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AN INTRODUCTION
TO THE STUDY OF
EXPERIMENTAL MEDICINE

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PART ONE

EXPERIMENTAL REASONING

CHAPTER I

OBSERVATION AND EXPERIMENT

Only within very narrow boundaries can man observe the phenomena which surround him; most of them naturally escape his senses, and mere observation is not enough. To extend his knowledge, he has had to increase the power of his organs by means of special appliances; at the same time he has equipped himself with various instruments enabling him to penetrate inside of bodies, to dissociate them and to study their hidden parts. A necessary order may thus be established among the different processes of investigation or research, whether simple or complex: the first apply to those objects easiest to examine, for which our senses suffice; the second bring within our observation, by various means, objects and phenomena which would otherwise remain unknown to us forever, because in their natural state they are beyond our range. Investigation, now simple, again equipped and perfected, is therefore destined to make us discover and note the more or less hidden phenomena which surround us.

But man does not limit himself to seeing; he thinks and insists on learning the meaning of the phenomena whose existence has been revealed to him by observation. So he reasons, compares facts, puts questions to them, and by the answers which he extracts, tests one by another. This sort of control, by means of reasoning and facts, is what constitutes experiment, properly speaking; and it is the only process that we have for teaching ourselves about the nature of things outside us.

In the philosophic sense, observation shows, and experiment teaches. This first distinction will serve as our starting point in examining the different definitions of observation and experiment devised by philosophers and physicians.

I. Various Definitions of Observation and Experiment

Men sometimes seem to confuse experiment with observation. Bacon appears to combine them when he says: "Observation and experiment for gathering material, induction and deduction for elaborating it: these are our only good intellectual tools."

Physicians and physiologists, like most men of science, distinguish observation from experiment, but do not entirely agree in defining the two terms.

Zimmermann [1] expresses himself as follows: "An experiment differs from an observation in this, that knowledge gained through observation seems to appear of itself, while that which an experiment brings us is the fruit of an effort that we make, with the object of knowing whether something exists or does not exist."

This definition embodies a rather generally accepted opinion. According to this definition, observation would be noting objects or phenomena, as nature usually presents them, while experiment would be noting phenomena created or defined by the experimenter. We should set up a sort of contrast, in this way, between observers and experimenters: the first being passive in the appearance of phenomena; the second, on the other hand, taking a direct and active part in producing them. Cuvier expressed the same thought in saying: "The observer listens to nature; the experimenter questions and forces her to unveil herself."

At first sight, and considering things in a general way, this distinction between the experimenter's activity and the observer's passivity seems plain and easy to establish. But as soon as we come down to experimental practice we find that, in many instances, the separation is very hard to make, and that it sometimes even involves obscurity. This comes, it seems to me, from confusing the art of investigation, which seeks and establishes facts, with the art of reasoning, which works them up logically in the search for truth. Now in investigation there may be activity, at once of the mind and of the senses, whether in making observations or in making experiments.

Indeed, if we chose to admit that observation is characterized by this alone, that men of science note phenomena which nature produces spontaneously and without interference by them, still we could

[1] Zimmermann, *Traité sur l'expérience en médecine.* Paris, 1774. Vol. I, p. 45.

not conclude that the mind, like the hand, always remains inactive in observation; and we should be led to distinguish under this head two kinds of observations, some passive, others active. I assume, for instance, what often occurs,—that some endemic disease appears in a region and presents itself to a physician's observation. Here is a spontaneous or passive observation which the physician makes by chance and without being led to it by any preconceived idea. But after observing the first case, if the physician has an idea that the appearance of this disease may well be related to certain special meteorological or hygienic circumstances, he takes a journey to other regions where the same disease prevails, to see whether it develops under the same conditions. This second observation, made in view of a preconceived idea of the nature and cause of the disease, is what we must obviously call an induced or active observation. I should say as much of an astronomer who, in watching the sky, discovers a planet passing, by chance, before his telescope; in this case he makes a fortuitous or passive observation, i.e., without a preconceived idea. But, if the astronomer, after noting the aberrations of a planet, goes on to make observations, to seek a reason for them, then I should say that he makes active observations, i.e., observations produced by a preconceived idea of the cause of the aberration. We might multiply instances of this kind *ad infinitum,* to prove that, in noting natural phenomena that present themselves, the mind is now passive, now active,—which means, in other words, that observations are made, now without a preconceived idea and by chance, and again with a preconceived idea, i.e., with intention to verify the accuracy of a mental conception.

On the other hand, if we concede, as we said above, that experiment is characterized by this alone, that men of science note phenomena which they have produced artificially and which would not naturally have presented themselves, even then we could not find that the experimenter's hand always actively interfered to bring about the appearance of these phenomena. In certain cases indeed we have seen accidents where nature acted for him; and here again, from the point of view of manual intervention, we shall be forced to distinguish between active experiments and passive experiments. Let me assume that a physiologist wishes to study digestion and to learn what happens in a living animal's stomach; he will divide the walls of the abdomen and stomach according to known operative rules and will establish what is called a gastric fistula. The physiologist will certainly think that he has made an experiment, because he has interfered actively to make phenomena appear which did not present themselves naturally to his eyes. But now, let me ask, did Dr. W. Beaumont make an experiment when he came across that young Canadian hunter who had received a point-blank gun-shot in the left hypochondria, and who had a wide fistula of the stomach in the scar, through which one could look inside that organ? Dr. Beaumont took this man into his service and was able to study the phenomena of gastric digestion *de visu* for several years, as he shows in the interesting journal which he has given us on this subject.[2] In the first case, the physiologist acted on the preconceived idea of studying digestive phenomena and made an active experiment. In the second case, an accident produced a fistula of the stomach, and it presented itself fortuitously to Dr. Beaumont. According to our definition, he made a passive experiment. These examples therefore prove that, in verifying the phenomena called experiments, the experimenter's manual activity does not always come in, since it happens that the phenomena, as we have seen, may present themselves as fortuitous or passive observations.

But certain physiologists and physicians characterize observation and experiment somewhat differently. For them, observation consists in noting everything normal and regular. It matters little whether the investigator has produced the appearance of the phenomena himself or by another's hands or by accident; he considers them without disturbing them in their natural state and so makes an observation. Thus, according to these authors, observations were made in both examples of gastric fistula cited above, because in both cases we had under our eyes digestive phenomena in their natural state. The fistula served only for seeing better and making observations under the most favorable conditions.

Experiment, according to the same physiologists, implies, on the contrary, the idea of a variation or disturbance that an investigator brings into the conditions of natural phenomena. This definition corresponds, in fact, to a large group of experiments made in physiology, which might be called experiments by destruction. This form of experimenting, which goes back to Galen, is the simplest; it

[2] W. Beaumont, *Experiments and Observations on the Gastric Juice and on Physiological Digestion.* Boston, 1834.

should suggest itself to the minds of anatomists wishing to learn, in the living subject, the use of parts that they have isolated by dissection in the cadaver. To do this, we suppress an organ in the living subject, by a section or ablation; and from the disturbance produced in the whole organism or in a special function, we deduce the function of the missing organ. This essentially analytic, experimental method is put in practice every day in physiology. For instance, anatomy had taught us that two principal nerves diverge in the face: the facial (seventh cranial) and the trigeminal (fifth cranial); to learn their functions, they were cut, one at a time. The result showed that section of the facial nerve brings about loss of movement, and section of the trigeminal, loss of sensation, from which it was concluded that the facial is the motor nerve of the face, and the trigeminal the sensory nerve.

We said that, in studying digestion by means of a fistula, we merely make an observation, according to the definition which we are examining. But after we have established the fistula, if we go on to cut the nerves of the stomach, in order to see the changes which result in the digestive function, then, according to the same way of thinking, we make an experiment, because we seek to learn the function of a part from the disturbance which its suppression involves. And this may be summed up by saying that in experimentation we make judgments by comparing two facts, one normal, the other abnormal.

This definition of experiment necessarily assumes that experimenters must be able to touch the body on which they wish to act, whether by destroying it or by altering it, so as to learn the part which it plays in the phenomena of nature. As we shall later see, it is on this very possibility of acting, or not acting, on a body that the distinction will exclusively rest between sciences called sciences of observation and sciences called experimental.

But if the definition of experiment which we have just given differs from the definition examined in the first place in that it admits that we make an experiment only when we can vary or can dissociate phenomena by a kind of analysis, still it resembles the first in that it also always assumes an intentional activity on the experimenter's part, in producing a disturbance of the phenomena. Now it will be easy to show that the operator's intentional action can often be replaced by an accident. Here too, as in the first definition, we might distinguish between disturbances occurring intentionally and disturbances occurring spontaneously or unintentionally. Indeed taking again the example in which a physiologist cuts the facial nerve to learn its function, I assume that a ball, a sabre cut or a splinter of stone, has cut or destroyed the facial nerve; there will result fortuitously a paralysis of movement, i.e., a disturbance, exactly the same as that which the physiologist caused intentionally.

It is the same in the case of numberless pathological lesions which are real experiments, by which physicians and physiologists profit, without any purpose on their part to produce the lesions, which result from disease. I emphasize this idea now, because it will be useful to us later, to prove that medicine includes real experiments which are spontaneous, and not produced by physicians.[3]

I will make one more remark by way of conclusion. If indeed we characterize experiment by a variation or disturbance brought into a phenomenon, it is only in so far as we imply that the disturbance must be compared with the normal state. As experiments indeed are only judgments, they necessarily require comparison between two things; and the intentional or active element in an experiment is really the comparison which the mind intends to make. Now, whether the alteration is produced by accident or otherwise, the experimenter's mind compares none the less. It is therefore unnecessary to regard as a disturbance one of the facts to be compared, especially as there is nothing disturbed or abnormal in nature; everything happens according to laws which are absolute, i.e., always normal and determined. Effects vary with the conditions which bring them to pass, but laws do not vary. Physiological and pathological states are ruled by the same forces; they differ only because of the special conditions under which the vital laws manifest themselves.

II. Gaining Experience and Relying on Observation Is Different from Making Experiments and Making Observations

The general objection which I make to the preceding definitions is that they give words too narrow a meaning, by taking account of only the art of investigation, instead of considering observation and experiment at the same time as the two opposite extremes of experimental reasoning. So we find these definitions lacking in clearness and generality. To give the definition its full usefulness and value, therefore, I think that we must distinguish what pertains to the method of investigation, used to gather facts, from the characteristics of the intellectual method, which utilizes facts and makes them at once the support and the criterion of the experimental method.

[3] Lallemand, *Propositions de pathologie tendant à éclairer plusieurs points de physiologie.* Thesis. Paris, 1818. 2nd edition, 1824.

In French the word *expérience* in the singular means, in general and in the abstract, the knowledge gained in the practice of life. When we apply to a physician the word experience in the singular, it means the information which he has gained in the practice of medicine. It is the same with the other professions; and it is in this sense that we say that a man has gained experience, or that he has experience. Subsequently the word *expérience* (experiment) in the concrete was extended to cover the facts which give us experimental information about things.

The word observation in the singular, in its general and abstract use, means noting a fact accurately with the help of appropriate studies and means of investigation. In the concrete the word observation has been extended to cover the facts noted; and it is in this sense that we speak of medical observations, astronomical observations, etc.

Speaking concretely, when we say "making experiments or making observations," we mean that we devote ourselves to investigation and to research, that we make attempts and trials in order to gain facts from which the mind, through reasoning, may draw knowledge or instruction.

Speaking in the abstract, when we say "relying on observation and gaining experience," we mean that observation is the mind's support in reasoning, and experience the mind's support in deciding, or still better, the fruit of exact reasoning applied to the interpretation of facts. It follows from this that we can gain experience without making experiments, solely by reasoning appropriately about well-established facts, just as we can make experiments and observations without gaining experience, if we limit ourselves to noting facts.

Observation, then, is what shows facts; experiment is what teaches about facts and gives experience in relation to anything. But as this teaching can come through comparison and judgment only, i.e., by sequence of reasoning, it follows that man alone is capable of gaining experience and perfecting himself by it.

"Experience," says Goethe, "disciplines man every day." But this is because man reasons accurately and experimentally about what he observes; otherwise he could not correct himself. The insane, who have lost their reason, no longer learn from experience; they no longer reason experimentally. Experience, then, is the privilege of reason. "Only man may verify his thoughts and set them in order; only man may correct, rectify, improve, perfect and so make himself every day more skilful, wise and fortunate. Finally for man alone does the art exist, that supreme art of which the most vaunted arts are mere tools and raw material: the art of reason, reasoning." [4]

In experimental medicine, we shall use the word experience in the same general sense in which it is still everywhere used. Men of science learn every day from experience; by experience they constantly correct their scientific ideas, their theories; rectify them, bring them into harmony with more and more facts, and so come nearer and nearer to the truth.

We can learn,—i.e., gain experience of our surroundings,—in two ways, empirically and experimentally. First there is a sort of teaching or unconscious and empirical experience, which we get from dealing with separate objects. But the knowledge which we gain in this way is also accompanied necessarily by vague experimental reasoning which we carry on quite unawares, and in consequence of which we bring together facts to make a judgment about them. Experience, then, may be gained by empirical and unconscious reasoning; but the obscure and spontaneous movement of the mind has been raised by men of science into a clear and reasoned method, which therefore proceeds consciously and more swiftly toward a definite goal. Such is the experimental method in the sciences by which experience is always gained by virtue of precise reasoning based on an idea born of observation and controlled by experiment. In all experimental knowledge, indeed, there are three phases: an observation made, a comparison established and a judgment rendered. By the experimental method, we simply make a judgment on the facts around us, by help of a criterion which is itself just another fact so arranged as to control the judgment and to afford experience. Taken in this general sense, experience is the one source of human knowledge. The mind in itself has only the feeling

[4] Laromiguière, *Discours sur l'identité: Œuvres.* Vol. I, p. 329.

of a necessary relation between things: it can know the form of that relation only by experience.

Two things must, therefore, be considered in the experimental method: (1) The art of getting accurate facts by means of rigorous investigation; (2) the art of working them up by means of experimental reasoning, so as to deduce knowledge of the law of phenomena. We said that experimental reasoning always and necessarily deals with two facts at a time: observation, used as a starting point; experiment, used as conclusion or control. In reasoning, however, we can distinguish between actual observation and experiment only, as it were, by logical abstraction and because of the position in which they stand.

But outside of experimental reasoning, observation and experiment no longer exist in this abstract sense; there are only concrete facts in each, to be got by precise and rigorous methods of investigation. We shall see, further on, that the investigator himself must be analyzed into observer and experimenter; not according to whether he is active or passive in producing phenomena, but according to whether he acts on them or not, to make himself their master.

III. The Investigator; Scientific Research

The art of investigation is the cornerstone of all the experimental sciences. If the facts used as a basis for reasoning are ill-established or erroneous, everything will crumble or be falsified; and it is thus that errors in scientific theories most often originate in errors of fact.

In investigation, considered as the art of experimental research, we find only facts brought to light by investigators and noted as rigorously as possible with the help of the most suitable means. There is no further occasion here to distinguish observers from experimenters by the character of the processes of investigation used. In the last section I showed that the definitions and distinctions which men have tried to set up on the basis of the investigator's activity or passivity cannot be sustained. Observers and experimenters, indeed, are investigators seeking to note facts to the best of their ability, using more or less complicated means for this purpose according to the complexity of the phenomena that they study. Both need the same manual and intellectual activity, the same dexterity, the same spirit of invention, to create and perfect the different pieces of apparatus or instruments for investigation which, for the most part, they have in common. Every science has its own kind of investigation and its equipment of special instruments and methods. This, after all, is plain enough, since every science is characterized by the nature of its problems and by the variety of the phenomena that it studies. Medical investigation is the most complicated of all: it includes all the methods proper to anatomical, physiological and therapeutic research, and, as it develops, it also borrows from chemistry and physics many means of research which become powerful allies. In the experimental sciences all progress is measured by improvement in the means of investigation. The whole future of experimental medicine depends on creating a method of research which may be applied fruitfully to the study of vital phenomena, whether in a normal or abnormal state. I shall not here dwell on the necessity of such a method of investigation in experimental medicine, and I shall not even attempt to enumerate the difficulties. I shall limit myself to saying that my whole scientific life is devoted to contributing my share to the immense work which modern science will have the glory of having understood, and the merit of having begun, while leaving to future ages the task of continuing and finally establishing it. The two volumes which will form my work on the Principles of Experimental Medicine will be devoted solely to elaborating the methods of experimental investigation applied to physiology, pathology and therapeutics. But as no one man can consider all aspects of medical investigation, I shall limit myself further in this vast subject, by dealing especially with systematization of the methods of zoölogical vivisection. It cannot be gainsaid that this is the most delicate and difficult branch of biological investigation; but I deem it the most fruitful and perhaps the most immediately useful for the advancement of experimental medicine.

In scientific investigation, minutiae of method are of the highest importance. The happy choice of an animal, an instrument constructed in some special way, one reagent used instead of another, may often suffice to solve the most abstract and lofty questions. Every time that a new and reliable means of experimental analysis makes its appearance, we invariably see science make progress in the questions to which this means of analysis can be applied. On

the contrary, a bad method or defective processes of research may cause the gravest errors, and may retard science by leading it astray. In a word, the greatest scientific truths are rooted in details of experimental investigation which form, as it were, the soil in which these truths develop.

One must be brought up in laboratories and live in them, to appreciate the full importance of all the details of procedure in investigation, which are so often neglected or despised by the false men of science calling themselves generalizers. Yet we shall reach really fruitful and luminous generalizations about vital phenomena only in so far as we ourselves experiment and, in hospitals, amphitheatres, or laboratories, stir the fetid or throbbing ground of life. It has somewhere been said that true science is like a flowering and delectable plateau which can be attained only after climbing craggy steeps and scratching one's legs against branches and brushwood. If a comparison were required to express my idea of the science of life, I should say that it is a superb and dazzlingly lighted hall which may be reached only by passing through a long and ghastly kitchen.

IV. Observers and Experimenters; the Sciences of Observation and of Experiment

We have just seen that, from the point of view of the art of investigation, observation and experiment should be considered only as *facts* brought out by investigators, and we have added that methods of investigation do not differentiate the men who observe from the men who experiment. Where then, you will ask, is the difference between observers and experimenters? It is here: we give the name observer to the man who applies methods of investigation, whether simple or complex, to the study of phenomena which he does not vary and which he therefore gathers as nature offers them. We give the name experimenter to the man who applies methods of investigation, whether simple or complex, so as to make natural phenomena vary, or so as to alter them with some purpose or other, and to make them present themselves in circumstances or conditions in which nature does not show them. In this sense, observation is investigation of a natural phenomenon, and experiment is investigation of a phenomenon altered by the investigator. We shall see that this distinction, apparently quite external and depending simply on a definition of words, still supplies the one meaning with which to grasp the important difference separating sciences of observation from sciences of experimentation or experimental sciences. We said, in an earlier paragraph, that the words observation and experiment, taken in an abstract sense, mean, the first, purely and simply noting a fact, the second, testing an idea by a fact. But if we consider observation merely in this abstract sense, we cannot deduce from it any science of observation. By simply noting facts, we can never succeed in establishing a science. Pile up facts or observations as we may, we shall be none the wiser. To learn, we must necessarily reason about what we have observed, compare the facts and judge them by other facts used as controls. But one observation may serve as control for another observation, so that a science of observation is simply a science made up of observations, i.e., a science in which we reason about facts observed in their natural state, as we have already defined them. An experimental science, or science of experimentation, is a science made up of experiments, i.e., one in which we reason on experimental facts found in conditions created and determined by the experimenter himself.

Certain sciences, like astronomy, will always remain sciences of observation, because the phenomena studied are outside our sphere of action; but terrestrial sciences may be, at once, sciences of observation and experimental sciences. Let me add that all these sciences begin as sciences of pure observation; only as we go into the analysis of phenomena do they become experimental, because the observer, turning experimenter, invents methods of investigation to penetrate bodies and vary the conditions of phenomena. Experimentation is only utilizing methods of investigation peculiar to experimenters.

Now experimental reasoning is absolutely the same, whether in sciences of observation or in experimental sciences. We find the same judgment by comparison based on two facts, one used as starting point, the other as conclusion, of our reasoning. Only in the sciences of observation, the two facts are always observations; while in the experimental sciences, the two facts may be taken exclusively from experimentation, or at the same time from experimentation and from observation, according to the special case and according to how deeply we go into experimental analysis. A physician observing a disease in different circumstances, reasoning about the influence of these

circumstances, and deducing consequences which are controlled by other observations,—this physician reasons experimentally, even though he makes no experiments. But if he wishes to go further, and to know the inner mechanism of the disease, he will have to deal with hidden phenomena, and so he will experiment; but he will still reason in the same way.

A naturalist observing animals in all the conditions necessary to their existence, and deducing from these observations consequences verified and controlled by other observations,—such a naturalist uses the experimental method even though he performs no experiments, properly speaking. But if he has to go on to observe phenomena inside the stomach, he is forced to invent more or less complex methods of experimentation in order to look inside a cavity hidden from sight. His experimental reasoning, nevertheless, is the same; Réaumur and Spallanzani alike apply the experimental method when making their observations of natural history or their experiments with digestion. When Pascal made a barometric observation at the bottom of the Tour Saint Jacques, and later took another at the top of the tower, we must admit that he performed an experiment; yet here were simply two comparative observations of air pressure carried out in view of the preconceived idea that this pressure should vary according to height. On the other hand, when Jenner,[5] in observing a cuckoo on a tree, used a spy-glass so as not to frighten it, he made a mere observation, because he did not compare this cuckoo with a previous cuckoo, to deduce a conclusion from the observation and to form a judgment about it. In the same way an astronomer first makes observations and then reasons about them to deduce a system of ideas which he controls by observations made in conditions suited to his purpose. The astronomer reasons like an experimenter, because the experience which he gains implies judgment throughout and comparison between two facts bound together in the mind by an idea.

However, as we have said already, we must clearly differentiate astronomers from the men of science concerned with terrestrial science, in that astronomers limit themselves perforce to observation, as they cannot go into the skies to experiment on the planets. In this power of the investigator to act on phenomena, precisely here

[5] Jenner, *On the natural history of the cuckoo.* (*Philosophical Transactions*, 1788, Chap. XVI, p. 432.)

is the difference separating the so-called sciences of experimentation from those of observation.

Laplace considers astronomy a science of observation, because we can only observe the movements of the planets; we cannot reach them, indeed, to alter their course and to experiment with them. "On earth," said Laplace, "we make phenomena vary by experiments; in the sky, we carefully define all the phenomena presented to us by celestial motion."[6] Certain physicians call medicine a science of observations, because they wrongly think that experimentation is inapplicable to it.

Fundamentally, all sciences reason in the same way and aim at the same object. They all try to reach knowledge of the law of phenomena, so as to foresee, vary or master phenomena. Astronomers foretell the movements of the stars; they deduce from them a quantity of practical ideas; but they cannot alter celestial phenomena by experimentation as do chemists and physicists the phenomena of their sciences.

If then, from the point of view of philosophic method, there is no essential difference between sciences of observation and sciences of experimentation, still there is a real one from the point of view of the practical consequences, which man deduces from them, and the power which he gains by their means. In sciences of observation, man observes and reasons experimentally, but he does not experiment; and in this sense we might say that a science of observation is a passive science. In sciences of experimentation, man observes, but in addition he acts on matter, analyzes its properties and to his own advantage brings about the appearance of phenomena which doubtless always occur according to natural laws, but in conditions which nature often has not yet achieved. With the help of these active experimental sciences, man becomes an inventor of phenomena, a real foreman of creation; and under this head we cannot set limits to the power that he may gain over nature through future progress in the experimental sciences.

The question remains whether medicine should continue a science of observation or become an experimental science. Medicine must doubtless begin as simple clinical observation. Then, since the human organism is in itself a harmonious unit, a little world (microcosm) contained in the great world (macrocosm), men have actually

[6] Laplace. *Système du monde.* Chap. II.

maintained that life is indivisible and that we should limit ourselves to observing the phenomena presented to us as a whole by living organisms, whether well or sick, and should content ourselves with reasoning on the facts observed. But if we admit that we must so limit ourselves, and if we posit as a principle that medicine is only a passive science of observation, then physicians should no more touch the human body than astronomers touch the planets. Hence, normal and pathological anatomy, vivisection applied to physiology, pathology and therapeutics,—all would become completely useless. Medicine so conceived can lead only to prognosis and to hygienic prescriptions of doubtful utility; it is the negation of active medicine, i.e., of real and scientific therapeutics.

This is by no means the place to begin examining so important a definition as that of experimental medicine. I propose to treat this question later with all necessary amplification. I shall limit myself here to saying that I think that medicine is destined to be an experimental and progressive science; and precisely because of my conviction in this respect, I am putting together this work with the object of contributing my share toward encouraging the development of scientific and experimental medicine.

V. Experiment Is Fundamentally Only Induced Observation

Despite the important difference, which we have just pointed out, between the so-called sciences of observation and of experimentation, observers and experimenters still have the common and immediate object, in their investigations, of establishing and noting facts and phenomena as rigorously as possible, and with the help of the most appropriate means; they behave exactly as if they were dealing with two ordinary observations. In both cases, indeed, a fact is simply noted; the only difference is this,—as the fact which an experimenter must verify does not present itself to him naturally, he must make it appear, i.e., induce it, for a special reason and with a definite object. Hence we may say that an experiment is fundamentally just an observation induced with some object or other. In the experimental method, search for facts, i.e., investigation, is always accompanied by reasoning, so that experimenters usually make an experiment to control or verify the value of an experimental idea. Hence, in this case, the experiment is an observation induced with the object of control.

Still, to complete our definition and to extend it to the sciences of observation, it is worth recalling here that, to verify an idea, it is not always absolutely necessary to make an experiment or an observation ourselves. We shall have recourse to experimentation perforce only when the observation to be induced is not already prepared in nature. But if an observation has already been made, either naturally or accidentally, or even by another investigator, then we may take it ready made, and produce it simply to serve as verification of the experimental idea. And this may be summed up again by saying that, in this case, the experiment is just an observation produced for the purpose of control. It follows that, to reason experimentally, we must usually have an idea and afterwards induce or produce facts, i.e., observations, to control our preconceived idea.

We shall examine later the importance of preconceived experimental ideas; let it suffice us now to say that the idea, by virtue of which we undertake an experiment, may be more or less clearly defined, according to the nature of the subject and according to the state of perfection of the science in which we are experimenting. Indeed the guiding idea of an experiment should include everything already known about the subject, so as to direct our search more surely toward problems whose solution may be fruitful in the advancement of science. In established sciences, like physics and chemistry, experimental ideas are deduced in logical sequence from ruling theories, and are submitted with a clearly defined meaning to the control of experiment; but in the case of a science in its infancy, like medicine, where complex and obscure questions are still to be studied, experimental ideas do not always emerge from rather vague conceptions. What then must be done? Must we abstain and wait for observations to present themselves spontaneously and so bring us clearer ideas? We might often wait long and even in vain; in any case we gain by experimenting. But in this instance we can guide ourselves only by a kind of intuition, as we catch sight of probabilities; and if the subject is entirely dark and unexplored, physiologists should not be afraid even to act somewhat at random, so as to try,—permit me the common expression,—fishing in troubled waters. This amounts to saying that, in the midst of the functional disturbances which they produce, they may hope to see some unexpected phenomena emerge which may give direction to their research. Such groping experiments, which are very common in physiology and therapeutics because of the complex and backward state of these sciences, may be called *experiments to see,* because they are intended to make a first observation emerge, unforeseen and undetermined in advance, but whose appearance may suggest an experimental idea and open a path for research.

There are instances, then, in which we experiment without having a probable idea to verify. However, experimentation in this instance is none the less intended to induce an observation, only it induces it with a view to finding an idea which shall point out a later path to follow in investigation. We may therefore say that the experiment is then an observation induced with the object of bringing to birth an idea.

To sum up, the investigator seeks and concludes; he includes both observations and experiments, he pursues the discovery of new ideas, even while seeking facts from which to draw a conclusion, or an experiment calculated to control other ideas.

In a general and abstract sense, an experimenter, then, is a man who produces or induces, in definite conditions, observed facts, to derive from them the instruction which he wishes,—that is, experience. An observer is a man who gathers observed facts and who decides whether they have been ascertained by the help of appropriate means. Thus it is that experimenters must at the same time be good observers, and that in the experimental method, experiment and observation always advance side by side.

VI. In Experimental Reasoning, Experimenters Are Not Separate from Observers

Men of science who mean to embrace the principles of the experimental method as a whole, must fulfill two classes of conditions and must possess two qualities of mind which are indispensable if they are to reach their goal and succeed in the discovery of truth. First, they must have ideas which they submit to the control of facts; but at the same time they must make sure that the facts which serve as starting point or as control for the idea are correct and well established; they must be at once observers and experimenters.

Observers, we said, purely and simply note the phenomena before letters of the alphabet in a box, till a force goes to fetch them, to express the most varied thoughts and mechanisms. This same vital idea preserves beings, by reconstructing the living parts disorganized by exercise or destroyed by accidents or diseases. To the physico-chemical conditions of this primal development, then, we must always refer our explanation of life, whether in the normal or the pathological state. We shall see, indeed, that physiologists and physicians can really act only indirectly through animal physico-chemistry, that is to say, through physics and chemistry worked out in the special field of life, where the necessary conditions of all phenomena of living organisms develop, create and support each other according to a definite idea and obedient to rigorous determinisms.

II. Experimental Practice with Living Beings

As we have said, the experimental method and the principles of experimentation are identical for the phenomena of inorganic bodies and the phenomena of living bodies. But it cannot be the same with experimental practice, and it is easy to conceive that the peculiar organization of living bodies requires special processes for its analysis and must offer difficulties *sui generis.* However, the considerations and special precepts, which we shall present to physiologists, to forearm them against sources of error in experimental practice, have to do only with the delicacy, mobility and fugitiveness of vital qualities and the complexity of the phenomena of life. Physiologists, indeed, have only to take apart the living machine, and with the help of tools and processes borrowed from physics and chemistry, to study and measure the various vital phenomena whose law they seek to discover.

Each of the sciences possesses, if not an individual method, at least particular processes; and the sciences, moreover, serve as instruments one for another. Mathematics serves as an instrument for physics, chemistry and biology in different degrees; physics and chemistry serve as powerful instruments for physiology and medicine. In the mutual service which sciences render one another, we must of course distinguish between the men of science, who use, and those who carry forward each science. Physicists and chemists are not mathematicians because they make calculations; physiologists are not chemists or physicists because they make use of chemical reagents

or physical instruments, any more than chemists and physicists are physiologists because they study the composition or properties of certain animal or vegetable fluids or tissues. Each science has its problem and its point of view which we may not confuse without risk of leading scientific investigation astray. Yet this confusion has often occurred in biological science which, because of its complexity, needs the help of all the other sciences. We have seen, and we still often see chemists and physicists who, instead of confining themselves to the demand that living bodies furnish them suitable means and arguments to establish certain principles of their own sciences, try to absorb physiology and reduce it to simple physico-chemical phenomena. They offer explanations or systems of life which tempt us at times by their false simplicity, but which harm biological science in every case, by bringing in false guidance and inaccuracy which it then takes long to dispel. In a word, biology has its own problem and its definite point of view; it borrows from other sciences only their help and their methods, not their theories. This help from other sciences is so powerful that, without it, the development of the science of vital phenomena would be impossible. Previous knowledge of the physico-chemical sciences is therefore decidedly not, as is often said, an accessory to biology, but, on the contrary, is essential to it and fundamental. That is why I think it proper to call the physico-chemical sciences allied sciences, and not sciences accessory to physiology. We shall see that anatomy is also a science allied to physiology, just as physiology itself, which requires the help of anatomy and of all the physico-chemical sciences, is the science most closely allied to medicine and forms its true scientific foundation.

The application of physico-chemical sciences to physiology and the use of their processes as instruments, suited to the analysis of the phenomena of life, present a great many difficulties inherent, as we have said, in the mobility and fugitiveness of vital phenomena. The spontaneity and mobility enjoyed by living beings make the properties of organized bodies very hard to fix and to study. We must return for an instant here to the nature of these difficulties, as I have already had occasion to do in my lectures.[1]

[1] Claude Bernard, *Leçons sur les propriétés physiologiques des altérations pathologiques des liquides de l'organisme.* Paris, 1859, Vol. I. *Leçon d'ouverture,* Dec. 9, 1857.

A living body differs essentially from an inorganic body from the point of view of the experimenter. An inorganic body has no sort of spontaneity; as its properties are in equilibrium with outside conditions, it soon settles into physico-chemical indifference, i.e., into stable equilibrium with its surroundings. Hence, all the phenomenal changes that it experiences will necessarily come from alterations occurring in surrounding circumstances; and we can easily see that by taking strict account of these circumstances, we can be sure of having the experimental conditions necessary to a good experiment. A living body, especially in the higher animals, never falls into chemico-physical indifference to the outer environment; it has ceaseless motions, an organic evolution apparently spontaneous and constant; and though this evolution requires outer circumstances for its manifestation, it is nevertheless independent in its advance and modality. As proof of this, we see living beings born, develop, fall ill and die, without the conditions of the outer world changing for the observer.

It follows that, in experimenting on inorganic bodies with the help of such instruments as the barometer, thermometer and hygrometer, we can put ourselves in identical conditions and consequently carry on well-defined and similar experiments. Physiologists and physicians have rightly imitated the physicists and have sought to make their experiments more accurate by using the same instruments. But we can see at once that outer conditions whose changes are of such importance to physicists and chemists are of much less value to physicians. Alterations in the phenomena of inorganic bodies are, in fact, always brought about by an outer cosmic change, and it happens at times that a very slight alteration in the surrounding temperature or in barometric pressure leads to important changes in the phenomena of inorganic bodies. But in man and in the higher animals the phenomena of life may alter without any appreciable outer cosmic change, and slight thermometric or barometric changes often exert no real influence on vital manifestations; and though we cannot say that these outer cosmic influences are essentially nil, circumstances occur where it would be almost ludicrous to take account of them. Such was the experimenter's case who repeated my experiments on puncture of the floor of the fourth ventricle, to cause artificial diabetes: he thought that he exhibited greater accuracy in carefully noting the barometric pressure at the moment of making the experiment.

However, instead of experimenting on man and the higher animals, if we experiment on lower living beings, whether animal or vegetable, we shall see that the thermometric, barometric and hygrometric data, which were so unimportant in the first case, must on the contrary be kept very seriously under consideration in the second. Indeed, if we vary the conditions of humidity, heat and atmospheric pressure for infusoria, the vital manifestations in these beings are altered or annihilated according to the more or less significant variations that we make in the cosmic influences cited above. In vegetables and in cold-blooded animals, the conditions of temperature and humidity in the cosmic environment again play a very large part in the manifestations of life. This is what is called the influence of the seasons, which is familiar to everyone. In fine, then, only the warm-blooded animals and man seem to escape cosmic influences and to have free and independent manifestations. We have already said elsewhere that this kind of independence of vital manifestations in man and the higher animals results from greater perfection of their organism, but does not prove that manifestations of life in these physiologically more perfect beings are subject to other laws or other causes. We know, in fact, that the histological units of our organs express the phenomena of life; now if the functions of these units show no variations under the influence of variations in the temperature, humidity and pressure of the outer atmosphere, it is because they are immersed in an organic environment whose degrees of temperature, humidity and pressure do not change with variations in the cosmic environment. Hence, we must conclude that fundamentally manifestations of life in warm-blooded animals and in man are equally subject to exact and definite physico-chemical conditions.

In recapitulating all that we have already said, we see that conditions of environment in all natural phenomena govern their phenomenal manifestations. The conditions of our cosmic environment generally govern the mineral phenomena occurring on the surface of the earth; but organized beings include within themselves the condition peculiar to their vital manifestations, and in proportion as the organism, i.e., the living machine, perfects itself, and its organized units grow more delicate, it creates conditions peculiar to an organic environment which becomes more and more isolated from the cosmic environment. We thus come back to the distinction which I established long since, and which I believe very fruitful, to wit, that two environments must be considered in physiology: the general macrocosmic environment and the microcosmic environment peculiar to living beings; the latter is more or less independent of the former, according to the degree of perfection of the organism. Moreover, we easily understand what we see here in the living machine, since the same thing is true of the inanimate machines created by man. Thus, climatic changes have no influence at all on the action of a steam engine, though everyone knows that exact conditions of temperature, pressure and humidity inside the machine govern all its movements. For inanimate machines we could therefore also distinguish between a macrocosmic environment and a microcosmic environment. In any case, the perfection of the machine consists in being more and more free and independent, so as to be less and less subject to the influence of the outer environment. The human machine is the more perfect, the better it defends itself from penetration by the influences of the outer environment; as the organism grows old and enfeebled, it becomes more sensitive to the outer influences of cold, heat, humidity, and in general to all other climatic influences.

To sum up, if we wish to find the exact conditions of vital manifestations in man and the higher animals, we must really look, not at the outer cosmic environment, but rather at the inner organic environment. Indeed, as we have often said, it is in the study of these inner organic conditions that direct and true explanations are to be found for the phenomena of the life, health, sickness and death of the organism. From the outside, we see only the resultant of all the inner activities of the body, which therefore seem like the result of a distinct vital force in only the most distant relations with the physico-chemical conditions of the outer environment, and manifesting itself always as a sort of organic personality endowed with specific tendencies. We have elsewhere said that ancient medicine considered the influence of the cosmic environment, of water, air and locality; we may indeed find useful suggestions here as to hygienic and as to morbid changes. But modern experimental medicine will be distinguished for being especially founded on knowledge of the inner environment where normal and morbid as well as medicinal influences take action. But how are we to know this inner environment of the organism, so complex in man and in the higher animals, unless we go down and, as it were, penetrate into it, by means of experimentation applied to living bodies? That is to say, to analyze the phenomena of life, we must necessarily penetrate into living organisms with the help of the methods of vivisection.

To sum up, only in the physico-chemical conditions of the inner environment can we find the causation of the external phenomena of life. The life of an organism is simply the resultant of all its inmost workings; it may appear more or less lively, or more or less enfeebled and languishing, without possible explanation by anything in the outer environment, because it is governed by the conditions of the inner environment. We must therefore seek the true foundation of animal physics and chemistry in the physico-chemical properties of the inner environment. However, as we shall see further on, it is necessary to consider not only the physico-chemical conditions indispensable to life, but also the peculiar, evolutionary, physiological conditions which are the *quid proprium* of biological science. I have always greatly emphasized this distinction because I believe that it is basic, and that physiological considerations must predominate in a treatise on experimentation applied to medicine. Here indeed we shall find the differences due to influences of age, sex, species, race, or to state of fasting or digestion, etc. That will lead us to consider, in the organism, reciprocal and simultaneous reactions of the inner environment on the organs, and of the organs on the inner environment.

III. Vivisection

We have succeeded in discovering the laws of inorganic matter only by penetrating into inanimate bodies and machines; similarly we shall succeed in learning the laws and properties of living matter only by displacing living organs in order to get into their inner environment. After dissecting cadavers, then, we must necessarily dissect living beings, to uncover the inner or hidden parts of the organisms and see them work; to this sort of operation we give the name of vivisection, and without this mode of investigation, neither physiology nor scientific medicine is possible; to learn how man and animals live, we cannot avoid seeing great numbers of them die, because the mechanisms of life can be unveiled and proved only by knowledge of the mechanisms of death.

Men have felt this truth in all ages; and in medicine, from the earliest times, men have performed not only therapeutic experiments but even vivisection. We are told that the kings of Persia delivered men condemned to death to their physicians, so that they might perform on them vivisections useful to science. According to Galen, Attalus III (Philometor), who reigned at Pergamum, one hundred thirty-seven years before Jesus Christ, experimented with poisons and antidotes on criminals condemned to death.[2] Celsus recalls and approves the vivisection which Herophilus and Erasistratus performed on criminals with the Ptolemies' consent. It is not cruel, he says, to inflict on a few criminals, sufferings which may benefit multitudes of innocent people throughout all centuries.[3] The Grand Duke of Tuscany had a criminal given over to the professor of anatomy, Fallopius, at Pisa, with permission to kill or dissect him at pleasure. As the criminal had a quartan fever, Fallopius wished to investigate the effects of opium on the paroxysms. He administered two drams of opium during an intermission; death occurred after the second experiment.[4] Similar instances have occasionally recurred, and the story is well known of the archer of Meudon [5] who was pardoned because a nephrotomy was successfully performed on him. Vivisection of animals also goes very far back. Galen may be considered its founder. He performed his experiments especially on monkeys and on young pigs and described the instruments and methods used in experimenting. Galen performed almost no other kind of experiment than that which we call disturbing experiments, which consist in wounding, destroying or removing a part, so as to judge its function by the disturbance caused by its removal. He summarized earlier experiments and studied for himself the effects of destroying the spinal cord at different heights, of perforating the chest on one side or both sides at once; the effects of section of the nerves leading to the intercostal muscles and of section of the recurrent nerve. He tied arteries and performed experiments on the mechanism of deglutition.[6] Since Galen, at long intervals in the midst of

[2] Daniel Leclerc, *Histoire de la médecine*, p. 338.

[3] Celsus, *De Medicina.*

[4] Astruc, *De Morbis Venereis.* Vol. II, pp. 748 and 749.

[5] Rayer, *Traité des maladies des reins.* Vol. III, p. 213. Paris, 1841.

[6] Dezeimeris, *Dictionnaire historique*, Vol. II, p. 444. Daremberg. *Exposition des connaissances de Galien sur l'anatomie pathologique et la pathologie du système nerveux.* Thesis, 1841, pp. 13 and 80.

medical systems, eminent vivisectors have always appeared. As such, the names of Graaf, Harvey, Aselli, Pecquet, Haller, etc., have been handed down to us. In our time, and especially under the influence of Magendie, vivisection has entered physiology and medicine once for all, as an habitual or indispensable method of study.

The prejudices clinging to respect for corpses long halted the progress of anatomy. In the same way, vivisection in all ages has met with prejudices and detractors. We cannot aspire to destroy all the prejudice in the world; neither shall we allow ourselves here to answer the arguments of detractors of vivisection; since they thereby deny experimental medicine, i.e., scientific medicine. However, we shall consider a few general questions, and then we shall set up the scientific goal which vivisection has in view.

First, have we a right to perform experiments and vivisections on man? Physicians make therapeutic experiments daily on their patients, and surgeons perform vivisections daily on their subjects. Experiments, then, may be performed on man, but within what limits? It is our duty and our right to perform an experiment on man whenever it can save his life, cure him or gain him some personal benefit. The principle of medical and surgical morality, therefore, consists in never performing on man an experiment which might be harmful to him to any extent, even though the result might be highly advantageous to science, i.e., to the health of others. But performing experiments and operations exclusively from the point of view of the patient's own advantage does not prevent their turning out profitably to science. It cannot indeed be otherwise; an old physician who has often administered drugs and treated many patients is more experienced, that is, he will experiment better on new patients, because he has learned from experiments made on others. A surgeon who has performed operations on different kinds of patients learns and perfects himself experimentally. Instruction comes only through experience; and that fits perfectly into the definitions given at the beginning of this introduction.

May we make experiments on men condemned to death or vivisect them? Instances have been cited, analogous to the one recalled above, in which men have permitted themselves to perform dangerous operations on condemned criminals, granting them pardon in exchange. Modern ideas of morals condemn such actions; I completely agree with these ideas; I consider it wholly permissible, however, and useful to science, to make investigations on the properties of tissues immediately after the decapitations of criminals. A helminthologist had a condemned woman without her knowledge swallow larvæ of intestinal worms, so as to see whether the worms developed in the intestines[7] after her death. Others have made analogous experiments on patients with phthisis doomed to an early death; some men have made experiments on themselves. As experiments of this kind are of great interest to science and can be conclusive only on man, they seem to be wholly permissible when they involve no suffering or harm to the subject of the experiment. For we must not deceive ourselves, morals do not forbid making experiments on one's neighbor or on one's self; in everyday life men do nothing but experiment on one another. Christian morals forbid only one thing, doing ill to one's neighbor. So, among the experiments that may be tried on man, those that can only harm are forbidden, those that are innocent are permissible, and those that may do good are obligatory.

Another question presents itself. Have we the right to make experiments on animals and vivisect them? As for me, I think we have this right, wholly and absolutely. It would be strange indeed if we recognized man's right to make use of animals in every walk of life, for domestic service, for food, and then forbade him to make use of them for his own instruction in one of the sciences most useful to humanity. No hesitation is possible; the science of life can be established only through experiment, and we can save living beings from death only after sacrificing others. Experiments must be made either on man or on animals. Now I think that physicians already make too many dangerous experiments on man, before carefully studying them on animals. I do not admit that it is moral to try more or less dangerous or active remedies on patients in hospitals, without first experimenting with them on dogs; for I shall prove, further on, that results obtained on animals may all be conclusive for man when we know how to experiment properly. If it is immoral, then, to make an experiment on man when it is dangerous to him, even though the result may be useful to others, it is essentially moral to make experiments on an animal, even though painful and dangerous to him, if they may be useful to man.

After all this, should we let ourselves be moved by the sensitive

[7] Davaine, *Traité des entozoaires*. Paris, 1860. Synopsis, xxvii.

cries of people of fashion or by the objections of men unfamiliar with scientific ideas? All feelings deserve respect, and I shall be very careful never to offend anyone's. I easily explain them to myself, and that is why they cannot stop me. I understand perfectly how physicians under the influence of false ideas, and lacking the scientific sense, fail to appreciate the necessity of experiment and vivisection in establishing biological science. I also understand perfectly how people of fashion, moved by ideas wholly different from those that animate physiologists, judge vivisection quite differently. It cannot be otherwise. Somewhere in this introduction we said that, in science, ideas are what give facts their value and meaning. It is the same in morals, it is everywhere the same. Facts materially alike may have opposite scientific meanings, according to the ideas with which they are connected. A cowardly assassin, a hero and a warrior each plunges a dagger into the breast of his fellow. What differentiates them, unless it be the ideas which guide their hands? A surgeon, a physiologist and Nero give themselves up alike to mutilation of living beings. What differentiates them also, if not ideas? I therefore shall not follow the example of LeGallois,[8] in trying to justify physiologists in the eyes of strangers to science who reproach them with cruelty; the difference in ideas explains everything. A physiologist is not a man of fashion, he is a man of science, absorbed by the scientific idea which he pursues: he no longer hears the cry of animals, he no longer sees the blood that flows, he sees only his idea and perceives only organisms concealing problems which he intends to solve. Similarly, no surgeon is stopped by the most moving cries and sobs, because he sees only his idea and the purpose of his operation. Similarly again, no anatomist feels himself in a horrible slaughter house; under the influence of a scientific idea, he delightedly follows a nervous filament through stinking livid flesh, which to any other man would be an object of disgust and horror. After what has gone before we shall deem all discussion of vivisection futile or absurd. It is impossible for men, judging facts by such different ideas, ever to agree; and as it is impossible to satisfy everybody, a man of science should attend only to the opinion of men of science who understand him, and should derive rules of conduct only from his own conscience.

The scientific principle of vivisection is easy, moreover, to grasp.

[8] LeGallois, *Œuvres*. Paris, 1824. Preface, p. xxx.

It is always a question of separating or altering certain parts of the living machine, so as to study them and thus to decide how they function and for what. Vivisection, considered as an analytic method of investigation of the living, includes many successive steps, for we may need to act either on organic apparatus, or on organs, or on tissue, or on the histological units themselves. In extemporized and other vivisections, we produce mutilations whose results we study by preserving the animals. At other times, vivisection is only an autopsy on the living, or a study of properties of tissues immediately after death. The various processes of analytic study of the mechanisms of life in living animals are indispensable, as we shall see, to physiology, to pathology and to therapeutics. However, it would not do to believe that vivisection in itself can constitute the whole experimental method as applied to the study of vital phenomena. Vivisection is only anatomical dissection of the living; it is necessarily combined with all the other physico-chemical means of investigation which must be carried into the organism. Reduced to itself, vivisection would have only a limited range and in certain cases must even mislead us as to the actual rôle of organs. By these reservations I do not deny the usefulness or even the necessity of vivisection in the study of vital phenomena. I merely declare it insufficient. Our instruments for vivisection are indeed so coarse and our senses so imperfect that we can reach only the coarse and complex parts of an organism. Vivisection under the microscope would make much finer analysis possible, but it presents much greater difficulties and is applicable only to very small animals.

But when we reach the limits of vivisection we have other means of going deeper and dealing with the elementary parts of organisms where the elementary properties of vital phenomena have their seat. We may introduce poisons into the circulation, which carry their specific action to one or another histological unit. Localized poisonings, as Fontana and J. Müller have already used them, are valuable means of physiological analysis. Poisons are veritable reagents of life, extremely delicate instruments which dissect vital units. I believe myself the first to consider the study of poisons from this point of view, for I am of the opinion that studious attention to agents which alter histological units should form the common foundation of general physiology, pathology and therapeutics. We must always, indeed, go back to the organs to find the simplest explanations of life.

To sum up, dissection is a displacing of a living organism by means of instruments and methods capable of isolating its different parts. It is easy to understand that such dissection of the living presupposes dissection of the dead.

IV. Normal Anatomy in Its Relations with Vivisection

Anatomy is the basis necessary to all medical investigation, whether theoretical or practical. A corpse is an organism deprived of living motion, and the earliest explanation of vital phenomena was naturally sought in dead organs, just as we seek explanation of the action of a machine in motion by studying the parts of the machine at rest. It seems, therefore, that the anatomy of man ought to be the basis of physiology and human medicine. Prejudice, however, opposed dissection of corpses, and in default of the human body, men dissected corpses of animals, in organization as close as possible to man. Thus Galen's anatomy and physiology were done mainly on monkeys. At the same time, Galen also performed dissections of cadavers and experiments on living animals, thus proving that he understood perfectly that dissection of cadavers is significant only in so far as it is compared with dissection of living bodies. In this way, anatomy is indeed only the first step in physiology. Anatomy in itself is a sterile science; its existence is justified only by the presence of living men and animals, well and sick, and by its own possible usefulness to physiology and pathology.

We shall limit ourselves here to considering the kinds of service which anatomy, whether of man or of animals, in our present state of knowledge, can render physiology and medicine. This seems to me the more necessary, because different ideas on this subject hold sway in science; in judging these questions it is, of course, understood that I take the point of view of experimental physiology and medicine, which together make up the truly active science of medicine. In biology we may accept various points of view which establish, as it were, just so many distinct sub-sciences. One science, in fact, is separate from another science only because it has a special point of view and a particular problem. In normal biology, we may distinguish the zoölogical point of view, the direct and comparative anatomical points of view, the special and the general physiological points of view. Zoölogy, describing and classifying species, is only a science of observation used as a vestibule to the true science of animals. The zoölogist merely catalogues animals by outward or inner characteristics of form, according to the types and the laws which nature offers him in the formation of these types. The zoölogist's object is classification of beings according to a sort of plan of creation, and for him the problem is summed up in finding the precise place that an animal should fill in a given classification.

Anatomy, or the science of animal organization, is more closely and necessarily related to physiology. The anatomical point of view differs, however, from the physiological in this, that anatomists wish to explain anatomy by physiology, while physiologists seek to explain physiology by anatomy, which is quite another matter. The anatomical point of view has dominated science from the beginning up to the present, and it still has many partisans. The great anatomists who took this point of view all contributed valiantly, nevertheless, to the development of physiological science; and Haller summed up the idea of the subordination of physiology to anatomy in defining physiology as *anatomia animata*. I can easily understand that the anatomical principle was destined necessarily to present itself first, but I believe that it is false in its limitations, and that it has to-day grown harmful to physiology, after having rendered great service, which I should be the last to deny. Anatomy, in fact, is a simpler science than physiology and consequently should be subordinate to it, instead of dominating it. Every explanation of vital phenomena, based exclusively on anatomical considerations, is necessarily incomplete. The great Haller, who summed up the anatomical period of physiology in his immense and admirable writings, restricted himself so far that his physiology is reduced to an irritable fibre and a sensitive fibre. The whole humoral or physico-chemical side of physiology, which cannot be approached by dissection and which treats of what we call our inner environment, was neglected. The reproach which I am making here against the anatomists who wished to subordinate physiology to their point of view, I make in the same way against chemists and physicists who wish to do the same thing. They are also wrong in endeavoring to subordinate physiology, a more complex science, to chemistry or physics, which are simpler sciences. This has not prevented great service being rendered to physiology by much work in physiological chemistry and physics, even though conceived from a false point of view.

In a word, I consider that the most complex of all sciences, physiology, cannot be completely explained by anatomy. To physiology, anatomy is only an auxiliary science, the most immediately necessary, I agree, but insufficient alone, unless we wish to assume that anatomy includes everything, and that the oxygen, chloride of sodium and iron found in the body are anatomical units of the body. Attempts of this kind have been revived in our day by eminent anatomists and histologists. I do not share these views, because they seem to me to create confusion in the sciences and to lead to darkness instead of light.

Anatomists, we said above, try to invert the true method of explanation, i.e., they take anatomy as an exclusive starting point, and propose to deduce directly from it all the functions solely by logic and without experiments. I have already protested against the pretentiousness of anatomical deductions,[9] by showing that they rest on an illusion of which anatomists are unaware. In anatomy, we must in fact distinguish between two classes of things: (1) The passive mechanical arrangements of various organs and apparatus which, from this point of view, are really nothing but instruments of animal mechanics; (2) the activity of vital units which put in play this diverse apparatus. The anatomy of corpses can certainly take account of the mechanical arrangements of the animal organism; inspection of the skeleton certainly shows a combination of levers whose action we understand solely through their arrangement. So with the system of canals or of tubes conducting fluids; and thus the valves in the veins have mechanical functions which put Harvey on the track leading to the discovery of the circulation of the blood. The reservoirs, the bladders, the various pockets in which secreted and excreted fluids reside, offer mechanical arrangements which more or less clearly indicate the functions which they must fulfill, without our necessarily having recourse to experiment on the living to learn it. But we should notice that these mechanical deductions are by no means absolutely restricted to the functions of living beings; we deduce everywhere, in the same way, that pipes are meant to conduct, reservoirs meant to hold and levers meant to move.

When we come to the active or vital elements which put all the passive instruments of the organism in play, then anatomy of corpses

[9] Cf. Claude Bernard, *Leçons de physiologie expérimentale*, Paris, 1856, Vol. II. *Leçon d'ouverture*, May 2, 1855.

cannot and does not teach anything. All our knowledge on this subject must necessarily come from experiment or from observation of the living; when, therefore, anatomists believe that they are making deductions solely from anatomy and without experiments, they forget that their starting point was the same experimental physiology which they seem to disdain. When anatomists deduce the functions of an organism, as they say, from their texture, they merely use knowledge gained on the living, to interpret what they see in the dead; but anatomy really teaches them nothing, it merely supplies them with the quality of a tissue.

So when anatomists meet with muscular fibres in some part of the body, they infer contractile motion; when they meet gland cells, they infer secretion; when they meet with nerve fibres, they infer sensation or movement. But what taught them that muscular fibre contracts, that gland cells secrete, that a nerve is sensory or motor, unless it was observation of the living, or, in other words, vivisection? Only, noting that these contractile, secreting or nerve tissues have definite anatomical forms, they establish a relation between the form of the anatomical unit and its functions, so that when they meet one, they infer the other. But, I repeat, dead anatomy teaches nothing; it merely leans on what experimental physiology teaches; and a clear proof of this is that, where experimental physiology has learned nothing as yet, anatomists can interpret nothing by anatomy alone. Thus, the anatomy of the spleen, the suprarenal glands and the thyroid is as well known as the anatomy of a muscle or of a nerve, and nevertheless anatomists are silent as to the uses of these parts. But as soon as physiologists have discovered something about the functions of these organs, anatomists will put the physiological properties noted into relation with their anatomical observations. I must also point out that anatomists, in their localizations, can never go beyond the teachings of physiology, except under penalty of falling into error. Thus, if anatomists, on the basis of physiological teaching, suggest that, where muscular fibres are present, there are contraction and movement also, they may not infer that, where they see no muscular fibre, there is never contraction or movement. Experimental physiology has proved, in fact, that contracting units are of various forms, among them some which anatomists have not yet been able to define.

In a word, to know something about the functions of life, you must study them in the living. Anatomy yields only characteristics by which to recognize tissues, but itself teaches nothing about their vital properties. How indeed could the form of the nerve cell show us the nervous properties which it transmits? How could the form of a liver cell show us that sugar is made in it? How could the form of a muscle fibre teach us about muscular contraction? We have here only an empirical relation established by comparative observation of the living and the dead. I remember having often heard de Blainville try to differentiate in his lectures between what should be called, according to him, a *substratum,* and what should be called, on the other hand, an *organ.* In an organ, according to de Blainville, we should be able to understand the necessary mechanical relation between a structure and its function. Thus, from the form of bony levers, he said, we conceive a definite motion; from the disposal of the blood of the reservoirs for liquids, and of the excretory ducts of glands, we understand that liquids are put in circulation or retained by mechanical arrangements that we can explain. But as for the encephalon, he added, no material relations can be established between the structure of the brain and the nature of intellectual phenomena. Therefore, concluded de Blainville, the brain is not the organ of thought, it is merely a *substratum.* We may accept, if we like, de Blainville's distinction, but if so, it will be general and not limited to the brain. Indeed, if we understand that a muscle inserted between two bones may act mechanically as a power drawing them together, we by no means understand how the muscle contracts, and we can just as well say that the muscle is the substratum of contraction. Though we understand that a fluid secreted by a gland flows out of its tubes, we cannot thereby conceive any idea of the essence of secretory phenomena. And we may just as well say that the gland is a substratum of secretion. To sum up, the anatomical point of view is wholly subordinate to the point of view of experimental physiology, as an explanation of vital phenomena. But, as we said above, there are two things in anatomy: the tools of the organism and the essential agents of life. The essential agents of life depend upon the vital properties of our tissues, which can be defined only by observation or by experiment on the living. These agents are the same in all animals, without distinction of class, genus or species. Here is the domain of general anatomy and physiology. Next come tools of life, which are nothing but mechanical tools or weapons with which nature especially provides each organism according to its class, its genus or its species. We may even say that the special tools constitute the species; for a rabbit differs from a dog only because one has organs that make it eat grass, and the other organs that force it to eat flesh. But as to the inmost phenomena of life, the two animals are identical. A rabbit is carnivorous if we give him meat ready prepared, and I long ago proved that all fasting animals are carnivorous.

Comparative anatomy is merely an inner zoölogy; its aim is to classify the apparatus or tools of life. These classifications should corroborate or rectify the characteristics suggested by outer forms. Thus the whale, which might be put with the fishes by reason of its outer form, is placed with the mammals because of its interior organization. Comparative anatomy shows us also that the tools of life are arranged in necessary and harmonious relations with the whole organism. Thus, an animal with claws should have the jaws, teeth and the articulations of the limbs disposed in a definite way. The genius of Cuvier amplified these views and derived from them a new science, paleontology, which reconstructs an entire animal from a fragment of his skeleton. The object of comparative anatomy, then, is to show the functional harmony of the tools with which nature has endowed an animal and to teach us the changes necessary in these tools according to various circumstances of animal life. But beneath all these changes comparative anatomy always shows us the uniform plan of creation; thus any number of organs exist, not as aids to life (they are often actually harmful), but as characteristics of the species or as vestiges of a single plan of organic composition. The stag's antlers have no use favorable to the animal's life; the shoulder blade of a slow-worm and the mammæ in males are vestiges of organs that have lost their functions. Nature, as Goethe said, is a great artist; to ornament forms, she often adds organs that are useless to life in itself, as an architect makes ornaments for his building, such as friezes, cornices and volutes which are useless for habitation.

The object of comparative anatomy and physiology is, therefore, to find the morphological laws of the tools and the organs which together make up organisms. Comparative physiology, in so far as it infers functions by comparing organs, would be an insufficient and false science if it rejected experimentation. Comparing the forms of limbs or of the mechanical apparatus of life may suggest the uses of these parts. But what can the form of the liver or the pancreas tell

us about the function of these organs? Has not experiment shown the mistake of likening the pancreas to a salivary gland? [10] What can the form of the brain or the nerves teach us about their functions? All that we know has been learned by the observation of the living, or by experiment. What can we say about fishes' brains, for instance, until experiment has clarified the question? In a word, anatomical deduction has yielded what it can. To linger in this path means lagging behind the progress of science and believing that we can impose scientific principles without experimental verification. That, in a word, is a relic of the scholasticism of the Middle Ages. But, on the other hand, comparative physiology, in so far as it relies on experiment and seeks the properties of tissues and organs in animals, does not seem to me to have separate existence as a science. It falls back necessarily into special or general physiology, since its object is the same.

We distinguish between the various biological sciences only by the goal which we set ourselves or the idea which we pursue in studying them. Zoölogists and comparative anatomists see all living beings as a whole, and by studying the outer and inner characteristics of beings, they seek to discover the morphological laws of their evolution and their transformation. Physiologists take a quite different point of view: they deal with just one thing, the properties of living matter and the mechanism of life, in whatever form it shows itself. For them, genus, species and class no longer exist. There are only living beings; and if they choose one of them for study, that is usually for convenience in experimentation. Physiologists also follow a different idea from the anatomists. The latter, as we have seen, try to infer the source of life exclusively from anatomy; they therefore adopt an anatomical plan. Physiologists adopt another plan and follow a different conception; instead of proceeding from the organ to the function, they start from the physiological phenomenon and seek its explanation in the organism. To solve the problem of life, physiologists therefore call to their aid all the sciences,—anatomy physics, chemistry, which are all allies serving as indispensable tools for investigation. We must, therefore, necessarily be familiar enough with these various sciences to know all the resources which may be drawn from them. Let us add, in ending, that from every

[10] Claude Bernard, *Mémoire sur le pancréas.* (*Supplément aux Comptes rendus de l'Académie des sciences.* 1856. Vol. I.)

biological point of view, experimental physiology is in itself the one active science of life, because by defining the necessary conditions of vital phenomena it will succeed in mastering them and in governing them through knowledge of their peculiar laws.

V. Pathological Anatomy and Dissection in Relation to Vivisection

What we said in the last paragraph about normal anatomy and physiology may be repeated for pathological anatomy and physiology. We find similarly three points of view appearing one after another, the taxonomical or nosological point of view, the anatomical point of view and the physiological point of view. We cannot here go into detailed study of these questions, which would include neither more nor less than the entire history of medical science. We shall limit ourselves to suggesting our idea in a few words.

While observing and describing diseases, men must have sought at the same time to classify them, as they sought to classify animals, and according to precisely the same principles, by artificial or natural methods. Pinel applied to pathology the natural classification introduced into botany by de Jussieu, and into zoölogy by Cuvier. It is sufficient to quote the first sentence of Pinel's *Nosography:* "Given a disease, to find its place in a nosological scheme." [11] No one, I think, will consider this the goal of all medicine; it is merely a partial point of view, the taxonomic point of view.

After nosology came the anatomical point of view; that is, after considering diseases as morbid species, men try to place them anatomically. It was thought that, just as there is a normal organization to take account of vital phenomena in the normal state, so there must be an abnormal organization to take account of morbid phenomena. Though the point of view of pathological anatomy can already be recognized in Morgagni and Bonnet, still it is especially in this century, under the influence of Broussais and Laennec, that pathological anatomy has been systematically built up. Men compared the anatomy of diseases, they classified changes in tissues, but they also tried to bring these changes into relation with the morbid phenomena and, as it were, to deduce the second from the first. The same problems presented themselves as in comparative, normal

[11] Pinel, *Nosographie philosophique.* 1800.

anatomy. In the case of morbid changes producing physical or mechanical alteration in a function, as for instance a vascular compression or mechanical lesion of a limb, men could understand the relation connecting the morbid symptom with its cause and could make what is called a rational diagnosis. Laennec, one of my predecessors in the chair of medicine at the Collège de France, immortalized himself in this field by the precision which he gave to physical diagnosis of diseases of the heart and lungs. But diagnosis became impossible in the case of diseases where changes were imperceptible with our present means of investigation. No longer able to find an anatomical relation, men said then that the disease was essential, i.e., without any lesion; which is absurd, for it amounts to acknowledging an effect without a cause. Men came to understand that, to find the explanation of such diseases, they must carry their investigations into the minutest parts of the organism where life has its seat. The new era of microscopic pathological anatomy was inaugurated in Germany by Johannes Müller; [12] and an illustrious professor in Berlin, Virchow, recently systematized microscopic pathology.[13] So in changes of the tissues, they found proper characteristics for defining diseases. *À propos* of this, they invented the name pathological physiology, to designate pathological function in relation to abnormal anatomy. I shall not have to consider whether these expressions, pathological anatomy and physiological pathology, are well chosen. I shall simply say that the pathological anatomy, whose pathological phenomena they define, is subject to the same objection of insufficiency that I have already made to normal anatomy. First, the pathological anatomists assume it proved that anatomical changes are always primary, which I do not admit, believing the contrary, that a pathological change is very often secondary and is the consequence or fruit of the disease instead of its germ; which does not prevent this product from later becoming a morbid germ of other symptoms. I shall therefore not admit that cells or fibres of tissues are always primarily attacked; a morbid physico-chemical change in the organic environment being able, in itself, to lead to the morbid phenomena, in the manner of a toxic symptom which occurs, without

[12] Müller, *De Glandularum secernentium structura penitiori earumque prima formatione in homine atque animalibus.* Leipzig, 1830.

[13] Virchow, *La Pathologie cellulaire basée sur l'étude physiologique et pathologique des tissus,* translated by P. Picard. Paris, 1860.