; the right cavities have scarcely any share in it. Hence the importance of causing an assistant to compress, or of tying the cardiac orifice of any good-sized vein that may chance to be opened in the neighbourhood of the chest. And in the application of a ligature there are certain precautions to be taken. You must be careful not to raise the vessel by introducing one of the branches of your forceps into the interior, and applying the other to its outside; in this way, acting on one side only of the cylinder, you run the risk of permitting the air to enter when you draw the vessel from the wound. The best plan is obviously to seize the vein by its external surface, and then tighten the ligature.

The influence of the dilatation and retraction of the pectoral parietes is not confined to the organs contained in the chest; the abdominal viscera are quite as directly influenced by the movements of respiration; and, as this constitutes an additional means of pressure on the venous tubes, I shall enter into a few considerations respecting it. The mechanism of the thoracic and abdominal cavities must first be called to mind. During inspiration, the ribs, their cartilages, and the sternum, are carried upwards and forwards; the diaphragm contracts and descends, carrying with it the lungs and pericardium. The chief diameters of the chest are thus increased in extent. But observe that its vertical dilatation is effected wholly at the expense of the abdomen. The diaphragm drives the abdominal viscera downwards in proportion as it acquires a plane instead of a curved form; expiration succeeds. The mechanism of this movement is the inverse of the preceding; the thorax is restored to its ordinary dimensions and relations by the elasticity of the costal cartilages and ligaments, by the relaxation of the muscles of inspiration and the contraction of those of expiration. The fibres of the diaphragm relax, and that muscle, driven upwards by the abdominal viscera, reacquires its vaulted shape. The ascent of the muscle is also promoted by the elastic retraction of the pulmonary tissue, which exercises a manifest traction on every point of the thoracic walls. The increased size of the abdomen is thus in the direct ratio of the contraction of the chest. The two cavities are jointed into one another in such a manner that a series of horizontal incisions, from the ensiform cartilage to the last intercostal space, would pass into both of them. The vertical diameter of the first cannot increase unless by diminishing that of the second, and *vice versâ*. Some physical consequences, bearing on the venous circulation, are deducible from these anatomical facts. The vascular trunks that traverse the abdomen to reach the right side of the heart, are influenced by respiration, but it is through direct compression of their walls, and not by the absence of equilibrium between the internal pressure and that of the atmosphere. In other words, when the chest expands, the abdominal viscera press forcibly on the venous trunks, diminish their diameter, and so drive the column of liquid where it can find an issue. The blood passes partly towards the thorax, partly towards the lower extremities, but in the latter direction it encounters the valves which arrest its progress; it is consequently forced to push on to the chest, and its entry into that cavity is further promoted by the dilatation of the right auricle. There are, therefore, at first, two currents in opposite directions, which, however, almost immediately coalesce into one. Expiration follows, and the abdominal viscera, compressed by the walls of the abdomen, press in turn on the vessels. The same displacement of the columns of blood, the same play of the valves, and impulsion of the blood towards the thorax are again produced. The blood contained in the abdominal veins and vena cava, is evidently urged on towards the central organ of the circulation during both movements of respiration. These phenomena have been very accurately described by M. Poiseuille, who has made numerous experiments in elucidation of the theory of their mechanism. Proceed we now to repeat some of them. The instrument is applied to the femoral vein of a strong dog, close to the crural arch; its extremity is turned towards the heart, and reaches into the abdomen about as far as the sacro-vertebral symphysis. We shall have no action of the valves to apprehend, for the point of the instrument is placed beyond them; and we may rest satisfied that the least reflux of the blood will influence the column of subcarbonate of soda. I commence: the liquid, which stood at zero, now oscillates. You observe that each oscillation corresponds to a respiratory movement. We obtain

During inspiration +45, +38, +40, +35 millimètres.
During expiration +70, +65, +75, +68 millimètres.

The column, therefore, never falls below zero. This is an important fact, and if you compare the position and other conditions of each vessel, you will easily understand why the phenomena should differ in the jugular and femoral veins; you have only to call to mind the general considerations into which I just now entered. Every kind of accidental pressure will cause the liquid to rise. I compress the walls of the abdomen with my hand, and the scale immediately marks +290, +300, +350 millimètres.

There is incontestable proof that the rise of the column during expiration depends on the pressure exercised on the vessels by the walls of the abdomen, in the fact that when those walls are cut open no sensible elevation in the tube is any longer observed. As regards inspiration, it would be curious to prove, directly, that the descent of the diaphragm is the cause of the increased pressure of the venous blood. The experiment has never been made, but it would be possible, by dividing both the phrenic nerves, as I did long ago, in the course of some experiments on vomiting, to paralyse that muscle, and so put a stop to its influence. I have no doubt but that were this done, the elevation of the column corresponding to inspiration would no longer be observed.

We are once again, Gentlemen, obliged to recur to the question of the means of determining the proportion of serum and clot that exists in normal blood. As this point is capable of becoming of the very greatest importance, our time cannot be considered wasted in its investigation. You remember the various difficulties we met with in inquiring into it, simple as it may appear; and though I had flattered myself that I had at length triumphed over them, a new and inexplicable fact has occurred which shows the imperfection of the methods of examination I had devised. I allude to the case of the animal into whose veins I injected distilled water. The animal was bled a few minutes after the operation, and the blood drawn coagulated perfectly well, scarcely any serum making its appearance. This absence of serosity is truly surprising; our injections must indubitably have augmented the aqueous part of the blood, and yet the solid elements appear the more abundant. We rarely observe a fact at all extraordinary in its characters without, in spite of ourselves, as it were, endeavouring to explain it. In this respect I pay my tribute, like the rest of the world, to the nature, or rather the weakness, of the human mind, ever impatient to learn something new. But you will do me the justice to allow that I never advance such explanations as final; I invariably submit them to the touchstone of experience, and when this proves them fallacious I seize the opportunity of pointing out to you the slight value of these extemporaneous hypotheses. I was extremely desirous of striking out some plan for ascertaining the proportions of the clot and serosity, and, after some unsuccessful efforts, hit upon that of evaporating the blood. This was a tedious process, but not to be rejected on that account. Still it could give us no insight into the cause of the clot becoming more voluminous than natural after an injection of water into the veins; new inquiries were, therefore, called for. There was, at the same time, a young woman, aged twenty, in my wards at the Hôtel-Dieu, completely anemic, and presented a most distinct *bruit de diable* in the carotid arteries; in a word, all the signs of a superabundance of serosity. Now, here is some of her blood, and positively I would not affirm that it contains more serum than clot.

As distilled water dissolves the globules of the blood, I mixed the latter liquid with sugar and water, which does not alter their characters in any way. In this case a centilitre of blood and sixty centilitres of water have been mixed together; the results are, that the colouring matter which, as you are aware, contains ferruginous particles, and is, therefore, of greater specific gravity, has fallen to the bottom, and the fibrin, or coagulable matter, remains suspended through the whole mass. At a distance, the liquid appears to contain fluid materials only, but, on looking closely into it, nothing can be more plain than that it contains fibrinous filaments, forming a sort of rude cellular structure by their interlacement. Now, I put a very small quantity of blood only into this sugar and water, whence it follows, that the structure represents a rare and light coagulum, the meshes of which would have imprisoned the globules had they not been precipitated. Suppose these distended cells, now floating in the sugar and water, approximated and compressed together, and you will have a regular clot, minus the globules. It is thus the coagulum is, in all instances, organised. This experiment was made with arterial blood. Here is a second mixture, composed of thirty centilitres of sugar and water, and three centilitres of arterial blood. The results are of the same character, but more strongly marked. You perceive that the fibrinous clot fills the vessel almost completely. Instead of filaments, as in the former mixture, we have lamellæ, a species of membrane, interlacing in various directions. Four centilitres of blood, and sixty of sugar and water, have been mixed together in this vessel. You observe a fibrinous clot here, above the colouring matter; this, by the way, reminds me that we may be fortunate enough, by the plan we are employing, to clear up the origin and history, generally, of what is termed the *buff* by pathologists. Certain questions respecting it, which have vigorously employed the pens of writers, such as, whether it is really a morbid product; whether it is contained in the blood, or formed after the blood is drawn from the body, shall be treated of at length in their fit place, namely, at the close of the present course. But I may now state, that I believe the *buff* to be nothing more than fibrin deprived of the colouring matter; fibrin, which is consequently lighter than that retaining the colouring material in its meshes: the latter, in virtue of its greater specific gravity, would sink to the bottom; and the former, in consequence of the same physical law, would rise to the surface. In the next vase, which contains 7 centilitres of blood, and 60 centilitres of water, we discover a phenomenon perfectly analogous to that which we remarked in the animal into whose veins we injected water, and in whose blood, notwithstanding, no spontaneous separation of the serosity took place. Here, in truth, in spite of the enormous proportion of fluid added to the blood, the clot fills the major part of the vessel, whereas there are only a few drops of serosity. Now, here is a case in which, no matter how sorely it may puzzle the vitalists, the same phenomenon occurred in an organised vessel, and in an inorganic vase. I do not make this remark, Gentlemen, under the influence of any systematic or preconceived doctrine; it is plain and straightforward inference from two facts of which you have yourselves been eye-witnesses. It is to me, who disavow every opinion and theory not based on almost physical certainty, a matter of little consequence whether this or that phenomenon be of this or that nature; whether it belong to the domain of physics or chemistry; or, on the other hand, exist under the influence of life, provided we succeed in establishing the cause, the mechanism, and the consequences of its existence. Eight centilitres of blood and 60 of distilled water, are mixed together in this last vessel, in which you perceive the clot is of very considerable size. Hence it follows, that blood, well mixed with even so large a proportion of liquid as this, will not, on that account allow of the appearance of serosity during the process of coagulation; and, as a further consequence, it is clear that the inspection of the clot cannot give any notion, approaching even accuracy, respecting the quantity of serum contained in the blood. In support of what I advance, here is the blood of the animal who underwent aqueous injections into the veins. Although a considerable time has elapsed since the blood was drawn, no greater quantity of serosity is now visible than immediately after the operation. Henceforth, therefore, we can feel no reasonable doubt but that, in following the method of simple inspection, we should have fallen into most serious errors. However, there is one thing more to be done, before we finally abandon that method of proceeding; namely, to examine the manner in which the organization of these coagula diluted with water is effected, and ascertain how the liquid conducts itself with respect to the fibrin. The microscope will, no doubt, throw some light on this problem, which at present appears very difficult of solution.

I have an experiment, which has led to a very novel result, to describe to you:—I mixed together 60 centilitres of water, 3 decigrammes of hydrochlorate of soda, and 20 cube centilitres of blood. The second named substance, which is the common salt used for domestic purposes, is remarkable, like sugar, for not dissolving the globules, but it promotes their separation from the coagulable matter, or fibrin. Hence the clot I now show you is of a very different size from those we have hitherto examined; besides, it presents a well-marked horizontal retraction, a peculiarity which was not observable in the other clots of our manufacture. From this it results that hydrochlorate of soda has considerable influence, not only on the coagulation of the blood, but on its colour; for you see that this clot is of a very beautiful arterial red hue, even in its centre, where it has not been in contact with the air. Nature has, therefore, had good reason for spreading this substance in such abundant profusion over the earth, and for giving man and the lower animals the instinctive desire for its use which they are well known to have.

Experience has long taught veterinary practitioners the vast advantages derivable from the use of sea-salt, and it, in consequence, takes rank amongst the most important substances in their Pharmacopœia. A phenomenon of analogous character is to be seen in this other vase, in which I have made a mixture of equal volumes of blood and salt water. You observe that the coagulum is really enormous,—an additional proof that the condition of the clot may furnish no evidence of the existence of any fluid in its composition, though fluid may really be present to a very large amount. This clot, too, is firm, solid, and resisting; no matter in what way I turn the vase containing it, not a single drop of serosity escapes. What has become of the salt water I added to this blood? Has it become chemically combined with it, or is it simply interposed between the cells formed by the fibrin during the process of *organization?* Do not be surprised at my use of the word; a little consideration will, I think, prove to you the justness of its application. Compare the phenomena of the coagulation of albumen and fibrin. In the case of the former substance you have nothing more than an amorphous body, the molecules of which have drawn near to each other, and assumed a new arrangement, under the influence of a physical or chemical agent, while that body itself has mechanically lost its transparence, its viscidity, and its liquidity, through the new arrangement of those molecules. If you divide the mass, the most scrupulous and attentive examination will not detect the least trace of organization, nor anything tending to show an analogy between the phenomenon which has occurred and that of crystallization. Fibrin, on the contrary, coagulates spontaneously; it is its nature to do so. Examine it closely when in this condition, and you will find a real texture, or parenchyma; you will observe filaments crossing each other in a variety of directions, and you will see that these filaments contract adhesions with each other, bound spaces of variable regularity, and thus form cells resembling those discovered by the microscope in our most delicate tissues. This will be rendered more than ordinarily evident, by placing it in any liquid having the property of holding it in suspension, and separating it from foreign bodies, as you have seen to be the case with a solution of sugar in water. I would not affirm that the coagulation of the fibrin is a purely vital phenomenon, but I have no hesitation in stating my belief that something more takes place in it than in crystallization; I look upon it as an action that occupies a middle place, if I may so express myself, between *vitality* and *inorganism.* Be all this as it may, one thing, at least, is certain, that had we, a month past, when we commenced our study of the coagulation of the blood, been shown a clot, such as that we have been speaking of, we should not have fancied, for a moment, judging from its consistence and solidity, that it could possibly contain much fluid. Here, on the other hand, is some pure blood of an animal bled yesterday; neither liquid nor solid has been added to it, and yet it presents a larger proportion of serum than that which really contained a large quantity of liquid. These facts suffice to show the necessity of mistrusting appearances. The man who makes his observations with the most scrupulous exactness may err egregiously in respect of the simplest facts; the history of physiology abounds with such cases, wherein observers who have triumphed over the most perplexing problems, have completely failed on questions of comparatively trifling difficulty. Let me, therefore, recommend you, as I have always done, experimental inquiry, as the counter-test of such facts as you may conceive you have discovered; believe me, it is the touchstone of truth.

I have a few words to say respecting a disease to which I adverted in my last lecture; I mean albuminous nephritis. You remember the case in which I injected human serum into the veins of a dog. In addition to the results of that experiment already narrated, I ascertained that its urine had become albuminous. I proceed to repeat the experiment: these two glass vessels contain urine; the one that of a patient labouring under nephritis, the other that of the dog in question. On pouring a few drops of nitric acid upon the two specimens, you observe that they both become turbid, whiten, and that, finally, whitish flocculi, of perfectly analogous character, in both instances, make their appearance. The colour of the two fluids is not exactly the same; but this is the sole difference between them, and probably depends on the variety of salts that one of them may contain. This appears to me a curious result, for if it were confirmed by further investigation, it would show the necessity of changing the mode of treating *albuminuria* at present adopted, which is utterly empirical; the ordinary prescription in such cases consists of bitter ptisans and mercurial pills. Here is another fact belonging to the same category; the urine I now show you comes from an animal who died at the end of twenty days' total abstinence from solid and liquid nourishment, and it appears to be albuminous, for nitric acid throws down a very abundant precipitate in it. These comparisons of similar affections, produced by different causes, are very important; for if, in the present case, the privation of food induced the disease, it is clear we ought to avoid bleeding, putting on low diet, or otherwise weakening any individual affected with it. I do not, however, lay any great stress on this point for the present, as the fancied resemblance may not be a real one.

The animal lying on the table is the subject on whom the injection of serum was practised. The lungs, as you perceive, are but little affected; neither in the kidney, or in any other part of the genito-urinary apparatus, are there any of the anormal granulations described by Dr. Bright to be seen: it would appear that there is no lesion of any kind in the tissue of the organ; but the most serious disorder, that which in all probability caused the fatal result, is an enormous invagination of the small intestine, which is also in a state of gangrene. The invagination was no doubt produced by the frequent efforts made by the animal to go to stool.

LECTURE XII.

Reflux movement of the blood in its vessels.—Movement of the blood in the capillaries.—Peculiarities of the capillary circulation.—Appearances and properties of fibrin.—Formation of nebulous and secondary clots.—Vascularity of the clot.—Physiology of the coagulation of the fibrin.

Gentlemen:—I shall conclude the subject of the movement of the blood in the arteries and veins by some considerations on the progression of the liquid in the opposite of the normal direction. The same tube that conveys it from the centre to the circumference, may, under certain circumstances, convey it from the circumference to the centre. Two mechanical agencies appear to concur principally in the production of this reflux of the liquid,—the elastic retraction of the walls of the vessels at the moment the left ventricle expands, and anastomotic communications. When an artery is cut across, the upper end retracts on itself, as well as that which corresponds to the heart, and consequently the blood rushes towards the points where there is least resistance. It will, therefore, chiefly direct itself towards the open orifice. But the flow of blood will not stop the moment the coats of the vessel retract; the anastomotic branches opening into its interior keep a constant current in movement within it. The blood travels freely in the contrary direction to that of its natural course towards the solution of continuity, more especially where several trunks open into each other, as at the base of the skull.

Surgeons who have recommended the ligature of both ends of a divided artery, have given us a wise precept; they have not described the physiological foundation for it, however. I propose measuring the force with which the blood tends to obey the retrograde force, with the instrument we have already so frequently employed. The experiment is quite new; I will have the hæmodynamometer applied to the primitive carotid, a ligature having been previously placed round it; in consequence of the anastomotic communications which unite this vessel with the opposite carotid and both vertebral arteries, it is probable that we shall find about the same amount of pressure in its superior and inferior ends. The impulsion must lose very little of its force by distribution among the branches which combine to form the vascular circle of the skull. The obstacles to circulation, and sources of slackened velocity, are not the same as when the blood is obliged to traverse the capillaries. Therefore, I repeat, I do not think that, on applying two tubes, one to the cardiac, the other to the distal extremity of the carotid, we shall have to note any considerable difference in the level of the two columns of mercury; still we must wait for the decision of experiment. The left carotid has just been laid bare, and the point of the instrument introduced into its cephalic extremity. We shall measure only the force with which the blood tends to advance in the contrary of its natural direction. The anastomoses of the right carotid and vertebral arteries are the chief routes of communication for the liquid. I turn the cock: the mercury oscillates between 60 + 70, 60 + 65 millimètres. This is nearly the degree of pressure we should find in the lower end of the vessel, that is, in the direct current of the blood. You perceive, therefore, of what importance it becomes to apply two ligatures, in order to stop the hemorrhage following division of such an artery as this; the blood would spirt with equal force from both orifices. I now compress the opposite carotid, and we find 50 + 55, 53 + 58 millimètres; there is, therefore, a slight diminution of pressure. This shows you that the anastomoses exterior to the skull are not the principal routes by which the blood returns; for, when we arrest the circulation in the vessel which feeds them, the level of the mercury is but very slightly affected. I no longer compress the right carotid, and the scale in consequence marks 70 + 73, 65 + 68 millimètres. The ascent of the mercury was, as you saw, very sudden. Such is the facility of the vascular communications between the arteries that carry on the cerebral circulation, that the ligature of one of those vessels does not prevent the blood from flowing to, and moving in, its interior. We have on a former occasion learned that the vertebral arteries will furnish the encephalon with blood enough for the support of the hydrodynamic phenomena. Still the singular fact of the supervention of apoplexy after ligature of one of the carotids remains to be explained. In order to judge more accurately of the comparative force with which the blood is moved by the direct impulsion of the heart, and indirectly through the medium of anastomoses, I now apply the instrument to the lower end of the carotid. The mercury gives 60 + 70, 65 + 75 millimètres. This is very nearly the degree of pressure in the upper end.

We will now leave the subject of the venous circulation for the present, and pass to that of the capillary system; and the change is a satisfactory one, for our existing knowledge of the capillary circulation is much more complete and accurate than our acquaintance with the general circulation. In the large vessels we can only judge of the state of the internal currents of blood by the appearance of their walls: according as the latter dilate, contract, become curved, straight, or elongated, we perceive that the former moves with such and such velocity and energy; in fact, it is by induction only that we are enabled to analyse its phenomena of progression. The thickness of the vascular tunics prevents us from directly inspecting the globules of the blood,—an obstacle which does not interfere with their examination in the capillary vessels, as, by the help of the microscope, we are enabled to follow perfectly well the displacements and oscillations of the coloured corpuscule floating in the serosity. The sole difficulty in this branch of the system consists in devising rational explanations for the phenomena observed. I constantly, as you are aware, brought the laws of hydrodynamics to bear, in explaining the circulation in the large trunks; to the same laws I shall refer in our new investigation.

The tenuity of the capillaries is, therefore, favourable instead of being an obstacle to the examination of the course of the blood in their interior. Injection lends very feeble assistance in the study of the capillary circulation: it can, at the most, do no more than give some anatomical notion of the material arrangements of the vessels themselves. The blood is not the only fluid that moves in the beautiful rate formed by their interlacement: there are certain organs and tissues into the capillary system of which that fluid does not appear to enter at all in the normal state; but, if you push an injection into the vessels, it will penetrate into the canals wherein fluids of another description usually circulate, quite as well as into those in which blood is ordinarily found. Thus, when the material injected is well fitted for the purpose, and cautiously introduced, the serous membranes will become covered with vascular arborisations. Now, the vessels you descry on its surface, when thus injected, were not, during life, traversed by blood; white fluids only were contained in them; but as these hold no opaque granules in suspension, their mode of circulation cannot be accurately followed during life. The lymphatic capillaries have as transparent walls as the sanguineous, and yet what we know of the phenomena occurring in them is exceedingly limited. If you lay bare and examine a serous membrane, you can discover no circulation in the interstices of its tissue; nevertheless, that tissue is principally formed of infinitely minute tubes. You cannot allege that there are no liquid currents in movement; the rapidity with which substances deposited on its surface are absorbed, gives you the proof of the contrary. When this system of white canals, in consequence of some morbid condition, comes into relation with the blood, a fluid hitherto unconnected with them, you may perceive myriads of vessels in the substance of the membrane. It is this change in the colour of the circulating fluid that betrays its presence; so long as it remains diaphonous we are unable to study its course.

I have already adverted slightly to the researches of M. Poiseuille respecting the capillary circulation. I have told you how that observer ascertained that the blood moves in the capillary vessels in the same manner as a liquid in an inert tube; in both cases a motionless stratum adheres to the inner surface by a sort of affinity. Its existence in vegetable tubes, also, has been established by the same experimentalist. Examine the course of the blood in a vessel spacious enough to allow of the passage of several globules abreast, and you will perceive that their velocity of movement is very great in the centre, and less so in the neighbourhood of its walls, while in the stratum of serum they are nearly motionless. In the axis of the vessels the globules are only subjected to a movement of translation; in the neighbourhood of the stratum to one of translation and rotation. The latter is more and more marked the nearer they are to the stratum of serum. The globules that happen to be dashed into it become motionless; those which merely touch it undergo a movement of rotation, as if they had jostled against an undulating surface. This stratum protects the vessels by means of its immobility, as it prevents the friction of the globules against their walls. It is only where it is in contact with the periphery of the vessel that it is perfectly at rest; the globules move with a progressively increasing velocity, in proportion as they are close to the axis of the vessel. This difference of velocity and movement in the globules placed in the centre, or near the periphery of the vessel, does not exist in its lateral direction only. Those occupying its inferior part are slower in their progress than those situate at its upper end, as may be easily ascertained by examining comparatively those two parts.

The irregularities occurring in the movements of the globules are, therefore, to be ascribed to their relative position to the adhering layer. Thus, let us suppose two globules advancing together with equal rapidity; one of them, jostled by its companion, is driven towards the periphery, its movement slackens, and it remains behind; the other continues on its way. Another jolt, from some other globule, restores the one remaining behind to its former place in the centre, and the moment this occurs it is borne away by the current, and recovers its previous velocity. In other cases a globule gets placed crosswise, so that both its extremities are immersed in the motionless stratum; its movement is thus slackened; others overtake it, press upon, and accumulate behind it; the passage is intercepted, and a sort of dyke opposes the further advance of the globules. Shortly after the globule which has caused all this disorder moves from its anormal position, becomes longitudinal, resumes its motion, and at the same instant all the others reacquire theirs. These agglomerations of globules very rarely occur in cases where the heart retains its full force, and the animal continues strong; they are, consequently, in general, observed only towards the close of an experiment. The existence of a motionless stratum being a continually present and powerful cause of slackened movement in the globules, it is indispensably necessary for the force that moves the blood to have a certain degree of power in order to carry on the capillary circulation. By means of anastomotic communication all these minute canals are made vicarious of each other. When a mechanical obstacle is created in any point, the globules stagnate; the resistance is out of proportion with the motor power, and the liquid remains at rest in its tubes. The central globules are not in the least influenced by the motionless peripheric stratum in the large vessels, in consequence of the distance separating them from it. In the capillary vessels, on the contrary, they are obliged to traverse a mass of serum, of which the central filament alone, if I may so speak, possesses any rapidity of motion. Haller, Spallanzani, and other physiologists, saw the globules advance, retreat, and move in a variety of directions; but these are not, as has been maintained, vital phenomena. The arrangement of the globules among each other, their relation to the motionless stratum of serosity, and other circumstances, give us the key to the comprehension of all thsoe peculiarities. We will continue our examination of this subject in my next lecture.

Gentlemen, in investigating the question of the appearance of albumen in the urine, I, of course, experimented on the largest number of animals I could procure; among the rest was a defibrinised dog. On my submitting his urine to the action of the usual reagent, a large number of flocculi, of albuminous appearance, were thrown down. Now, had I been inclined to give the reins to conjecture, a multitude of hypotheses might have been conceived, in explanation of the presence of albumen in this case; and had I not deliberately considered the matter, and examined the urine analytically, I should have regarded defibrination of the blood among the conditions capable of determining the symptoms of Bright's disease. Nothing, in truth, could have appeared more rational or more probable, considering the results of our previous experiments; nevertheless, close examination showed me that what I had at first taken for flocculi of albumen, resembled laminæ of cholesterine much more strongly. Here, again, is the same appearance, but still more marked, in the urine of another defibrinised dog. The quantity of micaceous particles is really immense, rendering the whole fluid almost solid in appearance; yet I have ascertained, by analysis, that this precipitate is formed neither of cholesterine nor albumen, but of nitrate of urea; and I have further to inform you, that I have, in the same manner, assured myself that the precipitate thrown down in the urine of the animal who perished from forced abstinence consisted of the same salt. For this analytic result I, indeed, prepared you in my last lecture.

The true, or physiological serum, contains (besides the water, albumen, chlorides of potassium and sodium, lactate of soda, carbon, phosphorus, and animal matter found in the serum removed from the body), the globules and coagulable matter of the blood. The latter, the substance well known as the fibrin, exists in the state of solution or suspension, and only becomes manifest to our senses when it changes its nature and solidifies; it then constitutes the clot; and by it, the most living part of the blood, if I may so speak, we will commence our study of the elements of that fluid. To accomplish this perfectly, we should be able, as I have before pointed out to you, to examine it while circulating in its proper vessels; but, unfortunately, this is impossible, and we are obliged to confine our inquiries respecting it to its solid form. Our recent experiments will prove of considerable use to me in this respect, for I am not aware that any other person has isolated the fibrin from the rest of the blood, at the moment of its assuming a distinct form, and organising into a net-work, to form the clot. Physiologists have hitherto been contented with beating up arterial or venous blood immediately after it had been received into a vessel, by which process numerous filaments became attached to the rods used for the purpose. These filaments were, in the main, composed of fibrin, but were also invariably coloured red, from the adhesion of the globules to them; the latter were only separable from them, so as to render the fibrin nearly colourless, by repeated washing. This is the method employed by butchers to prevent the blood from coagulating.

Another manner of procuring fibrin consists in allowing the blood to coagulate, submitting the clot to pressure in a linen cloth, and then washing it, so as to remove the colouring matter. But by this process we simply obtain a compact mass of fibrin, which is only available for the determination of the specific gravity and chemical composition of that substance; no one ever seriously attempted to grapple with the question of its organization when in this state. Both these methods, besides the expense of time and trouble they demand, are, therefore, radically imperfect. Had the nature of the blood been better understood, and had medical men not obstinately persisted in regarding the *buff* as a particular morbid element, they might have availed themselves, without any trouble, of a means of studying that substance. Thus theories, having no solid basis to rest upon, are perpetually, and even in the most trifling details, interfering with the progress of the science. When we examine the blood in circulation in a living animal, we perceive nothing but a liquid, bearing along with it the globular bodies to which I have so frequently adverted. No trace of fibrin is to be discerned, and hence the impossibility of studying it in the state of solution or suspension. As I have not, by my own researches, added anything to what is known through chemical analysis, I shall not trouble you with an enumeration of the proportions of carbon, oxygen, hydrogen, and nitrogen, entering into its composition; these you will find in the most recent works on the subject. To the question of its organization I wish more particularly to direct your attention. Some time past I had remarked, that on receiving blood into a vessel containing sugar and water, the fibrin separated from the globules, and formed a sort of lamellar tissue, with very delicate meshes, resembling in structure the organized membranes of the animal economy. This was too remarkable a fact not to excite my curiosity; I repeated the experiment, and the same phenomenon invariably made its appearance; you see on the table before me numerous specimens of it. All these vessels, which I also used in my last lecture, contain fibrin, more or less distinctly organized. One of the most remarkable circumstances connected with the formation of these net-works, is the fact that the presence of much liquid seems to offer no opposition thereto; thus, whatever be the quantity of water you add to the blood, provided it contain the least share of fibrin, that fibrin will detach itself from the rest of the fluid in the shape of long filaments, which contract adhesions with the inner surface of the vessel, interlace, become agglutinated together, and so form an undulatory net-work, modelled to the shape, and filling the whole interior of, the vessel. Here we have ten grammes weight of blood and sixty of water, yet you can distinctly perceive the parenchyma that has been formed. I propose that these cellular and filamentous, scarcely visible masses, be termed *nebulous clots;* for they really resemble a transparent cloud, or light thin froth. I examined a small lamella of this clot under the microscope with the following results: I first saw an infinite quantity of minute, sinuous, undulated lines, as it were, festooned, and placed side by side; here and there were some small globules, of less size than those of the blood, the nature of which I was unable to determine; next large septa, which divided the mass, and interlaced in a multitude of directions. Many observers have advanced as their belief that the fibrin is formed of globules; I must confess that I consider them in error, for though I have examined that substance with all the attention of which I am capable, I have never succeeded in discovering in it the slightest trace of a globular structure; the notion should, therefore, I think, until at least some new evidence be adduced in its support, be rejected as unfounded.

I have another very curious phenomenon to describe to you, connected with the same subject. I have removed one of these *nebulous clots* from a mass of liquid, and sat it aside to examine its structure more closely. I was on the point of throwing away the remaining contents of the glass vessel, when I perceived that a new clot, of still more cloudy appearance than the first, had formed in the interim; to this I have given the name of *clot of secondary formation.* By this unexpected occurrence I learned that part of the fibrin remained in solution in the liquid, and became organized on the removal of the first clot. I beg to add, that though this formation takes place in sugar and water, it does not do so in other vehicles, as, for example, a solution of subcarbonate of soda; it is necessary for its appearance that the fibrin be in a condition capable of coagulating. Every one of the mixtures of blood and sugar and water on the table produced clots, more or less voluminous, according to the predominance of the fibrin. Here are some more solid than those of which we have been speaking; they form numerous cells in the interior of the vessel, and the walls of those cells give off filaments, traversing their cavities in every possible direction. These cells are filled with the liquid; and it is very probable that the curious fact we observed of blood, to which we have added a large quantity of water, solidifying in such manner as to allow of the escape of only a small proportion of it, is to be explained in this manner.

But to pursue our analysis, physiologically speaking, of the coagulum; underneath the *nebulous clot* you perceive a more dense stratum; this is nothing more than what has been termed by pathologists the *buff.* Its composition is the same as that of the *nebulous clot,* but its texture is more compact, and its cells contain less fluid. This, which may now appear to you little more than a mere assertion, I trust I shall rigorously demonstrate to be the truth in due season; meanwhile let me assure you that the *buff* has no real claim to the importance usually attached to it; it is nothing more than so much fibrin, as I have already mentioned to you, separated from the globules during the process of coagulation. I have no doubt but that, in the course of a few years, medical men will smile at the recollection of the horror with which that terrible phantom was once regarded. The blood of some animals, the horse among others, contains a very large quantity in the normal state; and veterinary practitioners, herein much more sage than ourselves, far from looking on this mere physical effect as a pathological sign, have termed it simply the *white clot.* Nor is it in the blood only that fibrin is discovered in the free state; none of you are unaware that it enters into the composition of the false membranes developed in certain affections; that it is occasionally met with in collections of fluid that accumulate in the cavity of the pericardium, or pleura; and that it appears in its most evident form in the pseudo-membranes of adhesions formed between the edges of a solution of continuity, or after loss of substance.

There is another property connected with the formation of the clot, of which I have so far only spoken to you incidentally, I mean the retraction that takes place in it, to a greater or less extent, according to the conditions in which it is placed, conditions respecting which very little is known. Nevertheless, it is right to note this phenomenon when it occurs; and here is an excellent example of it in the clot formed in a solution of hydrochlorate of soda. I showed it to you in my last lecture; it then filled the whole vessel, it has now lost one-third of its former size by retraction. Remark, too, the extraordinary circumstance, that the most trifling movement of the vessel seems to communicate myriads of tremulous motions to the mass, which spread, by undulations, through its entire substance, and give it the appearance of one of the mollusca, moving itself by a series of vibratile contractions.

The word fibrin is of recent introduction, at least I no not recollect to have met with it in the writings of any author previous to Lavoisier; the name was given to the principle of the blood under consideration from the presumed identity of its composition with the muscular fibre, which, according to some physiologists, consists of filaments of fibrin. I have already mentioned to you an important fact, showing the fallacy of this notion, but shall, at a future time, enter more fully into the subject. MM. Prévot and Dumas have advanced, in an essay published in their joint names, that the coagulum is formed by the adhesion of the globules to each other. This theory of its formation has been generally adopted, and many chemists and physiologists of the present day subscribe to its correctness. This error does not surprise me, for as serious ones are committed in less difficult investigations; and, in truth, when we examine an ordinary clot it appears quite clear that the globules are present, adhering to the filaments of fibrin, and appearing to form an integral part of it. But this is, after all, a mere appearance; if you wash the clot sufficiently the globules are carried away, or dissolved, according to the nature of the liquid used for the purpose, and the fibrin makes its appearance with all its true characters. This is a fundamental fact, on which it is most important to have correct ideas; the globules are, it is true, present in the coagulum formed in blood drawn from a vein, but not as an integral part of it. I am the more anxious to impress this on your minds, as my conviction respecting it differs from that of observers of the highest order. One among them, whose immense and well-deserved reputation gives great weight to every opinion he originates or espouses, believes not only that the globules enter into the organization of the clot, but, further, that when they have once formed part of it, their structure changes if they be again separated from it.

I really cannot understand how Berzelius, the glory of modern chemistry, came to adopt such an opinion as this; one which falls to the ground before a most simple experiment. Take some blood, beat it up; you separate the fibrin thereby, and there remains a liquid, incapable of coagulating, composed of the globules and serum; next examine these globules with the microscope, and you discover in them the same lenticular form, the same investment, the same central point, and the same colour as before. No change has, therefore, taken place in them; you find them, after the operation, exactly what they were before it. Now this point is really of great interest; all my experiments go to prove that, strictly speaking, we might understand the possibility of life in blood composed of serum and fibrin only; as to the globules, nothing positive is known respecting their uses; and, for my part, I know no other kind of utility they possess beyond that of facilitating the microscopical investigation of the blood. Nevertheless, as they do exist, and consequently there must be some reason for, and usefulness in, their presence, in the midst of the vital fluid, they shall, in due season, receive their share of our attention. The phenomenon of the separation of the serum from the fibrin has been a favourite subject of inquiry with physiologists, but as they were unable to obtain the fibrin in a solid state without the globules, they thought that the latter enjoyed equally the property of coagulating. Burdach and Müller, however, appear to have attacked the question in the same manner as I now do. While engaged with experiments on the blood of frogs (the globules of which are, as you are aware, of a different form, and larger than those of the mammalia), they had remarked, that when that blood was laid on a filter a certain quantity of a transparent, colourless fluid passed through, leaving behind it a small reddish mass; the latter consisted of the globules; the separated fluid soon coagulated. The filter they employed was made of a rabbit's bladder, and the process was simple and ingenious, but little fibrin was obtained by it. The method of procuring it, by mixing blood with sugar and water, is more successful in its results. The species of membrane to which I have already called your attention, and which you see floating in these vessels, resembles strongly those lining the uterus during the early period after fecundation. At that period the uterus undergoes a particular modification: and I am inclined to conjecture that the distension its tissue undergoes dilates the orifice of the vessels terminating in it; the blood is distributed to it in greater quantity than usual, its fibrin transudes on the fœtal suface of the organ, and forms the membrane placed between it and the ovum. From what we know of the properties of the fibrin in the formation of the cicatrices of wounds, I should incline to believe that the fœtal membranes really become organized, though I have no decided opinion on the question.

In the *nebulous clot* I now show you, you will perceive a certain vascularity. The occurrence of this vascularity did not escape the genius of John Hunter, who rather divined, than discovered by research, the importance of these studies. I saw, indeed, in his anatomical museum in England, a specimen of a clot presenting an appearance of real organization. Here is another example of coagulated human blood, in which a very distinct organized framework is visible; this fibrin is colourless too, which results from our having employed sugar and water, and so prevented the solution of the globules. Had we mixed this blood with pure water there would still have been a very distinct clot; but, as the water would have dissolved the globules, the red colouration of the liquid would have rendered it impossible to scrutinise the structure of the coagulum properly; hence, according as we modify the nature of the liquid in which we place the blood, the results vary. I trust these experiments will lead to some valuable inferences; so far they are certainly very curious. Here, again, ten parts of dog's blood have been mixed with sixty of human serum; and observe the singular production to which the mixture has given rise. We have a cylinder widened at both ends, these being formed of areolar tissue, while the colouring matter is deposited in the centre; the *nebulous clot*, instead of appearing at the upper part, in its usual position, is seen at the bottom of the vessel, while the *cellular clot* is placed superiorly; the deep red substance you see in the middle constitutes the ordinary clot of pathologists; here and there are scattered membraniform formations, which connect the cylinder with the surface of the vessel. I must try whether a variation in the proportion of water will cause a modification in the arrangement of the fibrin, for I am of opinion—this I confess to be a simple conjecture however—that it does not solidify in the same manner in different acute affections.

Physiologists have exerted themselves much in endeavouring to ascertain whether the coagulation of the fibrin is a physical phenomenon, or one dependent on vitality. Certain substances possess the property of solidifying, such as pectic acid and gelatin; the latter, mixed with one hundred times its volume of water, forms into a mass when submitted to a temperature gradually cooled down. It is clear that no vital influence is in action in this instance; the serum also solidifies when brought into contact with acetic acid; but this is simply the result of the chemical action of the acid on albumen; but the case is different with fibrin. Besides, when albumen and serum are solidified by heat, or a chemical agent, no traces of arborization, which are, indeed, distinctive of fibrin, are detected. For my own part I do not attach such great importance to its being of chemical, physical, or vital nature; nevertheless, it is well I should remind you of a fact, which, if it had been correctly observed, militates against its being of the latter character. It is this: if we submit blood to the action of intense cold before coagulation it congeals; and if we then remove it from the influence of the low temperature it becomes liquid again, and next coagulates. I confess that this is one of the strongest objections that can be brought against the vital organization of the fibrin, for coagulaton usually destroys life in all animals. Nevertheless, as I only give you this fact on hearsay evidence, I draw no conclusion on the matter until I have verified it myself. I will expose some blood to the action of a frigorific mixture immediately after the lecture, and I hope to have something of a positive nature to communicate to you on the subject when we meet again.

If, instead of a saccharine solution, we add carbonate of soda, in solution, to the blood, in the proportion of sixty-three of the former to seven of the latter, the blood so treated remains liquid; it even undergoes a commencement of decomposition; no traces of fibrin or globules are to be found, for both are soluble in the alkali. Here, then, is a substance which evidently opposes the coagulation of the blood; and as the property possessed by that fluid of forming into a mass, exercises a powerful influence on our organs, any agent interfering with it must prove eminently prejudicial to the exercise of those functions. I, consequently, propose to undertake a series of experiments in which we will pass in review the action of the chief medicinal and alimentary substances on the blood.

I have made a mixture of ten parts of blood and sixty of a solution of hydrochlorate of ammonia in this vessel. The result is the formation of a clot filling the whole interior of the vessel, but of slight powers of resistance, tearing, with facility, and retaining much water in its meshes. Here is another mixture of the same substance, but in different proportions: sixty-five of the saline solution to five of blood. In this mixture the colouring matter has been dissolved, and a sort of partial coagulation takes place; the clots resemble the currant jelly (*gelée de groseilles*) of pathologists. The same substance will, therefore, produce different effects, according to the quantity in which it is employed. But do not, therefore, suppose that these researches are devoid of utility; they are intimately connected with pathology. Many and many a time you must have had occasion to observe at post-mortem examinations blood and clots resembling those now shown you. You took note, no doubt, of this character of the blood, but did you inquire into its cause? Probably not; yet it is this which it would first have come into your minds to do if you had not been preoccupied by the systems in vogue, which attribute everything to the solids, and refuse the liquids all participation in the production of morbid alterations. Here is a mixture of two parts of venous blood and sixty-eight of an aqueous solution of hydrochlorate of soda; it presents a scarlet-coloured clot; it is also distinctly organized, and is the seat of a kind of peristaltic motion, due to its extreme elasticity. This other mixture of sixty parts of the same saline solution, and ten parts of blood, is wholly solidified; it is firm, resisting, and, like the former, of arterial hue. It results, from all this, that certain bodies diminish, or destroy completely, the power possessed by the blood of forming into a mass, while others promote its tendency to coagulation.

LECTURE XIII.

Capillary circulation.—Ideas respecting the spontaneous movement of the globules refuted.—Proofs that the capillary circulation depends on the heart's motion. Coagulation at a low temperature.—Effects of loss of coagulability.—Transfusion of blood.—Action of carbonate of soda.

Gentlemen:—I shall, in this lecture, proceed with the subject of the capillary circulation. I may premise by informing you that the movement of the blood in the capillaries of one's own body may be seen in the following manner:—Place yourselves in a strong light, close your eyes, and then stretch the upper lids so as to render them as thin as possible; they will permit the passage of a few luminous rays, and you will perceive, though of course indistinctly, the blood moving from the upper part of the lid towards the tarsal cartilage. In investigating the capillary circulation in animals the microscope is always employed, and the animals on whom such examinations may be most effectually made are frogs and salamanders, among batrachians; mice and small rats, among mammalia. The tail of some fishes, too, is, from the transparence of the integuments, well adapted for inspections of this kind. I shall extract the greater part of the facts I am about to lay before you from M. Poiseuille's "Essay on the Capillary Circulation." As this Essay has not yet been printed, I shall quote some passages verbatim, commencing with the simplest phenomena.

The experimentalist separates the femoral artery and vein from their connections with the surrounding tissues by delicate dissection, and then passes a ligature round the thigh, taking care to tighten it forcibly; the circulation of the part is now carried on solely by the two vessels named. The animal is pinned down to a plate of cork, so as to make the interdigital spaces correspond to the object-glass of the microscope. When he has ascertained the degree of velocity of the globules he intercepts the course of the blood in the artery, leaving the vein free. The globules still continue to move, but they do so more slowly; and their movement gradually grows slower and slower, until, at the end of two or three minutes, it ceases completely. When the compression is taken off, each globule which was in a complete state of rest, instantly starts off with the rapidity of an arrow and recovers its normal velocity. Some physiologist, on observing the persistence of the movement of the globules after the impulsion of the heart had been prevented from acting on them, were, very naturally perhaps, induced to recognise a sort of inherent progressive force in those bodies, which, they further supposed, directed them from the arteries towards the veins. Others conceived that the latter vessels exercised a kind of aspiration on the globules. Both these notions are erroneous. The same phenomena to which I have already drawn your attention, as existing in the vascular trunks, occurs in the capillary vessels. The facts with which you are already well acquainted show that the movement of the globules is to be explained by the elastic retraction of the arterial walls below the ligature. When the passage of the blood is intercepted in a large artery the vessel retracts abruptly, the diminution of its diameter takes place suddenly. In the capillary system, on the contrary, retraction is a slower process, and this difference in point of elasticity in the small and large tubes, explains why, in the experiment I have just described, the globules continue to move for several minutes after the application of the ligature.

The mesentery of a frog is next separated from the animal, and spread out on a piece of glass. A certain quantity of blood escapes from the opened vessels, and as those are no longer dilated by the column of blood impelled forwards by the heart, they retract, and their retraction is so considerable that the diameter of some veins and arteries decreases to half its original amount. The flow of blood only ceases when the vessels have reached the utmost limit of elastic retraction; and the greater number do not undergo retraction throughout their entire extent, there are swollen points observed here and there. Now, this irregularity in the retraction of the capillary walls depends on the manner in which the small quantity of blood remaining in each tube is distributed and undergoes coagulation. Masses of globules accumulate in several points, and to each of these accumulations corresponds a swelling of the vessel; for the coats being arrested in their retraction by a physical obstacle are unable to retract so fully as if the cavity were empty. The notion of the existence of different degrees in the retractile force of the capillaries is, therefore, an incorrect one: remove any clots they may contain, from their interior, and the diminution in their calibre will be the same through their entire extent.

One of the chief proofs adduced in support of the opinion that the globules are endowed with a faculty of spontaneous movement is derived from the following experiment. It has been said that if, while you examine the circulation in a capillary vessel, you make a small hole in any point of its walls, the direction of the current within it is instantly modified. The whole column of blood, which a moment before obeyed one and the same impelling power, separates into two distinct columns: these, moving in contrary directions, rush towards the little orifice you have made, and effect their escape through it. The action of the heart has ceased to be felt beyond the accidental opening,—the movement of the globules there situate cannot, therefore, be explained by that action; besides their movement is a retrograde one. What other cause, it is asked, but an act of their will, can cause these intelligent corpuscula to move in this novel direction? The fact, Gentlemen, is unquestionably correct, and the inference drawn from it is, no doubt, specious enough; but a little reflection clearly shows it to be faulty. We know that the pressure exercised by the blood in the normal state on the walls of the arteries and veins is superior to that of the atmosphere. Now, this pressure is suddenly diminished at the point of section; to this point, consequently a rush of blood must, in pursuance of the laws of the equilibrium of bodies, take place. Whatever be the existing direction of their course it will now become that of the artificial opening: this movement of the globules is also promoted by the elastic retraction of the walls of the vessels, which press circularly on the blood, and, in the absence of the impelling influence of the ventricle, communicate a retrograde motion to it. I see no necessity for imagining hypotheses, when the physical explanation of the phenomenon is so easy and natural. But, further, if you distend a caoutchouc tube with an injection, and then make an opening in its central part, the contained liquid will rush thither from both ends of the tube. The case is the same as with the living vessel.

The oscillations of the globules, their times of stoppage, and their movements in various directions, depend, so long as the coats of the tubes are uninjured, on the position they hold with respect to the motionless stratum. A spontaneous obstruction frequently takes place through an accumulation of globules: those which happen to be in the neighbourhood of a collateral branch escape by it; others advance and retreat while waiting for the passage to become free again. Once the obstacle is passed they all resume their ordinary speed. When a capillary vessel contains a greater number of globules than its neighbour, the velocity of the current within it decreases greatly; this is the direct consequence of the contact of the globules with the peripheric layer of fluid. Let me next suppose an experiment in which you have separated a frog's web from the rest of the animal; the globules continue to move for a certain time; in the arteries the blood retrogrades; in the veins, on the contrary, it pursues its natural course. After a certain lapse of time the vascular tunics regain their normal diameter by help of their elasticity, and all is quiet in their interior. If you now slope the object-glass slightly the globules immediately move towards the dependant part, if the blood be still uncoagulated; but this change of place is not the result of a power inherent in them; it is a simply physical effect of their specific gravity being greater than that of the serum in which they float. If you slope the object-glass in the opposite direction, the globules change their position accordingly; place it horizontally, and you will find that the equilibrium becomes gradually established, and that, after a few oscillations, the globules again become motionless.

Hear another experiment, which proves in the clearest manner that the movement of the blood in the capillary vessels depends on the impulsion of the heart and the elastic retraction of the walls of the vessels. I extract it verbatim from M. Poiseuille's manuscript:—" The femoral vein, artery, and nerve of a frog are accurately separated to the extent of two centimètres, at the least from the surrounding tissues, and a ligature then passed round the thigh, excluding the vessels and nerve; a loose ligature, ready to be tightened at will, is thrown round the vein. A thread is next fixed to the extremity of each digit of the same limb, to facilitate the examination of the circulation in the interdigital spaces, without modifying it by pricking the tissues. The frog being pinned down to a flat piece of cork, and the web laid under the object-glass of the microscope, the ligature embracing the bone and muscles of the thigh is forcibly tightened. The experimenter is then certain that the circulation in the lower part of the limb is carried on by the dissected vessels alone. The circulation in the arteries, veins, and capillaries, goes forward in the same manner as before the performance of the operation described; jerking movements sometimes take place. The globules move more rapidly in the arteries than in the veins, and in the capillaries their velocity is less than in the other two orders of vessels; in some, however, it is greater than in others, for reasons to which we need not at present direct our attention. The observer now watches with especial care an artery and vein of the interdigital space submitted to investigation; he then interrupts the course of the blood in the femoral vein: the moment he does so the progression of the globules in the vessels of the digital interspace under examination becomes jerking, and this jerking mode of progression lasts a few seconds only, being followed by an oscillatory movement. The span of these oscillations at first equals the length of five globules, and soon decreases to that of two; the rhythm is identically the same in the artery and capillaries of the interdigital space, and they continue, to the number of forty-six in a minute, so long as the compression of the vein is kept up. While the femoral vein still undergoes compression, the experiment is varied by interrupting the course of the blood in the artery also; the oscillatory motion ceases at the same instant. The globules become quiescent in the artery, the capillaries, and vein of the extremity. If the femoral artery be then freed from constriction, oscillations of equal length in the three orders of vessels recommence. These experiments concluded, the heart of the animal is laid bare, and the number of the contractions of the ventricles counted; these are found to be one hundred and eighty-six in four minutes, or forty-six in a minute.

"*Remarks.*—The oscillations of the globules are produced, on the one hand, by the heart, which impels the blood into the arterial system, into the capillaries and the veins; on the other, by the retraction of the arteries and veins that follows their dilatation by the wave of blood driven forwards by the left ventricle."

One of the great objections urged against the opinion of those who deny the spontaneous movement of the globules is this: if you separate a portion of a living animal from the rest of its frame, as, for example, the web of a frog, you may see that the blood continues to move in the vessels, and escape by their open orifices.

Here, then, it is urged, there is a force in action on the globules independent of that of the heart. I cannot admit the correctness of this assertion; the movements that are observed in the globules arise simply from the flowing of the liquid, which suffers a less degree of pressure where the vessels are gaping than elsewhere. It is exactly the same thing as if I distended an elastic tube with an injection: when both its extremities are closed the liquid is quiescent, but the force communicated to the latter by the syringe still exists, though dormant, and becomes apparent the moment the cause producing the temporary equilibrium ceases to act. Turn the two cocks, and the liquid will escape by both points of issue: the same phenomenon occurs in the capillary vessels; their walls retreat in virtue of their elasticity. In the first case the hydrodynamic power is a syringe; in the second a muscular pump. Why, then, call by a different name the effects, identical in nature, which take place in both? Can what is physical in essence become vital just as you will it? I am well aware that of the two agents, which have each of them dilated a tube, one is organized, the other inorganic, but the dilatation which results from their action is an essentially mechanical phenomenon. For these reasons the experiment adduced by our adversaries in support of the opinion they espouse really militates against its correctness. We will pursue this subject when we next meet.

I told you, Gentlemen, that physiologists had, in general, come to no important or satisfactory results respecting the coagulation of the blood; and you will grant readily that it could not be otherwise, when you reflect on the unimportant character of the question about which they have busied themselves. Thus, for example, they have laboured to ascertain whether disengagement of caloric accompanies coagulation. If we call to mind the physical laws that preside over the changes of form assumed by matter, it seems as though we have in them an *à priori* solution of the problem. We know that when any body passes from the liquid to the solid form its molecules approximate, its volume decreases caloric and electricity are disengaged, and elevation of temperature consequently ensues; but this application of physical laws to the blood has not been accredited by experience, and is, therefore, inadmissible. The way to avail ourselves most usefully of applications of the kind to physiology is not to abuse them; in other words, to acknowledge the justness of those only which are confirmed by direct experiment.

Another question that has been much agitated is, whether coagulation can take place at a temperature below zero. I submitted some blood to a temperature of —14°, but found that before congelation could take place an exceedingly firm and consistent coagulum had formed. I sought by another experiment, to establish the influence of high temperature; the result was, that blood exposed to a heat of from 50° to 55° Réaumur, coagulated exactly as in the ordinary atmospheric medium. From the experiments I have myself made it consequently follows that neither *cold*, *heat*, *rest*, nor *motion*, prevent the occurrence of coagulation; however, we are acquainted with one condition in which it will not take place, whilst the blood is kept in motion; this is when we receive arterial blood directly from an artery into the body of a syringe which has been heated to 30° Réaumur; so long as the liquid in the inorganic instrument receives the impulsion communicated by the heart it does not form into a mass. You recollect how I availed myself of this circumstance in practising the transfusion of the blood of one animal into the vessels of another: it is not necessary that the quantity of fluid be small; we may collect nearly half a litre in a syringe, and the portion of it farthest from the left ventricle will not coagulate sooner than that which is nearest. But be all this as it may, you observe, Gentlemen, that these questions only possess such a share of interest as we may please to accord them; all the importance of the subject merges in the question of the coagulation, properly so called, of the fibrin. You are aware that I have been enabled to neutralize this property by the agency of various substances; and you also know that when the blood is deprived of its coagulable element it can no longer set into a mass; witness the blood in this vessel, which has already lain in it two days, and which you perceive, on my shaking the vessel in the least, is still perfectly fluid. But these are not the only agencies that modify coagulation; there are some substances which produce the same effect by inoculation, such as the virus of rabies, the poison of the viper, and many others, which it is unnecessary to enumerate, especially as I trust, at a later stage of the course, to be enabled to devote a few lectures to these high questions of pathology.

The fact that gives its chief importance to the study of coagulation is that, as has been shown to demonstration, the loss of this property by the blood, no matter under what circumstances it occurs, is followed by death. You remember that I refuted, by anticipation, an objection which might have been made to this statement. The question it regards is, whether the action of a substance on the blood is the same when injected into the vascular system, and when simply placed in contact with that fluid in a vase; in other words, whether vitality does or does not prevent chemical reaction. The investigation I have made leaves no doubt on the subject: *it is undeniable that the substances which you see liquefy the blood in glass vessels, act in the same manner on that fluid in the living tubes of our organs.* Preoccupied as men have been by the opposite opinion, they have taken no notice of the truths I have brought to light; nay, more, you shall learn, by a single fact I am about to relate, how deeply erroneous this prevalent opinion is, and what serious mischief must have been caused by ignorance of the history of the blood.

A celebrated surgeon, M. Dieffenbach, of Berlin, undertook to reinstate, in the favour of medical men, the operation of transfusion of blood from one individual to another, recommending it more especially in cases of obstinate bleeding from wounds, and in uterine hemorrhage. Now, as he had remarked that the coagulation of the liquid interfered with the success of the operation, he modified the mode of performing it, by extracting the fibrin of the blood he was about to inject, for the purpose of preventing the formation of clots, and the obstruction of the capillary vessels. Experience (that of M. Bischoff, of Heidelburg, among others) has since shown what melancholy results must be the necessary consequence of such a method of proceeding; I need scarcely warn you against ever having recourse to it. You have witnessed the results of the injection of defibrinised blood into animals in good health; what terrible effects may we not justly expect it to produce in individuals whose organism is already, at the time of operation, suffering under some morbid influence? However, transfusion, properly performed, is by no means to be disdained as a therapeutic measure; I have no doubt, indeed, that under certain circumstances, it may be employed with advantage, especially if the blood be procured by adapting the point of a syringe to an artery, and allowing the instrument to fill by means of the unassisted pressure of the liquid. The contractions of the heart cause the ascent of the piston, and the syringe soon fills with perfectly fluid blood, which may be injected without apprehension of mischief. Since I have employed this plan you have never known a fatal issue in cases where I have performed the operation of transfusion.

It has just this moment struck me, that we might modify the process of transfusion with advantage in this way: I will take a defibrinised animal, and at the same moment as an assistant tranfuses the blood of a healthy animal into its veins, open an artery, so that in proportion as the normal blood flows in the defibrinised liquid will flow out, and mingle as little as possible with the other. If the experiment prove successful, the plan will henceforth serve for replacing blood of deleterious qualities by such as is fit to carry on the functions that constitute life. This species of transfusion would not be more difficult or dangerous in execution in the case of the human subject, than that which we have so frequently practised on animals. There are arteries in the human body easily laid bare, and from their juxta-position to osseous surfaces, as the temporal, for instance, effectually compressible, if we do not wish to have recourse to ligature.

In support of what I had just stated respecting the effects of defibrination, you may examine the body of this animal, from which I removed successive portions of blood, reinjecting them into the veins, after having first deprived them of their fibrin. Symptoms of great gravity came on almost immediately, and the animal soon perished. Its blood had become so utterly unfit for circulation in the capillaries of its organs that it was extravasated into the various tissues, but especially into the parenchyma of the lung, to which it has given the appearance of a huge clot. With facts like these staring them in the face can men persist in contesting the permeability of our membranes, and the real existence of certain properties in the blood, whereby alone it is enabled to traverse the innumerable vascular canals of our tissues without undergoing exhibition? Again, if instead of at once removing all the fibrin from the blood of an animal we remove it by small portions, we find that these repeated subtractions induce local lesions, of which there is no mistaking the origin. While this is going on a very curious phenomenon takes place; the fibrin of the blood instead of diminishing, actually augments in volume. I offered you a conjectural explanation of this strange fact last year, which at least accounted perfectly well for the sudden emaciation of animals whose blood is defibrinised; I have learned nothing new on the subject since then, so that my original hypothesis is neither overturned nor confirmed.

In order to allow you to judge of the fact for yourselves, I have caused three bleedings to be practised, at equal intervals, in a large dog; one of twelve ounces, the second of ten, the third of eight ounces. On each occasion the blood was beaten up, filtered through a linen cloth, and then immediately reinjected into the veins of the animal: here, in these three vessels, is the fibrin furnished by the blood of the three bleedings. The first is whitish, supple, and elastic, in fact normal in its characters; the second is softer, more spongy, and more voluminous, though the quantity furnishing it was still less; these characters are still more distinctly marked in the fibrin of the third bleeding; it is much more voluminous than in the previous instance; its fracture, instead of being clean, shows irregular filaments, proving that the force of cohesion, by which it resisted traction, is unequally shared by its fibres. The normal fibrin has, therefore, undergone, by venesection and the reintroduction of defibrinised blood into the circulation, very remarkable modifications in its physical properties, in its volume, elasticity, and weight. When the two are weighed comparatively, it is found that the healthy fibrin, though inferior in point of mass, weighs considerably more than the other. Among the chemical characters of the two substances there is one which renders it impossible to confound this fibrin of secondary formation with the normal species; it is that if the former be submitted to a temperature of 60° Centigr. in a sand-bath, it liquefies just like so much albumen. This is the substance, Gentlemen, to which I have given the name of *pseudo-fibrin*. It is important to recollect that the animals on which these experiments were made, were invariably fed with the feculæ which contain no fibrin. The fibrin must, therefore, I still think, as I did last year, be got from the organs themselves, and thence reconveyed into the circulation; the muscles become atrophous, and wherever fibrin existed a notable diminution in the volume and aspect of the parts ensues.

It follows from the facts I have detailed that it is possible, by gradually defibrinising the blood, to alter its normal properties without, of necessity, depriving it of the power of coagulating. Henceforth, therefore, it will not be enough to examine if such and such blood contain fibrin, and in what quantity it contains it; we must also satisfy ourselves of the nature of that fibrin. This easy analysis of the blood will, I trust, be habitually had recourse to; by it we are enabled to obtain, without difficulty, some certain knowledge of the constitution of individuals; whereas the superficial examination now resorted to by medical men is calculated to lead them into error.

We have already remarked that normal blood, placed in a glass vessel with certain substances, coagulates, but that when mixed with others no solidification takes place. To pursue this view of the subject of coagulation, let me show you some new experiments I have been making. Here is some blood which was, immediately on its removal from the artery, mixed with putrified water; there is not the least trace of coagulation in it. In this other vessel ordinary water and blood have been mixed together; the whole has set into a mass; it is evident that in the first instance it is not the water that prevented the blood from clotting, inasmuch as water unmixed with putrid matters, has no effect on the coagulation of the same blood; the liquefaction must, therefore, be ascribed to the putrid particles held in suspension in the water, and, without doubt, to the hydrosulphate of ammonia formed by the process of putrefaction. This is really an important fact. In investigating it further I was first desirous of ascertaining whether a considerable quantity of the putrid liquid was necessary for the production of serious disorder, and I learned that a few drops only injected into the veins of an animal killed it almost instantaneously. The importance of this subject cannot be questioned, for the human being is constantly exposed, through respiration and otherwise, to the chance of the introduction of such deleterious molecules into his blood; once received into the circulation they soon manifest their presence by disorganizing the liquid with which they are mingled. If you examine the blood of an individual who has succumbed under the terrible symptoms developed by the absorption of miasmata, you will find that in point of colour, fœtidness, and liquidity, it actually resembles the blood in this vessel; here, then, again, the action on the liquid that has ceased to circulate is the same as that on the blood still moving through the vessels. I feel persuaded, Gentlemen, that we are in the right way to the discovery of the mechanism of those scourges that desolate certain countries,—of those terrible epidemics which, though known by different names, originate in the same cause. Is any great advance, I would ask, made towards explaining the black vomiting of yellow fever by ascribing it to a *specific gastritis?* If the partizans of inflammation fancy so, what will they say when we demonstrate to them that, by introducing a few drops of putrid water into the circulation, we shall produce an accurate imitation of the symptoms of their pretended specific gastritis; if we show them that decomposed blood, effused through the gastro-intestinal mucous membrane, constitutes the matters vomited? But will they, after this, be convinced that neither irritation nor inflammation have been in action? Perhaps they may, but that they should acknowledge their new-born conviction is too much to expect, I now proceed, in illustration of what I have been saying, to the autopsy of an animal who died in two hours from the injection of a little putrid water into the veins. As life lasted so short a time after the operation we shall probably not find any very extensive lesions. The first incision through the integuments allows us to feel pretty sure as to the fact of the blood being liquefied; observe how it flows from the wound; the muscles present a remarkable punctuated red colouration, such as is often met with in the brain, and is caused by a vast number of small petechiæ formed of extravasated blood; the lung has only retracted very slightly on opening the chest, nevertheless it is scarcely at all diseased; the chief disorders will, no doubt, be found in the abdomen, which I proceed to open. Here I may remind you of a fact to which we have so far met with no exception, namely, that when we inject subcarbonate of soda in solution the thoracic organs are those presenting the disorders productive of the fatal result. You see, in this case, that the mucous coat of the intestines is raised by a deposition of blood in the subjacent cellular tissue; on the surface, too, are large patches of albumen and mucous, the whole forming a very excellent case of gastro-enteritis for the disciples of a certain school; for my part I see nothing in it more than distention of the capillary vessels with blackish fluid blood, the elements of which, having become dissociated, partially transuded through the intestinal tunics. These pathological phenomena are, as we have frequently ascertained, more and more marked the longer the disease has lasted before causing death.

Here is the body of an animal into whose veins twenty-five grammes* of subcarbonate of soda were injected. Death was instantaneous, and must have been caused by profound disorganization of the lung; and, accordingly, we find that organ distended with liquid blood, which gushes out when an incision is made into its substance; there is, besides, bloody effusion into the pleura. These mechanical disorders constitute what pathologists would call pleura-pneumonia; in such a case as this they would forthwith set about counting the number of cells unaffected by the inflammation, describing minutely the different shades of colour presented by the lung, and ascertaining the quantity and weight of the pleuritic effusion, &c. But ask them the cause of these disorders, and all you will get from them is the words inflammation and irritation; and after this they pretend to be pathological anatomists! In order to leave no doubt in your minds as to the special influence exercised by the blood when liquefied by the subcarbonate of soda, on the lung, I opened the abdomen; you observe that the intestinal convolutions, and the various organs of that cavity, are all of healthy appearance.

* The gramme is equivalent to 0.0648 gr.

LECTURE XIV.

Phenomena of the capillary circulation.—Experiments on frogs.—Coagulation of the blood.—Effects of defibrination.—Delusive theories respecting inflammation.—Action of subcarbonate of soda.—Non-contagious nature of the plague.—Absurdities of quarantine.—Action of acids and alkalies on the blood investigated.

GENTLEMEN :—I promised in my last lecture to give some further account of M. Poiseuille's researches, and proceed to fulfil my engagement by quoting that gentleman's description of another of his experiments. It confirms the statements I have made respecting the exclusively passive participation of the capillary vessels themselves in the circulation through them. You will, from it, obtain new proof that no movement takes place in the capillaries without a corresponding one occurring in the arteries; and that every movement in the latter vessels, directed from the trunks to the minor branches, is transmitted through the capillaries without undergoing the smallest acceleration by any action of those infinitely minute tubes.

"A white mouse, about a month old, is pinned down to a thin plate of cork; the abdomen is then freely opened by an incision on the median line, and the small intestine and mesentery laid out for examination on a plate of glass. The velocity of the globules is seen to be greater in the arteries than in the veins; in the capillaries, where the form of those bodies is perfectly distinguishable, it is generally less than in the two other kinds of vessels: the movement is, at first, continuous, without either jerk or intermittence.—Forty minutes have now elapsed, the greater part of the intestine lies outside the abdomen; the animal has evinced much less irritability for the last quarter of an hour. The blood now moves with diminished rapidity in the arteries, capillaries, and veins, in the intervals between the contractions of the heart. The movement of the fluid has now changed from *continuous* to *jerking-continuous.*—Fifteen minutes later: the jerking motion is now much more marked; and the progression of the fluid is extremely slow during the intervals between the contractions of the heart.—Ten minutes later: the globules now stand still after each systole of the heart, and this intermittent movement exists in the arteries, capillaries, and veins. This repose is followed by a retrograde motion of the globules after each contraction of the heart; an oscillatory movement then takes place in all the arteries; no further movement is distinguishable in a great number of capillary vessels.—The animal has now been under experiment for an hour and twenty minutes, and has ceased to give any signs of life: the span of the oscillations increases considerably, and the globules retreat quite as much as they advance during these oscillations; there is a sort of struggle going forward between the weak contractions of the heart and the resistance made by the arteries to dilatation. At length the movement of the globules in the arteries is scarcely discernible; shortly after this the only movement in those vessels is of a retrograde kind; all is at rest in the capillaries, and a slow though natural movement persists in the veins. The passage of the blood from the branches of the arteries and veins towards the trunk becomes slower and slower, and a state of complete rest is established at the end of twenty minutes. It is plain that the arteries and veins contain a much less quantity of blood than at the outset of the experiment; as for the capillaries, their state in that respect seems to have undergone no alteration.

"*Remarks.*—The systolic movements of the ventricles continue after death in batrachians; this persistance of the contractions of the heart after death is also observable in certain mammalia, such as the dog, the rat, the mouse, &c. These movements, which are tolerably powerful immediately after the removal of the organ from the body of the living animal, grow gradually less and less energetic. The animal, on which I have made the above experiment, was becoming weaker and weaker every moment during its continuance, from the effects of the operation it had undergone, and at the same time the heart's contractions gradually lost their power; hence the *jerking-continuous*, *intermittent*, and *oscillatory* movements that successively replaced the *continuous* motion. After the death of the animal the heart continued to beat; while it did so, the oscillatory motion persisted also, but in proportion as the contractions of the heart lost their force still more completely, the retrograde movement gained the ascendant in the arteries, in consequence of the retraction of the vessels which ensues when they cease to be dilated by blood arriving from the heart; and through this retraction of the walls of the vessels towards their axis, the retrograde motion is kept up in the globules for twenty minutes after the animal had expired."

Globules in a state of quiescence in a limb separated from the rest of the body of an animal, have been sometimes seen to become suddenly agitated by movements in various directions, and this when no suspicion could arise of the object-glass having been altered from its horizontal position, or of its having been shaken in the least. What is the source of these misplacements—for you well know that I do not share the belief of those who look on them as spontaneous or voluntary? Nothing more or less than the manner in which the light falls on the microscope. In instances where such an appearance has been observed, there can be no doubt that the sun was at one moment overcast with clouds, which dispersed at the next; by the change in the state of the heavens the luminous calorific rays were enabled to reach the mirror, and, being reflected from its surface, to play on the web submitted to experiment. As liquids dilate more than solids, the blood finding itself, as it were, pent up in its vessels, in consequence of the change of temperature, rushes to whatever points offer the least resistance. To this want of adaptation of the dilatation of the contained blood to that of the containing walls, are to be ascribed the movements witnessed. The same phenomenon occurs when an artificial light, such as that of a wax taper, is employed; when the lighted wick is brought near the limb the globular movements recommence; they cease when it is removed. Nay, more, an effect of this kind is often produced by simply conveying the part under examination from a cool to a warm room. If the globules were motionless in the first, they would, in the other room, become the seat of a trifling motion, because the temperature or the medium in which they are now placed is higher than that of the room in which they were before examined.

But there is another prevalent notion respecting the movements of the globules, which, though extremely absurd, is still worth refuting. Suppose you have placed a drop of blood, previously dissolved in alkaline or sugared water, on the object-glass; you examine it, and observe some of the globules rising, others descending, and a third set oscillating hither and thither. But are these voluntary movements? No; they are a simple result of the action of the laws of equilibrium: the heavier go to the bottom; the lighter rise to the surface. The same phenomenon, precisely, occurs with particles of colouring matter suspended in distilled water; before the liquid becomes perfectly quiescent irregular movements take place, dependent, as in the case of the blood, on the specific gravity of the corpuscula suspended in it. Besides, the light often falls unequally on the different parts of the drop submitted to inspection; the most strongly heated points dilate the most, and hence oscillations follow. But again,—it is the usual habit, when we wish to separate the globules perfectly from the fluid in which they float, to spread out the minute stratum under examination on one plate of glass, and to cover it with another. In this case, too, movements are observed in the fluid, but these also depend on the establishment of the equilibrium. The globules rush towards the points where there is least pressure; the moment the pressure is equalised over the entire surface all motion of the globules ceases.

Hence, Gentlemen, wherever and however we study the progression of the globules, whether it be in thin tubes and in active motion in them, or having artificial obstacles to contend with; whether it be when they are removed from their vessels and spread out on a transparent surface, still the result is the same;—never, in any case, do we meet with anything indicative of their possessing a power of vital nature independent of physical laws. I am well aware that the contrary opinion has been, and still is, professed by many conscientious men: they have made experiments,—these I admit; they have drawn inferences from them,—these I refuse to accept. And why?—Because such deductions as theirs do not legitimately flow from the phenomena observed. It is a matter of perfect indifference to me whether the globules do or do not enjoy a special vitality; indeed, I should, of the two, rather wish, were it only for the prettiness of description, that all we are told respecting them were correct. The mind feels a kind of satisfaction in fantastic imaginings of the sort, and man loves to dwell on everything which can dignify him in his own eyes; proud of his intellect, he would delight to detect in each particle of his body an intelligent existence. But we must accustom ourselves to the study, not of what we should wish Nature to be, but of what she is. We shall be the gainers by doing so, for the discovery of the true is always ennobling: we shall be the gainers thereby, for her works are infinitely more perfect than aught human imagination is capable of creating.

You recollect, Gentlemen, I presume the result of my experiments respecting the influence of low and high temperatures on coagulation, which I made known to you in my last lecture. Struck, as I was, with the bold manner in which observers asserted that when blood is submitted to the action of an extremely low temperature it freezes without coagulating; and, further, that if, while in this condition, its temperature be raised, it reassumes the liquid form, and becomes organized into a normal clot, I felt anxious to repeat their experiments. With this view, after having plunged the glass tube I hold in my hand into a frigorific mixture marking —14° by Réaumur's thermometer, I let some blood fall into the vessel containing it; the blood presently became solid. After a certain lapse of time I withdrew the tube, and gradually raised the temperature of the mixture; but, although I watched the progress of the experiment with the most scrupulous attention, though I never once turned my eyes from the vessel, I did not see the blood reacquire its fluidity. Even at this moment it is, with the exception of a trifling quantity of serum which has escaped from it, in the same state as when I removed it from the frigorific mixture: I cannot even detect any appearance of increase or decrease in its volume. The discrepancy between these results and those announced by authors, affords me no little surprise; but, as such difference does exist, I cannot make up my mind fully on the subject for the present, and reserve my decision until we shall have the evidence of further experiment.

I am in the habit of causing the blood drawn from my hospital patients to be received into two vessels, of exactly the same form and size,—one empty, the other containing an aqueous solution of sugar, which possesses, as I have already made known to you, the property of precipitating the globules without dissolving them, and so allows us to follow accurately the different processes in the organization of the fibrin. These two glass vessels contain some blood taken from a healthy woman in the eighth month of pregnancy; in the one you observe a red and consistent clot, and, to all appearance, a normal share of serum; in the other, in which I had put sugar and water, we have a distinct fibrinous frame-work, formed of long delicate filaments suspended in the liquid, and adhering to the parietes of the vessel. In this case there is no indication of an abnormal state of the blood. Here are two other vessels containing some blood taken from a female who has miscarried in the sixth month of utero-gestation. The patient is so extremely weak and anemic that her ultimate recovery is a matter of doubt; accordingly, you will remark a material difference in the contents of the four vessels: the two last present a soft, flabby coagulum, floating in an excess of serosity, while its fibrin is inelastic and fragile. However, Gentlemen, this is a mere comparison, which may or may not be well founded; on the one hand, we have a healthy woman, with seemingly normal blood: on the other, an ailing person whose blood is evidently altered in its properties. This is a new point of view in which the study of the blood is likely to prove fruitful in pathological inferences, and we will avail ourselves to the full of it. I may, even now, point out to you the resemblance existing between the abnormal blood which I have just shown you, and that of animals partially defibrinised.

This brings me directly to the subject with which I have of late occupied your attention, namely, the effects of defibrination of the blood. You remember that the coagulable element of that fluid is, when gradually removed, reproduced with marvellous rapidity; the nature of this phenomenon has hitherto escaped our diligent researches, but we have at least ascertained that the fibrin found in animals from whom part of that contained in the blood had already been removed, is singularly modified; it is puffy, spongy in appearance, lighter, and yet more voluminous than normal fibrin; like albumen, too, it liquefies at a temperature of 60° Centigr. Feeling most curious on this subject, and therefore desirous to push inquiry relative to it as far as possible, I bled the animal, of whom I spoke to you in my last lecture, a fourth time; the blood, received into this vessel, has not coagulated; but what is much more extraordinary is, that thirty centilitres of it have supplied the large mass of fibrin you see on this plate. I confess all this surprises me beyond measure; the more we bleed the animal the greater the volume of the fibrin becomes. The hypothesis I ventured on, as explicative of this most strange phenomenon, is far from having the solidity I could wish; indeed, the possibility of the fibrin of the blood being replaced by that of the muscles or organs generally becomes extremely questionable, when we reflect on the various points of distinction which I have, by experiment, shown to exist between that principle as contained in the blood and in muscular tissue. With regard to the animal on whom this gradual defibrination has been practised, his state deserves consideration. He is so extremely weak that he lies constantly on one side; the cornea is dull; the contractions of the ventricle have lost their former energy, and there is marked anorexia. When the last venesection was practised he fainted, and the state of syncope lasted so long that it was supposed he was dead; however, a few moments after, respiration was re-established, and, some way or other, he continues to live on. I shall take care to acquaint you with the further results of the experiment.

In my last lecture I once again gave you an illustration of a fact simple in itself and easily cognisable, while it is also, through its consequences, of primary interest, and connected with every branch of pathology. I said, and proved what I said, that determinate causes deprived the blood of the faculty of coagulating; I showed that that liquid, when thus modified, transuded through the walls of the vessels, became effused into the parenchyma of the organs, and developed in them certain local affections, the production of which was, till of late, exclusively attributed to the solids. Was not this a most serious error,—this unvarying attempt to discover in the vitality of our tissues the cause of the majority of the disorders to which they are liable? I know you think so, for you have distinctly seen that in cases where the famous "*stimulus*," according to the prevalent doctrines, determines an afflux of blood to a part, there is in reality nothing more than a modification of the properties of the liquid, and an exemplification of the mode of action of one of the grand laws of physics, that of imbibition and exbibition, in organised membranes. But this lamentable ignorance of, or perhaps, more correctly speaking, contempt for, the principles of physical science has been prolific of baneful consequences.

Look to the condition of physiology from its origin; sunk into a kind of forced subserviency to an exclusive system of vitalism, we find it encumbered with absurd reveries and startling conjectures, and lending an unsound support to ridiculous pathological theories; even at that early period appeared the famous dogma on which the homœopathists have since laid their hands and tortured to their purposes—*similia similibus curantur*. An organ is *inflamed*, an invisible cause (the *stimulus*) is said, by some mysterious process, to have attracted the blood to it; to have enabled that fluid to surmount the obstacles to its passage which it encounters in the *contractions* to the capillary vessels, and to distend those vessels to such an extent that they give way, while it forces itself through the rent. What, then, becomes of the *stimulus?* No one can tell us; none know how that subtle agent managed to slip into one of the capillary vessels; none saw it enter; none have witnessed its departure; and far be from me the silly pretension to tell you more about it than the bright geniuses who invented it. But this is not all; let us turn to the plan employed to prevent the imminent conflagration of the animal machine consequent on the intrusion of this terrible *stimulus;* it is based on the excellent axiom I have just quoted. As the *stimulus* has drawn the blood to a particular spot, and therein acted as an immaterial invisible sucker, why we will simply (say the irritationists) set in action against it a living material one, nay, fifty, a hundred, two hundred, or more of them; and the arch enemy will soon be forced, by this imposing apparatus of *absorbing mouths*, to quit the scene of his exploits! I really feel a sort of shame, Gentlemen, in alluding to these extravagant absurdities, but their influence on our science has been so powerful, that no opportunity should be lost of heaping on them the ridicule they deserve.

But to turn from these disagreeable passages in the history of physiology: I have already frequently spoken to you of the subcarbonate of soda, a salt which exists, in the normal state, in the blood, mingled with the other elements of that liquid. This fact would authorize us in presuming, nay, in affirming, that that salt is an innocent substance; and so it is under the circumstances stated, but under others its character is very different. Thus, if injected into the veins in large doses, it immediately causes death, and in this manner consequently possesses the properties of an energetic poison. Experimental inquiry has removed all doubt respecting its mode of action; it has shown that the aqueous solution of the salt will, if placed in contact with blood, prevent the coagulation of that fluid, and further demonstrated, by injections performed on living animals, that its effects are the same on blood when within, or removed from, the circulating system. This is a fact of the highest interest; for the subcarbonate of soda enters liberally into the composition of many medicaments, is in consequence frequently introduced into the economy, and may, when there, induce most serious disorders. For my own part, I will always watch over its employment with much solicitude, and henceforth be more cautious about prescribing it even in moderate doses. For this practical, and, I dare to say, valuable result, we are indebted to experimental research alone.

You are also acquainted with the action of putrid water; you have learned that a small quantity of it injected into the veins liquefies the blood; in other words, that it kills; however, an important distinction is to be established between the two substances as regards their action; while the subcarbonate of soda acts specially on the lung, that organ is scarcely affected by the injection of putrid water into the veins, which, instead, exercises all its deleterious influence on the intestinal canal. Now, water is not the sole vehicle of miasmatic particles, the fluid we breathe frequently holds such atoms in suspension; it is putrid exhalations that give rise to the intermittent fevers of marshy localities, from which the inhabitants sometimes succeed in escaping, by covering the face with a veil during the hours of sleep. The air, while traversing its tissue, is, as it were, sifted, and so reaches the organs of respiration purified of the vegetable or animal molecules with which it is loaded. None will be found at the present day to question the fact that the yellow fever is produced by the absorption of miasmata, arising from the putrefactive fermentation of marine animals and plants of all sorts left on the shore by the ebbing tide. Hence that affection is to be looked on as resulting from infection; and if it were desirable to have other facts referrible to this question, they might be easily had. Does not every pathologist, without exception, now recognize the fact, that the "typhus," which is known to attack armies, the crews of ships, and, in a word, large assemblages of men, under whatever circumstances they may be congregated, is communicated by infection? I was a close observer of that terrible scourge of our population in 1814; and even at that period I remarked a particular liquidity in the blood of the individuals attacked with it. This fact made so strong an impression on me, indeed, that I was in the habit of prognosticating a fatal termination, or the contrary, according to the degree of its fluidity. Many authors have noted the occurrence of this phenomenon in such affections.

As I have touched on the subject of disorders which it is the fashion to call *contagious*, I may be permitted to say a word or two on the most frightful of them all—the plague. In consequence of the progress of physics and chemistry certain medical questions have become much less obscure than they formerly were; we are now enabled to attack them on philosophical principles, and to investigate them calmly and deliberately; and, as respects the subject of the plague, the result of this improvement is, that convincing arguments pour in from all quarters, calculated to prove that that disease originates, and is propagated, not by contagion from man to man, not by contact with merchandize or contaminated substances, but by infection. The atmospheric and other conditions of Egypt, its ordinary cradle, are eminently favourable to its development. Torrents of rain deluge the land in an instant after long-continued drought; an intolerable degree of heat is as suddenly exchanged for an icy coldness; add to these rapid transitions the annual inundations of the Nile, which, as it retires from the ground it temporarily covered, leaves behind it multitudes of carcases of animals exposed to the incessant action of the scorching sun of Africa; consider, besides all this, the insalubrity of the habitations and the want of cleanliness of the Arabs, and you will not question the justness of my statement. Nor must the terrible hurricanes, which have buried ancient Egypt under mountains of sand, be forgotten; like messengers of death, they bear with them, and spread far and near, the putrid emanations gathered in their passage from place to place, as though it were their office, after having shaken the soil to its foundation, to exterminate the beings dwelling on it.

Gentlemen, the question of the mode of propagation of the plague is far from being an idle or unimportant one. Admit for a moment, and for argument's sake, that it is an infectious disorder,—set aside the bugbear of contagion; what good end would those puerile precautions, misnomered sanitary, then effect? Where would then be the utility of your quarantines, which shackle the freedom of commerce; of your lazarettos, where you disinfect what never was infected,—where you neutralize a virus, the existence of which is to this hour a problem? Who can affirm, on conviction, that the plague is transmissible in bales of goods? Where are the experimental proofs of the position? Where even a few well-established facts, calculated to give it plausibility? None are to be found; but instead we have, as usual, a profusion of assertions; rumours originating in the terrors of panic-struck populations: facts cited with emphasis by the vulgar, but contested by men of science. People who look only to the surface of things, cry aloud, when they hear so much as a doubt let fall concerning the contagion of the plague; but all the clamours in the world are mute before the sage. The captivity of Galileo has not prevented our planet from operating its revolution round the sun, nor the latter body from remaining motionless in the centre of its system. Several medical men, M. Chervin among others, have demanded an authorization to devote themselves to the solution of the all-important question under consideration; to perform a series of experiments on contaminated goods, in an uninhabited island, far from the habitual foci of the plague; but government has refused its permission, so deeply does it dread living umbrage to absurd or interested prejudices.

The opinion I espouse is that held by persons whose position gives them the best opportunity for examining the question. Clot Bey, among the rest, is of opinion that the plague is not a whit more contagious than "typhus;" so that this question, too, is referrible to that of substances capable of liquefying the blood. It so happened that, while engaged in one of my recent courses of lectures in explaining my doubts as to the contagious nature of the disease, I had among my auditors M. Lacheze, a medical man who had just returned from Egypt, where he had had extensive opportunities of observing it. After I had concluded my lecture he came up to me and informed me that, in truth, the blood of plague patients never coagulated, and was always clammy and blackish. "You have," he added, "hit upon the true view of the question; all those who have observed the plague closely, and uninfluenced either by apprehension or prejudice, will share your opinion respecting it, if they have not already done so." By-and-by, when we come to study the action of gases on the blood, I will not neglect to examine thoroughly the effects produced on that fluid by the carbonate of ammonia,—a salt, you are aware, generated by the putrefaction of animal matters. The importance of ascertaining what substances destroy the coagulability of the blood flows in the most direct manner from what I have been saying. So far I am only enabled to enumerate a small number; the list will, however, soon be enlarged, for I am about now to commence a series of experiments, wherein I propose to bring the different soluble salts, &c., into contact with the blood; at the same time we must endeavour to find out those capable of promoting coagulation, and so of serving, if I may be allowed the expression, as antagonists to the others. It is possible that our investigations may not lead to the expected results, but the trial is well worth making.

As the carbonate of soda and putrid water unquestionably act on the blood in glass vessels in the same manner as on that circulating in the living tubes, I shall simply, in order to avoid a useless sacrifice of animals, place blood in contact in glass vessels with the following substances:—

Sulphuric Hydrochloric Acetic Oxalic Tartaric Lactic	Acids.

1*st Experiment.*—Sulphuric Acid.

Judging from medical practice and the notions generally admitted, we should fancy that this acid would augment the coagulability of the blood; in fact, it is employed internally to stop hemorrhage, and to cauterise surfaces from which blood flows in undue quantity. Besides this, "sulphuric lemonade" is one of the most common drinks given in our hospitals to arrest internal hemorrhage from the uterus, lungs, &c.; the acid is, consequently, looked on as promoting coagulation; but the truth is otherwise; here is a mixture of five centilitres of water, four drops of concentrated sulphuric acid, and one centilitre of blood. These substances have been in contact since yesterday, but the fibrin presents no appearance of coagulation; nay, more, the colouring matter is altered in its properties, as you see by its deep black colour. This action of the acid on the globules, I mean the removal of their normal colour, and that of their characteristic property of reddening when in contact with atmospheric air, is a further mischief in addition to the deleterious influence of this acid on coagulation; it is possible, therefore, that instead of putting a stop to hemorrhage, liquids containing sulphuric acid promote the escape of the blood from its vessels.

2*d Experiment.*—Hydrochloric acid.

Hydrochloric acid, in the same proportion, has also prevented coagulation.

3*d Experiment.*—Acetic acid.

This acid is used with our food, and certain young ladies consume it to a large amount; in order to remove a premature fulness of person they are in the daily habit of drinking a great quantity of it diluted with water, without reflecting on the future sufferings of which they thereby lay the foundation. Be this as it will, the blood before us has not coagulated; this property of acetic acid is well known to our pork-butchers, who are in the habit of using vinegar for preventing blood from clotting.

4*th Experiment.*—Oxalic acid.

Three grains of oxalic acid, dissolved in a little water, have completely liquefied the mixture; there is no trace to be seen either of globules or fibrin. This subtance has, as you well know, long been classed among energetic poisons, but its most extraordinary character is that sometimes it acts as a corrosive on the coats of the stomach; sometimes as an irritant on the entire intestinal canal; sometimes, it is said, on the nervous system. These differences may possibly be dependent on its action on the blood.

5th Experiment.—Tartaric acid.

Tartaric acid is an essential constituent of almost all our wines; it also enters into the composition of tartar emetic. No coagulum has formed in the vessel in which the mixture has been made; but you see that there is a slight deposit at the bottom, which will require examination under the microscope.

6th Experiment.—Lactic acid.

This is not a powerful acid, nevertheless it has proved energetic enough to prevent coagulation.

We next come to a series of substances more or less *alkaline;* it will be interesting to establish their action on the blood.

1st Experiment.—Pure potassa.

Eight drops of pure potassa, in solution, have been added to the blood in this vessel; the mixture is of purplish hue, and no coagulation has taken place, unless we are to take into account these flabby clots resembling currant jelly.

2d Experiment.—Ammonia.

This alkali has dissolved the colouring matter; the whole is quite fluid.

3d Experiment.—Lime-water.

The lime-water employed here has not dissolved the colouring matter; but it remains to be learned whether it has not otherwise modified its properties; here, again, there is no coagulation.

4th Experiment.—Hydrosulphate of Ammonia.

This salt has not, and the fact is a curious one, prevented coagulation from taking place. M. Bonnet, surgeon to the Hôtel-Dieu at Lyons, has proved that this substance forms in pus when left in contact with the air, as is demonstrable by its action on a piece of paper previously steeped in a salt of lead. He has gone further than this, and stated that the absorption of the gas produces the symptoms ascribed to the absorption of pus; an assertion, however, far from being distinctly proved. Besides, the gas does not remain in the blood, but escapes by the respiratory passages. It has, in this vessel, formed a very soft and fœtid clot.

5th Experiment.—Nitrate of potassa.

This salt has also effectually prevented coagulation.

Hence, of all the substances with which we have mingled the blood, one alone slightly promoted its coagulation, and this the very one to which we should have least inclined, *à priori,* to ascribe such a power. But errors of this kind will always be committed when we indulge in hypothesis instead of basing our opinions on experiment.

I shall terminate this lecture by injecting oxalic acid into the jugular of a dog; the vein is laid bare, and a ligature fixed to its upper end to prevent hemorrhage. I now introduce the point of a syringe into its lower end; the instrument contains 2 drachms of a solution of 2 grammes of oxalic acid in 50 centilitres of water; I shall, consequently, have injected about 35 cent. of the acid into the circulation of the animal. I push the piston, lest by an abrupt or jerking injection I should cause immediate death. The symptoms are, however, very slow in developing themselves, and the lateness of the hour obliges me to defer, for the present, prosecuting the experiment any further.

LECTURE XV.

Anatomy of the capillary vessels.—Researches of M. Berres.—Experiments on the blood with various medicinal substances.—Action of tartrate on antimony and potass in inflammation.—Injection of oxalic acid into the veins.—Pathological phenomena produced by miasmata.—Division of the nervus vagus.

Gentlemen:—One of the grand errors of the physiologists of the last century, and, indeed, of those of our own days also, consists in their supposing the capillary circulation to be regulated by other laws than those of the circulation in the large trunks. We have examined together the chief phenomena occurring in one and the other systems, and with this result: that they differ only in respect of the explanations that have been given of them; but that they are, in reality, linked together and confounded in one common origin. Who will henceforth have the hardihood to call in question the all-powerful influence of the heart on the progression of the liquid through the capillary tubes? Have we not seen pulsations in the column of blood contained within them corresponding to each contraction of the central organ,—diminishing when the impulsion gets weak, augmenting when it becomes strong? When the passage of the blood through a large-sized artery is suddenly arrested the liquid continues to move in the end of the vessel next the capillaries; so, in like manner, the latter continue, for a few instants after the impulsion has ceased to reach them, to be traversed by currents of blood. There is no such thing as aspiration on the part of the venous radicles; there is no such thing as instinctive or spontaneous locomotion of the globules; there are no phenomena observable but such as are a simple consequence of the elastic retraction of the walls of the vessels; so that the heart by its contractions, and the vessels by their elastic retraction, effect the circulation of the blood in the normal state. When both these causes act the movement is jerking; when the action of the first is neutralized, the jerking is transformed into a continuous movement, and this persists until the elasticity of the membranous tubes is thoroughly exhausted, after which all movement is at an end. To those persons who would ask in what respect the hydrodynamic phenomena of the large and small vessels resemble each other, I should, in reply, put the converse question: I should inquire in what they differ; in truth, I see none but points of similarity to be established, and no important distinction to be admitted, between them.

One of the first things to be done by any one attempting to draw up a physiological and anatomical history of the capillaries is, to separate them into distinct groups, each referrible to a certain type. The various points in the history of these vessels, which I have laid before you, are very easily verified in the tissues in which they run in an isolated manner; thus, the mesentery of young animals, the bladder of small rats, distended with urine (as it is at the moment of birth) and the interdigital spaces of the web of a frog, are well adapted for microscopic investigation. But though every capillary tube may be abstractedly looked on as a simple vessel commencing in an artery and ending in a vein, still there are differences to be pointed out as respects the mode of their arrangement in the tissues of which they concur in forming the parenchyma. Here commences a new series of difficulties. I have explained the advantages and disadvantages of adopting injection as your mode of facilitating the study of the capillaries; supposing even that you have, by patience and skill, acquired the knack of introducing the colouring liquid into those vessels alone that are traversed by blood during life, and into none others, still you will not have any very distinct notion of the disposition of the capillary rete. To insure anything like an accurate view of the system, you ought to be able to graduate the force employed in driving in the injection according to the resistance of the vascular walls, and to compose a mixture insusceptible of penetrating into the pores of membranes. Now, we are unfortunately far from having reached such a degree of practical skill as this; extravasation almost always occurs, either from laceration of the vessels or from imbibition; both those causes indeed, usually influence the result; so that, instead of linear canals, we get a succession of minute cylinders, interrupted by multitudes of swellings and ruptures.

I know not to what extent these difficulties have been triumphed over by M. Berres, a German anatomist, who has recently published a work entitled "De Anatomiâ Partium Microscopicarum Corporis Humani," professing to contain a detailed history of the capillary vessels. This production is one of the most complete that has yet appeared; the text is illustrated by an atlas of plates, in which the various modes of arrangement and of anastomoses of the capillary tubes, as well as the microscope used by the author, are carefully represented. In spite of all the labour M. Berres appears to have bestowed on his researches, I should be far from affirming that his book is a perfect one, and that henceforth this section of the science may be looked upon as fully understood. I have already, I believe, pointed out some omissions on his part; but as to pass his classification thoroughly in review would be rather a subject of curiosity than of really instructive study, and as what we want most especially is to have a general idea of the grand divisions of the vascular system, I shall content myself with enumerating the sixteen classes he has established :—

Class
I. Plexus vasculosus linealis cruciatus.
II. Plexus vasculosus undulatus fortior.
III. Plexus vasculosus undulatus tenuis.
IV.* Plexus vasculosus linealis pectinatus.
V. Plexus vasculosus erectilis linealis.
VI. Vasa arcuata, plexus vasculosi dentritici.
VII. Plexus vasculosus longitudinalis solidus.
VIII. Plexus vasculosus dentriticus.

Class
IX. Plexus vasculosus longitudinalis reticularis.
X. Plexus vasculosus longitudinalis cellularis.
XI. Plexus vasculosus excentricus longitudinalis ramosus.
XII. Vasa tenuissima, plexus vasculosi linealis cruciati.
XIII. Plexus vasculosus maculoso longitudinalis.
XIV. Plexus vasculosus excentricus radiatus.
XV. Plexus vasculosus excentricus sarmentosus involvens.
XVI. Plexus vasculosus pencilliformis erectilis.

You perceive, Gentlemen, that this classification is somewhat arbitrary; the denomination of each group indicates to a certain extent, it is true, the observed arrangement of the vessels; but it may be fairly asked, whether these different aspects do not rather depend on the kind of sections made through the tissues, than on a real difference in the disposition of the capillary tubes. Still it is right to add, that I had an opportunity, the other day, of recognizing the variety indicated in class XIV. I was examining an injected kidney, and I fancied I perfectly distinguished the branches directed from the centre to the periphery and terminating in a flexuous fasciculus, with radiated meshes; there was a lobule, furrowed with a sort of groove, interposed between each pair of vascular branches. I have also verified the correctness of the description of some other groups; so that, on the whole, I feel some confidence in the exactitude of the general results announced in the book.

Further researches on the intimate structure of the capillary vessels are, however, called for, before we are entitled to generalize, in an absolute manner, what I have told you concerning the progress of the blood in their cavity. We should be enabled to compare the principal apparatus with each other; the organ that secretes the urine and that forming the saliva cannot, in respect of their vascular arrangement, resemble those which elaborate the bile and pancreatic juice. Each tissue and parenchyma has its special mechanism, and its texture is thereby adapted to the functions devolving on it. The walls of the vessels themselves, in all probability, present a multitude of different degrees of porosity, according as they are destined to allow of the passage of this or that material from the blood. There is a vast field for inquiry here; the various secretions, mucous, serous, glandular, nutrition itself, all of them phenomena as admirable as complex, cannot be thoroughly understood unless we first obtain a profound knowledge of the organization of the tissues, membranes, and parenchymata.

But, Gentlemen, all our attention must not be lavished on the solids; if these merit much of our consideration, there is a still more important subject requiring investigation at our hands. How can we reasonably hope to modify the condition and functions of our tissues if we neglect the state of the liquids? Do not these contain, in the state of health, the materials to repair the losses of our organs? in the state of disease, those fitted to remedy their lesion? The liquids are the sole route whereby medicinal substances can enter the system; the sole source from which our organs derive vigour and life. We have made real progress since the beginning of the last session; what I then only suspected has since been rendered a matter of certainty. Some former experiments gave me a hint as to the disorders induced in the organism by alterations of the blood; and those we have made of late have placed this fundamentally important doctrine beyond all reach of being contested.

I have, since we last met, examined, under the microscope, the different mixtures of acid and blood then shown you; I have a few remarks to make respecting the results arrived at. As regards the chemical analysis, M. Frémy has undertaken that part of the investigation, and when he has concluded it, you shall be made acquainted with the details. All the acids with which we experimented have given rise to very nearly the same phenomena; I sought in vain for any traces of fibrin in the various solutions. The liquid acted on with acetic acid is almost transparent; some nebulous flocculi and very delicate filaments only are observable here and there. In the mixture made with lactic acid I succeeded in making out some delicate particles in suspension, and globules of a particular sort, forming small masses by their union; the colouring matter and fibrin had totally disappeared. In the vessel containing hydrochloric acid, I discovered some small bodies of unusual form held in suspension, some of them rectilinear, others curved in the shape of an S, others in that of a crescent; there were still some traces of globules to be seen, but in all probability in a state of disorganization, for they were larger than those of normal blood. Solution was least perfect in the mixture containing tartaric acid; here were to be seen large coloured globules of various dimensions; close to these, others of the same form, but colourless; next, some much smaller, and finally, irregular corpuscula. It remains to be ascertained whether these were the detritus of globules or of fibrin; upon this point I can scarcely hazard a conjecture. The colourless globules resembled the large white globules which are to be seen in blood immediately after it has been withdrawn from the vessels. In order to procure them we prick the finger slightly with a pin, scrape up the drop of blood discharged on a plate of glass, and examine it under the microscope. Now, no corpuscula of this kind are to be seen in blood in circulation, and it would consequently be very curious if tartaric acid possessed the property of rendering these globules apparent. All the other substances tried had entirely liquefied the blood; no trace either of globules or fibrin was perceptible in the mixtures made with them.

I now return to our experiments: as you no doubt expect, they will have reference to medicinal substances; for it is a matter of primary importance to ascertain, accurately, the kind of effect produced by these substances on a fluid which, in its turn, acts on all the tissues of the economy.

1st Experiment.—Carbonate of soda.

Here is a vessel containing 60 centilitres of water, 2 grammes of soda, and 5 centilitres of blood; the liquidity of the mixture shows clearly how this substance must act when introduced into the circulation.

2d Experiment.—Bicarbonate of soda.

Bicarbonate of soda, water, and blood, have been mixed in the same proportion as in the last experiment, in this vessel. No coagulation has occurred here either; there are even some crystals of the salt still undissolved, showing that the entire quantity thrown into the mixture has not been required to effect the liquefaction of the blood employed. The liquid is of a bright red hue. This substance, which enters in a very considerable proportion into the composition of the Vichy waters, has a peculiar action on the urine, communicating its own alkaline character to that fluid; it is employed in gout, articular rheumatism, and a variety of other affections, in doses of from one to four drachms daily. This experiment confirms what I before told you respecting the property possessed by the salt in question, of giving the blood an arterial colour, at the same time that it prevents it from coagulating.

3d Experiment.—Carbonate of ammonia.

This salt has not dissolved perfectly either; in the midst of the liquid is a small and very delicate clot. We cannot, from this experiment, form any decided opinion on the action of this substance.

4th Experiment.—Subcarbonate of potassa.

The subcarbonate of potassa is, as it were, a succedaneum of the bicarbonate of soda; it is employed under the same circumstances as the latter salt, but more especially in cases of phosphate of lime gravel, a very common affection in Burgundy. It dissolves the blood, but, herein differing from the salt of soda, it gives it a black colour.

5th Experiment.—Sulphate of potassa.

The sulphate of potassa has furnished a precipitate which I believe to be formed of albumen, or globules; I must endeavour to ascertain this with greater certainty.

6th Experiment.—Chloride of lime.

The effect produced by this substance is hardly appreciable;

however, there is an appearance of commencing coagulation of the fibrin.

7th Experiment.—Chloruretted water.

Here there is no coagulation; and the mixture is tinged black.

8th Experiment.—Sulphate of iron.

In this instance very evident chemical reaction has taken place; there is an abundant precipitate of albumen; no conclusion can be drawn from this experiment respecting the influence of the salt employed on the coagulation of the fibrin.

9th Experiment.—Alum.

Alum, which is, chemically, a sulphate of alumina and potassa, is employed externally, under the impression that it acts as a very powerful astringent, yet you see that not a trace of coagulum is to be detected here.

10th Experiment.—Bichloride of mercury.

This salt has produced a very strange effect; in the upper part of the vessel are globules in a state of solution, in the lower a combination of the chloride with albumen. The result of this experiment, therefore, very fully proves the excellence of the ordinary habit of employing albumen as an antidote for corrosive sublimate.

11th Experiment.—Acetate of lead.

The acetate of lead has precipitated the albumen without acting on the fibrin.

12th Experiment.—Ioduretted hydriodate of potassa.

We now come to a substance from the use of which very admirable effects are obtained in cases of chronic diseases of the bones and articulations, of scrofula, and various other complaints, till of late pronounced to be incurable,—I mean the hydriodate of potassa. You have all of you heard of the advantageous results of the exhibition of this medicine in cases of white swelling, old anchylosis, coxalgia, &c.; it is, indeed, one of the substances which I employ, with greatest success, in a variety of disorders. It has here produced a very remarkable effect; coagulation has occurred, but whether it be the albumen or fibrin which has solidified I am unable to determine at this moment. I dilute the mixture with water, and forthwith, as you perceive, a large number of brick-red flocculi appear. This experiment is worth the trouble of further inquiry, and it shall, therefore, be repeated.

13th Experiment.—Ferrated cyanuret of potassa.

The cyanuret of iron and potassa has completely liquefied the mixture.

14th Experiment.—Arsenite of potassa.

As for the arsenite of potassa, one of our most active medicines, it has so completely decomposed the liquid that it is impossible to say what name should be given to the matter you see in the vessel; all that is quite certain is, that not a shadow of coagulation is to be discovered.

15th Experiment.—Nitrate of silver.

Four grammes, or 1 drachm, of nitrate of silver, 60 centilitres of water, and 2 of blood, have been mixed here; a very distinct nebulous clot has formed. This salt is not much used internally, but its external applications are numerous and diversified in their intent; you perceive that it modifies the coagulation of the blood, but does not prevent it from taking place.

16th Experiment.—Alcohol.

Here are 10 centilitres of alcohol, 60 of water, and 1 of blood; the modification in the coagulation is only trifling, consequently this substance is not pernicious when taken in such quantities only, a circumstance which affords me much gratification for the sake of its very numerous consumers.

17th Experiment.—Phosphate of soda.

This salt has furnished a remarkable coagulum; you observe that it does not, in a single particular, present the characters of a clot of ordinary blood; there are undissolved crystals at the bottom of the vessel.

18th Experiment.—Tartrate of antimony and potassa.

I have already made some researches on the subject of tartar emetic, but they were not directed to the point now under consideration. This salt is much used in pneumonia, with a view to its acting on the respiratory organs, in rheumatism, &c.; some practitioners administer it in combination with the sulphate of quinine, in certain intermittent fevers. I have here put two grains of tartarised antimony into a mixture of six centilitres of water and one of blood; its action has borne principally on the globules, which are dissolved, and have tinged the liquid red; however, a nebulous clot has also formed. I am inclined to doubt, from the effects produced by injecting this salt into the blood, that it can produce favourable results by its immediate introduction into that fluid. Thus, I injected a small syringeful of a solution of one drachm of tartarised antimony, in four ounces of distilled water, into the jugular of a dog. The animal almost immediately gave signs of being under the influence of the salt by making movements of deglutition; such movements always precede the act of vomiting, and, indeed, seem necessary for its accomplishment. It is not, as has been advanced, through contraction of the muscular fibres of the stomach, that the contents of that viscus are expelled during the process of vomiting; far from undergoing retraction while that process is going forward, the organ actually expands, and it is in order to facilitate and promote this expansion that the animal intuitively swallows a certain quantity of air. This fact has not escaped the notice of the sailors on board our packets; when they see a passenger seized with involuntary movements of deglutition, they immediately say that he is going to be sea-sick. Once the stomach is distended with air, the abdominal muscles and diaphragm compress its walls, and expel the matters contained within it. These results, experimentally arrived at, led me further to the important discovery, in a therapeutical point of view, that the action of tartar emetic does not bear directly on the gastric mucous membrane. I removed the stomach of a dog, and substituted a pig's bladder in its room, and found that an injection of tartarised antimony into the veins induced nausea and vomiting as before. Be these matters as they may, however, the important point for present consideration is, that vomiting was soon followed by dyspnœa, cough, and fever; all these symptoms indicated the existence of a serious lesion of the respiratory apparatus, and death supervened in the midst of the various phenomena proper to pneumonia. After death the lung appeared somewhat altered in colour, it was grayish; it had, indeed, the look of a lung infiltrated with a little pus, but this was not really the case; consequently tartar emetic does not, like mercury, act simply in determining phenomena of obstruction; it attacks the blood chemically, decomposing some of its elements, and to this change in its composition were, in great measure, due the symptoms and cadaveric phenomena observed in the case adverted to. The lungs, however, were not the only organs affected,—there were dark red patches and vascular arborisations visible throughout the entire extent of the intestinal canal, indicating obstruction in the abdominal circulation. I in vein strove to push an injection through the mesenteric arteries; the liquid did not return by the corresponding veins. The obstruction of the capillary tubes was here, too, the starting point of the lesions produced; the *gastro-enteritis* I detected was no more than a mechanical effect of the exhibition of blood through the walls of the vessels, and its consequent extravasation into the surrounding tissues.

These chemical effects of tartar emetic appear the more extraordinary when we reflect on the confidence placed in this salt as a remedial agent in certain disorders. The author of the magnificent work on Mediate Auscultation has filled several pages in expatiating on the advantages he had derived from its administration, especially in the disorders already named,—pneumonia and rheumatism. Now, it happened that I was designated to succeed Laennec at the Neckar Hospital; and, out of respect to that illustrious observer, I continued the exhibition of the same preparations of antimony, and in the same affections as he was in the habit of employing them. But I must confess the results were not such as I had hoped to see them; though I had the same house-surgeon and the same apothecary as my predecessor; though everything, in short, remained precisely as he had left it, I could not satisfy myself that any very notable modification in the progress and duration of those affections was produced by the use of the vaunted drug. I consequently, after a few weeks' trial, ceased altogether to employ it. You must be careful, Gentlemen, about forming your opinion too precipitately respecting the efficaciousness of a medicine because it has apparently produced a few successful results. Who can assure you that the patient would not have recovered quite as well if you had not employed it? Rheumatism, for example, yields to bleeding, or to tartar emetic, to every imaginable kind of treatment,—yields, above all, to rest and diluents; in my hospital practice I never have recourse to the lancet, to tartarised antimony, or to leeches in the treatment of this affection, and yet I have no hesitation in affirming that all the cases of rheumatism I have treated have terminated favourably.

19*th Experiment.*—Cinchonine.

This alkaloid exists in cinchona bark in conjunction with another substance belonging to the same class, quinine. One grain of it added to this mixture has given rise to the formation of one of the most delicate clots I have ever seen; it resembles vegetable jellies before they have become completely solid by the progress of refrigeration.

20*th Experiment.*—Sulphate of quinine.

This substance has also formed an almost invisible clot; possibly, however, the formation of the coagulum in this instance depends on the presence of the sulphuric acid, which we are obliged to employ in order to produce a soluble material. We must vary the experiment in some way, in order to ascertain the nature of the action of the alkaloid on the blood with greater precision.

21*st Experiment.*—Decoction of digitalis.

The decoction of foxglove, well known to you for its sedative effects on the contractions of the heart, has prevented coagulation from occurring.

Here then, Gentlemen, you have a series of facts regarding a question of high interest, all of them completely new; these are so far mere trials, but we will not stop here; when we shall have exhausted the list of substances worthy our attention, we will repeat such experiments as may not have given us complete satisfaction. Once our review has terminated, we shall, I trust, have acquired some new and solid information on the *modus operandi* of medicines, one of the most obscure points in the science. So far, you will observe that we have met with few therapeutic agents capable of increasing the coagulability of the fibrin; for a solitary substance possessed of that power, we have found several capable of liquefying. Nevertheless, we must hope to be more successful in this way in our ulterior researches; they will doubtless afford us compensation for our disappointment. But even if our expectations in this respect fail altogether to be realised, we shall at least have the satisfaction of feeling that we have, by our results, shown the necessity of expunging from medical formularies a number of substances which are capable of inducing disorder in the economy. This single result is worthy of attention, as it appears to me; for our art has been instituted for the relief of the sufferings of our fellows, and not for the purpose of allowing us to make a vain parade of factitious science; to heap drug on drug without caring for the effects they produce. For my own part, I care little for the learned prescriptions in which the majority of practitioners delight; the mysterious dignity of their composition always seems to me calculated to throw chaff in the eyes of the vulgar, and rather to enhance the merit of the physician than really to effect the recovery of the patient. Though my *amour-propre* may often suffer thereby, I prefer infinitely to abstain from prescribing altogether, to ordering drugs respecting the effects of which I am ignorant. Far from discouraging you, Gentlemen, these considerations should animate you with fresh zeal; they point you out a vast and untrodden field for research. If you never lose sight of the precepts taught you from this chair—precepts of which I have invariably endeavoured to give you in my own person the practical illustration—if you are guided in your researches by a spirit of the strictest exactitude, rejecting all that is not proved by experiment and observation, each of you may hope one day to occupy an honourable place among the benefactors of humanity.

You remember that I introduced seven grains of oxalic acid into the jugular vein of a dog, at the close of my last lecture. The lateness of the hour prevented us from waiting for the results of the introduction of the poison to manifest themselves; I proceed to inform you of what has occurred since our separation. The animal was seized with dyspnœa soon after the operation; his condition grew more and more desperate every hour, and he expired the following morning. Here is his body: judging from the effects of oxalic acid on the blood in glass vessels, we must expect to find the blood in his body perfectly liquid. But do not let us anticipate,—I remove the anterior wall of the thorax, but the lungs do not collapse in consequence. I open the pulmonary artery; the blood flowing from it is liquid and brownish, just as it would have been had it been acted on with sulphuric acid; and, therefore, the conclusion is inevitable, that oxalic acid produces the same effects on the blood while circulating in the vessels, as when removed from them. This is an important fact; it confirms me more strongly than ever in the opinion that the vitality of the blood and of the walls of the vessels neither prevents nor even modifies the chemical action exercised by a variety of substances on that fluid. As for the lesions of the pulmonary organs, it is enough, for the present, to say that the cells are distended with effused blood; that they were thus rendered unfit for supporting respiration, and that death was produced by asphyxia. The autopsy will be proceeded with after the lecture, and such of you as are anxious to ascertain for yourselves the nature of any remaining lesions produced by the poison, may be present at it.

I now proceed to the autopsy of another animal, who died yesterday from the effects of an injection of putrid water into the veins; although he only survived the operation a single day, we should, in addition to finding the blood blackish, liquid, and clammy, detect morbid alterations in the intestinal mucous membrane; those existing in the lung are, on the other hand, likely to be unimportant. I almost venture on predicting the pathological results of this sort of experiments, for they have hithterto been invariably uniform. I may here allude to an opinion, which has been adopted without the least foundation, respecting the cause of the cadaveric rigidity remarked in all subjects dying of yellow fever, the cholera, and other affections originating in the same source,—miasmatic infection. It has been stated that the phenomenon depends on the solidification of the fibrin. Now, I appeal to the evidence of your senses; tell me if, in diseases of the description alluded to, as well as in those I produce in animals, the blood be not fluid and its fibrin liquefied? Here, then, is another erroneous notion to be rectified. I open the thorax of the animal, and you perceive, in truth, that the lungs present no apparent lesion; they have preserved their normal elasticity, and in nowise resemble those of the animal poisoned with oxalic acid. Here is one of my anticipations realised; and, further, it would appear to result from these experiments that the viscosity of the blood does not prevent it from traversing the capillaries of the lung, whereas the contrary is the case with the abdominal vessels of that class. I open the pericardium: the heart is flabby and collapsed, the blood in the right cavities liquid, black, and viscous; the left ventricle is perfectly empty. Passing to the abdomen we find the intestines black-coloured, *inflamed*, according to the current phrase; transudation has taken place on the internal surface, and is partly constituted by the colouring matter of the blood; this appearance fully accounts for the bloody stools of the dysenteric flux, such as this animal laboured under. We are thus enabled by our experiments to point out the mechanism of the most serious pathological conditions. Where are the phlogistic authors that can base their ephemeral theories on such conclusive and irrevocable facts as these?

We have another animal to examine; the mode of death in this case will supply as interesting materials for consideration as in the others. About six weeks ago I divided the right vagus nerve in this animal; his respiration became slightly laborious after the operation, but after the lapse of a few days he had recovered from its effects so completely that he presented no abnormal symptom. The day before yesterday I divided the nerve on the left side, as well as the cellular band that had formed between the divisions of that previously cut. I was desirous by this experiment to ascertain if the nervous action prevented the coagulation of the fibrin. The animal died a few hours after the second section. I have just opened the axillary vein; observe that the blood flowing from thence is as fluid as it is possible for it to be; the lung is evidently diseased; one of the lobes of the right organ is entirely *splenified*, to use the expression of certain pathologists. It is possible this lesion was developed under the influence of the section of the eighth nerve; still one of the left lobes, the nerve of which was only divided the day before yesterday, is almost as profoundly altered in character as the right, The blood distending the organ is also quite fluid. The experiment must be repeated several times before we can feel certain as to the effects produced by the section of the pneumogastric nerves on the important phenomenon of the coagulation of the blood.

LECTURE XVI.

Capillary circulation.—Substances which render the blood fluid.—Action of putrid matter.—Condition of the blood in scurvy.—Huxham's observations on the blood.—Experiments with acids.

Gentlemen:—The knowledge we have, up to the present day, obtained by the cultivation of animal chemistry is very limited; and, indeed, it is necessary to be deeply convinced of the importance of its study, and of the vast influence it is, in all probability, destined to exercise on the future advancement of our art, in order to contend with patience against the numberless difficulties of various kinds with which that study is beset. No branch of the science requires greater perseverance, practical skill, and devoted attachment to intellectual pursuits, on the part of those undertaking its investigation; while its cultivators expend their time in discovering a solitary fact, another outstrips them with ease by framing hasty hypotheses. But do not be discouraged from adopting the former course, from seeking laboriously for facts, by this seeming failure; you will merit better of your fellow men by the establishment of a single truth, than by the advancement of a host of brilliant speculations, though such may not be the opinion of the public, who judge blindly, for the most part, and seem to have a natural benefit for the whimsical in science as in everything else: be assured, however, that it will be the opinion of those for whom such labours are really intended—the scientific few.

The number and variety of the materials entering into the composition of the blood render their separation a very difficult task, and the accurate appreciation of the properties of each a truly delicate one. Such being the case, the almost total impossibility of detecting a few atoms of foreign matter in that fluid is evident even *à priori;* yet the presence of such trifling particles is, in many cases, sufficient to alter its constitution thoroughly, to change its nature to such an extent that it becomes unfit to perform the functions for which it is destined. Consider the experiment of the injection of a few drops of putrid water into the veins: scarcely have they entered the circulation when the animal is seized with most serious symptoms; he ejects, by vomiting, a blackish, clammy liquid, which is nothing more than blood that had been exhaled on the inner surface of the stomach, and the mucous membrane is found studded throughout its entire extent with patches of blood effused into the subjacent cellular membrane. This is evidently not the result of a modification in the properties of the walls of the vessels; it is the consequence of the change of constitution undergone by the blood, of the loss on the part of that fluid of properties adapted to those of the ducts in which it flows. This we *know*, because we are ourselves, in the case supposed, the authors of the disorders we observe; but if the same symptoms had been developed without our interference, could we, unassisted by chemical analysis, have succeeded in making out the starting point of the disease? And this is precisely our state of ignorance as regards many affections.

If it were possible to remove from the blood, one by one, the various materials which, by their union and chemical combination, constitute it in the normal state, I have no doubt we should succeed in obtaining very interesting information on the principal functions of the economy. Properties which now appear insignificant to us, and which we scarcely mention in our descriptions of the fluid, are possibly closely connected with the persistence of the normal state of the functions. As a proof of this I may adduce the discoveries we made last session. When enumerating the physical characters of the blood, I simply mentioned its coagulability, without specifying particularly the influence that condition might have on the greater or less facility with which the hydrodynamic phenomena of the circulation are accomplished. At that time, indeed, I was possessed with a notion that the faculty inherent in that liquid of setting into a mass, must act as an obstacle to its progression in the vessels. But, Gentlemen, I then took an imperfect view of the question; a beautiful contrivance, of which I subsequently discovered the existence, is employed to counteract any such tendency. With the view of depriving the blood of the power of coagulating I removed its fibrin; the animal that underwent the operation died almost immediately after its performance; the sudden abstraction of the coagulable element of the blood invariably puts an end to life. Here, then, (would the followers of a system of pure vitalism exclaim,) here, then, instead of simplifying, you have complicated the problem of the circulation; you see into what a dilemma you have got by your attempt to apply the characteristics of inert matter to organized bodies. So long as the blood was fitted for circulation in the living tubes it was incapable of moving in metallic ones; you deprive it of the faculty of clotting, and it at once acquires that of moving in metallic tubes, while it loses the property of being fit to circulate in living ones,—physical and vital laws are here reciprocally exclusive.

This view of the matter, Gentlemen, seems to me not only to go further than the facts warrant, but also to be founded on a most unsound basis. By removing the fibrin we modified the composition of the liquid and not its vitality; if we reinject it into the circulation we find that it does not act as a poison; it is not the tissues it attacks, but the walls of the vessels; it is not because it touches the most delicate and susceptible parts of the frame that it causes death, but precisely because it is retained in its vessels, and traverses their coats by imbibition. So true is this, that you may remove a considerable quantity of fibrin without producing any evil consequence, provided you are cautious about abstracting a small portion only at a time, and allowing some time to elapse between each two operations. I have found that when the process is conducted in this manner, either no disorder of the functions follows, or else some very trifling disturbance; whereas symptoms of the most serious character supervened in animals suddenly deprived of all the coagulable material of their blood.

If it be true that the sanguineous effusions occurring during life depend solely on a want of adaptation of the properties of the liquid to those of its containing tubes, similar effects must be producible in the dead body. Of this you shall be yourselves the judges. Here is a piece of intestine which I have had injected with defibrinised blood; you perceive by the colour of the organ that transudation has taken place through the walls of the vessel: instead of well-defined vascular ramifications we find reddish streaks, the central part of which is formed of a membranous cylinder, and the circumference by tissues impregnated with colouring matter. Rupture would appear to have occurred in different spots, but this is a mere appearance; transudation has really taken place through the natural pores of the vascular tunics. Another fact proves more conclusively than the preceding ones, that it is not from losing one of its elements, the fibrin, that it becomes incapable of circulating, but from its having ceased to be coagulable; the fact is, that we have it in our power, by the injection of certain substances, such as the subcarbonate of soda, for example, to liquefy the blood, and produce lesions of exactly the same description as those determined by the abstraction of the fibrin.

Whoever has studied pathology, not in written volumes, but in the great book of nature, must have been struck with one important fact, namely, that there exist certain morbid states which it is impossible to localise, and which, in reality, affect the entire organism. Certain diseases, I am aware, first attack a limited point of the body, and subsequently become general; thus, the lung may be first attacked; its tissue ceases gradually to be permeable, the blood is unable to find a passage through it, in order to come into contact with the oxygen of the atmosphere; the other functions then become affected. Prostration of strength supervenes, the tongue grows dry, the face assumes a vacant stare, the ideas wander, the limbs become the seat of involuntary trembling; hemorrhage from the mucous membranes of the nares and intestinal canal follows, and the individual dies. Are we to ascribe all these disorders to the violence or malignity of the pulmonary inflammation? Certainly not; such would be a very erroneous view of the case; admitting the affection to have been primitively local, it must also be allowed that it was only from the moment the blood ceased to possess the characters of the arterial fluid that the entire economy commenced to suffer. When any other organ is similarly or even more seriously diseased, you will not observe the same series of phenomena; obstructions of the lung are the only disorders attended with such serious consequences as those adverted to; for the special function of that organ is to vivify the blood, and once it ceases to play its part that fluid becomes unfit for the support of its functions generally.

There are other circumstances wherein a change in the constitution of the blood appears to open the scene, and wherein the organic lesions are secondary phenomena. The habitus of the disease is the same here, but the course of the symptoms is reversed. In the affection termed typhoid fever, the whole economy is affected at once; the different organs are, it is true, successively attacked, but *after* the whole frame has received the primary shock; thus, the intestinal follicles do not become red and swollen at the outset of the disease, but after the lapse of a certain time; at a later period still they ulcerate. They who look on the morbid state of those follicles as constituting the essence of the disease, reverse the natural order of things, and take for the cause that which is only an effect. Nothing is more common in these cases than obstruction of the pulmonary circulation; the vesicles become engorged, the bronchi choked up, and the patient dies asphyxiated. The spleen is the organ most closely resembling the lung in point of vascularity, and accordingly we find that almost all writers state that the spleen is constantly increased in size, diminished in consistence, and saturated with fluid, in subjects dying of typhoid fever. For my part, I would just as soon call that affection a *splenitis* as an *enteritis:* one and the other expression appears to me equally applicable, or, to speak more correctly, equally ridiculous.

The most striking point in diseases of this stamp, affecting the whole economy, is the fluidity of the blood, and the absence of the clot, or, if it exist at all, its small size and softened state. Up to the present time these conditions have been noticed by observers rather in order that they might appear to neglect nothing, than for the purpose of drawing from them any deductions capable of throwing light on the origin of the symptoms. I was long myself before I recognized the vastly important consequences of the loss of coagulability; now that my ideas on the subject are changed, my first care, when called in to a case, is to examine patiently the characters of the fibrin. If I find it soft, friable, soluble, and fusible, I am satisfied: I know thereby what organs must be affected, and I know what is the nature of their lesions.

One of the most remarkable among the morbid phenomena presented by animals into whose veins I had injected putrid water, was undoubtedly what is termed *inflammation* of the intestines; that is to say, exhalation of a matter having the colour of *washings* *of flesh.* This matter, which adheres to the mucous surface of the intestine, having there undergone a sort of gelatinous solidification, is nothing more than the fibrin of the blood coagulated in a peculiar manner. Of this I am perfectly certain, and for these reasons: I detached some of this intestinal exudation from the surface with which it was connected, carefully washed it, and thus separated the colouring matter from it, and caused the precipitation of delicate particles of fibrin. I employ the word particles designedly, in order to distinguish them from the globules, from which they materially differ; the globules, in fact, are, as it were, organized, effect a determinate form, and are all strongly similar to each other; what I term particles, are, on the contrary, an accumulation of very minute fractions of matter, of different forms and sizes, possessing no regularity or symmetry of shape. These little masses or filaments are not, therefore, composed of fibrin coagulated in the ordinary manner, inasmuch as the latter presents the cellular, vascular, and arborised organization developed in the normal clot, and in the different adventitious formations.

I felt anxious to ascertain whether the *gelatinous* clots that I produce by mixing blood with certain substances, such as subcarbonate of soda, might not bear some analogy to the masses of fibrin deposited on the surface of the intestine in the cases to which I have been alluding. Certain it is that a very wide distinction is to be established between the normal coagulation of the fibrin, either in the cicatrices of wounds, or even the substance of the coagulum, and the aggregation, I would almost say the agglutination, of fibrinous particles accumulated on the surface of *inflamed* intestines. Some years past MM. Prévost and Dumas, who then believed that the globules were partly formed of fibrin, advanced, as their opinion, that these masses were in reality globules condensed as a product of excretion on the surface of the diseased bowel. It is now a matter of certainty that the globules and fibrin are two different things, easily separable from each other; but it is also certain that the authors just named caught a glimpse of the question.

Another new and curious fact, which appears to me of very important character, is the influence exercised, as we have lately discovered, by the pneumogastric nerve on the vitality of the blood. I, much to my surprise, observed that the section of the eighth pair deprived that fluid of its power of coagulating, but whether by a mediate or immediate action, I am unable to decide. Here is the blood of an animal who died from the effects of division of that nerve; although it has lain in this vessel several days it is perfectly liquid. The minute particles you see adhering to the sides of the vessel are assemblages of deformed globules, placed side by side, and very slightly adherent to each other; in a word, they are without any of the properties characteristic of fibrinous coagulation. This question is important; for if it be true that the nervous system exercises an influence on the coagulation of the fibrin, it is plain that that property must be henceforth looked on as a physiological one, and not as one capable of being influenced by the application of merely physical principles. This evidence in favour of the action of the nervous system on the coagulability of the blood, suggested to my mind an experiment, which I caused to be performed, with the following result. I had long since remarked that the arterial blood loses its scarlet hue in individuals labouring under an attack of well-marked apoplexy. In such cases the temporal artery is occasionally opened with the view of diminishing the constantly-increasing pressure exercised on the brain by the entry of fresh blood into its substance; I have myself frequently performed arteriotomy under such circumstances, and have found the blood black and viscous. I have not satisfied myself whether it coagulates like venous blood, but here is the experiment performed to elucidate the question:—I directed that a small opening should be made in the skull of an animal; that sufficient liquid should be injected through it, under the dura mater, to determine the characteristic phenomena of compression of the brain; and, finally, that the animal should then be bled from an artery. These three glasses are filled with his blood: in each, as you perceive, the serum is turbid; it holds some of the colouring matter in solution.

You will remark, also, that the clot is tremulous, undulating, and so far from being of the healthy firmness, that the simple weight of this glass rod is sufficient to break it in two. There is one point, however, which makes me doubt whether the experiment was made with all the desirable precision, namely, the colour of the blood; instead of having the black hue of venous blood, it is scarlet, not only on the surface, but throughout its entire mass. I will have the experiment repeated in my presence, in order to feel more satisfied as to the fact of strong compression of the encephalon being or not being capable of producing the effect on the blood to which I alluded.

You will recollect that when engaged last year with the question of the condition of the blood in scorbutic individuals, I told you that to its non-coagulability were ascribable the peculiar phenomena of their disease, such as swelling of the gums and the large petechiæ, and the yellow and black œdematous infiltrations more or less constantly found in the limbs and trunk. I have lately requested some blood of such patients to be sent me, for the purpose of repeating my last year's experiments. This repetition is far from being unnecessary, as you will presently see.

A short while past M. James inserted an essay in the "Gazette Médicale" on this subject, wherein he developed, with considerable talent, the ideas I broached respecting it last year, and supported the opinion that the alkaline character of the blood might be the cause of the non-coagulability of that fluid, as well as of the symptoms of scurvy. But, though this Essay is a meritorious production, it furnishes decisive proof how very necessary it is to draw general conclusions with deliberate caution, for the inference drawn in it is quite fallacious. Here is the blood of an individual affected with scurvy sent me by M. Leuret, physician to Bicètre. The gums of the patient are, I am informed, considerably swollen; his teeth are quite loose; the surface is covered with large petechiæ, and some parts of the body are œdematous: nevertheless the blood presents a firm and consistent clot, the colouring matter alone is visibly altered in constitution, being of a brownish-red colour. Consequently we cannot look on this question as by any means settled.

Among the medicated drinks employed in fevers sulphuric water is one of those most frequently prescribed; I was consequently desirous of ascertaining the nature of its direct action on the blood. With this view I mixed a few drops of sulphuric acid with a larger quantity of water, proportionally, than that entering into the composition of the "sulphuric lemonade" of our hospitals, and injected a few centilitres of the liquid into the jugular vein of a dog. Death followed almost instantaneously, and the blood was found to be non-coagulable. At the present day no attention is paid to this condition of the blood, though our post-mortem examinations show it to be a frequent attendant on morbid conditions of the economy; but the medical men of the last century, whose powers of observation were quite as great as those of our own day, though their pretensions thereto were, perhaps, not so great, remarked the property of certain alkalious salts, as they called them, of preventing the blood from coagulating in glass vessels; and, further, noted the fact, that in certain epidemics that fluid lost the faculty of setting into a mass. I have of late been looking through the works of some of those writers. One among them, an "Essay on Fevers," by Huxham, a celebrated English practitioner, struck me particularly. The author, after having, in one of his chapters, established the differences between the thick and viscous blood of plethoric subjects, and that of individuals of an opposite temperament, in which the serosity abounds, and the globules are less numerous than usual, enumerates various substances capable of producing the last-named constitution of the blood, such as, among others, preparations of aloes, the volatile alkali, spirits of hartshorn, and laurel-water. Speaking of the latter, he says that "it makes the crassamentum vastly less dense, and exceedingly more soft and tender, and turns the serum red, or of the colour of Burgundy wine, as appears from the curious experiments of Dr. Nicholls and Dr. Langrish."* And elsewhere he expresses himself in these terms:—

"But, besides these, there is, moreover, a third state of the blood, of more dangerous consequence than either; I mean a state of it that more immediately tends to *dissolution* and *putrefaction*. This is evidently the case in some scorbutics (as they are called) where, without any considerable antecedent sensible disorders, (more than, perhaps, a kind of lassitude and languor,) persons have, on a sud-

* Essay on Fevers, p. 45. Ed. 1769.

den, an eruption of violet-coloured, livid, or even black and blue spots all over their body, and forthwith fall into profuse, and sometimes dangerous and even fatal hemorrhages, when they have scarce thought themselves, or been thought by others, to be under any manner of disorder. Abundance of instances of this kind happen: I have seen a great many, both in children and grown persons, and frequently foretold the ensuing hemorrhage.

"Where women have such eruptions, or black or blue vibices, or large irregular spots like bruises, they are always subject to a vast overflow of the catamenia, if not to other profuse hemorrhages. Nay, when persons of either sex are affected with these appearances, they are apt to bleed excessively from the slightest wound, and very often without any, from the gums, nose, guts, or urinary passages.

"The blood of such persons, when it hath been drawn off, in order to prevent the further progress of the hemorrhage, as was imagined, (which, by the way, is very improper, unless there are very manifest signs of a plethora,) always appears a *mere gore* as it were, not separating into crassamentum and serum as usual, but remaining in an uniform, half-coagulated mass, generally of a livid or darker colour than usual, though sometimes it continues long very florid; but it always putrefies very soon."*

Further on he adds:—"Pestilential effluvia also soon destroy the crasis of the blood and produces an universal gangrenous disposition in the humors. This is evident from the frequent and fatal hemorrhages, excessively fœtid sweats, vomitings and stools, and the general necrosis (*i. e.* gangrene) that follows, which have been observed in the plague and pestilential fevers by the best authors. The blood in these cases does not coagulate as usual."†

He also speaks of an epidemic that occurred in 1737, in which pneumonia appeared frequently as a complication, and which was especially remarkable by the blood being so extremely liquid that it was difficult to arrest the hemorrhage from leech-bites or opened veins. If, therefore, Gentlemen, medical men who had advanced but little in the sciences of physiology, chemistry, and physics, were sagacious enough to make such important observations as those I have just quoted, we may reasonably expect to obtain results of the highest value from our researches in the same direction, now that the means of investigation within our reach are more multiplied and more sure.

I have entered upon a series of experiments for the purpose of ascertaining the effects of the mineral and vegetable acids of the blood, and have commenced them with sulphuric acid. With a view to discovering in what dose that agent becomes poisonous, I took eight glass vessels, each of them containing five centilitres of blood; to the first I added one drop of concentrated sulphuric acid; to the second, two, and so on. The first mixture is already almost

* Op. Cit. p. 41.

† Op. Cit. p. 51.

completely liquefied, with the exception of a sort of sediment at the bottom of the vessel, perfectly distinct in nature from a true coagulum; the colouring matter is especially changed in constitution. Now, the conclusion deducible from this experiment, namely, that a single drop of sulphuric acid suffices to deprive five centilitres of blood of the power of coagulating, is certainly an unexpected one.

The alteration of the properties of the blood in the second vessel is much more marked; it has acquired a deep black colour, and there is a slight deposition of discoloured globules. In the third vessel, the black colour is still more evident, and the liquid is also more viscous; it is probable that some chemical action has taken place between the acid and fibrin, and I am persuaded the presence of such blood as this in the vessels of an animal would cause its instantaneous death. The appearance of the other mixtures is very much the same as that of the three we have examined; which seems to show that there is no great difference between the actions of two and eight drops of acid on a given quantity of blood: it would be well to experiment on smaller quantities even than a single drop, by dividing it, and studying the effects of the fractional parts. I have no doubt that a chemical action, the combination of the fibrin and sulphuric acid, must take place in these cases; but I have consulted some of our most distinguished chemists on the point without obtaining any important information from them, which is, indeed, little to be wondered at, when we consider that organic chemistry is in its infancy.

Some other inquiries of this kind have also led to results at variance with the opinions currently received respecting the effects of certain medicines. You are aware, for example, with what rapidity nitric acid coagulates albumen, and forms a nitrate with it. Well, I mixed ten drops of that acid with five centilitres of water, and poured them on two or three millilitres of blood, and no coagulation has taken place; there is only a slight precipitate at the bottom of the vessel, but whether of fibrin or albumen I am unable to say. In every trial I have hitherto made I have found all the acids act in very nearly the same manner on the blood: phosphoric acid, for example, which coagulates albumen with such energy, has completely liquefied the sample of blood I show you; a single drop only has been added, and yet the colouring matter has totally disappeared; there is an appearance of a precipitate of some sort, however, which I must examine under the microscope.

1*st Experiment*.—Citric acid.

This substance, which is used abundantly in effervescing and other draughts, has caused the liquefaction of the blood, though only employed in the same proportion as the nitric acid just spoken of; there is a slight precipitation of colouring matter. The injection of this agent into the veins of a living animal killed it instantaneously. We will examine its body at the end of the lecture.

2d and *3d Experiments.*—Boric and arsenious acids.

These acids have also liquefied the blood, without producing any other remarkable result.

4th Experiment.—Pure tannin.

Among the substances that have particularly excited my curiosity stands pure tannin, recently discovered by M. Pelouze, and now employed as an astringent; I have myself prescribed it with a view to diminish the abundance of various secretions, hemorrhages, and morbid discharges. It was supposed that by acting locally on the mucous membranes, by tanning them, as it were, this substance was enabled to put a stop to certain diarrhœas and abnormal fluxes. However, Gentlemen, the fiftieth part of a grain of this substance, which is, in reality, tannic acid, has prevented the coagulation of five centilitres of blood. It does not appear to have acted on the colouring matter, which you see here separated on a filter, and to all appearance possessed of its normal properties. According to the researches of M. Pelouze, tannin is a body possessed of the properties of an acid, and forming a tannate of fibrin when brought into contact with that principle.

5th Experiment.—Soluble cream of tartar.

Cream of tartar is administered in very large doses to individuals of every age and sex; mixed in the following proportion:—

Soluble cream of tartar - - - - - - - - - -	5 grammes,
Common water - - - - - - - - - - - - -	50 idem,
Blood - - - - - - - - - - - - - - - - -	2 centilitres,

it has given the whole a deep olive colour, and thrown down a slight precipitate of albumen. I do not mean to assert that this substance acts in the same manner when ingested directly into the stomach; it is indispensably necessary to ascertain whether absorption through the intestinal canal does or does not modify the chemical properties of bodies; and if it does, how it modifies them. As for the action of sulphuric acid, it is reasonable to conjecture that, to use the figurative language of M. Dutrochet, that substance is an enemy to endosmosis; that is to say, that it is not imbibed by the tissues in the same manner as has been demonstrated to be the case with pure water.

6th Experiment.—Carbonic acid.

Here is the blood of an animal asphyxiated with carbonic acid gas; it is distinctly coagulated, and yet, as you will recollect, we have in other instances found the blood liquefied by this acid, particularly in the case of the female suffocated with the fumes of charcoal, whose case was communicated to me by M. James.

7th Experiment.—Tartarised antimony.

Tartar emetic, employed in the mixture I show you, has not prevented the blood from coagulating; it would, consequently, appear calculated to facilitate the circulation of the blood through the lung, and promote the resolution of pulmonary hepatisation; yet it causes, as I showed you in my last lecture, this very hepatisation.

8th Experiment.—Ioduret of potassium.

Iodine is among the substances that appear to possess the power of increasing the energy of coagulation; this mixture of a gramme of the ioduret and a centilitre of blood is completely solidified.

9th Experiment.—Hydrochlorate of baryta.

The hydrochlorate of baryta is employed in cases of engorgement, white swellings, &c.; it has here produced a peculiar effect on the colour of the liquid, which is arterial red; there is a tolerably firm clot too.

10th Experiment.—Borate of soda.

This substance has caused the formation of a clot with about the degree of consistence of currant jelly.

Here then are a certain number of substances which, far from opposing coagulation, appear to promote it; they are more particularly common salt, hydrochlorate of baryta, tartar emetic, and ioduret of potassium.

I now proceed to the examination of the animal killed by an injection of citric acid into its veins. The first incision shows that the blood is liquid; its colour is very nearly the same as in the glass vessel, in which the proportion of acid employed was somewhat greater. According to my theory we should find a serious lesion in the pulmonary organs, and, in reality, the lung is not in a normal condition; blood and serosity both are effused into it, and the absence of true engorgement depends on the fact of the injection not having been sufficiently concentrated to put a sudden stop to the circulation. Here, again, is a result which perfectly coincides with those already obtained: all of them will, I trust, have a beneficial influence on the ulterior progress of the science and practice of medicine.

LECTURE XVII.

Action of defibrinised blood.—Effects of subcarbonate of soda.—Different degrees of coagulability in the blood of animals.—Action of oil on the blood.—Disease of the heart.—Substances which promote the coagulation of the blood.—Substances which oppose its coagulation.—Direct and indirect effects of cream of tartar.

Gentlemen:—We have a number of animals under experiment, whose condition will aid us in our study of the effects of non-coagulability of the blood; we will pass some of them in review.

Here is a dog, in the first place, who has been let blood to the extent of 450 grammes; immediately after the operation the fibrin was separated with care, and the rest of the fluid reinjected into the veins. Since then the animal has lain on his side, as if his limbs had lost even the share of strength necessary to support his body;

his respiration at once became plaintive, his movements few and difficult, and he appeared to have lost the power of hearing. But, in addition to all this, he passed sanguinolent motions, resembling very closely the alvine dejections characteristic of dysentery. Now, as too short a time has elapsed since the removal of the fibrin to allow of the formation of ulcerations in the intestines, it is more than probable that the blood escapes through the coats of the vessels on the surface of the mucous membrane. May we not legitimately establish some comparison between the intestinal hemorrhage thus occurring in defibrinised dogs, and that observed in individuals labouring under typhoid fever? In both cases the coagulability of the blood is evidently diminished, and we know that when the blood loses the healthy measure of that property it acquires the singular quality of being capable of extravasation by imbibition through the porosities of its tubes.

In this little vessel you may examine a specimen of the defibrinised blood injected into the veins of the animal in question; it has no particular aspect, it becomes red from the contact of the air, just as it would do did it contain its normal proportion of fibrin; in fact it is not distinguishable by its physical characters from ordinary blood; and yet we know that it is unfit for circulation. How utterly absurd, then, to imagine that we can ascertain the absence of some, or predominance of other elements of the blood, by simply breaking up the clot, or stirring the serum with the handle of a spoon!

Here is another dog that was in the enjoyment of the best possible health yesterday; he was gay, lively, and ready to bite every one that came in his way. He is no longer the same animal; he is now dull, dejected, and apparently indifferent to what goes on around him. Whence comes this metamorphosis? From a change in the degree of coagulability of his blood. Ten grammes of subcarbonate of soda, dissolved in half a pound of water, were injected this morning into the jugular; scarcely had the operation been performed when the pulsation of the heart became tumultuous and violent, and the function of respiration laborious; the animal had all the appearance of suffering under the action of a most violent poison. The medical man, in a case like this, would at once set about examining each apparatus, organ, and tissue, in hopes of discovering the starting point of the malady; he finds everything sound, every organ, without exception, perfectly healthy; and he has forgotten nothing in his scrutiny *except the liquids.* Subsequently effusions take place into the pleura, the pulmonary tissue, and on the surface of the intestines; let him now return to his examination, and he will discover that the solids are seriously affected. How is this diversity of conditions to be explained? In one case you discover nothing amiss, in the other very serious disorders; have your researches been defectively made, or the symptoms primitively ill marked? Neither one nor the other; so long as the liquids only were affected, the character of the malady escaped you, because you sought for it where it did not exist. The solids became diseased subsequently; this you ascertain, and you then fancy that the source of all the disorders observed exists in the proper tissue of those solids; against these you direct your treatment, and in doing so you commit a new error as serious as the former; you first mistake the cause, and next apply your remedial measures to an effect.

The animal from which we extracted a considerable proportion of fibrin presents a marked example of the analogy existing between the morbid states producible experimentally, and certain diseases observed in the human subject. He is evidently extremely ill, his pulse is small, his respiration gasping, various rhonchi are audible in the chest, and he has refused to eat, although tried with tempting food; but what I wished especially to point out to you is a sort of eruption which resembles, in no slight degree, the petechiæ of fever, and which, like them, appears to be produced by exhalation of blood into the substance of the skin. The hair has fallen off in the neighbourhood of several joints and at the lateral parts of the abdomen, and in the spots where the integuments are laid bare reddish-brown specks, which do not disappear on pressure of the finger, are perceptible. Whether these are true ecchymoses, or some cutaneous affection, independent of the general state of the economy, I really do not know, but it was worth pointing out to you, in order that we might remember to seek for it in future under similar circumstances. This animal is also affected with the species of purulent ophthalmia to which I have repeatedly invited your attention. The conjunctiva is fungous, puffy, and covered with a greenish film; and the cornea strewed with round, superficial ulcerations, which will inevitably end by perforating that membrane.

Gentlemen, we have by no means exhausted the subject of the coagulation of the blood, and of this I occasionally meet with very evident proofs where they are hardly to be looked for, as the following facts will show:—

Last Sunday I had a hog killed at my country-seat, which had previously been crammed for slaughter; the animal had acquired an enormous bulk, both from general enlargement and deposition of fat. I caused some of its blood to be received into a vessel directly, as it flowed from an opened artery, but though I took every possible precaution to insure success, I failed in producing coagulation; and, as you perceive, this specimen of the animal's blood is still perfectly fluid. It certainly appears more viscid than that of defibrinised animals, but this is the sole point of distinction between the two descriptions of blood; it does not present the slightest trace of the separation into serum and clot, which is always observable in the normal fluid. This fact is, I freely confess, totally at variance with all those we have heretofore observed on the same question. I shall not, for the present, endeavour to strike out any explanation of this difficulty, convinced, as I am, that ulterior experience will give us the true solution of what now appears incomprehensible. Be that as it may, however, the negative phenomenon in question is an extremely striking one; here was an animal in the enjoyment of sound, nay, of exuberant health; he dies of hemorrhage artificially induced, and his blood, though placed in the most favourable conditions for coagulation, does not solidify. I know full well that it might, in strictness, be urged that the animal's blood was not in a normal state; for animals crammed for our tables swallow an immoderate quantity of nutriment, and whatever time they do not employ in eating is spent in sleep. The hog of which I now speak in particular, had grown so stout, that there were absolutely five or six inches deep of fat all over the body; the muscular substance was almost wholly transformed into adipose tissue; so that, although it is in the nature of such animals to be fat, it is impossible to affirm that the condition described was a healthy one. It is even probable that the blood of this animal was modified in composition; that certain principles were wanting and others present in excess; for that of other animals of the same species, similarly fed, tends so powerfully to coagulate, that it is necessary to shake it violently while it flows from the artery, in order to insure its remaining in a liquid state, such as is requisite for its subsequent preparation in various ways.

I fancied that this blood might contain more fatty matter than usual, but on plunging a piece of paper into it you perceive that no indication confirmative of my supposition is obtained. I have, however, ascertained that it produces no change in the colour of turmeric paper, whereas you are aware the serosity of the blood is ordinarily alkaline. Examined under the microscope it presents irregular globules; they are in the main circular, but as their periphery is thickly set with minute prominent points, the whole wears a stellate aspect; there are some white globules also to be seen, which may be presumed to be formed either of fatty matter or coagulated fibrin. I will evaporate a certain quantity of the fluid, and then test it with alcohol and ether, in order to ascertain the real state of the case; this process will show, without fail, whether the blood be or be not combined with fatty matter.

You will not be surprised that I felt anxious to learn if I could produce a similar modification of the blood by artificial means. I, with this view, took some common olive oil, which contains a notable proportion of stearine, one of the constituent principles of fat, and mixed it with arterial blood at the moment of its extraction from the vessels of a living animal. The whole has, as you here perceive, become solid; but the solidification is of a peculiar kind. Judging by the degree of consistence and tremulous appearance of the mass, it must rather be looked on as a kind of jelly, than as a true coagulum; the simple weight of this glass rod suffices to cut through the whole mass. This mixture examined under the microscope, has presented the following appearances:—No globules are to be seen, but, in their stead, particle, small transparent masses, which, without doubt, are composed of a mixture of fat and fibrin, or albumen; there is no stellated aspect here, as in the case of the hog's blood.

This subject meets with further illustration in some observations made on specimens of blood taken from crammed oxen, and sent me by M. Briaune, a distinguished agriculturist. Although this blood was obtained from animals placed in very nearly the same conditions as the hog of which I have been speaking, it is perfectly unlike that we have just examined; it has rapidly separated into two parts, of which the firm and elastic clot lying in this saucer is one. The serum which is not in undue abundance, holds a large proportion of globules in suspension, and these are, to all appearance, regularly formed; the fibrin is abundant, elastic, resisting, and altogether of the most healthy description. The oxen furnishing this blood are fed wholly on the residue of beet-root from which the saccharine matter has been extracted for the manufacture of sugar; whereas, hogs are fattened on vegetable substances containing an abundance of fecula. It is possible, but not to be affirmed in the present state of our knowledge, that the difference of the animals' diet may account for that manifest in their blood. The information possessed on the distinctive characters of the blood in various animals is exceedingly vague; all we know for certain is, that such do exist to a very remarkable extent. Here, for example, is some horse's blood; you see that there is scarcely any serum, but, in its stead, a considerable mass, equalling the red clot in size, of what is called buff. This, which is nothing more than solidified fibrin, is wisely termed the white clot by veterinary practitioners, who are far from attaching the same degree of importance to its appearance as those practising on the human subject; if such blood as this were extracted from one of us, the medical man would declare it to be frightfully inflamed; that we were about to be attacked with pleurisy, or that an intense pneumonia was hatching in our thorax. As respects the influence of forced feeding on the blood, I must confess that until facts, so utterly at variance with each other as those I have adduced, are accounted for, I shall be unable to come to any satisfactory decision.

I have lately had an opportunity of ascertaining a fact which bears upon our inquiries regarding the sounds of the heart and the properties of the blood. The particulars of the case to which I allude have been carefully collected by M. Fauvel, my late house-surgeon, and I intend publishing them so soon as the results of the post-mortem examination are drawn up. For the present I may state to you that the patient, to whose case I advert, was a young girl affected with organic disease of the heart; in addition to hypertrophy of both ventricles, respecting the existence of which no doubt could be entertained, there was, in all probability, superadded a morbid condition of the auriculo-ventricular valves. On applying the ear to the precordial region a very loud sound of expulsion was heard, but the two normal sounds corresponding to the systole and diastole had totally disappeared; there was an evident movement of totality in the thorax dependent on the contractions of the heart. The patient had for the last two or three days been in a dying state; an emphysematous condition of the lung augmented the dyspnœa under which she ordinarily laboured, and, besides this, the proportions of the pectoral cavity were much diminished by the pressure of a large collection of fluid in the peritoneum on the diaphragm; in fact, there was no space for her to breathe in. Such was the state of things when, the day before yesterday, one of my colleagues in the hospital, who saw my patients on that day, prescribed a small bleeding: I say small, because, in truth, six ounces of blood are a mere trifle in the estimation of those who take away twelve or sixteen ounces at once, under the impression that such dyspnœa as that of my patient may be so relieved. The present is not the time to combat this pernicious theory, and I pass at once to the point more immediately interesting us. The blood drawn coagulated almost instantly; there was an abundant share of serum, but the clot retracted in the normal manner. She died yesterday morning, and here is some of her blood (obtained just this moment at the autopsy) which is perfectly liquid; it scarcely presents the least trace of even grumous coagula. In this instance, again, the circumstance of death by asphyxia has liquefied blood which had been the day before perfectly coagulable. This fact lends some support to the notion that there is a line of demarcation to be established between the blood of a living subject and of one who has just expired. You will readily understand that this is a question of vast importance, and well deserving of further investigation.

You have on this board, Gentlemen, lists of the different substances on which we have experimented with reference to their action on the blood, as well as of some others, which I have either examined in your absence, or am about presently to consider in this point of view. The two lists embrace respectively such agents as are capable of rendering the blood liquid, and such as are possessed of the power of promoting its coagulation. You must bear in mind, however, that the arrangement here made is not put forward as a final one; that, on the contrary, it will no doubt be found requisite, from improved experience, to modify it more or less.

Substances which Promote the Coagulation of the Blood.

Water.
Sugared water.
Hydrochlorate of soda.
Hydrochlorate of potassa.
Hydrochlorate of ammonia.
Hydrochlorate of baryta.
Serum of ascites.
Boric acid.
Borax.
Nitrate of silver.
Hydrosulphate of potassa and ammonia.
Seltzer water.
Vichy water.
Seidlitz water.
Ioduret of potassium.
Tartrate of antimony and potassa.
Sulphate of magnesia.
Alcohol.
Cyanuret of gold.
Cyanuret of mercury.
Acetate } of morphia.
Hydrochlorate } of morphia.
Mannite.

Substances which Oppose the Coagulation of the Blood.

Sulphuric acids.	Potassa.
Hydrochloric acids.	Lime.
Nitric acids.	Ammonia.
Tartaric acids.	Carbonates of soda,
Oxalic acids.	Carbonates of potassa,
Citric acids.	Carbonates of ammonia.
Lactic acids.	Nitrates of potassa,
Acetic acids.	Nitrates of lime,
Tannic acids.	Nitrates of strychnia.
Hydrocyanic acids.	Sulphate of morphia.
Soda.	Nicotine.

Among the substances which do not prevent coagulation, you observe I have placed *water*, a fluid which enters in large proportion into the composition of our ordinary drinks. Its possession of this property accounts for a fact which embarrassed us not a little some time past; I mean the small quantity of serum in the blood of the animal submitted to repeated venesection, the fluid withdrawn being on each occasion replaced by an equal volume of water, I fancied the water was retained within the spongy network of the clot; we now see the reason of the fact in the power water possesses of facilitating coagulation.

In the same list are found alcohol and sugared water, which, as it does not affect the globules, supplies us with a means of studying the fibrinous parenchyma of the clot.

Next come the hydrochlorates of soda and ammonia, the serosity effused in a case of ascites, ioduret of potassium, sulphate of magnesia and nitrate of silver; the latter, though it does not prevent coagulation, affects the colour of the fluid in a most singular manner, by converting it into an olive-green. On the other hand, almost all the acids liquefy the blood; I have to-day made numerous trials of them in new proportions. Thus, I stated to you the other day the nature of the action of a single drop of sulphuric acid on a given quantity of blood; and I have since pushed the experiment further. I put fifteen drops of the acid into thirty centilitres of water, and poured four drops of this solution into twenty centilitres more of water, and found that even this liquid, divided into infinitesimal doses, rivalling those of the homœopathists, has, notwithstanding, liquefied the blood and altered the constitution of the globules, although it did not coagulate the albumen.

Hydrocyanic acid is one of our most energetic poisons; it kills instantaneously. The liquid in the vessel I show you results from the mixture of six drops of that acid, five centilitres of water, and one centilitre of blood. The globules and fibrin have disappeared, and such is the alteration in the characters of the fluid, that I am not in the least surprised at the instantaneous destruction of life caused by this substance.

Mineral waters, being consumed to a very large amount, appeared to offer an interesting subject for investigation: I accordingly set about examining some of them, and have obtained the following results:—

Seltzer water, which is most extensively employed, and drunk even at table, furnished, when mixed in the ordinary proportion, a very distinct parenchymatous clot; the globules were not dissolved.

The Enghien waters, coming from a source near Paris, and containing hydrosulphates, have acted on the blood in the same manmer as those salts. And you will remark that this mineral water is not consumed to the same enormous extent as some others; patients are usually unable to bear a higher dose than five or six glasses daily, or to persist in its use longer than a month.

Seidlitz water has not prevented coagulation from taking place; a nebulous clot has formed at the upper part of the vessel into which some of it was poured, and a parenchymatous one at the bottom. Besides this, the scarlet colour of the liquid has not disappeared,—a new and demonstrative proof that atmospheric oxygen is not the only agent capable of giving the blood that colour.

Here is another mineral water—that of Vichy—which is much employed in calculous affections, gout, &c.; it is an alkaline compound, and contains a large proportion of bicarbonate of soda; so large, indeed, that the salt is extracted from the water and sold under the name of "Vichy pastilles." The ordinary dose of this water is six or seven glasses daily. Our first experiments led to a belief that it ought to liquefy the blood, and promote the occurrence of pneumonia; some modification of that opinion is, however, necessary; for the mixture I now show you of five parts of Vichy water, and one of blood, has furnished a clot. The coagulum is, indeed, a remarkable one, from its undulations, which resemble either the motions of a liquid confined within a parenchymatous structure, or the oscillatory movements of certain zoophytes.

Some natural Barrèges water has given rise to the formation of a jelly rather than a coagulum; the globules and fibrin both are completely dissolved, whereas the latter principle only was similarly acted on by the Enghien waters.

Ether, mixed with pure water, dissolves the globules, but does not prevent the formation of a coagulum.

The cyanuret of mercury, which is now employed in preference to the bichloride, in the treatment of certain affections, has not caused any evident modification in the process of clotting.

The cyanuret of gold, a compound which is also medicinally employed, has allowed of the formation of a very delicate nebulous clot.

The chloride of the same metal, to which recourse is habitually had by the practitioners of Montpellier, in the treatment of inveterate syphilis, has acted very energetically here; it has altered the constitution of the globules, and prevented coagulation.

Emetine, a proximate principle obtained from ipecacuanha and other vegetables, and on which I formerly composed an essay in conjunction with M. Pelletier, has thrown down a precipitate; this we must examine under the microscope in order to ascertain if the globules are in an anormal state.

Our next article, which is a most violent poison,—the nitrate of strychnia, has caused most perfect liquefaction.

Nicotine, a proximate principle obtained from tobacco, has caused coagulation, but not without modifying the constitution of the globules.

Mannite has also produced a clot.

The nitrate of bismuth has acted very nearly in the same manner as the nitrate of silver.

The ioduret of iron has totally solidified the mixture of water and blood to which it was added.

Before bringing this section of our subject to a conclusion, I have a word or two further to say respecting the singular action of cream of tartar, a substance which is taken medicinally to the extent of two ounces in a single dose. It is tartrate of potassa rendered soluble by a certain quantity of borax with which it is united. I had a drachm of it, dissolved in twenty centilitres of water, introduced into the veins of an animal; instantaneous death was the consequence, the effect on the blood being the same as in a glass vessel. The incomprehensible part of the business is that the substance in question should be so pernicious when introduced into the veins, whereas, when ingested into the stomach, its effects are scarcely perceptible. It cannot be supposed that the stomach acts chemically on it, for that viscus does not decompose either prussic acid, ether, or a multitude of other substances, which are, on the contrary, absorbed in precisely the same state as they are swallowed; animal and vegetable substances are, it is true, susceptible of decomposition in the stomach,—but here is a mineral compound. Tartar emetic, again, if placed in contact with a wound or the intestinal canal excites vomiting; if introduced into a vein it causes death. We must set about a serious inquiry into the source of these different modes of action of one and the same substances. All we can do for the present is to hazard a conjecture on the subject, and the most plausible one seems to be, that the same substance, when taken into the stomach, produces a less marked effect than when injected into a vein, on account of the slowness of the process of absorption in the first instance. This hypothesis remains to be substantiated or reversed by further experiment.

Here is the body of the animal killed by the injection of cream of tartar into its veins in the proportion stated. The first incision I make shows you that the muscles are completely discoloured; the lung is partly engorged, partly hepatised,—in the first instance the blood is imbibed into the tissue of the organ; in the latter it is additionally solidified. The cause of death was, in this instance, the sudden coagulation of the blood; the promptitude with which the fatal result occurred explains the healthy condition of the intestines.

[M. Magendie terminated the lecture by injecting five centilitres of water, holding two grammes of cream of tartar in solution, into the jugular vein of a dog. The animal did not appear to suffer any inconvenience from the experiment.]

LECTURE XVIII.

Pathological effects of defibrinising the blood.—Post mortem appearances of animals experimented on.—Illustrations drawn from hospital practice.—Case of white swelling of the knee-joint; occurrence of pus in the veins; reflections.—Differences between the pus in the veins and that in the articulation.—The phenomena of disease in the human subject confirm the results of experiments on animals.

Gentlemen:—You are now acquainted with the most important effects produced by defibrination of the blood; with the disorders entailed in the apparatus of the circulation by the removal of the proximate principle which endows that fluid with coagulability. The liquid escapes from its canals, becomes infiltrated into the meshes of the tissues, stagnates, and becomes disorganized, and determines functional disorders of a severity proportional to the extent of defibrination; finally, death ensues.

You see before you several dogs, which have all of them served us for the study of the effects of non-coagulability. Some of them are dead, others exceedingly ill, the remainder scarcely indisposed. Each of them shall receive some slight notice in its turn. Here, in the first place, is the animal I pointed out to you in my last lecture, as being affected with commencing ophthalmia, and as presenting hemorrhagic spots on the skin, where the hair had fallen off. To-day the eyes are in a more advanced stage of the disease; and, what is singular, they are unequally affected; the right is only a little red, and covered with puriform mucus, whereas the left is unfit for exercising vision. The ulcerations I showed you have extended more deeply; perforation of the cornea is distinctly perceptible in two or three places, and hernia of the iris has occurred through the openings. The aqueous humor has disappeared, and a small portion only of the vitreous humor is left behind. Do you suppose that by applying leeches round the orbit we should have succeeded in modifying the purulent secretion, and arresting the progress of disorganization? No: so utterly absurd does the notion appear to me that I would not so much as make the trial; nevertheless the very treatment, which I am unwilling to try on brutes, is daily had recourse to in the case of the human subject.

An individual of a weak and debilitated constitution, who, like many of the lower orders—to which class I suppose him to belong—scarcely ever enjoys a wholesome meal, and dwells in a confined, damp, unhealthy room, where the circulation of the air is extremely defective, is seized with ophthalmia; you find the conjunctiva red; the existence of inflammation is, therefore, as clear as the noonday, and, of course, leeches are instantly applied. But is it not possible that the affection described may be something more than a mere local malady? Is it quite impossible that the blood itself was altered in constitution, and that the injection of the conjunctiva depended on the difficulty encountered by that fluid in moving through its innumerable tubes? Let me recommend you to meditate seriously on these questions. The cutaneous eruption presents the same characters as when we last examined it, but the old spots have increased in size, and new ones made their appearance. Some of these seem to be replaced by ulcerations. I am assured that no such specks existed before the first defibrination was performed, nor can I myself call to mind having observed any; we must not, however, be in a hurry to generalise, as, after all, this may be a merely accidental occurrence. The other symptoms under which the animal labours are those characteristic of the severest forms of fever; dyspnœa, rapid and small pulse, inappetence, bloody stools, general prostration, and tendency to coma; not a single organ appears to be in a normal state.

Next comes another dog, from whom we have extracted a certain quantity of fibrin several times; the amount taken away on each occasion being small. The animal was let blood to the extent of four ounces daily, and the defibrinised fluid instantly reinjected into the veins. At first no symptoms of illness were perceptible, but the animal's strength gradually diminished, his appetite lost its keenness, his respiration became embarrassed, and the various morbid phenomena characteristic of defective coagulability, were distinctly developed. Observe, I beg of you, the condition of his eyes; so far there is nothing but an unusual degree of redness to be seen, and the cornea retains its natural transparence; but ulcerations will shortly appear, and the train of diseased conditions, already adverted to ensue.

Where the total mass of fibrin contained in the blood of an animal has been, in a great measure, removed, the only source whence he can derive the means of supplying its place is the food with which he is supported; the kind of alimentation employed in such cases must, consequently, exercise a very powerful influence on the degree of rapidity with which the animal recovers the fibrin he has lost. It is not a matter of indifference if we feed him with slightly or strongly-azotised substances. I propose to examine comparatively the nutritive properties of various alimentary substances; thus, for example, we may give defibrinised, plain, muscular flesh, and note carefully the time that elapses before the blood recovers the power of coagulating. It seems very natural to suppose that an exclusively animal diet is the best adapted to promote rapid reparation of the fibrin; yet the experiment must be made before we are entitled to affirm anything of the kind. I fed some dogs on the fibrin of the blood of oxen exclusively, and I found that though they were allowed to help themselves as abundantly as they pleased, their health suffered materially from the novel diet. They at first eat with hearty good will, to judge by the consumption of the eatable; but their appetite soon fell away, they became dull, lost their flesh, and all their functions got out of order. I must repeat these experiments, taking the precaution to extract a small quantity of blood from time to time for analysis. If the morbid symptoms were produced by increased coagulability of the blood, it would be curious to compare them with those occasioned by its diminution. These researches may appear futile and insignificant to such persons as deny the correctness of every medical inference, based on the supposition of a morbid condition of the liquids being a possible state; neither their criticisms nor their disdain will prevent me from patiently prosecuting my inquiries, for, in truth, I care very little for either one or the other.

But to return from this digression,—the animal before us was bled before the lecture, in order that we might ascertain the condition of his blood. You see that scarcely any coagulum has formed; in the midst of the serum floats a semi-liquid mass of pseudo-fibrin. I had nearly forgot to point out an interesting appearance, which we also observed in the preceding dog, I mean an eruption resembling forcibly the petechiæ of typhus. There are small red points to be seen, whereon the hair is thinly scattered; several of them, indeed, cover a tolerably large extent of surface. Judging from the colour of these spots, we are authorised in considering them to be constituted by blood extravasated between the layers of the skin, or into the subcutaneous cellular membrane; in fact, nothing more or less than ecchymoses. It is perfectly clear that the modifications occurring in the composition of the fluid are the cause of the symptoms under which the animal suffers, because previously to the performance of the experiment he was in the enjoyment of rude health. But it is not so clear whether the loss of one of its elements, or its having ceased to be coagulable, or those two altered conditions combined, have caused its extravasation.

Theoretically speaking, either supposition is equally tenable; but no difficulty can be felt in coming to a formal decision, if we find that the same morbid phenomena are producible whilst the blood still retains its normal composition. If such be the case, no doubt can possibly be entertained respecting the influence of deficient coagulability; and, in truth, the experiments we had previously made on the introduction of the subcarbonate of soda in solution into the veins were, from their result, sufficient to put an end to our uncertainty. We knew that that fluid had the power of effectually opposing the formation of the coagulum, but also that it possessed no direct poisonous influence. Veterinary surgeons are in the constant habit of injecting purgatives and other energetic substances into the vascular system of horses, and yet those animals suffer no other symptoms than those which follow the ingestion of the same medicines into the stomach. But the case is widely different when a solution of subcarbonate of soda is the injected material; if you desire to be convinced of this look at the animal I now bring forward. Eight days past a small quantity of that salt—ten grammes as nearly as possible—were introduced into his veins, and he, in consequence, fell extremely ill. The heart, lungs, stomach, all the viscera, in short, were simultaneously affected; exhalation of blood took place on all the surfaces, and in the substance of the various parenchymatous structures, as though that fluid had lost all its fibrin. Nevertheless no change had occurred in its constitution; the whole original amount of fibrin still remained in the circulation. Why, then, these extravasations? you will ask; because the liquid had been deprived of the most important of its properties, coagulability. The symptoms observed in this dog manifested a still more dangerous character than those occurring in defibrinised animals, because in it the blood did not, as in the latter, contain a sort of imperfect substitute for the solidifiable principle (pseudo-fibrin). The animal was beginning to recover its strength, and was, in fact, convalescent two days ago; but I then repeated the injection of ten grammes of subcarbonate of soda, and all the former symptoms immediately disappeared. The eyes, which had only been rendered very red and weeping by the first experiment, now present very serious lesions. The cornea has lost its transparency, and is ulcerated superficially in several spots; the alimentary canal is the seat of disorders of the kind attending every general morbid state caused by diminished coagulability of the blood; the stools are watery, and of a reddish-brown colour, as if serum and colouring matter had been thrown out on the surface of the bowels; the functions of the stomach are at a stand-still; there is absolute inappetence; the pulmonary tissue is also most seriously affected. On applying my ear to the thorax I detect various rhonchi, caused by the extravasation of blood into the bronchi and vesicles, and between the capillary tubes.

I opened the jugular in order to learn whether the blood be susceptible of clotting: the clot, we find, is small and friable; in fact, in a condition perfectly harmonising with the symptoms we witness. It is too much altered in constitution to admit of a normal exercise of the functions; it is not sufficiently so to put an end to their play. Hence, whether the fibrin remain in the vessels or not, the effects of non-coagulability are exactly the same. This is an important point, for it shows that when we find the blood of patients affected with fever, to give, on analysis, all its normal elements in their normal proportion, we are not at once entitled to conclude that that blood is fit for circulation. Before we pronounce such an opinion we must also examine the clot; if it be soft and friable, our patient is in a most dangerous condition; if it be completely absent, death is almost inevitable. Such is the degree of precision of which these experimental studies are capable, that we might boldly state this problem:—A given quantity of fibrin being removed, or of subcarbonate of soda being injected into the veins, what will be the nature of the symptoms? There is not one among you, Gentlemen, who would not, with the knowledge he now possesses, be capable of resolving the problem. It is possible, too, that the simple inspection of the clot, when performed by persons thoroughly acquainted with the history of the blood, would suffice to point out, in cases of illness, what organs are affected, and what is the nature of their lesions; the verification of this hint I leave as an interesting subject for your labours.

Whoever makes a discovery is very naturally led to exaggerate its importance; while yet ignorant of the influence it is destined to exercise on the progress of the science to which it refers, he fancies it of a nature to efface the recollection of all researches on the same subject. Gentlemen, I will not suffer myself to fall into this egregious error; I am far from asserting that my experiments on the effects of non-coagulability are of a kind to dissipate completely the cloud of obscurity which, even at the present day, envelopes the pathology of numerous diseases;—that typhus, cholera, the plague, and yellow fever, must henceforth be looked on as so many results of one uniform and identical cause,—the loss on the part of the blood of the property of solidifying. No; this would be a senseless notion, worthy of the quacks who vend their drugs as unfailing panaceas. It is perfectly clear that every disease has its particular origin, symptoms, progress, and termination, all of which are regulated by different laws. The characteristics of the one will be sought for in vain in the other, and your endeavours to refer them to a common type will be fruitless. To maintain that deficient coagulability was, in every instance, the sole morbid element, would be quite as absurd as the doctrine of those who see inflammation of the alimentary canal in every possible ailment. The object of our present experiments is simply to show that the continuance of life and the existence of non-coagulable blood, are incompatible in one and the same person; and that animals who die from this cause offer, both in the symptoms they suffer from, and in the lesions discoverable in their organs after death, numerous points of resemblance to individuals attacked with the malignant kinds of fever. The autopsy of this dog, killed by an injection of thirty grammes of subcarbonate of soda into the veins, will substantiate this assertion. The symptoms during life were considerable dyspnœa, frequent pulse, bloody stools, exhalation of blood from the pituitary membrane, and complete debility. What lesions, think you, shall we find in the body? Without a doubt we shall discover the lungs to be engorged; sanguineous effusion into the pleural cavities, peritoneum, and other serous sacs; bloody infiltration of the mucous membrane of the intestines; and the liver, spleen, and spongy organs redder and heavier than usual. In vain should we endeavour to trace the cause of death to the lesions of a single organ; all have contributed in the ratio of their functional importance to the production of that event. The first time I made an experiment of this kind I was perfectly ignorant of the nature of the disorders it entailed, and I, in consequence, premised, before commencing the autopsy, by confessing my complete ignorance as to the state in which we should find the principal apparatuses. I have, since then, become more bold, almost rash, I would say; I now attempt to declare before-hand the exact alterations of the organs to be discovered on inspection. Let us see to what extent I am justified in my boldness; to find myself in error would be a severe, but at the same time a useful lesson.

The first incisions show that the blood is liquid; this fact alone is sufficient to demonstrate the justness of my conjectures, for, inasmuch as the blood is fluid and unfit to form into a mass, it is evident that wherever there are capillaries extravasation must have taken place. And accordingly, the lung, we find, scarcely collapses on the chest being opened; its surface is of a reddish-brown colour, instead of being rosy; its tissue is firmer, contains less air than in the natural state, and is scarcely or not at all susceptible of dilatation and retraction. The size of the heart is apparently natural, its cavities are contracted and contain no coagula, though some of the grumous masses likened by pathological anatomists to currant jelly, are to be seen within them. The lining membrane of the ventricles and that of the arteries, are deeply coloured with imbibed blood; a condition which, in the opinion of many persons, would indicate endocarditis and arteritis. In the cavities of the pleuræ are effusions of sanguinolent fluid, resembling in anatomical characters the affection termed hemorrhagic pleurisy. The condition of the peritoneum is the same: here, too, is an effusion of the same kind, which gives a red tint to the surface of the abdominal viscera. The latter are evidently diseased: on cutting into the kidney and liver it becomes manifest that those organs are engorged with blood, and the spleen has lost its cellulo-vascular and normal structure; it constitutes a compact, homogeneous mass, resembling the tissue of a sponge that had been macerated for a long time in water. As for the intestines, it is not necessary to slit them up in order to perceive the sanguineous effusions that have formed between their component tunics. The serous covering is raised from place to place by small ecchymoses of variable size, which are sometimes solitary, at others communicating with each other, and appear in some spots to penetrate into the cavity of the peritoneum, rather by imbibition than by rupture. But the mucous membrane is the seat of the best-marked lesions; never was there a *gastro-enteritis* with more characteristic signs pointed out to the notice of the student: here are reddish cylinders interlacing in all variety of modes, which are nothing more than dilated and obstructed vessels; large follicular patches are spread over the whole surface of the bowels, and in every direction we find blood exhaled through the coats of the vessels. Even the corpora cavernosa of the penis are swollen with a notable quantity of fluid blood. Consequently every part of the frame is in a morbid state, because the material agent which establishes a relation between all these parts has ceased to possess its healthy constitution.

There are numerous facts, which on first view appear to have no connection with our present subject, that really belong to it. Of this kind is the following:—About three years past a woman was admitted into my wards affected with a sort of weakness of the left leg; she rather dragged the limb after her than employed it serviceably in walking, and she had frequent falls; subsequently the left arm was seized with similar weakness, and became subject to habitual convulsive movements; the patient, in fact, had the symptoms of chorea. This affection made such rapid progress that the poor sufferer could neither stand nor sleep, such was the constancy of the movements of her limbs. Antispasmodics, tonics, purgatives, and everything, in short, to which curative virtue is ascribed in such cases, failed utterly in procuring her any relief. She left my wards frequently in order to consult various eminent practitioners of Paris, but came back invariably as unrelieved as she had left me. After one of these absences, which lasted somewhat longer than usual, she returned yesterday in the following condition:—The lower two-thirds of the affected thigh are considerably swelled, the swollen part being of the yellow, brownish, and violet hue of ecchymosis: the phenomenon of fluctuation is manifestly to be detected here. Besides this, the tibio-femoral articulation has undergone complete dislocation, the ligaments are relaxed or ruptured, and the bones are no longer in contact; the leg is consequently susceptible of being moved in every possible direction, and this without causing any pain to the patient. It would appear as if the external soft parts were the only bond of union between the two divisions of the limb. I presumed that a collection of serosity must have formed in the situation of the joint, but on making a puncture there I found that nothing but blood escaped; this has been collected in the vessel before you. It presents not a trace of coagulation, but, on examining it under the microscope, I discovered, besides the regular globules, a larger quantity of white globules than I had ever before observed. I have never met with a case of this kind which terminated so unfortunately; death is inevitable, and an operation would, in all probability, hasten that event.

Here is another case in point, communicated to me by M. James. The patient to whom it refers was admitted into the Hôtel-Dieu, under Professor Breschet, for a white swelling of the left knee. The disease made such progress that amputation of the thigh was decided on, and performed by that gentleman. On examining the diseased articulation, it was ascertained that the corresponding surfaces of the tibia, femur, and patella, were affected with caries, and that a development of tubercles had taken place in the spongy substance of those bones. The soft parts were converted into a lardaceous parenchyma, and had lost every trace of their normal organization. Although all the vessels of the stump appeared to have been tied, hemorrhage supervened three hours after the operation, and rendered it necessary to remove the dressings, and apply a ligature to a small artery which furnished the blood. The state of the patient continued to be satisfactory for the first few days after the operation, excepting that the wound made no progress towards cicatrization. There was a constant oozing of sanguinolent matter from the surface of the wound, which tinged the dressings; but not a drop of true pus was formed. Towards the eighth day the left eye became tumid, red, and weeping, though the patient complained of no pain in it. The injection of the conjunctiva gradually increased, until there was true chemosis formed; at the same time the right knee swelled and became painful; fluctuation was here distinctly producible. Towards the close, the right elbow-joint was also attacked; the lesion of the eye grew more and more serious, and the patient finally lost all power of seeing with it. He died the fifteenth day after the operation. The post-mortem inspection showed that, although the amputation had been performed with a due attention to all the rules of the art, the stump was in the form of a cone, the exact reverse of what it should have been, *i. e.*, with its base where its apex ought to have been. No adhesions had formed between the surfaces; the neighbouring flesh was black, softened, and impregnated with blood, and the fluid which exuded therefrom had a most nauseating odour. At the ligatured extremity of the femoral artery was a small soft coagulum, not adherent to, but simply in juxta-position with its walls; the internal surface of the vessel was not more highly coloured than in the natural state. The corresponding vein was filled with pus as far as its union with the hypogastric, at which point a clot existed and appeared to have put a stop to the further progress of the pus. There was an accumulation of pus in the right knee-joint also, but none to be found in either the artery or vein of that side. In the right elbow-joint there were some traces of the same substance. The blood in the heart was liquid, with the exception of a small imperfect clot.

And now, Gentlemen, that I have given you an outline of the chief features of the case, let us rapidly recapitulate them, and see whether they confirm our theories or not. A man is admitted into the hospital with a white swelling of the knee; the limb is amputated; the soft parts are found to be affected with lardaceous degeneration, and tubercles are detected in the spongy tissue of the bones. From the first no tendency to the formation of adhesions is manifested; no exhalation of plastic lymph, in other words of fibrin, takes place, but in its stead, we have a sanguinolent discharge, which announces the non-advancement of cicatrization. These circumstances were in themselves enough to prove that the constitution of the blood had experienced morbid alteration; they were soon confirmed in the evidence they gave by the occurrence of chemosis and the abolition of vision. M. James has brought the eye with him; here it is, its appearance is the same as during life. Now examine it, and tell me, I pray of you, is it not exactly similar in its morbid state to the same organ in the animals we defibrinise? Had our attention not been roused by our physiological researches on pathological changes, we should not have dreamed of ascribing this affection to the condition of the blood; but the theories which flow directly from our scrupulously conducted experiments begin to force their way among the incredulous, and to bear their fruits. In the patient whose case is under consideration, effusions of purulent matter took place into the joints: the same phenomenon occurred in a hydrophobic patient into whose veins we injected a considerable quantity of water, and always follows introduction of serum into the vascular system of animals. Are these occurrences to be ascribed to irritation and inflammation? No; I have too good an opinion of your judgments to suppose for a moment that you could fancy that they were. Here is a fact which could not have been explained by any theory, no matter how ingenious; our experiments alone furnish the explanation of it, which in itself would suffice to demonstrate irrevocably the superiority of the experimental method over all others, if, indeed, that superiority could be for a moment called in question.

Next we find what persons are pleased to call an intense phlebitis, developed in the femoral vein, and occupying its entire extent. Believe them not, Gentlemen; this phlebitis, like all other reveries connected with inflammation, vanishes before the simple examination of facts. In the present instance this affection which, according to the statements of writers, attacks the entire economy, inflaming every obstacle it encounters, was stopped in its progress by a small coagulum formed at the entry of the hypogastric vein. Besides, the presence of pus in the blood suffices in itself alone to show that phlebitis is rather caused by a morbid alteration in the constitution of that liquid than by the inflammation of the vascular tissue itself. That tissue may, it is true, in consequence of prolonged contact with putrid matter, be modified in such manner as to lose all its characteristic properties, and even be destroyed; but this is not the part which it is made to play in the ordinary doctrine of phlebitis; instead of being active, it is, in truth, completely passive.

Microscopical inspection established the existence of a great difference between the pus contained in the femoral vein and in the articulations. In the latter are to be seen agglommerations of blackish and minute globules, with irregular borders, and forming masses of variable sizes; the infinitely minute atoms constituting the globules by their union are also perceptible. When this matter is washed the masses first, and then the globules disappear, leaving nothing behind them except the atoms of which I have just spoken. The pus contained in the vein is slightly red-coloured, because it retains some of the globules of the blood; some globular bodies, of a larger size, and traversed by lines, which form a kind of parenchyma in their tissue, are also to be seen. These points of distinction between the two specimens of pus establish in the clearest manner, in my opinion, that they are not identical, and that the effusions discovered in the joints cannot have been formed by the translation and deposition of that formed in the vein.

The blood contained in the heart was completely liquid, with the exception of one small, soft clot; that in the veins altogether so. The coagula of the arteries of the stump, which should become the bond of union of the divided parts, did not in the least adhere to the walls of the vessels, were soft, and furnished with none of the conditions necessary to arrest the hemorrhage that would have followed the fall of the ligatures; for, on considering the state of the blood, I can have no doubt but that hemorrhage would have come on at that period, and the patient perished of loss of blood, in the same manner as has too often happened after surgical operations.

Here, then, is a case examined in every possible point of view, and furnishing in all its particulars the confirmation of our previous experiments. And what are these theories, thus deriving support from cases taken at hap-hazard? They are those supplied by Nature herself; they are those obtained by examining facts, uninfluenced by preconceived opinions and party doctrines. Were pathological anatomy studied on these principles it would soon be enriched with valuable discoveries, and become a really useful science; but instead of following a plan like this, our hospital physicians content themselves with making a crucial incision through the principal organs, and noting down that such and such lesions affect such and such tissues.

LECTURE XIX.

Substances which affect the coagulability of the blood.—Action of bloodletting.—Pathological appearances of animals.—Experiments to determine the action of gases on the blood: oxygen, nitrogen, carbonic acid, chlorine.—Action of wines.—Do substances act differently on the blood when introduced into the stomach and injected into the veins?—Mode of formation of the buff.

Gentlemen:—It results from our experiments on the coagulability of the blood that the more we remove of the fibrin of that fluid in an animal, the more completely the portion left behind loses the characters of fibrin, and assumes different qualities, which give it the aspect of a new product. The substance so altered is certainly no longer true fibrin, yet it still resembles that principle strongly; so that there has rather been an imperfect transformation effected than a total disappearance of the proximate principle in question. This pseudo-fibrin, for so, you are aware, I have ventured to name the substitute for the real fibrin, does not possess the faculty or property of preventing the blood from becoming extravasated; although it be dissolved in that fluid and circulate with it, morbid exhalations into the substance of the tissues, between the tunics of the intestines, into the parenchyma of the lung, on the surface of mucous membranes, and even into the serous cavities, supervene.

The blood propelled forward by the left ventricle ceases to follow the course of its tubes; it oozes through their walls just as if these were formed with holes like a sieve. We are well acquainted with the anatomical characters of these artificial diseases, and what particularly struck us in them, was their strong resemblance to those met with in certain malignant fevers, designated by ancient authors by the epithet *putrid*. In the first place, let us inquire into the signification of this term. It gives expression to a correct idea, if we are to judge from the advanced decomposition of the fibrinised dog before you. The animal died only yesterday, and he is already in a state of putrefaction. The same phenomenon is observable in what are called typhoid affections; a nauseous smell is exhaled from the body during the life of the individual: the sweat, the pulmonary exhalation, the gases rejected from the stomach, and the alvine evacuations, are all of them distinguished by a characteristic fœtor. Scarcely have a few hours elapsed after death before the putrefactive fermentation sets in in the tissues; the solids lose their cohesion, the liquids their ordinary aspect.

The most extensively affected organ in this animal is the lung; and in like manner it is by no means rare to see the pulmonary circulation become embarrassed during convalescence from acute diseases, and the patient perish of what is called intercurrent pneumonia. Why this complication at the very time that all the functions were reacquiring their healthy characters? The reason of its occurrence is to be found much less in the nature of the preceding disease than in the treatment adopted for the cure of this. There can be no doubt that cases exist in which venesection may do much good, but there are others wherein a single and, *à fortiori*, several bleedings may do serious mischief. The universal practice of the ordinary run of medical men consists in opening a vein whenever the pulse is in the least frequent at the outset of an acute disease. If recovery take place shortly after, they maintain they did well to bleed, for by so doing they prevented the occurrence of inflammation; if the disease follow its course, they congratulate themselves still more warmly on having let the patient blood, and only regret their having drawn too little. It is true, that they who employ venesection according to the formula recently proposed by one of the professors at the school of medicine, will have no occasion to reproach themselves with the latter delinquency; but as the method in question is by no means a new one, it is needless for me to express an opinion concerning it, for experience has long since decided on its value; suffice it to say that it is, at the least, exceedingly doubtful whether it be always the *disease* that is *jugulé*.* But to return: I told you that when a first bleeding fails, a second, a third, a fourth, nay, a still greater number, are frequently had recourse to. These

* The term employed by M. Bouillaud to signify that inflammations are instantaneously arrested in their progress,—an effect which he attributes to his plan of copious bloodlettings, repeated frequently and at short intervals.

repeated bloodlettings not only diminish the mass of blood in circulation, but also alter its constitution. Now, as aqueous drinks, absorbed by the veins, are the sole means wherewith the patient is allowed to replace the blood he has lost, it follows that that fluid loses its proper share of viscidity and coagulability, and acquires, proportionally, a tendency to extravasation. When we observe obstruction of the pulmonary circulation supervene towards the decline of acute affections that have been vigorously treated by bloodletting, it is rational to suppose that the escape of the blood from its vessels is due to its having lost its normal properties. You persist in bleeding, and the severity of the symptoms increases; it is fair to inquire if it be not these bloodlettings that hasten the fatal termination of the cases.

This dog has presented another phenomenon which we had already witnessed, though in an inferior degree, in other animals submitted to similar experiments; I mean an œdematous infiltration of the extremities. While the trunk is remarkable for its meagreness, the limbs are rotund, and the skin investing them tense, just as if they had not participated in the general emaciation; but a few incisions of my bistoury show you how this seeming plumpness is caused; they are followed by a flow of reddish serosity, which had escaped from the blood-vessels into the surrounding cellular membrane. It is not simply transparent serum, for a certain quantity of disorganized globules are mingled with it, giving the whole the appearance of slightly viscous water, holding a few drops of blood in solution.

The same phenomenon is frequently observed towards the close of chronic diseases, and ranks among their most serious symptoms; there is not a nurse in our wards who does not know that death is to be feared when the legs begin to swell. Perhaps their scientific attainments are not so great as to enable them to affirm that in such cases there is atony of the *absorbents*, or superactivity of the *exhalents;* but if they are excusable for their ignorance of such explanations, those medical men are not so, who accept, and above all, who profess them at the present day. It is evident that such œdema depends on the weakened contractions of the ventricle, and on the consequent accumulation of the blood in the capillaries, which leads directly to its extravasation. Besides this, the composition of the blood itself must take a part in inducing its escape from its vessels; here are patients who had for a long time been submitted to more or less complete abstinence from nutritious food, after having first lost by venesection all the blood in their bodies except what was absolutely indispensable for the support of life; the proportional quantity of fibrin must have diminished. Is there no analogy to be established between the history of these patients and that of our defibrinised dogs? I shall continue this inquiry in my next lecture; meanwhile to our experiments on the influence of various agents on coagulation.

Before describing the results of my experiments on the admixture of various gases with the blood, it may be well to remind you in a few words of the essays hitherto made in this direction. Towards the close of the eighteenth century, soon after the separation of the principal constituents of atmospheric air had been effected by the chemists, it was believed for a while that the means of prolonging life to an indefinite period had been discovered. Inasmuch as oxygen, said the chemists, is the vivifying part of the fluid we respire, and enters in the proportion of a fifth into the composition of the atmosphere, the action of the latter will be rendered more and more salutary by the artificial addition of the former element. No sooner was the conception formed than it was put in execution, and you might have seen phthisical, strumous, and cachectic patients generally, inhaling pure oxygen. It was soon found, however, that the results obtained were widely different from those so sanguinely calculated on, and the originators of the plan were obliged to admit its practical inapplicability. The study of the action of gases at that period seems to have been limited to the effects produced on the colouration of blood and on respiration. It was easily ascertained that animals do not breathe better in a medium one-half formed of oxygen, than in the natural atmospheric combination of gases; it was also remarked that other substances possessed the property of giving the blood the scarlet tint characteristic of that contained in the arteries; but, so far as I am aware, no attempt has ever yet been made to learn what influence different aëriform fluids might exercise on the coagulation of the liquid in question. The utility of such an inquiry seems to me so evident that I think it unnecessary to dwell on the points which render it incontestable. Besides, it came as a natural supplement to my previous researches. To proceed at once, then, to the experiments I have performed :—

1st Experiment.—Oxygen.

This glass vessel contains a mixture of pure oxygen and blood, the gas having been introduced into it before the liquid. In proportion as I allow the mercury, which fills the lower part of the vessel, to escape, I observe that a certain quantity of serum accompanies it. It would appear that the serous portion of the blood is, in this instance, subjacent to the clot, which is of a scarlet-red colour, firm, and perfectly coagulated. As was to be expected, the oxygen has been partially absorbed, just as occurs in our respiratory organs; the serum which I have separated from the mercury is slightly coloured in consequence of its having dissolved some of the globules; I shall examine this liquid under the microscope.

2d Experiment.—Oxygen.

In this instance the blood was first introduced into the vessel, next the oxygen. There are about two lines deep of serosity placed underneath the clot, which presents the same appearance as that we have just examined. It would appear to result from these observations that oxygen facilitates coagulation, inasmuch as the part of the fluid in contact with the gas formed sooner, and with greater firmness, into a mass, than that which was far removed from it. Both clots are especially remarkable for their solidity.

3d Experiment.—Nitrogen.

In this instance no absorption of the gas employed has taken place; half of the serum lies above, and half below the clot; the latter has no vermilion tint at the points of contact with the gas. I remark that it is not vermilion-coloured, because arterial and not venous blood has been employed in all these experiments. The clot adheres, as in the other cases, to the inside of the vessel. The result of this experiment would appear to show that nitrogen does not prevent solidification.

4th Experiment.—Carbonic acid.

Here there is a brownish-red colour developed, approaching to black; the serum is reddish, and lies above the clot; the latter is tolerably firm; consequently this gas does not, as has hitherto been maintained, oppose coagulation; the phenomenon has taken place almost as perfectly as in oxygen.

5th Experiment.—Oxide of carbon.

This gas is esteemed of very deleterious nature; it has not been absorbed in the present case. The coagulum, for there is one here, too, is firm, and presents a brilliant surface superiorly, where it is bathed in deep red serum. We must repeat these experiments on living animals, in order to ascertain if the results will then be similar.

6th Experiment.—Quadro-carburet of hydrogen.

This gas, which is that used for lighting, has allowed of the formation of a firm clot; this latter presents a peculiarity which we have not remarked in any of our experiments, namely, a violet colour. The serosity is perfectly limpid, and distinct from the clot.

7th Experiment.—Cyanogen.

Here is a gas which, in various states of combination, is much employed in medical practice; it has been more abundantly absorbed than any other. It is probable that this depends on the alkalinity of the blood, for the gas in question has, as you are aware, a strong tendency to combine with alkalies. The clot is black, solid, very firm, retracted towards its centre, and suspended in the middle of the vessel; this peculiar retraction explains the fact of the serum being subjacent to the rest.

8th Experiment.—Chlorine.

Chlorine has, in the vessel I now show you, acted so energetically, that complete decomposition has resulted in the blood brought in contact with it. These black patches which you observe closely adherent to the sides of the vessel cannot, certainly, be called blood, though I know not how to designate the chemical combination forming them. The result of this experiment points out the temerity of recommending the inhalation of this gas in pulmonary phthisis, chronic catarrh, &c.

I have made a few experiments on the salts of morphia, choosing those most frequently employed in medicine. The hydrochlorate has given the blood a scarlet colour; the sulphate has produced a similar effect; the acetate has tinged the liquid deep brown, and appears to have exercised a particular action on the albumen.

I have, besides, made some essays with the fluids in habitual use as drinks, with the following results:—

1*st Experiment.*—Claret.

This wine, mingled with a small proportion of blood, has not coagulated it, but has allowed of the transudation of a small quantity of serum.

2*d Experiment.*—Ordinary wine.

This fluid has produced a very well marked change in the natural state of the blood; the albumen is agglomerated at the upper, and there is a small clot at the lower part of the vessel.

3*d Experiment.*—Claret and water.

The effect produced in this case is very much the same as in the preceding.

4*th* and 5*th Experiments.*—Beer and cider.

These liquors have precipitated the albumen and prevented the formation of a coagulum.

The action of the different bodies, just passed in review, on the blood, cannot be considered as irrevocably fixed by these experiments; almost all of them will require to be frequently repeated and varied in their mode of performance before we can feel any confidence in the correctness of our conclusions.

I have made a microscopical examination of some of these mixtures, which led to the establishment of the following particulars. I discovered a mass of globules of the ordinary shape in the blood brought into contact with the oxygen, remarkable for the indistinctness of their central point; besides these were to be seen a great quantity of other globules, resulting from the union, I would almost say, the agglutination, of smaller globules, and resembling pus pretty closely; and, finally, some of these same last described bodies, separated from each other and floating in serosity. In order to see them more distinctly, and determine their form accurately, I mixed them with the serosity of another specimen of blood, but to my great astonishment they dissolved in this added fluid and disappeared totally. These lenticular bodies, which are evidently of a distinct species, and had never been previously observed, either by myself, or, to judge from their writings, by other physiologists, appear to me to have been developed under the influence of the oxygen. I am, however, unwilling, of course, to make any affirmation on such scanty evidence: although experience has so far taught us in reference to our present investigations that the phenomena occurring in our glass vessels take place in the same manner in the interior of the animal economy, I think it necessary, in the present instance, to make a further experiment. The one I have in view is this: we will bleed a healthy animal, examine its blood under the microscope, and ascertain the form of its globules; next we will cause the animal to breathe pure oxygen, and then extract, as expeditiously as possible, another sample of blood, with the knowledge that it must have just been in contact with the gas; we shall thus see whether globules of the kind just alluded to, are produced by the cause presumed or not.

In some blood mixed with hydrosulphuric acid gas I found some globules which had undergone no morbid alteration, and, besides, an innumerable host of globular points, half white, half black, as though a line of intersection had separated an ordinary globule into two parts. But the most remarkable point connected with these globules was, that they appeared to execute movements, to oscillate in various directions, with extreme rapidity, describing curved, straight, or irregular lines, just as is done by the microscopic animalcules termed *monads.* This result of the action of sulphuretted hydrogen on the blood is really curious, and deserves to be made the subject of further experiment.

Nitrogen alters in nowise the conformation of the globules, or of the agglomerations of minuter globular bodies already mentioned in the instance of the oxygenated mixture. The actions of these two gases, which may be said to constitute of themselves the air we breathe, (for hydrogen and carbonic acid enter only accidentally into its composition,) would appear to be very closely analogous.

All I was able to detect in the mixture made with cyanogen gas was a mass of globules, among which were numbers possessing the normal characters, and a variety without them as well.

Such are the most remarkable facts elicited by our examination; they are, it is true, not of a very decided character, nor directly applicable to practice, still they are far from being to be despised. They will serve as guides for our ulterior investigations; and these will on the whole, I am persuaded, benefit the science we cultivate.

Allow me now, Gentlemen, to draw your attention again to a question of extreme importance, and one to which no decisive answer has hitherto been obtained. You remember the remarkable fact I particularly noticed at our last meeting but one, namely, that a substance which proves perfectly innocuous when ingested into the stomach, may become deleterious, and even cause death in a few moments, if injected into the veins. The reason of this—this vital problem, as it may be called—appears to me well worthy of being fathomed, and, with a view of doing so, I made several new experiments, the chief results of which you shall learn in a few words. A small quantity of claret injected into the veins of an animal caused almost instantaneous death; half a bottle of this same wine introduced into the stomach of another determined no more serious effect than a state of complete intoxication. Here is the fact we must strive to explain; its consequences in a therapeutical point of view are really immense, and this of itself entitles it to our maturest consideration. I still believe that the explanation I offered the other day was the true one;—that the less powerful effect produced by substances submitted to the action of the stomach, is ascribable to the slowness of absorption. I shall be ready to surrender this opinion the moment proofs of its want of solidity are adduced by any one, but so far I have only met with facts to all appearance confirmative of it. Thus, I injected a small dose of soluble cream of tartar into the vascular system of an animal, and he perished shortly after. On the other hand, I gave a dog two ounces of the same substance, and he was not in the least incommoded thereby. We may account for the difference in the results of these two experiments, by the presence in the one, and the absence in the other, of an abrupt and instantaneous action on the mass of the blood. Let us take another example, and truly they are not difficult to find: an animal swallows ether; the liquid, after a passage of more or less length, according to the dimensions of the œsophagus, reaches the stomach, and is there, like all volatile substances, imbibed or absorbed. In the coats of that viscus it meets with a sort of sieve or filter, which only allows of its passage molecule by molecule; it no doubt passes thence directly into the blood, but its admixture with that fluid is gradual and in the most minute proportions at a time; besides, scarcely has it been in contact with it, when it is borne along to the lungs. I purposely omit to mention the lymphatic vessels; for it would require to be far behind the science, as it exists at the present day, to attribute the principal part in the function of absorption to those vessels; of this some experiments of mine, long since performed, have left not a doubt. But, to return to the ether—in proportion as it enters the respiratory organs, it is in part thrown off from them by the process of pulmonary exhalation. In this case you see why the action of the substance on the blood is so powerless—a small quantity only of it can ever be said to be in contact with that fluid; and, further, it only passes through it, and, as it were, has no time to act. This is the case, too, with all other substances of the volatile or odoriferous classes, such as camphor, phosphorus, &c.

There are other matters which, instead of escaping from the economy by means of pulmonary exhalation, do so by particular emunctories; the kidney appears especially designed for such offices. Thus, sulphate of quinine, hydrocyanate of iron, nitrate of potassa, &c., have been found in the urine after the administration of those salts by the mouth. Absorbed in the intestinal canal they slowly reach the kidneys, and subsequently the bladder, whence they are excreted. I have an animal at present on a diet of "sulphuric lemonade," and will examine its urine to ascertain whether that fluid contains any proportion of the acid consumed as drink, for it is exceedingly important to establish why this substance kills when carried directly into the circulating system, whereas when taken by the stomach it determines no troublesome symptoms, except when its use has been long and immoderately persisted in.

In these different glass vessels we have mixtures of blood and various neutral tartrates. These salts do not appear to impede coagulation; on the contrary, the larger the proportion in which they enter into the mixture, the more solid we find the resulting coagulum.

The subject to which I next propose to direct your attention is one of deep interest, as is testified by the numerous discussions to which it has given rise; I mean the influence of pus on the blood. You are well aware of the nature of the important part which this organic product is made to play by the pathologists of the present day. Observe, then, I beg of you, the results in the vessels before you. Here have been mixed equal volumes of blood and normal pus, and you see that the colour only is changed, for the clot has formed in the most perfect manner. But here, on the other hand, is a mixture of artificial serous pus, water, and blood, which presents a very different appearance; the colouring matter floats on the top, and is partially dissolved in the serosity; the globules are few in number, and there is no coagulum. It is clear, then, that normal pus has not prevented the phenomenon of coagulation from taking place, while the contrary is the case with the serous pus; and you will recollect that it is only when pus occurs in this latter condition that the formidable symptoms ascribed to its absorption are seen to be developed,—that *laudable* pus, to adopt the medical phraseology, never gives rise to them.

In this third vessel, a larger quantity of water has been added along with the pus; no coagulation has occurred, but a greater number of globules are visible. In this fourth vessel, which I now show you, the globules are separated from the remainder of the mixture, which consists of a non-coagulated mass of blood and pus. Hence all these experiments go to corroborate the inferences we drew in the first instance; for still further certainty we will repeat them on the living animal.

I may here mention to you a fact which bears on the question of the passage of liquids into the blood. I injected some natural Barrèges water into the veins of an animal, which proved fatal; but the important point at the present moment is, that a phenomenon occurred perfectly in accordance with the statement I made a few moments ago; his breath blackened a piece of paper previously soaked in a solution of a salt of lead. This would tend to prove that when these hydrosulphurous waters are used medicinally, they partially escape from the system by pulmonary transpiration.

The blood you see here has been taken from the woman suffering from chorea, of whom I have already spoken to you; it has coagulated in a natural manner, and in this fibrinous and normal network you see the result of an admixture of one part of it with nine of sugared water.

We have next to proceed to the autopsy of two animals. The first, whose thorax I now open, underwent an injection of six centilitres of claret, and died forthwith. The lung, you observe, contains extravasated blood, and the probability which this condition of the organ gives of that fluid being in a morbid state, is confirmed on opening the pulmonary artery; its contents are non-coagulated, and resemble currant jelly. As the animal died in a very sudden manner, the lesions productive of death are confined to the respiratory apparatus: no morbid change had time to be effected in the abdomen.

This second animal was killed by the injection of from two to three centilitres of Barrèges water, twice repeated, into the veins: it is to be presumed that his blood also is in a fluid state. The state of the lung and blood, which is even more decidedly morbid than in the last instance, realises my expectations.

Here is a little guinea-pig which was kept for an hour in an atmosphere of hydrogen gas,—a considerable length of time, as it appears to me. However, our previous experiments lead to the belief that no very marked alteration in the qualities of the blood is to be looked for; accordingly you see that the lung is but slightly affected, and that the vessels contain black but well coagulated clots.

I forgot to mention an important fact to you a short while ago; it is this: one of the animals who underwent the injection of Barrèges water survived the experiment, and it was his blood that was used in the experiments with the gases. Now, this blood was altered in constitution, viscous and blackish, and yet, under the influence of oxygen and of nitrogen, it reacquired its scarlet colour. This leads me to think that in cases where a morbid condition of the blood prevails, as in malignant fevers, a useful end might be gained by causing the patients to inhale these gases. I will put the idea in practice in the case of some individuals affected with typhoid fever; there is no small share of plausibility in it, especially when we consider the well-known fact that patients labouring under typhus, yellow fever, &c., should be kept as widely apart as possible, and caused to breathe a pure air, so as to allow the blood to come in contact with the principles that vivify it, and thus, in its turn, to bear heat and life to all the organs.

I cannot bring the section of our subject referring to the coagulation of blood more appropriately to a conclusion, than by instituting some inquiry into the formation of what is called the *buff*. This buff is nothing more, as I have already mentioned, than the coagulable matter of the blood, which has separated from the colouring material and solidified apart. It is a pure and simple coagulum, as is proved by the fact of our being able to produce it at will, as you have seen in the case of the numerous parenchymatous and nebulous clots shown you in these lectures. Observations have, it is true, been made on this subject; the opinion has even been advanced that there is a certain degree of analogy traceable between it and the pia mater,—an essentially vascular membrane: thus, in the buff of the horse's blood there are quasi-organized filaments perceptible. However, it is certain that the fibrin separates in some cases from the general mass of the colouring matter to form the buff, but in others the colouring matter participates in its formation. When this white coagulum is acted on with heat, it is easily seen to be composed of two substances, fibrin and albumen. The former consists of filaments which circumscribe spaces wherein the non-organized albumen is deposited. But having established the nature of its constitution the main point remains to be settled. Now, is its formation to be accounted for? In some instances easily enough: here, for example, in this vessel were placed four centilitres of sugared water and one of blood; the whole was shaken, then allowed to settle, and after a certain lapse of time the white clot you perceive at the upper part formed, as the simple consequence of the difference existing between the specific gravities of the bodies brought in contact. Were the density of the serum modified it would gravitate to the bottom of the vessel and the globules rise to its upper part, just as we see the cream rise to the surface of milk.

But there is a further question to be asked:—Why are some specimens of blood buffy and others not so? To this it seems impossible to me to give an answer with our present amount of knowledge. In no single instance, for example, during the last two months, in which I have caused blood to be drawn in my wards, has this peculiarity made its appearance; where it does occur, there can be no doubt that it depends on the difference of specific gravity of the elements of the blood.

This coagulation of the fibrin occurs under various aspects; in some cases it forms a delicate nebule; in others a clot; in a third set of cases a parenchyma. Medical practitioners have been induced to attribute some importance to the production of these phenomena, because it has been fancied that relations exist between them and certain diseases. I will discuss the reality of these assumed relations in my next lecture; all I wish to lay down for the present is, that a notable difference exists in the density of the globules and of the fibrin, a fact which is easily understood, inasmuch as it is certain that globules calcined and reduced to ashes contain about from five to seven per cent. of iron.

LECTURE XX.

Pathology of typhus fever.—Neglect of the conditions of the blood by pathologists.—Passage of mercury through the capillaries.—Typhoid symptoms in a defibrinised animal.—Theory and causes of the buff.—M. Piorry's theory —Influence of respiration on the coagulation of the blood.

GENTLEMEN:—You are aware that a prize was some time past offered by the Institute for the best essay on the subject of continued fevers. Eighteen candidates have entered the lists, but I must confess that I have felt both astonishment and grief at the manner in which they have treated the question; hardly more than two or three make the least allusion to the state of the fluids, or venture on a suspicion of the possibility of their being morbidly affected. In every one of their memoirs we have a profusion of admirable anatomical descriptions; the colour, the consistence, and the size of the organs are carefully noted; the ulcerated follicles and patches of the intestine, and the points of eruption on the skin, are all counted with marvellous exactitude and patience; but not a syllable are we told respecting the condition of the blood. Again, the symptoms are successively passed in review, their degrees of gravity appreciated with the help of numerous facts, and a multitude of different treatments described for each.

I may here remark, that you may shrewdly suspect diseases, for which such various modes of treatment are recommended, to be really dangerous or difficult to be cured; nothing so difficult to cure as that which everything cures. Next comes, in its turn, a long discussion on bleeding; especially on the extent to which that mode of treatment may be carried with propriety. Whole chapters are devoted to the examination of the effects produced by the removal of blood, but not a word is said regarding the physical and chemical properties of that liquid. I am far from contesting the advantages to be derived from the anatomical study of disease, (though, truth to say, we have not yet gained much additional knowledge of their proper mode of treatment thereby,) but in order to trace back disorders to their causes, it is not enough to examine their material consequences.

The loss of its coagulability, Gentlemen, is not the only change in its constitution capable of causing its stagnation in the capillary system. Every possible degree of disproportion in size between the liquid and the tubes it has to traverse, inevitably induces mechanical obstructions to its passage and extravasations. Mercury, by reason of its fluidity, is one of the fittest substances for injection, yet its particles cannot, in the living subject, force their way through the ultimate ramifications of the blood-vessels. The force with which the operation is performed on the dead subject in our dissecting rooms triumphs over the obstacles encountered, but either the walls of the vessels are ruptured or excessively dilated; the same effect may be produced, though in a less degree, by the irregular action of the heart; but in many cases no rupture takes place, and the column of liquid remains motionless, because the injected metal prevents its advance. Mercury may be introduced with impunity into the stomach, because it enters the veins globule by globule; it may, in the same way, be absorbed after frictions externally, because it has been previously triturated along with some fatty material, and it is obliged to pass through the epidermis before it reaches the vascular rete of the chorion. On the contrary, in every case where it is directly injected into the circulation, and not, as it were, sifted, by passing through our membranes, it is found not to be sufficiently minutely divided; the globules unite in masses infinitely too large for the ducts through which their passage would have to be accomplished. The capillaries become immediately blocked up, all motion of liquids in their interior ceases, and if the functions of the organ on which the experiment is made be indispensable to life, the animal dies.

This is exactly what has happened in the case of the animal now laid on the table. Two hundred grammes of mercury were injected into the right primitive carotid, which instantly brought on all the symptoms characteristic of an *ictus sanguinis*, as it has been called; these proved fatal in a few minutes. Let us begin the autopsy by the skull; you observe that as I divide the integuments investing that cavity, minute drops of mercury escape between the lips of the wound, a fact which, by the way, shows the facility of anastomotic communication between the two external carotids. The metal was only injected into one of them, and yet it has reached the branches of both with equal ease. I now raise the bone and dura mater, and disclose to your view a very beautiful appearance of the pia mater. It resembles a silvery pellicle, spread over the nervous substance, following its various undulations, investing its eminences, and sinking into its depressions. The brain and cerebellum are invested on all sides by a rete formed by the interlacement of the various minute canals into which the injection has penetrated. But the column of mercury seems to terminate abruptly in some points, where, no doubt, the globules of the metal were too large to effect any further passage. The remark I made respecting the anastomoses on the exterior of the skull is quite as applicable here; such is the uniformity of distribution of the injected fluid, that it would be impossible to tell, by the simple inspection of the vessels of the pia mater, whether the metal was pushed into the right or left carotid.

Phenomena such as these incline one to ask, whether it be possible for the blood of the living subject to undergo morbid alteration of such a kind as to contain corpuscula incapable, from their size, of circulating through the vessels. In spite of the continual changes this liquid undergoes by the reception of new materials and the rejection of some of the old ones, it is difficult to conceive its admitting substances out of proportion, in point of size, with the tubes through which it moves, and for this reason these substances, before reaching the blood, must pass through porous membranes. Now the pores which must thus be traversed are so minute, that their diameter can scarcely equal that of the capillaries, and consequently it appears to me to be improbable that corpuscula of very large size, in comparison with those tubes, can directly enter the circulation.

But, on the other hand, here is a circumstance which may very easily come to pass; some chemical agent or other is absorbed, enters the circulation, and modifies the globules, or some one of the elements of the blood. A chemical process commences, and granulations are deposited in the interior of the minute vessels, block it up, and arrest the motion of the column of blood. It is in this manner that a concentrated acid injected into the stomach causes death, by coagulating the albumen of the blood, and obstructing the flow of that fluid mechanically.

I now present you one of the defibrinised animals under experiment, and which a few days past laboured under all the symptoms of purulent ophthalmia, redness and tumefaction of the conjunctiva, commencing opacity of the cornea, and discharge of tears mingled with pus. The condition of the different organic functions was indicative of a typhoid state of the system. The animal was put on a nourishing diet; strengthening food was given him in sufficient quantity, and, as you see, he has undergone a complete metamorphosis. He was at the period I have adverted to in a complete state of prostration; he is now gay and full of life. His eyes are quite cured, the cornea has recovered its natural polish and transparence, except in one or two spots, where you perceive specks indicating the site of the ulcerations that had commenced to form.

I called your attention some days ago to a curious appearance observed in several animals deprived of part of their fibrin, namely, the formation of ecchymoses and ulcerations of the skin; being at the time doubtful whether they were to be attributed to the altered constitution of the blood, or to a cutaneous disease in existence previous to our experiment, I determined to examine the next animal carefully before withdrawing its fibrin. This I have done in the case of the greyhound bitch I now show you. Several bloodlettings have been practised on her on different occasions, and in each instance the defibrinised blood re-injected into the veins. All the morbid phenomena usually developed in such cases (and with these you are perfectly well acquainted) have occurred in this animal, but besides these the skin, which was perfectly healthy before the operations performed on her had been commenced, presents a peculiar morbid condition, which I am glad to have an opportunity of showing you. When the animal stands erect you observe ulcerated patches of variable sizes, here and there, on the surface of its body, while the hair and integuments are perfectly natural in the intervals. It seems difficult at first to account for the position occupied by the ulcerations; but by making the animal lie down we soon discover that these are limited to the spots which come in contact with the ground. The right side is alone affected, because the animal constantly lay on it; and the largest patches are seen on the forearm and the prominent parts of the pelvis, because those parts most generally support the weight of the body. The similarity of these phenomena to those observed towards the close of some diseases in the human subject is most striking. The patients are reduced to a state of emaciation, and the pulse is small and weak, for the heart participates in the general debility, and its contractions scarcely possess the force required to set the columns of blood in motion. The dorsal decubitus is the least fatiguing attitude under such circumstances, and on the back generally, and on the sacrum in particular, the greatest amount of pressure consequently bears. The weakness of the patient condemns him to a state of complete immobility; the capillary circulation becomes mechanically suspended in the compressed parts, the integuments redden and ulcerate, and eschars form. Observe, too, that the ulcerations occur at the same period of their disease in the defibrinised dog and the typhoid patient; and in one and the other those spots only are affected where the passage of the blood into the capillaries is rendered impossible in consequence of mechanical compression. Is no inference to be drawn from such facts as these?

You will remember, Gentlemen, that a few days past I showed you a remarkable case of purulent ophthalmia in a subject whose blood was evidently in a morbid state. M. James, through whose means I was enabled to communicate the particulars of it to you, fancied that he had remarked a similar condition in an old man in M. Breschet's wards. This individual, who had been admitted with a violent contusion of the leg, accompanied with denudation of the tibia, fell in a few days into a state of complete debility; constantly lying on the back brought on the formation of a large eschar over the sacrum, and it is probable that he cannot long survive. Be that as it will, I have myself been to see him, and ascertained that the cornea is softened in such manner that it must soon undergo perforation. However, I must inform you that the state of the blood is by no means such as the disorders he suffers under would lead us to expect; that fluid is, on the contrary, perfectly coagulable, and has furnished the hard and resisting clot I now show you. The explanation of this singular fact I am at present unable even to guess; we must, therefore, content ourselves with noting the fact, with the intention of availing ourselves of it at some other time.

I taught you in my last lecture that the yellowish stratum which accidentally solidifies at the upper part of the coagulum, consists of fibrin separated from the globules, and that a physical agency perfectly independent of inflammation, namely, specific gravity, is the cause of this abnormal super-position of the fibrin. You also learned that this buff, which is only an occasional appearance in human blood, occurs almost constantly in that of the horse, and there ordinarily forms two-thirds of the total mass of the clot. I also showed you that we can produce at will this separation of the fibrin, by mixing sugar in solution with the blood, whereby the precipitation of the globules is promoted. You have seen numerous examples of this in the nebulous and other clots which I have had occasion to lay before you since the commencement of the present session. The main point which now remains to be determined is, why it should make its appearance in some cases and not in others; but before entering upon this question, for which, in truth, I scarcely hope to find an answer, I shall mention a fact I had forgotten to speak of before. In cases where a buffy clot is about to form, some yellowish serum is observed to accumulate on the surface of the blood; this is especially remarkable in the instance of the horse. Now, if this serum be removed, it will coagulate like the fibrin itself; and the same process will go on until all the fibrin of the blood has coagulated. I had remarked this fact, which struck me not a little, the first, as I had fancied; but it appears that Dr. John Davy noticed it before me, in his researches on the coagulation of the blood.

The practical question to which the subject of the buff leads is, whether we can rationally deduce any consequences from its presence. It has long been the invariable habit of authors of treatises on practical medicine to recommend attentive examination of the blood; and their instructions are generally followed. But the manner in which the task is performed is extremely superficial, and yet, from the information it furnishes, the necessity of a second bleeding is very often inferred. For my part, I have, for a number of years, paid no attention to this phenomenon than such as my curiosity to become acquainted with its cause has required; truth to say, I bleed my patients but little, and I do not perceive that they fare a whit worse than those of my neighbours. Nevertheless, this same buff has excited, and still continues to excite, laborious research on the part of medical men; and conscientious observers have evinced a zeal and sagacity in its study worthy of a better cause, at least to judge by the backward state in which our knowledge respecting it still is. M. Piorry, whose indefatigable ardour is really worthy of admiration, has of late published some researches on what he calls diseases of the blood, wherein the buff plays an active part in the production of morbid phenomena; to such a degree that the author proposes to range the condition producing it with the immense class of affections the titles of which terminate in *itis*, and, in short, to term this state of the blood an *hæmitis*. But, Gentlemen, I would ask you how can we draw such a conclusion as this, or any other inference of importance from the presence of the buff, when, as all the world knows, its formation depends on various circumstances that have no manner of connection with disease? Thus, if the opening in the vein be too small, or its parallelism with that in the integuments imperfect, or if a globule of fat interfere with the flow of the liquid, and cause the blood to trickle away, it is certain that no buff will form. But open the same vessel largely, and receive the blood into a narrow and deep vessel, and on the following day you will find the results in the two cases widely different. Now, by this simple exposition of facts, the question, in so far as regards the importance attributed to it in pathology, appears to be solved. What consequence can you attach to the appearance of a condition which extraneous agencies influence so materially. For, either it constitutes a pathognomonic sign, (and if so it ought to occur in all similar cases, which is not found to be the fact,) or it is a merely adventitious formation, without practical signification, and one which only deserves to be noted for form sake, far from being made the foundation of the course pursued in the treatment. But if medical men will cling to the opinion they have so long maintained, let them at least be consistent, and apply their measures of treatment to the vessel in which the blood is received; for it is this that modifies the coagulation of the blood and produces the morbid appearance so terribly dreaded; let them change its shape, and then they will have employed a much more infallible remedy for the evil they fear than bloodlettings repeated ever so frequently. But no; they bleed, because the *buff* is an *inflammatory* phenomenon; they bleed again to cause its disappearance, and, in truth, they are ordinarily successful in this respect after having let blood a certain number of times in succession, say three, four, five, six, or more. But that it should thus disappear is not in the least to be wondered at; there are two reasons why it should so happen: either the patient is worn out and exhausted by the frequent losses of blood he has undergone, and his impoverished fluids, deprived in a great measure of their fibrin, (in the manner our experiments have so satisfactorily demonstrated,) are incapable of furnishing any more of that principle for separation; or, if the individual be robust and plethoric, and his blood have resisted, in a measure, the means of effecting its decomposition employed with so much hardihood, all this proves is, that the last venesection was performed under conditions such as those I have just described as being favourable to the production of the phenomenon in question. Now I would seriously ask of you, is it upon such empty theories as these that men of sense and talent, men guided by the desire to be useful to their fellow-sufferers, and to raise their art to the highest rank; is it upon visionary notions, such as those I have been speaking of, that they should found their therapeutical doctrines? And here is a fact of no trifling import, more especially as it comes from a gentleman who is to a certain extent our opponent on the present question: M. Piorry has announced that in sixty-three bleedings, performed on pleuritic patients, under the most favourable conditions possible, the buff failed to make its appearance sixteen times.

You see, Gentlemen, how difficult it is to eradicate the most absurd prejudices; in spite of the evidence of our experiments men continue to maintain that the buff is the source and origin of inflammations,—in spite of their seeing us develop at will these same inflammations by depriving the blood of this same buff. You know that in each instance, where we removed a part of its fibrin from an animal by bleeding, or where we injected divers substances into the circulating system, you invariably saw the precise morbid disorders and lesions, affecting the same organs and assuming the same forms, so often seen in our hospital autopsies, produced at the exact hour foretold at the time of the experiment. And in defiance of all the information thus acquired, you would bleed in order to combat the ridiculous bugbear of pathologists, and, although you are aware that it is developed under every condition of the system, both in health and disease! But, you will say, must we then prohibit venesection in pleurisy, in pneumonia, &c.; and if we refuse to employ it in such cases, what treatment is to be adopted in its room? Gentlemen, I will state to you with fidelity my convictions on this point. If bleeding be prescribed *because* the blood is buffy, I say that they who so prescribe it act in defiance of facts, and hence I utterly reject, on this score, the propriety of its employment. But if bleeding be advised, because it relieves the patient, diminishes the oppression he feels, soothes his pain; and, finally, because patients habitually recover by, or rather after the use of this remedial agent, then, empiric as I am, I admit that we are justified in having recourse to it: nevertheless, I must, at the same time, declare that I cannot conscientiously affirm, in the majority of cases, that the malady would not have gone through its periods, and reached a fortunate termination, had venesection not been employed. And my doubts on this head are strengthened by the fact, that if, instead of weakening your patient, you support his physical and moral strength, and, watching the disease closely in all its phases, promote the occurrence of favourable crises, and assist Nature (by directing abstinence from solids, and the use of diluents) in overcoming the obstacles she encounters, you frequently see rapid recoveries occur, more rapid even than those witnessed as the sequent of abundant and repeated bloodletting. The methods of treatment, with which we are now acquainted, are unfit to fulfil such indications as those I have enumerated; this I am well aware of, and, indeed, in the present state of things, I am, as I have more than once declared, persuaded that it is wiser to stand still, and do nothing, than act, as we must do so often, under the apprehension of possibly increasing the violence of the disorder. For you must remember that the treatment by bloodletting, employed in almost every case of acute disease, but especially in those I have adverted to, is one of the means of inducing those very diseases in healthy animals. Bleeding lessens the quantity of fibrin, proportionally increases that of the serum, and weakens the energy of coagulation; and you are aware that whatever interferes with the coagulability of the blood—its most important quality—manifests itself by morbid alterations in the organs, whence, in their turn, result a variety of serious general affections. Upon this point I feel, Gentlemen, that I cannot address you too impressively, for the vast deductions it directly furnishes are of a kind to operate a most useful change in the theory and practice of our art.

But to return to the buff: this substance sometimes assumes particular appearances, of which M. Piorry has described a new species, distinguished by being granular, resembling an unequal irregular membrane, and by having its surface raised into eminences, as though it were tubercular. This author conceives that where such characters are observed in the buff, they announce that absorption of pus has taken place, and he consequently terms this abnormal condition of the fibrin *piohæmia*. Remark, however, that the absorption of pus by the blood-vessels is, in the first place, anything but a demonstrated fact; but, as I trust to have an opportunity of treating this subject with all the detail it requires at a future period, let it suffice for me now to say that my researches so far induce me strongly to believe that, when pus is found in the blood, it has been formed therein by some process or other, the nature of which is no better understood than that whereby it is produced externally to the vessels. But I am induced to reject the opinion advocated by M. Piorry, because the little yellowish masses, which, it is true, have a purulent appearance to the naked eye, present no globules of pus when examined under the microscope,—globules which, as I have already stated to you, are easily distinguishable from those of the blood by their much greater size and by other characters. Nothing is to be seen in the tubercles of the buff in question but a kind of filament, of which there are vast numbers, crossing each other in different directions, and thus bounding spaces in exactly the same manner as we have observed in the cellulo-fibrinous networks frequently shown you. I pushed the inquiry further, by trying if we could produce this *purulent buff* in animals. No plan could be certainly better calculated for insuring success, if such were to be obtained at all, than examining the blood of an animal after an injection of pus into the circulation had been practised: now, I have not, in any instance where this was done, met with the piohæmic coagulation of M. Piorry. I am far from denying the fact itself, but I think that the explanation offered of it is not well founded; I therefore propose fully investigating the question by-and-by.

In fine, Gentlemen, it results from the inquiry we have instituted into the formation of the so-called inflammatory buff, that it is impossible to tell, in all instances, why it should or should not make its appearance. In addition, however, to the circumstances whereby it is evidently influenced, and which I have already made known to you, it would appear that the intimate composition of the blood modifies its characters, according as itself is affected. In illustration of this, here is a mixture of natural Marienbad water and blood in which an undulating clot has formed, resembling those I have already laid before you, in other respects as well as in the oscillations propagated little by little over the entire mass: these, as you plainly see here, give the whole the appearance of a living animal. Here the globules remain attached to the fibrin, and are, as it were, incorporated with it,—a condition which must be attributed to this water and the salts it contains, because, had we employed an aqueous solution of sugar instead, the fibrin would have coagulated in the form of a buff, and the globules would have been precipitated. I varied the proportion of Marienbad water, but in every instance found its effects to be the same.

The different clots produced in our experiments (I keep them carefully in the open air in order to ascertain the nature of the changes effected by time) have presented a curious phenomenon, which will possibly guide us to the discovery of the means employed by nature in the formation of cicatrices. A stratum composed of two laminæ has formed at the upper part of every one of them. One of them is moist, and adheres inferiorly to the coagulum; the other, free superiorly, has become dry from the contact of the air, and prevents the evaporation of the fluid part of the blood. This stratum, which pretty accurately resembles a mushroom in point of form, glued as it is to the sides of the vessel, at the points where it came in contact with them, is analogous, as it appears to me, to that formed by snails to close the entry of their shell hermetically, and so preserve themselves from the action of the air. As occurring in wounds it may be assimilated to the epidermic matter which, according to certain anatomists, is the product of a particular secretion, created for the circumstances, and suspended when the necessity for it ceases to be felt. This fact is worthy of your attention; this is the instrument whereby union by first intention is effected, whereby the adhesion of the divided extremities of the capillaries is brought about, and the circulation kept up,—all of which could not be the case if the extremities of the divided vessels were left in free contact with the air.

I shall conclude this lecture with an experiment connected with the question of the coagulation of the blood. So far almost all the gases we have tried have promoted the occurrence of that phenomenon, especially oxygen, nitrogen, and cyanogen. I have commenced the trials I spoke of to you by making one of my patients in the Hôtel-Dieu, affected with typhoid fever, respire the former of those gases; from this first case, however, as it was a very slight one, no conclusion of any kind can be drawn. I must wait until some more cases are admitted in order to put my notion to the necessary test; meanwhile I should wish to ascertain if the act of respiration itself causes a variation in the degree of coagulability of the blood. I shall, therefore, repeat an experiment made by Bichat, which consists in fitting a cock to the trachea, opening the carotid on one side, and examining the changes which the blood undergoes according as the animal is allowed to breathe freely or otherwise. In the present case, however, our chief object is not to witness the change in the colour of the liquid, but to learn whether it does or does not, at the same time as it loses its scarlet colour, lose in some degree the property of solidifying. This, however, will not be a decisive experiment as regards the latter point, for death must occur before the animal could have remained sufficiently long without air. However, circumstances exist under which black blood circulates in the arteries: this is especially the case when the orifice of communication between the auricles is not completely closed after birth, whereby the affection termed cyanosis is produced. We are also acquainted with the influence of the eighth pair on the blood, and shall now add to the facts we have acquired those discovered by the present experiment.

The carotid and trachea have been laid bare beforehand; I now make an incision in the latter parallel with the cartilaginous rings composing it, and introduce a cock into the orifice so-made. Observe, Gentlemen, that the trachea possesses not the slightest share of sensibility; the animal did not evince the least sign of suffering when I divided it. I open the carotid now, and you see that the blood escaping from it is of scarlet colour—in fact, normal. I turn the cock; the animal begins to make violent and ineffectual efforts to breathe, and you will remark that the blood gradually assumes a darker tint; it has now become almost completely venous, although it flows from an artery. I receive a few drops into a glass, as I did of the scarlet-coloured fluid also. I open the cock, and the blood reacquires the red tint almost instantaneously; you will notice that it recovers the scarlet colour with greater facility than it loses it.

The state of asphyxia was kept up for two minutes; I now tie the artery, and remove the cock. The different samples of blood just obtained have all of them coagulated. Consequently, just as I had foreseen would be the case, the limited duration of the experiment prevents us from being able to draw any conclusion as to the influence of the act of respiration on the coagulation of the blood.

LECTURE XXI.

Causes which modify the course of the blood in the capillaries.—Theory of gangrene.—Gangrene from obstructed circulation; illustrative experiments.—Action of the ergot of rye.—Disease of the aorta.—Pathology of dysentery.—Action of pus on the blood.—The albumen and its properties.—Differences between albumen and fibrin.—Action of different substances on albumen.

Gentlemen:—Mechanical obstructions in the blood-vessels, no matter of what kind they be, cause the cessation of the circulation; and according as their action is temporary or permanent the blood resumes its course after a time, or its movement is completely suspended, and various disorders, sometimes mortification, follow. Many causes are capable of modifying the course of the blood in the capillaries; sometimes the particles of matter suspended in the fluid are too large for the diameter of the tubes; in other instances the fluid itself is too viscous: in others a tumour developed among the tissues presses on the vessels, and so impedes the movement of the blood. In a few moments after the blood has ceased to circulate in a limb, that limb dies, and putrefaction commences. The total extinction of life in a soft part causes the cessation of all its organic phenomena,—a condition termed *sphacelus* by pathologists. The mortification of a part is generally recognised by the total privation of motility, sensibility, and natural heat, by the disengagement of putrid gases, and by an alteration in consistence and colour. According as this or that symptom predominates, the morbid state is made to fill a particular place in nosological classifications: thus, the *dry* and *humid* gangrene of nosologists differ solely in the quantity of fluid present in the affected part. It is probable that the first species originates more particularly in some obstruction to the arterial circulation, whereby the passage of the blood through the capillaries is prevented; while the second would appear to be principally brought about by venous obstruction and consequent non-return of the blood to the heart. Our forefathers attached great importance to the accurate distinction of cases of dry and humid gangrene; but, as they neglected the study of causes, and directed all their attention to results, the history of these affections is as yet but little advanced. You may ascertain, certainly, that in the one species the eschars are corrugated, hard, and, as it were, mummified, and that in the other they are tumid, soft, and friable; but you will not from this knowledge derive any positive indications as to the treatment best fitted for each case. It is of much less importance to analyse the phenomena that follow gangrene, than those that precede it; the former belong to dead, the latter to living nature.

Some pathologists never see anything in gangrene but a termination of inflammation, and as they call every species of disordered condition occurring in the circulating system an inflammation, their opinion is not devoid of a certain share of plausibility. But the composition of the liquids is a point made of no account by them; the grand affair is to trace out the influence of the inflammatory element. When they find a part of a bright red colour, sensibly hotter than natural, and at the same time the seat of acute pain, they tell us that the violence of the inflammation has exhausted the organic forces, which, in consequence, react ineffectually against the process of disorganization. When, on the other hand, a limb becomes bluish and livid, without having presented the precursory signs of inflammation, they, notwithstanding, affirm that this inflammation existed, but assume that it was of a *malignant* kind, or that the vitality of the tissues was diminished before its outbreak. In one case too much fluid reaches a part, and we have one form of inflammation; in another case too little is conveyed thereto, constituting a second form of inflammation. The quantity of fluid is everything in the production of the morbid phenomena: its qualities are a matter of perfect indifference. Let us see whether this view of the question be the true one.

I injected a drachm of varnish, holding some sifted porphyrised animal charcoal in suspension, into the femoral artery of this dog. The experiment was performed in the morning of the day before yesterday, and the paw is already cold, motionless, and insensible; the animal does not lean on it when he walks, and gives no sign of suffering when I pinch the part, or drive the point of a bistoury into it. The limb is swollen below the ligature of the vessel, and the skin distended by the gases exhaled in the midst of the sphacelated tissues. To their presence is due the particular sensation experienced on pressing the integuments, a sensation which instantly reminds one of the crepitation produced by the accidental entry of air into the cellular tissue. The hair of the limb has in part fallen off, and from place to place the epidermis is raised by an accumulation of brownish liquid, such as occurs after a scald. These phlyctinæ are caused by the extravasation of some of the materials of the blood, which, stagnating in its canals, putrefied therein, and escaped through the pores of their coats. This animal consequently presents all the characters of confirmed gangrene.

Why, we may next inquire, has the introduction of a little varnish and charcoal dust into the vascular system thus determined the mortification of an entire limb? Because the capillary system, which establishes the communication between the arteries and veins of that limb, was mechanically obstructed. Our injection made its way through the trunk and principal divisions of the artery into which we drove it, but when it reached the capillaries it could not get on any further. Charcoal dust, no matter how minutely pulverised it may be, cannot pass through such delicate tubes as those through which the globules of the blood make their way with facility. The blood contained in the arteries and veins being consequently deprived of its natural movement, putrefies, and its decomposition entails that of the neighbouring solids. Life ceases, then, simply because the circulation is stopped through local capillary obstruction.

It might be urged, in objection to this reasoning, that as the femoral artery was tied, the gangrene of the thigh was caused by the ligature of that vessel, and not by the nature of the injected liquid. But on reflecting on the numerous communications which exist between the different parts of the vascular system in the dog, one feels convinced that the mortification of the member cannot have originated thus. Not a day passes without our tying the femoral artery either above or below the origin of the profunda, and yet we never observe the supervention of gangrene. The anastomoses between the arteries of the thigh and pelvis are so extensive that the ligature of the femoral scarcely slackens the circulation, and even this only momentarily: I have even tied the abdominal aorta in these animals, and they have continued to live, without gangrene occurring in any part of their bodies.

Stoppage of the circulation may result from weakening of the contractile power of the heart. In the cholera every observer was struck with the bluish and livid hue of the integuments, the fall of the temperature of the body, and the disappearance, more or less complete, of the cardiac sounds. These symptoms resulted from the want of due energy in the heart, and the consequent accumulation of blood in the superficial veins.

Here is another animal who yesterday underwent an injection of potato-fecula reduced to an exceedingly fine powder; nevertheless, its granules were still more voluminous than the globules of the blood. The vessel operated on in this instance was the carotid, and instead of introducing the fluid in the direction of the current of the circulation, that is, towards the encephalon, I turned the point of the syringe towards the heart, so as to prevent the cerebral circulation from being especially affected. The amylaceous matter was consequently distributed pretty uniformly through the whole arterial system; the passage of the blood became difficult in every point, but not sufficiently so in any one to cause an interruption in the functions of the different viscera. The animal now labours under fever, dyspnœa, cough, inappetence, diarrhœa, prostration, in a word, under all the signs of a general affection of the economy. I shall take care to have the changes in his condition watched: possibly the disease will become concentrated in some particular spot, and if so, this will no doubt be the lung, or some other organ abundantly supplied with capillary vessels; but we must not forget that before becoming local, it had been general.

It is not so easy in all cases to recognise gangrene in a part as the enumeration of the symptoms proper to that state would lead one to believe. A merely temporary suspension of the circulation may be mistaken for true gangrene; the state of stupor of a limb after the receipt of a violent contusion, or from the action of cold, and the presence of deep-seated, blackish ecchymoses, have sometimes been taken for evidences of that condition. Pseudo-membranous productions adhering to ulcerated surfaces have been mistaken in like manner for gangrenous eschars. But when a peculiar and fœtid smell is exhaled by a part, when putrid gases are infiltrated among the meshes of the tissues, and when the skin is raised into livid-coloured ampullæ, there can be no doubt of its being affected with gangrene; now, every one of these signs are present in the most marked manner in this animal, the subject of our experiment with charcoal.

The separation of gangrenous parts, when left to themselves, is the result of a process which has received the name of *eliminatory inflammation*. If you examine the limb of the animal whose femoral artery we have obstructed, you will perceive a line of demarcation between the dead and living parts; this line corresponds exactly to the point where we tied the vessel. Why, you will inquire, does this eliminatory inflammation fix on this spot for its development rather than any other? Because the vessels beyond it continue permeable, while those on this side are blocked up internally. Here, then, again, we have an hydraulic phenomenon erroneously designated as an inflammation; while the word eliminatory merely expresses what all the world knows, namely, that the dead parts separate from the living. It has been established as a precept in surgery, that amputation of a gangrenous limb should not be performed until the position of this line becomes evident, inasmuch as by operating too early the stump might be also attacked. Practitioners are not all agreed as to the soundness of this precept; some believe it to be of vast importance, others neglect it totally. It is clear that if you act on tissues in which no circulation goes forward, the operation cannot be successful; therefore, the first thing to be done is to ascertain in what spots the blood continues, and has ceased to move. The experiments which we have now in hand appear to me to be calculated to throw some light on these questions; if we succeed in proving that gangrene depends on the stoppage of the blood in its tubes, we shall have supplied some useful hints to the therapeutist.

A great deal has been written on the gangrene determined by the use of ergot of rye. I have made some experiments on this substance, and no matter how large the dose I have administered, either to man or the lower animals, I have never seen gangrene follow. Nevertheless, I am far from contesting the authenticity of the cases in which it is asserted such a phenomenon has been observed in the human subject; they are related by observers worthy of confidence. I may here remark that we are far from possessing any very accurate notions on the action of ergot of rye; it is prescribed in some cases to increase the energy of the contractions of the uterus, in others to diminish it; at one time given to arrest uterine hemorrhage, at another to excite it, &c.; consequently, as it appears to me, the phenomena following its administration should be submitted to further investigation.

Allow me, Gentlemen, in confirmation of some of the statements I have made, to show you a morbid specimen sent me just before the lecture by M. James. It is the aorta of a woman aged about sixty, presenting at its point of bifurcation into the primitive iliacs a swelling, formed by an aneurismal dilatation of its coats. These are thick, and very manifestly diseased; their internal surface is studded with a multitude of calcareous points, and on rubbing it with the finger, a rough sensation, very different from that produced by the healthy membrane, is experienced. These modifications of texture are of a kind to promote materially the coagulation of the blood; it has been ascertained that whenever that fluid flows through tubes of which the internal surface is deficient in polish, it forms into small masses; these are at first isolated, but subsequently become agglutinated, form into layers, and at length constitutes a single coagulum. This is exactly what has occurred in the case before us; the centre of the diseased vessel is blocked up partially by a fibrinous mass (of considerable age to judge from its texture, its yellow colour, and firm consistence), which adheres by different points of its surface to the arterial walls, where these have lost their internal membrane. In other places it is simply in contact with the vessel, or is even separated from it by an interval of one or two lines, thus allowing of the passage of the blood. The coagulum of the aorta gives off two prolongations into the iliac arteries; these vessels are not obliterated, but their capacity is notably diminished. The aorta does not appear to be healthy above the dilatation; through its entire course, from the sigmoid valves to the aneurism, it is covered with small, rough prominences, and bony and calcareous laminæ. The orifices and valves of the heart are in a normal state.

The nature of the morbid changes suffices to point out what must have been the symptoms during life; inasmuch as the impulsion of the heart, being exhausted by a mechanical obstacle in the aorta, was to act with its usual energy in the capillaries and veins, it is evident that the circulation must have been materially impeded, and the return of the blood to the heart rendered exceedingly difficult. This is telling you, in other words, that the lower extremities were œdematous,—a condition which I have been informed by M. James really existed. On a former occasion I called your attention to an example of marked œdema developed in a defibrinised dog; in that case, however, the œdema was general,—here only partial; because, in the former instance, the circulating fluid was altered in constitution, in the latter only its containing tubes. As there was a clot in the aorta, we can easily understand that the circulation must have been obstructed in the numerous ramifications of that vessel, the activity of exhalation augmented by the slow movement of the blood in the arteries, and the absorption of effused fluids by the veins rendered less perfect in consequence of the sluggish progression of the same fluid in these vessels. I had forgot to tell you that the inferior vena cava was somewhat compressed by the tumour of the aorta; the respective positions of the two vessels no doubt led you to suspect this complication. There was a double cause for the œdema observed—the clot in the aorta and the diminished calibre of the vena cava. Had the patient survived at all longer it is probable that the circulation must have totally ceased in these vessels, and the lower limbs been consequently seized with gangrene.

Here is another pathological specimen deserving of your consideration at the present time,—a piece of intestine taken from the body of an old woman who died in the course of yesterday. You will observe that the mucous membrane is black, but that this tint is not uniformly spread over the entire surface; from place to place are seen circular lines, which by their brilliant black hue relieve the general colour, and give the bowel a streaked appearance. This peculiarity is especially visible in the neighbourhood of the lower orifice of the rectum; it was not met with in so advanced a degree in the vicinity of the ileo-cæcal valve, and in the small intestine there was nothing to be seen but rare ecchymoses separated by mucous membrane of natural structure. The free borders of the valvular folds of the intestine are studded with ulcerations, remarkable for the black shining colour of their fundus, which seems as if it were coated with varnish. These various hues depend on the extravasation of blood, and especially of its colouring matter, between the tunics of the digestive tubes. The mucous membrane presents all the appearances of inflammation, which we are the better able to judge of, as we have it in our power to produce exactly similar ones in the living animal. Inject an alkaline solution into the veins, and you will develop these ecchymoses and morbid transudations on the surface of the intestine. The simple difference will be that, instead of black, they will be red; but the colour depends on the age of the disease; this woman had laboured under dysentery for nearly a year. You will never meet with this black tint in recent effusions; but after a certain lapse of time the colouring matter of the blood gradually loses its red hue, becomes darker and darker in appearance, and finally completely black. I have often had occasion to observe a similar colour in the mucous membrane of the bladder in persons who had died with chronic catarrh of that organ; such an appearance is not met with in acute cystitis.

The nature of the stools in dysenteric affections is not less remarkable than their anatomical characters. The patients so affected discharge bloody mucosities, which have been compared to the washings of flesh; the intestinal secretion, rendered more abundant by the afflux of a greater quantity of liquid to the parts, no longer consists of simple mucus; blood in substance escapes through the porosities or the ulcerations of the internal coat, becomes mixed with the mucus, and carries with it shreds of coagulated fibrin. Far from being unusually active in the inflamed parts, the circulation languishes therein, or is even suspended, as is distinctly ascertainable with the help of a microscope: from this state of things it results that the vitality of the mucous membrane is lessened, and in some points even destroyed. Its tissue softens from the action of intestinal secretions; the fæces, as they pass, detach mortified pellicles of the membrane, and the blood effused underneath it is exposed: such is the mode of production of the ulcerations.

I cannot take upon myself to affirm what was the intimate nature of the disease that proved fatal to this woman. Was the blood altered in constitution? This suspicion naturally entered my mind, for we find here all the disordered conditions of the circulation which characterize deficient coagulability in that liquid. Here is the intestine of a dog killed by an injection of subcarbonate of soda; the lesions are perfectly similar, with the single exception of their colour. It is very possible, then, that the dysenteric diarrhœa, and the other symptoms presented by this woman, originated in a diminution of the coagulability of her blood. M. James found no clots in the heart or large vessels; there was a tolerably abundant effusion in the cavity of the pleuræ, and the pulmonary tissue appeared infiltrated with sanguinolent serosity, precisely as we find it in the pneumonia produced by a superabundance of alkali in the blood.

You remember, Gentlemen, that I lately showed you an animal whom I had forced to swallow half a bottle of claret; such an experiment, performed on the human subject, would have produced none but agreeable effects, whereas it caused the death of this dog in four-and-twenty hours, though the animal is remarkable for its size and strength. This result will cause you no surprise, if you call to mind the nature of the direct action of the wine in question on blood, more especially the solidification of the albumen it produced, as exemplified in our recent experiments. Its effects have been of precisely the same kind when injected into the stomach; here is some of the animal's blood obtained from the heart and vessels after his death; the liquid is of a violet hue, and holds some grumous albuminous matter in suspension; indeed, the whole wears very much the appearance of a direct mixture of wine and blood. There is more than mere analogy of phenomena here; the resemblance between them, as occurring, on the one hand, in the vessel we employed, and, on the other, in the circulating system of a living animal, is perfect. If the doctrines held at the present day be correct, the organs of this animal should present their natural appearance, inasmuch as *they* are assumed to be the sole source of morbid influences, and we have only acted on the liquids. In order to convince yourselves of the emptiness of those doctrines, you need only turn your eyes to the lung of this dog, and examine the remarkable lesions of which it is the seat. In one place you observe the signs of engorgement, in another those of hepatisation, in both instances effecting a particular form, to which the term lobular has been applied by pathologists.

Among other substances which we brought in contact experimentally with blood, you will recollect pus was one of the most important: I have repeated the experiment on the living animal. You are well aware that the globules of pus are four times larger than those of blood, and will, therefore, readily comprehend why this experiment (although the direct addition of the morbid product in question did not destroy the coagulability of the sanguineous fluid) should have been followed by a fatal result. The phenomenon is a simply mechanical one: so long as the injection circulated through tubes proportional in point of calibre to the size of the globules, no ill consequence occurred; but once it became necessary for it to traverse the capillaries of the lung, the infinitely minute diameter of those vessels proved an invincible obstacle to its passage. The animal in consequence perished of asphyxia. The pathological inspection of the organs of respiration will convince you of this more firmly than anything I could say on the subject. The moment I cut into it a quantity of purulent matter oozes forth; it is needless to follow up the examination further, the state of the lung clearly shows that the passage of the blood through it was an utter impossibility, and that consequently in that organ originated the cause productive of death. This fact illustrates a question respecting which I made an observation or two the other day,—I mean the absorption of pus. As the very large proportional size of the globules of that adventitious fluid has been ascertained by all microscopical observers, we are justified in rejecting, provisionally at least, the opinion of those who believe in the occurrence of that phenomenon. The future will decide, I trust, with certainty.

We have next to undertake the study of another proximate principle which enters into the composition of blood, and plays therein a most important part, both in a physical and physiological point of view, Like fibrin it exists in the blood in a state of solution, and like it, too, is capable of forming into a mass, and participates in effecting the nutrition and growth of our organs: the principle I allude to is *albumen*. In addition to the property it possesses of retaining the liquid form when separated from the colouring matter, others of its qualities merit particular investigation. We saw, when engaged in the study of the general phenomena of the circulation, that certain liquids, pure water for example, had great difficulty in forcing their way through capillary tubes, but that if some viscid substance were added to them, they no longer encountered any obstacle to their passage. It would appear that the albumen of the blood performs a part of this kind in that liquid, and that when the latter loses its albuminous ingredient in any way it becomes extravasated, and is imbibed by the surrounding tissues. Each of the elements of the complex fluid which we are engaged in studying possesses its own particular characters, and retains them in spite of its being brought into such close connection with the others.

Before commencing our review of the facts bearing on the question of the coagulation of the albumen in the serum of the blood, let me remind you of the necessity of not confounding this serum with what is called the *liquor sanguinis*. As I have already mentioned to you more than once, the former contains no fibrin, while that principle exists in a fluid state in the latter. Be that as it will, the first point of difference we have to notice between the fibrin and albumen is, that the latter does not, as the former, ever undergo spontaneous coagulation. The influence of certain agents is necessary in order to cause the formation of an albuminous coagulum; among them ranks heat. So long as the temperature to which this principle is submitted does not rise beyond 60° Centigr. no change occurs in its transparence or viscidity; but when the temperature reaches from 65° to 75° Centigr. coagulation almost immediately occurs. Hence we need never dread the solidification of the albumen in the interior of the vessels of the living subject, for the temperature of our bodies never exceed 40° Centigr.; although men have borne a heat of 80°. This they have been enabled to do by means of the admirable cooling apparatus applied by Nature in the organs of cutaneous and pulmonary exhalation; the action of which is such that the body always remains within certain limits of temperature, no matter how widely that of the circumambient medium may vary.

There are other agents—for example, alcohol—which produce the same effect on albumen. Mixed with serosity, it solidifies that principle, whether the mixture takes place in a glass vessel or in the interior of the body. This will account for the sudden death which occasionally follows an excessive indulgence in spirituous liquors; especially as, if an animal be made to swallow a large dose of alcohol, solidified albumen is found in his vessels after death. You understand the theory of the circulation sufficiently well to know, that the passage of semi-solid blood through the minute tubes of our organs is a physical impossibility. Although albumen, coagulated by alcohol, resembles pretty closely that which has become solid under the influence of caloric, still it differs from it; for instance, in the former case the albumen occurs in the form of small flocculi, whereas, in the latter, it usually assumes that of large masses. This phenomenon of coagulation takes place in all species of albumen of identical character, but we must not, in consequence, confound together those existing in the white of egg and in the serum of the blood; if I am not mistaken these present particular qualities, which show that they are not perfectly similar in nature. MM. Berzelius, Chevreul, and Couerbe, have noted very remarkable differences between these two substances, from the existence of which it is fair to conclude that the same agent might act differently on them. You shall have an illustration of this in the effects produced by mixing a certain quantity of potass with the two varieties of albumen. I first pour a few drops of the alkali into a vessel containing white of egg, and instantly a transparent, solid, and elastic jelly forms, resembling isinglass, and chemically composed of albuminate of potassa: you are aware that animal proximate principles play the part either of acid or base in chemical combinations. On the other hand, when I treat some of the serosity secreted by the peritoneum in a case of acites, by the same reagent, all the change produced is a scarcely visible precipitation; the remainder retains its liquid condition.

Other proofs of these differences of constitution, no doubt, are to be found, and some of a more decided character than that just illustrated, but chemists are not acquainted with them. Let us try if we may not ourselves succeed in discovering some.

I pour some acetic acid on a small quantity of serum; from their union results an opaline substance, in all probability consisting of acetate of albumen. The same acid causes the formation of a nearly similar coagulum with white of egg. Let us next submit both compounds to the action of a moderately elevated temperature; the albumen obtained from eggs undergoes no change. I next expose that of the serum to the heat of the spirit lamp, and you perceive that I was not deceived in my expectations; we have by this simple process ascertained how the two varieties may be easily distinguished from each other. In proportion as this glass tube becomes heated, the mass it contains gradually loses its consistence, and finally becomes completely fluid, whereas the acetate of albumen procured from eggs remained solid when exposed to the same influence. The liquefaction is not permanent; if we remove the tube from the heat, the coagulum solidifies again.

Here is another mixture of ammonia and white of egg, which has given rise to the formation of a transparent gelatinous precipitate: when heated, this coagulates firmly, assuming the appearance of a vesicular, spongy matter, an albuminate of ammonia. Now, the albumen of serum, when submitted to the same processes, presents none of these characters.

You will remark here an essential difference between albumen and another of the elements of the blood—fibrin; namely, that a variety of substances solidify the former, whereas few possess the property of liquefying it, and that the precise contrary is the truth in the case of fibrin. Thus, *nitrate of copper* coagulates albumen, and prevents the solidification of the other proximate principle named. *Nitrate of silver*, a salt very much employed in practice, precipitates albumen in the form of flocculi. I admit that this salt only acts locally; but I am persuaded that if carried into the circulation it would infallibly cause death; this persuasion I will, however, shortly test, by direct experiment.

The *subacetate of lead* also precipitates albumen; and yet it is daily employed to a vast extent in the adulteration of wine. As a medicinal agent its exhibition has been recommended in cases of fever, and for the relief of the abundant expectoration of phthisical patients; but practitioners soon renounced its use, especially those of Germany, as they remarked that the vessels, through which its passage by absorption into the general circulation took place, always contained coagulated albumen.

The *nitrate of bismuth* is less soluble than the preceding salt, and is consequently less easily absorbed; notwithstanding this it solidifies the albumen.

The *sulphate of zinc* and *chloride of tin* also exercise a local action; they, too, coagulate the principle in question.

The *cyanuret of mercury* exercises a very feeble influence over it.

The *bichloride of mercury*, on the contrary, combines energetically with it. To this mode of combination albumen owes its power as an antidote of corrosive sublimate.

The *hydrochlorate of baryta*, which is employed medicinally in diseases of the bones, also coagulates the albumen of the serum, but less energetically than the last-mentioned substance.

The *ioduretted hydriodate of potass* has caused only a very trifling degree of coagulation. I have for some time past administered this salt to a young man, a patient of mine, affected with a chronic abscess, and decidedly since he has commenced its use the suppuration, which was previously exhausting him, has considerably diminished. Whether it is by acting on the blood, or in some other way, that its good effects are produced I know not, but I am inclined to think the former the most probable.

Ether faintly affects the albumen of the serum, but acts energetically on that of eggs; this it has converted into a white mass, in the same manner as heat would have done.

Oxalic, hydrochloric, sulphuric, and *nitric acids*, have furnished no result.

Boric acid throws down a very slight precipitate.

The *soluble cream of tartar* has formed a coagulum.

The *neutral tartrate* appears to be less energetic.

Seltzer water does not, it appears from the experiment before us, solidify albumen; it seems to me, therefore, well fitted for an habitual drink, for it at once promotes the coagulation of the fibrin, and does not interfere with the albumen; thus possessing two most advantageous properties.

The *hydrochlorate of soda*, *sulphate of magnesia*, and *tartaric acid*, have proved, in these experiments, without action on the substance under consideration.

LECTURE XXII.

Anomalies in the results of experiments.—Cadaveric phenomena are not to be taken for post-mortem appearances.—Effusions into serous cavities; causes which produce them.—Transformation of one species of albumen into another.—Composition of pseudo-membranes.—Fluids which contain albumen.—Experiments with substances which act on the albumen.—Nutritive qualities of gelatin.—Can fibrin be transformed into albumen?—Different effects produced when albumen is injected into veins and arteries.

GENTLEMEN:—Exactly a fortnight ago a circumstance of peculiar nature, and one which it is important you should be made acquainted with, occurred. It struck me that it would be useful to ascertain whether it were possible, by injecting an acidulated liquid into the veins of an animal whose blood had been artificially deprived of the faculty of solidifying, to restore it the coagulable property. I had already learned that blood, liquefied by admixture with subcarbonate of soda in a glass vessel, recovers the power of forming into a clot when a small quantity of dilute sulphuric acid is poured upon it. But whether the same phenomenon would occur in the living vascular system, I was, of course, unable to determine *à priori*. The animal I selected for the experiment was a dog, into whose veins I had already injected twenty grammes of subcarbonate of soda, on three different occasions; and what is no little extraordinary, the animal appeared to be in better health after the third than after the first injection. In consequence of the flourishing state of his health, there was a certain precaution necessary to be taken before commencing the injection,—one which should never be omitted by those who wish to come at positive results in therapeutics,—I mean the ascertaining, at the moment any remedy is administered, whether the morbid state supposed to exist, be really present. Many a drug has owed its vogue and pretended efficaciousness to errors which the habit of observing can alone insure us from committing. You try a new mode of treatment, your patient recovers: are you quite sure that the supposed complaint really existed. There is not an old woman without her formula against rabies; and many intelligent and conscientious persons will tell you of individuals who, after having been bitten by mad dogs, made use of certain drugs and recovered. Recovered from what, if the dogs were not diseased?

I consequently made a small opening in the jugular vein; the blood which escaped was evidently changed in point of colour, but scarcely had a minute elapsed when it formed into a well-organized coagulum. See how necessary it was to make this preliminary examination. Had I omitted it, I should of course have fallen into the error of believing that I had discovered the means of destroying and restoring coagulability; of, as it were, killing an animal by one chemical agent, and recalling him to life by another.

I next introduced an additional dose of twenty grammes of the subcarbonate, dissolved in two-thirds of a pound of distilled water, into the animal's veins; but even after this injection, making in all eighty grammes of the salt, the blood coagulated almost instantly after having been drawn. I was perfectly sure of the substance employed, because the injection of twenty-five grammes of it proved fatal to another dog in half an hour; the only difference of practically important nature between the two animals being that the last alluded to was much less strong and of smaller size than the other. The disparity of size was, however, far from being sufficient to account for the different results following the injections. I made a further trial with the blood of the surviving animal by pouring a small quantity of the alkaline solution on the portion I had removed from the vessels, and found that now everything went on as usual, the blood being perfectly liquefied by the admixture.

Well, Gentlemen, I have now to inform you that this animal died yesterday, and that I am still unable to account for the singular phenomenon I have just described. It has been suggested to me that it might depend on the existing *medical constitution;* but I confess that I have no great faith in the intervention of an influence of the kind. A certain medicine appears to succeed to-day, and in fifteen days hence it will fail when exhibited under the same circumstances; some persons would not hesitate to attribute the failure to the state of the *medical constitution*. But this, and similar methods of evading contradictory facts, are mere *ignes fatui;* with their aid we have explanations for everything, and need never fear being caught in a difficulty.

The autopsy showed that the animal died with all the signs of disordered pulmonary circulation; the lungs were heavier and more dense than is natural; and their parenchyma was generally œdematous. At the time the chest was opened, the pleuræ did not contain the least trace of effusion; but on examining them eighteen hours after, we found a certain quantity of fluid blood in their cavity. This effusion must have been the result of a simple cadaveric phenomenon, for the tissue of the lung had not been cut into, nor any large-sized vessel opened. Do not allow the practical consequences flowing from this fact to escape you; it is evident that, as our hospital autopsies are not made until twenty-four hours after death, certain lesions then most distinctly visible may be mere post-mortem phenomena.

Here is another case calculated to throw light on the history of effusions into the serous cavities. I yesterday injected a small quantity of bile into the pleura of this dog, and a violent *inflammation* immediately declared itself. The membrane, or rather the subjacent vascular rete, is extremely red, and its sensibility greatly augmented. The laminæ of the pleura may always be detached, for they remain unaffected; the stratum of capillary vessels underneath is the real seat of the injection. We read in books descriptions of the *inflammation* of the arachnoid, of its redness, and the arborisation of its vessels; but in such accounts there is both physiological and anatomical error. Neither the arachnoid nor pleura are traversed by blood-vessels; white fluids only penetrate into their substance. But to return to my experiment; the contact of the bile caused the minute vessels to contract, and so oppose the course of the blood in their interior; the consequent stoppage of the circulation caused the extravasation of serum. We can, by injecting any irritating substance into the serous cavities, determine similar effusion; this is even one of the best ways of procuring fibrin dissolved in the serum, and separated from the globules. You are aware that it is difficult enough to separate the fibrin from the other elements of the blood; and that by the ordinary processes it is obtained in mixture with the globules and colouring matter. On the contrary, under the circumstances to which I now allude, it escapes alone, in solution, in the serum; and once out of the vessels it promptly forms into masses, and may be easily obtained in the shape of filaments and lamellæ.

Hence it suffices to obliterate the capillary vessels in order to produce effusion either of the blood in substance, or of some of its elements. If you wish to observe the phenomenon still more distinctly, you have only to perform the experiment on large animals, such as the horse. Give this animal an artificial pleurisy, and quarts of fluid will be exhaled on the surface of the pleura; the fluid will subsequently allow the fibrin it holds in suspension to separate from it, not in a sort of confused crystallization, but in determinate forms, referrible to a common type of organization. The serosity is of a citron or fawn colour, and is subject to great variety in point of the proportion it bears to the false membranes formed along with it. These false membranes first present themselves as an opaque, caseiform matter, suspended in the serosity, or spread in layers on the pleura. If the disease advance towards cure, the effused liquid is seen to be gradually absorbed; the lung expands, and is separated from the thoracic parietes by the false membranes alone, which are found united into an amorphous stratum, subdivided by soft and tremulous laminæ. Reddish striæ soon appear in their centre, which are nothing more than the rudiments of blood-vessels. These are at first irregular, but soon assume a cylindrical form, and pass through a variety of gradations, until they are finally transformed into true tubes. Before the vascular tunics are organized, the blood appears to be contained in canals perforated in the interior of the fibrinous laminæ. These laminæ become elongated by the movements of the thorax, and stretch from the costal to the pulmonary pleura, ceasing to interfere with the process of respiration. The accidental tissue gradually augments in consistence, until it acquires greater powers of resistance even than the circumjacent parts; it is now definitively organized.

It is a curious fact, that the irritating liquid is completely absorbed before any morbid exhalation takes place on the surface to which it had been applied. There is no bitter taste in the fluid effused in consequence of the introduction of bile into the pleura.

At our last meeting we commenced the physiological study of the albumen of the serum, and I informed you that considerable differences were discoverable between this substance as found in the blood and in the egg of birds. This distinction, which is confirmed by our experiments, is of the greatest importance; for it appears, nay, it is certain, that the albumen of an egg cannot, as might have at first been supposed to be the case, perform the functions of that which belongs to the serosity. Nevertheless, I shall this moment have occasion to call your attention to a curious phenomenon which I think I have observed: I use these doubting expressions designedly, because the opinion I am about to state is founded on what must be considered only a commencement of an experimental inquiry. Here is the fact: I injected the albumen of four eggs into the veins of an animal since I last saw you, and it would appear that this albumen has changed its nature, and become transformed into albumen identical in character with that wherewith it was mixed. For the present I am unwilling to say more on the subject; for you must feel, Gentlemen, as well as I, that before giving out such an occurrence as real, our convictions ought to be founded on the most undubitable facts.

I have already, in the course of these lectures, made known to you such points of distinction as we have been enabled to trace between the solidification of the fibrin and the albumen of the blood: I shall now, therefore, simply bring to your recollection the spontaneous character of the coagulation of the former of those substances. I must, at the same time, inform you that when the fibrin passes from the fluid to the solid state in the living animal, it always carries with it, in the process of organization, a certain quantity of albumen. This is especially remarkable in the formation of false membranes, and, indeed, in a variety of other instances. Examine, for example, the liquid exhaled on the surface of cicatrices, which subsequently, by its solidification, forms the pellicle that covers them; treat it by the appropriate tests, and you will see that it is composed of albumen and fibrin. The same is true of the pseudomembranes of which we have just been speaking; in fact, every new formation of organized nature is constituted at once by the two elements.

In other cases the albumen is found in an isolated condition. If you leave some pus at rest, its component elements soon separate; an albuminous fluid goes to the top, and a sediment, more or less solid, is found at the bottom. Purulent matter is, in truth, formed of globules and albumen, mixed with different salts of the blood; and *apropos* of this I may mention to you that the origin of the globules of pus has of late given rise to numerous investigations. Among others, I have myself made some experiments on the subject, which have, at least, furnished the following result:—The serum contains, as I have just said, a certain proportion of albumen; I submitted some pus to the action of a pretty strong heat, and a flocculent precipitation of albumen took place, but the globules neither disappeared, nor underwent any modification, whence the necessary conclusion that they cannot be composed of albumen. If this negative character has not resolved the question of their nature, it has at all events advanced the inquiry an important step.

There are a considerable number of fluids in the economy which contain albumen, and even in larger proportion than the serum. The ovarian vesicles, for example, are filled with a viscid humour, which is yellowish and limpid, and resembles the albumen of egg. It would be an interesting subject of inquiry for the comparative anatomist to determine the relations which possibly exist between the albumen of the human ovary and that of the same organ in oviparous animals. Numerous pathological cases induce me to believe that the ovule is in the mammalia filled almost solely with albumen. I remember to have repeatedly seen the ovules, which dilate for the propagation of the species, hardened and coagulated by heat.

On the other hand, certain liquids, which appear almost wholly composed of albumen, really contain scarcely any of that principle. Such, for example, is the fluid I have named *cephalo-rachidian*, which fills the cavities and invests the exterior of the brain and spinal marrow. Still we may state in general terms, with truth, that a certain quantity of the substance in question will be found in all the great accumulations of fluid produced in the economy, whether their existence be the result of a physiological or a pathological process. Cysts almost always contain some, and in the tumours of that species developed in the ovaries a considerable proportion is always found.

Again, ulcerated surfaces have been known to throw off albumen. I have at this moment under my care two young persons affected with lumbar abscess; suppuration has ceased for some time, but has been replaced by the discharge of a viscid matter: I sent some of it to M. Pelouze, and that gentleman ascertained it to be formed of pure albumen. There are cases, too, in which the urine contains notable quantities of this principle; you are all of you acquainted with the affection termed *albuminous nephritis*, and the characteristic phenemenon on which its diagnosis is founded. It is useless, however, to enumerate all the liquids which neither in the normal nor abnormal state contain albumen; and I shall, therefore, at once proceed with the experiments commenced in my last lecture.

Sulphate of lime, dissolved in water, has solidified the albumen with which it was mixed; we must ascertain whether well-water, which contains a large proportion of that salt, will furnish the same result. The fact we observe may, to a certain extent, explain the natural dislike we have for selenitous water; it incommodes many people not a little, though there are others who make habitual use of it without suffering inconvenience.

The *subcarbonate of iron*, a salt employed in large doses in anemia, chlorosis, &c., has communicated its orange-yellow colour to the albumen.

Arsenious acid, one of our most violent poisons, has formed a coagulum.

The other substances which you perceive in these glass vessels have not produced any very evident effect. These are the *hydrosulphate of potass*, the *bicarbonate of soda*, *mannite*, *lime-water*, *lactic* and *phosphoric acids*. *Citric acid* itself, though it exercises a very deleterious influence on fibrin, has not had any appreciable effect on albumen.

You recollect that in my last lecture I examined at some length the differential characters of the albumen of egg and of serum; and in connection with this subject I must return a little more fully than I did just now to the extraordinary results of the introduction of the albumen of egg into the circulation of an animal. I took the whites of four eggs, and strained them through a linen cloth, in order to ascertain that they were pure, and then slowly introduced them with a syringe into the jugular vein of the animal you see on the table. Almost immediately after the operation he was seized with vomiting, an occurrence which I confess I know not how to explain. During the succeeding forty-eight hours the animal remained in a tolerably tranquil state; the first symptoms seemed to have completely passed off at the end of that time, and I then injected the white of two more eggs. On this occasion there was no return of the vomiting; and on the following day I repeated the operation with the quantity of albumen last employed; the animal died almost immediately after. It is just to infer from this experiment that the introduction of albumen into the circulation may produce disastrous consequences.

But the most surprising part was that the albumen of the eggs actually changed its nature, as it would appear, on being mingled with the blood,—losing the characters of white-of-egg albumen, and assuming those of the same principle as it exists in the serum. Some blood was drawn from the animal a few minutes after the injection, and conducted itself in the ordinary manner; that is, it separated into two parts, the one liquid, the other solid. But its serum, when treated with potass, far from forming a gelatinous mass, of opaque appearance, as you saw occur with the albumen of egg the other day, remained perfectly fluid. This is a pretty strong proof that the white of egg has lost its distinctive properties by being introduced into the circulation. Here is another equally decisive: I now take some of the serum of the animal's blood, and solidify it by pouring on it a few drops of acetic acid; I next heat it over the spirit-lamp, and you see that it immediately resumes the liquid form. Remember, too, that these experiments are made on serum which was extracted only twenty minutes after the injection of the albumen, and yet not a trace of the latter is to be discovered.

I have, at different periods, entered on researches respecting the nutritive qualities of gelatin; qualities which, in spite of the prolonged investigation of which they have been the subject, are still far from being thoroughly understood. In the course of my inquiries I fed animals on hard-boiled whites of eggs, and remarked from the first that the diet, in spite of the animal's hunger, was anything but palateable to them; they ate it with evident repugnance. They lost flesh rapidly, and in every instance where the animal was submitted to the diet for a sufficiently long time it died. Now, the result of experiment, combined with the dislike of animals for such food, (an effect of the instinct that causes them to avoid whatever may prove injurious to them,) argues against the alimentary property of albumen. The subject is an important one, but the present is not the proper time to investigate it.

You remember that on a former occasion we found that injection of serum into the veins rendered the blood incoagulable; it would be well to ascertain whether albumen would have the same effect on blood removed from the body. In this vessel is contained a mixture of albumen, blood, and water, in the proportion of one centilitre of each; a very distinct coagulum has formed, but it has not fallen to the bottom, because, as the albumen changed the density of the serum, the clot became comparatively lighter, and consequently rose to the top. In this second vessel I placed one centilitre of albumen, one of blood, and five of water; and here, too, coagulation has taken place. When the proportion of albumen is raised to three centilitres, those of water and blood remaining the same as in the last experiment, a coagulum still forms, as the contents of this third vessel show you; but the serum appears to be modified, and has become extremely viscid, as it occasionally does in certain forms of disease.

These facts, Gentlemen, throw no light on the cause of this animal's death (the one who died from the injection of whites of eggs into its veins); whether the blood was liquefied or not, can only be decided by the autopsy, to which we now proceed. The very first incision that I make shows that the blood is in a fluid state,—a fact which indicates the effect of this injection to be same as that of serum. As the blood is liquid, we must necessarily find the lungs engorged. I remove the walls of the thorax; those organs, you perceive, are far from being in a normal state. Their cells are distended with black blood; the left organ is infiltrated in a very remarkable manner: it is impossible for an animal's respiration to continue for any length of time with disorders such as these existing in its lungs. There is another morbid appearance which must not be forgotten, I mean this scirrhous mass containing a great number of softened tubercles in its centre: this is an unusual condition in animals, and may have had some share in the production of the phenomena we have witnessed.

As the animal survived several days after the first injection, the intestinal canal, in all probability, will present some pathological condition in relation with the morbid state of the blood; and, just as I had conceived would be the case, you see that the follicles are much developed, and form eminences underneath the tunics that invest them.

There are some physiologists of opinion that fibrin is nothing more than modified albumen; for my part, I think it would be difficult, not to say impossible, to establish such identity between these two substances by well-authenticated facts. M. Denis, of Commercy, respecting whose interesting labours I have already spoken to you in the present course, was among the number of those who embraced this opinion, with the most thorough conviction of its correctness. The principal argument he adduced in its favour was, that when fibrin is treated with nitrate of potassa it liquefies, and then acquires all the properties of albumen; among others, of becoming solid by heat. Such an experiment as this was undoubtedly of a nature to lead to the conclusion that the transformation of fibrin into albumen was really possible, for it appeared exceedingly improbable, considering the simplicity of the process, that any error could have crept into the announcements respecting its issue, nevertheless I fear that this has been the case.

The fact, when generally made known, created quite a sensation in its way, so ready is the world to receive everything with favour that possesses the character of novelty. M. Denis came and described it to me; but as I am one of those who like to see things with my own eyes, and not with those of others, I begged he would repeat his experiments in my presence. The trial took place in this theatre, several of you being present at the time; unfortunately the experimentalist did not succeed in producing any of the results announced. I should be grieved if what I now say discouraged in any degree those persons who endeavour, by conscientious researches, to throw light on questions so utterly obscure as those with which we are now engaged; but I should be well pleased if they had the effect of guarding them against that species of mistaken enthusiasm which has, in a multitude of instances, betrayed the most eminent men into serious errors. Be all this as it may, I by no means wish to affirm that albumen and fibrin are not one and the same substance; but if such be the truth, it must be proved in a different manner from what has yet been done, and I am of opinion that organic chemistry is not yet sufficiently advanced for the purpose.

I told you, Gentlemen, that the qualities of the albumen differed in different animals, and proved it also. You remember that that of eggs, when treated with potass, formed into a solid, gelatinous mass, rendered more opaque by heat; whereas the serum of human blood, treated by the same agent, did not coagulate even with the additional agency of caloric.

Here is a very curious fact, which substantiates the result of our experiment on the conversion of different species of albumen into each other. Before the lecture I put some human serum in a vessel, with one-tenth of its volume of albumen of egg; this I did with the view of ascertaining whether the result would be the same as in the circulating system of an animal; and however extraordinary it may appear, it is no less true, that here, also, the albumen of egg has disappeared, and that the serum treated with potass and heated, as is now done by my assistant, does not coagulate as the albumen of birds would do.

Continuing our investigation of the properties of the albumen existing in the egg, I injected the whites of five eggs, diluted with five times their volume of water, into the veins of a dog. The solution marked four degrees by the areometer, and, consequently, possessed a certain share of viscosity. Notwithstanding this, the animal suffered no inconvenience from the experiment; the functions continue in a normal state, and it is probable will so continue. This is certainly not the kind of result we should have looked for, judging from the issue of former experiments; but this contradictory fact only serves to teach us how utterly ignorant we are of the physiology of the blood, even where we fancy we are most thoroughly well informed. Here is a specimen of the animal's blood; it has coagulated perfectly well, and contains but a small proportional quantity of serum. This serum gives no evidence, by any unusual viscidity, of having had albumen added to it. I now submit it to the action of the two chief reagents, which disclose the presence of albumen; I pour a small quantity of potass on it; this produces no solidification, nor does the heat of the spirit-lamp coagulate it either. What, then, had become of the albumen that was injected? The certain fact is, that it has disappeared, but how, or by what series of transformations, the present state of our knowledge by no means enables us to declare.

Now, as the jugular was the vein whereby the injection was introduced in the preceding experiment, the fluid must have passed directly through the right ventricles into the lungs, and, consequently, have traversed the minute capillary tubes of those organs without causing any obstruction in their interior. But we know that if we had employed any other material endowed with a certain share of viscosity, such as oil, gum, &c., the pulmonary capillaries would have been obstructed inevitably. Numerous experiments have taught us that such substances, though innocent in themselves, cause death by their mechanical effects when injected into the vessels.

It follows, then, that there must be some intimate relation between the albumen of the blood and the capillary vessels, whereof the diameter does not exceed the one-hundred-and-twentieth, or even one-hundred-and-fiftieth part of a millimètre. In physical experiments on the flow of liquids, we find that pure water either will not pass at all, or at least with great difficulty, through very minute tubes, but that when a little albumen is added to the water no difficulty is any longer encountered in making it traverse the same canals. M. Poiseuille came to a similar conclusion through his ingenious hæmostatic researches.

Be this as it may, in order to examine the question in all points of view, I proceed to vary the experiment in such manner as to ascertain whether a substance, which traversed the lung without injuring that organ in any respect, will, in like manner, pass through the vessels of the brain without interfering with the due performance of its functions.

With this view I shall introduce the injection by an artery instead of a vein, and the carotid appears to be the best fitted for the operation. I shall make use of the animal who has already had the albumen introduced by the jugular, for as he has suffered no apparent inconvenience therefrom, I conceive that he may be looked on as perfectly healthy, and that the results of the experiment may be trusted to. I now introduce the point of the syringe into the carotid; the instrument holds about four ounces of a very viscid solution of albumen, as you will find if you examine what remains of it in this vessel. I push the piston gently; the animal appears to suffer some painful sensation; this is evident from his struggles. His state of anguish grows greater; I fear I shall be obliged to put an end to the experiment, the animal is so extremely ill at ease.

It would appear to result from this experiment, Gentlemen, that the introduction of a liquid by the arteries is much more dangerous than by the veins, for I have scarcely been able to inject a drachm of the albuminous fluid on this occasion. Besides this, the contractile force of the heart was powerful enough to raise up the piston, while I was endeavouring to push it down, and some blood has consequently made its way into the instrument. The animal is to all appearance dead; the few drops of his blood, which I now allow to flow, coagulate instantly. There is still some slight arterial pulsation; but it cannot long go on. The fact is altogether a most extraordinary one, but I trust the autopsy will explain it.

I have already made several experiments with dextrine, one of the elements of starch, intended to exemplify the relation borne by the globules of that substance to the capillary vessels in point of size. My present object is to examine another property of this substance. I have here a solution of dextrine, of about the same viscosity and consistence of albumen; I shall inject it into the veins of a dog, and see if its effects are the same as those of that substance: the liquid marks five degrees by the areometer.

The vein is laid bare; I introduce the point of the instrument, which contains about two ounces of fluid; the operation is terminated, and the animal gives no sign of suffering. On applying my ear to the thorax I find the respiratory murmur pure.

Lext us next introduce a small quantity of the same liquid by the carotid. You see that when I do this, no apparent effect seems to follow. These facts respecting albumen and dextrine, although they appear to us to be contradictory, are certainly not so in reality; our ignorance of all the intimate phenomena that occur in these experiments is no doubt the cause of the apparent contradictions we have just observed. Of this you may rest assured, that by continuing these experiments, and by varying them in numerous ways, we shall, in the end, find that these facts, instead of being reciprocally subversive, in truth lend each other firm support.

LECTURE XXIII.

Properties of the globules of the blood; their dimensions, structure, &c., in different animals.—Infusoria of the globules.—Globules of reptiles and birds.—Functions of the globules.—Their relations to each other in different animals.—Substances which act on the globules.—Experiments on animals.—Cyanosis.

Gentlemen:—The close of the session is so near at hand that I shall be obliged to pass rather rapidly over several of the topics connected with the history of the blood, reserving for another time their further and complete study. Thus, we must content ourselves, for the present, with the few facts we have ascertained respecting the albumen, and proceed briefly to examine the chief properties of the globules.

The discovery of these bodies was coëval with that of the microscope, and, indeed, their history is hardly more perfect at the present day than it was during the time of Leuwenhoeck, to whose zeal we are indebted for the earliest observations made on the subject: up to the present hour we know nothing of their uses. Now, though this state of things is not what might have been expected from the almost daily improvements in our scientific and practical acquaintance with optics, yet we may reconcile ourselves to it, as it has left for us of the present day a vast and most interesting, but at the same time most difficult material for study.

Such is the variety of their forms, dimensions, structure, &c., in different species of animals, that our first care must be, not to indulge too freely in generalization, and attribute properties to those of one class of animals which have been discovered in another. Not attending to this point has proved the source of very serious errors. For this reason it is advisable to describe the globules separately, as they exist in the mammalia, in birds, fishes, &c.

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The principal points of view under which it behoves us to consider them, are their structure, form, dimensions, and properties, and the effects resulting either from the influence of chemical agents on them, or from their contact with each other. I shall be the more particular in my examination of the relations they bear to each other, as certain physiologists of the present day, instead of recognising in the oscillations and displacements undergone by the globules an application of the most simple laws of physics, have, in order to explain these movements, made so many little beings of the bodies in question, possessed of a will, going to the right and to the left, forwards and backwards, as the fancy may take them, and resisting in the capillary vessels, sometimes successfully too, the impulse communicated to them by the heart.

We shall first endeavour to obtain an accurate idea of what bodies are called globules; for, as I said to you formerly, there are various kinds found in the same blood. In the human subject, for example, there are some that exist constantly, others non-persistent. Among the former are to be ranged the red globules, of different forms and dimensions; among the second we find the large white globules, of which the uses are equally unknown, though they deserve investigation at the hands of observers, because they form some of the normal constituents of the blood. Along with these are found other globules, infinitely smaller than the red and white ones, which it has been fancied belong to the lymph or chyle. So far, however, this opinion respecting their nature is merely conjectural.

The discovery of the two last orders of globules has been made within the few last years; and there is a third description, of still more recent discovery, to be mentioned. These are adventitious: it would appear that in the course of certain diseases, globules of particular structure and appearance are developed. Indeed, this is a demonstrated fact, and you may consult the works of Müller and Burdach respecting it with much advantage.

Here is the manner in which the globules are generally obtained for study. Some blood is received into a vessel, and beat up with rods; the fibrin is thus separated from the serum, and the globules gravitate to the bottom. A few of them are then spread on the object-glass of a microscope, and by the help of this instrument their circular form is recognised. There are some precautions necessary to be taken in order to make these observations, simple as they may appear. Numerous fluids, water for example, dissolve or alter the properties of the globules; care must, therefore, be had to receive them in the serum, their natural vehicle. Another process for procuring them consists in pricking one's finger with a needle, and spreading the drop of blood thus obtained on a plate of glass; but in this way the globules are so numerous that it is impossible to distinguish them accurately, unless the precaution be taken to suspend them in a small quantity of saccharine or saline water, which, as you are aware, do not dissolve these minute corpuscula.

I told you that their form is circular; but the point which strikes the observer most strongly after the first glance is, that the centre of these bodies has not the same appearance as their circumference. In the former position is seen a black or white point, according to its degree of nearness to the focus of the instrument, and the quantity of light that falls on it; so that sometimes one would fancy them perforated in the centre, while in other cases a sort of nucleus, distinct from the mass of the globule, is very perceptible. Both these opinions, founded as they are on direct observation, have been maintained by physiologists; but here, as in many other cases, the apparent has, I think, been taken for the real. However, it is extremely important to learn to distinguish the centre of the globules; for, according to some physiologists, it contains a solid nucleus in mammiferous animals; whereas, in the opinion of others, and I am among the number, it is more depressed and thinner in this point than elsewhere. I am induced, for the following reason, to deny the existence of a central nucleus in the human subject. If you take some blood, either of a fish or a reptile, and place the globules it contains in the focus of your microscope, you will distinctly perceive a peculiar swelling in their centre; next dissolve the globules, and examine the water used for the purpose; you will find no globules in it, but, instead, the minute bodies that formed their centre, for these are insoluble in water. Now, in the case of the human being, water dissolves the globules completely; no vestige of them is discoverable after the water has acted fully. This fact appears to me conclusive of the correctness of the theory I have adopted, and to that theory I shall consequently adhere until some very strong reasons indeed against it have been made evident. I am aware that M. Letellier, a gentleman well skilled in microscopical observation, affirms that water does not dissolve the globules of the mammalia more perfectly than those of reptiles, and that when these bodies are shaken in the liquid named, a multitude of minute corpuscula, which are, in his opinion, nothing more than the central nuclei, are precipitated to the bottom of the vessel. But on repeating the experiment I fancied I ascertained that the deposit was formed of incompletely dissolved globules. The corpuscula in question were of the same size as the globules themselves, and seemed to differ from them simply in being totally decolourised.

The exact dimensions of the globules are learned by means of a very simple instrument called the micrometer. It is composed of a small plate of glass, on which a scale of hundredths, two-hundredths, and even five-hundredths of a millimètre is engraved. The divisions of the scale are placed in the focus of the microscope, the magnifying power of the instrument being known; the globules are then placed there also, and those minute bodies are compared with the divisions of the scale. A very simple calculation then tells how many hundredths of a millimètre they embrace. By these means we have learned that the ordinary size of the red globules of human blood varies from the one-hundredth-and-tenth to the one-hundred-and-twentieth part of a millimètre. From this brief statement you will readily understand that we might, after ascertaining their thickness and superficial extent, calculate exactly how many globules a single cube millimètre of blood must contain, and so, were we acquainted with the exact volume of blood in the frame, the infinite number existing in the entire circulating system.

The term globule is inappropriately applied to these bodies, for their form is not spherical but lenticular. What proves this, and the fact can easily be ascertained, is, that when they roll under the microscope they turn their edge to the eye of the observer. This edge generally measures in point of thickness the fifth or sixth part of their superficial extent; seen in this manner the globule appears thicker than at its middle, which part has the appearance of being slightly depressed, and, as it were, excavated; this is, however, the case only with the globules of mammiferous animals, for those of others, such as reptiles and fishes, present a real swelling in their centre.

Continuing our microscopical examination of the movements of the globules, we observe them bending, twisting, rolling over each other, until they gradually recover their original appearance. But their intimate structure is not easily determined; according to some they are formed of an exterior investment and a central nucleus, like those of reptiles and birds; but observations, repeated over and over again, militate strongly against the correctness of this notion. In truth, even with a magnifying power of eighteen hundred times, I have never been able to satisfy myself of the presence either of an investment or of a nucleus. It is possible (but I would not affirm that such is the case) that these bodies are provided with an investment which tears. Observers are, indeed, generally of opinion that they are surrounded with a very delicate pellicle; and this idea receives some support from the fact that in the globules of dead subjects there is a sort of puckering visible, such as is presented by membranes of extreme thinness when they begin to dry; for instance, the outer skin of onions.

M. Donné, whose zealous and successful cultivation of microscopical researches is well known, conceived that this phenomenon might be looked on as a certain sign of the death of the individual in whom it was observed,—an idea which might have had a certain degree of importance in forensic medicine. But I have ascertained that the same phenomenon occurred in the globules of a healthy individual, full of life, when they had stood for a certain time in a vessel. Nevertheless, this fact seems to indicate that the globules are enveloped in a sort of membrane, and that this membrane is soluble in water, the acids, the alkalies, and a variety of other fluids; consequently they must possess some peculiar properties which enable them to retain their form; otherwise, as the blood contains water and various salts, the globules must of necessity be dissolved. I must endeavour to make out what these properties are; and with this view, I shall make an artificial serum, and observe what action it exercises on the globules. It has been stated that they owe their persistence in the lenticular form to the albumen; I will separate the albumen from the serum, which is the best way to ascertain if the blood would be unable without that principle to preserve its globules uninjured.

If you wish to observe the globules in the possession of their normal properties, you must take care that they are recent; otherwise you will see them when they have undergone serious changes, partial destruction of their edges, while their surface is mottled with minute spots; they now resemble a combination of small masses constituting an amorphous and irregular whole. Here is another fact which I have recently observed. For the first two or three days no very evident change occurs in the globules; they then assume the maculated aspect just alluded to; when in this state I have given them the name of *corpusculaires.* A third period ensues, during the continuance of which they become the seat of movements of totality, resembling those of infusoria; it is this which has probably made various authors fancy that the globules were, in truth, so many animalcules. These movements, which are exceedingly evident, resemble what is called in physiology *vibratile motions*, and are observable in various organs, especially in the mucous membranes of birds, and of the human subject. In the latter the borders only of the membrane are subject to these vibrations, whereas the centre even of the globule manifests the phenomenon distinctly.

By means of the microscope it has been ascertained that the vibratile motions of the extremity of the lamelles of oysters is due to the presence of minute animalcules; and, by a very surprising analogy, I have distinctly perceived small vibrions on the globules, moving on their surface, penetrating into their substance, and issuing from it by the edges. Finally, the globule diminishes in size and gradually disappears; it appears to be devoured by these infusoria, which has not been mentioned, I believe, by any other person. Some globules present a multitude of these animalcules; others a very few. Thus, the blood furnished by a nasal polypus which I excised lately, contained an innumerable quantity of them.

Hence it appears that, in order to commit no errors in our observations on the globules, we must know how long the blood has been removed from its vessels. This is a fact hitherto unknown; and I cannot believe that I am in error in announcing it now, for, as it was perfectly new to me as well as to others, I repeated my experiments frequently, and, in every instance, with the same results. As to the variations in volume occurring in different animals, you may profitably consult the analytical table of MM. Prevost and Dumas; I have recently found with M. Poiseuille that those of a torpid bat measured the one-hundred-and-thirty-fifth part of a millimétre.

If we examine the globules of another order of animals, reptiles for example, we shall find them possessed of a different structure and conformation. One of their diameters is evidently more elongated than the other; they present a very distinct spot in their centre, and when they turn on their axis a prominence is visible on their edge. When shaken in water they dissolve, except the central part, which is white when it has been well washed, and retains the elliptic form of the original globule. Besides these globules, there are other spherical, opaque corpuscula to be seen without any nucleus. It is possible that these are either nuclei separated from the elliptic globules, or these same globules in course of formation; but, after all, I do not see the necessity of trying to make out their nature by mere hypotheses; the truth is, that I know nothing about them.

The researches which have been made on the nature of the investment and nucleus of the elliptic globules, have led to a belief that the former presents much resemblance to the colouring matter of the blood, and have also shown that the whitish matter of the nuclei, when treated with acetic acid, forms into small, tremulous, and gelatiniform masses.

In birds the globules are elliptic, but they have no nucleus, and hereupon have arisen a host of hypotheses. It has been said, for instance, that they resembled the pollen of flowers; that, like it, they consisted of myriads of infinitely small corpuscula united in a heap; this, and the other suppositions to which I allude, are, no doubt, pretty and ingenious, but they are deficient in one essential point, the material proofs of their reality. The difference in these globules has reference not only to their structure but also to their size; the globules of reptiles have a mean size of from the forty-fifth to the seventy-fifth part of a millimètre; they are, consequently, much larger than those of mammiferous animals and birds.

The function of the globules in the act of the circulation is unknown, and, I fear, in spite of the laborious researches of which the blood is the subject, will long continue so. I have endeavoured to make elliptic globules pass through the blood-vessels of several of the mammalia; they all died, because the injected blood had coagulated in the syringe, and not on account of the want of proportion between the globules and the tubes to be traversed. I have also injected globules separated from the fibrin; the subjects of this experiment suffered no inconvenience from its performance. Here is an animal into whose veins I have injected the globules of three other dogs, stronger than himself, and yet, to all appearance, he is in a state of perfect health.

Allow me, Gentlemen, to interrupt for a minute or two my remarks on the globules, while I say a few words on a case to which I have already alluded. You, without doubt, remember the woman affected with chorea, whose history I related to you. All imaginable kinds of treatment were in vain tried for her relief; and, in the course of time, a complete luxation of the femur from the tibia formed, so complete that all contact between the articular surfaces had ceased to exist. The condyles of the thigh-bone appeared, anteriorly and internally, the head of the tibia posteriorly and externally. This singular luxation caused the rupture of several small blood-vessels, and laceration of the neighbouring tissues. The internal surface of the articulation was laid bare, and the line of union of the cartilage with the synovial membrane plainly visible. The cartilage was, as it were, dissolved and converted into a pultaceous and creamy matter; to this condition succeeded the formation of a sort of granulations, and these soon assumed a dermoid appearance, such as is observed to occur in the mucous membranes in cases of artificial anus and prolapsus uteri.

This case furnished me with a new opportunity of ascertaining that the synovial membrane does not invest the articular cartilage, as Bichat supposed it did. This opinion was one of the numerous seductive hypotheses with which that brilliant writer loaded the science of physiology. Others have supposed that the synovial membrane passed not over but under the cartilage,—a notion which is equally inadmissible; I should certainly, in the case of which I speak, have perceived the membrane in that situation had it really existed there. Another very remarkable fact, in a physiological point of view, connected with this case is, that the cavity of the joint was in contact with the air for two entire months, and yet, during that period, no fever or other disordered condition supervened. Yet we hear it daily announced with the utmost gravity from professorial chairs, that the introduction of atmospheric air into the articulations is a circumstance fraught with most injurious consequences. The granulations of which I spoke were developed in every part of the articulation, even on the fat situated behind the patella. The circulation was kept up in all these points.

I at first hoped that an anchylosis would have been the natural result of this really extraordinary dislocation; and in order to facilitate its formation every species of fracture apparatus was tried in turn, but the sudden and involuntary movements to which the limbs of the patient were exposed by her nervous disease, constantly put the splints out of place, and each day the luxation appeared still more marked than on the previous one. Finding the case thus obstinate, I considered it right to have recourse to amputation, though rather, indeed, as a means of relief than of cure. I performed the operation this morning without the patient's having felt the least pain during its performance,—almost without her having perceived what was going forward. Here is the limb; I have caused it to be brought here that we might examine together the lesions of which it is the seat.

I proceed cautiously, for I look on the case as a very curious one; in the first place you see that the synovial membrane is puffy, *inflamed*, as people say; you observe that on reaching the cartilage it becomes continuous with the granulations. The whole interior of the joint is imbibed with blood; the internal lateral ligament is totally destroyed; the external almost healthy; the crucial ligaments have disappeared, as also that inserted into the tuberosity of the tibia. It would be a difficult matter to find another case resembling this in the advanced nature of the lesions I show you, and the total absence of anything like constitutional reaction; and besides I know not how to find a satisfactory explanation of the extensive disease of this limb unless by having recourse to the *phlogistic theories*, wherewith all difficulties, as you are aware, both great and small, may at once be settled. But I have too favourable an opinion of your understandings, Gentlemen, to set about trying to amuse you with superannuated and ridiculous ideas.

In performing the amputation I employed a process of which I may as well give you a brief description. It is the usual habit to place the arm under the thigh, and divide the tissues by turning the hand round the surface; but this method is inconvenient, because the blood flowing from the vessels falls on the hand or coat-sleeve of the operator. In the present instance I commenced the operation below, and, having brought my first incision to the bone, I then divided the deep layer of muscles in the same manner; I took no more time to perform the operation in this than in the ordinary way.

To return to the globules; when these little bodies are left to themselves for twenty-four or thirty-six hours, they undergo, as I mentioned, a notable alteration in their appearance, they become puckered; besides this, a multitude of monads, or vibrions, appear at the same time in the serum; the gradual destruction of the globules effected by these new-comers, I have already adverted to. I felt anxious to learn whether these monads would attack fresh globules, and to decide the point put some globules extracted from newly-drawn blood into serum containing a large proportion of infusoria. I observed that they pounced on them with a sort of fury, and destroyed them totally in a very short time. It is very useful as a means of seeing the form of the globules, to bring them within the reach of these animalcules, inasmuch as they turn and twist them in every direction, and so allow the eye of the observer to ascertain their configuration with much accuracy.

Some globules of rabbit's blood mixed with the serum containing the infusoria, did not appear to excite in them the same degree of activity as those of human blood. I next tried them with elliptic globules, extracted from the blood of a bird; they turned them about, but almost immediately abandoned them. They seemed to regard with even stronger repugnance the globules of frog's blood, which are still larger. I saw them rush in troops to them, but almost immediately quit them, as it were, in search of better fortune elsewhere.

With respect to the relations of the globules to each other, here is what I observed. Those that are circular, when placed in serum, adhere to each other, and form flexible piles of variable length. It is not long since it was believed that these globules, thus heaped and piled one on the other, formed the basis of, or rather actually constituted muscular fibre; but besides that muscular fibre has not the appearance of globules piled on each other, it is matter of demonstration that it contains none of them. The modes of agglomeration of these little bodies are so numerous and various that it is useless to attempt their description.

The elliptic globules of birds do not form piles or chaplets; they become mutually attached, but this attachment takes place by all the points of their surface, and especially by the extremities of their greater diameter; they thus constitute masses of a particular aspect. They are then seen to adhere to each other by a single point, instead of being super-imposed by their whole surface, as in other instances. On mingling circular and elliptic globules together, I found that those of the same form adhered to each other; it would appear from this that they are under the influence of the electrical phenomena of attraction and repulsion.

As for their structure, it is certain that the globules of reptiles contain a central nucleus, surrounded with a lighter-coloured areola. In birds, on the contrary, the middle part of the globule is occupied by a nebulous matter having the appearance of a nucleus.

It is these bodies, as you are aware, that give the blood its colour; the nature of the colouring matter of the blood is not perfectly understood; chemistry has not yet revealed its composition. It has, however, been demonstrated that the globules undergo modification during the act of respiration, and also by the action of different reagents, which either destroy or change their colour.

In the experiments made in the course of these lectures we have established clearly that some substances act energetically on these globules, whereas others have no effect on them. Among the former all the acids are to be ranged. As you see by the contents of this vessel, hydrosulphuric acid not only destroys their colour, but their whole substance. On the other hand, the bicarbonate of soda tinges them of a scarlet colour. Tannic acid changes their colour to a pale pink. Chemistry throws no light on all these phenomena, which, nevertheless, belong to its province. Let us hope, however, that it will one day be enabled to solve these questions, all of which, especially the intimate composition of the globules, possess much scientific interest. On this latter point we have, as yet, nothing but hypotheses: thus, Everard Home, Prevost, and Dumas, consider them formed of a nucleus, the basis of which is composed of fibrin, and that the hæmatosine invests them in the living blood like a very thin bladder; but how is it possible to admit this to be their true structure, when it is clearly ascertained that the globules of man and other mammiferous animals contain no nucleus. Again, M. Donné thinks that they are composed of an extremely delicate network, containing albumen and hæmatosine within its meshes; but a most simple experiment shows the fallacy of this notion: compare, in truth, the action of water on the globules of mammalia and birds, and on fibrin; the former are not dissolved by it, the latter is insoluble therein. It has also been affirmed that they are formed by the albumen of the serum; but M. Lecanu has demonstrated the existence of very considerable differences between that substance and the globules. Other observers, M. Denis for instance, imagine that they are solely formed of hæmatosine. Now, in order to extract that substance from the globules these bodies must be treated repeatedly by sulphuric acid, alcohol, and ether; and it is an undoubted fact, that when they have been acted on by these reagents, their physical and chemical properties are totally altered. They then leave as residue a grayish powder, of which I show you a specimen on this saucer. When examined under the microscope this powder presents no trace of globules; it is brilliant, with a metallic aspect, and insoluble in water, and in dilute sulphuric and acetic acids,—fluids which dissolve the colouring matter of the blood perfectly. This hæmatosine retains the metallic part of the globules in the state of peroxide; and so large is the proportion of this oxide of iron, that it has been proposed to make medals of it; to its presence the globules owe their very considerable specific gravity. But there is no similarity between this hæmatosine and the colouring matter.

The question of the use of the globules in the circulation is still unresolved. I injected an enormous quantity of globules, (two pounds and a half,) taken from three dogs, into the veins of another animal of the same species. The dog died in a few days in a state of extreme weakness; his gate was staggering, like that of defibrinised animals. The autopsy, however, did not disclose the phenomena I should have expected to find in an animal who had died in the manner described. The lungs were, it is true, altered in appearance, and covered with petechiæ, as in defibrinised animals; but the blood had coagulated slightly in the vessels; the mucous membrane of the intestines was healthy. The results of this experiment consequently throw no light on the uses of the globules. Nor have I been more successful with some other trials of the same kind; thus, I injected 15 centilitres of turkey's blood into the veins of a dog at three o'clock yesterday afternoon; the animal died this morning. You see that its lung, which I show you, presents peculiar arborisations, and that, though it continues crepitating, it is engorged; the blood is liquid. But the most remarkable circumstance connected with this experiment is, that on examining the blood under the miscroscope, I found that no traces of the elliptic globules which I had injected in such large proportion were discoverable. It would appear that they underwent some modification in their passage through the capillary vessels of the animal; and this, if I may be allowed to hazard a supposition, would lead one to believe that the configuration of the minute vessels of mammalia and of birds differs.

I have twice repeated another experiment, consisting in the injection of the blood of twelve or fifteen frogs into the veins of a young dog. The globules of reptiles, as you are aware, are much larger than those of other mammiferous animals, and, besides this, they have a central nucleus, formed of a distinct element, different in nature from the colouring matter. In spite of all these differences of relation, of structure, &c., the injected globules disappeared totally. The animal that underwent the operation still lives, and to all appearance is in perfect health. I drew a small quantity of blood from him, and not a single globule of the elliptic form could I find, though I even brought them into contact with the infusoria of some serum, by whom they were turned and twisted in every direction. I am utterly unacquainted with the mechanism by which this disappearance of the globules in question is effected; but you will remark, gentlemen, that this is not the first time a fact of this kind has presented itself to our notice. You must remember that the albumen of eggs lost its distinctive properties after injection into the veins of a dog and assumed those of the albumen of serum. However, one thing is certain, namely, that the abnormal globules traversed the capillaries of the lungs; were it not so, we should have seen the phenomena of obstruction, or, as they are called, of inflammation, developed.

But, as you may urge, is it not possible that these globules were arrested in their progress through other less important organs?—No; for were this the case, we should infallibly have observed morbid phenomena of the same kind as those produced by the globules of starch and dextrine, of a tenth or twentieth of a millimètre in diameter. As for the mixture of bird's blood with that of a mammiferous animal, we recognised in it some acicular crystals, which disappeared in the course of four-and-twenty hours, whether from the disengagement of ammonia, or otherwise, I know not.

One of the most interesting questions regarding the history of the globules is to calculate approximatively the proportion which they bear in point of quantity to the other elements of the blood. In the present state of the science it is almost impossible to answer this question. If we defibrinise some blood there remain the serum and globules; the latter will in part fall to the bottom, and in part remain mixed with the serum, if the whole be allowed to stand. If we heat this, the albumen will become solid, but it will imprison the globules in its substance; and the same effect will take place if we evaporate the water of the serum in a sand-bath or in a vacuum. I am of opinion that the most likely way to succeed would be to dilute the serum with sugar and water,—a mixture which you well know does not dissolve the globules. Nevertheless, even in this way, there would still remain a little sugar and water, albumen, and some of the salts of the serum mixed with the serosity; but we should, undoubtedly, by this plan, come nearer the truth than in any other way.

It appears to be an undubitable fact, Gentlemen, that the proportion of globules and serum undergoes increase or diminution under certain circumstances; thus, after a certain number of bleedings, the blood is rich in serosity, but poorly supplied with globules. In the class of affections called anemic, the colouring matter of the blood is to a certain extent lost.

If the globular material of the blood be kept in a vessel for some time, the globules change their colour, disengage ammonia, and end by disappearing altogether. Here is some blood taken from a person affected with hypochondriasis, which I have now had by me for several days. It is in a state of putrefaction, and no trace of globules discoverable in it. In this instance, at least, their disappearance is not the result of the disengagement of ammonia; for you see that, on placing a piece of litmus paper in the fluid, it is immediately turned red,—an unanswerable proof that the fluid is acid, and not alkaline. We are, therefore, obliged to ascribe it in part to the infusoria of which I have spoken to you. These animalcules first appear like little black points, and gradually grow bigger: whether they use the globules as food, in the same manner as animals having a mouth, I cannot say, but certain it is that they destroy them.

On adding some common water to this blood I found that the vibrions appeared for a time to be benumbed, but they soon resumed their rapid and varied evolutions; a single drop of acetic acid put an end to them for good and all. That these are not the only animalcules found in the living economy, however, is certain. Thus, if you examine the black fringe which hangs from the edge of the choroid down to the pupil, in the eye of the horse, you will perceive a matter composed of minute granules, which granules are nothing more than masses of infusoria adhering to the membrane and communicating vibratile movements to it,—such, at least, they appeared to me. I think that this movement is not oscillatory, as described by M. Brown. The chyle, also, contains a large quantity of vibrions.

M. Donné imagines that the globules are so many utricles, or bags, containing a liquid. In order to ascertain whether this hypothesis was correct, I performed the following experiment:—I put some globules in oil; they dissolved in that medium, but no fluid made its appearance; the parts of the globules mingled with the oil, and, as you may see here, some few little masses of globular detritus are still imperfectly decomposed.

Independently of the red globules, there are others of another kind in the blood, as I mentioned to you, differing from those in point of size, conformation, and colour; they are known as the white globules. They are first recognised by their greater size, and, in addition, they may be known by having neither central spot nor prominence, but a small part of lighter colour than the rest, which gives them a peculiar appearance; they are flat and lenticular, like the red globules, and often stick to the piece of glass on which they are laid for examination; this last character will serve to distinguish them, for those of the coloured species float about, and oscillate hither and thither continually. In order to isolate them and examine them thoroughly, they should be submitted to a current of water, of acetic acid, or of ammonia. These fluids dissolve the red, without affecting the white globules.

Whatever the nature of these latter may be, I have never observed them in the blood in circulation. It has been supposed that they are nothing more than minute fragments of fibrin, which adhere to the object-glass in consequence of their coagulating. M. Letellier affirms, that if red globules be left in a vessel, white ones may be seen to gravitate to the bottom; this militates in favour of their being formed of fibrin. I have never seen bodies of this kind in the blood of either reptiles, birds, or fishes.

I have slightly alluded to the existence of other globules in the blood, cognisable by their mammillated, raspberry-like look. Some persons are of opinion that these have no real existence, and that their supposed presence depends on an optical illusion, due to the iridescent appearance produced when two plates of glass are not in immediate contact. There is another kind, of much smaller size, and of which I discovered a large quantity in a subject affected with albuminuria; they appear to belong more particularly to the lymph and chyle.

I shall conclude this lecture by performing two experiments, and by relating to you the particulars of a very interesting case which I recently met with at the Hôtel-Dieu. One of these experiments will consist in injecting some blood from a mammiferous animal into the circulating system of a bird; the other in introducing some reptile's blood into the veins of one of the mammalia.

I have had the temperature of the injection for the first experiment raised to 42° Centigr. The jugular vein of this goose has been laid bare; I now make a small incision in its coats, to allow of the introduction of this little silver syringe. Although I push the piston very gently the movements of inspiration become more and more violent; indeed, animals of this class suffer extremely from such experiments as these. I have now filled and emptied the syringe three times into the animal's veins; that is, I have injected about four centilitres, an enormous quantity, when one remembers that birds have, comparatively speaking, much less blood than the mammalia.

The dog now laid on the table is the one that has already had two injections of frog's blood into its veins; it does not appear to suffer much inconvenience from them; after we part I shall have a new dose introduced. The results of both these experiments shall be made known to you in my next lecture.

The case to which I propose now, in a few words, to direct your attention, is that of a little girl of ten years of age, who was placed in my wards about a month past with an organic affection of the heart, according to the hospital register. On examining her I found that the first sound, that produced by the shock of the point of the organ between the fifth and sixth intercostal spaces, either did not exist at all, or was masked by a very loud and prolonged friction sound. In spite of all the laborious research that has been bestowed on the diseases of the heart, the diagnosis of them is not always easy; in the case I speak of, for example, the friction sound might have been the effect of contraction of the aorta; but I felt disinclined to adopt that opinion, because the pulse was full and strong; the only other condition of serious import presented by the patient was a slight bluish tint of the lips. Several days had passed, and the patient was rather in a satisfactory state; she was even preparing to leave the hospital, when, almost quite suddenly, her whole body presented the certain marks of well-characterized cyanosis: in four-and-twenty hours she had ceased to live. The autopsy, carefully performed, brought the following state of things to light, as you will have an opportunity of seeing for yourselves by-and-by. The aorta and semilunar valves are perfectly healthy; here, then, no cause existed for the friction sound we had heard during the patient's life. But the pulmonary artery presents a very marked state of disease; it is extremely contracted; its greatest diameter scarcely measures two and a half, or three lines. Now, as direct experiment teaches us that whenever a liquid passes suddenly from a large to a smaller vessel a friction sound is produced, we can have no difficulty in comprehending how the sound in this girl was caused. It is to be presumed, that the limited width of the pulmonary artery was here, to a certain extent, a congenital defect, for the vessel has only two, instead of three sigmoid valves.

In addition to this contraction of the pulmonary artery, you observe that there is an opening at the upper part of the septum of the ventricles, measuring two lines by three. It is probable, however, that the quantity of venous mixed with arterial blood was very small, or the cyanosis would have appeared sooner, and been more highly developed. I am strengthened in this opinion by the fact, that as the ventricles dilated and contracted at the same moment, the liquid in each cavity must have acted as an obstacle to the passage of the other into the adjoining ventricle. The right ventricle has, in this case, almost as thick walls as the left. The septum between the auricles presented a similar opening which, for the same reasons as I have just stated, is not likely to have led to intermixture of the two kinds of blood. I think I may affirm, that death has here been the result of a gradual contraction of the pulmonary artery.

LECTURE XXIV.

Theories of inflammation; explanation of its phenomena.—German theories; effects of alimentation on the production of inflammation; obscurity of the causes of inflammation; its terminations and mode of treatment.—Concluding remarks.

Gentlemen:—The bird into whose circulating system I injected four centilitres of dog's blood at our last meeting, has supported, without apparent inconvenience, the extraordinary change thereby induced in the nature of its blood. The fact is, that it now lives in part by the help of nutritive materials that had been elaborated for the nourishment of a dog; microscopical inspection of its blood has detected none other than ovoid globules, such as exist in all animals of its class. The result of our earliest experiments of this kind consequently appears to continue to be produced, and will soon, no doubt, take rank among the acknowledged facts of the science, according to this general enunciation:—globules differing in nature from those of the animal into which they are injected, are either destroyed or modified in such manner that none but normal globules will, after a certain period, be discoverable. It is unnecessary for me to impress on your minds the vast importance of this result.

In the course of these lectures, Gentlemen, I have frequently drawn your attention to facts valuable from their direct application to the practice of our art; I have submitted the medicines in most frequent use to an experimental and physiological analysis in respect of their action on the blood, and found, sometimes to my surprise, that the results were widely different from what would generally have been expected. You saw, for example, what is likely to be the consequence of the employment of tannic acid, of Rabel water, and of citric or sulphuric lemonade, as a means of arresting hemorrhage. You learned that all these substances liquefy the blood, and you know what that signifies. I have recently had an opportunity of putting to the test one of the results of the series of experiments to which I allude.

I had a patient affected with severe uterine hemorrhage; my *interne* and myself had vainly tried all the remedies habitually used in such cases, when we thought of employing the *ioduret of iron.* You remember that that salt promotes the coagulation of blood removed from the body. A drachm of it was accordingly dissolved in two pounds of water; the patient employed it as an injection into the vagina several times in the course of the day, and on the morrow the hemorrhage had totally ceased. I do not, however, mean to conclude from this case that the ioduret of iron must be a sovereign remedy in all similar circumstances, but I am of opinion that our success in this instance shows the propriety of having recourse to it again,—and this, for my own part, I shall not fail to do.

I had intended, Gentlemen, to terminate the present course by a complete exposition of the doctrine of inflammation, but you will readily understand that to do anything like justice to such a subject, I should have much more than the time for a single lecture at my disposal. I proceed, nevertheless, to treat it as fully as circumstances permit.

You may have remarked that I avoid sedulously the use of the word *inflammation*, except when about to point out the erroneous notions it involves. Yet it expresses something. If its sole defect were to have been ill-chosen, to have been based on incorrect metaphor, the repugnance I feel for it would be puerile, and more worthy of the pedant than the physiologist, but such is not the case. You know as well as I do, how erroneous are the theories represented by the word, and how various authors in different countries have laboured to add new fallacies to the already abundant stock. Before recurring to these, let me briefly enumerate the principal phenomena that occur in the substance of tissues said to be inflamed, You can easily ascertain them for yourselves with the microscope.

In order to appreciate correctly the modifications that occur in the capillary circulation, it is necessary that we should first understand thoroughly its normal condition; otherwise we should commit no few errors, just as others have done before us. This small animal is tightly fixed to a plate of cork; and the eye, aided by the microscope, follows the course of the globules through the infinitely minute capillary tubes of the mesentery. Apply an alkali or any other chemical agent to any point of the membrane, and immediately the circulation is observed to stop there; nothing is thenceforth perceptible in that situation but an obscure and motionless spot, and around this the capillary vessels are swollen. It is evident that the blood which should have passed through the obliterated vessels, regurgitates into the neighbouring tubes. What I have now said of chemical agents is equally applicable to every substance which modifies the texture of the vessels, or the degree of compression of the liquids. From the moment a want of harmony between the diameter of the capillaries and the volume of the molecules of the blood is brought about, obstruction supervenes, and then follow the appearances ascribed to inflammation. Let us next see into the consequences of the stoppage of the circulation.

Inasmuch as a greater quantity of blood passes in a given time through such vessels as remain free in their interior, those vessels contain a great number of globules, and their colour consequently becomes deeper. This explains to you why inflamed tissues redden; it may also happen that blood finds its way into vessels ordinarily traversed by white fluids only; of this you have a familiar example in conjunctivitis.

The increase of pressure exercised on the inner wall of the capillaries necessarily causes their dilatation. Their coats swell, fill a larger space than before, and gives passage through their widened pores, either to the blood in substance, or to some of its materials. These phenomena of dilatation and extravasation produce more or less tumefaction of the part where they occur. The swelling reaches its maximum in the centre, for there all motion on the part of the liquids is suspended. At the circumference, where the blood continues to move, but in columns of larger diameter than usual, the swelling is less marked, and diminishes in proportion as we make our examination further from the inflamed point. Accordingly, you will remark that inflammatory swellings are generally conical, the most prominent point corresponding to the centre of the tumour, for there the obstruction is complete.

As the temperature of the body depends on the passage of the blood through the tissues, it is evident that it will increase in the direct ratio of the volume of the liquid. We have just seen that inflammation is accompanied by the accumulation of a greater quantity of blood than usual in the vessels, the animal heat of an inflamed part is, therefore, notably increased; this fact cannot escape the observation of any one who examines a part in that condition. The patient himself is conscious of the elevation of temperature. At the same time that the inflamed point is red, swelled, and hot, it becomes the seat of acute or dull pain, according to the nature of the tissue affected. Indeed, in the case of the ligaments, tendons, and cartilages, pain may be completely wanting, and its character varies exceedingly under different circumstances. The mode of distribution of the nervous filaments, their abundance or rarity, the presence or absence of an investing aponeurosis, explain the infinite varieties of sensibility observed in inflamed tissues. The nerves which ramify between the interstices of the different organs, and expand in the substance of their parenchymata, are compressed by the swelling of the vessels, and by the matters effused from their interior; it is they that transmit to the brain the impression of pain. It is possible, too, that the vascular coats themselves, according as they are, or are not, traversed by blood, become painful.

Inflamed parts frequently present unusual pulsations, whence it has been inferred that the vitality itself of the vessels is modified: the patient is conscious of these pulsations. But, I would ask, have the mechanical modifications undergone in this case by the local circulation been allowed their due importance? Whenever an artery is closed at one end, and so presents an insurmountable resistance to the passage of the fluids into the capillary rete, its coats support very considerable pressure, and dilate under each contraction of the heart. Now, this is the state of things in an inflamed part. The nerves, compressed at the instant each arterial pulsation takes place, transmit the impression to the brain; the phenomenon now alluded to is, therefore, physical in essence, though vital in its results,—the first cause of all the disorders observed is the obstruction of the capillary vessels.

In this way the analysis of the course of the blood in an inflamed part explains the true signification of the famous words—*dolor, calor, tumour, rubor*. The pain results from the compression of the nervous filaments by the obliterated or distended vessels; the heat shows that the blood passes in greater abundance into the neighbouring capillaries which continue to be permeable; the swelling is occasioned by the dilatation of the vessels, and by the extravasation of the materials of the blood; the redness depends on the presence of an unusual quantity of globules.

Now, in all this there is not a word about *irritation*, or *organic contractility*, or *insensible sensibility become sensible*, or *spontaneous movements of the globules;* and, in truth, such explanations of the phenomena are utterly deceptive. There is one experiment, in particular, which is cited as conclusive, and has for centuries served as the basis of numerous medical doctrines. We are told that when the mesentery of a living animal is pricked with a needle, the blood flows to the injured point from all sides. Why should the currents of blood change their direction and become retrograde in some of the capillaries of the part? There must be some force that attracts them, and this a stronger force than that of the heart itself, inasmuch as the globules often move in the opposite direction to that in which the action of that organ tends to propel them. This newly-developed force is *irritation*. You have, by your experiment, *stimulated* a point of the membrane, perverted its vitality, and so drawn thereto the blood contained in the surrounding vessels; all these phenomena are the result of irritation. Ere long you see the tissues swell, redden, grow hot, and painful, and extravasation occur in the vicinity,—all this is the result of a new process called *inflammation*. Call irritation *stimulus*, and inflammation *fluxus*, and you restore to its ancient importance the memorable axiom, *ubi stimulus, ibi fluxus.* Yet certain persons, forgetting these ideas, which are as old as medicine itself, talk to us of a new medical doctrine based on *physiology*.

Gentlemen, I have performed this experiment as well as others, and you shall learn the result. When one chances to prick a capillary vessel of the mesentery, the blood escapes by the opening, and the globules of the neighbouring tubes rush to the orifice produced, no matter what was the direction of their previous course. If, on the contrary, the point of your instrument simply enters the tissue of the membrane, without injuring any of its capillaries, the circulation continues as before: no abnormal movements of the globules are produced. These results merit serious examination. What, inflammation depends, you allege, on the exaltation of the vitality of a part, and here I find that pricking an artery determines an afflux of blood, while similarly injuring a membrane produces no derangement in the course of the fluid! And this, although the sensibility of the artery is positively null, in comparison with that enjoyed by the membrane in which it ramifies. If your theory were well founded, the violence of the inflammatory phenomena should be in harmony with the degree of vitality of the tissues: experience shows the precise contrary to be the truth. *Irritation* (to speak your language) is ineffectual in really *irritable* parts; powerful in those deprived of *irritability*. But such absurd and contradictory hypotheses as these are, I delight to perceive, beginning to sink into merited contempt. In a variety of essays recently published, among others that of Todd, of Liverpool, the question of inflammation is quite otherwise considered than has heretofore been the habit, and accurate microscopical researches substituted for rude and superficial observations.

But to the true explanation of the phenomena I have described: —When you pierce the wall of a capillary vessel, the blood, meeting with less resistance at the divided point than elsewhere, escapes through it, and flows out as long as the elasticity of the vascular tunics is not exhausted. But we know that the wounded capillary does not ramify by itself, but communicates by a multitude of branches with the neighbouring vessels of the same description, and that the pressure is equally divided among all. When you act on one of these vessels you act at the same time on all the rest. The most distant globules tend as strongly to escape by the orifice you have made as those in its immediate vicinity, because an equilibrium tends to become established. But the blood, we are told, has been seen to go backwards: to direct itself from the veins towards the arteries. Not a doubt of it; it would be marvellous were it otherwise; for, from the moment the resistance diminishes in any point, the liquid must, by virtue of the laws of physics, flow thereto from every possible quarter. Distend an India-rubber tube with water, and then make a hole in its centre, and you will find that the contained fluid reaches the opening from both ends with equal facility. The elasticity of its walls explains the phenomenon fully; and the same mechanism is in play in the living vessels. The globules are sometimes observed to suddenly reassume their natural course; when this is the case, by examining the point of the vessel injured, you will find that a clot has formed, and obliterated the opening; remove this clot, and the disordered movements instantly recommence.

The course of the blood is not modified when the membrane only is punctured, simply because the properties of the blood are unchanged thereby, and no solution of continuity effected in its tubes,—the impelling agent remains the same as before; there is no reason why any modification of the circulation should ensue.

If, instead of opening the vessel, you apply an acid to its outer surface, phenomena of a different nature follow; but these depend wholly on chemical action, as has been proved by M. Leuret in a series of most interesting essays. The acid combines with the tissue, renders it tough and hard, by taking up its water, and so diminishes the diameter of the capillary vessel that it becomes too narrow proportionally to the volume of the globules; these stagnate and become motionless behind the contracted point. The blood which should have passed through the spot in question, flows back into the surrounding vessels, dilates them, and makes them appear larger than they previously were. In this case, as in the former, these artificial irritations and inflammations are nothing more than mechanical results. And yet these are the experiments which serve as the basis of the theory of irritation and inflammation,—a theory in which nothing is taken into consideration but the modified vitality of the tissues. I am well aware that at the present day it has lost something of its pristine vogue, but it still enjoys a certain share of credit with some practitioners, and it is well to annihilate even its last vestiges of popularity.

I have by no means exhausted the list of superannuated hypotheses, wherewith attempts have been made to explain the nature of inflammatory phenomena. Take, for instance, the notions propounded by Döllinger, in Germany: this author commences a recent essay by establishing that the globules are so many little beings, independent of the action of the heart, moving where they will, and having no guide but their caprice. Hear the description the German physiologist gives of their evolutions, as detected with the microscope:—"The globules sometimes change their shape, and become double their natural length; but this elongation does not depend on the pressure exercised by the walls of the vessels on them, from these being too narrow. I think that this change of form is due to the inclination of the globule to unite itself to its fellow, whether by virtue of an attractive force, or of their common movement." And elsewhere,—"the globules are always in a state of internal antagonism: at one time they may be looked on as so many particular animal organisms, as infusoria, possessed of something of individuality; at another, as parts of a whole, existing only in relation to the mass, and depending on the general relations of the sanguineous system. This is the reason we see them attract and repel each other, move and be moved, separate from the circulating system, and seek again to re-enter it."

Hence, there can be no doubt that the globules are animals of the order infusoria, maintaining a state of internal antagonism, and borne one against the other by feelings of hostility! Nevertheless, their intercourse is occasionally of an amicable kind: but M. Döllinger must describe their pastimes. "I have seen," says this gentleman, "two globules meet, stop, swing against each other, repel each other alternately, approximate and separate; at length one of the two yielding, takes a determinate course, and returns, followed by the other." It is really fortunate that these little creatures find the means of chasing away ennui in these sports; to be for ever turning and jostling each other would have been by far too monotonous an existence.

With ideas like these on the functions of the globules, Döllinger has made these bodies play a most dramatic part in the phenomenon of inflammation. I might extract some additional passages of his book to show into what extravagant notions the imagination may betray even the best understandings; I shall, however, content myself with the specimens already quoted, and pass to the theory of Kaltenbrunner, one of Döllinger's pupils. Kaltenbrunner, on intercepting the action of the heart in a frog's web, observes an oscillatory movement follow, and continue for a certain time in the capillary canals,—he inquires into the source of this movement. "We must not," he says, "seek for its cause elsewhere than in the blood itself. That fluid is formed of globules; movement is an innate property of those bodies; they originate in movement, exist by movement, and disappear when they lose their mobility." Further on we are informed,—"When the movement of the globules slackens, they immediately lose the accurately defined character of their borders; the latter cease to be at all distinguishable when the globules stand at rest. One is undecided whether it should be said that they disappear because they have ceased to move, or cease to move because they disappear. . . . . . The globules, besides their innate mobility, have also an inclination to move towards a central point, that is, towards the heart. This inclination is perfectly evident in the movement of the lymph, a fluid which has so strong an analogy to the blood. . . . . . The lymphatic globules possess this intrinsic mobility as well as those of the blood and of pus."

"The molecules of the blood have a natural tendency to move in the capillaries in such manner as to enter the veins. This phenomenon must be looked on as a very important one in the act of the circulation; but, on examining into this property in healthy organs, it may be objected to this statement that, considering the velocity and regularity with which the blood is propelled into the arteries, to return by the veins without the least interruption, the heart is, to all appearance, the sole cause of the movement. But the harmony which subsists in the state of health between the forces maintaining the circulation in action is the sole cause of this illusion. . . . .The influence of the heart only is perceived, while that of the blood in the capillary vessels remains concealed. To prove the equal share taken by these two forces in the phenomena of the circulation, we have a means offered by nature in a variety of morbid states,—this means is *inflammation*."*

In the explanation of the phenomena of inflammation the same independence of the globules is maintained to exist; nay, more, they are not only said to be independent of that organ, but actually to oppose to it a rival and superior power. A struggle ensues, which Kaltenbrunner thus describes:—" The inclination of the blood to return to the heart undergoes a change in inflammation, of which the nature is unknown. The blood, instead of flowing towards the heart, is then inclined to accumulate at the point of inflammation, which assumes the character of a second heart. The circulation may be now looked on as the result of a conflict between the true and false heart."........."If the intensity of the inflammation increase, the circulation ceases completely at the point where it attains the maximum of violence, that is, towards the centre; the more intense it is, the greater its extent becomes, and the more the quantity of blood that flows towards the inflamed part augments. At length the animal dies; the force of the heart is annihilated; the vessels of the periphery of the body empty themselves, and the blood ceases to circulate, except in the immediate vicinity of the diseased part. Long after death oscillations continue to be perceptible in the vessels round the inflamed part; whereas the circulation and all motion of the blood have ceased for some time previous to death in those that are healthy." All is over, then, with the animal and his true heart! The false heart has got the upper hand, and, enjoying the victory, pursues its manœuvres in full security, for it no longer need fear interruption on the part of its defunct rival. But the chances of war are capricious; we shall now see the false heart succumb in its turn. "When the inflammation is less violent, the *vis medicatrix* of nature, and that of the heart, gradually recover their energy; the afflux of blood round the inflamed part diminishes, its movement grows regular, hæmostasis is at an end; and thus the equilibrium between the force which maintains the circulation by means of the heart, and that which depends on the capillary vessels, is re-established."

Gentlemen, I am unwilling to push our truly painful examination of this writer's extravagant conceptions any further; if tales such as these were not given to us as exact and well-observed facts, we might perhaps smile at their fantastic oddness; but really, brought forward as they are, they put one's patience to a most severe trial. Of course you do not expect one to set about refuting seriously such a host of absurdities; their simple rehearsal is in itself the severest criticism with which it is possible to lash their authors.

So far we have only considered the anatomy of the question; the causes, progress, termination, and treatment of inflammation, should also be submitted to minute examination, and no longer abandoned to the discretion of the nosologists. As for the causes of inflammation, these are so numerous and varied that one cannot comprehend how agencies of so utterly dissimilar a nature should produce the same effect. Let us take, for example, the mucous membrane of the eye; its superficial position and naturally white colour allow the slightest changes in its circulation to be immediately perceived.

An atom of straw gets entangled between the lid and globe of the eye; if it remain there a few moments only, the redness induced in the membrane is transitory, disappearing totally on the removal of the foreign body; if, on the contrary, it remains longer in contact with the conjunctiva, that membrane reddens and becomes painful, and epiphora and an altered condition of its secretion supervene; ophthalmia has set in. Again, divide the fifth pair in the interior of the cranium, on the pars petrosa, and far away from the eye; one of the effects of this section is to destroy the sensibility of the conjunctiva,—the eye becomes as void of feeling as the epidermis. Nevertheless, an ophthalmia quickly follows the division of the nerve. Here, then, we have inflammation preceded in one case by an exaltation, in the other by extinction, of tactile sensibility. You will not surely assert that the disease originated in irritation in both instances; it would be too absurd to refer to the same principle such dissimilar causes. It were much better to confess our profound ignorance of the intimate nature of the irregularities thus developed in the capillary circulation.

The causation of the disease in the foregoing instances is, to a certain extent, intelligible, but what one might refuse to believe, if observation had not many and many a time proved the fact, is, that the kind of alimentation to which an animal is submitted exercises a special influence on the circulation of the organ in question. Dogs fed exclusively on gelatin, albumen, or any other proximate principle, are invariably attacked with ophthalmia, which brings on softening and perforation of the cornea and total destruction of vision; and in some cases this ocular affection makes its appearance before the general health is seriously disordered.

Do you wish for another example? You have it in the conjunctival ecchymoses, injection, and puriform secretion, the increased sensibility of the retina, the perversion of the nutrition of the membranes of the eye in our *defibrinised* animals; but if, instead of diminishing the quantity of fibrin in circulation, you simply deprive it of the faculty of coagulating, as, for instance, by the injection of subcarbonate of soda, ophthalmia is then, too, developed. It suffices even to inject a little putrid water into the veins to produce inflammation of both eyes, or of one. But these are not all the causes of ocular inflammation that may be enumerated; far from it: the action of cold air, of damp, external violence, insolation, the reflection of the rays of the sun from white surfaces, the habit of working at very minute objects, and by artificial light, wounds, burns, deviation of the eyelashes, &c., &c., are to be added to the list. Again, we have scrofulous, venereal, blennorrhagic, gouty, rheumatismal, variolous, psoric, morbillous, ophthalmia, &c.; in a word, there is scarcely a pathological condition which may not lead to the development of that disease. It would be a senseless attempt to endeavour to range, under a common head, so many and so various morbid elements; why, then, designate them by a common epithet?

What is true in the case of the eye is true in that of every other organ as well. I know not how it has come to pass that persons maintain that every material lesion of our organs originates in inflammation, and this in perverted vitality of the solids. Do not forget that every tissue derives from the blood the materials of its structure; modify the blood, and you at the same time modify the progress of the fluid through the capillaries, and, in consequence, the nutrition of the various parenchymata. You may even, by particular kinds of diet, transform the substance of an organ into a totally different matter; this I have done in the case of the liver. I had remarked, in some experiments on the injection of fatty liquids into the veins, that the tissue of the liver assumed a singular aspect; and had even hazarded a conjecture that by varying the process one might succeed in making *fatty livers* at will; and, in fact, when I resumed my researches on different sorts of alimentation, I found that animals which had been fed exclusively on butter, or fat, all of them presented, on examination after death, that particular state of the liver known by pathologists under the name of *fatty*. During life no single symptom occurred authorising any suspicion of the change effected in the structure of the organ; the appetite continued tolerably good; the health apparently satisfactory. You see here the liver of one of these animals; it presents all the characters of fatty degeneration; pale, faded-leaf colour, and friability of tissue; when I plunge a scalpel into its substance the blade is withdrawn smeared with fat. If you take a slice of this liver and rub a piece of paper well with it, the character of the combustion of the latter will indicate the presence of fat. I begged of M. Frémy to analyse the fatty liver of several of these animals, and that young chemist ascertained that this condition of the liver is produced by the deposition of a considerable quantity of stearine in the areolæ of its parenchyma. The other constituent principle of fat, namely, oleine, M. Frémy was unable to find. I was desirous to learn whether the fatty liver of the human subject was similarly impregnated, and found here, too, that the chief fatty ingredient was stearine. This result appears to me really interesting, and one possibly calculated to throw some light on the etiology of this disease of the liver.

These facts show the possibility of our being able to substitute milder means for the barbarous practices now employed to render the liver of certain fowl fatty; why might not this end be as well attained by changing the nature of their food, as by putting out their eyes, confining them in the damp, deforming their chests, and cramming them almost to suffocation with vegetables?

Here is a dog which has for the last three weeks been fed on unpurified beef fat. He is weak, thin, and dejected; I have no doubt that the liver is in a commencing state of fatty degeneration.

You recognise in this animal a proof of what I told you just now respecting the influence of regimen on the production of ophthalmia; its eyes are red, and the lids coated with puriform matter. I beg you will notice the kind of fatty coating that agglutinates together the animal's hairs, and gives them a shining aspect. Might we not hazard the supposition that the oleine has escaped by the cutaneous exhalation, while the stearine has been deposited in the liver; it would be certainly worth while to test the correctness of this conjecture by chemical analysis.

I could not adduce a more striking example than this, to demonstrate the immense importance of alimentation in respect of the nutrition and diseases of our organs. Observe the harmony that subsists between the blood and the vessels containing it. So long as that fluid retains its normal characters, it traverses the capillaries of the liver freely; the moment it grows too viscid, it stagnates and allows some of its materials to pass by infiltration into the parenchyma of the organ. I do not myself know any signs—nor do I believe such are known by any one else—whereby the existence of a fatty condition of the liver may be diagnosticated in the living subject: except in certain cases of phthisis it is even impossible to suspect its presence. But suppose it ascertained that the liver is thus affected, what mode of treatment should we advise? Purgatives, to *stimulate the biliary secretion and disgorge the liver;* leeches to the anus, to *unload the mesenteric veins;* moxas and issues to the right side of the abdomen, to *displace the irritation;* venesection, to *lower the inflammatory state;* and many similar agents would, no doubt, be employed by the routine practitioner. For my part, if I had to combat an affection of the kind, I should commence by inquiring into the previous regimen of the patient, and ascertaining if he had not made excessive use of fatty substance; that is, of butter, fat, and oil. If such were the case, beyond a doubt the first thing to be done would be to change the patient's regimen; the liver might, possibly, then recover its normal structure. We know that when individuals use sorrel to excess, and in consequence void oxalate of lime gravel in their urine, this morbid condition of the renal secretion may be totally removed.

The inference from all I have now said is, that the properties of the blood cannot be modified without the occurrence of pathological phenomena in the capillary circulation. What we see occur in the conjunctiva permits us to judge of what takes place in deep-seated organs. Far from inquiring into the cause of these disorders, people are generally contented with referring them to favourite theories, and, with a word which is essentially meaningless, fancy that they express most important facts. If the circulation be suddenly disordered, we have an *acute* inflammation; if the progress of things be slower, the inflammation is *chronic;* if any tissue be found in a disorganized state, when no morbid process was previously suspected, still there has been inflammation; but here it has been *latent.* Everything is thus easily explained, especially whatever is, in truth, inexplicable.

If our acquaintance with the manner in which inflammation originates be imperfect, our notions respecting its various modes of termination are no less so. Here, too, we find nothing but routine classifications, based on the grossest of the phenomena presented by the diseased tissues. According to authors, the various modes of termination of inflammation are reducible to six categories; those of *delitescence*, *metastasis*, *resolution*, *suppuration*, *induration*, and *gangrene*. An inflammation being given then, the only difficulty consists in fixing to which of these categories it belongs. This would, no doubt, be simple and most ingenious, if each of these groups were founded on a reasonable theory of the phenomena referrible to it; but such is not the case: words we may have in abundance; correct ideas few or none.

By *delitescence* is understood the sudden disappearance of inflammation. Why this prompt cessation of the morbid symptoms? Because the circulation, which was momentarily disturbed, resumed its natural course before considerable obstruction and extravasation had time to form. You have an example of this in what occurs when you apply a burning liquid to your hand; it reddens; more blood than natural flows to it; there is now *irritation;* plunge it into ice-cold water, the quantity of blood rushing to the part is diminished; the redness decreases; you have arrested the *inflammation* in the outset. I have used the expressions irritation and inflammation to make you feel how unfit these words are in such a case, inasmuch as they turn attention from the chief point, the influence exercised by temperature on the progression of the blood in the vessels. For the just comprehension of these phenomena, I beg to refer you to our experiments on cold and heat with the hæmodynamometer.

By *metastasis* is meant the sudden and spontaneous removal of inflammation from the part it occupies to some more or less distant quarter. I shall not trouble you with all the hypotheses which have been imagined to describe the route taken by the inflammatory element in such cases. Some make it travel with the blood; others have supposed it to be summoned by the sympathies; others have been reduced to making it jump from one place to another by virtue of an unknown power, emanating from the vital properties. There is a deal of vagueness connected with the meaning of this word metastasis; the solution of the problem is reserved for experimental research.

Resolution is the gradual disappearance of inflammation, and constitutes its most favourable termination. The disease in this case goes through all its periods *gradatim;* the pain diminishes, the swelling disappears, the parts return insensibly to their normal condition, and the free exercise of their functions. How do the obstructed vessels recover their lost properties? I believe that their recovery depends on the modifications undergone by the blood accumulated in the part. This blood at first became solid by the separation and infiltration of its liquid portion through the pores or ruptured openings of the vascular tunics; at a latter period the coagulated fibrin liquefies, becomes fluid again, and is carried along by the blood, propelled by the heart's action. Once the capillaries are rendered free in their interior, the extravasated materials re-enter the circulation, and the swelling disappears. The increased heat of the inflamed part explains very clearly these chemical changes undergone by the blood accidentally arrested in its course. Observe what happens in vast ecchymoses produced by violent contusions: the blood remains in the fluid state for a certain time; its aqueous part then passes by infiltration into the circumjacent tissues; a solid coagulum occupies the centre of the injured part; this clot gradually softens, breaks down into small pieces, which dissolve in the serosity, and when rendered perfectly fluid are wholly removed by absorption. By the same mechanism, as it appears to me, the resolution of inflammation may be explained; nevertheless, these views require substantiation with the microscope.

When the materials of the blood, arrested in the vessels, or extravasated from them, are softened, they do not always re-enter the circulation; too prolonged a sojourn in the inflamed part disorganizes the structure of the tissues, and life, momentarily suspended therein, threatens to cease almost completely. The tumour gradually loses its hardness, its centre *points*, and there fluctuation is perceptible, while elsewhere it is hard and tolerably firm; in other words, the affection terminates in *suppuration*. Pulsative pain, fluctuation, and attenuation of the skin, announce the formation of pus, when the inflammation is superficial; but it is more difficult to assure oneself of its presence when the disorder is deeply seated in a limb; we are then obliged to have recourse to the rational signs, with which I have nothing at present to do. You perceive that the essential difference between the terminations by resolution and by suppuration consists in the fact, that in the former the molecules of blood are absorbed after having softened; whereas, in the latter, they undergo further modification, are completely transformed, and expelled from the system.

Authors are not agreed as to the mechanism of suppuration, and as to the substances which produce pus: the question does not, however, appear to me unanswerable. We have seen the principal materials of the blood become extravasated, and so constitute inflammatory engorgement; in proportion as they liquefy they combine, intimately, with the tissues in the midst of which they are effused, so that pus is formed at once from the solids and liquids. What proves that it is formed in the diseased part itself is, that it assumes particular characters, according to the organ or tissues in which it is found: thin and grayish in the bones; opaque and caseiform in the cellular membrane; flocculent in the serous, and greenish and thready in the mucous membranes; reddish in the liver; yellowish-gray in the muscles; pus presents special characteristics wherever it is examined. One of the gentlemen present, Dr. Gluge, is able to distinguish from each other the different species of pus by the simple inspection of their globules. I have been among the witnesses of a variety of tests to which his powers have been submitted, and, in every instance, he has been successful. I brought pus from the hospital which had been collected in the lung, the pleura, the peritoneum, and the cellular tissue, and he invariably announced its origin with perfect correctness. I recollect even having endeavoured to entrap him by presenting him with some artificial pus of my own making, as though taken from one of my patients, but he was not to be deceived. I have, therefore, not the least doubt that the globules of the various species of pus may be recognised by certain physical characters. This important fact is another to be added to the list of experimental discoveries.

When the inflammatory engorgement remains stationary, when the hardened condition of the tissues augments, while the other inflammatory phenomena disappears, the disease is said to terminate by *induration*. This mode of termination is proper to glandular organs, and generally succeeds a slow, obscure inflammatory action. The modifications undergone by the indurated parts are not yet precisely understood; all we know is, that the extravasated matters do not re-enter the circulation, that they become organized, and soon form an integral part of the tissues. By what series of phenomena does inflammation of the testicle effect the transformation of the testicle into a scirrhous mass? How comes it that encephaloid of that organ often appears to follow simple disordered conditions of the progression of the blood? There can be little doubt that the nature of the materials effused gives a particular character to the manner in which the disease terminates. According as these remain solid or liquefy, the consistence of the part undergoes changes which would furnish most interesting matter for analysis, but which writers have been contented to designate by different epithets without seeking their cause.

On the termination by *gangrene* I have already spoken to you at sufficient length.

It remains for me to discuss the treatment of inflammation; but this is so vast a subject, Gentlemen, and the indications it embraces are of such an import, that it would be impossible for me even to glance at its chief points in the few minutes I have still at my disposal. Let me state, however, that if the phenomena of inflammation cannot be referred to one origin, the same is even more emphatically true of its treatment. What is to be understood by the strange denomination of antiphlogistic? Will broths and mucilaginous liquids restore the blood its coagulability, and prevent its extravasation through the coats of its vessels? Are leeches the sole means of removing the *thorn* that stimulates the inflamed part? It is rational to attack the blood, as the movement of that fluid is disordered; but it is its composition, much more than its volume, that requires to be modified. What is well fitted for one inflammation, would be injudiciously employed for another. In one case the action of the organs needs stimulation,—in another, depression; in one patient the blood ceases to circulate, because it is too fluid; in another, because it is too viscous; in a word, each inflammation calls for its particular therapeutical measures.

You have remarked, without doubt, Gentlemen, that in the course of the experimental inquiries which I now bring to a conclusion for the present, I have often hesitated about the right inference to be drawn therefrom, and that occasionally I have assumed a result which further experience contradicted, or appeared to contradict. You remember the hog, for instance, of which I spoke to you some time past, whose blood was perfectly coagulable. Fancying that an oleate of soda had formed in this case, I procured some of that substance, and injected a certain dose of it into the veins of a dog, but the animal continued to enjoy excellent health. The condition of the blood in this animal was, consequently, incomprehensible, as far as we were able to judge at the time. Nevertheless, the explanation of the seeming anomaly was the simplest in the world, though only discovered by chance,—the butcher who bled the animal beat up its blood with a rod; in other words, defibrinised it, before sending it to me. The circumstance furnishes a new proof of the necessity of seeing everything with one's own eyes, and not trusting to the accuracy of any one, even in the most apparently trifling particulars. But indecision, such as that I allude to, nay, even errors themselves, are beneficial to the interests of the science, when the experimentalist has the wisdom to re-examine over and over again dubious facts: the art of performing experiments with all necessary care is thus acquired. Such is the method I have adopted from the time I engaged in the task of teaching, and far from regretting that I have done so, I believe that to it, in a great measure at least, I am indebted for the honour of occupying this chair.

Before we separate, allow me, Gentlemen, to thank you cordially for the kind interest with which you have attended these lectures.* The conversations I have had with some of you, and the objections you have occasionally made, by letter or otherwise, to the doctrines I have laid down, convince me that my intentions have been understood and appreciated. If your attention has been constantly kept alive, and your assiduity never allowed to relax, I am far from attributing any merit to myself on that account. No, Gentlemen, flattering as I shall always esteem your approbation to be, to obtain this was not the first object of my ambition; my chief desire was to advance the science we cultivate, and be useful to my fellow men,—to raise the science of medicine from the inferior rank it holds, and place it in its true position,—at the head of human knowledge.

* Lest this should be considered an indelicate expression on the part of M. Magendie, we beg to remind the reader that the lectures at the College of France are, as at the other scientific institutions of Paris, FREE TO THE PUBLIC.—*Rep. Lan.*

THE END.