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VIVISECTION:

WHAT IT IS,

AND

WHAT IT HAS ACCOMPLISHED.

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[Read before the New York Academy of Medicine, December 13, 1866.]

MR. PRESIDENT AND GENTLEMEN OF THE ACADEMY—As the subject of vivisection has been recently brought to the public notice in a way calculated to excite especial interest in its merits and demerits, I have thought it appropriate to make it the topic of a brief address, and to examine the question of its propriety and usefulness, as a means of improvement in the medical art. For this purpose, I propose to discuss the objections which have been urged against it, and to show how far they are valid, and how far destitute of foundation.

The objections urged against vivisection are principally threefold; viz.:

First, that it is cruel;
Secondly, that it is liable to uncertainty and deception; and
Thirdly, that, in point of fact, it has not led to valuable results.

These three objections may be considered in succession.

First, as to its cruelty. The injustice of this charge may be appreciated, when we remember the aim and motive of all such experiments. Their object is the improvement of medical knowledge, and consequently the relief of human suffering and the cure of human diseases. Physiologists have sometimes been charged with recklessness and disregard of the lower creation in following out their passion for experiment; and it is said of one of them that he destroyed, in the course of his life, "no fewer than three hundred and seventeen animals," with this object. Now if we destroy, every year, four hundred thousand cattle in South America* for no other purpose than to supply ourselves with boots and shoes, it does not seem a very reckless or extravagant thing to sacrifice a comparatively small number of dogs and rabbits for the

* The number of hides imported into the United States, during the year 1865, from Buenos Ayres alone, was 400,978. From the whole of South America, 525,438.

acquisition of knowledge which is to benefit the human race. This is a legitimate and praiseworthy object; and experimental vivisection is no more open to the charge of cruelty on this account, than the dissection of human bodies for the study of anatomy is open to the charge of sacrilege and impiety.

Besides, it can never be properly the aim of the physiologist to inflict unnecessary suffering. On the contrary, it is desirable on every account to avoid doing so. No class of men can be more averse to the infliction of suffering than the physicians and naturalists who make the functions of the animal organism their special study; and none are better qualified to judge of the existence and degree of sensibility in particular cases.

It is perfectly true that vivisection may be and has been abused, in certain instances, by reckless, unfeeling, or unskilful persons. But this abuse of a practice, which is rare and exceptional, cannot with any reason be urged against the practice itself when conducted with judgment and propriety. I have myself witnessed this abuse, especially in the case of a veterinary institution in a foreign country, where the pupils were allowed to practise surgical operations on the living and sensitive animal, for the purpose of learning the operative manipulations. Nothing could be more shocking than the performance of these operations. Many of them were evidently cruel and unnecessary, and excited the hearty condemnation of all who visited the place. But this practice had nothing to do with physiological investigations. It was confined to veterinary institutions abroad; and neither at that time, previously nor since, so far as I am aware, has it ever been in any way resorted to in this country.

It is also proper to remember that the great majority of cases of painful operation on animals, for physiological experiment, date from a time anterior to the discovery of ether and chloroform, when surgical operations on the human subject were equally painful. Since the discovery of anæsthetics, their use has been adopted by the physiologist as promptly as by the surgeon; and in point of fact, at the present day, the great majority of physiological experiments are so performed as to be entirely painless. The protecting influence of anæsthetics, when fully administered, is absolute and complete, and the most extensive incisions may be performed without the animal experiencing the least consciousness of the operation. The infliction of pain, therefore, as a general rule in physiological operations, has no existence, and does not apply as an objection to the practice.

But there is a very small class of cases in which anæsthetics cannot be used; and these cases present a difficulty to the minds of some, who are ready to acknowledge the propriety of vivisection in other instances. For here, at least, the animal must feel. Not being etherized, he remains sensible; and that degree of pain which naturally attends the experiment cannot, in such a case, be avoided.

Now the difficulty which prevents the use of ether, in these cases, is a perfectly simple one. They are cases mainly of experiment upon the nervous system, in which it is required to ascertain the existence or non-existence of sensibility in particular parts. Of course, therefore, if the sensibility of the animal were already suspended by ether, there would be no means of judging of its existence or otherwise; and the operation could have no possible result. If the experiment be done at all, therefore, it must be done without the use of ether.

But a little examination will show how small, in reality, even in these cases, is the amount and frequency of unavoidable suffering; and how exaggerated are the pictures which have sometimes been drawn of their harshness and inhumanity. For, to begin with, it is not all experiments, even on the nervous system, which are accompanied by pain. Contrary to what might be anticipated, observation has shown that many of the nerves and nervous centres are insensible, and cause no pain when exposed and manipulated. Sir Charles Bell noticed many instances of this. In speaking of the two classes of nerves, and the sensibility of one of them, he says: "While, on the contrary, nerves not of this original class or system are comparatively so little sensible as to be immediately distinguished, insomuch that the quiescence of the animal suggests a doubt whether they be sensible in any degree whatever."*

Again he says, in the same connection: "If the fifth nerve and the *portio dura* of the seventh be both exposed on the face of a living animal, there will not remain the slightest doubt in the mind of the experimenter which of these nerves bestows sensibility." †

Also in reporting his *Experiments on the Nerves of the Face:* "An ass being thrown, the portio dura was divided on one side of the head. On the division of this nerve, the animal gave no sign of pain."‡

Again: "This experiment of cutting the respiratory nerve of the face, or *portio dura*, gave so little pain, that it was several times repeated on the ass and dog, and uniformly with the same result."§

In describing still another experiment, he says: "In cutting the *portio dura*, it was not evident that the animal suffered any pain at all."‖

This fact has also been often observed with regard to the pneumogastric nerve, which may be divided in the middle of the neck, without showing any sign of sensibility whatever, or one only of a very obtuse character.

* Bell, Nervous System of the Human Body, 3d Ed., London, 1844, p. 42.
† Ibid. p. 42.
‡ Philosophical Transactions. Vol. XXXI., p. 412.
§ Ibid. p. 413.
‖ Ibid. p. 417.

The same thing is true with regard to a large portion of the nervous centres. No fact is better established in physiology than that the entire substance of the hemispheres of the brain, the cerebellum, the olfactory ganglia, the corpora striata, and the tubercula quadrigemina, are completely insensible. Consequently when the cerebral lobes of a bird or quadruped are wounded or removed, this is an operation which produces absolutely no pain whatever. The truth is, that ordinary sensibility is localized, like other nervous functions, in particular parts of the nervous system, and is not to be found elsewhere. Accordingly, the experiments which have demonstrated this truth are doubly valuable: first, by showing the fact that sensibility is so localized in particular parts; and secondly, by showing upon what portions of the nervous system we may continue our investigations, without the production of pain to the animal.

Beside, in these cases of experiment on the nervous system, the preliminary operation of cutting through the flesh and subjacent parts may, in most instances at least, be performed under the influence of ether, and the final examination alone take place after the animal has recovered from his etherization. This reduces the actual infliction of pain to very narrow limits, both in respect of degree and duration.

This may easily be appreciated from what sometimes occurs in medical practice. Suppose we have a patient suffering from apoplexy and partial hemiplegia, and we wish to know whether sensibility remains unimpaired in the affected part. In order to determine this, we do not require to torture the patient, nor to subject him to any barbarous or extensive injury. Just so much impression is required as will reveal the presence or absence of sensibility; and, when that is done, the work is accomplished. Now the experiments upon the nervous system of animals, to which I allude, are of precisely the same nature. Their object is the same, and the means employed often identical. When sensibility exists, the pain which indicates it is momentary in duration, and need not be excessive or violent in degree; when it does not exist, of course there is no pain whatever.

I believe, therefore, that vivisection, when practised with care and judgment, is not open to the imputation of cruelty; but that both the objects for which it is undertaken, and the manner in which it is performed, place it beyond the limitations of such a charge.

The second and third objections which have been enumerated may be best examined in connexion with each other.

In order to place the matter in its proper light, let us therefore consider as briefly as possible: first, what experimentation upon living animals is; and second, what it has accomplished.

First of all, this method of study comprises all varieties of experiment performed upon living animals. The term "vivisection" does not fully express its character; for, although cutting operations are often a part of the means employed, they are not always so, and the essential nature and objects of the investigation are the same in either case. It makes no difference, of course, either for or against the propriety of this method, whether we experiment upon an animal by cutting his carotid artery, or by confining him in an atmosphere of oxygen or carbonic acid. The particular means employed vary, of necessity, with the special object to be accomplished. But, in all instances, the distinguishing character of experiments of this class is simply that they are performed upon the living body; and this distinguishing character is the same, whether we operate upon the animal by means of incisions and ligatures, by subjecting him to the respiration of particular gases, by confining him to a regimen of food, peculiar either in quantity or quality, or by the administration of drugs and medicines. The subject of discussion is not, therefore, vivisection in its narrowest sense, but the entire method of experiment upon living animals, as a means of study in physiology and the kindred sciences.

Now it may be stated, to begin with, that this method of investigation is the only one by which physiology can be successfully studied. This fact is now perfectly well settled by the results of long experience, and many attempts to accomplish the same end by other means. Physicians have often endeavored to guess at the vital properties of the internal organs, or to infer their functions from analogy; but these attempts have nearly always failed of their object, and have often resulted in complete deception. The physiologists who relied upon such means have been frequently misled in a direction exactly contrary to the truth; and the real solution of the question has only been reached afterward by a resort to direct experiment on the living body. In 1809, the anatomy of the spinal nerves and their double origin, by anterior and posterior roots, from the spinal cord, was already known. At that time, Alexander Walker conceived the idea on theoretical grounds that the anterior roots of these nerves were sensitive in their function, and the posterior roots endowed with the power of motion. It was only afterward, when Sir Charles Bell, Magendie, and others, experimented directly upon these parts, in various animals, that Walker's surmises were found to be exactly wrong; and that the anterior roots are in reality the organs of motion, and the posterior roots the organs of sensibility. After the discoveries of Lavoisier, in 1780, in regard to respiration, it was thought that the blood became heated in passing through the lungs. But subsequent experiments made it evident that the lungs are not perceptibly warmer than the other internal organs; and Bernard,* many years afterward, by carefully introducing thermometers into different blood-vessels in the living animal, showed that, in point of fact, the blood is a little warmer on the right side of the heart

* Cl. Bernard. Liquides de l'Organisme. Paris, 1859. Vol. I., p. 110.

than on the left, and accordingly that it becomes cooled rather than heated in its passage through the lungs.

These are some instances of the inefficacy of surmises or analogical inferences as a source of knowledge in physiology, when compared with direct experiment.

Now the reason why experiments on living animals are the only source of certain knowledge in physiology is a very simple one. Physiology is the study of the vital actions of the living body; and these actions do not take place anywhere but *in* the living body, and consequently cannot be observed or investigated anywhere else. A complete knowledge of anatomy may be obtained by dissection of the organs after death; but their vital functions are not to be learned in this way, because they have ceased, and cannot again be put in operation. The most remarkable facts have sometimes remained unknown or misunderstood for this reason. The lacteal vessels, for example, were discovered by Asellius, in 1622; and though they had often been seen in the bodies of recently killed animals subsequently to that time, it was thought, from their small size and the small amount of fluid contained in them, that the circulation of chyle must be very limited in quantity. But in 1856, when these vessels had been known to anatomists for over two hundred years, two French physiologists introduced a canula into the thoracic duct of living animals, continued the experiment while digestion and absorption were going on, and obtained in this way, in horses, from forty-five to fifty pounds of lymph and chyle in twenty-four hours, and in oxen from eighty to one hundred pounds in the same time,* a quantity which was never before even suspected; and revealed in this way an activity of one of the vital functions far beyond anything which could be surmised from examination of the dead body.

The chemical actions, also, which go on during life are only to be learned by their investigation in the living body. For the chemical reactions which take place in the test-tube and crucible, by the manipulations of the chemist, are modified and altered in the interior of the body by the various influences which surround them there; and it is impossible to guess what these alterations will be, until we try the experiment directly on the animal during life. Any endeavor to anticipate or infer these changes, without direct experiment, always results in a mistake, and retards our knowledge instead of advancing it. It is perfectly true that examinations of the dead body and ordinary chemical experiments are exceedingly useful as preliminaries, by suggesting probable ideas or supplying convenient materials; but the final question must always be decided by an examination of the functions as they actually take place during life.

* Colin. Physiologie Comparée des Animaux Domestiques. Paris, 1856. Vol. II., p. 100.

It is therefore certain that if our knowledge of physiology is to be advanced at all, it must be by means of experiment on living animals; *since there is no other method upon which we can rely.* There is no other way of learning what the vital functions really are, than by seeing how they take place while life is actually going on. We might as well expect to learn the phenomena of magnetism by experimenting with substances not magnetic, as to study the phenomena of life anywhere but in the actions of the living body.

In the next place, the method by experimenting upon the living animal is capable of being employed with judgment and practised with success. This has not always been conceded to it; and the objection is sometimes made that it is, for various reasons, uncertain and fallacious, and that it is unlikely to lead, in any case, to valuable results.

The most frequent form in which this objection presents itself is that, in operating upon the living animal, we violate and disturb the vital functions to such a degree that they exhibit unnatural appearances, and so lead to erroneous conclusions. It is said that the necessary steps of the experimental operation—the incisions, ligatures, mutilations, etc.—place the organs in an unnatural condition, and interfere with the regular performance of their functions;—so that they vitiate the results of the experiment, and render them worthless or deceptive.

This objection, however, is one which certainly would never be made by a practical experimenter, for the reason that the difficulty to which it refers is always present to his mind, and one against which he guards by unremitting precautions. This is the very first and simplest lesson which is learned by the experimental physiologist. We might as well object to the researches of the astronomer that the refractive power of the atmosphere distorts the rays of light passing to his telescope, and thus vitiates his observations. The astronomer is perfectly familiar with this refractive power of the atmosphere. He knows the mode of its operation, and makes due allowance for it in his calculations. So the physiological experimenter, in prosecuting his observations on the living body, sees at once that the external agents which he employs in his operations, themselves exert a certain amount of influence in producing the result. It is always his object, in arranging an experiment, to reduce these disturbing influences to the smallest possible compass. It is frequently necessary to vary the method of procedure for the same experiment, in order to determine how much, if any, of the final result is attributable to the principal conditions, and how much to the accessory manipulations. Abundant instances of this are to be found in the history of investigation upon almost every physiological question; and he would be a bungling experimenter indeed who should leave out of consideration the physical conditions which he had himself employed in operating upon the living body.

This objection, therefore, is not something which has been neglected or overlooked by physiologists; but, on the contrary, it is better understood by them than by any one else. In order to illustrate how old a thing this objection is, and how frivolous a form it has sometimes assumed, let me quote an instance which happened in connexion with the discovery of the circulation of the blood. In the time of Harvey, two hundred and forty years ago, it was believed that the blood in the veins moved from the heart outward toward the extremities. Harvey asserted that it moved in exactly the opposite direction, viz., from the extremities toward the heart; and the proof of this was that when a vein was divided, while the circulation was going on, all the blood flowed from the end near the extremities, whence it was coming, but none of it from the end toward the heart, whither it was going. But this plain experiment did not satisfy his opponents, who made to it the objection which we have been considering; and Harvey replies to them in this way:—

"And that no one," he says,* "may seek shelter in asserting that these things are so when nature is disturbed and opposed, but not when she is left to herself and at liberty to act; that the same things do not come to pass in morbid and unusual states as in the healthy and natural condition; they are to be met by saying that if it were so, if it happened that so much blood was lost from the further orifice of a divided vein because nature was disturbed, still that the incision does not close the nearer orifice, from which nothing either escapes or can be expressed, whether nature be disturbed or not."

It was Harvey, therefore, who appreciated fairly the influence of the disturbing causes in this case; while those who distrusted him were reduced to the preposterous assumption that cutting a vein toward the heart prevented the blood from flowing out of it, and that cutting it toward the extremities produced a current which did not previously exist. The physiologist, therefore, was right, and his opponents were wrong.

We must recollect, also, that mechanical contrivances and operations are an absolute necessity in physiological experiment. If the functions and processes of the living body were plainly exposed to view and capable of being learned by simple inspection, there would be no need of experiment of any kind. It is because they are, in fact, concealed from observation, and carried on in the secret recesses and cavities of the body, that we are obliged to resort to experiment for their investigation. Even for the most palpable and external of the vital phenomena, as, for example, the inhalation and exhalation of gases by the breath, some artificial contrivance is necessary in order to secure accuracy in our results. These artificial contrivances, accordingly, when properly used, so far from being a source of deception for the physio-

* Works of Harvey. Sydenham Edition. London, 1847, p. 121.

logist, are the necessary instruments by which he is enabled to accomplish his ends.

There are other difficulties, however, beside that just mentioned in the way of physiological investigation; and they require to be met by certain plain and simple practical rules, which are perfectly well known to physiologists, and universally adopted by judicious experimenters. Physiological experiment in fact is an art, which is to be learned, like other arts, and practised with care and judgment in order to be successful. Let me mention some of these difficulties, and the means by which they are avoided.

I. In the first place, the principal peculiarity which distinguishes the phenomena of organic life is that they are *complicated.* The result of a physiological experiment is often not a simple one, as in physical and chemical investigations, but depends on the combined operation of various forces, each of which exerts its own share of influence. The same phenomenon, therefore, is susceptible of various interpretations, until we have mastered all the elements which combine for its production. If the pneumogastric nerves, for example, which are distributed to the lungs, be cut off in the middle of the neck, this operation after some days stops respiration and produces a peculiar engorgement and solidification of the lungs, by which they are rendered incapable of supporting life. But this is not owing to the direct influence of these nerves upon the lungs; for they also send branches to other organs, and espcially to the larynx, by the recurrent or inferior laryngeal nerves. The natural movements of the larynx in respiration are necessary to the healthy action of the lungs. The substance of the lung, accordingly, after division of the pneumogastrics, is solidified and engorged, not because its own nerve has been cut off, but because the branch distributed to the larynx has been cut off at the same time. The proof of this is, that if the recurrent laryngeal nerve be divided on both sides, the pneumogastrics remaining uninjured, the lungs after a time present the same kind of engorgement and solidification as in the former case.

A similar relation of this nerve and its branches was recognised by Galen, while studying the influence of the pneumogastrics in the formation of the voice.

"A lesion, accordingly," as Galen says,* "of these latter nerves destroys the voice, because the special nerves of the throat, which I call the 'recurrent nerves,' form a part of their substance. And though the former are distributed to many different parts, the latter, which belong especially to the vocal organs, have a particular claim to be regarded as the vocal nerves."

II. In the second place, the vital phenomena are *variable* at different times. The living body is not an inert structure, composed of unchangeable materials. It is an active and impressible organ-

* Galen, De Locis Affectis. Book I., Chap. VI.

ization, susceptible to the influence of external causes, and subject also to variations of an internal and periodical nature. It is most necessary, therefore, in performing a series of experiments for any purpose, that the surrounding conditions should be always absolutely identical. The quantity of material used, the quality of the instruments, the time and order in which the different steps of the operation are performed, should all be arranged with precision; for any of these circumstances are liable to influence the result. If a quantity of venous blood be drawn from the ox, and shaken up with atmospheric air, it immediately changes color, and assumes a bright arterial hue. But if another specimen of blood be drawn from the same animal in the same way, and allowed to remain at rest for forty-eight hours, and then shaken up as before, but little or no change in color takes place; for the blood, after being drawn from the vessels, suffers an alteration which renders it less susceptible to the reddening influence of the air.

The condition of the animal as to rest and activity, youth or age, fasting or digestion, will exert a similar influence. Blood drawn from the portal vein, while fasting, will present a clear and transparent serum. If drawn from the same vessel during digestion, its serum will be turbid and milky from the absorption of chyle. The blood of the renal veins, while the kidneys are at rest, is dark-colored like the average of venous blood. When these organs are in active secretion, it is bright and florid, like arterial blood, and circulates with greater rapidity than before. Consequently, the blood of the portal vein and that of the jugular vein, if examined in the same animal while fasting, will be alike; if examined during digestion, they will be different. If the blood of the renal veins be examined while the kidneys are at rest, it will be venous in color; if examined while they are active, it will be arterial.

These variations, therefore, in the physiological phenomena, are not productive of deception when properly examined; but on the contrary, are the sources of greater knowledge, and lead to further discoveries. All the natural phenomena are variable to a certain extent, but they vary according to certain definite laws, which may be discovered and understood by proper investigation.

It follows from this that any apparent contradiction in physiological experiment, instead of destroying the value of its results, may in reality enhance our knowledge, and open new paths of inquiry. This has very often happened in physiology, and has been productive of the most valuable discoveries. It would therefore be a most injudicious thing for the experimenter to neglect or throw aside any phenomena which he may notice, because they appear to be in opposition to others previously observed. This would be entirely wrong; for, provided both sets of observations be carefully made, the last is as important as the first. To neglect a fact actually observed is as much a falsehood in science as to imagine one that has never been seen. Neither the new nor the old observation, accordingly, is to be disregarded, but equal value should be given to both. If there be any apparent discrepancy between them, they will certainly be reconciled by subsequent investigation.

III. Thirdly, in experimenting upon living animals, there is a certain caution to be observed in comparing the results of the same experiment in *animals of different species.* This is very important; first of all, because different animals vary in their mode of life, in the details of anatomical structure, and in the activity of special functions,—and also because they all differ, more or less, from the human species, whose physiology is the particular object of our study. Now the rule to be adopted for our guidance in this respect, is as follows: Every experiment on the living body has usually two different results, viz. a *direct* result, and an *indirect* result; the direct result being due to its effect on the particular organ interested, while the indirect result follows, in a secondary manner, upon the disturbance caused among several associate organs, and is due to the relation existing between them.

Thus, the division of the spinal cord causes immediately paralysis of motion and sensibility in the parts below. The paralysis of the bladder also causes retention of the urine, consequent decomposition of the urea into carbonate of ammonia, an irritating condition of this fluid, and so, lastly, inflammation of the urinary passages. Paralysis of the lower extremities is therefore a direct effect of division of the spinal cord;—inflammation of the bladder is its indirect effect.

Now it is found that in all animals having the same general structure, the direct effect of a physiological experiment is the same, or varies only in rapidity, intensity, and duration. It is therefore always safe to draw conclusions from the resemblance of these direct effects in different species. The whole usefulness of physiological experiments depends upon the fact that the general structure and functions of the animals employed for that purpose are the same. In all the higher animals there is the same general arrangement of the skeleton, the same structure of the spinal cord, a similar distribution of the nerves; and the same principal organs of circulation, respiration, and digestion. The only difference between them is in the relative size and proportion of different parts, but not in their essential character. Their functions accordingly are identical. In all these animals the anterior roots of the spinal nerves are motor, and the posterior roots sensitive. In all, the lungs absorb oxygen and exhale carbonic acid. In all, the stomach secretes an acid gastric juice which dissolves the food. In all, the liver produces sugar, by the process of nutrition and secretion.

But when we come to observe the indirect results of an experiment, we find that it varies essentially in different animals, owing to the variable importance of different parts. Thus in all animals, division of the spinal cord will produce paralysis of the hinder extremities; but it will only produce inflammation of the bladder in those animals in which the urine contains urea. Division of the seventh pair of nerves will paralyze the muscles of the face in all species; but in the horse it also produces a serious impediment to respiration, because this animal can only breathe through the nostrils, and the movements of the nostrils are therefore more important in him than in other species. The direct effect of an operation, accordingly, which is always the same, is generally, also, the most important for our purposes; the indirect effect, which varies in different species, is often useful in explaining the peculiarities of a particular function.

It is evident, therefore, that we must exercise a certain reserve in applying the results of an experiment on the lower animals to man, or to animals of different species. If these results be direct and immediate in the mode of their production, it is always safe to make application of them to the human subject; if they be indirect, we cannot do so with certainty until we have learned all the successive steps by which they are produced.

IV. Finally, it is very essential for the experimental physiologist to be always acquainted with the *individual peculiarities* of the animals upon which he operates; and for this purpose he should keep them under his observation for a certain time before performing an experiment. For particular animals are distinguished by individual peculiarities, to a certain degree, like the members of the human species. These peculiarities relate not only to the excitability of the nervous system, the degree of intelligence and the mental disposition, but they affect the entire organization in its functions of nutrition, secretion, muscular contractility, etc. One animal will habitually feed with appetite and relish, devouring a large quantity and digesting it readily. Another takes his food sparingly, is fastidious as to its quality, and requires a much smaller quantity than the first, though to all appearance equally well nourished. In one animal the secretions are either more abundant or more easily excited than in another, the senses more acute, the motions more varied and vigorous. It is particularly in experiments upon the nervous system, in which the effect of an operation is indicated so much by the state of the disposition, the posture, manner of using the limbs, etc., that previous observation of the individual habits of the animal is requisite; for where the effect of an experiment is to be seen in the increase or diminution of a natural function, in order to appreciate these changes we must have a proper standard of comparison in the ordinary habits of the animal.

I have thus endeavored to show what experimentation upon living animals is, and in what manner it is successfully practised. Let us now see what it has accomplished.

This kind of experiment is by no means a new thing. It has been resorted to, at various times and by different observers, for over seventeen centuries; and the periods at which medical science has especially advanced have been exactly those periods in which it has been most assiduously employed. Its results, both direct and indirect, have been incorporated with the mass of our medical knowledge, and have become so thoroughly a part of our scientific patrimony, that the source from which they were derived is often overlooked. But, in point of fact, it is not too much to say that *every important discovery in physiology has been directly due to experiments on living animals;* and that we owe many of those in practical medicine, surgery, and hygiene, either directly or indirectly to the same source.

Let us enumerate some of these discoveries, and recall the manner in which they have been made.

I. First, the *Circulation of the Blood.* From the time of Hippocrates down to the middle of the second century, it was supposed that only the veins contained blood, while the arteries were thought to be tubes for the conveyance of air. This was a very natural conclusion from such examinations as were made at that time. For those who opened the bodies of men or animals after death found the veins full of blood, while the elastic arteries, having emptied themselves into the veins and capillaries, resumed their cylindrical form as soon as they were cut across and the air admitted to their interior; so that the anatomist, seeing the open and gaping mouths of these vessels, concluded that they were air tubes, and called them "arteries" on that account. There were then, as every one supposed, two kinds of vessels distributed throughout the body, viz. the veins for conveying blood, and the arteries for conveying air.

This error in regard to the arteries was detected by Galen, who was accustomed to experiment upon animals, about the year 150. In the first place, he opened the arteries while the blood was still in motion, and showed that, however small the orifice, it was only blood that escaped, and not air. This, however, did not satisfy his opponents, who maintained that, in reality, air escaped first from the wound, only that it was invisible, and that the blood, which was also discharged, was drawn into the artery from distant parts after the vessel had emptied itself of air. To meet this objection, Galen exposed an artery and included a portion of it between two ligatures. Now, if the vessel were opened between the ligatures, and if it in reality contained only air, nothing but air could escape from it, as no blood could then be drawn into it from distant parts. Galen opened the vessel and found that blood escaped as before.

"For we have often," he says,* "exposed the large arteries convenient for this purpose, and asked the disciples of Erasistratus whether the artery thus exposed did not seem to contain blood.

* Galeni Opera. An Sanguis in Arteriis Natura contineatur.

They were obliged to confess that it did, both because Erasistratus asserted that the blood passed into the arteries when they were uncovered, and because the fact was evident to the senses; for, having placed ligatures on both ends of the inclosed portion of the artery, and made an incision into the vessel between them, I showed that the artery was itself full of blood."

This was the first great discovery made in regard to the circulation, viz. that the arteries, instead of serving to distribute air, are in reality filled with blood, which moves through them to its destination.

But the remainder of the subject was still involved in obscurity. The ancients were then acquainted with only two main facts in this respect, viz. the introduction of air into the lungs, and the movement of the blood in the vessels; but how these functions were accomplished, and what was their mutual connexion, was almost unknown. Their idea of the circulatory system, as improved by Galen, was this: The mesenteric veins, coming from the intestine to the liver, brought to that organ the chyle, to be worked up in its interior into the materials of the blood. The blood, thus formed in the liver, was thence conveyed to the right side of the heart. After its arrival, a part of it was supposed to be filtered through small openings in the septum of the ventricles, and so conveyed into the left cavities of the organ. The blood which remained in the right cavities was venous blood, and was immediately distributed, by the veins, throughout the body to the different organs and tissues. That portion which had passed through the septum of the heart was elaborated, in the left ventricle, into a new and more active kind of blood, called "*spirituous*" blood,—the same which we now know by the name of "arterial." So the left ventricle was regarded as an organ for the elaboration of "spirits," as the liver was supposed to be an organ for the elaboration of the blood; and by a mixture of the two was produced "spirituous" or arterial blood, first fully formed in the left ventricle.

Here, then, were two kinds of blood, venous and arterial, contained respectively in two different sets of vessels—the veins and the arteries; but, according to Galen and his followers, both these two kinds of blood were conveyed from the heart, as a centre, outward to the organs and tissues, the venous current running in the same direction with the arterial, so that every organ was supplied, as he supposed, with two kinds of blood—venous blood by the veins, and arterial blood by the arteries.

This was the state of medical knowledge, in regard to the circulation, for more than a thousand years. It was somewhat modified, in the sixteenth century, by Vesalius and Servetus, who maintained that there were no openings in the septum of the heart, leading from one side to the other, but that this communication took place through the vessels of the lungs, so that that portion of the blood which passed from the right side of the heart to the left must make its way through the pulmonary vessels and not through the substance of the heart itself. Still this doctrine attracted but little attention, and is acknowledged to have had little or no influence on the progress of physiological ideas.* Nothing, in fact, was changed except the place for the elaboration or spiritualization of a portion of the blood; this elaboration taking place, as Servetus said, not in the heart, but in the lungs.

The direction in which one-half the entire blood was supposed to move was still exactly wrong; and the blood circulated obstinately in the veins from within outward, in all books of anatomy and physiology of the sixteenth century. Even when Fabricius of Aquapendente discovered by dissection that the veins contained valves, opening toward the heart and shutting backward toward the extremities, neither he nor his pupils ever thought of any other use to attribute to these valves than that of preventing a too rapid downward current toward the limbs. So impressed were they with this fixed idea, that the blood passes from within outward along the veins, that, with the valves actually under their eyes, they did not see the physical obstacle thus presented to any current in an outward direction.

Notwithstanding the valves, therefore, the blood continued to run the wrong way for three-quarters of a century after the time of Vesalius and Servetus.

But early in the seventeenth century, Harvey began his investigations, and accomplished more than had been done by the accumulated labor of centuries. His is universally acknowledged to be the most brilliant discovery ever made in physiology; and it is difficult to imagine how any other single one of equal importance can be made hereafter. For his researches changed the whole aspect of one of the most essential functions of the living body, and showed how thoroughly its nature had been misconceived ever since the periods of antiquity.

From what has already been said, it would be natural to suppose that Harvey supplied the existing deficiency, and made everything right that was before wrong, by the simple discovery of the true motion of the blood in the veins, and its direction from the extremities toward the heart. This, at present, seems to be all that was wanting to clear up the previous obscurity, and, at the same time, the most palpable fact yet to be discovered. It appears to us almost the first thing with which a new investigator would necessarily commence.

But it happened quite differently. Harvey began with the motions and function of the heart; and the direction of the venous current, instead of being the fact with which he commenced, formed in reality the completion or termination of his work, in the order of time.

In point of fact, the movement of the blood in the veins was

* Milne-Edwards. Physiologie. Paris, 1858. Vol. III., p. 17.

not the only thing in which the older physiology of the circulation was at fault. For the true mechanism of the heart and blood-vessels was also entirely unknown. No one had any idea what caused the blood to move, nor why it moved in any particular direction. There was no definite knowledge of the physical forces of the circulation. Harvey was sensible of this deficiency, and accordingly he set to work to examine for himself, and to see by actual inspection what was in reality the action of the heart and blood-vessels. The account which he gives of his own attempts in this work, his difficulty in at once understanding what he saw, and the mode in which he gradually came to comprehend the appearances and action of the parts, is a masterpiece for faithful and graphic description, and for the candor and sincerity of purpose which appear in every word.

"When I first gave my mind to vivisections," he says,* "as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection and not from the writings of others, I found the task so truly arduous, so full of difficulties, that I was almost tempted to think, with Frascatorius, that the motion of the heart was only to be comprehended by God. For I could neither rightly perceive at first when the systole and when the diastole took place, nor when and where dilatation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning; so that the systole presented itself to me now from this point, now from that; the diastole the same; and then everything was reversed, the motions occurring, as it seemed, variously and confusedly together. My mind was therefore greatly unsettled, nor did I know what I should myself conclude, nor what believe from others; and I was not surprised that Andreas Laurentius should have said that the motion of the heart was as perplexing as the flux and reflux of Euripus had appeared to Aristotle.

"At length, and by using greater and daily diligence, and collating numerous observations, I thought that I had attained to the truth, that I should extricate myself, and escape from this labyrinth, and that I had discovered, what I so much desired, both the motion and the use of the heart and arteries. Since which time I have not hesitated to expose my views upon these subjects, not only in private to my friends, but also in public, in my anatomical lectures, after the manner of the Academy of old."

Harvey, then, begins with the action of the heart; and he tries, as he says, "with greater and daily diligence," to understand what it is. He sees that the heart shows alternately, and with rapid recurrence, two different appearances, a pause and a motion, incessantly repeated one after the other. In the pause it is "soft, flaccid, and lying, as it were, at rest." In the motion it is hard,

* Harvey's Works, Sydenham Ed., p. 19.

erected, rises upward to a point, and "looks narrower and more drawn together." In the pause it is of a deep blood-red color; and in the motion it becomes paler, as if emptied. He thus concludes that its motion is one of tension and constriction, like a muscle, and that at this time it drives the blood out of it by contraction.

Now, this was the very opposite of what was before supposed; for it was then thought that the stroke or impulse of the heart happened at the time that it was dilated or filled with blood; not, as it seemed to Harvey, when it was emptied. But Harvey did not undertake this labor merely to replace one hypothesis by another, nor was it his disposition to make a bargain with probabilities. His genius led him at once, and without hesitation, to the positive decision of this point, by making an incision into the ventricle and seeing when the blood was actually expelled.

"But no one," he says, "need remain in doubt of the fact, for if the ventricle be pierced, the blood will be seen to be forcibly projected outward, upon each motion or pulsation, *when the heart is tense*."

Having settled this point, Harvey goes on to the arteries; and, comparing their dilatation and contraction with that of the heart, he sees that the two sets of motions alternate with each other—that the systole of the heart corresponds in time with the diastole of the arteries, and the systole of the arteries with the diastole of the heart; and that if an artery be punctured, the blood is driven out from it at the time when the heart contracts, and when the artery itself, therefore, is dilated.

Accordingly he concludes that the impulse, or diastole, of the arteries is due to the shock of the blood driven into them by the contraction of the heart.

He gives his account of the motion of the blood through this part of the vascular system, in a manner equally striking and artistic, and with the most admirable faithfulness of detail. "Even so," he says, "does it come to pass with the motions and action of the heart, which constitute a kind of deglutition, a transfusion of the blood from the veins to the arteries." "And if any one," he continues, "bearing these things in mind, will carefully watch the motions of the heart, he will perceive not only all the particulars I have mentioned, viz. the heart becoming erect, and making a continuous motion with its auricles, but farther a certain obscure undulation and lateral inclination in the direction of the axis of the right ventricle, the organ twisting itself slightly in performing its work. And, indeed, every one may see, when a horse drinks, that the water is drawn in and transmitted to the stomach, at each movement of the throat; the motion being accompanied by a sound, and yielding a pulse, both to the ear and touch; in the same way it is with each motion of the heart, when there is the delivery of a quantity of blood from the veins to the arteries, that a pulse takes place and can be heard within the chest."

Now, at the present day, the comparison of the arterial current with the deglutition of water, through the gullet of a horse, seems a coarse illustration of a familiar and delicate function; but when employed by Harvey, it was the bold and forcible expression of an entirely unknown truth. For at that time, though the course of the blood through the arteries was known, the pulsation, both of these vessels and of the heart, was altogether misunderstood. The heart and arteries were thought to dilate simultaneously, by an expansion, somewhat like that of the chest in respiration; and the object of this motion was regarded, in a vague sort of way, as a kind of fanning or refrigeration of the blood. The action of the heart, as an *impulsive* organ, was entirely unknown, and the ventricle was considered, by the older anatomists, only as a place for the production and elaboration of what they called "spirits," and "spirituous blood." If Harvey, therefore, had done no more than this, he would still have taken rank as one of the most eminent of medical discoverers.

But this was simply the beginning of his investigations, and an introduction to his main discovery.

The venous blood was still thought to be distinct from the arterial blood. It passed, from the vena cava and right side of the heart, round through the lungs to the left ventricle; and from the left ventricle was driven through the arterial system, to the various parts of the body. But that was all. What became of it subsequently was unknown, until Harvey, led onward by his first investigations, began to think, as he expresses it, "whether there might not be a motion of the blood, as it were, in a circle," the whole blood passing from the arteries to the veins in the extremities, and again from the veins to the arteries in the cavity of the chest.

Now it is curious to observe in what way Harvey was at last led to comprehend this truth, and to convince himself of its reality. *It was by estimating the quantity of blood passing through the heart in a given time.* Harvey saw that a considerable quantity of blood was driven out of the heart at each pulsation. Estimating this quantity at its lowest possible amount,—half an ounce, three drachms, or even one drachm, according to the size of the heart,—he was struck with the remarkable fact that, in the course of half an hour or an hour, more blood passed through the heart, from the veins to the arteries, than was contained in the whole body. As this passage was incessantly going on with the same rapidity, it was obvious that the necessary quantity of blood could not be supplied, as Galen had supposed, from the food and drink, since it would require at least forty or fifty times as much food as is taken in the whole course of twenty-four hours. Harvey therefore came to the conclusion that the same blood which passes outward in the arteries is brought back, by a returning current, in the veins. Finally, he came to the positive demonstration of this returning current, by opening the veins,* and

* Harvey's Works, Sydenham Edition, pp. 50, 53, 65, 120, 141.

tying and compressing them at various points;—and thus saw and proved that the blood returns to the heart by the veins, and so, following a continuous round, performs the incessant movement of the circulation.

Such was the history of the discovery of the circulation. Let us now pass to that of *Respiration.*

II. The first important acquisition in regard to respiration was made by Sir Robert Boyle, in 1670.* He employed for this purpose the air-pump, then recently invented, by which he was enabled to exhaust the air from a closed receiver. He experimented on kittens, birds, frogs, fish, snakes, and insects; and showed that in all these animals the presence of atmospheric air is necessary to the maintenance of life, and that when stupefied by its withdrawal they may be resuscitated by reädmitting it. This was confirmed, in a more positive manner, in regard to fish, by the Swiss experimenter, Bernouilli,† who found that ordinary water contains atmospheric air in solution, and that when deprived of this by boiling, it becomes unfit for the respiration of fish. In this way it was established that in all species, both terrestrial and aquatic, it is the atmospheric air which is essential to respiration.

The next important fact discovered was that atmospheric air, by continued breathing, becomes vitiated and unfit for respiration. This was shown first by Boyle, who, by confining animals in a closed vessel, found that after a time they became suffocated, and that, in order to keep up respiration, it was necessary to remove the vitiated air and introduce a fresh supply.

The third point was established by Mayow,‡ in 1674, who, by experimenting upon mice, found that by their respiration the air was not only vitiated, but also diminished in volume. He measured the amount of this diminution, and found that it was seven per cent. of the whole volume of the air. He concluded that something in the air was used up or consumed by respiration, and he gave to this substance the name of "nitro-aëreal spirits."

Fourth, Priestley, in 1772,§ continued these experiments on air vitiated by respiration, in order to determine how it might be regenerated and again made capable of supporting life. He inclosed mice in tight receivers until the air was thoroughly vitiated and rendered unfit for respiration. He then subjected this air to a great variety of processes, and again tested it by introducing into it other mice. He tried, in this way, the effect of long keeping, of the application of a high temperature, condensation, rarefaction, and contact with vegetable mould; all without success. Finally, he found that by making plants grow in it the vitiated

* Boyle. Philosophical Transactions, Vol. V., pp. 2011-2055.
† Milne Edwards. Physiologie. Paris, 1857, Vol. I., p. 382.
‡ Ibid., p. 389.
§ Philosophical Transactions, Vol. LXII., p. 147.

air became regenerated, and again fit for the respiration of animals. He thus established the reciprocal relation of animal and vegetable respiration; viz. that animals, by breathing, deprive the atmospheric air of a certain property which is restored to it by vegetation.

Black had already, a few years before, studied the properties of carbonic acid—which he called "fixed air," because it is fixed in the calcareous and alkaline carbonates—and found it to be a product of the respiration of man and animals.

But the greatest advance in our knowledge of this function was that made by Lavoisier, from 1775 to 1780;—when he established, at almost the same time, the true composition of the atmospheric air and the part which it plays in respiration. His experiments, so far as they relate to our present subject, may be briefly stated as follows. He deoxidized atmospheric air by the calcination of mercury, and found: First, that animals could no longer breathe in it; and secondly, that it had diminished in volume. He concluded that the atmosphere was composed of two airs or gases; one respirable, the other non-respirable; that the respirable air was that which had combined with the mercury (oxygen), and the non-respirable that which was left behind. He then collected the oxide of mercury, reduced it again to the metallic state, examined the gas which was evolved in this process, and found that it amounted to exactly the same volume by which the air had been previously diminished. He then added this evolved gas to the non-respirable residue, and found that the mixture had again become respirable, and that animals no longer died in it. These experiments were performed with sparrows.

He then went further, and examined more closely the air which had been vitiated by continued respiration. He saw that, in its incapacity to sustain life, this vitiated air resembled that which had been simply deoxidized by the calcination of mercury. But, on further examination, he found two points of difference between these cases, viz. First, that the diminution of volume was much less in one case than in the other; and secondly, that the air vitiated by respiration precipitated lime-water and consequently contained carbonic acid, while the other did not. He found also, that, in order to renovate the air which had been vitiated by respiration, two things must be done. Not only we must take away from it, by means of lime or a caustic alkali, the carbonic acid which it contains, but we must also restore to it a quantity of oxygen equal to that which it has lost. He established in this way the following important conclusions:

First, that respiration acts only on the respirable portion of the air, or oxygen, while the remainder, or nitrogen as we call it, is entirely passive in the process; and secondly, that when animals are confined in a limited space, they die when they have absorbed, or converted into carbonic acid, the greater part of the oxygen, and so reduced the atmosphere to the state of an irrespirable or mephitic gas.*

In this manner were developed the two main features in the process of respiration, viz. absorption of oxygen and exhalation of carbonic acid. Other details of interest and value have since been added by other observers, relating to the time, place, quantity, and manner of the production and disappearance of these two gases. But the principal facts were established at that time; and our real knowledge of the function of respiration dates from Lavoisier, as that of the circulation dates from Harvey.

There are two operations immediately dependent on the functions of circulation and respiration, the history of which is directly connected with our present subject.

III. One is that of *Transfusion of the Blood* The earliest form in which the idea of transfusion presented itself was that of injecting into the blood-vessels certain medicinal agents. This was first done in England about the middle of the seventeenth century, under the auspices of Sir Robert Boyle; at which time it was shown that a solution of opium might be injected into the blood-vessels of a dog, so as to produce a narcotic effect upon the brain without killing the animal.

After the feasibility of this operation had been established, it excited much interest in various parts of Europe. It seems even that the priority of the discovery was a matter of some dispute; for there appeared in the Philosophical Transactions of the Royal Society of London, for 1665, the following communication, entitled,

"*An Account of the Rise and Attempts of a Way to conveigh Liquors immediately into the Mass of Blood.*

"Whereas, There have lately appeared in publick some *Books* printed beyond the Seas, treating of the Way of *injecting Liquors into Veines;* in which Books the *Original* of that *Invention* seems to be adscribed to others besides him to whom it really belongs: It will surely not be thought amiss if something be said whereby the true *Inventor's* right may beyond exception be asserted and preserved: To which end there will need no more than barely to represent the *Time* when and the *Place* where and among whom it was first started and put to tryal. To joyn all these circumstances together 'Tis notorious that at least six years since (a good while before it was heard off that any one did pretend to so much as thought of it) the Learned and Ingenious *Dr. Christopher Wren* did propose, in the *University of Oxford* (where he is now the worthy Savilian Professor of *Astronomy*, and where very many curious Persons are ready to attest this Relation) to that noble Benefactor to Experimental Philosophy, *Mr. Robert*

* Lavoisier. Expériences sur la Respiration des Animaux. Mémoires de l'Académie des Sciences Paris, 1777.

Boyle, *Dr. Wilkins* and other deserving Persons, that he could easily contrive a way to conveigh any liquid thing immediately into the Mass of Blood; videl.: by making ligatures on the Veines, then opening them on the side of the ligature towards the heart, and by putting into them slender Syringes or Quills, fastened to Bladders (in the manner of Clyster-pipes) containing the matter to be injected; performing that operation upon pretty big and lean Doggs, that the vessels might be large enough and easily accessible.

"This proposition being made, Mr. Boyle soon gave order for an apparatus to put it to experiment; wherein at several times, upon several doggs, *Opium* and the influsion of *Crocus Metallorum* were injected into that part of the hind legs of those animals, whence the larger vessels that carry the blood are most easy to be taken hold of: whereof the success was that the opium, being soon circulated into the brain, did within a short time stupefy tho' not kill the dog."*

These researches were soon followed by those of Richard Lower, on the transfusion of blood from the vessels of one animal into those of another. His first experiments were done in 1665, and were reported by Mr. Boyle to the Royal Society in the following year. The experiments consisted in placing a ligature upon a dog's carotid artery, opening the vessel below the ligature, and inserting into it a quill of the proper size to serve for a canula. This canula was then connected with the jugular vein of another dog, so placed as to receive the blood coming from the carotid artery of the first in a direction to pass downward to the heart and so mingle with the mass of the circulating current. The jugular vein of this dog was then opened above the insertion of the canula, and he was allowed to bleed from it; the blood lost in this way by the second dog being replaced by that which was transfused from the vessels of the first.

These experiments were quite successful, and first showed that death from hæmorrhage might be prevented by transfusion. Lower, however, found one practical difficulty in the performance of the experiment, viz. that the blood of the first dog sometimes coagulated in the artificial tube and stopped the current; so that he was obliged to take out the canula and clear it with a probe.

"For this must be expected," he says,† "when the dog that bleeds into the other hath lost much bloud, his heart will beat very faintly, and then, the impulse of bloud being weaker, it will be apt to congeal the sooner. . . . But to prevent this trouble and make the experiment certain, you must bleed a great dog into a little one, or a *Mastive* into a *Curr*, as I once try'd, and the little dog bled out at least double the quantity of his own bloud, and left the Mastive dead upon the table, and after he was

* Philosophical Transactions. Vol. I., p. 128.
† Ibid. p. 353.

untyed, he ran away and shaked himself, as if he had only been thrown into water."

These results soon led to the idea of performing the same operation on the human subject. This was first done in France by a physician and a surgeon, named Denis and Emmerets. They were led to believe that the operation might result in the cure of diseases by introducing healthy blood from a foreign source into the veins of the patient; and some apparently successful results of this kind excited among the profession great enthusiasm in its favor. But these expectations soon proved unfounded, and several cases of failure afterward produced so much reaction against it, that in 1668 the Parliament of Paris passed a law forbidding the operation to be performed except by consent of the Faculty.

The matter remained in this condition until 1818, when Dr. James Blundell, of London, brought back the operation to its original object; applying it, not to the cure of diseases, but to the preservation of life after exhausting hæmorrhage. He also discovered and exposed the principal error in the previous mode of doing the operation; viz. the use of blood of animals of a different kind, instead of those belonging to the same species.

He performed thirty-three experiments on animals, and showed by them:

1st. That dogs exhausted by hæmorrhage may be resuscitated, even after momentary stoppage of the respiration, by injecting the blood of other dogs.

2d. That human blood, injected into a dog in large quantity, sufficient to replace an exhausting hæmorrhage, though it produces a temporary reanimation, does not save life, but the animal dies some hours afterwards.

3d. That transfusion of blood, in animals of the same species, will be successful, whether the blood used be arterial or venous.

4th. That blood may be received into a cup and passed through a syringe, without being thereby rendered unfit for the purposes of life.*

The transfusion of blood, thus placed upon its proper footing, and still further improved by the investigations of Prevost and Dumas, Milne Edwards, Dieffenbach, and Bischoff, was adopted by the profession, and is now known as an established and useful operation, applicable to cases in which an exhausting hæmorrhage, in a healthy person, has brought the patient to the point of death. In these cases it has long been known, as mentioned by Blundell, that there is an interval, often of several hours, after the hæmorrhage has ceased, during which the patient is evidently sinking, and when other means of restoration are of no avail. Bérard has recorded fourteen such cases, in which the operation of transfusion was successful. They are as follows:†

* Researches, Physiological and Pathological; by James Blundell, M.D., London, 1824.

† Bérard, Cours de Physiologie, Paris, 1852. Vol. III., p. 219.

1st. A case of abundant hæmorrhage following delivery. All the signs of approaching death. Extremities cold; respiration imperceptible. Two ounces of blood were transfused by Blundell, from the husband, and the operation repeated a few minutes afterward. Recovery.

2d. By Dr. Doubleday. Uterine hæmorrhage. The patient was at the last extremity. Six ounces of blood were tranfused from the husband in three injections. The patient returned from death to life. Recovery.

3d. By Brigham, of Manchester. Bleeding after delivery. Loss of consciousness; cold extremities. Death near at hand. Ten or twelve ounces of blood were injected at several times. At the third injection the patient began to revive. Recovery.

4th. By Dr. Burton Brown. Hæmorrhage after delivery. The patient presented a cadaverous aspect. Extremities cold; pupils dilated; no pulse, either at the radial or carotid. Three injections of blood at intervals of five to ten minutes. Recovery.

5th. By Waller. Uterine hæmorrhage. Death imminent. Thirty-nine drachms of blood were injected at intervals of five minutes. Recovery.

6th. By Banner, of Liverpool. Uterine hæmorrhage. Unconsciousness. About fourteen ounces of the blood of the husband injected at six intervals. The patient's condition remained doubtful for some time, but terminated in recovery.

7th. By Ingleby. Uterine hæmorrhage after delivery. Patient cold and insensible. Injection of four ounces of blood furnished by the husband. After seven minutes the pulse reappeared. After thirty minutes the patient was partially restored. Ultimate recovery.

8th. By Dr. Klett. Non-puerperal uterine hæmorrhage. Cold sweat, dimness of vision, hiccup, and an altered expression of face, announcing the approach of death. Injection of blood furnished by the husband. The patient at once opened her eyes, and her countenance changed for the better. Reccvery.

9th. By Dr. Klett. Uterine hæmorrhage. The patient, whose aspect was cadaverous, took leave of her friends. Articulation imperfect. A small quantity of blood was injected from the husband. Life was reëstablished as if by the effect of electricity.

10th. By Mr. Lane. A young man subjected to the operation for strabismus. Obstinate hæmorrhage. Condition of the patient desperate. Three and a half ounces of blood injected from a young woman. Recovery.

11th. By M. Bougard. A female patient, exhausted by hæmoptysis and subsequent abundant hæmorrhages. Injection of several ounces of blood, furnished by a young woman. The patient lived till the following year.

12th. By Dr. Savy. Uterine hæmorrhage in the third month of pregnancy. Skin icy cold; eyes lustreless; lips pale; limbs relaxed. Transfusion of blood, furnished by a healthy servant. Recovery.

13th. By Nélaton. In this case the result was finally unsuccessful, as the patient died of puerperal complications; but the first effect of the operation was as satisfactory as possible.

14th. By Domène. Hæmorrhage after delivery. Signs of approaching death. Transfusion of blood from a woman. Consciousness returned a few minutes after the operation. Recovery.

Two successful cases of transfusion are also recorded in the British and Foreign Medico-Chirurgical Review for July, 1857: one by the late Professor Brainard, of Chicago, in the *Chicago Medical Journal* for February, 1860; and one in the *New York Medical Journal* for November of the present year.

In these cases, as noticed by Bérard, there is no doubt that the operation saves the life of the patient; for in animals reduced by hæmorrhage to the condition of apparent death, recovery never takes place unless transfusion be performed.*

IV. Next, *Artificial Respiration.* There can be no need to do more than allude to the usefulness of this operation, which has so often been successful in cases of suspended animation. It is intimately associated with the nature and mechanism of natural respiration, and these two elements of the function were naturally studied, to a great extent, together. In the middle of the sixteenth century Vesalius had found that by blowing up the lungs with air, after the chest was opened, the stoppage of the heart's action might be delayed for a short time. But the first attempt at continuing life indefinitely in this way was that of Robert Hook in 1664, who showed that by alternately inflating the lungs with bellows, and then allowing them to collapse, an artificial respiration might be established which could be kept up for a long period. Hook reported this result to the Royal Society, where it attracted all the attention which its importance deserved.

"I did, therefore," he says, in a second communication,† "heretofore give this *Illustrious Society* an account of an Experiment I formerly tryed of keeping a Dog alive after his *Thorax* was all displayed by the cutting away of the *Ribs* and *Diaphragme*, and after the *Pericardium* of the Heart was also taken off. But divers persons seeming to doubt of the certainty of the Experiment (by reason that some Tryals of this matter, made by some other hands, failed of success), I caused at the last Meeting the same Experiment to be shown in the presence of this *Noble Company*, and that with the same success as it had been made by me at first."

But this was not sufficient. Plain as the matter seems to us now, it was not then certainly known in what manner the continuance of respiration maintained the circulation of the blood; and by some this was thought to be accomplished not through the

* Cours de Physiologie. Vol. III., p. 222.
† Philosophical Transactions. Vol. II., p. 539.

chemical influence of the atmospheric air, but by the physical movement of the parts. To settle this point Hook contrived another experiment. Having opened the chest as before, and fitted his bellows to the trachea, he then fitted a second pair of bellows to the further end of the first, and at the same time made numerous incisions over the outer surface of the lungs. He then worked the second pair of bellows very rapidly, "by which," he says, "the first pair was always kept full and always blowing into the lungs," the lungs being themselves always full and without motion, but having a constant blast of air passing through them. Life was sustained in this experiment as in the other. Hook therefore showed,

1st. That life might be indefinitely prolonged by the alternate inflation and collapse of the lungs in artificial respiration; and

2d. That this was accomplished, not by the simple motion of the parts, but by the continual introduction of fresh air into the respiratory passages.

Artificial respiration was subsequently employed by Brodie, Hope, Legallois, Wilson Philip, and others, both for studying the action of the heart and blood-vessels, and for resuscitating asphyxiated animals. It was also applied to the human subject, and is now a recognised means of preserving life in cases of drowning, hanging, asphyxia from breathing noxious gases, and the suspended animation of the newly born.

V. *Digestion.*—There is a curious fact in the physiological history of digestion, viz. that the first source of definite knowledge on this subject consisted of experiments performed, not upon animals, but upon a man. Alexis St. Martin, who was accidentally shot in 1822, in such a way as to produce a gastric fistula, enabled Dr. Beaumont to make a series of observations which were attended with valuable results. Dr. Beaumont then positively demonstrated:

1st. That the stomach secretes a peculiar acid fluid, which is known as the gastric juice.

2d. That the gastric juice is secreted only during digestion and when the stomach is excited; and

3d. That it has the power of dissolving the ingredients of the food, and is accordingly a true digestive secretion.

But we cannot always have a man with a gastric fistula to experiment with; still less make one in the human subject at will. Accordingly the subject has since been studied by means of fistulæ artificially established on animals. I need not recall all the discoveries which have been made in this way by Blondlot, Schwann, Bernard, Lehmann, and others. I would only mention two important considerations: First, that these subsequent labors have fully confirmed the principal facts discovered by Beaumont, and have shown that the properties of the gastric juice are essentially the same in man and animals; and, secondly, that by them our knowledge of the subject has been greatly extended, and some of Dr. Beaumont's errors—unavoidable for a first experimenter in a new field—have been explained and corrected. Our knowledge of the remainder of the digestive fluids, the pancreatic and intestinal juices, and the bile, as well as the sugar-producing function of the liver, has been almost entirely due to experiments on the lower animals.

VI. *The Nervous System.*—The study of the nervous system seems to have been more difficult than that of the other great divisions of the animal organism. This is probably because the functions of the nerves and nervous centres are so different in their nature from the physical and chemical phenomena of nutrition, that they require, in most instances, peculiar methods of investigation, not adapted to the study of the latter. It is also, no doubt, partially owing to the fact that the intricate structure of the nervous system requires a longer study of its anatomy before that of its functions can be successfully pursued. However this may be, it is certain that the more important discoveries in this department date from a later period than the others. Galen discovered, by experiments upon living animals, the functions of the recurrent laryngeal nerves, as connected with the voice. But from his time the labors of investigators were principally confined to unravelling the anatomy of the nervous system, until the early part of the present century. The first important advance was made at that time by the discovery that the endowments of sensation and motion occupy distinct localities in the organs of the nervous system.

The exact history of this discovery has been open to some dispute, owing mostly to the fact that the ideas of physiologists on this point were developed somewhat slowly, and only gradually assumed the definite and precise form which they now present. The main part of the discovery has been claimed principally for two experimenters, viz. Sir Charles Bell in England and Magendie in France. The share which each really had in accomplishing this result is as follows:

Sir Charles Bell's investigations were guided by an idea, based upon anatomy, that the different nervous tracts, connected with different parts of the brain, had different endowments. He considered the cerebrum as connected with the anterior columns of the spinal cord and the anterior roots of the spinal nerves, and the cerebellum as connected with the posterior columns and the posterior roots. His first experiments, in 1811, on the spinal cord and nerves, were undertaken for the purpose of ascertaining, through them, the distinct endowments of the cerebrum and cerebellum.*

* Bell. Nervous System of the Human Body, 3d edition, London, 1844 p. 24; also, "An Idea of a New Anatomy of the Brain;" quoted in a Narrative of the Discoveries of Sir Charles Bell in the Nervous System, by Alexander Shaw. London, 1839; p. 40.

These experiments consisted in laying open the spinal canal of the rabbit and irritating alternately the different columns of the cord and the different roots of the nerves. He found by this means that irritation of the anterior columns and roots produced convulsion of the muscles; while irritation of the posterior columns and roots had no such effect.* These experiments were performed, first, upon the uninjured animal, and subsequently upon those which had been stunned by a blow on the head.† They showed simply that the power of motion resided in the anterior roots of the spinal nerves, but not in the posterior roots; and consequently that the nervous roots, as well as the columns of the cord, were dissimilar to each other. It did not then appear where the power of sensation was located; nor what was in reality the function of the posterior roots.‡ This was necessarily left in doubt; since, in all but the first of these experiments, sensibility had been destroyed before opening the spinal canal.

The next and most successful experiments of Sir Charles Bell were those performed, in the living animal, on the nerves of the face. They were communicated to the Royal Society of London in 1821.§ These were certainly the first experiments which showed the important fact that the Seventh cranial nerve is a nerve of motion, while the sensibility of the parts resides in the Fifth pair. But even here, Bell's idea was not that of the simple distinction of sensation and motion in these two nerves. His object was to show the existence of a great system of *Respiratory Nerves*, separate from those of sensibility and voluntary motion. He regarded the Seventh pair as the great Respiratory nerve of the face, and proved that when it is divided in the living animal, the respiratory movements of the lips, nostrils, and cheeks are abolished, while sensation remains. But he still regarded the Fifth pair as a nerve of sensation and voluntary motion for the whole face; the Seventh being simply superadded, to provide for the movements connected with respiration. It is impossible to read his original paper, in the Philosophical Transactions for 1821, without being convinced of this fact.

Nevertheless, this discovery was a very important one, and led to important practical results. For, before that time, surgeons were in the habit of cutting the Seventh nerve for the cure of facial neuralgia—of course, with very unfavorable results, as the operation not only failed to relieve the neuralgia, but also produced a paralysis of motion in the face. But after Sir Charles Bell had shown that the Seventh was only a nerve of motion, while sensibility resided in the Fifth pair, the old operation was abandoned, and accordingly it is now the Fifth pair only which is divided for the relief of tic douloureux.

* Narrative of the Discoveries of Sir Charles Bell, &c., p. 41.
† Ibid. p. 87. ‡ Ibid. p. 46.
§ Philosophical Transactions, Vol. XXXI., p. 398.

In 1822 the study of the spinal nerves was taken up by Magendie, and the separate endowments of the two roots fully estalished. In order to exhibit the exact and valuable character of these experiments, and to show how little they deserve the violent criticism which has sometimes been made upon them, I will quote the original report by Magendie, in the *Journal de Physiologie Expérimentale et Pathologique*, Vol. II. (1822); p. 276:

"For a long time," he says, "I was desirous of doing an experiment upon an animal in which I should divide the posterior roots of the spinal nerves. I had several times made the attempt, but failed, in consequence of the difficulty of opening the spinal canal without wounding the cord, and either destroying the animal or inflicting upon him a serious injury. But in the course of the last month there came into my possession a litter of eight pups, only six weeks old; and I thought this a good opportunity of renewing my attempt to open the spinal canal. The result was that I was enabled, by the aid of a sharp cutting scalpel, to uncover, nearly at a single stroke,* the posterior half of the spinal cord, still enveloped by its membranes. The only thing then remaining to expose almost completely the surface of the cord, was to cut through the *dura mater* surrounding it. This was accomplished without difficulty, and I then had under my eyes the posterior roots of the lumbar and sacral nerves. After taking up these roots, one after the other, with the blade of a fine pair of scissors, I divided them on one side, the cord remaining untouched. I was uncertain what would be the result of this operation. Accordingly I sewed up the wound in the integuments and kept watch upon the animal. At first I thought that the limb corresponding to the divided nerves was entirely paralysed. It was insensible to pricks with a pointed instrument and to the strongest pressure, and it also appeared to be motionless. In a few moments, however, to my great surprise, I saw it move very distinctly, though sensibility had altogether disappeared in it. A second and a third experiment gave precisely the same result; and I began to regard it as probable that the posterior roots of the spinal nerves might, indeed, be different in function from the anterior roots, and that they might be more particularly devoted to sensibility.

"It very naturally occurred to me also to cut the anterior roots, leaving the posterior untouched. But such an enterprise was more easily conceived than executed; for how could the anterior part of the cord be laid bare without injuring the posterior roots of the nerves? At first, the undertaking appeared to be hopeless; but, after thinking the matter over for forty-eight hours, I determined to make the attempt to pass in front of the posterior roots, a kind of cataract-knife, with a very narrow blade, so that I might cut the nervous roots by pressing

* Owing to the cartilaginous condition of the vertebræ in young animals.

the edge of the knife against the posterior surface of the bodies of the vertebræ. I was obliged, however, to give up this plan, on account of the large veins which occupy that part of the spinal canal, and which I wounded at each attempt to carry the knife forward. But in making these trials, I noticed that, by drawing aside the dura mater, I could catch a glimpse of the anterior roots, united in bundles, just at the spot where they penetrate the membrane. This was all that I desired, and in a few seconds I had divided all the anterior roots upon which I wished to operate. As in the previous experiments, I made the division on one side only, that I might use the other as a term of comparison. It may easily be imagined with what curiosity I watched the effects of this operation. The result was unequivocal. The limb was completely motionless and relaxed, while at the same time it retained a perfectly evident sensibility. Finally, that nothing might be neglected, I divided at once the anterior and posterior roots. This produced entire loss both of sensibility and movement.

"I have repeated these experiments in various ways, in several species of animals; and the results above described were confirmed in the most complete manner, both for the anterior and posterior limbs. I am still continuing these researches, of which I shall give a more detailed account in a succeeding number. For the present, it is sufficient that I can state positively that the anterior and posterior roots of the spinal nerves have different functions; and that the posterior appear more particularly devoted to sensibility, while the anterior are more especially connected with the power of motion."

In this way was the distinction of sensibility and motion in the spinal nerves finally established.

It is a little remarkable, in this respect, that Sir Charles Bell should be sometimes quoted as speaking in condemnation of this kind of experiment; for whenever he does so, he appears as a witness against himself. In reviewing, at the present day, the works of Sir Charles Bell and their results, it is evident that everything of value in the physiology of the nervous system which he discovered, viz. the motor character of the anterior roots, and the distinction between the fifth and seventh cranial nerves, he discovered by means of experiment upon living animals. These discoveries were positive and permanent, and retain at this day their full value. But all Sir Charles Bell's theoretical views, which he thought of so much importance, such as the division of the nerves into *regular* and *irregular* or *superadded*, and the great *respiratory system* of nerves, are now destitute of interest, and have nearly passed out of mind.

There is one circumstance connected with the discovery of the distinct properties of the spinal roots, which is so curious in some respects that it has become a kind of joke in the history of physiology. At quite an early period, in 1809, Alexander Walker conceived the idea, on certain theoretical grounds, that the anterior spinal roots were nerves of sensation, and the posterior roots the nerves of motion. He was thoroughly convinced of the truth of this doctrine; so much so, that twenty-five years later he published a work entitled "The Nervous System, Anatomical and Physiological," in which he reiterated his views, and maintained that Sir Charles Bell and Magendie were entirely wrong. In vindicating his claims in this work, he says (page 136):

"In 1809, viz. twenty-five years ago, the writer published the true doctrine of the nervous system; *a.*, ascribing sensation and action to distinct nerves and columns; *b.*, ascribing these truly, viz. sensation to the anterior and action to the posterior nerves and columns."

Also (page 137), "In 1815, viz. six years after his first publication of this doctrine, the writer repeated and extended it, in *Thompson's Annals of Philosophy*, still ascribing sensation and action to distinct nerves and columns,—and ascribing these truly, viz. sensation to the anterior and action to the posterior nerves and columns."

Also (page 138), "Thus the title, to which either Sir Charles Bell or M. Magendie have presumed to lay claim, viz. the ascribing distinct functions to distinct nerves and columns (and ascribing these wrongly, as the writer has shown), dates twelve or thirteen years after this was done (and done rightly) by the present writer."

Now, Alexander Walker had a very bad opinion of experiments, especially when performed upon the nervous system, to which, he thinks, they are "totally inapplicable."* He has a paragraph on page 97, headed:

"*Utter Worthlessness of all Experiments on Living Animals, in which Functions are destroyed or disturbed.*"

In the course of this paragraph he remarks (page 105), "The rage for experiments on living animals will now be easily understood. All fools have eyes and fingers. They rejoice that some fantastic use of these may be called experiment. This becomes a happy compensation for the want of reasoning powers. The number of pretenders naturally renders the method universal; and each blockhead thinks himself wise when he has made an experiment which he does not understand. Henceforth," he says, "let these men know that amplitude of eye and hand is a wretched substitute for littleness of brain, and that reasoning powers are essential to all advances in physiology."

In a previous paragraph (page 11) the writer says that, where

* The Nervous System, Anatomical and Physiological. By Alexander Walker. London: 1834, p. 105.

we are dealing with living and complicated functions, "the performance of experiment is the act of a person who has not the slightest notion of the use and application of experiment, and it becomes the mere play of a driveller or an idiot. Of this," he adds, somewhat innocently, "the reader will have a striking illustration in the sequel."

From his description of certain experiments, however, it must be confessed that Walker does not seem to have had any practical acquaintance with them.

Subsequently to 1822, other discoveries of great interest in the nervous system have been made: by Longet, Magendie, and Bernard, on Recurrent Sensibility; by Mayo and Marshall Hall, on Reflex Action; by Schiff, Vulpian, and Philipeaux, on the Regeneration of Nerves; by Legallois and Flourens, on the Properties of the Medulla Oblongata; and by Brown-Séquard, on the Crossed Action of the Spinal Cord;—all by means of experiments on living animals.

VII. Experiments of this kind formed a large part of the discovery, by John Hunter, of the modern *Operation for Aneurism.* Previously to 1785, the best treatment for this affection consisted in opening the aneurism and tying the vessel in the wound, or in applying a ligature immediately above and below the part. This operation, however, was usually unsuccessful, owing to fatal secondary hæmorrhages from the aneurismal tumor, or from the ligatured part of the vessel. The manner in which the improvement in treatment took place was as follows:

"Mr. Hunter,* from considering the manner in which the spontaneous cure of external aneurisms was effected, was led to propose and practise, in the year 1785, the operation which justly bears his name. . . . The only point on which he required information was, whether the repeated hæmorrhages which took place (when the ligature was placed immediately above the aneurism) were really in consequence of the artery being diseased; and this he endeavored to elucidate by observation and experiment. He laid bare the inner coat of an artery (in the dog) by dissecting off by layers the two outer ones until he saw the blood through the remaining membrane; the wound was then closed, and the animal was killed three weeks afterward, when the parts were found consolidated, the canal of the artery being neither increased nor diminished. This was not supposed to be sufficient to establish the fact that an injury of this kind might be inflicted on a sound artery with impunity; and Sir Everard Home, after dissecting off the outer and middle coats, in a similar manner, laid a piece of lint upon the artery, to prevent adhesion, instead of closing the wound. It healed up at the end of six weeks, when the dog was killed, and the artery being injected, the coats of it were found of their natural thickness and appearance.

* Guthrie, on the Diseases and Injuries of Arteries, London, 1830, p. 148.

"Satisfied by the result of these experiments that ulceration of the artery depended more on its being previously diseased than on the injury committed upon it, he fairly supposed that if he placed his ligature higher up on the artery, on a sounder part, he would have a better chance of avoiding the hæmorrhages which had so frequently proved fatal."

Experience afterward showed this operation to be successful, and it has since been the means of saving many limbs and of preserving many lives which would have been sacrificed under the older method of treatment.

VIII. There are few improvements in surgery which have been attended with more satisfactory results than those which were introduced into the operations of resection and extirpation by the discovery of the *Office of the Periosteum in the Regeneration of Bone.* When a large portion of any bone requires to be removed, on account of fracture or disease, the permanent deficiency of the bony parts, after the healing of the wound, is a very serious inconvenience; so much so that surgeons were frequently in the habit of amputating the entire limb rather than retain a member which, if preserved, would be practically an incumbrance to the patient instead of an advantage. The study of the mode of reparation of bone, by means of the periosteum, has shown how this difficulty may be avoided, and how much advantage may be gained in the preservation of injured and diseased parts.

Du Hamel, in 1740,* and Hunter, in 1772,† first learned, by experiments on pigeons, fowls, and young pigs, that the growth of bone takes place mainly from the exterior, and probably by the nutritive power of the periosteum. Subsequently this question was further examined experimentally by Flourens, Heine, Murray, and others. Mr. Syme, of Edinburgh, in 1837,‡ endeavored to ascertain, as he expresses it, "whether the periosteum, or membrane that covers the surface of the bone, possesses the power of forming new osseous substance independently of any assistance from the bone itself." He extirpated the middle portion of the radius of a dog, with its periosteum, and found, as Sir Astley Cooper had previously done, that after the recovery of the animal there was no bony union of the parts, but only a ligamentous band, running from one bony extremity to the other. At the same time he did a similar operation on the radius of the opposite leg, only leaving the periosteum in its place; and on this side he found, at the end of six weeks, instead of the gap presented by the first leg, "a compact mass of bone occupying the space left by the portion removed." He also detached the periosteum in another dog without removing the bone, and inserted a piece of

* Mémoires de l'Académie Royale des Sciences, 1741, '42, and '43.

† Works of John Hunter. Palmer's edition. London, 1835. Vol. IV., p. 315.

‡ Transactions of the Royal Society of Edinburgh. Vol. XIV., p. 158.

metal under the membrane, in which, at the end of six weeks, he found a thin plate of bone, entirely disconnected with the original osseous surface.

Wagner, in a monograph on the *Process of Reparation after Resection and Extirpation of the Bones*,* points out the usefulness of these experiments, and the reasons upon which their value depends.

"The anatomical study of the process of reparation," he says,† "may be made either on man or on animals. It would be better, no doubt, to make this study upon man, but for this purpose it would be necessary to have the opportunity of dissecting, at different periods, human limbs which had been the subjects of resection. It would be necessary that the patient should die, or that secondary amputation should be performed; and, judging from the poverty of our knowledge in this respect, it seems probable that such opportunities are only rarely met with. If so, this would go to prove the safety of the operation of resection. It is true that it is still more rare to have the opportunity of dissecting the body of a person who has before been the subject of resection, and who has survived a certain time after the completion of the cure.

"We must, therefore, have recourse to experiments on animals, and by this means we may follow all the steps of the reparative process."

Wagner's experiments, which were mainly performed with dogs and pigeons, carried still further our knowledge of the process of reparation and the part taken in it by the periosteum.

But the most recent and extended investigations on this subject were those of Leopold Ollier, in 1858.‡ He operated by separating, in the living animal, a lamina of periosteum from the surface of the tibia and transplanting it among the neighboring muscles. He found that the periosteum, thus removed from its original locality, not only continued to live, but produced bony tissue in its new and unaccustomed situation. This happened even when the lamina of periosteum was entirely separated from its original attachments and transplanted, like the skin in autoplastic operations on the face. It took place even when the separated periosteum was transplanted into distant regions, as into the popliteal space, or under the skin of the groin or that of the back. In all these cases, the new bone produced had the natural structure of osseous tissue, as shown by microscopic examination.

Ollier also practised resections and extirpations of the long bones, making always a comparative experiment in order to judge of the influence of different modes of operating. He extirpated the radius and metatarsal bones, upon rabbits and dogs, taking

* In Archives Générales de Médecine (1853), Vol. II., p. 712.

† Ibid, p. 713.

‡ *Recherches Expérimentales sur la Production Artificielle des Os*, &c. Journal de la Physiologie de l'Homme et des Animaux. Janvier, 1859.

away also the periosteum in some instances, and in others leaving it undisturbed. He found that when the periosteum was left, a new bone was developed in from one to three months afterward; but when it was removed, no bony regeneration took place. In extending his observations to cases of resection of the joints, Ollier found that when the articulating extremities of the bone were removed, with the periosteum and articular capsule, the new joint formed was simply a solid fibrous band uniting the divided extremities of the bone; but when the periosteum and articular capsule were left, the articulating extremities of the bones were regenerated in their natural form, and a true articulation, with a more or less complete synovial cavity, formed between them.

The practical importance of these results, for patients suffering from injuries or diseases of the bones, requires no discussion. It has been fully appreciated by surgical practitioners at the present day.

IX. In 1861, Dr. S. Weir Mitchell, of Philadelphia, published the results of two years' researches on the *Venom of the Rattlesnake, and the Treatment of Rattlesnake-bites.** In these researches Dr. Mitchell investigated the anatomy of the fangs, poison-glands, and accessory parts, in the rattlesnake; the mode of production and discharge of the poison, its composition and properties, its action on various species and classes of animals, and the value of different remedies and antidotes. For this purpose he kept several of the living snakes constantly under his observation, and by repeated personal inspection and manipulation familiarized himself with their habits and physiological peculiarities from every point of view. He performed seventy-three experiments with these animals, and observed the action of their poison on frogs, serpents, reed-birds, pigeons, dogs, and rabbits.

The paper, in which the account of these experiments is given, has no superior in medical literature for the clearness and elegance of its style, the abundance of its material, and the precision of its results. It illustrates very distinctly the value of strictly elementary researches as a necessary preliminary to those of a more practical nature.

"When I first," as the author remarks,† "engaged in the study of the venom of the rattlesnake, it was with the intention of ascertaining what value Bibron's antidote possessed. To effect this single end I procured four or five snakes, and proceeded to subject animals to their fangs, and afterward to give the supposed antidote.

"After destroying many animals and attaining only negative

* Smithsonian Contributions to Knowledge. Researches on the Venom of the Rattlesnake, by S. Weir Mitchell, M.D., Washington, 1861; and an article on the Treatment of Rattlesnake-bites, in the North American Medico-Chirurgical Review, Vol. V., p. 270.

† N. A. Med.-Chirur. Review, Vol. V., p. 270.

results, I began to perceive that I was working in the dark, and that it was altogether impossible to obtain useful results, without possessing definite knowledge as to the nature of the venom, the mode of its formation and ejection, and the whole natural history of the disease to which it gives rise."

It would be difficult to find a medical treatise which should illustrate more fully than this the judicious caution and reserve which guide the physiological experimenter, and which enable him to avoid the sources of error that lie in his way. The constant employment of comparative and counter-experiments, and the frequent variation of the methods employed, indicate the care and faithfulness of the investigations, and inspire a well grounded confidence in their results.

In the study of antidotes and remedies, Dr. Mitchell shows the existence and origin of two classes of fallacies, which have heretofore vitiated much of our knowledge on this subject.

First, *Fallacies arising from want of exact knowledge as to the secretion of venom, and the mode in which the serpent uses its fangs and ejects the poison.*

Second, *Fallacies arising from want of information in regard to the natural history of the disease caused by the venom of serpents.*

Finally, the paper shows the comparative value of the various antidotes in vogue; the usefulness of the "intermittent ligature," in checking and controlling the entrance of the venom into the circulation; and the true method of employing stimulus to sustain the patient and preserve him from the fatal effects of the poison.

X. I shall refer in detail to only one more instance of the usefulness of these experiments, viz., the origin and prevention of *Parasitic Diseases.* There are certain parasites which infest the human body, some of which are exceedingly troublesome, and others often fatal. One of these is the tapeworm. The tapeworm, as we know, is derived from eating pork which is infested with another parasite, viz. cysticercus. But the cysticercus is so different from the tapeworm in size, structure, and appearance, that no connexion was formerly supposed to exist between them. The identity of the two was discovered in Germany about the year 1845–50, by Siebold and Küchenmeister.* By experimenting upon mice, rabbits, dogs, and cats, these naturalists found that certain species of cysticercus and tænia were identical with each other, and that a cysticercus in the flesh of one animal, when eaten by another, became developed into a tapeworm in the intestine of the second. This discovery led, soon afterward, to the important result that the human tapeworm is derived from the cysticercus of the pig; and showed the way in which infection by this parasite may be avoided in the human species.

* Transactions of the Silesian Association for National Instruction, Scientific Department. Session of July 7th, 1852.

A more recent and striking instance of the same kind is that of the trichina spiralis. This worm has been known, since 1832, as infesting, sometimes in large numbers, the voluntary muscles of the human subject. It was found on many occasions in dissecting-room subjects and in hospital patients, and was examined by Owen, Henle, Bischoff, Luschka, Harrison, and Farre. But in these cases there was no evidence of the patient having suffered anything from the presence of the worms; and, notwithstanding their great numbers, it was the universal opinion of the profession that the trichina spiralis was a harmless parasite in the human muscle. Nothing was known of its origin or mode of development.

But a little over ten years ago, certain physiologists in Europe began to experiment with these parasites, and, by administering flesh infected with trichina to pigs, dogs, and rabbits, they found that the worm became further developed in the intestine of these animals, and that it there began to acquire a perfect sexual organization. In 1859, Professor Leuckart, in Giessen,* continued the experiments, and showed this development to be complete; the muscles of the second animal becoming infested with the progeny of the trichina, which he had swallowed with his food.

This was the state of our knowledge when, in the year 1860, the members of a family in Dresden were taken ill with symptoms of intestinal irritation, fever, and general pains. One of them, a servant girl, died; and, at the autopsy, her muscles were found to be full of trichina spiralis. These infected muscles were examined by Dr. Zenker and Professor Virchoff, who administered a portion of them to a healthy rabbit. This animal died at the end of a month, and was found to be also infected with trichina. A part of his flesh was given to a second rabbit, who also died, infected, four or five weeks later. The flesh of the second rabbit was then given to a third, who became infected in the same way, and also died at the end of a month. In the meantime, inquiries had been made at Dresden, and a part of the pork, which had been served as food to the family shortly before they were taken ill, was examined, and found to be infested with trichina.

By these investigations, the medical profession became aware of the existence of a fatal and heretofore unknown disease, and at the same time were made acquainted with its source and the manner of its production. Undoubtedly, many cases of trichinosis had happened before 1860; but they were not understood, and the patient was supposed to be laboring under some form of fever or rheumatism. We now know that the trichina, when first introduced into the system, creates a violent disturbance by irritation of the intestine, and by penetrating the tissue of the voluntary muscles. It is often fatal in this stage; but if the patient survive the first five or six weeks, the parasites become encysted in the

* Untersuchungen über Trichina spiralis. Leipzig, 1860.

muscular flesh, and remain there, quiescent and comparatively harmless, for an indefinite period. It was these old and encysted specimens which were first observed, and which could not be referred to any previous illness.

Other experiments have since shown how trichinosis may be detected in pork, and what precautions are necessary to prevent infection from pork used as food.

The importance of this subject may be further estimated from two facts: First, that since 1860 several epidemics of trichinosis have happened in Germany, in two of which over five hundred persons were affected, of whom one hundred and twenty died;* and secondly, that pork in this country is also subject to trichinosis, as appears by the report of a committee of the Academy of Sciences, of Chicago, published in the Chicago Daily Republican of April 13, 1866. This committee examined the flesh of 1,394 hogs brought to Chicago, and found twenty-eight of the number, or one in fifty, infected with trichina. Cases of fatal poisoning from this cause in the human subject have also happened in this country, two of which have fallen under my own observation.

It is to experiment upon living animals, therefore, that we owe our knowledge of this new and dangerous disease, always liable to present itself to the medical practitioner.

In conclusion, there is one point to which I would allude, which seems to me of decisive importance. It is sometimes asserted by those who oppose physiological experiment, that there are other sources of information more legitimate and allowable, and that the chief of these is the observation of the phenomena of disease met with in the treatment of the sick. But do those who take this ground appreciate what they really inculcate when they recommend that we should rely upon this source of knowledge, and neglect experimentation? Do they overlook the fact that such a course is simply *waiting for accidental experiments* to help us in our inquiries? What was the case of Alexis St. Martin but an experiment, accidentally arranged, by a gunshot wound? When we are called to a case of disease, we do not wish to manipulate and experiment with it; we wish to cure it. In order to cure it, we must know its pathology, and understand it beforehand. And if we do not so understand it, then our observations and manipulations in that case are experiments on the living man, and cannot be regarded as anything else. Take the case of the Facial nerve and the Fifth pair. Sir Charles Bell himself says † that, previously to his time, as the result of operations on the face for tic douloureux, "after fifty years of experience, we remained ignorant of the distinction in these nerves."

* Epidemics of Hedersleben and Quedlimburg. L. A. Gosse; Des Trichines spirales, Genève, 1866, p. 30.

† Nervous System of the Human Body, p. 77.

It is true that, in many instances, our observations in disease, and our attempts for its cure, must necessarily be of the nature of experiments upon man. But when experiments upon animals have led the way, and have cleared up, in advance, some of the important points in physiology or pathology, they can only be regarded as fortunate and precious aids in our study of disease. The assistance which this method of investigation may be to us in the future can be judged of by what it has done in the past; for the science of Medicine is one which is constantly advancing, as each generation receives the benefits and feels the impulse communicated by its predecessor.

I have enumerated above the more striking improvements in our knowledge due to physiological experiment, and those most directly productive of practical results. Every medical man, however, will comprehend that these form but a small proportion of the acquisitions which we really owe to it. An immense number of details in every department of physiology have also been learned from this source, which were not directly connected with any especial practical point in medicine or surgery. But they have none the less served to enlarge and complete our physiological knowledge, and to bring it, in numberless minor particulars, into harmony with the truth. And there can be no doubt that the greatest value of physiology, as connected with medical and surgical practice, is the influence which it exerts in a general and intangible way;—by making the practitioner acquainted with the natural functions of the animal organs, and their variations within physiological limits, so that he is enabled to study, with a more intelligent motive and a clearer conception, the processes and phenomena of disease. It is this reasonable and more intelligent method of study which distinguishes the science of Medicine, as a whole, at the present day, and which will undoubtedly be productive of equal improvement in the future.

The benefits of a physiological discovery, accordingly, are not confined to the particular point with which it is especially connected; but it reacts on other similar questions, and raises new points of inquiry, or reconciles previous discrepancies; and its greatest advantage is, after all, that it enlarges, in a general way, the boundaries of physiological knowledge. If I have confined myself, therefore, in the foregoing pages, mainly to instances in which experiment has led directly to practical results, it is because the value of such instances may be more easily and rapidly comprehended, and may be presented with greater preciseness and detail; and because they show, in a clear light, one aspect of the importance of physiological studies, and the means by which they are successfully pursued. If it had not been for experiments on living animals we should, at this day, be ignorant of the circulation of the blood; ignorant of the nature and mechanism of respiration; of the properties and functions of the nervous system; of the operation of transfusion; of artificial respiration; of the origin and pathology of parasitic diseases; of the best treatment for serpent bites and other venomous wounds; of the office of the periosteum in the regeneration of bone;—perhaps of the Hunterian operation for aneurism; and, for all that appears to the contrary, we should still be cutting the seventh pair of nerves for the cure of tic douloureux.

I have thus endeavored to indicate some of the principal benefits which have resulted to scientific and practical medicine from this mode of investigation; and to place it, both in regard to its objects, its methods, and its results, in the position to which it is entitled, as an invaluable assistant to the medical art.