AN

ELEMENTARY SYSTEM

OF

PHYSIOLOGY.

By JOHN BOSTOCK, M. D.

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PREFACE.

WHEN we reflect upon the distinguished share which our countrymen have borne in improving the knowledge of the animal œconomy, it may appear not a little remarkable, that we have no original work in our language which contains a systematic and connected view of modern physiology. My object in the following pages is to endeavour to supply this deficiency, by presenting the student with a concise view of the present state of the science, embracing an account of the most important facts and observations, as well as of those theories and hypotheses which have been the most generally received, or have been sanctioned by the authority of the most eminent names. As my design is to furnish an elementary treatise, my first aim has been perspicuity, both in language and in arrangement; and although it has not been my intention to produce what may be styled a popular work, yet I conceive that it contains so little of what is strictly technical, as to be generally intelligible to those who are not conversant with the medical sciences. Viewing it, however, in connexion with its appropriate object, I have endeavoured, in all cases, not merely to afford the student a digested abstract of the present state of our information on the various topics which it embraces, but by referring him to the sources whence I have derived my information, to enable him to examine for himself how far I have given a correct account of them, and to assist him in pursuing the investigation of any part in which he may feel more particularly interested. And I may here remark that in no instance have I given a reference to any book which I have not myself examined, and that in order to facilitate the progress of those who may be disposed to follow me in this track, I have appended a list of the works that have been consulted, with the dates of the editions employed.

With respect to references, the plan which I have proposed to myself has been to indicate, in all instances, the original sources of my materials, while, at the same time, I have avoided filling my pages with a multitude of quotations, which could afford no additional authority to the subject in question. On one point I profess myself to have exercised all the care of which I was capable, in scrupulously assigning to each individual the share of merit which justly belongs to him in the discovery of facts or the formation of theory. During the progress of information, and while knowledge is rapidly advancing, it not unfrequently happens that the same fact is discovered, or the same train of reasoning developed, by different individuals entirely independent of each other; but where the priority of publication is clearly proved, we ought to be cautious in admitting any claims that are subsequently brought forwards, while all instances of that disingenuousness which aims at suppressing the names of preceding writers, for the purpose of procuring a surreptitious celebrity, cannot be too severely reprobated. But to the credit of the English be it said, that such examples are among them extremely rare. It is impossible to peruse the various scientific and medical journals of the metropolis without observing, that so far from keeping back the various discoveries that are made in other countries, one of their first objects is to obtain priority of information on these topics, and it is truly gratifying to observe with what quickness and accuracy it is transmitted to us from the various parts of the continent.

I feel it incumbent upon me to make some observations upon the portion of the work which I have devoted to the consideration of physiological theories. When we call to mind the fate of those which have hitherto been given to the world, how even the most elaborate of them, and those which appeared to be the best founded, have been successively discarded, and given place to some new speculation, which has, in its turn, shared the fate of its predecessors, we might be tempted to regard the whole as undeserving of any share of our attention. But, although the truth of the foregoing statement cannot be denied, still, I apprehend, the subject possesses many claims upon us. It might be sufficient to allege that the theories of eminent physiologists form a curious part of the history of the science, that they mark the progress of knowledge, and exhibit, in an interesting point of view, the operations of the mind in its attempts to arrive at truth. It can surely never be considered as an unimportant or trifling pursuit, to inquire how such men as Haller and Hunter reasoned, or by what mode of investigation they were led to the discovery of the truths which immortalize their names.

And further, although no one in the present day can be insensible to the comparative value which ought to be attached to facts and to opinions, still they are often so intimately blended together that it is extremely difficult to separate them, and this is more especially the case in the study of physiology, where, from obvious causes, it is much less easy to make observations, or to perform experiments tha may lead to unexceptionable results, than when we operate upon inanimate matter. Even the most unfounded, and, as we now conceive them, the most absurd theories of the chemical and mathematical physiologists of the seventeenth century, were, to a certain extent, derived from what at that period was called experiment, and which satisfied the minds of the learned men of the age, yet if we except the simple detail of anatomical observation, there is perhaps scarcely a single statement against which we should not now be disposed to offer some objections. And there is another circumstance which renders it necessary for the student to be made acquainted with the most noted theories of the older writers, that without this knowledge their works would be unintelligible;

for such complete possession had these topics taken of their minds, that we can scarcely peruse a single page without meeting with some theory, either expressed or implied, which is intimately connected no less with their observations than with their reasoning.

And I shall further advocate the cause of theory, as being a direct means, and that a very important one, of acquiring a knowledge of facts. In prosecuting our investigations we are necessarily guided by some object, and this is, in most cases, a preconceived hypothesis, which we wish either to put to the test of experiment, or to inquire how far it can be reconciled with the structure of the body or the operation of its functions. This is the natural process by which information is acquired; the errors into which we have so frequently fallen do not consist in the legitimate employment of hypothesis, but in our being so much influenced by it, as to "mistake the scaffold for the pile," to regard a train of reasoning in the same light with a deduction from facts. Who that has attended to the state of chemical science for the last 50 years can reasonably doubt that its progress was prodigiously accelerated by the formation of the Lavoisierean theory, by classing the insulated facts in a systematic form, by generalizing the conclusions that appeared to be fairly derived from them, by introducing a uniform nomenclature, and by discarding a mass of antiquated opinions and phraseology? Yet this edifice, so beautiful in its separate parts, and which seemed so consistent as a whole, and so firmly connected together, appears destined to fall before the powerful genius of Davy; its strong holds have been assailed, and it totters to its very base.

One of the prevailing errors of the present day I conceive to be a fondness for constructing new arrangements, and for introducing new terms into all the physical sciences. This is, no doubt, partly owing to the rapid increase of information, which, to a certain extent, produces a necessity for change of both system and of language; but the innovations have been carried to a most unreasonable length. Indeed this is so much the case, that, in some departments, the attention is almost entirely engrossed by the study of nomenclature, while in consequence of the variety of denominations that are given to the same object, and the number of technical terms with which the memory becomes charged, we defeat our very end and object, which is to produce a uniformity of names, and a greater simplicity in our designation of things. In the following work it has been my aim to form an arrangement, which shall be no further technical than is absolutely necessary for announcing the subjects as they successively fall under our notice; scarcely a single new term is introduced, while, with a very few exceptions, I have employed the old terms in their most generally received acceptation, and have endeavoured always to use them precisely in the same sense.

Although I am not sensible that I have omitted any reasonable means for rendering my work complete, yet I am fully aware that, after all the pains that I have bestowed upon it, it must contain many parts that require correction, both with respect to the statement of facts and the inferences that are deduced from them. It is, in a great measure, from this consideration that I have determined to publish it in separate volumes, in order that I might be able, in the second volume, to correct the errors and supply the deficiencies which should be pointed out as occurring in the first. And I beg to remark upon this subject, that I shall pay every attention to the criticisms that are made upon the work, and shall thankfully avail myself of all the information that I can obtain from this source.

I cannot conclude these remarks without adverting to the great advantage which I have derived in the prosecution of this work from the very extensive and valuable library of the Medical and Chirurgical Society. When we reflect upon its recent date, and bear in mind that it was established by a few private individuals, depending for its support solely upon the sense of its utility, we cannot estimate too highly the public spirit of those who were the most active in its original formation. To the survivors it is a sufficient reward to witness the success of their labours, but I trust that I shall be pardoned for introducing in this connexion the name of Dr. Marcet, among whose claims to the gratitude of his profession, perhaps the very considerable share which he had in the establishment of the Medical and Chirurgical Society, must be considered as the most important.[1]

[1] I am aware that it is seldom proper to obtrude upon the public feelings of a personal nature, yet I shall hope for the indulgence of my readers if I offer my testimony to the distinguished merits of my much valued and ever to be lamented friend. His character has been so justly delineated by the elegant and correct pen of Dr. Roget, that I can do no more than give my warm assent to the sentiments expressed in his memoir, an assent sanctioned by an unreserved and confidential intimacy of twenty-eight years.

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INTRODUCTORY OBSERVATIONS.

THE term Physiology, according to its original meaning, is nearly synonymous with Natural Philosophy; but it has for a long time been always used in a more limited sense, and restricted to that branch of science which treats of the functions of the living animal body, and of the powers by which these functions are exercised.[1]

Notwithstanding the value which must have been at all times attached to the study of the animal body, both as holding the first rank in the scale of natural objects, and as being intimately connected with the various departments of medicine, its functions were seldom made a distinct object of investigation until the beginning of the last century. Although the writings of the ancient physicians, and of the earlier among the moderns, abound in physiological speculations, they are rarely brought forwards in a connected or systematic form; so that we are obliged to collect our knowledge of their tenets more from a number of scattered fragments, that are dispersed through works on medicine and pathology, than from treatises expressly devoted to the subject.

[1] Some of the Germans have lately employed the more correct term biology, as designating the science, which essentially consists in the knowledge of those properties which distinguish animate from inanimate matter.

Hippocrates may be regarded as the father of physiology as well as of medicine, although from the more complicated nature of the former, the actual advances which he made in it were probably not very considerable. We observe in his writings many traces of the Pythagorean philosophy, but, at the same time, we meet with a large proportion of what is original, or, at least, what has not been traced to any other source. One of his leading tenets is the existence of a principle which he styles nature (φυσις), and to which he ascribes the direction and superintendence of all our corporeal actions and movements. To this principle he attributes a species of intelligence, and conceives that one of its most important offices is to attach to the body what is beneficial, and to reject from it what would prove injurious;—an hypothesis which, although expressed in different ways, and clothed in a more or less mysterious form, has continued to be a popular doctrine to the present day. Besides this nature, which is regarded as the prime agent, there are other subordinate principles or faculties (δυναμεις) which especially operate in the production of the various functions.

With respect to the body, he conceives it to be composed of three kinds of substances, solids, fluids, and spirits, which are themselves formed by the combination of the four primary elements. The nature of the body is supposed to be materially affected by the nature of the four elements which enter into its composition; as well as by the four qualities of hot, cold, moist, and dry; which, by their respective combinations and proportions, produce the four temperaments. These are considered as original predispositions existing in the body, influencing both its mental and corporeal character, and laying a foundation for the diseases to which the individual is more especially liable. In his account of the different functions of the body, although we observe many marks of sagacity and acuteness, yet there is much that is inaccurate and erroneous. His acquaintance with the minute structure of parts was limited; but little was known of the nature of the external agents which affect the corporeal organs; while the use of the organs themselves was derived from vague conjecture or false analogy. Hippocrates also adopted the mysterious opinions of Pythagoras respecting the occult power of particular numbers; and he believed that the stars exercise an influence over the operations of the body.[2]

Little or no advance was made in the science of physiology from the time of Hippocrates to that of Aristotle. The genius of Aristotle and the course in which it was directed were, in many respects, well adapted for the improvement of this science. He appears to have been the first among the ancients who advanced comparative anatomy and natural history to any considerable degree of perfection; and while he enjoyed great advantages for obtaining information on these topics, he cultivated these advantages with

[2] The best view of the Physiology and Pathology of Hippocrates, which are every where blended together, is contained in his treatise "De Natura Hominis." Opera a Fœsio, t. i. p. 224—231.

much assiduity. Hence he acquired an extensive acquaintance with natural objects, and may be considered as having made an actual advance in our knowledge of the animal œconomy, although perhaps less than might have been expected from the means of information which he enjoyed, and the powers of mind which he displayed on other topics.

After the death of Aristotle we have another long interval, during which no progress was made in physiological science, when it received a new impulse from the genius of Galen. A considerable share of the celebrity which this extraordinary character attained is derived from his physiology. When we compare his treatise "On the Use of the Parts of the Body" with any work on the same subject published before his time, we cannot but admire the superiority of his information and the ingenuity with which he applies it to the explanation of the animal œconomy. Yet on both these points he is not without considerable deficiencies and inaccuracies; his physiology is frequently founded upon fallacious principles; and in his application of such as are more correct, he displays more of what may be termed ingenuity, than of that cautious discretion which is so necessary in the investigation of any intricate point connected with the actions of vitality.

Galen was a warm admirer and encomiast of Hippocrates; he professed to agree with him in all his fundamental doctrines, and to aim at little more than to elucidate and amplify his principles. But although he sets out from the same point, he soon deviates into a more intricate path; and he becomes so involved in abstruse and complicated hypothesis, that we are no longer able to trace the simplicity of the original in the refined speculations of his commentator. He assumes the four elements and the four qualities; but in his application of them either to physiology or to pathology, he introduces so many minute distinctions and intricate combinations, as to give a new aspect to the doctrine. The real merit of Galen, however, consists in his knowledge of anatomy and in his acquaintance with the minute structure of parts, in which he made very considerable advances upon his contemporaries. The diligence which he displayed on these points is worthy of our warmest applause; yet the encomiastic flattery of his followers, by the excess to which they carried their admiration, has perhaps somewhat tended to diminish his reputation. From many circumstances connected with the history of the age, as well as from the candid confession of Galen himself, we may conclude that he rarely, if ever, dissected the human subject; but that he examined the bodies of apes, and of other animals the most nearly resembling it; and from these, by making what he deemed the proper allowances, he draws up his descriptions. But his zealous disciples would not admit of what they thought an imperfection in the works of their master; and to such an extent did they carry this principle, that they even considered it as more probable that the human body should have undergone a permanent change in its anatomical structure, than that Galen could have committed an error.

The superior talents of Galen, and the unrivalled reputation which he obtained, seemed to repress all further efforts for the improvement of physiological science; and his immediate successors, regarding him as beyond the reach of competition, were satisfied with implicitly adopting his opinions, without attempting to inquire into their correctness, or to extend their application. The spirit of the times but too powerfully coincided with this feeling. The Roman empire began to exhibit unequivocal marks of decline: in every department of literature there was a deficiency of genius, and nothing more was now attempted than to imitate the standards of excellence which had adorned the preceding age. But Rome, even in its most splendid period, had bestowed little attention upon the physical sciences; and the efforts which were now made were altogether imperfect and unavailing. Nor was the revival of letters in the 15th century, which roused the intellectual powers, after a dead repose of nearly 1000 years, productive of the same benefit to physiology as to many other departments of science. From a variety of causes, partly perhaps of an incidental nature, and partly depending upon the limited knowledge which was then possessed of the powers and properties of natural bodies, the physiologists of that period fell into the error of ascribing the phenomena of life to the operation of the laws which influence inanimate matter. Hence arose the contending sects of the chemists and the mathematicians; the former accounting for all the operations of the animal œconomy by the chemical action of the components of the body upon each other, the latter by the principles of mechanics. It is not necessary, in the present day, to enlarge upon the waste of genius and the misapplication of experimental research, which originated from this fatal error; it may be sufficient to remark, that although important facts were occasionally brought to light, and many elaborate investigations were instituted, from which some valuable information may be deduced, yet that not one single hypothesis was proved, nor one single principle established, of all those upon which so much labour and learning were bestowed.

While the chemical and mechanical sects were thus dividing the opinions of the most learned men of the age, a new doctrine was gradually rising up, which, although in the first instance it was equally remote from the principles of true science, yet after having received a number of successive purifications, it at length appeared in a more correct form, and occasioned the complete overthrow of both the contending parties. For this revolution we are indebted principally to Stahl. This distinguished character was brought up in the school of the chemists; but being possessed of a powerful understanding, he soon deserted the tenets of his preceptors, from a full conviction of their futility. He was forcibly impressed with the difference between the changes which the components of the body experience during life, and what would take place in the same substances under other circumstances. Hence he concluded that when they form a part of the living system, they must be possessed of some additional principle, that counteracts the effects that would otherwise be produced.

To the agent which thus opposes the physical powers of matter, and to which the body owes its vital properties, he gave the name of *anima*. He conceived it to possess powers of a specific nature, and he especially attributed to it a species of intelligence which enables it to act the part of a rational agent, and to superintend all our corporeal operations.[3]

To Stahl, therefore, we must ascribe the merit of clearly perceiving the inadequacy of the actions of either chemical or mechanical causes to explain the phenomena of life, a truth which we now regard as incontrovertible, and which, obvious as it appears, had been overlooked or disregarded by the most acute and learned of his predecessors. But after having thus established a firm basis for his hypothesis, he was led astray by the fashionable metaphysics of the day. Instead of investigating the nature of the animal functions, and ascertaining the laws which direct them, he deemed it sufficient to refer them all to an hypothetical principle, which he invested with powers accommodated to his purpose. In its general aspect the *anima* of Stahl may seem to bear a near relation to the φυσις of Hippocrates; but there is this essential difference between them, that Hippocrates employs his term merely as a general expression of the facts, whereas Stahl considers his hypothetical principle as

3 Theor. Med. ver.; Physiol. sect. i, mem. 3, § 13. It may afford a topic for literary discussion, how far Stahl borrowed his notions from Vanhelmont, whose hypothetical agent, which he named *archeus*, bears a near resemblance to the *anima*. But I should be disposed to refer Stahl's doctrine to Hippocrates, with whose writings he must have been conversant.

something distinct from the body, which actually produces its powers and faculties. But although the hypothesis of the *anima* was in itself gratuitous and altogether objectionable, it had the good effect of turning the attention to the phenomena more immediately connected with life, of enabling us to trace their connexion with the other operations of nature, and ascertaining more correctly the laws by which they are respectively governed. This progress was indeed attended with much difficulty, and the advances which were made in it were very gradual; but it is the correct plan of proceeding, and that which must eventually lead to the true theory of animal life, and to the just principles of physiology.

When knowledge is acquired by slow degrees, and truth is not elicited until after many unsuccessful efforts, it is not easy to assign to each individual the exact share which he contributed to the progress of improvement, or to ascertain precisely in what proportion his exertions may have conspired to the final result. In the present instance I am disposed to attribute a considerable share of merit to Hoffmann, who although a hasty and multifarious, rather than a correct and consistent writer, seems to have been one of the earliest who entertained correct notions respecting the general principles and objects of physiology. He had the sagacity to perceive that much of what Stahl ascribed to the operation of his *anima*, might be more correctly attributed to the action of the nervous system; and he appears to have been among the first who duly estimated the importance of this part of our frame in the vital operations.[4]

Contemporary with Stahl and Hoffmann was Boerhaave, a man perhaps equal to them in the general powers of his mind, or, if he possessed less genius and originality, he was superior in judgment and information. No one ever enjoyed greater fame as a teacher; and from this circumstance, as well as from their intrinsic merit, his doctrines acquired a degree of ascendancy over the public mind which had, perhaps, not been equalled since the time of Galen. But the genius of the age was not favourable to the continued dominion of any hypothesis; and as the theory of Boerhaave wanted the substantial support of facts, its celebrity did not long survive its founder. He was a professed eclectic; he selected from all preceding writers what appeared to be valuable in their respective systems, and endeavoured to mould the materials thus collected into one harmonious whole. Hence his system was the result rather of learning than of information; and although it indirectly tended to the detection of error, it induced a state of mind which led to its own downfal.

But whatever advances may have been made until this period in physiological science, they will appear

[4] Hoffmann's writings occupy no less than six large folios, and as he is generally deficient in arrangement, it is no easy task to select those parts which may exhibit the clearest view of his doctrines. His great work, "Medicina Rationalis Systematica," contains many remarks upon the nervous system, which show how much importance he attached to it. See particularly lib. i. sec. 3.

of small amount when compared with the mass of knowledge which burst upon us about the middle of the last century, and for which we are principally indebted to Haller. This celebrated man is, in every point of view, entitled to the appellation of the father of modern physiology, whether we regard the unremitting assiduity with which he cultivated the science, or the actual advancement which he effected. Every circumstance of talent, character, and situation, conspired to promote his great object. In learning, in industry, in discrimination, he has seldom been excelled; he devoted a large portion of his life to the cultivation of physiology, while his rank and fortune gave every facility to his exertions. What, however, more especially entitles him to the highest commendation, is the method which he introduced and established, of investigating the phenomena of the living body solely by observation and experiment, and keeping hypothesis entirely in subjection to these two leading principles. So powerful an effect indeed have his influence and example produced, that, since his time, the science has assumed altogether a new aspect; and from the publication of his "Elements," we may date the commencement of a new era in physiology.

This great monument of learning and industry was still in progress when Cullen entered upon his career; a man of a very different turn of mind, yet one who was eminently useful in this department of knowledge. He excelled in general views rather than in minute researches; and, without adding many new facts to our previous stock of information, he arranged into a very beautiful and interesting system those of which we were already in possession. Few persons have contributed more than Cullen to sweep away the useless rubbish of antiquity; and there is a spirit of philosophical scepticism that pervades his writings, which happily coincided with the inquiring genius of the age in which he flourished.

Among the authors who have been most successful in the cultivation of physiology, we must class John Hunter. He possessed a remarkable share of boldness and originality of conception; his mind was equally ardent and acute; and to these qualities he added the most patient industry in the investigation of nature under every aspect in which she presents herself to our notice. The high situation which he held in this metropolis, both as a practitioner and a teacher, and the noble memorial of his talents, which is deposited in the College of Surgeons, have conspired to raise his reputation to the highest pitch of celebrity. In the explanation of the operations of life, he professed to proceed entirely upon the result of observation and experiment; but, in this respect, he exhibited a singular example of self-deception, for his writings are, in fact, full of hypothesis and abound with theories expressed or implied; hence he has unhappily introduced into physiology a kind of metaphysical language, which has certainly tended to impede the progress of science, by substituting new expressions for new ideas; thus leading us to suppose that we had gained an addition to our knowledge, when, in fact, we were only employing new forms of speech. There is, however, no one since the time of Haller to whom the science is more indebted for new facts than to Hunter; and upon these his fame will be amply supported when his speculations are forgotten. In his physiological hypotheses, Hunter makes perpetual reference to the existence and operations of what he calls the vital principle. It is not easy, on many occasions, to determine how far his expressions are to be received in a literal, or how far in a metaphorical sense, but many of them strongly resemble the Stahlian doctrine, of an intelligent principle, connected with the body, directing its motions, and preserving it from injury or destruction. In his explanation of the functions and operations of the living animal, he not unfrequently confounds physical with final causes, and attributes to the specific effects of life, actions that ought to be referred to the powers belonging to inanimate matter.

Among the modern physiologists there is no one who has more just claim to our attention than Bichat, whether we regard him as an observer of facts, or an improver of theory. In the course of a short life he acquired an accurate and extensive knowledge of anatomy, and made many discoveries in this department of science, which seemed to have been so entirely pre-occupied by his predecessors. In his views of the animal œconomy, he proceeded upon the principles of correct philosophy; he regarded the vital functions as of a description essentially different from any other natural phenomena, and diligently applied himself to obtain an accurate knowledge of them, to observe their relation to each other, and to arrange them accordingly. His classification will, indeed, in many of its parts, appear too refined, and his speculations to savour too much of metaphysical subtilty; but we must regard him as having possessed an unusual share of genius and acuteness, and as having made very considerable additions to the stock of physiological knowledge.

This slight sketch of the labours of preceding physiologists will tend to point out both the cause of the imperfection of the science and the method of advancing it. We find that for a long course of years, every one who attempted to explain the operations of the animal œconomy, employed only those powers which belong to inanimate matter; and that, at a later period, when the inadequacy of this mode became apparent, instead of inquiring into the actual nature of the specific powers of vitality, it was deemed sufficient to have recourse to certain hypothetical principles, derived from false analogies or from the mistaken philosophy of the age. The more correct opinions of the present day, for which we are in a great measure indebted to the sagacity of Haller, have led us to conclude that all the appropriate actions of the living system may be referred to the two classes of motion and feeling; and that these depend upon two principles inherent in the body, contractility and sensibility,—the one seated in the muscular fibre, the other in the nervous matter. To the action of one or other of these principles, every corporeal change may be ultimately referred; and it is through their immediate operation that all the functions are performed. Hence we have a foundation for an arrangement of the functions into contractile and sensitive, to which I propose to adhere in the following work; and for reasons which will be hereafter more fully detailed, I shall begin with the former class. The functions which belong to this division are, the circulation of the blood, respiration, animal temperature, secretion, digestion, assimilation, absorption, and generation.

But, before I proceed to the individual functions, it will be necessary to give an account of the nature of the two powers of contractility and sensibility, and of the organs by which they are exercised—the muscles and the nerves. It will be also found advantageous to premise a description of membrane and bone, because these substances constitute, as it were, the basis of the body; and without some knowledge of their nature, it would be difficult to comprehend the operation of the muscles and the nerves, or the connexion which they have with the system at large.

After reviewing in succession the various contractile functions, I shall proceed to the other great division, the sensitive. These comprehend what are commonly styled the five senses—sight, hearing, smell, taste, and touch; and besides these are other classes of sensations, which appear equally specific, although from the mode in which they operate, or the organ by means of which they are exercised, their distinct nature has not been so generally recognised. Of these, some of the most important are the sensation that attends muscular contraction, that of heat and cold, and that of hunger.

The connexion between the corporeal and mental part of our frame is so intimate, that it is impossible to acquire a complete knowledge of the one, without paying some attention to the other. There is a very important class of phenomena of an intermediate, or perhaps, more properly, of a compound nature, where an effect upon either a contractile or a sensitive function is succeeded by some intellectual operation, or where an intellectual operation produces a change in the action of the corporeal organs. Some of the more important of these will be briefly noticed; and although it will be my object to encroach as little as possible upon the province of the metaphysician, I shall be unavoidably led to consider some of those topics which are only indirectly connected with physiology; of this description are the effects of association, habit, imagination, sympathy, and volition.

A very curious subject connected with the animal œconomy, which must engage a share of our attention, respects the causes which produce the differences between individuals, both those which more immediately affect the external form, giving rise to the varieties of the human species, as they are termed, and those which seem to depend more upon the internal actions of the system, constituting the temperaments. This subject will naturally lead us to notice the curious subject of craniology; and, connected with this, I shall venture to offer some observations upon the much controverted question, of the nature of the connexion between the intellectual faculties, and the organ by which they are exercised. In the last place, I shall make some remarks upon the natural progress of the animal body, from the commencement of its existence through its state of maturity, to its decline, and final dissolution, by which its component parts fall into decay, and its appropriate powers are at first impaired and ultimately destroyed.

ELEMENTS

OF

PHYSIOLOGY.

CHAP. I.

OF MEMBRANE.

When we examine the structure and composition of the animal body, the most obvious division of its component parts is into solids and fluids; the first being fixed and permanent in their nature, and affording the basis by which the general form is determined; while the latter are lodged in appropriate receptacles, formed by the solids, are generally in motion, or are undergoing some obvious changes in their quantity or quality. The science of anatomy, which professes to describe the mechanical structure of the body, and the physical relation which its parts bear to each other, is principally concerned with the solids; while both the solids and the fluids are equally the province of the physiologist, whose business it is to study the nature of all the substances that enter into the animal frame. The solids, as being the most durable part of the fabric, and as forming the organs necessary to prepare the fluids, and to apply them when prepared to their different uses, seem to offer themselves as the first objects of our attention; although, upon a more minute examination, we may find the fluids to be of no less importance in our œconomy, as either by the intervention of external agents, or by the action of their components upon each other, they are, in most cases, the media through which those operations are effected, which are essential to life. It must, however, be remarked, that the terms solid and fluid, as applied to the components of the body, are rather relative than positive; there is scarcely any part so solid, which may not by desiccation or by mechanical compression be rendered still more compact; and most of what have been called animal fluids are composed of water, containing different species of solid matter imperfectly dissolved, or merely in a state of mechanical diffusion. The principal varieties of solids, considered in relation to their form and structure,[5] are the bones, with their appendages, the cartilages and the

[5] The method of viewing the animal body as composed not merely of a number of organs, but each organ as itself composed of a variety of textures, which are more or less common to the different organs, appears to have originated with Dr. C. Smyth, as illustrative of the phenomena of inflammation. It was carried to a much greater degree of minuteness by Bichat, who extended it to all parts of the body, and employed it as a leading principle of his systematic arrangement. It must no doubt be regarded as one of the greatest improvements that have been introduced into our science; but I have not formally adopted it in the following pages, because the elementary nature of this work seemed scarcely to render it necessary; while, at the same time, it might have led to useless and tedious repetitions. The reader may, however, observe that the general principle is always held in view, and is, in many cases, directly referred to.

ligaments, the muscles with the tendons, the membranes of all descriptions, the various kinds of sacs and vessels, the fat, and the cerebral matter. If we arrange the solids of the body with regard to their chemical composition, and to the uses which they serve in the animal œconomy, we may place them under five divisions:—the osseous matter, the membranous, the muscular, the adipose, and the cerebral;[6] and we may say, in general terms, that the comparative degrees of their solidity or fixedness are in the above order. I shall not, however, follow this arrangement in the description of the solids, as the plan which I propose to adopt in the following pages is founded rather upon the functions which the body exercises, than upon its composition. I propose to give an account of both the solids and the fluids, as I successively treat of the functions of those parts in which they exist in the greatest quantity or the most perfect state; in consequence however of its general diffusion through all the organs of the body, it will be found convenient to commence with the membranous matter.[7]

[6] M. Beclard reduces the primitive classes of organized solids to three:—the cellular, the nervous, and the muscular; his division of cellular comprising the osseous, the membranous, and the adipose of the above arrangement. Add. à l'Anat. Gen. de Bichat, p. 2. This is likewise the arrangement of Cuvier; see Dict. des Scien. Nat. "Animal."

[7] I have not thought it expedient to adopt any of the arrangements of the subjects of physiology, which have been formed with so much labour and ingenuity by some of the modern French writers; those, for example, of Dumas, Bichat, Richerand, and

Membrane is the most simple in its structure of any of the organized parts of the body; it is the most extensively diffused, and exists in the greatest proportion. The coverings, not only of the body at large, but of each of its individual parts, both internal and external, are principally composed of membrane; and it lines all the cavities in which the different organs are situated. It constitutes the main bulk of the bones, and determines their figure; the earthy matter upon which their strength and hardness depend, being deposited in a tissue of membranous cells. Membrane also enters into the structure of muscles, not only affording them an external sheath, in which they are each of them enclosed, but the same matter is also interposed between their fibres, separating them into bundles, to which it, in like manner, affords a distinct covering, and these into still smaller bundles, until it appears at length to envelop each individual fibre. The membranous matter composes very nearly the whole bulk of the tendons, by which the muscles are attached to the bones; of the ligaments, by which the bones and other solid parts are connected to each other; and of the cartilages, which form the basis of many parts of the body, supplying the place of bone, and which also cover the ends of

Magendie. The object of these authors appears to have been to produce a system which should be equally applicable to physiology and to anatomy; but it may be doubted whether their plans have not become complicated and unnatural, in proportion as they are rendered more comprehensive. For some useful remarks on this subject, see Ed. Med. Journ. v. xv. p. 565.

the bones, and assist in the formation of the joints. It also enters very largely into the composition of the hair, the nails, and other similar parts connected with the surface. It likewise composes what is called the cellular texture,—a series of cells or interstices, which extends over a great portion of the body, fills up its intervals, and serves to unite the different parts to each other. Membranous matter is the chief ingredient in the glands, both those which are attached to the absorbent system, and those which are appropriated to the office of secretion. The brain is also enveloped in a covering of membrane; and it is probable that the nerves are composed of a series of fibres enclosed in membranous sheaths, analogous to those of the muscles. The pouches or sacs, which are found in different parts of the body, such as the stomach and the bladder, are almost entirely composed of membrane; and what perhaps must be regarded as the most important of all the purposes which it serves, this substance composes the principal part of the tubes or vessels with which the animal body is so plentifully furnished.

From this account of the extent and distribution of membrane, we find that it must exceed in quantity all the other solids of the body taken together, and that it enters as a principal ingredient into almost every part of the animal frame, the enamel of the teeth being, as we are informed, the only solid in which it cannot be detected.[8] This is indeed so com-

[8] Blumenbach's Physiol. § 22. See Hatchett, Phil. Trans. for 1799, p. 328.

pletely the case, that were it possible to remove the earth from the bones, the muscular fibre, the nervous matter, and the fat from the soft parts, to empty the vessels and to evaporate the fluids generally, the size and figure of the body would remain nearly unchanged. Membrane may therefore be considered as the connecting medium between the different parts of the body by which they are held together, the basis to which they are all attached, and the mould in which the particles of the other kinds of matter are deposited.[9]

It will appear from these observations upon the extent of membrane, that I employ the term in rather a more comprehensive sense than ordinary; and that I include under it, not only what have been usually called membranes, but the whole of the substance which possesses the same mechanical structure and the same chemical properties. What I have styled membranous matter nearly coincides with the white parts of the older anatomists,—the cellular texture of Haller, the tissu muqueux of Bordeu,[1] and the tela mucosa of Blumenbach;[2] the first of these terms was, however, used in a vague manner, without any very distinct appropriation; and Haller's, Bordeu's, and Blumenbach's appear objectionable, as they are derived from hypothetical opinions respecting the nature of this substance, which are probably not altogether correct.

The mechanical structure of membrane, as it exists

9 Cuvier, Tab. El. p. 25.

1 Recherches sur le Tissu Muqueux. 2 Physiol. § 21.

in the different parts of the body, has been minutely examined by various anatomists, and was particularly attended to by Haller. He described it as composed of a vast assemblage of lines or fibres, in their state of ultimate division too small to be perceived by the eye, but which, by the union of a sufficient number of them, are formed either into larger visible fibres, or into plates, according to the structure of the parts in which they are situated. He was at much pains to detect this fibrous structure in all the organs of the body, and to show that the membranes, however differing in their apparent texture, or whatever degree of firmness they possessed, were all resolvable into the same substance. This he calls cellular web; and although his idea of its structure may not be entirely correct, yet he made considerable advances upon the knowledge of his predecessors.[3]

All the solid parts of the body, he supposes, by mechanical division or by maceration in water, may be made to assume the fibrous appearance. In its most simple state the fibre is to be regarded as a straight line, and by the approximation of these lines, in different directions with respect to each other, all the various forms are produced that enter into the composition of the animal body. He further conceives, that the greatest part of the solids, in their primary state of aggregation, compose plates with interstices between them, and that the most compact

[3] El. Physiol. lib. i. § 2.

membranous body consists of this texture in a condensed state. Whether the tendons and ligaments ever actually possessed the mechanical structure of this cellular texture may be reasonably doubted; but it appears that, by proper methods, a structure somewhat resembling it may be exhibited in parts that are naturally of the densest consistence.

The uninterrupted continuity of the membranous matter, all over the body, was one of the discoveries of Haller. He employed much accurate dissection, and instituted many experiments, to prove this point; but it is now so generally admitted, as to render it unnecessary to adduce any arguments in its favour. It follows indeed as the direct consequence of the view which we have taken of membrane, regarding it as the basis of the whole body, into which all the other parts are moulded, by which they are at the same time enclosed, and which serves the purpose of connecting them together into one whole.[4]

The idea which was entertained by Boerhaave respecting the structure of membrane, is in itself so improbable, and seems so contrary to the evidence of the senses, that it is surprising it should have been so generally received;[5] and may serve as one among other proofs of the unlimited confidence which was formerly placed in all his opinions. He supposed that the simple fibres, or the smallest into which it

[4] El. Physiol. lib. i. § 1, 2; and Prim. Lin. c. 1, § 1—13.

[5] See Haller's Phys. lib. i. § 3, for a long list of authors who adopted it.

is possible to conceive them to be divided, by their union, compose a membrane of the first order or series, which, when coiled up, will form a vessel of the first order or series. These vessels, by being placed in contiguity or being interwoven together, form a membrane of the second order or series, which is again coiled up into a vessel of the second order; a third series of membranes and vessels is then formed, and others in succession, until they acquire a sufficient magnitude to be visible to the naked eye. According to this hypothesis it follows, that, except the earth of the bones, no part of the body is properly solid but the coats of vessels; and that, all the fibres which are cognizable by the senses, are only a congeries of vessels arranged in these ascending orders.[6] It is scarcely necessary to observe, that this hypothesis is entirely gratuitous, that there is no foundation for these regular gradations of vessels and membranes, and that the actual degree of vascularity of the different parts is infinitely varied. Both from the effect of injections and from microscopical observations, we may conclude not only that membrane generally is much less vascular than the muscular parts, but that different

[6] Although this may appear to be the fair deduction from the expressions that are employed by Boerhaave, it must be acknowledged that we do not find the doctrine laid down by him so explicitly as might be expected, from the statement of Haller, or even of Van Sweiten. See the references to Boerhaave, given by Haller; Van Sweiten's Com. on Aph. 39; also Gorteri Med. Compend. § 1—5, and fig. 1, 5, 14—21.

membranes differ very much in this respect from each other; and it even appears, that there are large portions of membrane which are without vessels of any description.

For the refutation of the hypothesis of Boerhaave, we are indebted to Albinus,[7] and still more to Haller; but in accomplishing this object, Haller probably went too far into the opposite extreme; for he is disposed to regard some of the ultimate parts of which the solids are composed, as unorganized.[8] This opinion he seems to have adopted, partly from an idea that a vascular structure is essential to organization, and partly, because, in the division of the larger fibres into those that are more minute, we must at length arrive at a fibre which is too small to admit of any further subdivision. But this latter objection is rather metaphysical than physiological, and refers more to the fineness of our instruments, than to the actual state of the parts, while the former is an assumption without proof, and will probably be found to be incorrect.

The idea that vascularity is essential to organization, or rather, that in descending from larger to smaller parts, organization and vascularity must cease at the same time, has been generally entertained by the most eminent modern physiologists. It seems to have originated, in a great measure, from the skilful injections of Ruysch,[9] who proved by his

[7] Acad. Annot. lib. iii. c. 1. [8] Prim. Lin. c. 1, § 16.

[9] See Albinus ubi supra.

preparations, that many of the white parts of the body, as they were termed, which were thought by the ancients to be entirely without blood, possessed numerous vessels demonstrable to the eye. Wm. Hunter, in his strictures upon the doctrine of Haller, adopts this opinion; he always speaks of organization as synonymous with vascularity, and takes it for granted, in reference to the minute structure of the body, that where there is no circulation there is no life.[1] An error of a similar kind formerly prevailed respecting the distribution of the nerves; for as sensation was admitted to be an appropriate quality of the living body, it was assumed that no living part could be without sensation; and of course, that the smallest parts into which an organized body could be conceived to be divisible, were nervous filaments.[2] But there is reason to believe, that both these notions are incorrect; and although our views must be, to a certain extent, hypothetical, when we venture to describe the structure of parts that are too small to be visible, yet we may at least form a plausible conjecture upon the subject.

We may agree with Haller in conceiving, that there is an actual solid fibre, the basis of the whole animal frame, to which the vessels are superadded as distinct appendages, but we cannot admit the existence of the inorganic concrete, which he describes

[1] Med. Obs. and Inq. v. ii. p. 27.

[2] Boerhaave, Inst. § 301; Prælect. § 440; see also Bell's Anat. v. i. p. 404.

as filling up the spaces between the fibres.[3] The fibre itself, although not essentially vascular, we must suppose to be a regularly organized body, composed of particles bearing a certain relation to each other, and possessed of certain specific properties. By the conjunction of these fibres, membranes of all forms are produced, and among others the vessels; but the coats of these vessels are composed of fibres that are themselves without an internal cavity, and have no kind of circulation through their substance. With respect to the question, how these ultimate fibres, which are without vascularity, can be said to possess life, I conceive it to be a dispute about words. If a certain assemblage of parts, when taken as a whole, exhibits vital functions, we may say that every individual portion of it is alive, although the imagination may form a conception of its ultimate parts, as not being possessed of any characteristic of life.

In the present state of our knowledge it is perhaps impossible to make any estimate of the size of the ultimate fibres of membrane. The current opinion among the physiologists of the last century was in favour of its being almost inconceivably minute; an opinion, which was partly founded upon the microscopical observations of Leeuwenhoek and others, and partly upon the hypothetical train of reasoning of which I have already given an account. Haller speaks of it as a geometrical line, or as possessing length without any

[3] Prim. Lin. c. i. passim.

sensible thickness; and to give an idea of its minuteness, he observes, that there are animals so small, that the most powerful glasses are barely sufficient to render them visible to the eye, yet these animals contain a complicated set of organs, each of which possesses a fibrous structure.[4] It may, however, be reasonably doubted, whether the ultimate fibre be of the same size in all animals; and, upon the whole, it is rather probable that in the human subject it possesses a magnitude, which is more within the limits of our comprehension.

We are indebted to Fontana for a number of microscopical observations on membrane; and although it is necessary to receive such observations with great caution, in consequence of the numerous errors and deceptions to which they are liable,[5] yet his remarks are so candid, and what he describes is so credible, that I am disposed to place some confidence in them. By using glasses of moderate powers, he found that a compact tendon was composed of a number of flat-

[4] El. Phys. lib. i. § 1.

[5] If in this, or in other parts of the following work, I may appear to speak in a disparaging manner of the labours of those who have devoted their time to microscopical investigations into the minute structure of the components of the body, I beg to observe, that it is done in no degree upon individual, but entirely upon general, considerations. An historical detail of the errors into which this instrument has led even those who have been the most skilful in its application, would have the effect of inducing us to place but little confidence in hypotheses and speculations that are derived from objects which can only be detected by the use of high magnifiers.

tened plates, which he calls the primitive fasciæ, and which are connected together by cellular substance of a more lax texture. By maceration, or mechanical division, these fasciæ were found to be made up of cylinders, in the form of solid threads of a spiral or waved form:[6] these, we are expressly told, are neither hollow nor vascular; homogeneous in their consistence, and of the same size in all the parts of the same animal. These primitive cylinders or tendinous threads are about the $\frac{1}{13,000}$ of an inch in diameter; they form a large portion of the substance of the whole body, and, according to the opinion which was prevalent when Fontana wrote, to which we have already alluded, he calls them non-organic[7]. These observations, as far as they can be depended upon, entirely oppose the vascular hypothesis of the Boerhaavian school, and present a much more rational and intelligible idea of the construction of the ultimate parts of the animal fabric.

The properties which more especially belong to membrane, are cohesion, flexibility, extensibility, and elasticity. It will be unnecessary to enlarge upon the importance of these properties in a system like that of the living body, in which great strength is necessary, together with lightness and a capacity for

[6] The observations of Monro would, however, lead us to suppose that this spiral or waved appearances is an optical deception. Obs. on the Nervous System, c. xxii. pl. 35, et seq.

[7] Sur les Poisons, t. ii. p. 222, et seq.; pl. 6, fig. 1, 2, 3, 4, 5; pl. 7, fig. 1, 2.

free motion, and where the parts are perpetually varying in their bulk and relative position.

Of these properties, elasticity may, in some degree, be regarded as the specific quality of membranous matter; as it appears to be the only one of the constituents of the animal body which possesses it. The muscular fibre is flexible, and is capable of being extended, but it does not appear that it is properly elastic. As we advance in our subject, we shall be more able to estimate the advantages to be derived from this property: at present I shall briefly notice, that it is an essential agent in the action both of the arteries and of the thorax, so that it is of prime importance in the functions of circulation and of respiration. In some instances it co-operates with the muscles in the motion of the joints, and it is frequently employed to restore the situation of a part that has been previously moved by muscular contraction from its natural position.

Besides the above properties, which are universally admitted to belong to membranous matter, but which it possesses in common with other natural bodies, some physiologists have ascribed to it qualities of a more specific or appropriate nature. Many of the modern French writers, as Bichat[8] and Richerand,[9] have supposed that the different forms of membrane have a degree of spontaneous contractility and sensibility connected with, or inherent in them; but this opinion seems to be derived from the idea that con-

[8] Traité des Memb. p. 54; Anat. Gen. t. i. p. 116.

[9] Elem. de Physiol. t. i. p. 48.

tractility and sensibility are necessarily attached to every part of a living body, and to membrane among the rest. So far as the opinion depends upon any general principle concerning the nature of a living body, I shall think it sufficient to refer to the observations which were made above, respecting the connexion between life and vascularity. And as to the facts which have been adduced to prove this point, I conceive them to be altogether inconclusive. The alleged instances, where pure membrane is supposed to exhibit marks of contractility, are either of a very dubious kind, and so obscure as to afford no sufficient ground for any theoretical deduction, or, when they are more obvious and decisive, they appear to be easily referable to the effects of elasticity. Of this kind are the shrinking of a cavity that has been preternaturally distended, the retraction of a tense membrane when suddenly cut across, and the collapse of tubes or sacs of various kinds, upon the removal of some extraneous force that had stretched them beyond their ordinary size; but in all these cases we see nothing more than the operation of the elastic power of the membranes, modified by their situation, or by the nature of the parts connected with them.

Professor Blumenbach ascribes to membrane a specific power, which he calls contractility, or vis cellulosa; but he employs the term contractility in a peculiar sense, and expressly distinguishes it from the moving power of the muscular fibre.[1] The con-

[1] Instit. Physiol. § 40, 59.

tractility of Blumenbach consists in the contraction which membrane is occasionally observed to exercise, when it has been over-distended; and the stretching force is withdrawn, and, like the cases mentioned above, may be referred to elasticity. The principal example of it which he adduces is the action of the cellular substance in propelling the serous exhalation into the lymphatic vessels—an operation which is very obscure, and with the nature of which we are too little acquainted for us to make it the basis of an hypothesis.

Under the title of tone or tonic power, the Stahlians formerly described a peculiar property as belonging to membrane; and this term has been lately employed in the same way by Bordeu[2] and Bichat.[3] Bordeu describes the tonic power as that property of the cellular, or, as he styles it, the mucous texture, by which each of the separate cavities or cells of which it is composed acts upon all those around it, and keeps them in a state of equilibrium or of uniform distention. Bichat expressly states, that what he terms the tonic power of membranes exists in all the three species into which he divides these bodies: the mucous, the serous, and the fibrous; the instances, however, which he adduces of its effects are not very explicit. This, as well as the tone of Bordeu, appears to be very similar to the vis cellulosa of Blumenbach; and it seems probable, that as far as they depend

[2] Recherches sur le Tissu Muqueux, § 70.

[3] Traité des Membranes, p. 62, 101, 133.

upon any one power, they may all be referred to the action of elasticity.

With respect to the sensibility of membrane, considered as a matter of fact, independent of any speculation concerning the nature of organization or vitality, the opinions of physiologists have been various; and it was especially the subject of a warm controversy, about the middle of the last century, between Haller and Whytt.[4] As is often the case in points of this nature, the question has been decided by a kind of compromise; it is generally admitted with Haller, that simple membrane is insensible in its healthy and natural state, but that it is liable to inflammation, and that it then becomes sometimes exquisitely painful.[5] The cause of this fact, the excessive degree of pain, which is excited by disease in parts that are, at other times, without sensation, is perhaps not altogether understood. We may remark concerning it, that one effect of inflammation is to enlarge the bulk of the inflamed part, and the pain is generally in proportion to the difficulty with

[4] Haller, Mem. sur la Nature Sens. et Irrit. des Part. and Op. Min. t. i.; Whytt's Essay on Sensibility and Irritability, and Appendix.

We are informed by Wilson, Lectures on the Bones, p. 44, that the doctrine of the insensibility of the periosteum and various other membranous bodies, while in their healthy state, was taught by Wm. Hunter in his lectures, as early as the year 1746, which was previous to the publications of Haller on this subject.

[5] Bichat, Anat. Gen. t. i. p. 119; Blumenbach, Physiol. § 210.

which the part admits of this extension. A high degree of inflammation may exist in loose cellular texture, and we may be scarcely sensible of its existence, while the inflammation of the periosteum of the smallest bone, as of a tooth, of the sclerotic coat of the eye, or of the tense membrane about the finger nail, will be almost intolerable. In these cases we shall probably always find, that even if the inflamed part be without nervous filaments, which give it sensibility, still that there are some branches of nerves immediately contiguous to it, which in consequence of the firmness of all the neighbouring parts, are pressed upon and irritated, while the blood-vessels connected with them are in a state of plethora; for it seems to be a general law of the animal œconomy, that no cause is more powerful in producing pain than a certain degree of pressure upon a nerve, while its sensibility is augmented by an unusual determination of blood.

It is probable that much error and confusion took place on the subject of the sensibility of membrane, among the anatomists and physiologists, after the revival of letters, in consequence of their blind veneration for the ancients. Hippocrates, who had but an imperfect knowledge of the existence and use of nerves, confounded them, or at least placed them in the same class, with the tendons, from some similarity in their visible structure and appearance, and having observed very serious effects to ensue from injuries of the proper nerves, he laid it down as a maxim, that tendons and other membranous parts are among the most sensible organs of the body.[6] This erroneous opinion materially influenced, not only physiological speculations, but medical and surgical practice, even as late as the middle of the last century, long after the distinction between nerves and tendons was thoroughly understood. Even Boerhaave fully subscribed to this doctrine, in which he is warmly seconded by his learned, but obsequious commentator, Van Sweiten;[7] and the influence of the old hypothesis upon our language may still be observed in the present day.[8]

John Bell, in his usual animated and impressive manner, describes the dreadful effects which this opinion, concerning the great sensibility of membrane, formerly produced in the operation of lithotomy.[9] As the bladder principally consists of membrane, it was agreed by all the learned operators, for a succession of ages, that it would be improper to cut or divide any part of it; and, therefore, in order to extract the calculus, a variety of instruments were employed for the purpose, as it was said, of dilatation, but which, in fact, caused the most

6 Fœsii, Œcon. Hipp. "Νευρον." See also Pliny, Hist. Nat. lib. vii. c. 20; Le Clerc, Hist. de la Medecine, liv. iii. c. 3, § 5; and Haller, Mem. sur les Part. Sens. et Irrit. t. i. p. 19; and Opera Minora, t. i. p. 411.

7 Aphorismus 164; and comment. in eund.

8 Stuart argues with much ingenuity to prove, that tendons and nerves are the continuation of the same substance, differing merely in the mechanical arrangement or disposition of their parts. Diss. de Struct. and Mot. Mus. C. vii.

9 Surgery, v. ii. sect. 3.

cruel laceration of the organ itself and of the neighbouring parts. It is truly astonishing to observe, how the weight of authority bore down the clearest dictates of reason, and the most decisive results of experience; and how the most obvious facts were warped and misconstrued, before mankind would submit to prefer the evidence of their own senses to the mere hypothetical opinions of the ancients.

From these remarks it will appear, that except bone, membrane may be regarded as the most simple in its properties of all the organized parts of the body. By this expression, I must be understood to mean, that the properties which belong to it, are likewise found in many other natural objects. Cohesion necessarily belongs to all solids, while flexibility, extensibility, and elasticity are possessed by many vegetable and some mineral substances, and also by dead animal matter; whereas spontaneous contractility and sensibility are the exclusive properties of the living body. We are, however, as much unacquainted with the intimate nature and immediate cause of the properties of membrane as of contractility and sensibility, only we are much more familiar with these operations.

As I have several times made use of the term organization, and shall frequently have occasion to employ it in the subsequent parts of this work, it may be desirable to give a clear explanation of it. In its most extensive acceptation, it may be regarded as nearly synonymous with the word arrangement, signifying that the parts of the organized body are placed according to some specific structure visible to the eye. Thus the serum of the blood, when coagulated and dried, in its chemical and mechanical properties, almost entirely agrees with membranous matter, yet in its texture it is obviously different. We say that the serum is not organized, because its texture is perfectly homogeneous, it is cut or broken with equal facility in every direction; whereas, in a tendon, which is organized, there is a regular distribution of the particles in a specific form, and according to a determined arrangement. The term is not so generally applied to mineral substances, yet in reality crystallization seems to be analogous to this kind of physical organization. It is doubtful whether the term can with propriety be applied to any fluid; and if, for reasons which will hereafter appear, it should be applied to the fibrin of the blood, still the greater part of the animal fluids can have no title to it. It is a question which we cannot perhaps very easily determine, whether any of the solids are not organized. I have already considered this point with respect to the ultimate fibres of membrane, but it may still be supposed to be the case with some other of the components of the body; for example, with the earth of the bones, which has been conceived to be merely deposited in cells of membranous matter, upon which its form entirely depends. This point we shall examine more fully when I come to treat upon bone; and as we advance in the subject, we shall more accurately learn what substances are organized, and what are to be considered as composed of unarranged particles, or rather, if there be any which fall under this description.

But besides this kind of physical organization, the word is employed by physiologists in a more restricted, but at the same time in a more correct sense, when it is applied to a system, composed of a number of individual parts, possessing each of them appropriate powers and functions, but all conducive to the existence and preservation of the whole. An animal body is thus said to be organized, or to consist of a number of organs or instruments. A vegetable, in like manner, is an organized body, consisting of separate parts, as the roots, the sap vessels, and the leaves, each of them constituting a distinct organ or instrument for performing some appropriate action, yet all composing one connected system. It is this species of physiological organization which properly distinguishes animate from inanimate matter; and where we are able to ascertain its existence, it may be regarded as a sufficient characteristic of the presence of life.

From these observations, it will appear that membrane has a perfect organization, although one which is more simple than that of some other parts of the body, as being possessed of fewer powers, and made up of fewer component parts. Some writers, especially of the French school, seem to regard organization as necessarily connected with contractility and sensibility; and to consider those parts which are neither contractile nor sensitive, as inorganic. I have already referred to the opinion of Fontana on this point, and a similar kind of doctrine is maintained by Dumas; he speaks of the cellular substance as being slightly organized, and even calls it a kind of inorganic sponge;[1] and a similar doctrine seems almost necessarily to follow from the view which Bordeu takes of the subject.

The opinion of the learned naturalist Cuvier respecting the nature of organization, is, on the contrary, somewhat more restricted than the one which I have adopted. He conceives it to be essential to an organized body that it be composed of both solids and fluids, the latter being the media through which its functions are performed, and the former being necessary to contain the fluids.[2] This view of the subject is probably correct, so far as respects any organized being of which we are able to ascertain the independent existence; but when we attempt to conceive of the ultimate parts of which it consists, we must at length arrive at a solid fibre, which contains no fluids, and which is, however, composed of regularly arranged particles.

With respect to the essential distinction between organized and unorganized bodies, this author points out the following circumstances: their structure, the mode in which they are originally produced, that in which they are supported, and that by which they are finally destroyed. With respect to structure, I have already remarked, that it is essential to an organized

[1] Principes de Physiol. t. ii. p. 5.

[2] Regne Animal, t. i. p. 14, 15.

body to be composed of separate parts, which are heterogeneous and dissimilar to each other, yet which all combine together to form one whole; whereas the parts of a simple unorganized body are homogeneous, so that into whatever number of portions it is divided, still each portion may retain every property of the whole mass, and constitute a perfect existence. Its individual parts have no relation to each other except those of cohesion and physical attraction; whereas the components of an organized body possess numerous relations of a more complicated nature, each having its appropriate and specific powers, which enable them to form a whole, to the perfection of which every individual part is necessary. As to the production, support, and destruction of organized bodies, and the way in which they differ from the same operations in those that are without organization, we shall be able to enter with more advantage upon these topics, when we have made ourselves acquainted with the functions to which they are respectively subservient.

Having now considered the structure and physical properties of membrane, I must proceed to give an account of its chemical composition and the effect of chemical re-agents upon it. So very imperfect was the knowledge of animal chemistry, even as late as the time of Haller and Cullen, that they supposed all the soft parts to consist of the same substance, differing only in its mechanical arrangement. Haller had an opinion, that membrane, being the least complicated part of the body, consisted principally of the simple fibres, which served as a kind of basis to the whole system, and that the fibre itself was composed of earthy particles cemented by gluten.[3] The discoveries of the pneumatic chemists, and especially of the French, who have assiduously cultivated this department of science, proved that Haller's opinion is fallacious, and that earth is not an essential constituent of membrane. His hypothesis of the connecting gluten is equally gratuitous, and is quite contrary to the more correct notions of modern chemistry. The particles of membrane, as well as those which compose any other solid, are held together by their attraction for each other, not by any connecting medium. It appears indeed that membrane acts mechanically in uniting the different parts of the body, and in maintaining the proper form of those substances, which are of so delicate a consistence, as not to be able to preserve themselves in a compact state, for want of a greater degree of cohesion between their particles. The soft pulp of the nerves, for example, and the adipose matter, seem to be retained in their present form, merely by the membrane in which they are imbedded; but this is quite independent of the consistence or structure of the membrane itself.

Although Cullen had no direct share in the great revolution in the doctrines of chemistry which commenced about sixty years ago, yet his notions on this, as on most other topics to which he paid any attention, were much more correct than those of his predecessors. He pointed out the mistake of Haller re-

3 El. Phys. lib. i. sect. 1.

specting the basis of the body being formed of solid particles united by a cementing material, and maintained, on the contrary, that the simple fibre is an homogeneous compound; but, with the exception of the bones, he conceives this compound, which he calls the animal mixt, to be of the same nature in all the different parts of the body.[4] So little indeed was he acquainted with the constitution of animal matter, that he expressly says, we know nothing of it, except that it consists of some concreting substance united to water, and that the differences which it exhibits in various parts of the body are merely owing to the proportion which the connecting matter bears to the water. When we recollect that Cullen formed his opinions on physiology before we were made acquainted with the gaseous bodies which enter into the composition of animal matter, and that the only method then employed for its analysis was simple combustion or destructive distillation, we ought not to be surprised at the incorrect opinions which he entertained upon the subject.

The experiments of Hales, who obtained large quantities of fixed air, as it was then termed, from urinary calculi, and afterwards those of Priestley, who procured azote from the muscular fibre, by means of the nitric acid, may be considered as among the earliest which threw any light upon the real nature of animal substances; but for the first regular analysis of them, and, especially, for the first attempt to dis-

[4] Inst. of Med. § 10, 13.

tinguish their different species, and to show the nature and proportion, both of their primary compounds and of their ultimate elements, we are principally indebted to the French. Fourcroy early devoted himself to the department of animal chemistry, and enriched it with many important discoveries; but his opinions are not to be regarded as, in all instances, entirely correct. This is the case with respect to membrane. Finding that a large, and as it seemed, an indefinite quantity of jelly could be extracted by boiling from many membranous bodies, he was disposed to regard membrane as essentially composed of jelly, or, at least, as differing from it rather in its physical, than in its chemical properties. Speaking of the different textures, the cellular, tendinous, ligamentous, &c., which he classes together, under the head of membranous or white parts, as possessing the same chemical constitution, he informs us that they dissolve entirely in boiling water, and form with it, while it remains hot, a viscous fluid, which, when cold, concretes into a transparent and tremulous jelly.[5] This statement is erroneous in two respects; in the first place, there is probably no single article of these enumerated by Fourcroy, which is completely soluble in water when boiling under the ordinary atmospheric pressure; and, secondly, although jelly, in a greater or less proportion, may probably be procured from all of them, yet they differ very much in the quantity which they

[5] System of Chem. Knowledge, by Nicholson, vol. ix. p. 319, et alibi.

contain; and there are some in which it forms only a small proportion of their solid contents.

We are indebted to Mr. Hatchett for a much more correct view of the subject; from his experiments we learn, that what may be considered as the basis of membranous matter is a substance, which, in its chemical properties, is identical with the albumen of the egg, when in a state of coagulation.[6] Albumen naturally exists in the form of an adhesive fluid, miscible in water, but when subjected to a temperature of about 165°, it experiences a remarkable change in its physical properties. By the operation of heat it is converted into a solid, which is no longer capable of being dissolved in water; and if after coagulation it be gradually exposed to a higher temperature, it is reduced to a firm semi-transparent body, very similar to some of the more compact varieties of membrane.

But although albumen appears to be the essential part of membrane, that which gives it its general form and determines its peculiar texture, yet it probably always contains jelly, and in some cases, even much more copiously than the albumen itself. Jelly is very soluble in water, especially when heated; it is thus separated from the albumen, and by the evaporation of the water, may be obtained in a state of purity. One of the most striking characteristics of jelly is the property which it exclusively possesses, when united to a quantity of water, of being dissolved by heat,

[6] Phil. Trans. for 1800, p. 399, et alibi.

and again becoming concreted by cold, without, as it appears, undergoing any change in its chemical constitution.

Another substance which appears to enter into the constitution of membrane, or is at least frequently found connected with it, is animal mucus. This, like jelly, is soluble in water, but it does not possess the property of gelatinization, and it differs from jelly in many of its chemical relations. Animal mucus appears to be nearly related to albumen; and indeed the constituent upon which its characteristic properties principally depend, would seem to be a mere modification of this substance.

A considerable proportion of both the bulk and weight of membrane, as well as of all the other soft parts, consists of water; and it has been supposed by many eminent physiologists that upon the relative quantity of the water and the solid matter depend many of the morbid changes of the body, as well as the natural varieties in the constitution and temperament of different individuals. Boerhaave entered largely into these speculations; they were refined upon, with much ingenuity, by his pupil and successor Gaubius,[7] and were, to a certain extent, adopted even by Cullen.[8] Membrane, when no longer forming a part of the vital system, is capable of having its properties much affected by the quantity of water with which it is combined; and there are some facts connected with pathology and the practice of medicine, which would

[7] Instit. Pathol. § 150—168. [8] Inst. § 13.

lead us to conceive, that the elasticity, and perhaps even the density, of some of the external parts of the body may be influenced by being exposed to warmth and moisture. But there is always great difficulty in applying these mechanical explanations to the living body; and when we reflect how many mistakes have occurred on the subject, we must feel the necessity of proceeding with the greatest caution.

With respect to the ultimate chemical elements of which membrane is composed, we find that, like other animal substances, it consists essentially of oxygen, hydrogen, carbon, and azote. It has been observed that membrane is less disposed to undergo the putrefactive fermentation than any of the soft parts of the animal body, from which circumstance, as well as from the relation which it bears to jelly, it has been supposed to contain a less proportion of azote than the muscular fibre; an opinion which appears to be sanctioned by the analysis of Gay-Lussac and Thenard.[9] The greater fixedness of membrane, or its less disposition to become putrid, may be immediately attributed to a stronger attraction between its particles, or it may depend merely upon its containing but a small portion of either blood or fat, substances

[9] Children's Thenard, p. 357; Thenard, Traité de Chimie, iv. 204.

The following are the results of Thenard's analysis:

	Carbon	Oxygen	Hydrogen	Azote
Fibrin........	53.365	19.865	7.021	19.934
Albumen......	52.883	23.872	7.540	15.705
Jelly	47.881	27.207	7.914	16.988

which have always a powerful tendency to decomposition. The more dense varieties of membrane contain less water than many other of the solids of the body, and this may be one reason why they are less disposed to putrefy; as we invariably find that moisture favours decomposition.

By the assistance of heat membranous matter is soluble in the mineral acids; its combination with the sulphuric and muriatic acids is not attended with any peculiarly interesting phenomena, but the effects which are produced by the nitric are more remarkable; the membrane is partly dissolved and partly decomposed, and several new compounds are produced, which will be more fully described when I treat expressly upon the analysis of animal substances. Membrane is also soluble in the pure fixed alkalies, and a saponaceous fluid is formed, which has been employed, in some instances, instead of the coarser kinds of soap, made in the usual process, by the combination of alkali and oil. Membrane has a strong affinity for the tanning principle; by uniting with a portion of tan, it is converted into a dense, flexible, elastic substance, little susceptible of putrefaction, which, under certain modifications, forms the basis of leather.

The jelly, which enters so largely into the composition of some kinds of membrane, has also a strong attraction for the tanning principle, and forms with it a substance, which appears in its chemical properties to be very similar to that produced by tan and the proper basis of membrane, the albumen. But it necessarily differs in its mechanical consistence, as the jelly, at least in the form in which it is usually employed, is dissolved in water, and of course without any regular organization of its particles, or even any adhesion between them. The mineral acids and the pure alkalies, when assisted by heat, readily dissolve jelly, but nothing very interesting or important results from their combination; the compound of jelly and alkali does not appear to possess that saponaceous property which exists in the compound of albumen and alkali.[1]

It appears from the analyses of MM. Gay Lussac and Thenard, referred to above, that jelly, as well as albumen, contains less azote than the muscular fibre; and they also found that the proportion of oxygen in jelly is considerably greater than in either of the other two substances. It is probably upon this circumstance, as well as upon its less compact mechanical constitution, that the greater tendency of jelly to pass into the acid state depends; and hence it is that jelly has been generally considered to be less completely animalized than the other soft parts of the body; for one of the characters which distinguish animal from vegetable substances is, that the former evolves an alkali, and the latter an acid, during their spontaneous decomposition. As, however, there are some animal bodies that become acid, so there are some vegetables that become alkaline, and are found to contain azote. These may

[1] Phil. Trans. for 1800, p. 379.

be considered as forming, on each side, the connecting link between two of the great kingdoms of nature.

An interesting experiment was performed by Mr. Hatchett, which tends to illustrate the difference between the chemical constitution of albumen and jelly: he found that if coagulated albumen be immersed for some time in diluted nitric acid, at the temperature of the atmosphere, it is gradually converted into a substance resembling jelly.[2] We may suppose that, in this case, the nitric acid parts with a portion of its oxygen to the albumen, and consequently, that jelly is to be regarded as differing from albumen in containing a greater proportion of oxygen; an opinion which is supported by the analyses of Gay-Lussac and Thenard.

The physiological relation which subsists between jelly and albumen is no less deserving of our attention. It is ascertained, that if we examine the same membranous parts at different ages, those of the young animal will be found to contain a greater proportion of jelly, and those of the older of albumen. It is on this account that the parts of young animals, such as the foot of the calf, are principally employed in the preparation of jelly as an article of diet; and every one must be acquainted with the difference between the soups formed from veal and from beef, in the greater proportion of jelly contained in the former. We thus perceive that the young animal,

[2] Phil. Trans. for 1800, p. 385.

not only in its physical and mental powers, but even in its chemical constitution, is less completely possessed of the characteristics of the animal than when it has arrived at a more mature age. As we advance in the subject, we shall find that the same observation applies to other parts of the corporeal frame besides the membranous matter.

Having described the structure, properties, and composition of membranous matter in general, I now proceed to give a more particular account of some of the different forms under which it exists. I shall confine myself at present to those species of membrane which are dispersed in various situations through the body, and are connected with all the different parts of it; as it will be more convenient to reserve for their appropriate places the account of those bodies that contain some other ingredients superadded to the membrane, which gives them their distinguishing qualities, as the bones and the muscles, and likewise those that, in consequence of their peculiar organization, serve for the purpose of some specific function, as the blood-vessels and the glands.

The first species of membranous matter that I shall notice is the cellular texture, as it is the most extensively diffused, and is that which has been conceived to form the basis of all the rest, or to constitute, as it were, the original structure, from which the others have all been produced. Many of the modern physiologists have paid attention to this substance, but Bergen would appear to be the first author who explicitly states the fact of its general diffusion,[3] while we are indebted to Haller for a number of experiments and observations, which form the correct foundation of all that we now know respecting it. According to this author, the cellular texture is found in nearly every part of the body, being contiguous to each separate organ, frequently entering into their substance, and connecting their different parts with each other. It is, in short, the substance which fills up the spaces that exist between every other part, which preserves them all in their proper situation, prevents them from unduly pressing upon each other, or interfering with their motions or functions of any kind. The description of it which was given by Haller, and which has been generally admitted by subsequent anatomists, is, that it consists of an irregular assemblage of plates that cross each other in various directions, forming a kind of net-work, and leaving a series of irregular cells.[4] Many circumstances prove that these cells communicate with each other. Thus when air or fluid of any kind is introduced into a part of this texture, it may be diffused all over the body, without employing such a degree of force as can be supposed sufficient to produce a rupture of the membrane itself. It is this inflation of the cellular texture by air that produces the disease of emphysema, where, generally in consequence of an accidental injury, a preternatural opening having been formed between the vesicles of the lungs and

[3] De Membrana Cellulosa, in Haller. Disp. Anat. t. 3, p. 79, et seq.; Bergen's Essay is dated 1732.

[4] El. Phys. lib. i. sect. 2.

some part of the cellular substance contiguous to them, a portion of the air which is received in inspiration passes into this texture.[5] In some of these cases the whole body has been puffed up in the most extraordinary degree, and the patient has actually been destroyed by suffocation. The fluid of anasarca, which is deposited in the same cells, although less moveable and less penetrable than air, is also liable to pass from one part of the body to another, in obedience to the laws of gravity, from the trunk to the extremities, and again from the extremities to the trunk, according as the posture of the patient has been erect or horizontal. And, to a certain degree, this is likewise the case with collections of pus, although, from well-known circumstances attending its formation, the transmission of this substance along the cells is less general and less extensive than in the former instances.

A valuable addition to the knowledge which Haller gave us respecting the cellular texture was made by W. Hunter, who first clearly pointed out a difference in the nature of the cavities that are contained in this substance, both in respect to the communication with each other and to the uses for which they are destined.[6] The adipose matter, which is dispersed over most parts of the body, is contained in the cellular texture; and Hunter's observations go to establish the fact, that the fat is not lodged indiscriminately

[5] This effect is distinctly noticed by Bergen, p. 85, as a deception practised by beggars and by butchers.

[6] Med. Obs. et Inq. v. ii. p. 26, et seq.

in all of them, but that it has peculiar cells destined for its reception, which do not communicate with each other, and which are distinct from those that contain the air in emphysema or the water of anasarca. These particular cells he calls, from their contents, adipose, in opposition to the general ones, which he calls reticulated. It is not pretended that any difference can be detected in the appearance or visible structure of these two kinds of cells; but the difference between them is conceived to be sufficiently proved by their situation and their contents. The parts of the body, where the fat is principally accumulated, are not the same with those which are the most subject to anasarca; and it is also observed, that fat never passes from one cell to another in the way that air and water do, but that each portion of fat always remains stationary in the same cell in which it was originally lodged. The opinion of Hunter, although it can scarcely be said to be demonstrated by any direct anatomical proof, is generally admitted to be correct, as it coincides with all our pathological and physiological observations, and is most consonant to our notions of the nature of fat, and the relation which it bears to the other parts of the system.[7]

An account of the structure and properties of the cellular texture was made the subject of a separate publication by Bordeu, but his description does not possess that degree of accuracy which can give much weight to his opinions. He entitles it the mucous

7 M. Beclard has offered some additional circumstances in favour of this opinion, p. 15.

texture, from an hypothesis which he formed of its origin. He speaks of it as consisting of fibres that are enveloped in a stratum of mucus, so as to compose a spongy texture, without any regular figures; and it may be inferred, that he regards it as almost without organization.[8]

The latest writer who has described the mechanical structure of the cellular substance is Bichat; and it is the more deserving of notice, as it professes to be the result of careful observation, and differs, in some respects, from that of his predecessors. He informs us that if we accurately examine a portion of the cellular texture, we shall perceive it to be composed of extremely thin plates, with a number of fine filaments crossing them. That the eye is not able to trace any thing further than these plates and filaments; that the plates do not exhibit any appearance of a fibrous structure, or, in fact, of any specific organization, and that the filaments seem incapable of more minute subdivision. It may be inferred from his expression, that this is an account of what we are able to detect by the naked eye. The filaments he supposes to be exhalent and absorbent vessels; but this conclusion is deduced, not from any thing of a vascular structure which was discoverable in them, but simply from the circumstance, that there must be an apparatus for exhalation and absorption, somewhere connected with these cells; and unless these filaments perform this office, we are ignorant by what

[8] Recherches sur le Tissu Muqueux.

means it is effected. Although the plates do not possess any visible organization, yet the author argues that they are organized, upon the general principle, that every part must be so which is connected with the living body.[9]

It is perhaps to be regretted, that such an accurate observer as Bichat did not employ the microscope, before he had given so decided an opinion upon the structure of these parts; for if we are to form any speculations upon the subjects of minute anatomy, there can be no doubt that, by the cautious use of this instrument, we may be enabled to discover parts that cannot otherwise be detected. The necessity for an organ may be regarded as a proof of its existence, but it is a species of reasoning which we should employ with great reserve. It seems very certain, that the cavities of the cellular texture, at least those which Wm. Hunter calls reticular, always contain more or less of an albuminous fluid, which is, in some way, separated from the blood; and it is equally certain, that this fluid would accumulate in them, were it not removed from time to time, by an operation, which can be performed by no method with which we are acquainted, except by the action of the absorbents. We may then say, that the cavities are provided with an exhaling and absorbing apparatus, but beyond this we are not able to proceed. Blood-vessels are seen passing through the cellular texture in various directions, part of which we may

9 Anat. Gen. t. i. p. 106, et seq.

presume are expended in support of the substance itself; and there are also branches of nerves found in it, but they appear rather to be crossing it, in order to be distributed to some other parts, than to be destined for the use of the texture itself.

With respect to the form of the cavities, we have no accurate knowledge. All the writers who have described them since Haller, speak of the cells as having an irregular or indeterminate figure, and generally compare them to those of a sponge. But this illustration, although it is commonly referred to, and is mentioned even by Cuvier,[1] cannot be considered as very appropriate, except so far as regards their free communication with each other. The cavities of a sponge are of a tubular form, whereas it may be presumed that those of the substance in question are rather narrow spaces with acute angles, the sides of which are flattened, and, when not forcibly expanded, we may suppose to be in contact.

Some physiologists have indeed gone so far as to deny altogether the existence of any cellular arrangement, and to assert that the appearance of cavities, which is produced by injecting air or fluid into this texture, depends not upon any cavities previously existing there, but upon the substance itself being so soft, and at the same time so tenacious, as to admit of having its parts distended and forced asunder, in

[1] Tab. Elem. p. 25. This learned naturalist, in the Dict. Scien. Nat. article "Animal," describes the cellular texture as consisting of a number of small plates irregularly thrown together, and forming small communicating cells.

the same way as if we inject air or fluid into a mass of softened glue.[2] But, notwithstanding the respectability of the authors who have advanced this opinion, I think it is clearly contrary to the obvious conclusion which would be drawn by an unbiassed observer of the various states of this texture, which appear to me totally different from what would take place upon the supposition of its being a uniform mass, not to insist upon the theoretical objection of any part of the body consisting of unorganized matter.

I have anticipated the greatest part of what might be said on the properties of the cellular texture in my remarks on membrane generally. Notwithstanding the opinion of many very eminent physiologists, I am not disposed to admit of its possessing either contractility or sensibility; but it exhibits more marks of vitality, or rather, it is more intimately connected with the functions of the system at large than some other species of membranous matter. It is especially liable to be the seat of inflammation, and in consequence of the laxity of its texture, is frequently the depository of large collections of pus. The theory of inflammation is a subject that must occupy our attention in a subsequent part of the work; at present it may be remarked, that an essential and obvious feature of this process consists in those vessels which generally contain only a colourless fluid, becoming so far enlarged as to admit of the red particles of the blood, that this change occurs in the vessels of the

[2] See Beclard, p. 19—23.

cellular texture, and that probably the seat of it is in the exhalents of the part. It is stated that the cellular texture possesses the power of speedily renewing itself, after it has been wounded or destroyed; and that in the process of the healing of wounds, or repairing injuries of any kind, it is this part which is first formed, or which first recovers itself; and it has also been supposed that it is the cellular substance which is first produced in the development of the embryo; but this opinion is derived from no very accurate discrimination of the nature of the substance, and is to be regarded as at least very problematical.

The chemical composition of the cellular texture, like that of membrane in general, has been thought to consist, in a great measure, of jelly. I have already referred to Fourcroy's account of it, but it appears that he had not a sufficiently correct idea of the nature of jelly, or at least of the distinction between jelly and the animal substances which the most nearly resemble it. Bichat says that boiling water first hardens and afterwards dissolves the cellular texture; but this statement is made in a loose manner, and it may be doubted how far it was the result of direct experiment.[3] Nearly a similar account of the chemical composition of the cellular texture is given by Cuvier;[4] but, like that of Bichat, it is stated incidentally, and is to be considered rather as a commonly received opinion, than as one explicitly adopted by this learned naturalist.

[3] Anat. Gen. t. i. p. 111. [4] Regne Anim. t. i. p. 26.

The next species of membranous matter which I shall notice are the bodies that are especially denominated membranes, to which the generic term was originally applied. They consist of thin semi-transparent sheets or plates, which generally form the coats or coverings of some other parts, and which differ from the cellular texture in the greater continuity of their structure. Haller, in conformity with his general doctrine on this subject, supposed that the proper membranes, like all the other white parts, consist merely of condensed cellular substance; and he was at much pains to prove this point, by examining them after they had been macerated in water for a long time, and were approaching to a state of decomposition.[5] To a certain extent this idea is well founded, but, as a matter of fact, independent of any speculation on their origin or the mode of their formation, it does not appear that the whole substance of membranes can be resolved into this texture; for although there may be a considerable quantity of it attached to, or connected with them, still there appears to be a solid basis, which is no longer capable of further subdivision into more minute plates. The proper membranes form the subject of a very elaborate publication by Bichat, in which he has arranged them into different classes, has minutely examined the structure and functions of each, and pointed out their connexion with the other parts of the system. His work contains many interesting details, and deserves

[5] El. Phys. lib. i. sect. 3.

to be carefully studied; yet I think that his distinctions are sometimes too refined, and that his opinions are occasionally rather ingenious than just. But there is a real foundation for the division which he makes of membranes into the three kinds of mucous, serous, and fibrous; of these I shall give some account in succession.

The mucous membranes are named from the peculiar semi-fluid substance with which their surface is covered, proceeding from numerous small glands that are imbedded in them. This kind of membrane always lines those cavities which are disposed in the form of irregular passages, or canals, that open to the atmosphere, and are connected with the skin at their extremities. Of these, the principal are the mouth, the nostrils, the œsophagus, and the intestines, composing the great system of the digestive organs, and those connected with the urinary organs, and the uterine system. These membranes are attached to the parts which they cover by a smooth and dense surface, while their external surface is soft and pulpy, and generally irregular from numerous projections of various kinds, which differ according to the uses for which the part is destined. According to Bichat they are divisible into distinct layers;[6] but this seems scarcely compatible with the account which he gives of their organization, and with the idea which is commonly entertained of their texture; so that it is probable that the layers which have been supposed to be

[6] Traité des Mem. art. ii. § 3.

detached from these membranes were merely portions of condensed cellular substance, by which they were connected to the subjacent parts. The mucous membranes are the immediate seat of some important functions: in the mouth and nose, they constitute the organs of taste and smell; in the stomach, of digestion; and in the intestines, of the assimilation of the food, and the separation of the nutritive from the fœcal part. On this account they differ from most membranous bodies, in being plentifully supplied with blood-vessels and nerves, as well as in possessing an extensive apparatus of glands and absorbents. As I shall have occasion to enter more particularly upon these points, when I treat upon the different functions that are connected with the mucous membranes, I shall at present only remark concerning them, that unlike most of the membranous bodies, they exhibit, in a high degree, the powers of vitality, and are intimately connected with all the general actions of the system.

The second species of membranes, the serous, differ materially from the mucous in their seat, their texture, and their properties. They are always found in close cavities that do not communicate with the atmosphere, as those of the thorax and the abdomen; they form coats for most of the individual organs which are essential to the animal œconomy, as the heart, the lungs, and the different abdominal viscera, and they frequently afford an external covering for those parts which are lined internally with a mucous membrane. The serous membranes in their texture are dense, smooth and compact, comparatively thin, but of considerable strength in proportion to their bulk, and are not divisible into any regular layers; they have their surface always moistened with a fluid which exhales from them, as is supposed, in the gaseous state. No glandular apparatus has been detected in them, and on this account the fluid has been ascribed rather to a kind of infiltration through small pores, than to what can properly be called secretion. Although in some states of disease the exhaled fluid accumulates in cavities that are lined with serous membranes, forming different species of dropsies, in health it is always removed as fast as it is generated, proving that the absorption exactly keeps pace with the exhalation. We have, therefore, a clear manifestation of the effect of these two processes; and yet we are not able to detect the apparatus by which they are carried on, a circumstance which, I conceive, is unfavourable to the opinion of Bichat, that the filaments of the cellular texture are a system of exhalent and absorbent vessels. The specific use of the serous membranes, as consisting of a smooth surface, which is always lubricated by an albuminous fluid, is to give a capacity for the free motion of the parts which they enclose upon each other, at the same time that they are prevented from adhering together, to which, from their frequently being in close contact, they would be liable, without the intervention of this fluid. The serous membranes have scarcely any vessels of sufficient size to convey red blood, and have very few, if any, nerves; they are therefore without sensibility, and exhibit only in a low degree the general powers of vitality. They possess a considerable share of elasticity and expansibility, but are not properly contractile, nor do they manifest any properties except those which are common to every part of the body.

The third class of membranes, the fibrous, are named from their obvious texture, as consisting of a visible assemblage of fibres, united into a continuous extended surface. They differ from both the former kinds in not being moistened by any fluid, but in their general aspect they are more similar to the serous, being dense, thin, and smooth, although, according to their situation, and the uses which they serve, they are more varied in their form and consistence. Among the most important of the fibrous membranes are the periosteum, which surrounds the bones; the dura mater, which lines the skull; the aponeuroses, those membranous expansions which surround certain muscles and separate them from each other; the capsules of the joints, and the sheaths of the tendons. The structure of these bodies is distinctly and obviously fibrous, and they seem to possess a texture which is more dissimilar to the cellular than any which we have yet examined. The fibres, so far as we are able to separate them by maceration or by dissection, appear to be strong dense cords, without blood-vessels, nerves, glands, or specific apparatus of any kind, and of course they are possessed of no properties but those which belong to every part of the body. Their use in the animal œconomy is principally mechanical; to enclose soft substances and preserve them in their proper form, to separate them from each other, and to keep them in their relative position.

The chemical composition of the proper membranes is similar to that of the membranous matter generally; they consist of a basis of albumen, united to different portions of jelly and mucus. It is stated, that in proportion to the density of their texture, is the quantity of albuminous basis compared to that of the other ingredients. The fibrous membranes consequently contain little else but albumen, while the mucous, at least some of the more delicate of them, are nearly soluble in water. The membranes contain no earth, and only a very small quantity of any saline matter; in an experiment of Mr. Hatchett's, 250 grs. of bladder left a residuum of no more than ·02 gr.[7]

The chemical composition of these bodies, as is the case with membranous substances generally, differs according to the age of the subject from which they are procured; in young animals they contain more jelly, while, as age advances, the proportion of albumen is increased.

Tendons and ligaments are bodies that nearly resemble the fibrous membranes in their minute texture and their chemical composition. It was upon tendons that Fontana's observations were made; and even with the naked eye we can easily observe their structure, as consisting of longitudinal fibres, lying parallel to each other, and closely united together.

[7] Phil. Trans. for 1799, p. 333.

It is generally admitted that no nerves are sent to them, that they possess very few if any blood-vessels, and no organs have been detected in them for the purpose either of secretion or absorption. Their principal use is to connect the muscles with the bones, and to serve as cords or ropes to transmit the action of the muscles to a distant point, and in doing this, their operation appears to be entirely mechanical. The ligaments, in their texture, nearly resemble tendons; they are, like them, compact, strong, and flexible bodies, but they are generally more dense in their consistence, and their fibrous texture is generally less distinctly marked. They have no nerves, but they have a few blood-vessels distributed to them, and they appear to possess somewhat more connexion with the vital powers of the system. Their use is sufficiently expressed by their name; they are principally employed in connecting the bones with each other, particularly about the articulations. In their chemical composition, tendons and ligaments nearly resemble the more compact membranes; their basis appears to be coagulated albumen, united to different proportions of jelly and mucus; they contain no earth, and only a minute quantity of saline matter.

Cartilages are bodies which, in many respects, nearly resemble ligaments, although they differ from them in some important particulars. It is not easy to perceive any fibrous texture in them; on the contrary, their obvious appearance is that of an uniformly dense, membranous matter, not extensible, but highly elastic. Their use is to cover the ends of the bones, especially about the joints, where, for the purpose of motion, a smooth and firm surface is required; and in many parts they supply the place of bone, where strength is necessary together with a degree of flexibility, as about the thorax, the trachea, and œsophagus. They are described by the most correct anatomists as being without visible vessels or nerves.[8] They appear to consist principally of albumen, with little, if any jelly and mucus; it is said that a portion of earthy matter is always found in them, which Dr. Davy estimates at $\frac{1}{200}$ of their weight,[9] but Mr. Hatchett does not consider it as essential to their constitution.[1] Cartilages appear to hold a kind of intermediate place between membrane and bone, a circumstance to which I shall have occasion to recur when I come to treat upon this latter body.

I have stated that the fibrous membranes, the tendons, and the cartilages, possess neither blood-vessels nor nerves; that they are not furnished with any organs that we can detect, for the purpose either of secretion or absorption; and that they do not exhibit any of the appropriate powers of vitality, being neither contractile nor sensitive; it may then be asked, how are they connected with the vital system, or in what sense are we to regard them as possessing life? The question, I acknowledge, is one that cannot be easily answered, and which may perhaps be thought to turn

[8] Haller, El. Phys. 29, 4, 27. Soemmering de Corp. Hum. Fab. t. i. § 27.

[9] Monro's Outlines, v. i. p. 68.

[1] Phil. Trans. for 1799, p. 381.

rather upon the definition which we give of certain words and expressions than upon any absolute facts that we can adduce. I have already mentioned, that some eminent physiologists conceive life to be always necessarily connected with vascularity and with sensibility; and these writers do not hesitate to call the dense membranes and the tendons dead or inanimate, seeming to regard them as only mechanically attached to the more vital parts.[2] But to this opinion it may be objected, that there is no portion of the body, which, as far as we can judge, has not a regularly organized structure, and that this can only be produced by the operation of some vascular action, probably analogous to secretion, by which the matter that composes them may be deposited, particle by particle, in its proper situation. We shall also learn, as we advance in the subject, that every part of the body is liable to decomposition, and is probably removed in process of time, particle by particle, in a manner precisely the reverse of that in which it was formed, and that this process can only be effected by the absorbent system. Although therefore we are not able to detect either a secretory or an absorbent apparatus in these parts, yet we have sufficient proof of their existence, and they must be considered as constituting the medium through which their vitality is preserved, and which connects them with the other parts of the living system.

Some writers, especially of the French and German

[2] Carlisle, in Phil. Trans. for 1805, p. 12, &c.

schools, have indeed attempted to explain how this addition and subtraction of matter may be produced without the intervention of vascular action, and have not hesitated to compare it to the effect of mere attraction, similar to what occurs in the formation of crystals. Linnæus, who was too fond of fanciful analogies between things that had little real resemblance, has, in some degree, contributed to this opinion by his well-known aphorism, which he employed to distinguish between the three kingdoms of nature. He says that minerals grow, vegetables grow and live, animals grow, live, and move.[3] But between the growth of minerals and that of animals there is this essential difference, that crystallization consists merely in the apposition of particle to particle, by which the substance has its bulk increased, but without those parts that are already formed having their arrangement, in any way, altered or changed. The consequence of which is, that if a crystal be broken into a number of parts, each part forms as perfect a crystal as the larger one; and, on the contrary, if a small crystal be placed in a saline solution, where it may receive additional matter, the small crystal will differ from the larger one simply in its bulk. But this is not the case with a membrane or a tendon. If we compare the corresponding parts in a young animal and in one that is fully grown, we shall find that the latter is not merely increased in size, or has received an addition of new matter at its external

[3] Philos. Botan. Introd.

surface, but that every individual part of it is of a different size, and is differently connected with the adjoining parts from what it was originally; so that it is demonstrable to the eye, that the small tendon could never have acquired the size of the large one, until the former had had all its particles gradually removed, and new particles deposited in their place. This subject will be considered more fully when I come to treat upon absorption; it is noticed at present for the purpose of explaining the difference between animal growth and the mere increase of bulk which constitutes crystallization. The proper answer to the question proposed seems therefore to be, that the life of these parts consists in their being under the influence of those actions by which the growth of the body is effected and its organization preserved.

All animals, except those of the simplest structure, possess an outward covering, which connects their parts together, protects them from injury, and prevents the too powerful impression of the external agents to which they are exposed. In the human species, and in those classes of animals that are the most nearly allied to it in their structure and organization, this part is called the skin; and I have classed it among the membranous bodies, because, although it possesses some characters peculiar to itself, it agrees with the membranes in many of its properties, both anatomical, physiological, and chemical.[4] The skin

[4] See Bonn de Cont. Mem., who perhaps carries the analogy between the skin and the other membranes too far.

has been divided by anatomists into distinct layers, or rather into distinct organs, which possess peculiar structures and functions; the principal of which are the epidermis, or cuticle; the rete mucosum; and the cutis, or true skin.

Of these the epidermis is the external covering. It is a thin semi-transparent body, adhering uniformly to the parts on which it is laid, and closely applied to all their inequalities. It does not possess any blood-vessels or nerves that can be detected, it exhibits no marks of sensibility, and seems to have but little connexion with the vital powers of the system. It is frequently destroyed from various accidents, and is quickly reproduced, without cuasing any material derangement, or any sensible change in the functions of the subjacent parts. With respect to its minute structure, we are informed, that it consists of a thin expansion, in which no specific texture of any kind can be perceived; for the laminated or scaly appearance, which was thought by Leeuwenhoek,[5] and some of the older writers, to be natural to it, appears to be the effect either of disease, or of mechanical violence. In some parts, indeed, where it is thicker than ordinary, it is capable of an imperfect division into layers; but these do not seem to possess any very distinct line of separation, and are irregular and not well defined.[6] Some physiologists have considered it as a substance merely spread over the sur-

[5] Arcana Naturæ, p. 205.

[6] Bichat, Anat. Gen. t. ii. p. 749, et seq.; Gordon's Anat. p. 327.

face, like a crust or film, and supposed it to be formed by exudation from the cutaneous vessels,[7] while both Bichat and Cuvier seem inclined to regard it as without any regular arrangement of its parts, and possessed of no visible organization.[8]

As the cutaneous perspiration issues from the greatest part of the surface of the body, it follows that the epidermis must be furnished with pores or passages of some kind for its transmission; yet, with the exception of Bichat,[9] anatomists have confessed themselves unable to detect these passages. Indeed, one of the most remarkable properties of this part is its power of retaining fluids of all kinds, and preventing their escape from the surface. It is well known that it retains, for some time, the matter that is discharged from the cutis by a blister; and those who are conversant with dissections must have observed how much less rapidly the surface dries up when it is not deprived of its cuticle.[1] Various explanations of this fact have been proposed; Winslow and Bonn suppose that when the epidermis is detached from the cutis, a portion of the latter adheres to the former, which mechanically closes up the pores; Albinus and Meckle, that it transudes through the

[7] Winslow, Anat. sect. 7, § 2, 140; Haller, El. Phys. 12, 1. 12; Meckle in Cruickshank on Ins. Persp. p. 10, 27.

[8] Anat. Gen. t. ii. p. 752, 757; Tab. Elem. p. 55; see also Lawrence's Lect. p. 274; Gordon more decidedly states it to be "truly inorganized or non-vascular." Anat. p. 239.

[9] Ubi supra, p. 746; see also Winslow ubi supra, 147-9; and Bonn, § 6.

[1] Chevalier's Lectures, p. 116.

substance of the cuticle; Bichat conceives that the pores pass through in an oblique direction, and, consequently, that their sides are pressed together when the body is distended; while the observations of Cruickshank[2] would lead us to conclude that the epidermis is possessed of a kind of elasticity which tends to close the pores, unless they are forcibly kept open by the passage of some fluid through them. Perhaps none of these suggestions entirely removes the difficulty; we may, however, go so far as to remark, that the matter of perspiration, being discharged in the form of vapour, is enabled to pass through very minute pores; and that the epidermis, when removed from the part to which it is attached, will shrink, and thus close up any openings which it possessed while in its natural situation.[3]

There has been as much difficulty respecting the vessels that secrete or convey the matter of perspiration as the openings by which it is discharged. Wm. Hunter conceived that he was able to detect them merely by separating the epidermis from the cutis, when they might be seen passing from one to the

[2] Cruickshank, p. 13, et seq.

[3] Mr. Chevalier's recent publication contains an account of some observations which he has made upon this part, which lead us to a new, and, as it appears, a more probable account of its functions than any which had been previously proposed. He conceives it to be composed of "an infinite number of small velamina regularly arranged," so as to form "a bibulous and exquisitely hygrometrical covering." Lectures on the general Structures of the Human Body, p. 133, pl. 2, fig. 1—6.

other, like the fine threads of a spider's web,[4] but this idea is not countenanced by the observations of subsequent anatomists.[5] Bichat, indeed, speaks of the exhalants that pass from the cutis to the epidermis as being sufficiently visible;[6] on this point, however, as well as respecting the pores of the epidermis, it is difficult to reconcile his descriptions with those of other observers of acknowledged accuracy. We are, therefore, still left in doubt, both respecting the organization of the epidermis, and its connexion with the other parts of the system: yet there are many facts which show that this connexion exists. The facility with which the epidermis is reproduced, when it has been destroyed, is alone a sufficient proof of this point, for the reproduction can only take place in consequence of a regular deposition of particles from vessels appropriated to the purpose. And its own structure, when considered as a whole, exhibits evident marks of what may be termed organization; for although it is difficult to see any thing of this kind, when we examine only small portions of it, yet we observe that there are particular parts of the body where the epidermis is always thick, and other parts where it is always thin, and this obviously connected with the uses of these parts.[7] It is also liable to a visible change in its

[4] Med. Obs. and Inq. v. ii. p. 52, pl. 1, fig. 1, 2.

[5] Gordon, p. 239; See Ruysch, Adv. Anat. 3, 8; and the Strictures of Albinus, Acad. Annot. 7, 3.

[6] Anat. Gen. t. iv. p. 746.

[7] Albinus, Acad. Annot. lib. 1, p. 27; Winslow ubi supra, 143.

structure from various morbid causes, so as necessarily to imply a connexion with the vascular system; from which we may infer that this is the case at other times, although the minuteness of the parts prevents us from discovering it. The analogy of the inferior animals leads us to the same view of the subject, for the scales of fish, the thick folds with which the elephant is covered, and other similar substances, are properly productions of the epidermis, or are analogous to what we consider as the epidermis in the human subject.[8] As far as its chemical composition has been examined, it seems to consist almost entirely of albumen.

There is a circumstance with respect to the epidermis which deserves to be noticed; its property of being thickened by pressure. I have already remarked, that it is naturally thicker in certain parts of the body, as in the soles of the feet, and the palms of the hands; and we find that, in these situations, the natural thickness is increased by exercise, especially if long continued, and not too considerable. The final cause of this change is very obvious; and it affords one instance, among many others, of that admirable adaptation of the organs to their appropriate uses, by which they are not only fitted for performing certain actions, but are endued with the power of accommodating themselves to incidental circumstances. But the physical cause of this change of structure is not so easy to comprehend, and we are scarcely in possession of any facts which

[8] Blumenbach's Phys. § 177, note.

can enable us to explain satisfactorily the mode in which it is produced. It may, perhaps, be referred to an increased action that is excited in the secretory vessels of the cutis, analogous to some other operations of the body, where an increase of vascular action in a part promotes its natural functions; but this explanation is not without its difficulties, and is, at least, entirely conjectural.[9]

The nature of the next layer of the integuments, which lies under the epidermis, has been much controverted. Its existence was first announced by Malpighi, who described it as a stratum of soft matter, disposed in the form of fibres, crossing each other in various directions, which was situated between the epidermis and the cutis.[1] Some of the modern anatomists have conceived it to be merely a thin layer of pulpy matter, without any distinct reticulated structure;[2] while Bichat, whose acuteness always entitles his opinions to great attention, altogether doubts its existence as a proper membrane, and supposes that what Malpighi saw and described is nothing more than a network of extremely delicate vessels, which, after having passed through the cutis, ramify on the surface in all directions.[3] There are, however, high authorities

9 This supposition would accord with Bonn's idea of the formation of the epidermis, as composed by the continuation of the cutaneous vessels, united together by the fluid which exudes from them, § 7.

1 Opera Post. p. 28, et alibi.

2 Blumenbach, de Gen. Hum. Var. § 42; et Physiol. § 180.

3 Anat. Gen. t. ii. p. 665.

in favour of the original opinion of Malpighi, some, at least, of which appear to be derived from original observation.[4] Cruickshank, who has examined the skin with great accuracy, speaks of the rete mucosum as a substance, of the existence of which he entertained no doubt, and which might be easily detected in all individuals, and even in some of the internal parts of the body.[5] We have a still more recent account of it by Dr. Gordon, who, after controverting the opinion of Bichat, informs us that it was easy to demonstrate the existence of a distinct membrane, between the cutis and epidermis, in the Negro, but that it was not to be found in the European.[6] It is difficult to decide between such high authorities; the evidence in favour of the existence of this body seems, however, so strong as scarcely to allow us to doubt upon the point; but we may, at the same time, coincide so far with Bichat as to suppose that the reticulated texture, which Malpighi described, consists rather of a network of vessels ramifying on the surface of the cutis, than forming a part of the corpus mucosum itself.[7]

Malpighi announced this body as being the part from which the colour of the skin proceeds; and,

[4] Albinus, Acad. Annot. lib. 1, c. 1, passim; Ruysch, Advers. Anat. 3, 8.

[5] On Insens. Persp. p. 3, et seq. p. 22, 36, et alibi.

[6] Anat. p. 242; see also Lawrence's Lect. p. 276; and Beclard, p. 272.

[7] For a perspicuous account of the opinions of the modern anatomists respecting this organ, I may refer to a valuable paper in the Ed. Med. Journ. v. 18, p. 247.

do not turn down the leaves -

whatever opinion we may entertain respecting its structure or its nature, it seems to be generally admitted, that neither the epidermis nor the cutis are the proper seat of colour, but that this depends upon something which is situated between them. In the Negro it is black, in the Chinese it is yellow, in the aboriginal American of a copper colour, while in the European it possesses different shades of red and olive, more or less approaching to whiteness.[8] These different shades of the skin afford a presumption in favour of the existence of the corpus mucosum, or of something corresponding to it: neither the epidermis nor the cutis of the Negro, when separately examined, are black; nor does it appear that there is any difference in the colour of the blood, so that their complexion would seem necessarily to depend upon something not contained in the vessels, and distinct from the other integuments. Besides the general question respecting the existence of the corpus mucosum, it has been asked, what is the exact nature of the colouring matter? is it inherent in the substance, or is it something superadded to it? It has been asserted that the colouring matter may be dissolved or suspended in water, and it has been compared to the pigmentum nigrum of the eye, or, by others, to the oily matter which gives the peculiar colour to the hair; but it is premature to form conjectures about the nature of a substance the existence of which is still doubtful.[9]

[8] Blumenbach, de Gen. Hum. Var. Physiol. § 181.

[9] It would appear to be this part that is the immediate seat of

The dark colour of the skin in the inhabitants of the torrid zone has been popularly ascribed to the influence of the sun upon the surface of the body,[1] but the tinge produced on the skin by exposure to a bright light appears to have no connexion with the permanent colour of the negro. The blackest complexions are not found in the hottest regions, and there are some considerable tribes, nearly under the equator, whose skin is whiter than that of many Europeans.[2] Besides, the brownness produced by the sun is not transmitted from parents to their offspring, whereas the children of negroes are equally black in whatever climate they are born, and their complexion is not altered by any number of generations; while we find, on the contrary, that after three or four successive stages, the original colour, whether white or black, is almost entirely obliterated by the union of parents from different varieties. It has not been ascertained upon what part of the integuments the sun acts, whether upon the epidermis, the corpus mucosum, or the cutis; but it is probably upon the epidermis, because we are informed that the tan of the skin may be removed by blisters.

As connected with the account of the corpus muco-

the peculiar colour which is produced by the internal use of nitrate of silver, as I have observed that where, in consequence of scars, the epidermis and cutis adhere together, the surface has not acquired the tinge. In a patient in Guy's Hospital, who had been repeatedly scarified, and had the skin blackened by the nitrate, the marks of the scarificator exhibited the appearance of white lines.

[1] Buffon's Nat. Hist. by Wood, t. iii. p. 405.

[2] Haller, El. Phys. 12. 1. 14.; Blumenbach's Phys. Elliotson's, note, p. 412. Lawrence's Lect. p. 291.

sum, it will be proper to notice a singular variety, which occasionally occurs, where the skin is entirely without colour. In the complexion of the fairest European female, there is always a mixture of red or brown, but in these individuals, who from their appearance have obtained the name of albinos, the skin is of a dead pearly whiteness. In almost all persons there is a correspondence between the shade of the skin and that of the hair and eyes, and this is found to be the case in the albino, for the hair is perfectly white, and the eye is without that substance which gives the various colours to the iris. From the relations of travellers, it may be supposed that albinos are more frequent in some parts of the world than in others, and especially among the Africans and Indians, but they are not very uncommon in all the temperate countries of Europe. This peculiarity appears in both the sexes, and has a tendency to become hereditary, but its origin is entirely unknown.[3]

The term Albino is derived from the Portuguese, and was applied by them to individuals whom they found on the coast of Africa, that in every respect resemble the negroes, except in their colour. The same description of persons has been also found in the isthmus of Darien, and in some of the oriental isles, and so numerously as to have induced some writers to conceive that they formed distinct tribes;[4]

[3] It would appear from the following passage in Pliny that this variety was observed among the ancients. "In Albania gigni quosdam glauca oculorum acie, a pueritie statim canos, qui noctu plusquam interdiu cernant." Nat. Hist. lib. vii. cap. 2.

[4] See Buffon, v. iii. p. 328, 344, and 419.

but for this opinion there appears to have been no foundation. In this part of the world we have sufficient proof that the albino is an accidental variety, although, as was remarked above, with some tendency to propagate itself when it has been once produced. Besides the whiteness of the skin and the peculiar appearance of the iris, which is of a bright rose colour, the eye is so sensible to light, that the individual is scarcely able to keep it open in the sunshine, although in the shade or the dusk of the evening, the vision seems to be perfect.

Buffon, according to his usual speculative manner, attributes this peculiarity to an effort of the constitution to assume, what he calls, the primitive colour of nature, which he supposes was white, and which has been changed, by various circumstances, into the shades which it now exhibits.[5] Saussure has given us an accurate and interesting account of two albinos that were born at Chamouni,[6] but it is to a conjecture of Blumenbach's that we are indebted for our knowledge of the cause. He conceived that the pink colour of the eye and its delicate sensibility depend upon the absence of the pigmentum nigrum, the black mucous substance which is spread over the posterior part of the organ. The conjecture of Blumenbach was completely verified by Buzzi of Milan, who had an opportunity of dissecting the eye of an albino, and found it to be entirely without the pigmentum nigrum. He afterwards examined the skin, and he

[5] v. iii. p. 422. [6] Voyages dans les Alpes, § 1037—1043.

found that the corpus mucosum was either entirely wanting, or at least that it was perfectly white, so as to escape his observation, and he naturally attributes the absence of colour in the surface to the state of this part. We may therefore conclude that the same cause operates upon the eye and the skin, that the redness of the one and the whiteness of the other depend upon the same physical defect in their organization, and that it is derived from the parent, although we are entirely ignorant of what it is in their constitution or habits which can give rise to this peculiar condition in their offspring. Albinos have been born in different climates and countries, and under circumstances that have no point of resemblance, to which we can, with any probability, refer the phenomena.[7]

Under the corpus mucosum lies the cutis, or true skin, a body of considerable thickness, tough, flexible, extensible, and elastic, of a dense texture, composed of a number of small fibres or plates, closely interwoven and firmly united together. Its external surface is compact and smooth, while the internal is more loose and irregular; it is connected to the parts below it by the cellular texture, and it passes into this substance by almost insensible degrees. Besides this, which constitutes the proper basis of the cutis, or as it has been termed, the corium, there

[7] Brewster's Enc. Art. Albino; Nich. Journ. v. xix. p. 81; Lawrence's Lect. p. 281, et seq. For some interesting observations on white varieties of animals, see Hunter on the Anim. Œcon. p. 243, et seq.

is attached to it a very extensive system of nerves, blood-vessels, and absorbents, which are dispersed over every part of it with the greatest minuteness. The sensibility of the skin differs very much in its different parts, but in its general extent it may be considered as possessing the most acute degree of feeling of any of the structures of which the body is composed; and it is accordingly observed in surgical operations, that the most severe pain is experienced during the division of the skin. Its external surface, when examined by a microscope, is found to be rendered unequal by little eminencies or projections. These, which have obtained the name of papillæ, are supposed to contain each of them the small branch of a nerve, of which they constitute the ultimate ramifications, and seem to be the immediate seat of the organ of touch, as well as of all the other sensations which reside in the surface of the body. They are the most easily detected, and are supposed to be the most numerous, in those organs which have the most exquisite sensibility, whether it be that of touch generally, as in the points of the fingers, or in other organs, where there exists some sensation of a more specific kind. The blood-vessels, with which the skin is so plentifully furnished, ramify in all directions over its surface, forming innumerable plexuses, and probably producing that appearance which Malpighi mistook for a reticulated membrane. These vessels render the skin one of the most vital parts of the body, subject to a variety of diseases, and intimately connected with various functions, especially with those of animal temperature, secretion, and absorption.

With respect to the minute texture of the skin, Haller describes it as being the same with that of membranes generally; he says it is composed of threads and plates, which are short, interwoven, and closely adhering together, the external part being more dense, and the interior gradually passing into the cellular texture.[8] The account which Bichat gives of it is essentially the same, except that he conceives it to be composed entirely of threads or fibres, which are interwoven together in all directions, leaving spaces between them of various forms and sizes.[9] This structure may be easily detected by maceration in water, when the tissue of fibres may be seen, with the interstices or areolæ, through which the hairs, vessels, and nerves probably pass, and by which they are supported. They do not seem, however, to pierce through the skin in a straight direction, but to pursue a winding course, so that it is very difficult to perceive the actual pores through which they have proceeded.[1] The small cells or cavities under the skin, formed by the membranous plates or bands which connect it with the parts below, are generally filled with fat, and there are also connected with certain parts of the skin, a number of sebaceous glands, which secrete an oily fluid, that is probably of a specific nature different from the fat.

[8] El. Phys. 12. 1. 2. [9] Anat. Gen. t. ii. p. 658, et seq.
[1] Gordon's Anat. p. 232.

The properties of the cutis must be considered under two points of view; those which are attached to it as composed of a membranous basis, which gives the skin its general form and consistence; and those which belong to the system of nerves and vessels that are connected with this basis. The properties of this basis are probably the same with those of the other kinds of membranous matter, and are altogether of a mechanical nature; it possesses cohesion, flexibility, extensibility, and elasticity in an eminent degree. In addition to these, some writers have ascribed to it contractility, and what they have called tonicity, but I conceive that this has been done without sufficient foundation, and that the facts which have been adduced, are either of a very dubious nature, or are more properly referred to the effects of elasticity.

We conclude partly from analogy and partly from observation, that the papillæ contain the ultimate terminations of the nerves, and are the immediate seat of the sensation which resides in the skin. In all parts of the body it is found that the sensibility of the nerves resides principally, if not entirely, in their extremities, where they are either divided into extremely minute filaments, or spread out into a thin expansion. In what degree the microscopical observations that have been made upon the cutaneous nerves enable us to trace them into the papillæ is perhaps a little doubtful; but so far as they can be depended upon, they lead to the opinion, that this is their ultimate destination. Besides the nervous filament, the papillæ are supposed to contain, each of them, a minute branch of an artery and a corresponding vein, together with an exhalent and an absorbent; but the existence of these latter vessels appears to be derived rather from conjecture than from actual observation. It is however certain that the skin is the seat of an extensive system of exhalation and absorption, although it may be very difficult to determine the actual termination of the vessels, or the exact apparatus by which these functions are performed. With respect to the properties of the cutis, considered in its most extensive relations, we may therefore conclude that, in addition to the mechanical qualities mentioned above, it possesses those immediately dependant upon nerves and blood-vessels, but that it is without contractility, which is exclusively attached to the muscular fibre.

Although the chemical composition of the cutis has been much attended to by the modern experimentalists, our knowledge concerning it is still imperfect. The best English systematic writers, as Aikin, Brande, Henry, Murray, Thomson, and Ure, describe it as consisting chiefly of jelly, and the same opinion appears to be generally adopted by the French chemists. Mr. Hatchett, whose researches on these subjects are peculiarly valuable, also regards the skin as being principally composed of a kind of jelly, although of a more dense consistence and less soluble nature than ordinary. Seguin, who has paid particular attention to the chemical composition of the skin, in connexion with the process of tanning, enters more minutely into the subject, and supposes that it consists of two parts, which differ in their chemical, as well as in their physical properties: a texture of interlacing fibres, which form its basis; and a semifluid matter mechanically interposed between them. The fibrous part he considers to be nearly similar to the muscular fibre, and to be formed of an oxidated jelly, and the semifluid matter to be of a mucous or gelatinous nature.[2] The idea of the fibre consisting of oxidated jelly appears to be quite hypothetical, and as far as we have any light thrown upon the subject by experiment, I should be led to the opposite conclusion, that the jelly is more oxidated than the fibrous part of the skin. Upon the whole it is probable that the fibrous part of the skin, which constitutes its proper substance or basis, is composed of albumen, like the other membranous bodies, and that it has intermixed with it a quantity of matter, of a different chemical nature, which we may suppose to be a compound of jelly and mucus.

There is a class of bodies connected with the external surface of almost all animals, which, although very various in their shape and appearance, are analogous to each other in their origin and their chemical composition. They may be divided into two varieties, the first consisting of nails, claws, hoofs, scales, &c.; the second of hairs, bristles, wool, quills, and feathers. The first may be considered as weapons of defence or protection; they are either productions of the skin, or at least they are so intimately connected with it,

[2] Fourcroy's System by Nicholson, v. ix. p. 353.

that it is often difficult to detect the exact line of demarkation between them. They are generally considered as more immediately attached to the epidermis; and it is observed, that, in some instances, they occupy the place of this body, lying directly upon the cutis, and not having any thing exterior to them.[3] We may frequently perceive in this class of bodies a kind of fibrous or laminated texture, although this entirely disappears in those that are the most dense, when they become nearly homogeneous. They are chiefly composed of albumen, with different proportions of jelly and mucus.

Hair and feathers differ materially from the bodies just described, both in their origin and their structure; they proceed from a kind of bulb or root, which is situated below the cutis, through which they pass, and project beyond its external surface. They consist essentially of an external tube and an internal pulp. In hair the tube is very delicate, and is entirely filled with the pulp; in the quill the tube is firmer, and the internal part is proportionably much smaller in

3 The nails in particular are described by anatomists as being actually a production of the epidermis of the finger; and in proof of this it is stated, that by maceration, the nail may be removed along with the epidermis. Haller, El. Phys. 12. 1. 15; Winslow, sect. vii. 192; Albinus, Acad. An. lib. ii. c. 15. But these two bodies differ so much, both in their structure and in the manner in which they are connected with the contiguous parts, that I conceive it would be more proper to say, that the nail occupies the place of the epidermis, and that they adhere firmly together at their junction, than that they constitute the same organ.

quantity. Although hair seems so smooth to the touch, we are informed by Bichat,[4] that it actually possesses an imbricated or bristled texture, the processes all pointing in one direction, from the root to the tip, analogous to the feather part of the quill, and that it is upon this structure that the operation of felting depends, in which the hairs are mechanically entangled together, and retained in this state by the inequalities on their surface.[5] Next to the bones, hair is said to be the most indestructible of the constituents of the body; and there are accounts of its having been found in old tombs, after all the soft parts had entirely disappeared. The hair of different individuals differs considerably in its thickness, being, as it is said, from $\frac{1}{300}$ to $\frac{1}{700}$ of an inch in diameter; and it is no less variable in its other physical properties, some kinds being much more dense and elastic than others, a circumstance which, according to Mr.

[4] Anat. Gen. t. iv. p. 787.

[5] There is, however, reason to doubt the existence of these bristles, which, it appears, have never been detected by the most powerful microscope; see Leeuwenhoek's figure in Phil. Trans. No. 140; Fontana on Poisons, tab. i. fig. 1; and the plates in Hooke's Micographia, pl. iii. fig. 2; also Young's Nat. Phil. v. ii. p. 190. Dr. Fleming, Phil. of Zool. v. i. p. 88.; and the author of the art. "Anatomy," in Dr. Brewster's Enc. v. i. p. 842; describe the hairs as being conical, tapering from the root to the tip. Dr. Gordon, Anatomy, p. 444, conceives them to be solid, and the same idea was maintained by Hooke. The different opinions which microscopical observers have held on this point, which it might be supposed could have been so easily decided, afford a useful illustration of the degree of confidence which we ought to place in such observations.

Hatchett, depends upon the proportion of jelly which it contains.

We are indebted to Vauquelin for an elaborate analysis of hair, from which we learn, that it consists principally of an animal matter, united to a portion of oil, which seems to contribute to its flexibility and cohesion. Besides this there is another substance of an oily nature, from which the specific colour of the hair is derived, and there are also small portions of iron, manganese, sulphur, and the phosphate and carbonate of lime.[6] The animal matter, which constitutes nearly the whole bulk of the hair, is conceived by Vauquelin to be a species of mucus; but Mr. Hatchett has more correctly designated it as being chiefly albumen, united to a small quantity of jelly. Vauquelin found that the colouring matter of hair is destroyed by acids; and suggests that when it has suddenly changed its colour and become white, in consequence of any great mental agitation, it is owing to the production of an acid in the system; but this idea seems very hypothetical, and I conceive it more probable that the effect depends upon the sudden stagnation of the vessels which secrete the colouring matter, while the absorbents continue to act and remove that which already exists.[7] As the colour of

[6] Ann. Chim. t. lviii. p. 41.

[7] I have suggested an explanation of the fact upon the supposition that it is an actual occurrence; but although every one must have heard of numerous instances of the hair becoming suddenly grey, I do not find any cases related where it happened under the immediate observation of the narrator; and when

the hair seems to depend upon a peculiar kind of oil, and as there is often a correspondence between the colour of the hair and the skin, it has been supposed that the colouring matter of the corpus mucosum must, in like manner, be of an oily nature; the conjecture is not without plausibility, but it has not been confirmed by any direct facts or experiments.

In their natural state all these bodies are without sensation, and they possess no visible blood-vessels; but under certain circumstances, they are subject to a species of inflammation, when vessels may be detected, at least in some of them, and they become acutely sensitive. The painful sensations in this case appear to proceed, not from any nerves that are distributed to the organs themselves, but from the increased bulk of the part, as produced by the state of inflammation pressing upon and irritating some contiguous nerves, in the same manner as in the inflammation of the ligaments and tendons. An obvious use of hair, in the inferior animals, is to protect the body from external cold, but except on the head,

we reflect upon the manner in which the hair grows, by protrusion from the bulb, it is certainly difficult to conceive how, when once grown, its physical properties can be changed. The existence of the disease called Plica Polonica, which might seem an analogous circumstance, is now generally disbelieved. Among the older writers we meet with narratives, apparently well authenticated, where the hair is said to have continued to grow after death, and even to attain an extraordinary length, but, upon whatever evidence they may appear to rest, we may safely conclude that there is some fallacy or inaccuracy in the statement.

this cannot be considered as applying to the human species, nor can we easily conceive what is its object in our œconomy; yet it is contrary to our ideas of the nature of things to suppose that what is so constantly found to exist should not be formed for some useful purpose.

CHAP. II.

OF BONE.

In treating of bones I shall first give an account of their external form, their internal structure, and their physical properties; afterwards we shall examine their chemical composition; in the third place I shall inquire into the mode of their formation; and shall conclude with some remarks upon the nature of their connexion with the other parts of the living system.

§ 1. *Form and Structure of Bone.*

With the general form and appearance of bones every one is sufficiently familiar; they are hard and inflexible bodies, without contractility or sensibility, very little subject to decay, and are perhaps the only substances to which the term solids strictly applies. They serve as a defence and support to the soft parts, either affording them a case, in which they are lodged and protected from injury, as in the instance of the brain and lungs; or as pillars to which the more flexible and delicate organs may be attached and kept in their relative position, as particularly takes place with respect to the muscles. The bones are also fixed points against which the muscles re-act; when they commence their contractions, they form a system of cylindrical levers, by which all the movements of the body are effected; and they likewise very essentially contribute to these movements, by the share which they have in the formation of the joints; so that, in conjunction with the muscles, they constitute the principal organs of the important function of locomotion. In man and the higher orders of animals the bones are generally speaking in the interior of the body; and even when they approach towards the surface, are always covered by muscles or membranes; but in the crustacea, the testaceous mollusca, and in certain insects, the bones compose an external case within which all the soft parts are contained.

The larger bones are nearly uniform in different individuals, but there is some irregularity among the smaller ones, so that the total number of bones in the skeleton is not always the same; in general, however, they amount to about 260,[8] exhibiting every variety of figure and size, according to the structure and uses of the particular parts in which they are found. They may be arranged into three classes—the long round bones, the broad flat ones, and the short bones approaching more or less to the square form; to the first class belong the bones of the upper and lower extremities, to the second class those of the skull, and to the third the vertebræ. These three kinds of bones, as we shall afterwards find, differ not merely in their external shape, which may be conceived to

[8] Sœmmering, Corp. Hum. Fab. § 12. Boyer, Anatomie, t. i. p. 12. Monro's Outlines, v. i. p. 12.

be an incidental circumstance and one of little importance, but likewise in the more essential points of the mode of their growth and their mechanical structure. They are also distinguished by the uses which they serve in the animal œconomy. The long bones are more immediately adapted for the purposes of motion, either enabling us to shift our position from place to place, constituting what is termed locomotion, or to act upon other bodies that are contiguous to us, as is especially the case with the hands and arms; the flat bones obviously serve for the protection of the soft parts; while the third class of bones are usually found in those organs, where it was necessary to unite in the same part a considerable degree of strength with the capacity for free motion.

It would be foreign to the purpose of this work to enter upon a description of the forms or uses of the individual bones, but it may be proper to make a few observations upon the beautiful mechanism of this part of the animal fabric, and to show how admirably each of its individual organs is adapted to its particular use. For this purpose we may take the example of the upper and lower extremities. In the human subject the arms are obviously intended, not for support, but for acting upon contiguous bodies. They are therefore so attached to the trunk as to be easily applied to them in all directions, the upper part admitting of free motion, and at the same time possessing considerable strength, while the extremity of the limb is composed of a great number of smaller bones, that have less motion upon those immediately connected with them, yet the whole assemblage constituting an apparatus which is capable of executing all the various movements that are necessary for the purposes of life, with a degree of precision and velocity that would be almost inconceivable, were we not so familiar with its operations. The arm is so placed as to be applied the most easily to the objects that are before us and nearly on the same level; the joints of the elbow and wrist are obviously fitted for the same purpose; while the structure of the hand and fingers points them out as the organs of what has been called prehension, and as being singularly adapted for examining the texture and figure of the bodies that are within our reach.

The lower extremities are equally fitted for their specific object,—the support of the body and its various progressive motions. They are strong pillars so placed as to bear its weight in the most advantageous manner; the foot is so adjusted to the leg as to form a firm basis, while its smaller parts possess that degree of motion upon each other, which assists in the changes of position, without admitting of that variety of complicated actions that are observed in the hand, which, in the foot, would have been not only useless but even injurious, as necessarily diminishing the stability of the body.[9] Without going into a more minute detail, it may be asserted, that if

9 Cuvier, Leçons, t. i. p. 473, &c. Bichat, Anat. Descrip. t. i. p. 284, et seq. Blumenbach, de Gen. Hum. Var. § 5, 10. Mr. Abernethy's third Physiological Lecture contains many interesting observations on the bones and joints.

a skeleton was to be found of an unknown animal, with extremities formed like those of man, we should be at no loss to decide concerning its general habits; that it was essentially a biped, that its body was intended to be kept in the erect position, that it was neither a flying nor an aquatic animal, but that its natural abode was the surface of the earth. Nothing therefore can be more unfounded than the speculations of those metaphysical physiologists, on the one hand, who, for the purpose of assimilating the human form and functions to those of the monkey, have conceived that man was naturally a quadruped, nor of those, on the contrary, who consider the latter animals as bipeds. Technically speaking, they are quadrumanous,[1] their extremities all possessing the characters which point them out as instruments of prehension.

The form and structure of the articulations are among the most interesting parts of the animal œconomy. According to the language of anatomists, every part where two bones are connected together is denominated an articulation, whether they admit of any degree of motion upon each other, or are firmly fixed together;[2] but I shall only notice in this place those articulations which are moveable, where the bones are united by ligaments, or other membranous bodies of a flexible nature, so as to be capable of changing their direction or relative position. The

[1] Buffon, v. x. p. 15. Cuvier, Tabl. El. p. 94.

[2] Sabatier, Anat. t. i. p. 20. Boyer, Anat. t. i. p. 55. Bichat, Anat. Gen. t. ii. p. 174.

moveable articulations present a great variety of forms, which have received appropriate technical names, but they may be generally referred to two principal classes, —the ball and socket, and the hinge. In the ball and socket joint the moveable body is furnished with a round end, which plays in a corresponding hollow in the fixed bone; while in the hinge both are furnished with processes and depressions, which are mutually adapted to each other. The hip-joint is an example of the first, and the elbow of the second species of articulation; it is obvious that the first admits of a rotatory motion in all directions, while the second is capable of being moved in two directions only.

Although the general form of the articulation may be observed in the solid body of the bone itself, yet in most cases cartilage materially contributes to the accurate completion of these parts, and the whole extent of the articulating surface is always covered with this body. Many obvious advantages arise from this construction. The smoothness of the cartilage, as well as its elastic nature, admits of a more easy motion than could have existed if the two hard substances had been in immediate contact, while, at the same time, the parts are less liable to injury from violent concussion, than if they had possessed a more rigid texture. In order to facilitate motion, by diminishing friction, the joints are enclosed in a membranous bag, filled with a dense lubricating fluid, called synovia, which is always interposed between the moveable extremities. To complete the mechanism of these parts, they are provided with a suitable apparatus of ligaments, which serve to keep the bones in their relative situations, and to regulate the motions of the joints, so as to prevent their displacement, except under circumstances of extraordinary violence. To enter into a description of the ligaments, or indeed of any of the individual joints, would be to encroach upon the province of the anatomist; I shall only further observe on this subject, that in no part of the body is the adaptation of means to ends more apparent than in the construction of the joints and the apparatus connected with them.[3]

The mechanical structure of bone formed a part of the investigations of Malpighi, and he is considered as having been the first who announced that its basis consists of an animal matter, the texture of which resembles that of the cellular substance.[4] The experiments of Duhamel proved, that the animal matter, under certain circumstances, assumed a laminated appearance;[5] but we are indebted to Herissant for the important fact, that bone contains an earthy matter, and that many of its specific properties depend upon this ingredient. He distinctly states that bone is essentially composed of two substances,—the one a cartilaginous basis or parenchyma, which gives the general form to the part; the other a peculiar earthy matter, which is deposited in the

[3] An interesting example of this kind has been lately pointed out by Mr. Earle, in the structure of the spine in certain birds. Phil. Trans. for 1822, p. 576.

[4] Anat. Plant. p. 19.

[5] Mem. Acad. for the years 1739, 1741, 1742, 1743, passim.

cartilaginous basis, and is the cause of its hardness.[6] This may be demonstrated by digesting bone in diluted muriatic acid, so as to dissolve the earthy matter without acting upon the membrane, when we procure a substance, retaining its former bulk and shape, but converted into a soft, flexible, and elastic body. In this process we have removed the earth, and left the membrane, and by burning the bone we may reverse the operation, for we may suffer all the animal matter to be consumed, while the earth is left untouched, preserving, in a great measure, its former texture.

The general opinion among modern anatomists respecting the structure of bone, and the manner in which its membranous part is arranged, is, that like the other soft solids, it is essentially composed of fibrous laminæ or plates, which are so connected together, as to form by their intersection a series of cells, analogous to those of the cellular texture, in which the earth is deposited. Gagliardi conceived that the plates were held together by small processes, like nails, the form of which he minutely describes;[7] but this has not been confirmed by subsequent observations, and seems to have been a mere

6 Mem. Acad. for 1758, p. 322.

7 Anat. Ossium, passim, and fig. 2. This treatise would appear to exhibit one of those remarkable cases of self-deception, which are occasionally met with, even in the palpable science of anatomy. Probably in the same light we must regard the account given by Havers of the longitudinal and transverse pores; see his Ostologia, § 35—37. Havers's description is, however, partly sanctioned by the authority of the elder Monro. Anatomy of the Bones and Nerves, p. 13.

fanciful conjecture. Bichat has even denied the existence of the laminated structure of bone, and has endeavoured to show that all the facts and experiments which seem to demonstrate its presence are fallacious, and depend, either upon the peculiar mode in which the bone has been treated by the operator, or upon some other cause, which induces the laminated appearance, although the laminæ did not previously exist.[8] To a certain extent the opinion of Bichat may be correct. By a kind of loose analogy, which is so often introduced into all departments of science, the substance of bone has been described as consisting of regular concentric rings, like those that compose the trunks of trees; an analogy that was probably derived from the hypothesis of Duhamel respecting the formation of bone, which will be presently noticed. These concentric layers certainly do not exist,[9] but I think it equally certain that the membrane of bone is composed of plates, very similar in their general form and disposition to those of the cellular texture, and it is probable that the earthy matter is inserted between these plates, and thus is likewise disposed to

[8] Anat. Gen. t. ii. p. 155, et seq. Cheselden says, "Nor are the parts of bones disposed into visible lamellæ, stratum super stratum, as many have painted." Osteographia, Introd.

[9] The mechanical structure of the membranous part of bone has been elaborately developed by Scarpa, who has detailed a series of accurate observations on it in its various states of growth and disease, as well as by subjecting it to the action of chemical reagents. He very satisfactorily refutes the idea of the membranous part of bone being composed of a series of regular concentric laminæ. See his essay De Penit. Oss. Struct. p. 16, et alibi.

assume the laminated structure. The proof of this structure will appear when we come to consider the internal conformation of bone, and the appearances which it exhibits when partially decomposed.[1]

When a bone is divided longitudinally, so as to disclose its internal structure, we observe its different parts to exhibit a variety of appearances, especially with respect to the greater or less compactness of its composition. These varieties have been reduced to two,—the hard or compact, and the alveolar or spongy. Generally speaking, there is no bone which does not exhibit both of these textures, the compact forming its external, and the spongy its internal part. The long bones consist of a hollow cylinder of compact matter, including a quantity of the spongy substance ; but the proportion of the two varies much in the different parts of the same bone. The shank or body of the bone consists principally of the compact with but little of the cellular matter, while the extremities or heads of these bones are principally composed of the cellular matter with only a thin crust of the compact substance. It has been asserted, al-

[1] As we may presume that the earthy part of the bone is moulded into its appropriate form by the membrane into which it is deposited, we may judge of the structure of the latter by that of the former, which from its firmer consistence it is more easy to ascertain. Now whether we examine the bone during its formation in the fœtal state, or after it has had its membrane destroyed by the action of fire, we find the earth to assume the appearance of fibres, which, when the bone is perfected, have a tendency to a laminated arrangement.

though I do not find that the experiment has been accurately made, that equal cylinders of the same bone, taken from different parts of their length, contain the same absolute quantity of solid fibres, but differently disposed. The flat bones generally consist of an external covering of the hard substance on each of their surfaces, with a layer of the spongy matter interposed between them; while, in the short bones, the disposition and proportion of the two kinds of texture is more irregular. In the large bones of the extremities, where the structure is seen to the most advantage, the compact substance is found to be a completely solid body scarcely exhibiting any visible arrangement, either fibrous or laminated; but, as we proceed towards the inner part, we find the substance to be less and less dense, until, at length, it becomes completely cellular, forming what have been termed the cancelli. In the centre of the bone there is scarcely any of the spongy matter, and a considerable hollow space is left, which is filled up with a series of membranous cells, in which the marrow is lodged: some writers have called this the reticulated part of the bone.

I have been thus particular in describing the structure of the long bones in order to show how admirably the arrangement of their parts is adapted to the purposes for which they are destined. Their extremities are the fixed points from which the muscles re-act, and where greater space was required for the insertion of the tendons; their diameter is, on this account, considerably increased, and their osseous matter is disposed in nearly an equal degree through their whole substance; while, in the middle of the bone, which is more exposed to external violence, and where nothing was wanting but mere strength, the bony plates are all consolidated together into a compact dense ring, leaving the centre nearly hollow. This form of the part, as consisting of a quantity of compact matter disposed round a central cavity, has the important effect of increasing the strength of the bone without adding to its weight. The resistance of a cylindrical body to a force applied transversely may be mathematically demonstrated to be increased in proportion to its diameter, so that the same number of fibres, placed as it were round the circumference of a circle, produce a stronger bone than if they had been all united in the centre, and the diameter of it had been proportionably diminished.[2] We accordingly find that the hollow cylindrical bones are always placed in those parts of the body where the power of resisting external force was an important object; but where, at the same time, it was very desirable not to add unnecessarily to their weight.

Although the hard external part of the bone is a perfectly compact body, in which we can scarcely perceive any trace of a specific organization, yet there is reason to conclude that it is made up of fibres and plates similar to those of the spongy or

[2] Monro, Anatomy of the Bones and Nerves, p. 21; Porterfield, in Ed. Med. Essays, v. i. p. 112.

cancellated part, and differing from it principally in its greater degree of condensation. When we examine a bone during the process of ossification,[3] we find that those parts which afterwards become the most completely solidified are of an evident fibrous texture; and we observe the fibres to become more and more numerous as the process advances, and to adhere more and more closely together, until, at length, the substance becomes perfectly compact. And when a bone is subjected to any operation, by which its substance is decomposed, and its texture destroyed, as by calcination, by maceration in diluted acids, or by long exposure to the atmosphere, it always exhibits a laminated or fibrous appearance, and shows a tendency to separate into longitudinal portions. Besides, the transition from the compact to the spongy part of the bone is not marked by any decided limits, but they pass into each other by insensible degrees, so as to show that there is no essential difference between them.

The direction of the fibres is found to vary, in the three kinds of bones, according to their respective forms; in the round cylindrical bones they are long, and lie parallel to each other,—in the flat bones they generally exhibit a radiated structure,—while in the short bones their direction is more irregular, depending, in each particular case, upon the figure of the bone to which they belong.

In its physical properties, bone is the most sim-

[3] Albinus, Icon. Oss. Fœtus, tab. 1, fig. 1, 2.

ple of any of the components of the body. Membrane, as I remarked on a former occasion, is not possessed of any properties that are peculiar to the living system, and which do not belong to many other substances; but bone, when in its most perfect state, is neither flexible, extensible, nor elastic, and, in short, has no mechanical properties but those which belong to every kind of solid matter.

§ 2. *Chemical Composition of Bone.*

The chemical nature of bone was very imperfectly understood until about forty years ago. It had, indeed, been discovered by Herissant to be a compound of an animal and an earthy substance, but nothing was known respecting the exact nature of either of its ingredients. Gahn seems to have been the first who discovered that the earth was the phosphate of lime;[4] an earthy salt, which is insoluble in

[4] The claim of Gahn to this discovery, which was long doubtful, is, at length, fully established by Berzelius. See Progress of Animal Chemistry, p. 76.

The more accurate researches of contemporary chemists, and especially of Mr. Hatchett, MM. Fourcroy and Vauquelin, and Prof. Berzelius, have discovered that the earth of bone is less simple than was previously supposed to be the case. Besides the phosphate of lime, which forms nearly 82 per cent. of the weight of the earth, it contains, according to Berzelius, the fluate and the carbonate of lime with the phosphates of magnesia and soda. Thomson's Chem. v. iv. p. 485. The analysis indicates a considerable excess of lime above that necessary to saturate the acids; and the same excess, although in a little different proportion, is indicated by the experiments of Mr. Dalton; Manchester Mem.

water, bears a high temperature without being decomposed, so as to give to bone the property of resisting, in a remarkable degree, most of the external agents to which it is exposed, and to render it the most durable of any organized body with which we are acquainted. Accordingly the bones of animals are found in a tolerably perfect state after a lapse even of many centuries, and after having been exposed to all the revolutions to which the surface of the earth is incident. Indeed, from the discoveries which have been lately made by the modern geologists, we are induced to believe that bones still remain, which have existed long before any traditionary or historical records of which we are in possession, and when the earth was peopled by animals of a different kind from any of its present inhabitants.[5]

The nature of the animal matter of bones was still longer in being understood, and we are indebted to Mr. Hatchett for our knowledge on this subject. He found it to possess all the characters of condensed albumen, the substance which I have already mentioned as the basis of membranous matter of all descriptions. Of the different species of these bodies, it appears the most nearly to resemble cartilage; and, from the observations that have been made on the original formation of bone, it is reasonable to conclude that it is identical with this substance. Be-

v. iii. ser. 2d, p. 5; this, however, as he observes, may, perhaps, be owing to a quantity of carbonic acid being driven off by the calcination.

[5] Clift in Phil. Trans. for 1823, p. 84.

sides the solid animal matter, bones contain a quantity of jelly, which may be extracted from them by boiling,[6] and we find that this jelly is much more abundant in the bones of young than of old animals. Before the experiments of Mr. Hatchett, the same erroneous opinion was entertained respecting the animal matter of bones as of membrane, that it consists of jelly, an opinion which is maintained even by Bichat[7] and Cuvier,[8] as well as by other eminent physiologists, whose works are of recent date. Perhaps this may be the case with some of the bones of very young animals; but, with respect to the perfect bones of the adult, it is certain that, unless the water be applied under a degree of compression so as to raise its temperature much above the ordinary boiling point, as takes place in Papin's digester, a small portion only of the bone will be dissolved.

Besides the marrow which occupies the central cavities of some of the larger bones, the pores and cancelli of the bone itself contain a kind of oily matter, which has been thought to differ from marrow merely in possessing a greater degree of fluidity.[9] The marrow is said to be lodged in a series of mem-

6 Proust in Journ. Phys. v. liii. p. 227; Berzelius on Animal Chem. p. 78; see also Ann. Phil. v. xii. p. 106, for the process employed at Geneva for procuring jelly from bone; but, as is remarked by the editor, it is the substance alone to which the term "gelée" is applied that we are to consider as jelly.

7 Anat. Gen. t. ii. p. 160, et seq.

8 Tab. Elem. p. 32; Leçons, t. i. p. 103.

9 Boyer, Anat. t. i. p. 38.

branous cells which, like those in which the fat is deposited, do not communicate with each other; while, from the observations that have been lately made by Mr. Howship, it seems probable that what has been called the oil of bones is deposited in longitudinal canals that pass through the solid substance of the bone through which its vessels are transmitted.[1]

Many conjectures have been formed concerning the use of the marrow and the oil of bones, but they all seem to be unsatisfactory. The general opinion of physiologists about the time of Boerhaave and Haller was, that the oil served to render the bones less brittle, and that the marrow was deposited in the centre to be carried into the body of the bone, and diffused through its substance, as it was required for this purpose. Even the most approved of the moderns, as Sabatier and Boyer, seem still to attach some importance to this hypothesis, for it is stated by them, although, perhaps, with less confidence than by their predecessors.[2] As to the oil, it does not appear that it could have the effect which has been assigned to it under any circumstances, and it is still less probable when considered in its actual relation to the bones, because it appears that the oil which is found in them, is rather lodged in separate cavities than mixed up with the earthy matter, or diffused through the substance of the bone generally. But

[1] Med. Chir. Trans. v. vii. p. 393, et seq.

[2] Sabatier, Anat. t. i. p. 16; Boyer, Anat. t. i. p. 40.

after discarding the old hypothesis, we have little that is more satisfactory to offer in its room; the only plausible conjecture that I can form is, that the marrow and the oil of bones serve the same purposes in the animal œconomy with the other oily secretions; and, as it was desirable for the bones to be either hollow, or filled with a substance which should not add much to their weight, advantage was taken of this circumstance to employ them as deposits or reservoirs for adipose matter.[3] With respect to the use of the fat generally, this will be treated of hereafter.

§ 3. *Formation of Bone.*

There are few subjects in physiology that have afforded more scope for speculation and hypothesis than the origin of bone, or the manner in which the process of ossification is accomplished. The ancients, who were ignorant of the nature of bone, could not be expected to form any accurate notions on this subject, and they accordingly satisfied themselves with saying, that there was present in the fluids an ossific matter which became condensed, as some thought, by the operation of animal heat, some by the evaporation of its watery parts, or, according to others, by mere pressure. But these opinions, and many others equally vague and gratuitous, were refuted by the modern physiologists, and particularly

[3] Mr. Wilson, in his Lectures on the Skeleton, entertains the same idea respecting the use of the marrow, p. 48, et seq.

by Haller and Albinus.[4] Haller made a number of minute observations upon this point, which, although they did not lead him to a perfect knowledge of the subject, at least enabled him to avoid the gross errors of his predecessors. His opinion was that as the growth of the body generally depends upon the arterial blood, so that of each of its individual organs is immediately effected by an impulse given to the vessels of the part, by which an additional quantity of fluid is carried to it; and that, in consequence, either of some provision of the system, or of some occasional exciting cause of a more mechanical nature, the action of particular arteries is augmented at certain periods of life so as to cause their successive developement. With respect to the bones, his idea was that the osseous particles being, as he styles them, of a gross nature, the small arteries of the fœtal bones are not capable of receiving them. At a certain period, however, as the heart acquires more force, it propels its contents more powerfully, and thus distends the vessels, and enables them to receive the earthy particles. But, after a certain quantity has been deposited, and the bone has acquired a certain degree of firmness, its rigidity resists further distention; and, at length, by the continued addition of the osseous matter, the whole becomes solidified, and concretes into a perfect bone.[5]

[4] See Acad. Annot. lib. vii. c. 6, for a sketch of the opinions of the earlier anatomists upon the nature and formation of bone, as well as for the author's own views upon the subject.

[5] El. Phys. 29. 4. 23, et seq.; Op. Min. t. ii. p. 595, &c.

Many objections present themselves against this hypothesis, both when it is considered in its general outline and in its detail. It is altogether of too mechanical a nature, and attributes all those changes to a mere alteration in the diameter of the vessels, which probably depend upon some action immediately connected with the functions of life. If we descend to particulars, we may ask what became of the osseous matter before the arteries of the future bone were sufficiently capacious to receive it? The arteries of the bone begin to convey the earthy particles before they are large enough to admit the red particles of the blood, so that, from the very earliest period of fœtal existence, we have arteries which are obviously larger than those that are sent to the bones when they begin to acquire their earthy matter; why, then, we may ask, was not this matter deposited in these larger arteries? and, in short, according to this mechanical view of the subject, what prevents the whole body from becoming ossified, as each separate artery, or system of arteries, acquires sufficient magnitude to admit the passage of these gross particles? On this, however, as on many other topics in physiology, it is extremely easy to overthrow the hypotheses of others, but very difficult to substitute more correct or consistent ones in their place; and, on the subject now under consideration, I confess that I am not in possession of any adequate means of explaining the difficulty. Under these circumstances, I shall proceed to give a brief description of the phenomena that attend the process of ossification; and, without attempting to reduce them to a regular theory, I shall offer some remarks upon them, and shall endeavour to show how far they can be reconciled with the other operations of the animal œconomy, and how far they must be admitted to be inexplicable. And I must here remark, that although I have thought it necessary to be explicit in my objections to Haller's hypothesis of ossification, yet I am fully disposed to allow him every degree of merit for the accuracy of his statements; for it is to his treatise on the formation of bone [6] that we are indebted for the first, as well as some of the best observations that we possess upon the subject.

When we examine the fœtus, in the earliest stages of its existence, as soon as we are able to observe the rudiments of its future limbs, and the different parts which are destined to compose the skeleton, we are able to trace the figures of some of the larger bones, but they appear to be composed of a matter which is perfectly soft or semi-fluid, contained in a delicate membrane. By degrees the parts acquire more consistence, and the membrane becomes more dense, until they gradually assume the appearance and exhibit the properties of cartilage. This cartilage, which is at first transparent and colourless, after some time exhibits opake whitish spots on different parts of its surface, which, when examined by the microscope, are found to consist of a number

[6] Exper. de Ossium Form. in Oper. Min. t. ii. p. 460, &c.; 556, &c.

of delicate lines; these increase in size and in density, and at length red points are seen to be dispersed through them, indicating that the blood-vessels of the part are sufficiently capacious to admit the passage of the red globules through them. From this period, which, according to Blumenbach, is, in the human subject, about the seventh or eighth week after conception,[7] the earthy matter is copiously deposited in its appropriate cells; the parts, which were at first soft and afterwards elastic, now become hard and rigid, so that the blood seems to be scarcely capable of forcing a passage through its vessels, compressed as they are by the dense matter which accumulates round them in all directions, and either entirely obliterates them, or at least greatly diminishes their number and capacity.

From Mr. Howship's elaborate observations on the process of ossification, which seem to have been conducted with so much accuracy, it might appear doubtful, whether the first deposition of phosphate of lime is not anterior to the formation of the cartilage, for he informs us that, in the long bones, the first appearance of osseous matter is a short hollow cylinder, which is said to exist before any cartilage can be distinguished, and which is conjectured to be secreted by the vessels of the periosteum.[8] Before, however, we can admit this inference, we must decide in what sense the term

[7] Inst. Phys. § 642.

[8] Med. Chir. Tr. v. vi. p. 263, et seq.; v. vii. p. 387, et seq.

cartilage is to be employed; and it must be proved that the soft matter, in which this osseous cylinder is formed, is not itself the future cartilage, merely in a soft state, united to a large proportion of water. At all events there seems as much propriety in assuming the previous existence of the cartilage as of the periosteum, which is conceived to act so important a part in the process, and which does not appear to be distinctly visible at this early period.

During this deposition of bony matter another very important operation is going forwards. The cartilage, which is destined to become the basis of the future bone, is homogeneous in its texture, and contains no cavities of any kind, but while the different parts of it are changed in their chemical composition its mechanical structure undergoes an equal alteration. In proportion as the secretory arteries deposit the proper bone, the absorbents carry off the cartilage; but although their action corresponds in point of time, they differ as to the seat of their operations, the greater quantity of osseous matter being deposited on the outer surface of the bone, while the absorption is carried on at the centre, so that when the external part of the bone acquires its proper degree of hardness, the interior is either formed into a complete cavity or into the loose cellular substance that has been described. In contemplating this very curious metamorphosis, the following questions, among many others that might be asked, present themselves to us. What is the origin of the phosphate of lime, which gives the bones their solidity? Is it received into the system of the mother along with her food, or have her organs of assimilation or secretion the power of generating this earthy salt? Supposing the lime to be received into the circulation, what cause determines it to pass into those particular arteries that go to the bones, and how is it disposed of at other times, before and after the period of ossification? Then if we conceive the earthy matter to be present in the blood, and to be conveyed by the vessels to its proper destination, how is it deposited by the arteries? Is it poured out from their extremities, is it deposited from their sides, or does it remain lodged in them, so as in fact to convert the arteries themselves into the osseous fibre? Each of these opinions has had its supporters, and it would perhaps be difficult to adduce any decisive argument either for or against any one of them; but upon the whole it is the most agreeable to the general actions of the animal œconomy to adopt the idea that the phosphate of lime is poured out from the extremities of the vessels.

But although it seems, on many accounts, the most agreeable to the laws of the animal œconomy, to suppose that the minute arteries pour out the earthy matter from their extremities, yet there are some circumstances respecting the mechanical mode in which the operation is effected, which it is not easy to reconcile to this supposition. We may conceive that from some unknown cause the arteries of certain parts of the cartilage acquire a disposition to deposit the earthy matter which they contain, yet how can these depositions acquire the particular form which they exhibit? They do not seem to follow the direction of the arteries which accompany them in their course, but they sometimes assume the radiated structure, branching off from a common centre, or at other times compose masses of parallel lines, shooting out in a course contrary to that of the vessels which are supposed to carry the materials for their formation. If we might be permitted to indulge a conjecture upon the subject, it would seem as if certain portions of the cartilage acquired an affinity for the earthy matter, and that when the deposit began to be formed, it received accessions of new matter in consequence of the affinity between the particles of the phosphate of lime producing a species of crystallization.

And here again a new series of questions occurs. What is the nature of the texture in which the earth is deposited? The cartilage previously appears without any cavities; are the solid particles then simply interposed between the particles of the membrane as it were incidentally? or is a space formed for their reception, in consequence of a portion of the animal matter being removed by the absorbents precisely at the same time that the earthy matter is conveyed to it? Is the original cartilage entirely removed, and new animal matter deposited in its place, possessed of a different arrangement of its parts? or is there some chemical change going forwards in it, by which the membrane has both its form and its composition changed at the same time? To these questions we are, I conceive, unable to return any satisfactory answers; yet until they are solved, it is obviously impossible to form a perfect theory of ossification.

In order to investigate the subject, and to throw any real light upon the nature of the effect that is produced, we must first of all inquire, what is the exact nature of the animal matter that occupies the place of the future bone or composes its basis. Some physiologists, as Haller, Sabatier, and Boyer,[9] have stated that it is gelatinous, while others, as Bichat,[1] have called it mucilaginous; but both these terms, we may presume, were employed in a vague sense, referring more to the physical properties and consistence of the substance, than to its chemical nature, which it is probable was never accurately examined, nor indeed was the knowledge of animal chemistry sufficiently advanced to enable the earlier writers to obtain any correct knowledge on this point. Even Bichat, when he styles it mucilaginous, does not appear to have affixed any other meaning to the word, than that of a semi-fluid substance, possessed of a certain degree of tenacity; for the proofs which he brings in support of his position are altogether inadequate. As far as we are able to form an opinion on a point in which we are entirely guided by conjecture, it is more probable that the first rudiment of the bone is

9 Haller, El. Phys. xxix. 4, 23. Sabatier, Anat. t. i. p. 13; Boyer, Anat. t. i. p. 40; see also Gibson in Manch. Mem. v. i. new series, p. 151.

1 Anat. Gen. t. ii. p. 189.

gelatinous than mucilaginous. We find that all the membranous parts of young animals contain a considerable quantity of jelly, and that as they advance in life the proportion of jelly gradually diminishes, while that of the albumen, which constitutes the proper membrane, is increased. Besides, mucilage appears, in all cases, to be the product of glandular secretion, and we have no proof of the existence of any organs of this description connected with the fœtal bones.

In its second, or what has been called its cartilaginous state, I am not aware that any direct experiments have been performed upon its chemical nature; but as it is then in a condition which admits of more minute examination, and exists in much larger quantity, we are better acquainted with its physical properties, and there is reason to suppose that the substance which occupies the situation of the future bone, is nearly of the same chemical nature with the membranous matter that afterwards enters into its composition. Still, however, the mechanical disposition of its parts differs so much in the two states, that it seems most probable, and is most analogous to the usual operations of the system, that the first cartilage should be entirely removed, and that a new deposition of animal matter should take place. We are not able to determine precisely what is the nature of the change which induces the partial opacity of the cartilage in those places which afterwards become the centres of ossification, whether it be merely a greater condensation of the part, or the abstraction of a portion of the water contained in it, or whether it be the commencement of the actual deposition of the osseous matter.

The next thing that we observe is the presence of the vessels carrying red blood, a circumstance which must no doubt depend upon an increased local action; but what is the immediate cause of this, or what connexion it has with the previous condition of the cartilage, is altogether unknown. I have already pointed out the difficulty of explaining the manner in which the deposition of the earthy matter is brought about, and indeed enough has been said to prove that although we are acquainted with the different steps of the operation, and with the order in which they succeed each other, we are scarcely able, in a single case, to decide upon their efficient cause, or to point out any connexion between them.

In considering the formation of bone, and especially the immediate source whence its component parts are derived, it may be proper to notice an hypothesis, which for some time enjoyed a considerable share of celebrity, and which although at present discarded, deserves to be mentioned as having led to some important facts on the subject. Duhamel, an ingenious French naturalist, had formed an opinion, that the successive layers or annual rings of wood, which are formed in the trunks of trees, are deposited from the inner bark, or rather that the inner bark of each year is, during the following season, converted into the alburnum, or the external layer of the proper wood. This hypothesis he endeavoured to extend to the bones, and for this purpose devised a set of experiments, which were prosecuted with much diligence. It had been discovered,[2] that when an animal has had madder mixed with its food, the bones become tinged with a reddish colour. He accordingly gave madder to an animal for a certain period, then omitted it for some time, and afterwards again resumed its use, when upon examining the bones after this plan had been pursued, he informs us that they exhibited alternate rings of a red and white colour, corresponding to the times when the animal had used the madder or omitted it. His conclusion was that the bones are formed of concentric laminæ or rings, which are deposited from the periosteum or investing membrane; and the results of his experiments, as he reported them, were generally conceived to afford decisive evidence of the truth of his hypothesis. Mr. John Bell shrewdly remarks, that when speculators perform experiments, they generally find exactly what they desired to find, and so it appears to have been with Duhamel. We are now assured that the succession of differently coloured rings which Duhamel described, could have no existence, or that if any thing resembling them took place, it could have no connexion with the periods during which the madder had been given or withheld. The hypothesis was indeed very satisfactorily controverted by Haller,[3] who, at the same time, gave the

[2] Haller, El. Phys. xxix. 4, 26; see also Duhamel in Mem. Acad. for 1739; and Gibson, in Manchester Mem. vol. i. new series, p. 146.

[3] El. Phys. xxix. 4. 33—36.

proper explanation of the phenomenon, supposing that it depended upon the affinity which exists between the colouring matter, and the phosphate of lime;[4] this opinion Rutherford has since confirmed by direct experiment,[5] and has correctly referred it to the general principle by which colouring matters are peculiarly disposed to unite to earthy salts, a principle upon which the operation of mordents in the art of dyeing depends.

On the subject of ossification, I shall only further remark, that its immediate cause appears to be unknown, but that, in its general nature, it may be considered as analogous to those operations which we ascribe to the function of secretion, where the arteries possess the power of either separating particles already existing in the blood, and appropriating them to some specific purpose, or of forming new combinations, which may be afterwards separated and employed in different ways. The only circumstance that is peculiar to this case is, that the secreting process is confined to a limited period of our existence; that it commences without any assignable cause; and when it has proceeded for a certain length of time, and supplied the wants of the system, it ceases in a way which is equally inexplicable. When we come hereafter to treat more particularly upon the growth of the body, and the

4 § 26.

5 Blake on the Teeth, p. 138, et seq. It is not a little remarkable that Hunter fell into the error of supposing that the madder attaches itself to the animal matter of the bone, and not to its earthy part. Home's Lect. on Comp. Anat. p. 64.

gradual developement of its different organs, we shall observe this adjustment of its physical condition to the circumstances in which it is placed, not only as respects the whole system, but in each of its individual parts; and we shall find, with regard to the bones in particular, that they receive their perfect form, and complete constitution, in the order which is the best adapted to the situation of the animal. As to the cause which determines these effects to be produced at certain periods of our existence, we can say little more than that we find it to be a matter of fact. It is a part of the general constitution of the animal system, that, at regular times, certain changes should take place without our being able to assign any physical cause for them. In the present case, the final cause is sufficiently obvious; at the commencement of our existence, softness and flexibility are absolutely requisite, and hardness would be injurious, while, as the necessity for resisting external violence gradually arises, the capacity for resistance is proportionably produced.

The power which the constitution possesses of repairing bones when accidentally injured, is, perhaps, more wonderful in its operation than that which originally produced them, as it exhibits, in a more remarkable manner, that mutual adjustment of the different corporeal actions, and the adaptation of it to fortuitous circumstances, which distinguishes the animal machine from all mechanical contrivances. Not only do we find that if a bone be completely divided the fractured ends are quickly cemented together, and rendered as firm as before the injury; but even, after a considerable portion of the bone has been removed, a new piece is generated to supply the deficiency.

A similar kind of controversy subsisted, for a long time, respecting the reparation of bone as concerning its original formation. The older writers supposed that the soft mucus or jelly, which is effused in the first instance, was condensed by heat or pressure into a hard gluten, which formed the uniting substance.[6] This they called callus, and conceived that it always retained its membranous state, and was never converted into proper bone. Some physiologists supposed that this callus was immediately produced from effused and coagulated blood, and others that it was derived from the periosteum of the old bone. It is now, however, generally understood that the process by which bone is repaired is very similar to that by which it is originally produced; the arteries of the divided bone[7] throw out a soft

[6] Boerhaave, Aphor. 343, &c. cum comment. Sweiten.

[7] From the experiments of Mr. Wood, it would appear, that the vessels which are principally concerned in this process are, in the first instance, those which belong to the internal membrane of the bone. Manch. Mem. v. iii. new series, p. 275, et seq. This opinion, as well as the other modern doctrines respecting the reparation of bone, allowing for some inaccuracy in the terms employed, necessarily depending upon the imperfect state of chemical science, may be found in the writings of Haller; see Op. Min. t. ii. p. 477, et alibi; see also Scarpa de Oss. Struct. p. 31.

matter, called lymph, the nature of which has not been exactly ascertained;[8] this becomes gradually converted into cartilage, or rather, perhaps, is replaced by it, after being itself previously absorbed; the earth of bone is then deposited in this cartilage, and the cartilage either removed, or new moulded, in the manner which was described above. But what is the immediate cause by which this change is effected, why the arteries throw out this substance, how it is moulded into its proper form, whence the supply of earth is derived just at the exact period when it is required for the wants of the system, are questions that have not yet been satisfactorily answered. The hypotheses that have been formed upon the subject have been, in some cases, the mere expression of the fact in different words; in others, the substitution of the final for the efficient cause; or they have proceeded upon the assumption of some imaginary agent created by the fancy of the writer to meet the present emergency. We cannot doubt that there is a proper efficient cause for this, as well as for every other change which occurs in the system; and that, were our knowledge of the animal œconomy complete, we should be able to refer it to the general laws

[8] We should be induced by analogy to conclude that the matter effused, in this case, is coagulated albumen; but I believe it has not been made the subject of distinct experiment. Mr. Dowler's experiments prove that fibrine enters into the composition of the buffy coat of the blood, and probably of the fluids which are poured out in the adhesive inflammation of the soft parts. Med. Chir. Trans. v. xii. p. 86, et seq.

by which the body is directed. At present, however, our acquaintance with the minute operations of nature is extremely limited, and we are only retarding the advancement of science by premature attempts at explaining them.

§ 4. *Connexion of Bone with the living system.*

Having now taken a view of the structure of bones, of their physical properties and chemical composition, and made some remarks upon the mode of their growth and formation, it remains to consider the nature of their connexion with the system at large, and the properties which they possess, as forming a part of a living organized body. I have already remarked, that bone, in its most perfect state, possesses few blood-vessels, compared with many other structures; it does not seem that any nerves are sent to it, and we judge of the presence of the absorbents, rather from observing effects which can be ascribed to no other cause, than from being able actually to demonstrate their existence. Bone is, consequently, devoid of sensibility, and is also equally without contractility; it partakes only in a small degree of the general action of the system, and its changes of all kinds are effected slowly, and often in an almost imperceptible manner. Yet, like all other organized parts, we have reason to suppose that every portion of it is connected with both the arterial and the absorbent systems, and that, in process of time, each particle is removed, and fresh ones deposited in their place. This gradual exchange of old for new matter is proved by the phenomena which attend the growth of bone.[9] A solid organized body cannot grow by the distention of its parts, or by the accretion of new matter to its external surface, but by the gradual re-modelling of the whole. If the secreting vessels be supposed to act more powerfully than the absorbents, the new matter is either conveyed more rapidly, or in greater quantity, than the old matter is removed, so that the bulk of the whole is ultimately increased, and yet the operation is effected so gradually, that the general form of the bone and the relation of its different parts to each other are not materially altered.

These observations refer to the bones in their healthy state. When labouring under disease, they exhibit very unequivocal marks of vitality, being subject to affections which are precisely similar to the inflammation, swelling, and suppuration of the soft parts, making allowance for the difference of their mechanical structure. And although healthy bone is insensible, yet, in some of its diseased states, it becomes exquisitely painful; and, in this case, it

[9] The experiments of Duhamel, on the effect of madder upon the bones, were generally supposed to afford the most direct proof of this interchange of particles, even by those who admitted the hypothesis of the concentric layers to be imaginary. But the experiments and reasoning of Mr. Gibson have shown that the removal of the red matter depends upon the serum, which circulates through the vessels of the bones abstracting the colour from the phosphate by its superior attraction for it. Manchester Mem. v. i. new series, p. 160.

may be presumed that the sensation arises, not from any nerves actually sent to the bone itself, but from its increased bulk and firm texture distending the nerves that are distributed upon the contiguous parts, as takes place with respect to dense membranes of all descriptions.

The same general observations, with respect to the nature of their vitality, will apply to the bones as to the cartilages and the tendons; but there is one point respecting it, which appears to present an additional source of difficulty; are we to consider the earthy matter as organized and possessed of life? Perhaps, at the first statement of this question, every one will be disposed to deny the possibility of life being attached to an earthy salt, and, in a general sense, the objection is valid. But when we come to consider the subject in its most minute relations, it will not be easy to point out any essential difference between the earthy and the animal matter which enters into the constitution of bone. They are both derived from the blood, and deposited by vessels connected with the arterial system; they both possess a specific determinate arrangement; and they are both, after a certain period, taken up by the absorbents, and again carried into the mass of circulating fluids. It is not improbable that, before they are either of them expelled from the system, or are again applied to any other use in it, they undergo decomposition, and that part of their elements may be employed in forming new compounds, while the remainder may be rejected by some of the excretory passages. I should be inclined, therefore, to say, that the phosphate of lime, while forming a part of an organized body, is alive, because the bone is so generally; but the phosphate of lime, or its elements, while they are circulating in the blood, or passing off by the kidney, or alimentary canal, cease to be so, in the same manner as the carbon which is expired from the lungs, or the mucus which is expelled from the mouth, are not considered as being alive, although they may, perhaps, a short time before, have been employed in the composition of a muscle or a nerve. This view of the subject will lead us to reject the mechanical idea which has been entertained by some physiologists, that the earthy matter of the bones is simply deposited in the interstices of the membrane, and has its particles kept together merely by the cells in which they are lodged. I conceive that the earthy particles have an affinity for each other, and perhaps for the membrane, by which they are combined in a form that belongs to them, as necessarily as to any of the soft parts, although it produces in them a peculiar arrangement, which may not be found in any other substance.

CHAP. III.

OF MUSCLE.

THE next subject which we are to consider is the muscles, and I shall arrange what I have to say respecting them under six heads. I shall first describe the form and structure of muscles; second, their chemical composition; in the third place, their properties; fourth, their uses; fifth, their mechanism; and lastly, I shall offer some remarks upon the hypotheses that have been formed to explain their action.

§ 1. *Form and Structure of Muscles.*

Muscles constitute what we call the flesh of animals, but although these terms are now by every one regarded as synonymous, the older authors made a distinction between what they styled the flesh, and the fibrous part, regarding this latter only as the proper organ of motion; and it was not until the middle of the seventeenth century that this error was rectified by Steno.[1] In their usual form, muscles are composed of masses of fibres,[2] lying parallel to

[1] De Musc. Obs. Specimen in Mangeti Bib. An. t. ii. p. 518, et seq.

[2] Croone appears to have been the first physiologist who had a distinct idea of the fibrous structure of muscles, and that muscular motion depends upon the contraction of the fibres. We

each other, intermixed with a quantity of membranous matter, a structure which is visible to the naked eye, and may be rendered more apparent by cutting the muscle transversely, and macerating it, for some time, in hot water, or in alcohol.[3] The whole muscle is enclosed in a membranous sheath, which covers it in every part, except where its ends are attached to the bones. We observe that the fibres are disposed into small bundles, called lacerti, each of which is also inclosed in a sheath of membrane, and that these bundles are divisible into still smaller bundles, apparently without any limit, except what arises from the imperfection of our instruments.

Although the fibres of many of the muscles appear to be of considerable length, yet it has been doubted whether this be actually the case, or whether what appears to be one continuous fibre may not, in reality, be made up of a number of smaller ones that are connected at their extremities; the authorities for each of these opinions are nearly balanced, but, perhaps, those for the continuity of the fibre may, upon the whole, preponderate. The fibre is represented by many writers as exhibiting a wrinkled or

learn from Eloy, Dict. Hist. "Croone," that he published a treatise, "de Ratione Motus Muscul." in 1664; see also Acta Erud. for 1682; Phil. Trans. 1681, Phil. Col. No. II. p. 22.

[3] For a most correct delineation of the disposition and direction of the fibres of the different muscles connected with the trunk of the body, I may refer to Prof. Tiedemann's very beautiful lithographic plates of the arteries; a work which is no less admirable as a specimen of art than of anatomical accuracy.

waved appearance; but there is reason to doubt whether this be its natural state, and whether it may not depend upon the condition in which it is found, when it is examined after death, and detached from the neighbouring parts. In most muscles, the centre is thicker than the rest, and appears to contain more fibres; this is called the belly; hence it gradually diminishes in size to the extremities, one or both of which terminate in a membranous body, which is either a tendon, or an expanded membrane, called an aponeurosis, according to the situation of the muscle, and its connexion with the neighbouring organs. There are considerable interstices between the muscles, which are occupied by fat and cellular texture, and in these intervals a safe lodgment is afforded for the trunks of the blood-vessels and nerves. Most of the large muscles are situated near the surface, covering the bones, and filling up the spaces between them, so as to produce the general form and outline of the body. Besides the aponeuroses, which are attached to the muscles, and the membranous sheaths which cover them externally, and inclose their lacerti, expanded membranes are often found entering into the body of the muscle, and dividing them into separate portions. All these varieties of mechanical structure are obviously adapted to the uses of the individual muscles in which they are found, and there is no part of the animal œconomy which exhibits more of this kind of adaptation than the muscular system.

With the exception of some of the viscera, muscles are more plentifully supplied with arteries than any other parts of the body; they are distributed among the fibres in numerous branches, which continue to subdivide with so much minuteness, as at length to become no longer visible. The capillary veins are equally, or even more numerous than the arteries, and form a complete vascular net-work; the contents of which are gradually discharged into larger and larger vessels, until the blood at length arrives at the main trunks. The veins that belong to the muscles are remarkable for the number of valves which they contain. The ultimate termination of the blood-vessels, or the manner in which the arteries are connected with the veins, is not very accurately ascertained; but this is a point which will be considered with more propriety hereafter.

The apparatus of nerves, which is sent to the muscles, is very considerable; and especially to those which are under the control of the will, being greater in proportion to their size than to any other part of the body, except the organs of the senses. The nerves that belong to the voluntary muscles proceed almost exclusively from the brain itself, or from the spinal cord, whereas the muscular coats of the viscera are, for the most part, supplied immediately from the ganglia. The former are so much more numerous than the latter, that, according to the remark of Haller, the nerves that go to the thumb are more in quantity than those that supply the whole substance of the liver. There are many curious circumstances connected with the distribution of the nerves, and the course which they take, as, for example, where a nerve runs for a considerable distance, as if for the express purpose of supplying a particular muscle, which might have received its nerves from a nearer source; and where two or more nerves come to the same muscle when there is no apparent reason, from the structure of the part, why any one of them alone might not have been sufficient.[4] It has been thought that each separate fibre, or, at least, each of the smallest bundles into which the fibres are arranged, contains one of the ultimate branches of an artery and a nerve; our actual observations scarcely enable us to decide upon this point, but there is some reason to suppose that it may be the case.

I have now been describing the structure of muscles as it appears to the naked eye, but many anatomists have attempted, by the aid of the microscope, to ascertain the nature of the ultimate fibre, as it has been called, or that which is no longer capable of further subdivision without a breach of its substance. As is generally the case in microscopical observations, the descriptions that have been given by these writers are very various, both as to the size and the form of the ultimate fibre; and there is also a want of uniformity in the terms which they have employed to express the gradations of the component parts of the muscle, which apparently increases the discordance of their statements.

[4] These and other apparent anomalies of a similar kind are explained by the very ingenious hypothesis of Mr. C. Bell. See Phil. Trans. for 1821, p. 398, et seq.

Leeuwenhoek, who is celebrated for the early use which he made of the microscope in anatomical researches, describes the ultimate filament as being almost inconceivably minute, some thousands of them uniting to form one visible fibre. We learn from him that the ultimate fibres are serpentine and cylindrical bodies, lying parallel to each other; that they are of the same figure in all animals, but differ considerably in their size. He states that their size bears no proportion to that of the animal to which they belong; and that even, in some instances, the smallest animals have the largest fibres; as, for example, the fibres of the frog are said to be larger than those of the ox.[5]

Muys, an industrious Dutch anatomist, was engaged, for several years, in investigating the minute structure of muscles, and his description, in many respects, agrees with Leeuwenhoek's, except that he supposes the ultimate filament to be always of the same size. He imagines that the fibres are distributed into regular gradations or series, and that the smallest fibrils of which the last series is composed, are some hundred times less than the finest hair, a proportion larger indeed than that assigned by Leeuwenhoek, yet still too minute to permit us to form any conception of it. Many other accounts of the structure of muscles have been published from time to time; some anatomists described them as being straight, others zig-zag or waved, and others wrinkled

[5] Arcana Naturæ, p. 43, et seq.

or knotted: some as being solid and others hollow, while many eminent physiologists have conceived that they are jointed, and consist of a number of parts, connected together like a row of beads.[6] Borelli, a learned and ingenious Italian, well known for his elaborate work on muscular motion, announced that the fibre consists of a series of hollow rhomboidal vesicles, and deduced from this structure a theory of muscular contraction, which he supported by a long train of mathematical problems, and while mathematical reasoning was fashionable in physiology, his demonstrations were conceived to be incontrovertible. A peculiar modification of Borelli's opinion was proposed by Stuart, who thought that the muscular fibre was composed of a string of vesicles, immediately formed from the substance of the nerves, which he conceived was similar to that of the tendons, and that these vesicles were covered by a net-work of blood-vessels.[7]

[6] Haller, El. Phys. xi. 1, 3—6; Sœmmering, Corp. Hum. fab. t. iii. § 14; Prochaska, de Carne Mus. p. 19, et seq.

[7] Dis. de Mot. et Struct. Mus. c. 8.

The idea of the vesicular structure of the muscular fibre was embraced by Hooke, in part at least from his own observations, and was at first admitted by Leeuwenhoek, although he afterwards, upon further examination, retracted it. It appears to have been previously employed by Croone as the basis of his hypothesis of muscular contraction, and was adopted for the same purpose by Keill and Stuart, yet notwithstanding the sanction of so many learned names, it seems to be totally void of foundation. See Leeuwenhoek, Arcan. p. 43, 54, and 58. Phil. Trans. Phil. Col. No. v. p. 152, and No. vii. p. 188, April 18, 1682.

Another opinion entertained respecting the nature of the muscular fibre was, that it is entirely composed of vessels, either possessing some peculiar structure, or consisting of the small branches of arteries. This hypothesis, which appears to have been first broached by Hooke, was adopted by many learned physiologists, especially those of the mechanical sect, and was made the basis of some of their speculations concerning muscular contraction. A number of facts were adduced in its support, but they may all be explained by the numerous vessels which are dispersed through the muscles, without having recourse to the supposition that the fibre itself has a vascular structure.[8] Many celebrated names, and among others those of Willis[9] and Baglivi,[1] are attached to an erroneous opinion, that besides the longitudinal fibres, muscles possess transverse fibres, crossing the others at right angles, and that these are important agents in muscular action. This diversity of opinion has in part arisen from the uncertainty which attends all microscopical observations, and in part, no doubt, from the state of mind with which the observers made their inquiries, biassed by a favourite notion, and anxious to discover some appearances which might support their hypotheses. The sagacity of Haller perceived the futility of these fanciful opinions, and his authority greatly contributed to effect their downfal. Since his time the

[8] See Carlisle, Phil. Trans. for 1805, p. 7.

[9] De Motu Muscul. p. 46.

[1] Opera, p. 399.

subject has been examined by Prochaska, an able anatomist of Vienna, by the Abbé Fontana, by Sir A. Carlisle, and Mr. Bauer. We meet likewise with a great number of valuable remarks on the muscles, and on the mode of their actions in the writings of Bichat, who, although he has not added any absolutely new facts or observations, has arranged the knowledge which we possess on the subject with much ingenuity, so as to present many parts of it under a novel and interesting aspect. If his classification should appear too minute and intricate, and some of his opinions rather subtile than well founded, still there is in them much that is extremely important both to the anatomist and the physiologist.

Prochaska, in entering upon his work, proposes a nomenclature of the component parts of the muscle, which professes to be derived from the actual structure of the parts. To the larger divisions of the muscles he applies the old term of lacerti, using it in the same sense with Haller and other preceding anatomists; the term fibre he restricts to the smallest divisions of the lacertus, which can be easily separated by mechanical means, while the still more minute parts, which are only to be detected by the use of glasses, he calls the threads or filaments. He informs us that each of the fibres, as well as the lacertus, is inclosed in a proper membranous sheath, but it does not appear that this is the case with the filaments, a number of which are invested in one common sheath, and are connected together by a fine web of cellular texture. The fibre, when properly prepared, and separated from all extraneous matter, he conceives to be of the same thickness through the whole of its extent, and continuous from one end of the muscle to the other, not as Haller and many other anatomists have supposed, consisting of a number of smaller fibres connected together by their extremities. Leeuwenhoek, Muys, and most other preceding writers had described the fibres as being cylindrical, but Prochaska says that they are obviously of an irregular polyhedral form, and that they are generally flattened, being thicker in one direction than the other. The fibres are not always of the same diameter, they differ in different animals, and likewise in different parts of the same animal, and he also observes that they are smaller in young subjects, and increase in size as the body increases in bulk generally. These circumstances, as he remarks, render it very difficult to institute any very accurate comparison between the size of the fibre in different animals, and render Leeuwenhoek's observations on this point very doubtful.

With respect to the ultimate fibres, or, as he styles them, the filaments, their shape and extent is said to be similar to that of the larger fibres, being flattened polyhedrons, reaching the whole length of the muscle. They differ, however, from the proper fibre in being always of the same magnitude, and this he estimates, nearly as Muys had done, at about one-fiftieth part the size of the red globules of the blood. As the fibres are of different diameters, the number of filaments contained in each fibre must be necessarily different, varying from 100 to 400 or 500. The filaments are solid and homogeneous; when prepared for examination they have a number of depressions or wrinkles on their surface, which gives them a waved appearance, and, when viewed in a certain direction, makes them appear somewhat serpentine or zig-zag, but these depressions he conceives are produced by the blood-vessels, nerves, and membranous bands which crossed them.[2]

The account which Fontana gives us of his microscopical observations on the ultimate muscular fibre, is, on the whole, not very different from that of Prochaska. By the use of a fine needle he divided the muscular fibre into small filaments, which seemed to be incapable of further subdivision; these he calls the primitive fleshy filaments, and some hundreds of them unite to compose what he denominates a primitive fleshy fasciculus or bundle, by which he probably means the same division that Prochaska simply calls a fibre. The primitive filaments are described as solid cylinders, marked externally with transverse lines or bands at equal distances; the filaments lie parallel to each other, and are not twisted together, as is the case with the primitive filaments of membrane; and from this circumstance he says that the two parts may, at all times, be distinguished from each other. The extreme branches of the blood-vessels and nerves, although so plentifully distributed through the muscles, do not seem to enter into the substance of the filaments, nor even of the primitive

[2] De Carne Musculari, p. 25, et seq.

fasciculi. The smallest vessel capable of containing red blood, is about three times larger than the muscular filament, and the smallest nerve about four times larger than the smallest blood-vessel, so that there is no difficulty in detecting them when they are mixed with the filaments.[3]

The observations of Sir A. Carlisle differ, in many respects, from those of preceding writers, especially of Prochaska and Fontana. He describes the ultimate fibre, by which he appears to mean the filament of the above authors, as " a solid cylinder, the covering of which is a reticular membrane, and the contained part a pulpy substance regularly granulated, and of very little cohesive power when dead." He speaks of it as what may be very easily detected by a microscope, and as not being so extremely minute as had been previously conceived, but he prudently declines stating its actual size. The extreme branches of the blood-vessels and nerves are seen ramifying on the surface of the membrane inclosing the pulp, but we are not able to trace them into the body of the fibre. There is, upon the whole, a simplicity and clearness in this description which inclines me to place confidence in it, but, at the same time, it would be desirable that the observations should be repeated and confirmed, as the authority of some of the anatomists who differ from him is too respectable to be hastily abandoned.[4]

The account which Mr. Bauer gives us of the mus-

[3] Sur les Poisons, t. ii. p. 228 ; vol. vi. fig. 6, 7, 9.

[4] Phil. Trans. for 1805, p. 6.

cular fibre differs considerably from that of either Prochaska, Fontana, or Carlisle. In examining the globules of the blood with his high magnifiers, he found that these bodies, when deprived of their colouring matter, were of the same diameter with the ultimate muscular fibre, and that the fibre was in fact composed of a series of the globules arranged in straight lines. He confirmed his observations by a subsequent experiment, in which, by a certain degree of maceration, he succeeded in reducing a muscle, first, into a number of fibres of the same diameter with the globule, and, by continuing the operation, into the globules themselves; the size of the globule, when deprived of its colouring matter, and consequently that of the muscular fibre, he estimates at $\frac{1}{2000}$ of an inch in diameter.[5]

Among the more noted hypotheses that have been formed respecting the nature of muscles, independent of their visible appearance, I must not omit to mention one which prevailed very generally about 50 years ago, and was zealously defended by Cullen, that muscles are, to use his own expression, the moving extremities of nerves.[6] The fibres of the muscle are supposed to be continuous with those of the nerve, and to be absolutely the same substance, but that they experience a change in their structure, so that when the nerve is converted into muscle it loses the power of communicating feeling, and acquires that of producing motion. This doctrine of Cullen's seems

5 Phil. Trans. for 1818, p. 174, 175; pl. 8. fig. 4, 5, 6.

6 Instit. § 29, 94.

to have been the result of the physiological speculations that he had formed respecting the nature of life. Following up the idea of Hoffmann, that the animal functions exhibit phenomena of a specific kind, which cannot be referred to any other powers in nature, he classed them together under the denomination of vital; and as both sensation and spontaneous motion were obviously to be placed among the vital operations, he hastily concluded that they must proceed from the action of the same organs. In answer to this hypothesis I think it sufficient to observe, that substances which differ in their appearance and structure, as well as in their physical and chemical properties, can have no claim to be regarded as identical. And with the same remark I may dismiss an analogous speculation, that muscle and tendon are the same substance, differing only in the more condensed state of the latter; an opinion which was transmitted from the ancients, embraced by Boerhaave and his disciples, was adopted by Albinus, who studied the muscles with such minute attention, and was in short so generally admitted, even in the middle of the last century, that Haller[7] and Sabatier[8] scarcely ventured to give a decided opposition to it.

Besides the bodies which I have described above, to which the name of muscles has been generally applied, muscular fibres appear under a different form, and one which is less obvious to the eye, but which is no less necessary to the existence of the animal. I

[7] El. Phys. ii. 1, 18. [8] Anatomie, t. i. p. 242.

refer to those structures, where fibres, which appear essentially to resemble those of the proper muscles, are attached to membranous expansions, composing what have been called muscular coats. These muscular coats are connected with the hollow cavities that exist in different parts of the body, in the form either of pouches or cylinders, and are destined for the transmission or lodgment of various bodies of a soft or fluid consistence, and which propel their contents by means of these fibres. The mechanical structure of the muscular coats is considerably different from that of the proper muscles; the fibres are much shorter, and instead of lying parallel, as is always the case with the muscles, they seem to be interlaced or twisted together, and, according to Prochaska, sometimes even to anastomose or bifurcate.[9] The fibres of the muscular coats do not exhibit that division into lacerti or bundles, nor have they the regular belly or tapering extremities of the others. Their immediate attachments are also different; the proper muscles have one of their ends at least terminating in a tendon or fibrous membrane, while the muscular coats are attached to membranes that exhibit less of the fibrous and more of the cellular texture.

The uses of these two classes of bodies are likewise very different. The proper muscles are always designed to produce the motion of some part of the body, by altering its relative position with respect to the other parts, while the motions that are caused by

[9] Tab. 6, fig. 2 and 3.

the fibres of the muscular coats are designed to operate solely upon the contents of the organ to which they belong, and consist in a number of small contractions, in each of which a few fibres only act at the same time. And these two kinds of organs differ moreover in the connexion which they have to the other parts of the living system, and particularly to the nerves; for while most of the proper muscles are supplied with nerves, either from the brain itself or from the spine, which may be regarded as an immediate appendage to the brain, or are, many of them, more or less dependant upon the will, the muscular coats are, in most cases, supplied from the ganglia, and their action is entirely involuntary.

From this difference in their structure and properties most anatomists have restricted the term muscle to the regular masses of parallel fibres; but Bichat applies it generally to both of them. The proper muscles, as being the media through which we observe the operation of both sensation and motion, those qualities which are essential and appropriate to animal existence, he styles muscles of animal life, while the other class he calls the muscles of organic life, in consequence of their being destined principally for these organs, which serve for the support of the individual, but which do not present so obviously the phenomena of sensation and motion.[1] The correctness of Bichat's names depends upon that of his peculiar theory of vitality, which will be examined

[1] Anat. Gen. t. ii. p. 400.

hereafter; I shall therefore adhere to the former nomenclature, and when I speak of muscle in the abstract I must be understood to refer solely to the larger masses of parallel fibres, while to the others I shall apply the usual term of muscular coats.

In man and the more perfect animals muscles generally possess a reddish brown colour, but this seems not to be essential to them, as by sufficient ablution in water, or by maceration in alcohol, they may be deprived of it, and be rendered nearly white, without having their texture apparently altered. As their colour is most considerable in animals with red blood, it has been usually attributed to a quantity of red blood remaining attached to the fibres, either extravasated through them, or simply contained in the vessels; but Bichat endeavours to show that this is not the case, and that the colour depends upon some foreign substance that is combined with the fibre. He founds his opinion upon the circumstance, that in the same animal some of the muscles are always much redder than others, and yet that they do not appear to have a greater quantity of blood sent to them, and also that in different classes of animals the colour of the muscles does not appear to correspond with the quantity of red blood circulating through their vessels.[2] But whatever be the nature of the colouring matter, we may conclude that it is not necessary to the constitution of the fibre or to its specific properties; for some of the muscular parts that are the most

[2] Anat. Gen. t. ii. p. 327.

contractile are of the lightest colour. Generally, however, the red colour prevails in the voluntary muscles, and it has also been observed that in those which are naturally coloured, the shade becomes deeper in proportion to the degree in which the muscle is exercised.[3]

Besides the membranous substance, which seems to enter into the necessary structure of a muscle, which envelopes its fibres and lacerti and forms its sheath, determining its figure and preserving it in its proper position, there is likewise a quantity of cellular substance of a more loose texture interspersed through the body of the muscle, and filling up the cavities between its separate parts. This is similar to the cellular substance which enters so largely into the composition of the body generally, and like it appears to be intended to contain both fat and the peculiar albuminous fluid. It has been conceived that, besides the proper fat which is lodged in its appropriate cells, muscles contain a quantity of oil of a more fluid consistence, which is intimately united with them, and serves to lubricate them, to assist their movements, and to prevent their adhesion; but this opinion is rather founded upon conjecture and the supposed utility of the substance, than upon any experiments which have directly proved its existence.

A quantity of albumen, of jelly, and of the peculiar substance called extractive matter, may be procured from the muscles by boiling, but it does not

[3] Home's Lect. on Comp. Anat. p. 31, et seq.

appear that these form any essential part of their substance, and it is doubtful whether the jelly be always present in the muscles of the adult. In young animals it appears that the muscles, as well as the membranes and bones, contain a considerable quantity of jelly, but as they advance in age this jelly disappears, and is replaced by albumen. When muscles are digested in warm water, a quantity of saline matter is separated from them, but it is doubtful whether this be attached to the muscular fibres themselves, or be merely lodged in the different vessels that pass through them; the former however appears the more probable supposition, because, as far as it has been examined, it would seem not to be the same combination of salts which exists in the blood.

When the muscular fibre has been macerated for a sufficient length of time, and is cleared, as much as possible, from all extraneous matter, we obtain it in a pure state. It is nearly white, without much taste or smell, and if it be kept free from moisture, it will remain a long time without undergoing decomposition or experiencing any change.[4] If the water which has been employed in the maceration, and which contains albumen, jelly, extract, and various salts, be evaporated to dryness, and then treated with alcohol, the extract alone is dissolved, and by the evaporation of the alcohol, may be obtained in a pure state. This substance was discovered by M. Thouvenel; it has a brown colour, an acrid taste, and an aromatic odour,

[4] Fourcroy's System, by Nicholson, v. ix. p. 335.

is soluble both in water and in alcohol, and would seem to be the ingredient which gives the specific flavour to the flesh of different animals, and especially to be the part which forms the brown crust on roast meat. M. Thenard has given the name of Osmazome to the substance.[5]

The salts that are contained in muscular flesh or connected with it, are principally the phosphates of soda, ammonia, and lime, and the carbonate of lime. For the discovery of the phosphate and carbonate of lime we are indebted to Mr. Hatchett.[6] Fourcroy and Vauquelin inform us that they have detected sulphur and potash in muscles,[7] and Prof. Berzelius the muriate, phosphate, and lactate of soda;[8] but perhaps there is still some uncertainty respecting the nature of the salts, and the mode in which they exist when entering into the constitution of the muscle.

§ 2. *The Chemical Composition of Muscle.*

The muscular fibre, in its pure state, is readily acted upon by various chemical re-agents. Most of the stronger acids and the caustic alkalies dissolve it; but it will not be necessary to enter into a detail of the phenomena that occur with any of these, except with the sulphuric and the nitric acids. By digesting the

[5] Chimie, t. iii. p. 759; Berzelius, however, does not think this substance to be a distinct proximate principle. See his View of Animal Chem. p. 82.

[6] Phil. Trans. for 1800, p. 395. [7] Ann. Chim. t. lvi. p. 43.

[8] Thomson's Chem. v. iv. p. 474.

fibre with sulphuric acid, and then removing the acid by carbonate of lime, M. Braconnot converted it into a peculiar kind of extractive matter, from which, by means of alcohol, he procured a substance named leucine, in consequence of its white colour; this, by the action of nitric acid, is converted into what is called nitro-leucic acid.[9] By the nitric acid fibrine is partly decomposed and partly dissolved, while a quantity of gas is disengaged, consisting principally of azote, united to about one-tenth of its bulk of carbonic acid. The same kind of gases are produced by other animal substances, when they are treated with nitric acid, but the muscular fibre differs from most of them in extricating a larger proportion of azote, indicating that a greater quantity of this substance enters into its composition. As this is the element which prevails in animal bodies, and particularly distinguishes their chemical composition from that of vegetables, muscles are said to be the most completely animalized part of the body; and it is worthy of observation, that in the same degree as the animal constitution differs from the vegetable in its chemical constitution, it acquires, at the same time, its most characteristic physiological properties. It is generally understood that the muscles of the animals with red blood, which possess the greatest variety of functions, and enjoy them in the most perfect state, contain more azote than those of fish or reptiles; and that in animals

9 Ann. Chim. et Phys. t. xiii. p. 118, et seq.

of the same species, those of adult age contain more azote than the same animal soon after birth. This is one among several instances where it would appear that the full developement of the animal functions is necessarily connected with a certain chemical constitution; but although this appears to be a matter of fact, we are not authorized to conclude, with some modern physiologists, that the vital powers are the necessary result of a peculiar chemical mixture.

When the action of nitric acid upon the muscular fibre is promoted by heat, the muscle is quickly dissolved in large quantity, the fluid assumes a deep yellow colour and acquires a degree of unctuosity, while, at the same time, there is a rapid escape of gas, consisting principally of a mixture of azote and carbonic acid, together with a quantity of nitrous gas. After the acid has dissolved a considerable portion of the fibre, globules of oil appear on the surface; these, when the fluid cools, assume a concrete form, and are found to be a substance of a peculiar nature, which, from possessing properties intermediate between those of fat and wax, has obtained the name of adipocire. Portions of the oxalic and malic acids are also formed, and besides these several other substances are contained in the fluid, and some, as it appears, of a peculiar nature, generated by the action of the nitric acid upon the muscular fibre.

The substances which result from the mutual action of nitric acid and fibrine, have been minutely examined by Fourcroy and Vauquelin, and appear to be both numerous and complicated in their relations to each other. Besides the gases that are disengaged, and the oxalic and malic acids that are left in the fluid, it would appear, that at least three other new substances are produced ; a peculiar fatty or adipocirous substance, an acid, which from its colour has been called the yellow acid, and a substance nearly approaching to a resin in its properties, which possesses an intensely bitter taste.[1] Professor Berzelius, however, throws some doubt upon the nature of these two latter substances, as distinct chemical products, and seems more disposed to consider them as being merely combinations of the nitric acid with different portions of the muscular fibre.[2] The subject indeed still remains in some degree of obscurity ; yet it appears probable that a portion both of the acid and of the fibre is decomposed, and that the substances generated by the decomposition of the fibre contain a larger proportion of hydrogen and perhaps also of oxygen, with less azote and carbon, than the muscle in its original state.

There are several reasons which lead to the opinion, that the concrete fat or adipocire, which is formed by the action of nitric acid upon muscular fibre, differs in its constitution from the fibre itself, in consequence of the addition of oxygen and the abstraction of azote and carbon. With respect to the yellow substance, which appears to be the result of some further change in the composition of the

[1] Ann. Chim. v. lvi. p. 37.

[2] Medico-Chirurg. Trans. v. iii. p. 205.

muscular fibre, we are not sufficiently acquainted with its nature, nor indeed have we yet that exact knowledge of its properties in its uncombined state, for us to decide upon its composition, or the relation which it bears to the adipocire or the entire fibre. Analogy, however, would lead us to conjecture that this is a still further remove from the muscular fibre, and that the abstraction of still more carbon, and the addition of more oxygen, than what is sufficient to convert muscular fibre into adipocire, would convert adipocire into the yellow resin. But this is a point which must be determined by future experiment.

There is a very peculiar change which the muscle undergoes in its chemical composition, under certain circumstances, which was, in the first instance, effected spontaneously, but which has been since imitated by various artificial processes. There was an immense burial-ground in Paris, called La Cimetiere des Innocens, which, in consequence of some improvements that were going forwards, it was determined to remove. This place had been the receptacle for a considerable part of the population of Paris for several centuries. The number of burials was supposed to be some thousands annually; the bodies were deposited in pits or trenches about 30 feet deep, each capable of holding from 12 to 1500 bodies, which were then covered with a few feet of earth, so that the whole area, occupying about 7000 square yards, was converted into a mass, consisting principally of animal matter, rising several feet above the natural level of the soil. Upon opening the ground for the purpose of removing this prodigious collection of dead bodies, they were found to be entirely altered in their nature and appearance. What had formerly composed the soft parts of the body was converted into an unctuous substance, of a grey colour, and of a peculiar but not very offensive odour. According to their position in the pits, and the length of time they had been deposited, the bodies had undergone this transformation in a more or less perfect manner. The transformation was found to be most complete in those bodies that were nearest the centre of the pits; and when they had been buried about three years, and in these cases, every part, except the bones, the hair, and the nails, seemed to have lost all their specific properties, and to have acquired those of this peculiar substance.[3]

We are indebted to Thouret[4] for an interesting detail of the circumstances that attended this opening of the burial-ground, and to Fourcroy for a chemical analysis of the peculiar substance into which the bodies were converted. By subjecting it to the action of the appropriate chemical re-agents, he found it to be a saponaceous compound, consisting of ammonia united to adipocire, the same substance which is produced by the action of nitric acid upon muscle. When the adipocire was freed from the ammonia, and obtained in a state of purity, it was found very nearly

[3] See also Home in Phil. Trans. for 1813, p. 149.

[4] Journ. de Phys. t. xxxviii. p. 249.

to resemble spermaceti, both in its physical and chemical characters.[5]

It was afterwards found that this conversion of muscular flesh into adipocire might be produced by other means besides that of inhumation; mere immersion in cold water, especially in a slow running stream, completes the operation much more speedily than the spontaneous decomposition which was carried on in the burial-ground at Paris; and the action of diluted nitric acid is still more rapid; but in this case a portion of the acid adheres to the adipocire, of which it is very difficult entirely to deprive it.[6] On account of its resemblance to spermaceti it was proposed to establish a manufacture of adipocire, from the carcases of such animals as were not proper for food; and attempts of this kind have been actually made, both in this country and in France, but in both cases without success, in consequence, as it appears, of the difficulty of entirely removing from the adipocire its unpleasant smell and dingy colour. From observing what takes place, when the conversion of muscle into adipocire is effected by means of diluted nitric acid, we may conclude that this substance differs from the muscular fibre, in containing less carbon and azote and more hydrogen and oxygen.

§ 3. *Properties of Muscle.*

I now come, in the third place, to give an account of the properties of the muscular fibre, and these I

[5] Ann. Chim. t. iii. p. 120; t. v. p. 154; t. viii. p. 17.

[6] See Gibbes, in Phil. Trans. for 1794, p. 169; and for 1795, p. 239.

shall arrange under the heads of physical and vital; the first comprehending those which are connected with its mechanical form, its structure, and its obvious external characters; the second comprising its powers as forming part of a living organized body. With respect to the first class of properties, those which I have denominated physical, there is some difficulty in precisely ascertaining their nature, in consequence of the close attachment which there is between the muscular fibre and the membranous matter, so as to render it impossible to separate one from the other without altering, or rather entirely destroying, its texture. It seems, however, reasonable to conclude that the muscular fibre possesses all the properties that belong to membrane, although not in the same degree. The muscular fibre is cohesive, but much less so than membrane, while its flexibility is perhaps greater; it is highly extensible, and perhaps elastic. Its extensibility is probably equal to that of membrane; indeed, in most instances, they are brought into action at the same time, and must necessarily exist in the same degree, as the two substances are so intimately connected together; but I am not acquainted with any experiments that have been expressly made upon this point. There is a considerable difference between the extensibility of the proper muscles and that of the muscular coats; the former are much limited in this respect by their local position, and their connexion with the neighbouring parts, while the muscular coats, from their structure, possess almost an indefinite power in this respect, and frequently exhibit very remarkable instances of it. The change in the size of the uterus, the stomach, and the bladder, from their most contracted to their most extended states, is well known to every one; and although, in these cases, it is not certain in what degree the membrane and the muscular fibre individually partake in these changes, yet it is reasonable to conclude that it belongs to them both nearly in an equal degree.

There is considerably more doubt respecting the elasticity of the muscular fibre; for although the proper muscles, and still more the muscular coats, exhibit decisive marks of elasticity, yet here it is impossible to say how much of this belongs exclusively to the fibre, and how much it has in common with the membrane. The soft and yielding nature of the fibre, when detached from the membrane, would not indicate any great degree of elasticity;[7] and as Sœmmering remarks,[8] the almost total absence of this property in the fibre after death would lead us to the same conclusion, yet there are some phenomena which render it probable that the fibre is possessed of this quality in a certain degree.[9] To the elastic nature of the muscles generally, as consisting of a compound of fibre and membrane, I should refer the

[7] See Carlisle, in Phil. Trans. for 1805, p. 3.

[8] Hum. Corp. Fabrica, t. iii. § 5.

[9] The crassamentum of the blood, when obtained in a fibrous state, is in some degree elastic; but although it agrees with the muscular fibre in its chemical properties, it probably differs in its mechanical organization.

natural contraction of Whytt;[1] those actions which Cullen, and many physiologists since his time, have called tone, or tonicity, and which Bichat has classed under the head of contractility from texture, in which, after a part has been distended by any cause, when the distending force is withdrawn, it gradually recovers its natural form and dimensions. But this point will be considered more fully hereafter.

But all these physical properties of muscle are little worthy of our attention, compared to its vital properties, or those which it possesses as forming a part of a living organized body, giving rise to a class of phenomena, which we consider as essentially connected with life, and the cessation of which indicate that its complete extinction has taken place. Of these properties, by far the most important and interesting, if not the only one, is that which has been styled by many modern physiologists irritability, but to which I have preferred giving the name of contractility. It may be defined, that power which the muscular fibre possesses of diminishing its length, or of contracting and shortening itself. From a very extensive range of facts and analogies we conclude that this power is never exercised without the agency of some direct independent cause, to which the name of stimulant has been applied; and we arrive at this conclusion, not merely from the general principle, that every effect in nature must have its appropriate cause, or that every event is preceded by some other

[1] Essay on Vital and Invol. Motions, sect. 1, § 3.

event, which stands to it in the relation of a cause, but from actually observing, that when we see muscular contraction take place, and have an opportunity of examining all the previous circumstances, we can assign the exact event which has produced the contraction. It must, however, be admitted, on the other hand, that the stimulants, or the causes of contraction, are as different as possible from each other in their nature; and in fact, as far as we can judge, have not any single property in common, except that now under consideration, of producing the contraction of the muscular fibre.

Since the time of Haller, who had the merit of first clearly comprehending the nature and extent of this property, and strictly pointing out its effects, the term irritability has been generally applied to it.[2] This word was, I believe, first used in physiology by Glisson, an ingenious English anatomist, who lived about the end of the seventeenth century, and who entertained some opinions on the subject of muscular action, that were to a certain extent correct, and were a considerable advance upon the knowledge of his predecessors.[3] Although I feel a great objection to alter the language of science unnecessarily, yet, in the present instance, a necessity for the proposed change exists. In the first place, irritability is a term employed both in physiology, in pathology, and in

[2] Mem. sur la Nature Sens. et Irrit. des Parties du Corps Animal, mem. 1, sect. 2.

[3] See particularly his treatise, "De Ventriculo," c. v. in Mangeti Bibl. Anat. t. i. p. 80, et seq.

ethics, and in each of these sciences in a different sense. In physiology it simply designates a certain faculty in the muscular fibre; in pathology it signifies a peculiar condition of the vital powers generally, and of their relation to external agents; and in ethics it expresses a certain state of the temper and feelings. Then with respect to physiology alone irritability is objectionable, because it was employed by Haller and his disciples to express a peculiar property, which is necessarily connected with an hypothesis, not the mere contractility of the muscular fibre, but the way in which the contraction is produced, or rather implying the nature of the cause as well as the effect; and although I am disposed to think that the hypothesis of Haller is correct, yet it is still but an hypothesis; and the influence of language over opinion is so great, that it is always desirable to avoid those phrases which may, even unconsciously, affect our judgment on such topics. The term contractility has the advantage of simply expressing the fact, its use has been sanctioned by several eminent physiologists, and although it may be perverted or improperly applied, it seems in itself to be unobjectionable.[4]

In considering the subject of contractility, it will

[4] Blumenbach uses it in a different sense, to express the re-action of the cellular texture, making it synonymous with his *vis cellulosa*. Some of the French physiologists employ it in a more general way to express every motion of any part of the body. Fournier, Art. "Contractilité," Dict. des Scien. Med., defines it to be the motion of which all parts of the body are capable except the nerves.

be proper to begin by giving an account of muscular contraction, describing its phenomena and direct effects, then those of the relaxation of the muscles, and I shall afterwards make some remarks upon stimulants, or the agents which act upon the fibre. Relaxation is the natural state of the muscle, or that condition which it affects, when not acted upon by any external cause. Upon the application of a stimulant its contraction commences; its surface, which was before smooth, now becomes furrowed and wrinkled, its belly swells out and grows hard and firm to the touch, while the ends approximate, and the whole muscle is rendered thicker and shorter.[5] This is an account of the general effect of contraction, but many questions present themselves when we minutely consider the operation. It has been a subject both of theory and experiment, whether the specific gravity of a muscle be increased during its contraction, or whether the fibres gain in thickness precisely what they lose in length;[6] an inquiry which, as will be seen hereafter, is not a subject of mere curiosity, but is connected with the theory of contraction, or the intimate nature of the operation by which it is produced.

Some experiments were performed by the older anatomists, and especially by Glisson, which seemed to prove that the muscle was altogether diminished in bulk during contraction; and experiments of a similar kind have been more lately made by Sir G.

[5] Haller, El. Phys. xi. 2, 17—20. [6] Ibid. § 22.

Blane and Sir A. Carlisle. Sir G. Blane, however, was led from his results to conclude that the absolute bulk of a muscle, and of course its specific gravity, is not changed during contraction. He enclosed a living eel in a glass vessel filled with water, the neck of which was drawn out into a fine tube; then by a wire introduced into the vessel he irritated the tail of the animal so as to produce strong contraction, during which he observed that the water in the tube remained stationary. He also compared the two sides of a fish, one of which had been crimped, and thus brought into a state of strong contraction, the other left in its natural condition, and he states that he found their specific gravity to be the same. On the contrary, when he tried the effect of extension on a vegetable substance, as caoutchouc, its specific gravity was diminished by being stretched out, but it recovered its former density when it contracted.[7]

Sir A. Carlisle tried an experiment upon a man's arm, analogous to that of Sir G. Blane on the tail of the eel, but with a different result. The arm was immersed in a jar of water, with which a barometrical tube was connected, and when the muscles were made to contract strongly, the level of the water was raised in the tube, showing that the bulk of the muscles was increased by contraction, while at the same time he informs us that the specific gravity was increased; and he states that the same change occurs in crimped fish.[8] Probably, however, experiments of this kind

[7] Select Dissert. p. 239—241. [8] Phil. Trans. for 1805, p. 22, 23.

do not admit of a very decisive result, because we may suppose that while one set of muscles is contracted, another set is relaxed, and that besides the simple effect of contraction, there may be a displacement of parts, or some alteration in their arrangement, which may affect the bulk of the limb, without there being any absolute change in the density of the muscular fibre itself.[9]

It has likewise been a subject of controversy whether the quantity of blood in muscles be diminished during their contraction. Most of the earlier writers supposed it to be the case, and this opinion was adopted by the Boerhaavians,[1] but it may be inferred that they were influenced rather by hypothesis than by actual observation; for as it was the general opinion that the muscles were rendered smaller by contraction, it was concluded that a portion of their blood must be squeezed out. We are indeed, as a proof of this, told by Winslow,[2] and some of the most respectable anatomists of his age, that the muscles become paler during contraction, and again resume their colour when relaxed; and this was particularly stated to be the case with the heart of the frog, and

[9] The experiments of Sir Everard Home, on the increased size of muscles during contraction, only prove that their bulk is increased in one direction. Lect. on Comparative Anatomy, p. 33. Mr. Herbert Mayo, by employing the heart of a dog, has obviated the objections which exist against the experiments of Sir G. Blane and Sir A. Carlisle; he finds the bulk to remain precisely the same during the states of contraction and relaxation. Anat. Comment. p. 12.

[1] Boerhaave, Instit. § 401. [2] Anatomy, sect. 3, art. 1, § 48.

that of the chick during incubation; but, as Haller remarks, this effect does not depend upon the blood being expelled from the substance of the muscles of the heart, but from the cavity of the ventricles, the heart in these animals being so transparent as to permit the colour of its contents to be perceived externally.[3] The opinion that the blood is expelled has been more lately adopted by Bichat, but his reasons for it do not appear of much weight. He principally insists upon the well-known fact, that during the operation of drawing blood from the arm, the flow is increased by contracting the muscles;[4] but the additional quantity of blood that is expelled in this case is not derived from the capillary vessels dispersed through the fibres, but from the larger venous trunks, which are pressed upon by the swelling out of the bellies of the muscles. Sir A. Carlisle also adopts the same opinion, but rather upon general grounds, than as derived from any decisive experiments; he only states, in a loose way, that the muscles become pale during contraction,[5] without alleging any proof of the fact; and it may be remarked, that if, as he supposes, the absolute size of the muscle be increased by contraction, it does not seem likely that the quantity of blood in it will be diminished.

The nature of muscular contractility, or the relation which it bears to the other powers of matter, has been a subject of long and learned discussion, yet

[3] El. Phys. xi. 2, 21.
[4] Anat. Gen. t. ii. p. 378.
[5] Phil. Trans. for 1805, p. 27.

until very lately it was entirely misunderstood. Now, indeed, that we are become familiar with the conception of it, as a property of a specific nature, inherent in the muscular fibre and peculiar to it, it seems scarcely possible to conceive of any quality that possesses more distinct characters. Yet, although Glisson, Baglivi, and others, made some approaches to a correct view of the subject, it remained involved in much obscurity until the time of Haller. This great physiologist, however, clearly pointed out its nature, marked its specific differences, and announced it as being exclusively attached to the muscular fibre; and what, perhaps, should be regarded as the most important of all his numerous discoveries, and the one which had the greatest effect in promoting our knowledge of the animal œconomy, he showed in what respects the phenomena of muscular contractility differ from those of nervous sensibility, and referred them respectively to their appropriate causes.[6] Even the most learned and judicious of his immediate predecessors or contemporaries, as Hoffmann, Boerhaave, and Cullen, were not sufficiently aware of this distinction, and perpetually confounded their effects.[7] But what is a still greater, and, as it now appears, a less pardonable error, they had not learned to distinguish between contractility and elasticity: they referred many of the

[6] El. Phys. xi. 2, 4, et seq.; Mem. sur les Part. Sens. et Irrit. Op. Min. t. i. p. 329.

[7] Even the acute and accurate Bichat has not clearly perceived the distinction, and to this cause may be ascribed much of the obscurity and intricacy which we occasionally find in his works.

effects of the former to the operations of the latter; and also endeavoured to account for them entirely upon mechanical principles, such as are alone applicable to elastic bodies. Yet the difference between these two powers is most obvious and essential. Elasticity always depends upon simple re-action, and is never the source of actual power; it merely restores, in a contrary direction, the force which had been impressed; and even, when acting to the greatest advantage, the effect which it produces can never be greater than the amount of the cause, and the re-action can never take place as long as the cause continues to be applied. Thus the force with which a steel spring recoils, even supposing it to be a perfect elastic, is only equal to that which is required to bend it, and as long as the force remains applied, it is impossible for the recoil to take place.[8]

But in muscular contraction we observe a very different train of events. The mechanical effect is infinitely greater than the mechanical cause producing it, and indeed bears no physical proportion to it, while at the very time that the cause is applied, and is acting with all its force, the re-action commences and far surpasses the force of the agent.[9] But what is still more

[8] J. Hunter appears to have been one of the first physiologists who very clearly distinguished elasticity from contractility. Treatise on the Blood, p. 105, 106.

[9] Fordyce, in Phil. Trans. for 1788, p. 25, observes, that if the inside of the heart be slightly scratched by a needle, it will contract so strongly as to force the point of the needle into its substance.

decisive against the doctrine, that contractility is only a modification of elasticity, is, that the most considerable effects of muscular action are frequently produced without any mechanical cause at all, where the agent is of a kind which has no relation to any of the mere physical properties of matter. No fact in the whole range of natural phenomena occurs more frequently to our observation, yet such is the devoted attachment to theory which takes possession of the mind, and so difficult is it to shake off established errors, that all the mathematical physiologists attempted to explain muscular contraction by the laws of mechanical impulse. We can scarcely review, without a feeling of humiliation, the absurd hypotheses which were invented by the most learned men of the age, and were brought forwards, with all the aid of geometrical demonstration, and enforced by a string of problems, theorems, corollaries, and lemmas. To all this learned trifling it is sufficient to reply, that a mechanical force, of an indefinite extent, is frequently produced without the intervention of any mechanical cause whatever, and must therefore be referred to a principle of a totally different nature.

One of the most remarkable circumstances respecting contractility is, that in all muscular action, however powerful the stimulant be that is applied, still after some time the effect ceases, and the muscle becomes relaxed.[1] And this succession of alternation after contraction occurs, even although the stimulus

[1] Cullen's Instit. § 108.

continues to be applied. This we perpetually observe in all our experiments upon muscles, with either mechanical or chemical substances, it likewise takes place in all the natural operations of the system, and is to be observed, in a very remarkable degree, in the muscles that are under the control of the will. In performing any voluntary action, where the mental energy continues to be exercised with equal, or even with greater power, although our very existence immediately depended upon it, we find ourselves unable to persevere in the action beyond a certain length of time. The muscles that have been contracted become, what is termed, exhausted,[2] and a certain period is necessary to elapse before they are again capable of being stimulated or excited into action. In a majority of instances we may observe a degree of correspondence between the subsequent exhaustion and the previous stimulation, but many causes interfere to prevent these two circumstances from bearing an exact ratio to each other.[3]

[2] The term *exhausted* is here employed as the one in common use, without any reference to the hypothesis from which it originated, because a word did not occur which might convey a more simple expression of the fact. On this subject the reader may peruse with advantage Dr. Park's remarks in the Quart. Journ. v. ii. p. 228.

[3] It may, I think, be questioned, whether exhaustion ever takes place in cases of simple contractility, or whether it be not confined to those in which the sensibility of the nerves has been called into action. We are indebted to Dr. Wollaston for an interesting observation, which renders it probable that the state of exhaustion, or rather the alternation of contraction and relaxation, is much

The phenomena which attend upon the relaxation of a muscle are precisely the reverse of those of its contraction: the belly becomes soft, its swelling subsides, and the wrinkles disappear from its surface; the force of contraction no longer existing, the ends, not being drawn together, recede, and the whole resumes its natural state. Relaxation is generally conceived to be merely a passive effect, and to consist simply in the absence of contraction, but when parts have been displaced by contraction there is a necessity for some absolute power to bring them baek to their former situation. This power is, in most cases, that of the antagonist muscles. The muscular system is so arranged that, in most parts of the body, one muscle or set of muscles has another muscle or set of muscles which act in precisely a contrary direction, and is intended to produce precisely the opposite effect; one muscle draws a part to the right hand, another to the left; one muscle raises it, another depresses it, and when either muscle has been in action, it generally happens that the opposing muscle then acts and produces the contrary effect. Besides the antagonist muscles, another counteracting force, which is often useful in replacing parts, is elasticity. Muscles

more rapidly produced than is commonly supposed. He founds his opinion upon a peculiar vibratory sound which is perceived when the finger is inserted into the ear with a moderate degree of force. This acute philosopher conceives that, in this case, the voluntary effort, although apparently continuous, "consists in reality of a great number of contractions repeated at extremely short intervals." Phil. Trans. for 1810, p. 2.

are frequently so situated that when they contract they move some elastic membranous body, or not unfrequently a quantity of elastic membranous matter enters into their own composition, which, when the fibres relax, re-acts and restores the parts to its natural position. Examples of this kind occur in the muscles about the thorax and the larynx, where the muscles are connected with cartilages, which are compressed or distended according to circumstances, and which immediately re-act when the contraction ceases. A third means by which muscles are replaced after contraction is the force of gravity: it not unfrequently happens that the action of a muscle has the effect of raising up some part and sustaining it without support, and of course, when the muscular contraction ceases, the part falls down by its own weight. This frequently occurs in the motions of the extremities. The hollow muscles and the muscular coats are excited to contract by some substance which distends their cavities; the act of contraction, by discharging the distending substance, removes the exciting cause, and relaxation naturally ensues.

But although relaxation has generally been considered as simply a passive state, in which the muscle merely ceases to act, and where it is brought into its ordinary condition by other agents, the contrary doctrine has been occasionally maintained; and as it has found a supporter in Bichat, it may be necessary to notice it. He conceives that relaxation is in part at least an active effect, and that it consists in something more than the mere cessation of contraction.

He founds his opinion, as it appears to me, upon very insufficient grounds of reasoning; the only fact which he adduces is, that if the heart be grasped during its diastole, it may be felt to press with considerable force upon the hand.[4] But this is too vague an experiment on which to build an opinion of so much consequence. The heart is a remarkably complicated organ with regard to its mechanism, and as we shall afterwards find, when we come to treat upon its motions, there are many circumstances respecting it which require to be taken into consideration.

I have already remarked upon the multifarious nature of stimulants, as they have been called, those agents which possess the specific power of exciting the muscular fibre to contraction. It has been asserted, and is indeed literally true, that every body in existence is a stimulant to the muscular fibre, because, independently of any other quality, the mere contact of a material substance produces this effect. Stimulants have been arranged in various ways,[5] but perhaps the most convenient and comprehensive is into the three heads of mechanical, chemical, and what we may term vital. Mechanical impulse of all kinds, beginning with the slightest touch that is capable of being perceived, and proceeding to a degree of vio-

[4] Anat. Gen. t. ii. p. 468.

[5] See Smith, de Mot. Mus. p. 32. Blumenbach, § 52, divides them into chemical, mechanical, and mental; Blane into internal and external, arranging them according to the situation where they are applied rather than the mode of their action. Select Diss. p. 245.

lence short of that which absolutely destroys the texture of the part, are of the first class; a great variety of chemical substances that have few properties in common, as alcohol, acids, alkalies, metallic salts, and many vegetable acrids, are of the second class; while in the third we may place those agents that seem to operate immediately upon the vital powers without producing any apparent physical change in the part, as the electric fluid, and particularly that modification of it which constitutes galvanism. Independent of any external agents, the muscles are thrown into the strongest contractions by a variety of nervous affections which arise from internal causes, and above all from the act of volition. By a process which will probably always remain inexplicable, we no sooner will the motion of any muscle than it obeys the summons with promptness and accuracy.

It is a remarkable circumstance connected with the effect of stimulants upon the muscular fibre, that particular sets of fibres are specifically acted upon by particular stimulants, and this without any difference that we can discover in the fibre itself, or any thing in the nature of the stimulant which could enable us to predict the result. Thus certain substances taken into the stomach produce the healthy action of this organ, and cause its fibres to exercise their vermicular motion, so as in due time to propel its contents into the intestines, while others instantly excite the violent action of vomiting. Substances which have passed through the stomach without producing any particular effect upon its fibres, when they arrive at the intestines, throw these organs into strong contractions. In the same way the urine acts specifically upon the bladder, certain sapid substances upon the salivary glands, and in short there is scarcely one among the muscular coats, or among the system of muscular fibres, that contribute to the production of the organic functions, which have not some specific or characteristic property of this kind. In the proper muscles, those which serve for motion, the effect of stimulants is more uniform, and may, for the most part, be referred to the ratio of quantity.

Concerning the specific nature of contractility, it is most remarkable that it should be called into action by such a variety of agents, and I know of no method of explaining this singularity. The effect of all the substances is, however, the same in kind; or if there should be any difference in this respect, it is in the proportionate degree of their intensity and their duration, or their subsequent and secondary operation on the system. This great variety in the nature of the stimulating agents affords an additional proof of the absolute impossibility of accounting for muscular action upon any mechanical principles; for it not only seems to be equally affected by the two great powers of mechanic impulse and chemical attraction, but by mere mental impressions, which, as far as we can judge, have no resemblance to any of the properties of matter.

Besides contractility, or the proper Hallerian irritability, the muscular fibre has been supposed to possess another specific or peculiar quality, which has been called tone or tonicity. I have already remarked that Cullen, as well as many of the modern physiologists, have insisted upon this property of muscle; and it is a term which is very extensively employed in pathology, but in this case apparently with little precision, and probably without any decided meaning being attached to the use of it. Physiologists have attempted to describe it more accurately, and it has been illustrated by the retraction which a muscle exhibits when its fibres are divided transversely, or by the drawing up of one side of the face when the muscles of the other side have become paralyzed, and by other similar occurrences. It seems, therefore, to be a contraction which the muscular fibre exhibits when not under the influence of any distending force, which takes place without the intervention of any external stimulant, is slow in its operation and limited in its extent, and is not subject to the alternations of relaxation.[6] Although, therefore, both its direct and its ultimate effect be contraction, yet, in every respect, it differs from the proper contractility which has been described above, and I conceive has no connexion with it. Such a power undoubtedly exists in the muscles, but it is by no means certain whether it ought to be referred to the proper muscular fibres or to the membranous matter to which they are attached. There are many circumstances which seem to render it probable that all the soft solids of the body are kept in a state of moderate distention, and that when this

[6] Fordyce, in Phil. Trans. for 1788, p. 30, et seq.

distention is removed, the parts slowly contract, but in a manner which more resembles the re-action of an elastic body than the contraction of the muscular fibre, and I should therefore refer it either to the elasticity of the membranous matter, or to that of the muscular fibre itself, and not in any degree to its proper contractility, to which it seems to have no analogy. Haller accurately discriminates the power of contraction, which the muscular fibres possess in common with all other matter, and which he calls the general contractile power or the dead force, from its proper irritability, as differing both in its seat, its mode of action, and its effects.[7] This general contractile power or dead force seems evidently to be the operation of elasticity, and I conceive that there is nothing in the phenomena that have been ascribed to tonicity, which may not be referred to the same power.

Among the different modifications of contractility which are pointed out by Bichat, I regard that which he styles contractility from structure in the same point of view, as an effect of elasticity, whether residing in the membrane or in the proper fibre, but as having no relation to the Hallerian irritability, of which, indeed, he appears to be himself well aware. I shall not at present follow this author through all his complicated arrangement of the different species of contractility, as the propriety of his divisions depends very much upon his general views concerning the

[7] El. Phys. xi. 2, 1—3.

nature of life, which will be better understood when we are further advanced in our subject.

Besides the specific property of irritability, many physiologists ascribe to the muscular fibre a degree of sensation, and even Haller himself, to whom so much merit is due for the sagacity with which he has discriminated between the powers of the muscles and the nerves, employs expressions from which it might be conceived that he considers the *vis nervea* as an actual property of the muscular fibre itself, as well as its irritability, or *vis incita,* as he styles it.[8] The vis nervea of Haller is that power in the muscular fibre which enables it to receive impressions conveyed to it by the nerves, but this supposed vis nervea ought not to be regarded as a function of its contractility, for in fact it is nothing more than the power which the fibre possesses of receiving the impressions that are made upon it, so as to cause it to contract, the impressions that are conveyed to it by the nerves being only one among the other stimulants which produce its contractions. Bichat supposes that the muscular fibre possesses proper and inherent sensibility, but the muscular sensibility differs from the vis nervea of Haller, as Bichat ascribes to muscles an actual degree of feeling which does not seem to differ essentially from the sensibility residing in the nerves. The doctrine which was so warmly contended for by the antagonists of Haller, and which may, perhaps, be regarded as the most popular at the present time, is in its essence precisely

[8] El. Phys. xi. 2, 15.

the reverse of this opinion, although they are frequently confounded together. According to the neurologists, as they have been termed, where a stimulant acts upon the muscular fibre, the immediate action is not upon the fibre itself, but always, in the first instance, upon the nervous filaments connected with it; but the further discussion of this question, as well as the nature of sensibility itself, and the distinction between this power and contractility, must be referred to the chapter on the nervous system.

I must remark in this place, although the subject will be more fully considered hereafter, that muscular contractility is much influenced by many of those faculties or functions which seem to be intermediate between the corporeal and mental parts of our frame. Thus the power of volition is exercised most conspicuously in every thing which is connected with muscular contractility, and indeed is the grand theatre on which its effects are manifested. The operations of habit and of sympathy are also sufficiently obvious, and in many cases, as I shall afterwards have occasion to point out, these effects appear to be the result of the immediate action of the muscular fibre itself, independently of the intervention of the nervous influence.

§ 4. *Use of Muscles.*

Having given an account of the form and structure of muscles, of their chemical composition and their properties, I now proceed to consider their uses. The general use of the muscles is sufficiently obvious; they are the grand organs of motion, both of that by which the body is moved from place to place, constituting loco-motion; that by which each of its separate parts is moved, when we act upon the contiguous bodies in our intercourse with the external world; and that by which many of the various minute actions are performed, which are essential to the exercise of the vital functions. In short muscular motion seems to be concerned in almost every operation that is produced, either by the system at large or by its individual parts. All these effects are brought about by the simple act of contraction, or that by which the fibres shorten themselves, and by approximating the ends of the muscles, draw together the parts to which the ends are attached. Nor is the operation of the muscular coats less important than that of the proper muscles, although it is less obvious to the eye. Here the fibres, not being collected together into large masses, which act simultaneously, the result of their contraction is not the movement of any particular part, but they act, as it were, fibre by fibre, producing in the organ to which they are attached a peculiar kind of undulatory, or, as it has been termed, vermicular motion, from its resemblance to the crawling of the worm, which serves to keep their contents in a state of perpetual agitation, to mix them intimately together, and ultimately to produce their expulsion.

The particular operation of the muscular coats will be more fully explained when we come to that part of our subject which treats of those functions that depend immediately upon their action; but before we quit this subject it will be necessary to inquire into a point, which has often been discussed, whether all the spontaneous motions that are observed in the body are to be referred to contractility, under one or other of its species? We know that both elasticity and gravity are the cause of motion in the body, but they are sufficiently distinct from contractility; the question is, therefore, whether motions which do not appear to depend upon any of the usual physical powers of matter are all to be referred to contractility? The difficulty which exists in this case is, that there are certain very obvious motions of particular parts, where no muscular fibres have been detected, and yet where the quantity of motion and the size of the part is so considerable, that we might have supposed that the fibre would have possessed a sensible magnitude. One of the most remarkable examples of this kind, and what has generally been adduced as that by which the controversy must be ultimately decided, is the Iris. In this part, which is not very minute, and in which the motions are very rapid and considerably extensive, no muscular fibres have been detected; for although many eminent anatomists and physiologists have, at different times, announced their existence, the latest and best observations appear to decide in the negative. How, therefore, is the iris moved? There is nothing in its action analogous to elasticity, and nothing which can arise from the force of gravity. Blumenbach has attempted to solve this difficulty by saying that the iris and some other parts, which are in the same predicament, possess a peculiar power, which he calls their vita propria, and that this is the cause of their contraction.[9] But when we come to consider this hypothesis, it will appear to be merely a form of speech which throws no light upon the phenomena or upon their cause, and does not tend to generalize analogous facts, but which forms a part of that system of obscure causes which is too often had recourse to by physiologists, when they are at a loss for a rational explanation. Nothing, however, can be more injurious to the progress of science than this method of substituting new words for new ideas, and of advancing hypotheses which, when we come to examine into their real foundation, must be regarded as merely verbal. I have no hesitation in confessing that there are many parts of physiology which are yet unexplained, and the motions of the iris I conceive to be one of these.[1] Upon the whole, however, when we perceive any motions which seem, in all respects,

[9] Inst. Phys. § 42 and 273.

[1] Since the above was written the question respecting the muscular fibres of the iris is decided in the affirmative by the microscopical observations of the accurate and indefatigable Mr. Bauer; Phil. Trans. for 1822, p. 78. See also the still later observations of Mr. Jacob, Med. Chir. Trans. v. xii. p. 514. I may remark that his exquisitely beautiful engravings do not appear on all points quite to correspond with the magnified figures of Mr. Bauer. Berzelius also informs us that the iris has all the chemical characters of muscle; View of Animal Chemistry, p. 86. Another consideration which might be alone sufficient to prove the muscularity of the iris, is deduced from the fact, that in certain individuals the motions of this part is under the control of the will; we are informed by Dr. Roget that this is the case with his eye. Travers's Synopsis of the Diseases of the Eye, p. 72.

to agree with those which are the evident result of muscular contraction, it may not be unreasonable to conjecture that they depend upon the same cause, even although we are not able to detect the apparatus by which they are produced.

§ 5. *Mechanism of Muscles.*

In considering the mechanism of muscles, we must bear in mind that their action consists essentially in the approximation of their extremities, in consequence of the shortening of their fibres, and that the immediate effect of this is to move any body to which the ends are attached. It, however, generally happens that one of the ends is connected with some fixed point, while the other is much more moveable, and of course the bone or other solid body which is attached to it is moved in the same manner. In order to promote the symmetry of form and the facility of motion, we find that, in many cases, the flesh of the muscle itself is not inserted into the body which is to be moved, one or both of the ends terminating in membrane, which, according to the situation or use of the part, is either condensed into a strong cord, constituting a tendon, or spread out into a membranous expansion.

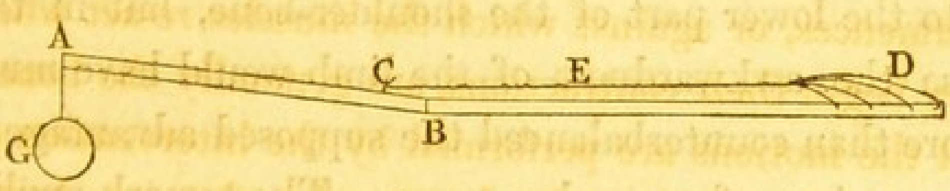
Although a very slight knowledge of the structure and functions of the body would render it obvious that the muscles are the great instruments of its motions, yet no accurate conception of the mode in which they operate seems to have been entertained before the publication of Borelli's celebrated work on animal motion.[2] This writer very ingeniously referred the actions of the muscles upon the bones and solid parts, to the effect of a mechanical power acting upon a lever. He reduced his doctrine to a mathematical form, pursued his idea through all the organs of the body, and clearly explained the mode in which every individual action is produced, in a most elaborate and minute detail.[3] Winslow may also be mentioned as among the first of those who presented a clear and correct idea of the subject, which, although posterior to that of Borelli, may, in some respects, be considered as deserving of even more commendation, because it has the merit of not being clogged with any fanciful hypothesis, which is unfortunately the case with that of the former writer.

The fixed points of the body, from which motion commences, or against which the muscles re-act when they begin their contractions, are generally the bones, and the motions are performed by the intervention of joints. Considering the bones, therefore, as being acted upon by the muscles after the manner of levers, the part where the muscle or tendon is inserted into the bone will represent the power, the joint the ful-

[2] De Motu Animalium.

[3] Although Borelli has been generally supposed to have established his theory of the mechanism of muscular motion, it may be proper to remark that it has been called in question by Barthez. See Art. "Barthez" in Suppl. to Encyc. Brit.; also Journ. de Phys. t. xlvii. p. 271.

crum, and the part that is moved constitutes the weight. Writers on mechanics have divided levers into three kinds, according to the relative position of their three essential parts; the weight, the power, and the fulcrum. Those of the first kind have the fulcrum in the centre; in those of the second kind the weight is in the centre; while in the third the power is in the centre; the bones are of this last description, in which the power is placed between the fulcrum and the weight. The motion of the fore-arm may be taken as an example of the effect of muscular contraction and the manner in which it is produced. When we wish to raise a weight by bending the elbow joint, it is effected by muscles situated below the shoulder, which have tendons inserted into the top of the bone of the fore-arm near the elbow.[4] Let A B represent the fore-arm, B D the shoulder-bone,

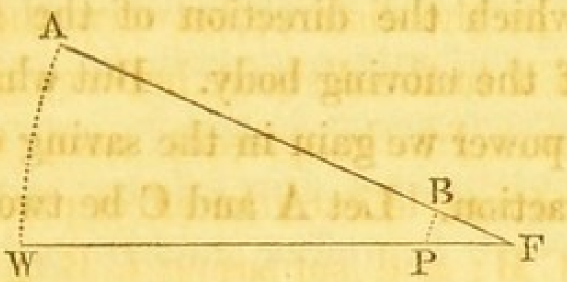


D the muscle, E the tendon, C the insertion of the tendon into the fore-arm, and B the elbow-joint. The contraction of the muscle tends to approach C to D, which, as D is a fixed point, is effected by bending the joint B, raising up the point C, and consequently any weight G which may be attached to it. The consideration of the manner in which the muscle acts in this case, proves that the mechanism of the

[4] Winslow, sect. 3. art. 5; and Mem. Acad. for 1720.

animal body is calculated to produce a great loss of absolute power. It is an established position in mechanics, that in the action of levers, the power is to the weight as the distance between the weight and the fulcrum is to the distance between the power and the fulcrum. In the present case, therefore, the power of the muscle is to the effect produced by it as A B is to C B; and supposing C B to be one-twentieth of the length of A B, one-twentieth only of the power of the muscle is exerted in raising the weight, the rest is expended in acting against the disadvantage of the position. We shall, however, find it to be a general fact, or, as it is termed, a law of the animal œconomy, that muscular power is always sacrificed to convenience. Had the object been to raise the weight with the least possible power, the muscle would have been placed on the fore-arm, and the tendon inserted into the lower part of the shoulder-bone, but in this case the awkwardness of the limb would have much more than counterbalanced the supposed advantage of the saving of muscular power. The remark applies with still greater force to the fingers. At present they are moved by the contraction of muscles placed on the fore-arm, and are connected to them by long delicate tendons which pass over the wrist and hand. But if this order had been reversed, and the flesh of the muscle had been placed on the fingers, the hand would have been almost useless from its clumsy form.

Another important advantage which arises from the present construction of the muscles, as consisting of levers, where the power is situated near the fulcrum is, that we acquire a great degree of velocity. Let W P F represent the bone of the fore-arm, in which

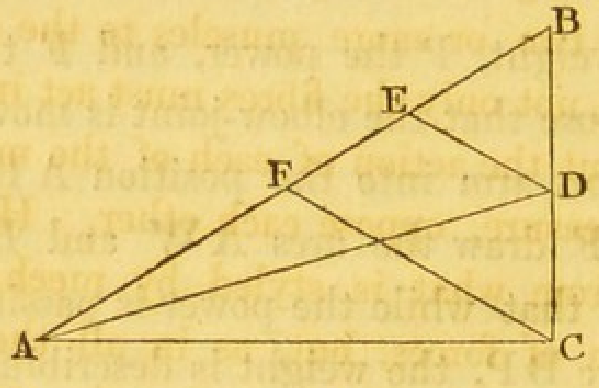


W is the weight, P the power, and F the fulcrum. Let us suppose that the elbow-joint is moved so as to bring the fore-arm into the position A B F. From the centre F draw the arcs A W and B P, and it will appear, that while the power is passing through the small arc B P, the weight is describing the large arc A W. Now the arcs are to each other as the radii W F and P F, and the arcs are passed over in the same time, the velocities will therefore be as the lines W F and P F, or in this case as 20 to 1. Paley judiciously remarks that there are many more cases in which it is useful to raise a small weight rapidly than a large one slowly.[5]

Besides the loss of power which is occasioned by the nature of the lever, in consequence of the power being applied nearer the fulcrum than the weight; there are other circumstances in the construction of muscles which produce the same effect, by which there is a very considerable expenditure of absolute power; but in this, as in the former case, this loss of power is always attended with some very obvious advantage.

[5] Natural Theology.

Most of the muscular fibres are so placed as to act obliquely, and it was well known that by this arrangement a quantity of power is lost, in proportion to the degree in which the direction of the fibres differs from that of the moving body. But what we in this case lose in power we gain in the saving of the quantity of contraction. Let A and C be two fixed points,



and let B be some moveable point, which is to be brought down to D. If it were done by a straight line, passing in a perpendicular direction B C, it would be necessary that the fibre should contract equal to half its length, supposing B D to be equal to D C; whereas the same effect will be brought about by the oblique line A B, contracting through the space E B only, which is evidently a less proportion of its length, E B being less than A F by E F. It is obvious that the antagonist muscles will be less stretched, that there will be a less displacement of parts, a less degree of pressure upon the vessels and nerves, and that less distention and straining of the membranous matter will ensue, the smaller is the degree of contraction of the fibres, and the less alteration the muscle consequently experiences in its general form. In pursuance of the same principle we may remark that the extent of action in a muscle is necessarily in proportion to the length of the fibre, for it is obvious that a long fibre will have to diminish in a less proportion than a shorter one to produce the same degree of absolute contraction.[6]

A third source of loss of power depends upon the situation of the muscles with respect to each other, an action being seldom performed without the concurrence of two or more muscles to the same effect. In this case, not only the fibres must act in an oblique direction, but the action of each of the muscles must, in some measure, oppose each other. Here is a loss of power from what is styled by mechanicians the composition of forces, but, as in all the former instances, the present construction is attended with many important advantages. When two or more muscles act upon the same point, the effect will be to draw the body in the diagonal; and we consequently have it in our power to alter the direction of the motion with great ease and accuracy, by throwing, at pleasure, a little more or less energy into one or other of the muscles, and drawing the body into any of the intermediate positions. Thus a great variety of motions may be produced by two muscles only, and the body is less liable to feel fatigued in any one part by the exertion being, as it were, diffused through a larger space, and a less quantity of it being required at each single point.

A fourth circumstance connected with the mecha-

[6] Winslow's Anat. sect. 3, art. 1, § 54.

nical construction of muscles, which proves a source of the loss of power is, that the tendon is generally inserted into the bone at an acute angle, whereas, in order that the power should have operated to the most advantage, it ought to have acted upon the lever in a perpendicular direction. Upon the same principle, power is also lost by having the muscular fibres inserted obliquely into the tendons; but although power is thus sacrificed, it is obvious that the present arrangement is much more commodious; and indeed, in many cases, it would have been impossible for the muscles to have acted perpendicularly upon the bones, or to have been differently inserted into the tendons without a total change in the form and arrangement of all the body.

A fifth cause by which muscular power is lost arises from the circumstance of the two ends of the muscle pulling against each other. This is obviously the case where both ends are moveable, and where one end is fixed, as much force is expended on this as on the moveable extremity, and before this latter can produce any effect, it must counteract the resistance offered by the former, and in this case exactly half its mechanical power is lost before the motion commences. Besides what have been mentioned, physiologists have pointed out other circumstances which, in like manner, cause a loss of absolute power, but where this loss either necessarily arises from the nature of muscular contraction, is essentially connected with the form of the body, or is compensated by some obvious advantage.

Amidst so many examples, where muscular power is expended for the purpose of producing some important benefit to the system, there are a few instances of a contrary kind, where the parts are evidently formed for the purpose of assisting muscular action. The heads of the bones into which the tendons are inserted, not unfrequently swell out into a rounded projection, by which means the muscles act upon the bone at a less oblique angle, and this we observe to take place more particularly in those cases where the greatest exertion of muscular power is required, as in the muscles of the trunk and the lower extremities. For the same purpose some bones are provided with processes of considerable length, which seem to be solely intended for the insertion of muscles, and the same appears to be the principal object of the small detached bones, which are occasionally found near the joints, as the patella and the sesamoid bones. These, however, can only be regarded as exceptions to the general rule, and in all cases we perceive the operation of the principle which was stated above, that the quantity of power employed appears to have been no object in the construction of the body, but that it is always sacrificed, without any reserve, either to general convenience, to the symmetry of the form, to the gaining of velocity, or to the saving of the extent of contraction. The advantages which arise from the velocity of our movements, from the facility with which we can alter their direction, and from the connexion of the muscles to the moving points, by means of tendons, which, like ropes, serve to convey the force from the point where it is actually generated, to the part where it is wanted to be employed, are so obvious as to require no further illustration. It is not, however, so evident what is the advantage to be gained by the saving of contraction, yet so much attention appears to have been bestowed upon this point, that we must suppose there to have been some urgent reason for it. As the intimate nature of muscular contraction is itself unknown, it is scarcely to be expected that we should be able to give a satisfactory solution of this difficulty; for it does not obviously depend, like the former circumstances, upon any general principles of mechanics, but upon something specific in the nature of muscular contraction. I shall, however, hazard a conjecture upon the subject, after premising that it is merely to be regarded as such, and must therefore be maintained no longer than it appears to be countenanced by the phenomena, or serves satisfactorily to explain them, without violating any established principle or well-ascertained facts.

The only conception that we can form of the contraction of the muscular fibre is, that it consists in the approximation of the individual parts of which it consists, whether it be of the whole fibre, or of some of its constituents, which give it its specific properties. This attraction does not seem to bear any resemblance to the attraction of gravity, or to that of chemical affinity, the one operating upon large masses of matter, the other upon the separate particles of which they are composed. The contraction of the fibre appears to differ from them both in its causes and in its phenomena, and we may therefore suppose that it is essentially different in its general laws and modes of action. The force of the attraction of gravitation increases as the distances decrease, and this is probably the case with chemical attraction, but it would appear that the attraction of contractility has not this property, or at least that this property, if it exist, is counteracted by other circumstances. Of these circumstances one is sufficiently obvious, the re-action of the membranous matter attached to the muscular fibre. Whether this be the only cause, or whether there be others that operate, whether the attraction between the particles of the muscular fibre increase with the decrease of distance, whether it be the same at all distances, or whether it may not even decrease in the direct ratio of the distances, are questions that it is entirely beyond our power to answer. But this is certain, that as the particles approximate, the membranous matter will be compressed or bent out of its ordinary situation, and that this compression will increase as the distances diminish; that, therefore, as the degree of contraction increases, a greater force will be required to continue it, and still more to go on augmenting it.

As an illustration of the nature of the process, we may conceive of two bodies differently electrified so placed, that when they act upon each other they may compress a spiral spring. Let them be brought within the sphere of their mutual attraction, we may suppose that a certain quantity of electricity will be necessary to cause them to commence their approach to each other, and that as they advance, and the mechanical resistance of the spring becomes greater, they are unable to proceed without a new accession of electric fluid, and the nearer they approach the greater quantity will be requisite. This supposition or conjecture would reduce the effects of muscular contractility to an attraction between the individual particles of the fibres, which is counteracted by the elasticity of the membranous matter that is connected with them. Whether this attraction, like that of gravity, increases as the distances decrease, and is counteracted in its operation merely by the elastic nature of the membrane, is a point upon which I do not pretend to offer any opinion. We can only say that the membrane must be compressed, and in proportion to the increase of compression so will be the necessity for an increase of power to continue the contraction.

In speaking of the animal fabric we are obliged to employ terms derived from the workmanship of other bodies. We therefore speak of the sacrifice of power and of its expenditure, as we should do in a machine of any description, where the object of the engineer is to œconomize labour. But no idea of this kind ought to attach to our conception of the human body, where a certain construction of parts was adopted, as being the most useful for all the purposes of life, and those powers are assigned to it which are necessary for preserving it in its proper condition. Muscular contractility is one of these powers, and of course the proper quantity was given for the due performance of the functions that depend upon it.

Having given an account of the mechanism of muscles and the nature of their operation, it remains for me to make some remarks upon the force, the velocity, and the extent of this power. From the observations that have been made above, upon the quantity of force that is expended in order to produce a certain effect, we may conclude that the absolute force of muscular contraction, the power which the fibres actually exert, is exceedingly great. The mechanical physiologists, and especially Borelli, attempted to estimate the degree of this force, and although we cannot place implicit confidence in their estimates, as they proceed, in some measure, upon hypothetical and erroneous principles, yet we may allow that they are sufficiently correct to assure us that it exceeds very much any idea that we should have previously formed respecting it According to his estimate the flexor muscles of the thumb possess a power equal to nearly 4,000 lbs. which may be considered as at least 100 greater than the actual power which they are capable of exercising.[7]

There are many cases in which the velocity of muscular contraction is no less remarkable than its force. As an example of this the muscles connected with the organs of speech are often adduced, where, in rapid enunciation, the number of distinct contractions that take place, in order to form certain combi-

[7] Prop. 126; the estimate of Borelli is, however, conceived by Pemberton to be considerably exaggerated.—See Introduction to Cowper's Myotomia Reformata.

nations of vocal sounds, is very great, each word, or rather each syllable, requiring several different contractions, which must succeed each other in rapid succession, with proper intervals between them. The motions of the muscles connected with the fingers, in playing upon musical instruments, is no less remarkable, for here the contractions are generally greater in extent, and therefore must proceed with proportionably more velocity, although they do not succeed each other so rapidly as those of the vocal organs.

The extent of muscular contraction, or the degree in which any particular muscle is capable of shortening itself, has not been very accurately ascertained. In the proper muscles it has been thought that the fibres never diminish to more than one-third of their natural length, and even this must be considered as a remarkable case. In the muscular coats indeed the contraction of the whole substance is much greater, but then it is not ascertained what portion of it belongs to the proper fibre and what to the membrane. It has been remarked that some of the lower tribes of animals, as the polypi and the actiniæ, appear to contract their limbs to a much greater degree, and there is every reason from analogy to believe that they are provided with organs for the purpose of motion, which may be considered as muscular; but scarcely enough is known concerning their nature to enable us to employ them as the basis of any calculation that we may form upon the subject.

§ 6. *Hypotheses of muscular Contraction.*

After this account of the structure of the muscular fibre, of its chemical nature, its properties, uses, and mechanism, I come, in the last place, to consider the hypotheses that have been invented to explain its action. Two distinct questions here present themselves; first, What is the efficient cause of the contraction of the fibre, or by what physical cause is it produced? and secondly, What is the cause of contractility, or of that property of the fibre which produces contraction? These questions have generally been confounded together, or have been considered as involving only one subject of inquiry, and yet I apprehend they are clearly distinct, and that we may conceive it possible to afford a satisfactory answer to one of them, without our being able to solve the other. We may perhaps discover something in the mechanical construction of the fibre, in the arrangement of its particles, or in the mode in which its constituents are connected to each other, which may explain to us the reason of their alternate contraction and relaxation, upon the application of the appropriate exciting cause; or, on the contrary, without being able to accomplish this object, we may perceive the correspondence between the operation of some external circumstance and the action of contractility, which will warrant us in regarding this as the cause of the effect. Both the above questions are highly interesting, but they are unfortunately both of them of very difficult solution.

With respect to the efficient cause of muscular contraction it may be remarked generally, that every attempt to account for it on the principles of mere mechanics must be obviously abortive, because in the operation of the muscles we have an actual generation of power. In the best contrived machinery we have only the existing power applied in a new direction, better adapted for some particular object; but power is never actually generated. In those engines which act from the mere force of gravity, what we gain in power we lose in velocity, or the reverse; and when from the re-action of an elastic body, as from the recoiling of a spring, there seems to be a real production of power, the effect thus apparently produced is no greater than was originally employed in compressing it, and its effect is necessarily limited to a short period, for when this power is expended all motion ceases. On this account it will be unnecessary for us to enter into any detail of those hypotheses which attribute muscular contraction to any mechanical construction of the fibre; as that of Borelli, who supposed that it consisted of a series of rhomboidal vesicles, which were in some way made to expand when the muscle contracted, and to collapse when it was relaxed, an hypothesis which he laboured with much care and supported by a long train of mathematical reasoning. Or the hypothesis of Steuart, which is framed with a great appearance of learning and geometrical precision, and which supposes that the muscular fibre is composed of a string of vesicles formed of the substance of the nerves, which during muscular action are inflated by the ingress of the nervous fluid. But it is sufficient to remark concerning them, that the whole rests upon a supposition which is not countenanced by a single direct fact, and that should we admit the vascular form of the fibre, we are still as much at a loss as at first to know in what manner it becomes distended, and shall have to call in some new agent to perform this part of the operation. Nor shall we find a better explanation of the efficient cause of muscular contraction by having recourse to any chemical operation, such as the production of a gaseous body, which was a favourite notion with the physiologists of the seventeenth century, who supposed that there was an effervescence excited in the muscle. This effervescence was attributed to various causes; by some to the mixture of an acid and an alkali, which were imagined to be brought together in some mysterious manner, while others, and those among the most learned and ingenious men of the age, such as Willis, Bellini, Mayow, and Keill, ascribed it to a fermentation or effervescence, excited by a union of the particles of the muscular fibre with the nervous fluid, or with some etherial spirit contained in the blood. When electrical phenomena began to be attended to, it was supposed that the fibres of the muscle might be disposed in such a manner as to form a kind of battery, which should produce contraction by its explosions; and after the discovery of galvanism, an elaborate attempt was made by Valli of Pisa, to account for muscular action by supposing that the muscles consisted of an arrangement of parts analogous to that of the elements of the galvanic pile.[8] It will be quite unnecessary to enter upon any formal examination of these hypotheses, which are now completely discarded; it is sufficient to observe concerning them that they are not supported by any foundation of facts, that they have scarcely any analogies in their favour, in short, that they were purely gratuitous, and could never have been tolerated, had not the mind been disposed to listen to any thing which promised to throw the smallest ray of light upon a subject that was involved in so much obscurity.

There is, however, one hypothesis which it may be proper to notice, not from its intrinsic excellence, or from its giving us any real insight into the nature of muscular contraction, but as affording a curious fact in the history of science, and one which it may be useful to relate, in order to impress upon our minds the danger which even the most learned men incur, when they permit themselves to confound a train of reasoning with a deduction of facts. I refer to the theory which was formed by Keill to explain the nature of muscular contraction. Keill was a man of considerable talents, a profound mathematician, and a zealous cultivator of the various departments of medicine and physiology. In forming his theory he set out by adopting the notion of Borelli, that the

[8] Experiments on Animal Electricity, with their Application to Physiology, by L. Valli. See Brit. Crit. for Mar. 1794. Jour. de Phys. t. xli. contains several letters of Valli on the subject.

muscular fibre consists of rhomboidal vesicles, resembling a string of bladders, which communicate with each other. When these vesicles are inflated, provided at the same time that their substance be not actually distended, it is obvious that the total length of the fibre will be diminished, and hence that the whole muscle will be shortened. Now the blood and the animal spirits were made the agents to produce this distention, and in proof of this idea it was stated, that upon the tying up of the artery and nerve that lead to a muscle, the part loses its power of contraction. But this inflation may be accomplished in two ways, either by a greater quantity of blood and animal spirits being sent into the vesicles, or by the quantity previously in them being rarified and thus made to occupy a greater space; and after deliberately considering both these points, and weighing their respective probabilities with much acuteness and ingenuity, a decision is made in favour of the latter supposition. Nor is there less ingenuity displayed in the mode in which this rarefaction is supposed to be brought about. It is stated that the blood contains a great quantity of air mechanically mixed with its fluid particles, which by various means may be separated from it, and caused to assume the elastic state. In some way, which does not indeed seem to be very thoroughly explained, this air gets into the vesicles, and is kept there in a state of compression, the elasticity of the air being balanced exactly by the strength of the vesicles. If however we could suppose any other substance to enter the vesicles, which might cause the particles of the air to acquire a great increase of their elasticity, the vesicles must become distended, and this effect is conceived to be brought about by a quantity of the nervous fluid coming into contact with the air. When these two bodies are mixed together, a sudden increase of bulk is the consequence, the vesicles are quickly distended, and the muscle proportionably shortened.

Having thus explained the manner in which muscular contraction is produced, which, as the author triumphantly observes, is accomplished without having recourse to any chemical hypothesis of fermentation, effervescence, or precipitation, we are next to explain how these minute vesicles can, by their action, produce such prodigiously great effects. And here we have a long and very abstruse train of mathematical reasoning, founded upon the principle, that if a bladder by its inflation will raise a certain weight, by dividing the bladder into a number of smaller ones connected together, we diminish the capacity of the cavity without diminishing the extent of its contraction, so that the same bulk of gas will raise a much greater weight than if it had been all contained in one bladder. Proceeding upon this principle, we may diminish the size of the bladders and increase their number to an indefinite extent, and thus, with only a given quantity of gas, we may produce an unlimited effect. Thus if a bladder of a certain size will cause a muscle to shorten itself to the extent of an inch, if the bladder be divided into 100 parts, and these be disposed in a straight line, the contraction will still be equal to an inch, but the quantity of actual inflation and the force required to produce it, will be 10,000 times less than when the single bladder is employed.[9] It is not a little curious to observe how much acuteness is displayed in constructing the several parts of this hypothesis, what advantage is taken of the various circumstances that appear to favour it, and how the supposed objections that might be urged against it are repelled; yet I need not say that the whole fabric is destitute of the slightest foundation, that it all rests upon a set of gratuitous positions, that have not the least ground of probability for their support, and that, on this account, all the ingenuity manifested in the finishing of the subordinate parts is completely misapplied.

There is much more simplicity and less violent improbability in the hypothesis of muscular contraction that was advanced by Prochaska, although, I fear, we must admit that it is equally without foundation. From his explanation of the structure of the muscles he concludes that the minute branches of the arteries are everywhere connected with the ultimate muscular filaments, that they creep about them, and cross them in all directions. Hence he argues that when these vessels are rendered turgid by an accession of blood, in passing among the filaments they must bend them into a serpentine form, and thus diminish their length, and that of the muscle generally.[1]

[9] Keill, Testamina, No. V. [1] De Carn. Mus. § 2, c. i.

In connexion with this hypothesis of Prochaska's I think it not improper to notice a microscopical observation of Hales's, which although it may have been perverted by the causes which are so apt to affect all observations of this kind, comes from too respectable a quarter to be entirely neglected. He informs us that when he viewed the muscles of a frog with a powerful lens, he observed the fibres lying parallel to each other, with the blood running up and down between each fibre in the small capillary arteries. If the muscle was then made to contract, to use his own expression, "the scene is instantly changed from parallel fibres to serieses of rhomboidal pinnulæ, which immediately disappear as soon as the muscle ceases to act."[2] We may easily conceive that an appearance similar to what Hales describes would follow from the shortening of the fibres and the necessary contraction of all the parts connected with them, but it does not throw any light upon the nature of the operation, nor does it enable us to judge whether the fibres or the vessels were the prime agents in the production of the change.

The only other opinion which I shall notice on this subject is the one that was brought forward a few years ago by Sir G. Blane, and that indeed more in the form of a conjectural speculation, than of a formal hypothesis. As he was led from his experiments to conclude that the actual bulk of the muscle is not altered during its contraction, but that it

[2] Statical Essays, vol. ii. p. 59.

gains in thickness exactly what it loses in length, he observes that we may account for this change by supposing that the muscle is made up of particles of an oblong form, and that when the muscle is contracted, the long diameter of the particle is removed from a perpendicular into a transverse direction.[3] This speculation has certainly the advantage over Keill's, in containing a much smaller number of assumptions, but its foundation is equally gratuitous, and, like that of Prochaska, seems totally inadequate to produce the effect in question.

After such melancholy examples of failure before our eyes, it will not be expected that I should attempt to unravel a mystery, which has hitherto remained in such impenetrable obscurity. It may, however, be desirable to state in what degree the efficient cause of muscular contraction is a legitimate object of inquiry, and towards what points we ought particularly to direct our attention. In the first place, the simple act of contraction must consist in the approximation of the particles of which the fibre is composed, and this may be brought about in various ways. The fibre itself may be condensed in its whole substance, or it may be bent or folded up into a kind of zig-zag form, or the attraction between some of its parts may cause the whole to be corrugated, thus shortening it in its perpendicular direction without producing any actual condensation. We have, however, no proof either from the evidence of our senses, or

[3] Select Dissert. p. 243.

from any correct deduction of reasoning of any specific structure or constitution of the fibre which can, in any degree, explain the manner in which this approximation is effected. It is not likely that any further discovery can be made upon this subject by the aid of microscopes, for it appears that there is a limit to the employment of high magnifiers, beyond which the liability to ocular deception is so great as to counterbalance any supposed advantage from the increased magnitude of the object. If, therefore, any additional information can ever be acquired on this point, it is more likely that it will be done by observing the effects produced on the whole muscle, and by tracing the analogy between these effects and other natural phenomena, than by the mere examination of the separate fibres. And if we are unable to account for the approximation of the particles, still less are we able to explain why the various things which we call stimulants, so extremely heterogeneous in their nature, and which have no other common property, should all coincide in producing the same effect upon the fibre. This is so unlike the operation of any other physical cause with which we are acquainted, that we must for the present consider it as an ultimate fact, one of those mysteries in nature which daily present themselves to our observation, but which elude all our attempts to refer them to any more general principle.

The other inquiry which I proposed, what is the cause of contractility, remains involved in as much obscurity as the one we have been considering. The attempts that have been made to explain it are not less numerous than in the former case, nor can they be considered as more fortunate, although they may probably appear less palpably absurd. Before we enter upon the inquiry it will be proper to have a clear and explicit statement of its object, a circumstance which is always necessary in philosophical investigations, but which seems to be particularly so in this instance, where several hypotheses have been advanced, which in fact appear to be no more than mere verbal explanations, or peculiar expressions which do not convey any distinct idea to the mind. Our object is to inquire whether, when a muscle contracts in consequence of the application of a stimulus, this event is uniformly preceded by any other event, so that the latter may stand to the former in the relation of its cause. This necessary antecedent to contraction may be something of a peculiar and specific kind, or it may be referable to some of the other agents in nature. If it be of the former description, we are to prove that it is governed by appropriate laws, that cannot be referred to any other power, and we are to point out in what this specific difference consists. If it be found to belong to any of the known agents, we are to prove the reality of this connexion, to show that the effect never takes place without the presence of this supposed agent, that when this agent is present the power of contraction continues, and that an increase or diminution in the quantity or force of the agent is always attended by a corresponding increase or diminution of the contractile power.

It will be necessary for us to examine more in detail the hypotheses that have been formed to account for the cause of contractility, than those concerning the efficient cause of contraction, because while the latter have been all nearly discarded and are generally neglected, the former are many of them of modern growth, are maintained by many living authors, are daily referred to by physiologists and pathologists, and are made the foundation of many topics both of speculation and of practice. We may arrange these hypotheses under two divisions; first, those which ascribe muscular contractility to the presence of some extraneous agent or power superadded to the animal body; or, secondly, those which ascribe it to some peculiar state or function of the body itself. The idea that contractility depends upon the presence of free caloric may be adduced as an example of the first, and, as an instance of the second, the opinion that contractility necessarily results from a peculiar chemical composition of the muscular fibre. The first class of hypotheses will not detain us long, because they have been brought forwards in a less formal shape, and because, being less clogged with obscure speculations, and being of a more palpable nature, they are more easy to refute. Because it was observed that there is a connexion between the temperature of an animal and the degree of its contractility, some physiologists have conceived that contractility depended immediately upon caloric, or the matter of heat interspersed in an uncombined state between the fibres. Others, perceiving how remarkably the muscles are affected by the electrical fluid, supposed that this was the immediate cause of muscular contractility, and set themselves to invent different modes in which what they styled animal electricity might be generated. With respect to both these hypotheses we may remark that caloric and the electric fluid are found to be very powerful stimulants to the fibre, and it would appear, with respect to the first of them, that a certain range of temperature is necessary for the existence of the contractile state. Another opinion concerning the cause of contractility, which must be placed in our first division, was fashionable a few years ago, according to which the immediate cause of this property was ascribed to oxygen. It was conceived that oxygen is absorbed by the lungs during respiration, is carried by the arterial blood to the muscles, and gives them their contractile power. It was imagined that in various states of the system, and from various incidental causes, oxygen was absorbed and carried to the muscles in very different quantities, and that in proportion to the quantity their contractility was increased or diminished. This speculation, which appears to have been first formally brought forwards by Girtanner,[4] and was zealously adopted by Beddoes, was applied by him very extensively to pathology, and was made the foundation of some supposed improvements in the practice of medicine. For a short time this doctrine obtained a considerable share of popularity, but when the first impression of

[4] Journ. de Phys. t. xxxvii. p. 139.

novelty had subsided, and its real merits began to be canvassed, it was found to be built upon a set of entirely gratuitous positions, and was almost universally abandoned. It has, however, lately found a supporter in M. Richerand, a writer more remarkable for a popular air which he gives to his works, and for the liveliness of his imagination, than for the correctness of his judgment. He adds to it the additional speculation, that the union of the oxygen in the arterial blood and the elements of the muscle is brought about by the nervous fluid, which produces an effect something like that of the electric spark.[5]

In the second class of hypotheses that have been formed to account for contractility, there is one that has been detailed with a considerable degree of minuteness, and has had a great variety of arguments, and even experiments, adduced in its favour; I refer to that which ascribes contractility to the chemical composition of the fibre. It is found that a certain proportion of chemical elements composes a body endowed with certain properties, and whenever the elements are put together in a proper proportion, these properties are the necessary result. Thus, a certain proportion of sulphur and oxygen forms sulphuric acid, a substance which possesses a set of qualities necessarily belonging to it: it is a heavy, unctuous, acid fluid, and we may correctly say, that these properties are necessarily attached to its chemical composition.

[5] Elements of Physiol. § 163; see also Blumenbach's Inst. by Elliotson, § 50, 54.

The same reasoning is applied to the muscular fibre; this body is composed of carbon, hydrogen, azote, and oxygen, which all of them exist in a certain proportion, and when they are united together they form the body which we call a muscular fibre, which possesses a certain set of physical and chemical properties, and also the physiological property of contractility. Contractility is said, therefore, to be as much the necessary result of the chemical elements which compose the fibre, as acidity is of the compound of oxygen and sulphur which composes sulphuric acid. In order to prove this hypothesis by experiment, an attempt has been made to show, that if by any means an alteration be made in the proportion of the elements of which the fibre is composed, without, at the same time, destroying its texture, or its physical properties, a corresponding change is brought about in its contractile power. Humboldt particularly directed his attention to this point, and endeavoured to demonstrate that a very slight change in the chemical composition of the muscle entirely destroys its contractility, while, by restoring the original composition of the muscle, the contractility is also restored. As oxygen is the most variable of the components of the muscular fibre, or at least that which is the most easily added and subtracted from it, by means of chemical re-agents, his experiments chiefly consisted in observing the effects of this substance upon contractility, and by employing galvanism as a test of the presence of the contractile power, he found that it was perceptibly affected by very slight variations in the proportion of the chemical elements of the muscle.[6] There are some interesting experiments that lead to the same conclusion in the thesis of Smith,[7] a work which, in consequence of its peculiar destination, has been little known to the public, but which contains more valuable and interesting matter than many bulky volumes that have acquired a high degree of celebrity. The experiments were performed about the year 1766, long before the chemical theory of contractility was thought of. Their immediate object is to show the effect of chemical agents in increasing or diminishing the power which the muscles possess of being affected by stimulants of various kinds, and in some cases their operation was such as rather to indicate a change in the composition of the muscle than merely in its contractility.

A train of reasoning has been brought forwards by the chemical physiologists, in favour of their views of contractility, derived from the state of the muscular fibre after death, which is found to differ very much according to the mode in which life has been destroyed. If an animal in full health be suddenly killed, the muscles are firm and rigidly contracted, and they remain a long time without undergoing the process of decomposition; whereas, on the contrary, if death ensue after violent exercise, if it be caused by lightning or electricity, or by the operation of some kinds of poisons, the muscles are relaxed and soft, have lost

[6] Experiences sur le Galvanisme, &c. par Jadelot; also Ann. Chim. t. xxii. p. 51; Journ. Phys. t. xlvi. p. 465; t. xlvii. p. 65.

[7] Test. Phys. Inaug. de Actione Musculari, Appendix.

all their contractility, and much sooner become putrid; and it is found that in all instances these two states or conditions correspond to each other, *viz.* the degree of contractility remaining in the muscle and its tendency to putrefaction. As the decomposition of the substance of a muscle is obviously a chemical operation, and as it thus appears to be so intimately connected with its contractility, it was concluded that contractility is the necessary result of a peculiar combination of chemical elements. This argument in favour of the chemical theory has been also extended to the connexion that has been observed between the contractility of the muscular fibre and the coagulability of the fibrin of the blood. The greatest part, if not all those circumstances which affect the contractility of the muscle, are found to produce a proportionate and corresponding effect upon the coagulation of the fibrin. But the fibrin of the blood, it is said, is a mere chemical compound; any change in its coagulability must therefore depend upon an alteration in its chemical constitution, and as the muscular fibre exactly resembles it in its chemical composition, and as there is a strong similarity between their respective properties of contractility and coagulability, so it is inferred that the former must likewise depend upon a chemical combination. For the facts respecting the blood we are principally indebted to J. Hunter, who, however, brought them forwards with a very different, and even a directly opposite, view, to prove that the blood, in consequence of its exhibiting properties so analogous to those belonging to the muscular fibre, is the appropriate seat of vitality. This hypothesis will be discussed in its proper place; but in the mean time I may remark, that whatever conclusion we form concerning Hunter's speculation on the life of the blood, still it indicates an intimate and necessary relation between its physiological and its chemical properties.

It would appear then, upon the whole, that there are some striking facts and strong analogies in favour of the chemical hypothesis, and they certainly go so far as to prove that there is a very intimate connexion between the chemical composition of the fibre and its contractile power. But they do not prove any thing besides this; they demonstrate that a connexion exists between the two circumstances, not that one is the cause of the other. Indeed there is an obvious and well-known fact which is decisive against this supposition. A muscle immediately after death has the same chemical composition as during life, yet if life be completely extinguished, contractility is gone and can never be restored. If it be said that a chemical change in the muscle commences immediately after death, but that it is too slight to be detected, I reply that a change which is imperceptible cannot be adduced as the foundation on which to build an hypothesis. And were it even proved that this alleged change did exist, it might still be objected that the progress of chemical decomposition bears no proportion to the alteration in the state of the contractile power, the former commencing very slowly, and by almost imperceptible degrees; the latter proceeding rapidly, and in a short time having arrived at the utmost change which it ever experiences. Besides, although it be a less direct argument against the chemical hypothesis, and one that is merely analogical, yet it possesses considerable weight, that although there are many substances that possess nearly the same chemical composition with the muscular fibre, yet no other body in nature exhibits any property which is at all similar to contractility; it does not, indeed, resemble any other quality in nature, and although exclusively attached to a substance composed of certain chemical elements, it seems more natural to suppose that it is superadded to these elements, than necessarily resulting from their combination.

Another opinion respecting the cause of contractility, which has been frequently brought forwards, although scarcely in the form of a regular hypothesis, is, that this property depends upon the mechanical structure of the fibre. We observe a body possessed of a very peculiar arrangement, unlike every other in nature, and possessed of an equally peculiar property, it is therefore concluded that the property is the necessary result of the peculiar structure or arrangement of the parts of which the body is composed. But the answer that was made to the chemical theory applies to this with still more force; that long after the contractile power is extinguished by death the structure remains unaltered, and as any material alteration in this respect would be of a more palpable nature than the former, so we may conclude, with more confidence, that it has not taken place.[8]

Many physiologists of the first eminence, among whom we may include Haller and Cullen, when endeavouring to account for the cause of contractility, have thought it sufficient to say, that this attraction between the particles of the fibre, which causes it to contract, is nothing more than a peculiar mode or species of the attraction which subsists between the particles of all matter. And this appears to be nearly the opinion of Fordyce, when he ascribes contractility to what he calls the attraction of life,[9] and of those physiologists who speak of it as an attraction depending upon the operation of the vital principle. Many facts and experiments have been adduced in favour of the hypothesis of attraction, which all tend to show that there exists a greater degree of cohesion between the particles of the muscular fibre during life than immediately after death, and before we can conceive that it has experienced any material change in its chemical or physical constitution. Observations of this kind occur not unfrequently in the writings of the older physiologists; among the moderns Sir G. Blane attempted to prove this diminished cohesion by a direct experiment on the muscles of the thumb,[1]

[8] This view of the subject may derive some confirmation from the fact discovered by Hunter, that a muscle may have its fibres much shortened without any diminution of its contractility. Home's Lect. p. 40.

[9] Phil. Trans. for 1788, p. 25.

[1] Blane's Select Dissert. p. 237.

and remarks of the same kind have been made by Carlisle[2] and by Bichat.[3] With a view to the same conclusion it has been observed, that when a muscle is ruptured during life it is the tendinous part which is disposed to give way, while, on the contrary, after death the fleshy part is always weaker than the tendon.[4] Although perhaps it is impossible to perform any very decisive experiments upon this subject, yet we admit the truth of the position, that during muscular action the particles of the fibre are more strongly attracted together, and indeed the very nature of the operation implies that this must be the case. We must also bear in mind that mechanical violence is itself a stimulus, and that in consequence of the admirable arrangements of the animal œconomy, the very circumstance, which would otherwise tend to disorganize the muscles, has the immediate effect of preventing this disorganization from taking place. Yet, although we admit the existence of this increased attraction as an actual fact, it affords no explanation of the cause of contractility. The hypothesis, when stripped of the peculiar language in which it is conveyed, amounts to nothing more than the expression of the fact in new terms, for still the fundamental difficulty remains, what it is which determines the attraction between the particles of matter to exert their power in this peculiar manner

[2] Phil. Trans. for 1805, p. 3; the author remarks that the diminished cohesion after death occurs only in the direction of the fibre.

[3] Anat. Gen. t. ii. p. 398.

[4] Carlisle, in Phil. Trans. for 1805, p. 4.

and under these peculiar circumstances. I have, therefore, no hesitation in concluding that, in the present state of our knowledge, contractility ought to be regarded as the unknown cause of known effects, a quality attached to a particular species of matter possessed of properties peculiar to itself, and which we are not able to refer to any general principle.

CHAP. IV.

Of the Nervous System.

In the last chapter I gave an account of one of the appropriate powers of the animal body, contractility, and of the organs by which it is exercised; I now proceed to the other property that belongs exclusively to animal life, sensibility.[5] As contractility is always

[5] One of the objections that were urged above against the use of the word irritability to designate the appropriate power of the muscles, applies to sensibility as expressing the power attached to the nervous system, that besides its technical and physiological sense, it is frequently employed in a general way to indicate a peculiar state of the feelings or character. Were I to venture to introduce a new term, I should propose that of *sensitivity*, which might bear the same relation to the nervous system, that contractility does to the muscular. M. Cuvier, in his report on the experiments of M. Flourens, which will be more particularly noticed hereafter, remarks upon the ambiguity which, in the French language, attaches to the word "sensible;" this term being applied to a body capable either of receiving, of producing, or of conducting sensations. In English, part of the difficulty may be removed by employing the word "sentient" in the first, and "sensitive" in the third of these cases; but we have still a fourth, and that the most ordinary use of the word sensible, as expressing the state of the intellectual powers. This paper contains many valuable observations upon the importance of a correct nomenclature in the science of physiology. As an illustration of the inaccurate or indeterminate mode in which the terms connected with this subject are employed, I may remark that Helvetius supposes the mind, "la faculté de penser," to be entirely composed of "sensibilité phy-

attached to the muscular fibre, so sensibility is always attached to the nervous system; it is found in no other part; and wherever the nervous matter can be traced, sensibility, in a greater or less degree, may always be detected.

In treating upon this subject I must begin by a brief description of the nervous system, and its anatomical structure, together with its physical and chemical properties. I shall, in the second place, consider its vital powers or faculties, and the mode of their operation, and this will naturally lead me to make some observations upon the use of the nervous system. I shall next inquire into the nature of the connexion that subsists between the muscles and the nerves, and I shall endeavour to distinguish between their effects upon the animal œconomy in general, and upon the different parts of which it is composed. When we have thus taken a view of both the powers which characterize animal existence, we shall be prepared for forming a classification of the functions, ascertaining in what degree they depend upon the muscular or the nervous systems, and what is the nature of the relation that they bear to each other.[6]

sique" and memory; De l'Esprit, dis. 1, ch. 1; while Richerand conceives that there may be sensibility independent of the presence of nerves; Physiol. Inst. p. 22.

[6] Willis may be considered as the first among the moderns who investigated with much success the structure and functions of the nervous system. Boerhaave paid comparatively but little attention to it: Haller again studied it with much minuteness; and since his time we may select the names of Vic-d'Azyr, Sœmmering, the Wenzells, and Drs. Gall and Spurzheim, whose labours on

§ 1. *Description of the Nervous System.*

The nervous system consists of four principal parts or organs; the brain, the spinal cord, the nerves, and the ganglia.[7] The brain is a body of a pulpy consistence, resembling a soft coagulum, filling the hollow bone called the skull, which gives the form to the head. It is of an extremely irregular figure, having a number of projections and depressions, corresponding partly to the irregularities of the skull, and partly produced by convolutions and cavities in the brain itself. It is covered externally with various membranes, of which the most important are two, an external one, thick and dense, which lines the skull, and an internal one, more thin and delicate, which closely invests the central mass, and follows its surface into all its depressions and cavities. To these the older anatomists give the names of the Dura and Pia Mater, in conformity to a whimsical hypothesis, that these two were the origin of all the other membranes of the body. The internal cavities of the

this subject have been peculiarly valuable. The late investigations of Dr. W. Philip, of Mr. C. Bell, and of M. Magendie, fully entitle them to a place in the above list. An interesting, and, so far as I am able to judge, a very accurate abstract of the successive discoveries that have been made on the subject of the brain and nerves is given by Sprengel, Hist. de la Med. t. iv. sect. 12, c. 4.

[7] It may be proper to remark, that whenever the terms "nervous system," "nervous matter," "nervous power," &c. are employed, without any addition or restriction, they are to be understood in a general sense, as including or referring to all the four parts mentioned above.

brain, which are called ventricles, are lined with a serous membrane, secreting an albuminous fluid, which, in the healthy state of the organ, is removed by absorption as rapidly as it is produced, but which is occasionally collected in considerable quantity, giving rise to the formidable disease of hydrocephalus.

Scarcely any thing is known with respect to the use of the different projections and depressions of the brain; but there are two points connected with its form, that it is important to notice, as they seem to throw some light upon its physiology and the nature of its functions. These are, first, the division of the encephalon or cerebral mass, into the cerebrum, or brain properly so called, and the cerebellum, or lesser brain; and secondly, its division into the two hemispheres. The greater part of the nervous matter within the skull composes the proper brain or cerebrum; it occupies the whole of the upper part of the head, and is separated by a dense membrane from the lesser brain, except at the common basis of both, where they are united. There is a dense membrane, projecting directly downwards to a considerable depth, from the upper part of the skull, and extending from the fore to the back part of the head, which divides the brain into the two hemispheres as they have been called; the cerebellum is likewise divided by a similar membrane into two hemispheres.

When we cut into the interior of the brain we find it to be composed of two substances, that differ in their colour and consistence; these have obtained the names of the cortical or cineritious, and the medullary matter. The cortical, as its name imports, is on the outside, and is of a reddish-brown colour; it is obviously of a softer consistence than the medullary part, and it leaves by desiccation a smaller quantity of solid residuum. In the fœtus it is considerably less firm, and at this period it bears a larger proportion to the medullary matter than it does in the adult. It evidently contains a greater number of blood-vessels; and more may be brought into view, when it is examined by the microscope. On this account it was conceived by Ruysch to be composed entirely of blood-vessels, with the connecting cellular membrane, an opinion which was at one time very generally adopted, and to which Haller inclines,[8] although the mere inspection of the part would seem to prove its inaccuracy. Malpighi supposed that he had detected a glandular structure in this portion of the brain,[9] an idea which was embraced by many eminent anatomists, and which may be thought to receive some confirmation from the microscopical observations that have been lately made upon this organ.[1]

[8] El. Phys. t. x. 1, 12.

[9] Exer. de Cerebro, in Mangeti Bib. Anat. vol. ii. p. 56.

[1] The medullary matter, both from its aspect and relative position, is generally considered as constituting the nervous substance in its most perfect state; and Drs. Gall and Spurzheim have conjectured that the use of the cineritious is to form or secrete the medullary part. Recherches sur le Systeme Nerveux, § 2. The particular facts from which they derive their hypothesis are, that the nerves appear to be enlarged when they pass through a

To the base of the skull, connected with the brain by the intervention of the medulla oblongata, is attached what has been commonly called the spinal marrow, but which has been more correctly termed by Gordon the spinal cord. Like the brain it is enclosed in membranes, it possesses both cineritious and medullary matter, although their respective position is reversed; it has a longitudinal furrow, dividing it imperfectly into two halves, analogous to the hemispheres of the brain.

To the lower part of the brain, or rather perhaps, according to the more accurate researches of the modern anatomists, to the medulla oblongata, are attached a number of small white cords, called nerves, composed of medullary matter, possessing a distinct fibrous structure, and enclosed in sheaths of membrane. These principally pass from the brain to the organs of the external senses, and bodies of a similar kind pass from the spinal cord to the muscular parts; the former have been called the cerebral, the latter the spinal or vertebral nerves; both of them are disposed in pairs, and proceed in corresponding

mass of cineritious matter, and that masses of this substance are deposited on all the parts of the spinal cord where it sends out nerves. Prof. Tiedemann, however, remarks, in opposition to the above opinion, that in the fœtus, the medulla is formed before the cortex, and he limits the use of the latter to the conveyance of the arterial blood which may be necessary to support the energy of the perfect nervous matter. Med. Repos. vol. xv. p. 315. See also Magendie, Physiol. t. i. p. 162.

directions to the two sides of the body. At their commencement from the brain or spinal cord, anatomists generally reckon nine pair of the former nerves and thirty of the latter, but they soon divide into numerous branches, which are distributed to all parts of the body. In their passage they frequently anastomose or communicate with each other, and these communications are sometimes so numerous and intricate as to form a complete net-work, to which the name of plexus has been applied. From these plexuses new nerves originate, which seem to be independent of those which produced them. When the nerves arrive at their ultimate destination, they generally ramify into small branches, which become more and more minute, until they seem at length to be melted down into a kind of pulp, and are no longer visible to the eye.[2]

[2] Besides these two classes of nerves, the cerebral and the spinal, there is one nerve, or set of nerves, that appears to hold an intermediate relation between the two, or to have a direct connexion with both the brain and the spinal cord; this is the intercostal nerve, with its ramifications. Some nervous twigs that descend from the brain unite with the branches that are sent off from the spinal cord; these form series of ganglia on each side of the spine, from which numerous nerves proceed that are distributed over all the thoracic and abdominal viscera. From the way in which the intercostal nerve is composed, it would seem adapted to combine the influence of all the parts of the nervous system, and to afford a supply of this influence to each individual organ, which, in this way, have a direct nervous communication, and it is from this circumstance that its popular name of sympathetic is derived. It must, however, be remarked, that both the anatomical and the physiological relation which this nerve bears to the

In speaking of the relation which subsists between the brain and the nerves, it has been usual to describe the latter as derived from the former, or as productions of its substance. This manner of viewing the subject probably arose, in some measure, from the hypothesis of the animal spirits, which were supposed to be lodged in a series of tubes, that served as a receptacle for them, and conveyed them to all parts of the body.[3] And even since the doctrine of the animal spirits has been called in question, the same kind of language is maintained, and the nerves are spoken of, in a vague way, as fibres actually continued from those of the medulla of the brain. Of late, however, the directly contrary opinion has been advanced by Drs. Gall and Spurzheim, that the brain is an appendage to the spinal cord, or that it is to be regarded as a kind of large tubercle or ganglion, connected with it, in the same way as other ganglia are connected with the nerves that are contiguous to them.[4] This view of the subject has been ingeniously defended by Prof. Tiedemann, by a reference to the progressive developement of the nervous system in the fœtus, in which we find that the spinal cord is formed before the brain, and also by the analogy of

other parts of the system have been the subject of much discussion. See Bichat sur la Vie et la Mort, p. 249 et seq.; also Beclard, p. 61, and Mr. Shaw's Strictures upon Bichat in Lond. Med. Journ. t. xlix. p. 456. Richerand has some good observations upon this part of the nervous system, Phys. t. i. p. 108.

3 Monro on the Nervous System, p. 24.

4 Recherches sur le Système Nerveux, sect. 1.

the inferior animals, where, as we pass on from the most perfect organization to that which is less so, the brain disappears before the spinal cord.[5] Perhaps this is more a verbal distinction than an actual difference in the conception of the object, for when anatomists speak of the nerves as being productions of the brain, they probably mean no more than that the brain is the centre to which the affections of the nervous system are to be referred, employing the phrase rather in a physiological, than in an anatomical sense.

The ganglia are small knots or masses of nervous matter, which are situated along the course of the nerves, generally where two or three of them form an angle, and especially in the different parts of the thorax and abdomen. They are composed of a mixture of two substances, which appear analogous to the cineritious and medullary matter of the brain; they are of a redder colour and are more copiously supplied with arteries than the nerves; they are also of a firmer consistence, and are covered with a denser membrane. Anatomists are generally agreed that the nerves which proceed from a ganglion are larger than those which enter into it, as if, in their passage through it, they had received an additional quantity

[5] Medical Repos. v. xv. p. 310. From some later observations made by Tiedemann and Serres on the developement of the nervous system in the human fœtus, we learn that the spinal cord is the part which is first formed, afterwards the medulla oblongata, then the cerebellum, and lastly the cerebrum. Beclard, add. à Bichat, p. 44.

of matter.[6] With respect to their texture we are informed by Monro,[7] and the account which has been more lately given by Scarpa [8] is fundamentally the same, that the filaments of the different nerves which compose the ganglion proceed individually without interruption, but that they are all twisted together into an irregular bundle, and that filaments from different nerves are united in the formation of a *new* nerve. In this way it would appear that a mechanical connexion is established between the parts that receive their nerves from the ganglia, and we may presume that this will contribute to a sympathy between their actions.[9]

With respect to the distribution of the nerves it may be remarked, that the greatest part of the nervous matter is sent to the organs of sense and of voluntary motion, that the viscera are much more sparingly supplied with nerves, the glands have still fewer, while some of the membranous parts appear to be entirely without them.[1] Generally speaking the nerves which supply the organs of sense seem to proceed immediately from the base of the brain, or rather from the medulla oblongata, while the muscles receive their nerves from the spinal cord; but there are some ex-

6 Haller, El. Phys. x. 6, 11. Sœmmering, Corp. Hum. Fab. t. iv. § 157.

7 On the Nervous System, c. 19.

8 De Nervorum Gangliis, § 6, 7, et alibi, tab. 2.

9 We have a very full abstract of all that refers to the ganglia in Johnstone's "Essay," with a copious list of references.

1 Haller, El. Phys. x. 6, 9. Sœmmering, Corp. Hum. Fab. t. iv. § 131. Blumenbach's Inst. Phys. § 210.

ceptions to this rule. There is much more irregularity with respect to the course of the nerves that go to the viscera; they generally take their immediate origin from some of the ganglia and plexuses that form part of the intercostal system, and they are connected with each other in a great variety of ways, apparently for the purpose of producing a direct nervous communication between all the viscera, as well as between each viscus and the other parts of the body. It is worthy of notice that the nervous system generally, including the brain, the spinal cord, and all their ramifications, is so disposed, that if the body be divided into two lateral halves, by a plane passing perpendicularly through its centre, the nerves of the two parts will be almost exactly similar to each other, while, at the same time, they are so united by plexuses and anastomoses of various kinds, as to ensure a complete connexion between the two parts and an entire correspondence of their sensations.

A circumstance connected with the anatomical structure of the brain that deserves to be noticed is the great quantity of blood which is transmitted to it by the arteries. Haller made a calculation, from which he concluded, that one-fifth of all the blood sent out of the left ventricle of the heart is carried to the head, although the weight of the brain in the human subject be not more than one-fortieth of that of the whole body.[2] This estimate has been thought to be too large, but even if we reduce the quantity of blood

[2] El. Phys. x. 5, 20.

to one-tenth, according to the idea of Monro,[3] it will be a very great over-proportion. There are many curious contrivances, connected with the circulation through the head, for preventing this great quantity of blood from producing any injurious effects upon the brain by its pressure or its unequal distribution, in consequence either of its stagnating in the vessels, or being too violently propelled through them, but the description of these is rather the province of the anatomist than the physiologist. Many conjectures have been formed respecting the use of this great quantity of blood, and it gives a degree of plausibility to an opinion, which was entertained by Hippocrates,[4] that the brain has some analogy to a secreting organ. It has been conceived that one use of the ventricles, as well as of the various internal convolutions of the brain, is to afford a more extended surface, by which the blood-vessels may enter its substance at a greater number of points, and consequently, in smaller quantity at any one part, while, at the same time, they are more firmly supported in their passage by the greater quantity of investing membrane.[5]

Both the chemical and the physical properties of the nervous matter are obviously peculiar to itself, unlike what we meet with in any other of the con-

3 On the Nervous System, p. 3.

4 De Glandulis, Opera, t. i. p. 272, l. 17.

5 The conjecture of Sir Everard Home is not without plausibility, that the fluid which the ventricles contain, varying in its quantity, may serve to equalize internal pressure. Phil. Trans. for 1814; p. 471, and for 1821, p. 32.

stituents of the body, but wherever it is found it exhibits nearly the same properties. Its general appearance is too well known to require any description, but there is one circumstance which has lately been the subject of much discussion, how far it is to be considered as being composed of proper fibres. It is generally agreed that the medullary part of the brain, when examined in its most perfect and recent state, especially after it has been artificially hardened or condensed by the action of heat or certain chemical substances, if it be carefully scraped with a blunt instrument, exhibits the appearance of fibres of considerable magnitude, with furrows between them.[6] These furrows or striæ are, for the most part, placed in such a direction as to converge towards the base of the brain, and it has been a question, whether these fibres merely unite, forming what are termed commissures, or whether they actually cross each other, and pass on to the opposite sides of the body. That this decussation takes place with respect to some at least of the fibres, near the union of the cerebrum and cerebellum, is an opinion of ancient date,[7] and has

[6] Haller, El. Phys. x. 1, 13. Cullen's Phys. § 29. The fibrous structure of the brain was the foundation of a great part of Descartes' hypothetical opinions respecting the animal spirits.

[7] For the opinions that have been entertained on this point from the time of Aretæus to the present day, see Sœmmering de bas. Enceph., p. 19; also Dr. Cooke's elaborate work on Nervous Diseases, v. ii. p. 109. It is remarkable that the question concerning the decussation of the fibres of the optic nerves appears still to be undecided, although it is a part which, from its

occasionally been announced in modern times, as the direct result of anatomical observation; but for its full establishment and clear demonstration we may consider ourselves as indebted to Drs. Gall and Spurzheim.[8] Still, however, the quantity of fibres which can be seen to decussate, is so small compared to the whole mass of cerebral matter, as to leave

size and situation, might have been supposed peculiarly favourable for the purpose. See Vicq. d'Azyr, p. 51, pl. 17, fig. 1, No. 32.

In the number of the Lond. Med. and Phys. Journ. which is just published, for Dec. 1823, we have a brief account of some experiments on this subject by Treviranus. Upon examining the optic nerves, after their consistence has been hardened by the action of alcohol, he finds that a part of the fibres pass on from their origin to the retina of the same side, while those of the interior and lower portion of the nerves appear to unite together, but it could not be perceived that any of them actually crossed. It may, perhaps, not be going too far to infer that the same structure may exist in the central parts of the brain, and that the greatest part of the fibres of this organ may simply unite or be brought into close apposition. I ought to apologize for offering an opinion upon an anatomical question, but I had the good fortune to be present at some examinations of the brain which were made by Dr. Spurzheim, and the impression produced upon my mind was that comparatively few of the fibres could be actually seen to decussate: it is impossible to speak too highly of the dexterity of his operations.

[8] See especially their sixth sect. and the observations made upon it by the members of the Institute, who were selected to report on their memoir; from this, and from various papers which were published in consequence of the controversy that ensued on the originality of the observations of Drs. Gall and Spurzheim, we may conclude that the connecting fibres had been occasionally observed by anatomists, but that the circumstance had been little attended to, and was not an opinion at that time generally received.

some doubt whether it be sufficient to explain all the pathological consequences that have been deduced from it. I allude to the well-known fact, that an injury inflicted on one side of the brain exhibits its effects on the opposite side of the body, proving, at all events, the transmission of the nervous influence in this particular direction, whatever may be the physical structure of the organ. The spinal cord, as well as the brain, possesses a fibrous texture, although, as it would appear, less distinct than the brain, and it differs from the brain in the effects resulting from disease or injury, which are generally observed to produce paralysis on the same side of the body with that on which the injury has been inflicted.[9]

Of late years we have had many microscopical observations on the minute structure of the brain. Prochaska, by employing a powerful lens, found it to be composed of a pulp, containing a number of small globules or rounded particles; the pulp itself appeared to consist of flocculi, likewise formed of globules, connected together by fine cellular substance, the ultimate globules being of a tolerably firm consistence, and about eight times less than the red particles of the blood.[1] These observations, in their more essential parts, have been confirmed by the still more

[9] See Monro on the Nervous System, c. 9; also Yelloly, in Med. Chir. Trans. t. i. p. 187, et seq.; this valuable paper evinces the uncertainty which prevails even among the first anatomists respecting a matter of fact apparently of easy determination.

[1] Op. Min. t. i. p. 342.

recent and elaborate examination of the Wenzels, who, by using higher magnifiers, detected more clearly the constitution of the brain, as composed of a series of these small globules, which were apparently of a cellular texture, and which constituted the whole solid mass of the organ.[2] It may seem remarkable that neither Prochaska nor the Wenzels could perceive any specific difference between the minute structure of the medullary and the cineritious matter, as we can scarcely doubt that the latter is more vascular, and it may be inferred to be so from the observations of Mr. Bauer, who confirms the existence of the globules, and remarks that they are disposed in lines, so as to give the brain its fibrous appearance. We are further informed by him, that the diameter of the globules varies from $\frac{1}{2400}$ to $\frac{1}{4000}$ of an inch, the general size being $\frac{1}{3200}$; they are both larger and in greater proportion in the medullary, than in the cortical part of the brain.[3]

According to Sir Everard Home these globules are connected together by a peculiar gelatinous substance, which he conceives to act a very important part in the animal œconomy. He goes so far as to state that "there can be no doubt that the communication of sensation and volition, more or less, depends upon it;" he even regards it as the very essence of life, and, referring to Hunter's doctrine of the materia vitæ, he remarks, "this grand idea of Mr. Hunter's

[2] De Structura Cerebri, p. 24, et seq.

[3] Phil. Trans. for 1818, p. 176; and for 1821, p. 27, et seq.

M. Bauer, by his discovery of this transparent mucus, has realized."[4]

The fibrous structure of the nerves is said to be more obvious than that of the medulla of the brain. Monro[5] and Fontana[6] have examined the nerves with the microscope, and have described them as being composed of a number of longitudinal cylinders, connected together by cellular substance, which, like the muscular fibres, may be divided into portions that are more and more minute, until at length we arrive at the primitive or ultimate nervous filament. This, according to the latter of these authors, is about twelve times greater than the fleshy fibre, and may be easily distinguished from it by its texture, as well as by its size. It is of a waved or tortuous form, and is composed of a cylindrical canal, containing a viscid pulpy matter, evidently different from the substance of the canal itself. Monro describes the ultimate nervous filament as a brownish pulpy matter, surrounded by a number of white transparent bands,

[4] Phil. Trans. for 1821, p. 32, 33. It may appear not a little remarkable, that so zealous and intelligent a disciple of the Hunterian school, and one whose pursuits and acquirements so well qualify him for judging of its tenets, should have thus broached the most direct system of materialism that has been given to the world. I shall have occasion hereafter to state the arguments which have induced me to adopt the immaterial hypothesis, but the example and authority of Sir Everard Home should certainly operate as a strong motive with those who embrace this view of the subject for exercising perfect candour towards their opponents.

[5] On the Nervous System, c. 13, and Tab. 13, fig. 1—14.

[6] Sur les Poisons, t. ii. p. 18, et seq. pl. 3 et 4.

but it would seem that this appearance of bands was merely an optical deception, produced by the effect of light acting upon the waved surface of the cylinder.[7] In speaking of the shape of the nerves I have employed the term cylindrical, in conformity with the account which is usually given of them, but we are informed by Sœmmering, who must be regarded as one of the highest authorities, that they are of a conical form, the apex being at the part where they are sent off from the brain, or that they gradually increase in diameter as they proceed from their origin to the organs for which they are destined.[8] With respect to the general structure of the nerves it may be further remarked, that the intercostal nerve and the par vagum are said to differ from the other nerves in the disposition of their fibres, which, instead of being straight and parallel, are irregularly connected to each other and twisted together.[9]

The ultimate nervous fibre, as described by Fontana, is however very much smaller than the fibres that seem to compose the substance of the brain, when we scrape it with a blunt instrument; so that if we are to believe in the reality of both these formations, and to suppose that the minute structure of the brain is similar to that of the nerves, we must conjecture that the visible striæ or fibres of the brain are analogous to the lacerti or larger masses that enter into the composition of the muscles, and that,

[7] On the Nervous System, c. 13 and 22; See Sœmmering, § 138.

[8] Corp. Hum. Fab. t. iv. § 144.

[9] Wilson's Lectures on the Skeleton, p. 7.

were their consistence sufficiently solid, they might be resolved into smaller primitive fibres, like those that are found in the muscles. This view of the subject seems to receive some confirmation from the observations of Reill on the nerves, which, as they are among the latest, so likewise are probably to be regarded as among the best that we possess upon the subject. He describes these bodies as composed of very fine filaments, that seem to differ in thickness, from that of a hair to the finest fibre of silk. These filaments are each of them enclosed in a delicate sheath, call neurilema, and in their course down the nerve they divide, subdivide, and unite again, in the most varied manner, producing a perfect connexion among themselves in every part. A number of these filaments forms a larger bundle or fasciculus, which is always enclosed in its sheath or neurilema, and these fasciculi divide and unite in the same way with the primitive filaments. Most of the nerves consist of several of these fasciculi, although there are nerves which contain only a single one, and perhaps some of the smallest consist only of an individual filament. The different filaments, as well as the fasciculi, are tied together by the substance which forms their sheaths, and the same body seems to compose the general sheath or covering of the whole nerve, presenting altogether a structure which is considerably analogous to that of the muscle.[1]

[1] De Structura Nervorum, c. 1—4, and plates. Mr. Mayo has conferred an obligation upon the student of anatomy, by presenting him with a translation or abstract of many of Reill's

For our knowledge of the chemical composition of nervous matter we are indebted, in the first instance,[2] to Thouret[3] and Fourcroy,[4] who gave us some important information respecting it, and what they left imperfect has been more lately supplied by Vauquelin.[5] The general result of these experiments is

treatises, with the accompanying plates. These, which are in some measure to be regarded in the light of diagrams or plans of the brain, are characteristic and expressive, but they appear to me to exaggerate the fibrous structure of the parts, even after they have undergone the action of the chemical re-agents by which their substance is hardened, and their natural divisions rendered more distinct. In speaking of the anatomical structure of the nerves it may be proper to advert to the curious experiments of Dr. Haighton on the re-production or reparation of nerves. Phil. Trans. for 1795, p. 190 et seq. It appears from them that after a nerve has been completely divided, and its functions totally suspended, it gradually resumes its powers, and the ends are found to be connected by the formation of a new substance. We should not previously have suspected that a part possessed of such delicate functions could have been so easily restored, or that the newly formed portion, which is obviously different from the other parts of the nerve, would have proved adequate to perform the office of the organ in its original state.

[2] It will be amusing, and may not be altogether uninstructive, to the student of animal chemistry to peruse Lemery's account of the chemical analysis of the brain, written about the commencement of the last century: Course of Chemistry, p. 506....10. Lemery was an intelligent and industrious experimentalist, to whom the science lies under considerable obligations.

[3] Journ. de Physique, t. xxxviii. p. 334. Thouret particularly pointed out the circumstance of the little comparative tendency of cerebral matter to undergo decomposition, p. 329.

[4] Ann. Chim. t. xvi. p. 282.

[5] Ann. Chim. t. lxxxi. p. 37.

that the medullary matter is a peculiar chemical compound, unlike any other of the constituents of the body; that in some respects it resembles a saponaceous substance, being miscible with water, and forming with it an emulsion, which remains for a long time without being decomposed. Fourcroy was not able to procure any proper oil from the cerebral substance, nor to obtain any decisive indication of the presence of oil as entering into its composition, but Vauquelin has found in it two species of adipose, or rather adipocerous matter, which are soluble in alcohol; likewise the peculiar animal principle which is called osmazome, together with a quantity of albumen, a small quantity of phosphorus, and some salts. The albuminous matter is capable of being partially coagulated both by heat and by acids, but either it is in a state of combination, which gives it specific properties, or it is an essentially different kind of albumen from that which exists in the blood. The action of nitric acid upon brain is considerably different from that which it exerts upon most other animal substances, in disengaging from it little or no azote; the gases which it evolves being principally nitrous and carbonic acids. If brain be gradually heated, a great proportion of its weight, especially of the cineritious part, is evaporated in the form of water, so that the solid matter which is left amounts to no more than about one-fourth of the whole; this forms a half-solid friable mass, which may be again reduced to an emulsion by the addition of water. If alcohol be digested upon dried brain, a part of it is converted into a substance very like spermaceti, but it does not appear in what way this change is effected. Pure potash dissolves brain, and disengages from it a large quantity of ammonia, and ammonia is also evolved from the solution of brain in nitric acid, when it is much concentrated. A quantity of carbonaceous matter is left in these processes, and the same takes place from the action of sulphuric acid. Brain is found to contain a quantity of saline matter, which, however, seems to be less than in many other of the components of the body; it consists principally of the phosphates of lime, soda, and ammonia.

§ 2. *Vital Powers or Faculties of the Nervous System, and the Mode of their Operation.*

When we consider the brain and nerves, as forming a part of the living system, our first inquiry must be, what properties they possess in common with the other organs of the body, and what powers they have that are peculiar to themselves. The answer to this question may be anticipated from what has been already stated; of the two specific powers that distinguish living from dead matter, spontaneous motion and sensation, the first is confined entirely to the muscles, while the latter is equally confined to the brain and nerves. When a nerve is acted upon in such a manner as that its appropriate power is excited, motion is not necessarily produced, nor any other visible change but the animal feels. On the other hand, there are many cases in which motion is produced that is unattended with sensation; of this kind are most of the minute operations that compose the internal functions, of which, in a state of health, we are perfectly unconscious, and which are only known to us by their effects.[6] These two powers, therefore, motion and sensation, although in a great number of instances they are connected together, being reciprocally the cause of each other, are not, however, necessarily connected, either of them may exist separately, and when they are connected it is not in any regular proportion. We conclude, therefore, that it is the office of the nervous system to produce sensation, but the way in which this is accomplished, or the succession of changes by which it is immediately preceded, we shall find it extremely difficult to ascertain. With respect to the relation which the different parts bear to each other, it has been generally supposed that the brain is the centre of the nervous system, or that part to which all the others are subservient, and that the nerves receive impressions from external objects

[6] The separation of the two vital powers is well exemplified in the very interesting experiments of Mr. Brodie on the action of poisons on the animal system. Phil. Trans. for 1811, p. 178, et seq. We learn from them that certain substances have the effect of destroying the sensibility and the power over the voluntary muscles, while the action of the heart and of the organic functions appears to be affected only, as it were, in an indirect manner. The same distinction is still more amply and extensively established by the experiments of Dr. Philip, to which frequent reference will be made in the subsequent parts of this work. A similar conclusion may be drawn from an experiment of M. Magendie's on the effect of prussic acid. Quart. Journ. v. iv. p. 350.

and transmit these impressions to the brain, where they become sensible to the mind, constituting perceptions.[7] This view of the subject is, in the main,

[7] I have ventured to employ the terms "sensation" and "perception" in a sense somewhat different from their ordinary acceptation, but by so doing it appears to me that we avoid part of the obscurity which attaches to the subject. Sensation is generally used to express the effect produced on the sensorium by an impression transmitted to it by a nerve, whereas I think it will be found more convenient to extend it to all the actions of the nervous system. It will, therefore, include both the organic and animal sensibility of Bichat, and the nervous and sensorial powers of Dr. Philip, while perception, as I would propose to use the term, constitutes a mode or species of sensation, and is the result of the latter of these only, corresponding, to a certain extent, with Bichat's animal sensibility, and more nearly with Dr. Philip's sensorial power. Quart. Journ. v. xiii. p. 97. The "sentiment" of Magendie and some other French writers, is nearly synonymous with "perception." Physiol. t. i. p. 142; and Bichat uses the word "tact" in nearly the same sense. Sur la Vie, &c. p. 83. Legallois, however, employs the word "sentiment" as correlative to "mouvement," expressing nervous action generally, p. 2, et alibi. The circumstance of applying the same term to a different faculty from that to which it had been appropriated by physiologists of so much eminence as Bichat and Philip, is of itself an objection to my nomenclature, but I know of no other method of expressing my meaning clearly, unless by the invention of some new term, to which I feel a still stronger objection. Locke, Essay, b. ii. ch. 1, § 3, and the other modern metaphysicians, so far as I am acquainted with their works, make sensation a mode of perception, the difference between the terms referring rather to some difference in the degree in which the understanding is affected, than in the part of the nervous system which is called into action.

correct, although the experiments and discoveries of the modern anatomists have led to some modifications which must be noticed.

All questions respecting the action of the nervous system are involved in much obscurity, which, in some measure, attaches to the nature of the subject. Although we have found that there are many difficulties connected with the complete understanding of muscular contraction, yet we may form a plausible conjecture concerning its mode of action, and can distinctly trace its operation from its commencement in the fibre to its effect on the part that is moved. We do not indeed see how the action of the stimuli that are applied should cause the particles of the fibre to approximate, but we can clearly see the connexion between the approximation of the particles and the consequences that ensue. In the nervous system, however, we have no phenomena of this kind to guide our reasoning, and although we can prove that the nerves are the media by which external impressions are conveyed to the brain, we are totally at a loss to account for the manner in which the conveyance is managed.

In proportion to the deficiency of our knowledge upon any topic, so is generally the obscurity of our language, and the terms which we employ, when speaking of the nervous system and its actions, being originally metaphorical, and being used in different senses on different occasions, increase the difficulty of obtaining accurate ideas upon the subject. The word sensibility, which is employed by physiologists to express the peculiar power of the nervous system, is applied in common language to a certain state of the mind or character, so that before we employ it in scientific discussions we must begin by discarding our accustomed associations.[8] Physiological sensibility may be defined, the power which the nervous system possesses of receiving and transmitting certain impressions, and producing corresponding changes in the sensorium, but it is essential to notice that these two operations are not necessarily connected together, or that it is no necessary part of this sensibility for these impressions to be perceived by the mind, or to become perceptions.[9] In what particular cases these

[8] See note in p. 219.

[9] Although the existence of sensation without perception has been admitted by various physiologists, especially by Cullen, Whytt, and Bichat, the subject has generally been rendered somewhat obscure, either in consequence of the hypotheses that have been connected with it, or the terms that have been employed. Cullen uses the word "sensation" in a sense considerably different from the one proposed above, but, it must be admitted, that on this, as on every other occasion, he expresses himself with great perspicuity. Institutions, § 32, 36, et alibi. Bichat has involved the subject in his complicated doctrine of the two vital principles, one serving for organic, the other for animal life. Scarpa correctly defines "simple sensation" to be nervous action which is not attended with consciousness. Tab. Neur. § 21. The subject is clearly stated by Dr. Park; he, however, substitutes the term "reflexion" for "perception;" but I prefer the latter as being less metaphorical. Quart. Journ. v. i. p. 155, et seq. Richerand correctly divides sensibility into perceptibility, and into sensation without perception; but he afterwards describes this latter too vaguely, as being common to every thing that has life, and being diffused through both animals and vegetables. El. Physiol. by De Lys,

powers are exercised separately, or where nervous action is not succeeded by perception, will be considered hereafter, but the possibility of the occurrence is generally admitted.[1]

Assuming it, therefore, as an established fact, that the brain and nerves are the primary seat of sensibility, we must inquire into the mode by which this faculty operates. The operation we shall find to be of two kinds; the first depending upon the action of external bodies on the nervous system, the second upon the re-action of the nervous system itself on some of the corporeal organs.[2] The body is furnished

p. 27. The existence of nervous action without perception is one of the points which Mr. C. Bell establishes as marking the difference between his divisions of the nerves. Phil. Trans. for 1821.

[1] If a new term be thought necessary to express the power which certain parts of the nervous system possess of exciting perceptions, the analogy of our language would suggest *perceptivity;* but I have not ventured to introduce either this term or *sensitivity* into the text. M. Richerand has employed the word "perceptibilité" in the same sense. El. Physiol. t. i. p. 44.

[2] In an ingenious experimental essay by M. Fleurens, of which we have an analysis by Cuvier, Ann. Chim. et Phys. t. xx. the author appears to regard these rather as two distinct powers of the nervous system, than as different modes of the operation of the same power; and probably this opinion will be found to be correct. But until we have a larger body of facts, or some of a more decisive nature, it must be considered rather as a verbal distinction, than as one derived from an accurate knowledge of nervous action, and which affords us any new ideas upon the subject. We may, however, observe that the recent investigations of Mr. C. Bell lead us to a conclusion very similar to that of M. Fleurens, as we find that different nerves, or at least different nervous filaments, are concerned in these operations. A remarkable instance of the degree

with certain instruments, denominated organs of sense, consisting essentially of two parts, a peculiar conformation of an organized substance, which is specifically adapted to receive and modify certain impressions, and a quantity of nervous matter suitably disposed for the reception of the impressions after they have been thus modified. The nervous matter that belongs to the organs of sense is connected by nerves to the brain, and these nerves possess the power of conveying the impressions along their course to this organ, where they produce perceptions. In this operation there are three distinct stages, the original impression on the sentient nervous extremities,[3] the transmission of the sensation along the trunk of the nerve, and the reception of it by the brain; and it may be laid down as a point, proved by the most ample deduction of facts, that an external impression cannot be perceived by the mind, without going through the successive steps of this process.

One of the most important of the external senses is the touch; it is extended over a great part of the surface of the body, but its most delicate seat is the points of the fingers. When a substance presses upon the finger, some peculiar change is induced upon the expansion of nervous matter, which is connected

in which the judgment is perverted by preconceived hypothesis occurs in the writings of Baglivi, a physiologist of genius and originality, who supports the doctrine, that the proper sensibility of the nerves resides in their membranous coats. De Fibra Motrice Spec. lib. i. cap. 5, corol. 4.

[3] Cullen's Physiol. § 29, 30.

with the cutis; a certain effect is immediately propagated along the nerves that lead from the hand to the brain, and a third change is then produced in the brain itself. That these three successive changes are all concerned in the operation is proved by daily experience, in which we find that if either the organ itself be injured, the nerve be interrupted in its course, or the brain be in any way deranged, the proper effect does not follow from the application of the impression. The example of the eye, another of the organs of sense, may be adduced, as affording a still clearer conception of the subject, the impressions of sight being of a more distinct and specific kind than those derived from the touch. The eye is an optical instrument, consisting of a lens, which is adapted for receiving the rays of light, and bringing them into a proper state for forming an impression on the retina, an expansion of nervous matter, that is situated at its posterior part. The action of the lens upon the rays of light is entirely mechanical, and differs in no respect from the effect that is produced upon them by a transparent substance of the same shape and density. The nervous expansion at the back of the eye is connected with the optic nerve, and this communicates directly with the under part of the brain. Now it is found as necessary for vision that the nerve should be in a perfect state as the eye itself, and we always find, that although both the eye and the nerve be perfect, if the brain be diseased, the correct perception of sight is not excited. We cannot, indeed, perform direct experiments upon those organs of the external senses that are situated in the immediate vicinity of the brain, as the eye and the ear, in consequence of the short course of their nerves, and the impossibility of coming into contact with them, without deranging parts immediately essential to life; but there are many pathological facts which prove the necessity for the entire state both of the organ of sense and the communicating nerve. Blindness and loss of hearing are as certainly produced by an affection of the optic and auditory nerves, or by any circumstance which prevents them from performing their accustomed actions, as by a disease of the eye and the ear itself, and without any physical derangement of the part, we have frequent examples, where mere pressure upon the nerves produces the same effect, and where, upon the removal of the pressure, the faculties of the organ are again restored.

The second mode in which the nervous system operates is by its re-action on some of the organs of the body, an operation which, with respect to the succession of events, is the reverse of the one which has been described above. Of the actions of this description one of the most important to our existence, and the most frequently exercised, is the faculty of voluntary motion. Here the affection originates in the brain, in which some change takes place; this is transmitted down the nerve into the muscle, where an effect is produced on the fibre, which causes it to contract, and in this, as in the former case, all the three stages are equally essential. I do not at present enter into any discussion concerning the nature of these changes, but propose merely to point out the order of their succession, and their dependence upon each other. Now, in this instance, in consequence of the space which intervenes between the parts where the action commences and terminates, we have the most ample means of observing the necessity of the integrity of the nerve as the medium of communication. If the nerve be divided in its course we may exert the volition, and produce the necessary change in the brain, but no motion will ensue in the muscle; at the same time our own feelings will not indicate to us that any thing has occurred out of the ordinary course of events, and we are only aware of the defect by finding ourselves unable to produce the desired contraction. And we have it in our power to prove that, in this case, the defect does not depend upon the morbid condition of the muscle, because if we irritate the nerve just below the point where it is divided, we find that the muscle will contract, in the same manner as if a similar kind of irritation had been applied to the nerve in its entire state. We may also extend the same kind of trial to the brain, for by irritating the upper part of the nerve, above its division, we shall have a sensation produced in the brain, similar to what would have followed the application of the same stimulus to the remote extremity of the nerve.

We perceive that the two modes in which the power of sensibility operates, so far as the order of the phenomena is concerned, are exactly the reverse of each other, but that the same parts are called into action, and are equally connected together.[4] We may then conclude that sensibility is the appropriate and exclusive faculty of the nervous system, and that it has two distinct modes of action, the one originating from external impressions, which are propagated from the extremities to the centre, the other depending upon a change in the brain itself, which proceeds in the contrary direction, from the centre of the nervous system to its extreme parts. Besides these physical functions of the nervous system, there are others, which either belong to it, or are, at least, always connected with it, of an intellectual or moral kind, which constitute the science of metaphysics; so far, however, as they are attached to the corporeal frame or affect its functions, they will be considered in a subsequent part of the work.

Having ascertained that the nervous system is the organ of sensibility, either as proceeding from external impressions carried along the nerves to the brain, or transmitted by them in the contrary direction, from the brain to the voluntary muscles, our next subject of inquiry must be, in what manner is this operation effected? The question may be thus stated in direct terms. When an impression made upon an organ of sense is transmitted by a nerve to the brain, or when the exercise of volition is communicated to the nerve, so as to produce the corresponding effect upon the

[4] This remark must be understood in a general sense only, as from some recent discoveries there is reason to conclude, that these two powers or operations are actually exercised by different portions of nervous matter.

muscle, what change does the nerve experience, or in what way is it acted upon, so as to admit of this transmission? Three hypotheses have been invented to account for this power of the nerves; the one which is the oldest, and has been the most generally received, is that the brain and nerves are provided with a certain fluid, called the animal spirits, which serve as the medium of communication between the different parts of the nervous system; the second supposes that this transmission is effected by means of the vibrations or oscillations of the particles of the nervous matter itself; while the third ascribes the action of the nerves to the operation of electricity.

The hypothesis of the animal spirits is popularly ascribed to Descartes, and he may, perhaps, be considered as the person who reduced it to a regular form, and contributed, by his authority, to its general reception, although traces of it may be found in the writings of Hippocrates.[5] The principal ground of this hypothesis seems to have been the idea that the brain is a secretory organ, an idea which was suggested by the great quantity of blood sent to it, and by some supposed resemblance in its structure to other secreting glands.[6] Yet, as nothing cognizable by the senses is produced by it, it was concluded that it must secrete something of a subtile or etherial nature, peculiarly suited to the performance of the functions which belong to the brain, and which are so unlike

[5] On this point it will be sufficient to refer to the learned work of Dr. Good, v. ii. p. 22, et seq.

[6] Descartes, Tractatus de Homine, § 14.

those of other material substances. It must be recollected, that about two centuries ago, every thing that could not be otherwise explained was referred to the agency of some kind of refined spirit, an idea which appears to have been originally derived from the alchemists, and, after being incorporated with the metaphysics of the age, gave rise to a long train of mysticism. Almost every philosopher of that period adopted more or less of these notions. Newton's ether is well known to have proved an abundant source of speculation to multitudes of those who called themselves his followers, and who seem unfortunately to have copied almost the only error which this great man committed. Upon this slender foundation was built the hypothesis of the nervous fluid, or the animal spirits, as they have been termed; yet their existence was assumed as an ascertained fact, and even their different affections and diseases were spoken of with as much confidence as if the authors had been treating upon something which was the immediate object of their senses, and with which they were perfectly familiar.[7] The doctrine of the

[7] Haller devotes no less than ten pages of his great work, El. Phys. x. 8, 11—16, to learned discussions respecting the nature of this imaginary agent, inquires whether it be albuminous, spirituous, acid, sulphureous, aëriform, or etherial, and concludes that it bears a resemblance to what has been termed the spiritus rector of plants, a substance nearly as little understood as the one which it is intended to illustrate. The respect which must always attach to whatever comes from Haller's pen prevents those reflections which we might be inclined to make on the occasion, and we are induced rather to lament the low state of physical science when

animal spirits has likewise become a subject of popular belief, and has given rise to a variety of expressions, that are every day employed in our common language. There does not, however, appear to be the least shadow of proof of their existence, either from experiment or observation; there is no analogy in their favour, the structure and physical properties of the nerves do not seem adapted to the office that has been assigned them; and, in short, the whole is an hypothesis entirely unfounded and quite gratuitous.[8]

The hypothesis of vibrations had been imperfectly stated by many of the earlier physiologists, but it was so much detailed and embellished by Hartley, as to be, by common consent, connected with his name.[9]

he wrote, than to impute to him any deficiency of judgment. Stuart, in his learned dissertation, on the structure and action of muscles, thus defines the nervous fluid; "tenuissimum, dulcissimum, mobilissimum, et minime cohœrens, aut coagulationi obnoxium sanguinis." C. v. § 13. Its existence is advocated by Sabatier, t. iii. p. 224, and Boyer, t. iii. p. 311. Plenk devotes a section to the description of its physical properties, many of which, however, it must be allowed are negative; Hydrologia, p. 49. It is indirectly admitted even by Cuvier, Regne Anim. t. i. p. 31, and more directly by Dr. Good, v. iii. p. 24—27.

[8] Does not the curious fact which has been established in the late controversy respecting the effect of dividing the eighth pair of nerves, that the nervous influence may be transmitted along a divided nerve, even when the parts are one-fourth of an inch asunder, afford a direct argument against the idea of this influence depending upon the passage of a subtile fluid? See Quart. Journ. v. xi. p. 325, and v. xii. p. 17.

[9] The hypothesis of vibrations was very explicitly laid down by N. Robinson, who published his treatise on the spleen some years before Hartley's "Observations" appeared; to the general

According to this doctrine the action of the nerves consists in a vibration of the particles of which they are composed, by which impressions are transmitted along them, and conveyed to and from the brain in perception and volition respectively.[1] This hypothe-

idea of the vibratory action of the nerves he also subjoins the additional speculation of the "machinulæ," which bear a close resemblance to the "vibratiuncles." New system of the spleen, &c. p. 1, c. 7. Some of the French metaphysicians, especially Condillac, preceded Hartley in supporting the doctrine of vibrations; Condillac's work on the senses was published about two years before Hartley's.

[1] Strictly speaking the hypothesis of vibrations should be subdivided into the opinions of those who suppose that the particles of the medullary matter itself are the agents, or that there is diffused or dispersed through them a subtile ether which acts the sole or the principal part. Hartley adopts the supposition of the intermediate action of the ether, p. 21, and by thus encumbering his hypothesis with this imaginary agent deprives it of its only recommendation, that of simplicity. If we could conceive how the ether can receive all the various modifications of external impressions, and transmit them to the nervous particles, there would be no difficulty in dispensing with the operations of the ether. Dr. Young's view of the subject coincides, in a considerable degree, with Hartley's, except that for the hypothetical ether he substitutes the electric fluid. See Med. Lit. p. 99, 100; and Lect. v. i. p. 740. Blumenbach more directly and explicitly admits the plausibility "of the doctrine of a nervous fluid, which is thrown into oscillatory vibrations by the action of stimulants;" and as if not satisfied with the two hypotheses, he argues in favour of the similarity of nervous action and the elective influence. Nor does he stop even here, but goes on to state that by the oscillations of this ether, Hartley "very ingeniously explains the association of ideas, and again, by the assistance of this, most of the functions of the animal faculties." Physiol.

sis has the advantage over that of the animal spirits, inasmuch as it does not assume the existence of any imaginary agent, but it may perhaps be questioned whether it possesses any other recommendation. We have no more direct evidence of the vibration of the nervous matter than of the fluid of the Cartesians, and we may remark that the general aspect and structure of the nerves appear perhaps less adapted to vibration than to secretion. The principal arguments that have been adduced in favour of the Hartleian hypothesis are certain facts, in which it seems that when an impression has been made upon an organ of sense, the effect is continued for some time after the impressing cause is removed, and that it gradually subsides in a way which was thought most analogous to a vibratory motion.[2] The facts to which I refer occur particularly with respect to the sense of sight, and will be detailed when I come

§ 226. It cannot but excite some surprise to observe the facility with which so eminent a physiologist and naturalist becomes involved in such an intricate tissue of unfounded speculations. His judicious and intelligent commentator, Dr. Elliotson, very candidly admits the futility of the whole train of deductions. See his note in loco. With respect to the hypothesis of vibrations I may remark, that the fact alluded to above, of the transmission of the nervous influence through the interval between the parts of a divided nerve, seems more decisive against this speculation than against that of a nervous fluid. The solution of continuity must certainly put an effectual barrier to the propagation of the vibratory or oscillatory action.

[2] Hartley on Man, c. i. § 1, prop. 3, et seq.; Belsham's Elements, c. iii. § 4.

to give an account of this faculty, and I shall defer to that part of the work the further consideration of the hypothesis of vibrations, as its merits will be better understood when we have made ourselves more fully acquainted with the nature of the external senses.

The electric hypothesis is of modern origin, and has indeed scarcely yet been brought forward in an arranged and methodical form. It principally rests upon various experiments that have been made by Dr. W. Philip, and other English physiologists, to elucidate the laws of the nervous system, in which it appeared that when a nerve was divided, so as entirely to intercept the transmission of its action, the place of the nerve might be supplied by a galvanic apparatus.[3] The further examination of this hypothesis must be

3 Valli's speculations on the action of the two metals upon the parts of living animals led him to assert the identity of electricity and of the nervous fluid. Journ. de Phys. t. xli. passim. The same opinion is, to a certain extent, countenanced by Dr. Young, Lect. v. i. p. 740, and was formed previously to the experiments of Dr. Philip. Mr. Abernethy goes still further; for he seems strongly inclined to regard some subtile fluid, analogous to electricity, not merely as the prime agent in sensation, but as even constituting the essence of life itself: singular as it may appear, we find this highly respectable and intelligent writer sliding into materialism, at the very time when he is directing the force of his genius against this doctrine. See Lectures on Hunter's Physiol. p. 26, 30, 35, 80, et alibi. It is scarcely necessary to observe that metaphysically speaking, the subtile or etherial agents that are called into aid in our explanation of the vital phenomena, are as truly *material* as the densest stone or metal.

likewise deferred, until we have had an opportunity of considering more minutely the nature of the facts on which it is supported, especially those connected with the functions of secretion and digestion.[4]

§ 3. *Use of the nervous System.*

The uses of the nervous system, the subject which we are next to consider, may be resolved into two; first, to maintain our connexion with the external world, by receiving external impressions and producing voluntary motions: and secondly, to unite the different parts of the animal frame into one whole.[5]

Although it is possible to conceive of a kind of

[4] Although the full consideration of this hypothesis is deferred until we come to that part of the work which treats more immediately of the facts from which it is derived, I may anticipate the discussion so far as to remark, that, before the electric hypothesis can be considered as proved, two points must be demonstrated; first, that *every* function of the nervous system may be performed by the substitution of electricity for the action of the nerves; and secondly, that *all* the nerves admit of this substitution. We must not rest satisfied with its apparent action upon the stomach, which is at best a dubious case, as far as the operation of the nerves is concerned; we must show that volition can be transmitted by the electric fluid, and that this fluid is equally capable of stimulating the nerves of the involuntary, as of the voluntary muscles.

[5] The researches of Mr. C. Bell lead to the conclusion that these two functions of the nervous system are exercised by different descriptions of nerves; the first by certain cerebral and spinal nerves, which pass directly from the brain or spinal cord to the organ which receives the impression; the second by those which pass from one organ to another, including what he terms the superadded system of nerves.

independent existence being carried on, at least for a limited space of time, in which the animal should be cut off from all surrounding objects, and in which the exercise of his functions should be confined to the simple continuance of life, this state of insulation could not be long maintained, and even while it lasted would be attended with the suspension of all those circumstances which render life desirable. We are, at every instant, receiving impressions from the objects which surround us, some of them for the immediate purpose of supplying our corporeal frame with the materials necessary for its support, and others acting more directly upon the mental faculties, and producing a species of re-action upon some of the organs of the body, by which we are led to accomplish those objects, which are scarcely less essential to our present state of existence, than what contributes to its immediate physical support. Now in consequence of its power of receiving and transmitting the impressions of external objects, the nervous system is the great apparatus by which these effects are accomplished.

Nor is the second use of the nervous system less important, that of uniting the various parts of the animal frame into one connected whole. The different functions which depend upon contractility, such as the circulation, respiration, and digestion, have all no doubt a necessary connexion with each other. The circulation could not be carried on unless the digestion produced the materials of which the blood is composed; respiration must cease unless the heart propelled the blood through the lungs; and digestion can only be performed by the blood being conveyed to the minute arteries of the stomach, after it has received its proper action in the lungs. But still, if we may use the expression, the dependance of these functions upon each other is a kind of mechanical dependance. We may conceive of a being that should have all these operations going forward, according to their respective laws, yet that there should be no consciousness of identity, and no connexion between these operations, except the physical relation which they bear to each other. If, for example, we could, by a mechanical operation, propel the blood along the arteries, and, at the same time, by artificial means, produce the alternate motions of the lungs, we might imagine it possible to procure a supply of arterial blood, which, when conveyed to the stomach, might act there so as to cause this organ to digest the substances contained in it, and to prepare from them a quantity of nutritive matter for the purpose of sanguification.

Probably the life of vegetables consists in this kind of physical connexion between the different functions, in which, by mechanical and chemical actions alone, a succession of changes takes place merely depending upon the physical operation of various external agents. This may be considered as nearly coinciding with the organic life of Bichat.[6] In the supposed case of the animal, as stated above, if we only conceive a force

[6] Sur la Vie et la Mort, p. 3.

to be applied, so as to set the fluids in motion, we might imagine the rest to be accomplished by the ordinary properties of matter, exercising its attractions and affinities, as the different substances are brought within the sphere of their mutual action. But still the being would be no more than a species of automaton, without homogeneity and destitute of consciousness. The nerves, on the contrary, pervade every part, and give to the whole set of organs and functions a necessary vital dependance upon each other, so as to bestow upon the animal the feeling of individuality, and to connect all its operations without any visible change in its structure and composition. A great part of the sciences of medicine and of pathology consists in tracing the operation of this nervous connexion between the different parts of the body, and observing the effects which are propagated to distant organs or functions by the affection of any single organ or function. And this connexion is not of that kind which we may denominate physical, where the change is extended to remote parts in consequence of an alteration in the mechanical or chemical constituents of the body, but it is to be referred to that sympathetic connexion between the parts, which can be accounted for upon no other principle but the operation of the nervous energy.

The information which we gain by investigating the anatomy of different kinds of animals, and comparing them with the human subject, confirms and illustrates this idea of the use of the nervous system. It is remarked by Blumenbach, that in the cold-blooded animals, where the size of the brain bears only a small proportion to that of the nerves proceeding from it, there is much less sympathy between the different organs and functions of the body, while, at the same time, each separate part possesses a greater share of individual vitality. This we see exemplified in the length of time during which life remains attached to their limbs when divided from the body, the power which some of them possess of re-producing parts that have been removed, and, as we descend lower in the scale of organization, the still more extraordinary power of being multiplied, like vegetables, by mechanical division.[7]

This view of the subject will serve, in a great measure, to answer a question, which was formerly the subject of much controversy among physiologists, whether there be, what has been termed, a sensorium commune, a part of the nervous system, from which volition originates, and to which all impressions are referred or conveyed, before they excite perceptions. The question has been proposed in another form, although essentially of the same import, whether, when an impression be made upon an organ of sense, as, for example, upon the eye, the perception exists in the eye or the brain? Is the last change which takes place, immediately previous to perception, an action of the nervous matter that is connected with the eye or of the brain itself? The general result of

[7] Blumenbach, Specim. Physiol. p. 20; Ebel, Observ. Neur. in Ludwig, Scrip. Neur. t. iii. p. 152.

our experience leads us to conclude that there is a common centre of perception, and that in the human species it exists exclusively in the brain.[8]

The proof of the existence of a common sensorium depends upon the facts that have been referred to above, where impressions made upon an organ of sense are not followed by a perception, provided the nervous communication between the organ and the brain be destroyed or injured. And the same conclusion seems to be confirmed by a series of facts the reverse of these, where, when an effect has been produced upon the brain, similar, as we may suppose, to one which had, on some former occasion, been transmitted to it from an organ of sense, it has excited the idea of an external impression, although the organ of sense may have been destroyed. This is the case with persons who, after having arrived at maturity, have had the eyes entirely destroyed, yet such individuals continue to dream of visible objects, and are able to recall visible ideas with perfect facility. It is partly also upon the principle of the actions of the brain producing effects similar to those that follow from impressions upon the extremities of the nerves, that we account for the mistaken perceptions that are experienced after the loss of a limb, which are fre-

[8] The word *brain* is here employed in its most extensive sense, to signify all the parts of the nervous system except the nerves and the spinal cord. We have some reason to suppose that the medulla oblongata is more immediately essential to certain nervous operations than either the cerebrum or cerebellum.

quently not to be distinguished from those that formerly existed in the part.[9]

In man, as has just been stated, the sensorium appears to be exclusively confined to the brain, but as we descend in the scale of beings, to those whose functions, and especially whose nervous functions, are less perfect, it would appear that the sensorium is more extended. In some of the amphibia we may conjecture that the spinal cord partakes with the brain in all its faculties, and, as we advance to animals that have a still simpler organization, the brain entirely disappears, and the spinal cord seems to be substituted in its place. There is, however, reason to doubt whether, in this case, the animal possesses any degree of what can properly be called perception, and whether the sole object of its nervous system may not be to convey impressions from one part to another, which are necessary for the functions of the animal, but which do not excite any ideas of consciousness.

The same kind of communication by means of nerves, which we have found to be necessary with respect to the brain, is equally so with respect to the spinal cord, which may be regarded as a common centre for the greatest part of the nerves that supply

[9] Porterfield on the Eye, v. i. p. 364. The peculiar feelings experienced by those who have lost a limb, which is described by the author as occurring in his own person, probably depends in part upon another cause, the comparison which they make between the sound and the mutilated extremity; an individual who was born without legs, or who had lost both of them in infancy, would never have these false perceptions.

the muscles of voluntary motion. When the spinal cord is compressed or divided in any part, the limbs that are supplied with nerves which branch off from it below the injury, are palsied. If the injury take place near the lower extremity of the spine, the lower limbs alone become insensible, and, as none of the functions essential to life are affected, the patient lives with all his faculties and powers unimpaired, except that of loco-motion. The nearer to the head the injury is situated, so much more extensive is the derangement of the different functions; and there are cases upon record, where, after a dislocation or fracture of some of the cervical vertebræ, all power has been lost over the voluntary muscles, and the functions of the abdominal and thoracic viscera have been nearly suspended, yet, for the short time that life was capable of being continued under these circumstances, the cerebral functions and the mental faculties have remained in a sound state.

Considering, therefore, the brain as the centre of perception, it necessarily follows, that an injury to this organ is attended with a diminution or loss of sensation to the whole system, although each of the organs of sense and motion may be individually in a sound state. This is proved by our daily experience of the effects of external violence upon the brain, and of various diseased states, either of the nervous matter itself, or of other bodies in its vicinity, such as tumours of the skull, thickening of its membranes, or effused fluids of any kind, pressing upon the surface of the brain or contained within its cavities.

One of the most frequent causes of the loss of nervous power is pressure, and this may take place without any permanent injury to the part compressed, for we frequently observe that when the pressure is removed the organ resumes its ordinary functions.[1]

A question has been asked respecting the organs of voluntary motion, which is analogous to the one that has been noticed above respecting the organs of sense. When we exercise our volition, and produce muscular contraction, is the first effect of volition some change in the brain itself, or does the will act immediately upon the muscles? The question may perhaps be regarded as merely a verbal one, or at least as involving more of a metaphysical than a physical inquiry, and it must be acknowledged that, like many others connected with the nervous system, it is one to which we are unable to give a decisive answer. Analogy is, however, strongly in favour of the opinion that some change ensues in the brain itself, and that this change is the cause of a subsequent change in the nerve, and this again of the change in the muscle. The division of the communicating nerve produces

[1] The case of the Parisian beggar, which has been brought forwards to explain the nature of sleep, Hartley on Man, v. i. p. 46, although it does not correctly apply to that state, is a good illustration of the effect of pressure in producing a temporary abolition of the nervous functions. To the same cause may probably be referred, in part at least, the coma which was observed to ensue in the experiments of M. Rolando upon deep-seated injuries of the brain, which must have been necessarily attended with a considerable effusion of blood in the interior of the organ. Magendie, Journ. de Physiol. t. iii. p. 95, et seq.

the same loss of voluntary power that it does of perception, and although the process be reversed, as to the order in which the parts are affected, each separate step in the process may be supposed to be equally essential in the one case as in the other.

Although this mode of reasoning has been generally adopted by the modern physiologists, a contrary doctrine was maintained by many of the writers of the last century, and especially by the Stahlians. It was conceived to be a necessary consequence of their hypothesis, respecting the connexion between the muscular and the nervous systems, that the soul, as they termed it, is co-existent with the different parts of the body, and is extended through all the organs of sense and the parts subservient to motion. The disciples of Stahl supposed the soul to act directly upon every part of the body, and to be immediately concerned in every vital function, whereas the opponents of this doctrine maintained that the soul acts only upon the brain, and is immediately concerned in the sensitive and intellectual functions alone.[2] I shall have occasion to state my objections to the Stahlian hypothesis hereafter, but I may remark in this place, that even were it to be admitted, the above consequence does not necessarily follow from it.

We have now, therefore, proceeded so far as to conclude that the brain is the common centre of the nervous system, to which all the impressions of external bodies on the extremities of the nerves are

[2] See Haller, El. Phys. x. 8, 24.

referred, and from which originate all the actions that are executed by the organs under the control of the will.[3] Physiologists, however, have not been satisfied with assigning the brain generally as the sensorium commune; they have been anxious to find out some particular portion of it which might be regarded as the more essential organ to which all the rest are subservient. The investigation is a curious one, and, although it may have been rendered ridiculous by the whimsical opinions to which it has given rise, it is in itself a legitimate object of inquiry. It may be regarded as essentially the same, although expressed in more correct language, with the discussion which occupied so much of the attention of the older metaphysicians and physiologists, respecting the seat of the soul, by which word, so far as they had any accurate notions upon the subject, they appear to have intended to express the organ of perception and volition, or rather that material organ to which these faculties are attached, or through which they operate. There are many circumstances with respect to the structure and organization of the brain, which have led to the supposition, not only that its various parts must each of them exercise some peculiar function, but that certain portions of it possess the specific

[3] Haller, El. Phys. x. 8, 23—25. The agency of the brain is here extended to every operation of the nervous system in which either perception or volition is concerned; in simple nervous action, as for example, in that which is carried on between the different abdominal viscera, we have no evidence that any parts are concerned except the nerves themselves.

powers of the nervous system in a much greater degree than others. The fibrous or striated appearance of the brain, which has been lately so much attended to by anatomists, seems also to lead to the same conclusion, as the uniform direction of these striæ and their regular disposition, converging to certain parts of the cerebral mass, would induce us to regard them as analogous to the fibres which compose the nerves, and intended to convey the nervous influence to some particular organ, which is more essentially or necessarily concerned in perception.

It would be an unprofitable waste of time to relate the various notions that have been entertained upon this subject, as they are, for the most part, purely hypothetical, and destitute even of a shadow of proof. It may be proper, however, to notice an opinion that prevailed at one time among some eminent physiologists, that the immediate seat of perception is not in the brain itself, but in the investing membrane, an opinion evidently connected with the mistaken hypothesis derived from the ancients, respecting the sensibility of membrane generally, to which I have already had occasion to refer, and which, as we shall afterwards find, was applied to many other parts of the animal economy. There is also another opinion on this subject that may be noticed, but certainly more in consequence of the celebrity of the name to which it is attached, and to the favourable reception which it experienced among men of science, than of its intrinsic merit. I allude to the idea of Descartes, who pointed out the pineal gland as the peculiar organ of the nervous functions, or, as it was termed, the seat of the soul.[4] The pineal gland is a small projection at the basis of the brain, which, in many respects, is curiously organized, and appears to be carefully protected from external injury. It was therefore conjectured that it must serve some important purpose, and this conjecture appeared to be confirmed by the circumstance that, upon examining the brains of certain idiots, they were found to contain a quantity of earthy matter. This sand was supposed to be an extraneous substance which, from accident or disease, was lodged in the part, and impeded its functions, and, a connexion thus appearing to exist between a disease of this part and an imperfection in the nervous powers, it was concluded to be the immediate seat of these faculties. There was some plausibility in this reasoning. But Descartes, although a man who very effectually promoted the progress of

[4] Tractatus de Homine, pars Quinta. This organ is thus described by Muraltus; "Hæc (glandula pinealis) radicibus quatuor, aliquando binis, aliquando unica, insignibus medullaribus, i. e. nervis omnibus in compendio junctis suffulcitur: hæc omnium objectorum motus excipit: anima in hac sola per hos motus sensilia externa, et omnes ideas, quæ a sensibus proficiscuntur, apprehendit, tanquam in centro, &c." Clavis Medicinæ, p. 508, (1677.) A perspicuous abstract of Descartes' system may be found in Sprengel, Hist. de la Med. t. v. sect. 5, ch. 4. He informs us, contrary to what is commonly related, that the existence of earthy matter in the pineal gland was first detected by Huet. For the form of the organ and its connexion with the other parts of the brain, see Vicq d'Azyr, pl. 8, fig. 1, Nos. 17, 18; pl. 12, No. 14; pl. 13, No. 14; pl. 14, No. 20; pl. 16, No. 45. This splendid performance must be admitted to supersede every other reference.

knowledge, lived before the full establishment of the inductive method of philosophizing. He neglected to inquire into the natural state of the gland; it has been since found that, in the adult human subject, earthy matter is always present in it, and indeed composes a considerable part of its substance.[5]

The investigation of the particular seat of nervous sensibility has been diligently prosecuted by some of the modern anatomists, and they have undoubtedly proceeded upon a more correct plan, if they have not been more successful in their result. They have not been satisfied with mere conjecture, but, in order to discover this supposed seat of sensibility, they have adopted two modes of inquiry.

According to the first they have examined the brain after it has been injured by accident or disease, and have noticed what effects have been produced upon its faculties; whether the destruction of any particular part has been followed by the loss of any particular faculty; or whether there is any one part, the destruction of which seems to be necessarily connected with the total loss of the sensitive functions. But although many accurate examinations have been

5 Blumenbach's Physiol. p. 125; and Comp. Anat. by Lawrence, p. 296. Sœmmering, De acervulo cerebri dissertatio, in Ludwig, Scrip. Neur. t. iii. p. 322. An analysis is given of the earthy matter, but it is not sufficiently accurate to enable us to ascertain its nature; they seem to indicate that it contains lime, and it is stated that the oxalic acid enters into its composition, p. 337. See also Sœmmering, Fab. Hum. Corp. t. 4, § 52, et Wenzel de Struct. Penit. Nerv. p. 316.

made, and many curious facts that bear upon this point have been brought forward,[6] nothing very important seems to have been established, except that the medullary matter in general is more sensible than the cortical. It seems likewise to be proved that the sensibility of the medullary part itself increases as we proceed nearer to the centre of the brain, where we also find a much more elaborate system of organization, and a much greater variety of separate parts, all of which we may fairly conclude serve some appropriate purpose connected with the nervous powers. Indeed the result of our examination of the brain, after it has been injured or diseased, is that it is capable of undergoing a much greater degree of disorganization in its mechanical structure, than could previously have been supposed compatible with the maintenance of its functions, without their being very materially affected. With respect to the more external portion of the brain, it is well known that it may be pierced or cut, or even that large masses of it may be removed without any very material effect being produced upon the perceptive faculties,[7] and we frequently find that large abscesses are

[6] Haller, El. Phys. x. 7, 22.

[7] This was very remarkably exemplified in the experiments of M. Legallois and Dr. Philip, and in some that have been made more lately by M. Flourens, of which we have an analysis by Cuvier, Ann. Chim. et Phys. t. xx. Sir Everard Home's experience leads him to a conclusion still more singular, that all the functions of the brain remain after the destruction of the whole of its medullary matter. Phil. Trans. for 1821, p. 31. See likewise Monro on the Brain, p. 38, with the references; and

formed in it, or tumours and excrescences of various kinds, which, if they do not compress the remaining part of the brain, seem to produce little injury to its functions. There are also many curious and well authenticated pathological facts on record, where different parts of the medulla of the brain have been destroyed, and even those which, from their situation with respect to the organs of sense, might have been supposed the most essential, and yet the nervous powers have remained nearly in their ordinary state.

The facts that have been observed with respect to hydrocephalus bear immediately upon this question, and lead to conclusions that are very unexpected. When the water collects in the ventricles that are near the base of the brain, if the skull yields to the

Dict. des Scien. Med. Art. "Hydrocephale," p. 243, et alibi, by Itard. This is also the inference that we are to draw from an amusing, although not very scientific paper in the Edinburgh Review, vol. xxiv. p. 434; if we adopt implicitly all the statements that are brought forward in this article, it would be very difficult to assign any use for the brain. Of the two most remarkable cases quoted, that taken from Dr. Quin, it may be remarked, is vaguely and briefly related, and is moreover anonymous. On Dropsy of the Brain, p. 104. The other case, which was very minutely described by Dr. Heysham, is, on many accounts, deserving of our attention, and serves to show in a remarkable manner the independence of the contractile functions upon the nervous system, but it does not throw much light on the connexion between the brain and the sensitive functions. I may be allowed to observe that the reasoning of Dr. Hull would have been more perspicuous had the terms employed been used in a more definite manner; the essay is, however, a valuable collection of facts. Manchester Mem. vol. v. p. 475.

distending force from within, and the pressure be not too suddenly applied, the bones separate and the skull becomes enlarged to an immoderate size. When the head is examined after death, the cavity of the skull is found to be filled with a fluid, surrounded by a kind of bag of cerebral matter. Until lately it was assumed as an obvious and well ascertained fact, that, in these cases, a considerable part of the brain was actually removed by the absorbents, but even if, according to the statement of Dr. Gall and some of the continental anatomists, the substance of the brain is not actually diminished, still its texture and organization must be very materially deranged.[8]

[8] The opinion, that in those cases of hydrocephalus where the skull allows of the extension of the size of the head, and the consequent formation of a large central cavity, the substance of the brain is not actually removed, but has only the relative situation of its parts changed, was maintained by Sir Everard Home, probably before it had been promulgated by the continental anatomists. In the Phil. Trans. for 1814, p. 474, after giving an account of a case in which the head had acquired an enormous size, while the mental faculties were but little impaired, he adds, "The cerebrum is made up of thin convolutions of medullary and cortical substance, surrounding the two lateral ventricles, which are unfolded when the cavities of these ventricles are enlarged, and in this unfolded state the functions belonging to this part of the organ can be carried on." As the brain in this case was not examined, we may infer that the above conclusion was derived from dissections of morbid brains that had been previously made, and it will hence afford us another instance in which Sir Everard Home has anticipated Drs. Gall and Spurzheim in what has been supposed among the most novel of their doctrines. Some remarks are made by Morgagni on this subject, Book 1, letter 12, art. 13, et seq. which show that he had a somewhat similar idea, although pro-

From the gradual way in which the symptoms of the disease manifest themselves, we may be certain that, for a long time before death, the head must have been nearly in the same state in which it is found upon dissection, yet the faculties, both physical and intellectual, have remained in tolerable perfection, and the patient has rather suffered from general indisposition, and from the inconvenience of an unwieldy head, totally disproportioned to the rest of the body, than from a defect of the powers of the nervous system.[9]

Another method which has been employed in order to ascertain the seat of the sensorium commune, is to endeavour to trace up the nerves of the different organs of sense to one spot within the brain, which might be considered as their origin. But although this plan has been attempted by the most skilful anatomists, it has not been successful; many of the nerves may indeed be traced up to the base of the brain, or to some part immediately connected with the medulla oblongata, but this cannot be accomplished with respect to the whole of them. It would

bably less precise than that of Sir Everard Home. He speaks of "the substance of the cerebrum itself" adhering to the skull "in the form of a membrane;" of "the brain being extended almost to the thinness of a membrane;" but, at the same time, he conceives that there are many cases where the brain is actually destroyed. It does not exactly appear whether he thought the loss of the sensitive and intellectual functions were in proportion to the *actual destruction* of the brain.

9 Dr. Male has detailed one of the most remarkable cases of this kind which we have on record in the Ed. Med. Journ. v. ix. p. 398.

appear, therefore, that no anatomical centre of this kind has yet been detected, and when we find that the accurate Sœmmering, as the result of his researches, has fixed upon the halitus, or fluid in the ventricles, as the primary seat of sensibility,[1] we can scarcely expect that it ever will be discovered.

And besides the difficulties, both pathological and anatomical, which we have found in our attempts to fix upon any spot within the cerebral mass as the immediate seat of the perceptive faculties, there are some circumstances which lead us to doubt whether any organ of this kind actually exists. There are certain considerations which have induced many physiologists to conclude that, although the brain is to be regarded as the common sensorium, yet that the expression can only be employed in a general way, when we speak of the cerebrum and cerebellum as contrasted with the nerves and spinal cord. An opinion has long prevailed that different portions of the brain are subservient to different offices, as, for example, that

[1] He admits that every part of the brain has been injured, without any corresponding injury having been perceived in its functions, and he, at the same time, objects to the doctrine that all the brain in its whole extent is to be regarded as the seat of the mental powers. Fab. Hum. Corp. t. 4, § 98. Hence he conceived himself reduced to the dilemma of fixing upon the fluid of the ventricles as the appropriate organ of the noblest faculty which is possessed by man. In § 59, he says, "peculiare organum sensorii communis si ponere fas est, vel si propria sedes sensorio communi in cerebro est, haud sine veri quadam specie hoc in humore (ventriculorum) quæri debet." On this subject see Sprengel, Inst. Med. t. ii. p. 237, et seq.

some are more peculiarly connected with the organs of sense, some with voluntary motion, and others with the different vital functions. Willis was among the first who distinctly pointed out certain phenomena that were supposed to lead to this conclusion. His idea was, that the cerebrum or proper brain is the organ of the perceptions derived from the external senses, and of voluntary motion, while the cerebellum is the source of the involuntary and vital functions.[2] This opinion, which was embraced and zealously defended by Boerhaave, and many of his disciples,[3] was derived partly from observations and experiments on the effects of injuries to the two parts of the cerebral mass respectively, and partly from the investigations of comparative anatomy; but although many curious coincidences were pointed out, yet the objections against the hypothesis that were adduced by Haller and others were so decisive,[4] as to prove that it is unfounded.[5]

But although it may be admitted that we have

[2] Cerebri Anat. c. 15, p. 74.

[3] Boerhaave, Instit. § 401, 415. See also Haller, El. Phys. iv. 5, 8.

[4] El. Phys. x. 7, 36.

[5] Dr. Philip, whose experimental investigations have given him the means of forming a correct opinion on this subject, concludes that the division of the cerebral mass into cerebrum and cerebellum has no relation to the voluntary and involuntary muscles, but may rather refer to some distinction between the different sensorial functions. Exper. Enq. p. 108. The late investigations by M. Fleurens seem to show that the cerebrum is the more immediate seat of perception, and the cerebellum of volition.

not succeeded in our attempts to appropriate different parts of the cerebral mass to different nervous functions, either according to the arrangement of Willis, or of any other which has been proposed since his time, still there are certain considerations which may lead us to suspect that this appropriation may actually exist. And on this question we may derive some important information from the experiments of M. Legallois and Dr. Philip, and the discussion to which they have given rise. It appears to be fully established by these physiologists, and more particularly by Dr. Philip, who has stated his opinion with more precision and accuracy than M. Legallois, that the action of the nerves is different from, and independent of, that of the brain. He accordingly assigns to them different denominations, styling them respectively the nervous and sensorial powers.[6] It appears that the former of these consists simply in the transmission of certain effects by means of the nerves from one part of the system to another, in which the brain is not necessarily concerned, and that consequently they are not referrible to the common sensorium. Our inquiry, therefore, will be limited to Dr. Philip's sensorial power, which he supposes to be composed of the functions of perception[7] and volition. Now there is at least no improbability in the supposition, that these functions, which appear to be so clearly distinct in their nature and operation, might be

[6] Enquiry, p. 186. Phil. Trans. for 1815, p. 90.

[7] Or, as he terms it, sensation; see note in p. 242.

attached to different parts of the brain,[8] and perhaps some confirmation of this opinion may be derived

[8] A very elaborate train of experiments has been lately performed on this subject by M. Fleurens, of which we have an abstract by M. Cuvier. M. Fleurens proposed to discover from what part of the brain irritation may be propagated to the muscles, to what part sensation extends, proceeding from the extremities towards the centre, and what is the immediate seat of volition when it is exercised in effecting muscular contraction. He endeavoured to ascertain these points by gradually removing successive portions of the brain and noticing the effects of each removal, when he was led to conclude that the two first of these, or, as we may regard them, the centres of irritation and of sensation, are in the medulla oblongata, at the part where the tubercula quadrigemina are attached to it. We are informed that the great mass of the cerebrum and cerebellum are not immediately concerned either in muscular irritation or in sensation; they appear, however, to be necessary to perception, especially the former, which is also supposed to be the immediate seat of memory and intellect. The effect of removing the cerebellum was to destroy the voluntary power over the muscles; the animal appeared to retain its volition, but had lost the capacity of exercising it. M. Cuvier speaks in high terms of commendation of the paper of M. Fleurens, and it must be confessed that if the experiments be confirmed, they will lead to very important views respecting the nervous functions. Ann. de Chim. et Physique, t. 20. Shortly after the publication of the above abstract, M. Magendie published a detail of a series of experiments, very closely resembling those of M. Fleurens, which had been performed twelve years before by M. Rolando. They appear, indeed, to have been even more decisive, and to have been made upon a greater variety of animals, than those of M. Fleurens. They were, I believe, entirely unknown in this country, and seem to have been nearly so in France; and it must be acknowledged that this coincidence, where we have no ground for suspecting any unfair claim to originality on the part

from some of his experiments, in which the two functions of the sensorial power were not always affected in the same degree by the same application to the brain, or by the same mechanical injury to its structure. Possibly we may go so far as to conjecture that while the perceptive faculties have their common centre in the medulla oblongata or its appendages, volition has a more extended connexion with the brain, a conjecture which may derive some support from the more intellectual nature of this latter faculty. So far as we are able to come to any conclusion upon so intricate a subject, it would appear that the brain itself is the more immediate seat of the proper intellectual faculties, or that it is through this part of the nervous system that the mind acts upon the physical organs. But the further consideration of this question, as well as the doctrine of craniology, will be referred to a subsequent part of the work.[9]

of M. Fleurens, must tend very materially to strengthen our confidence in the accuracy of the results. The principal circumstance in which the conclusions of M. Rolando differ from those of M. Fleurens is, that whereas the latter physiologist considers the cerebellum to be the regulator, as he terms it, of the voluntary motions, M. Rolando regards it as the origin of them, thus more completely separating the primary seat of perception from that of volition. Magendie, Journ. de Physiol. t. iii. p. 95, et seq. See some observations upon the subject by M. Magendie, p. 153.

[9] It may be worthy of remark, that Sir Everard Home has given his sanction and authority to the doctrine of craniology in the most explicit manner, although the mode of reasoning upon

Although we have so little certain knowledge respecting the exact parts of the brain, which may serve as the respective centres of the two functions of perception and volition, some very interesting experiments have been lately made by Mr. Bell and M. Magendie,[1] which confirm the opinion that had been advanced by physiologists as a plausible conjecture, that the transmission of these two powers from the extremities to the brain, or from the brain to the extremities, is effected by different nerves, or at least by different nervous filaments. The nerves that proceed from the spine have a double origin, or are composed of two nerves, one proceeding from the anterior, and the other from the posterior part of the cord. Now, it has been found by direct experiment, that the parts to which the nerves are sent are deprived of motion and sensation respectively, accord-

which he founds his opinion may appear somewhat questionable. Phil. Trans. for 1821, p. 31.

[1] I have associated the name of M. Magendie with that of Mr. Bell as the *discoverer* of the fact referred to in the text; but, in justice to Mr. Bell, it is necessary to state, that his experiments were clearly antecedent to those of M. Magendie, although M. Magendie might have been unacquainted with them. On this subject the papers of Mr. Shaw furnish very decisive evidence, and should be perused, as well for this as for the other valuable matter which they contain, connected with the double function of the nerves. See Lond. Med. Journ. v. xlviii. p. 343, and 457; v. xlix. p. 449. It is always a painful task to notice what may appear like disingenuousness, especially in those whose attainments are of a superior order; such a reflection cannot, however, but be suggested by the perusal of a late memoir of M. Magendie's, in Ann. de Chim. et Phys. t. xxiii. p. 429.

ing as the anterior or posterior roots of the nerves are divided. From this separation of the functions of the nerves, and the appropriation of each function to a specific organ,[2] we gain an analogical argument for the same kind of separation in the brain, and this conjecture would appear to be sanctioned by the experiments of M. Fleurens and M. Rolando.

A very interesting series of facts has been lately brought forwards by Mr. Bell respecting the functions of the nerves, which throws considerable light upon their mode of action and their connexion with the other parts of the system. There are certain circumstances in the anatomical structure and the distribution of the nerves which led to the arrangement of them into two classes, and which indicated that they each serve different purposes in the animal œconomy. From the situation of these two sets of nerves with respect to their origin and to each other, Mr. Bell has given them the names of symmetrical or original, and irregular or superadded. The first set, which might perhaps be called more appropriately, the general nerves, consist of the fifth pair of the cranial and all the spinal nerves; they have double roots, one of which is connected with ganglia; they pass laterally to the two halves of the body, the two sides having no connexion with each other, and they are distributed to all the muscular parts that

[2] This separation of the nervous functions has been extended by M. Magendie to the two pillars of which the spinal cord is composed. Journ. de Physiol. t. iii. p. 153.

are under the control of the will. They appear to be the organs of perception and volition, deriving, as we may conjecture, these two functions from their double roots. These we may regard as that part of the nervous system which serves the purpose of establishing our connexion with the external world.

Mr. Bell's second set of nerves proceed by single roots from the base of the medulla oblongata, or the parts immediately connected with it; they proceed in a much more irregular manner than the former, and are distributed to all the organs which are concerned, either directly or indirectly, in the function of respiration. From this circumstance they have received the denomination of respiratory nerves, as well as that of superadded or irregular. Their course is designated by this last term, as they pass from one organ to another in the most intricate manner, connecting them together, passing across the general nerves, occasionally uniting with them, and forming the connecting link between the two halves of the body. These nerves are not under the control of the will, and are not capable of exciting perception; they are, therefore, furnished only with the faculty of transmitting the nervous influence, or with what Dr. Philip styles nervous power, in opposition to sensorial. Some very curious and important pathological deductions have been made by Mr. Bell, from the new views which he has given us on the subject of the nerves, and we have also a number of additional remarks by Mr. Shaw, which confirm and illustrate Mr. Bell's doctrines, and give us reason to expect that they may be applied with great advantage to the practice of medicine and surgery.[3]

The result of our observations upon the nervous system and its functions is, that it has two distinct powers, that of receiving and transmitting impressions, which is exercised by the nerves and spinal cord, and that of perception and volition, which is more immediately exercised by the brain.[4] Upon this principle Blumenbach has arranged the organs of these functions into the two classes of sensorial, comprehending the brain and its immediate

[3] See Bell, in Phil. Trans. for 1821, p. 398, et seq.; also Phil. Trans. for 1822, p. 284, et seq.; and Shaw, in Quart. Journ. v. xiii. p. 120; and Med. Chir. Trans. v. xii. p. 105; Lond. Med. Phys. Journ. v. xlviii. p. 343, and 457; v. xlix. p. 449. Experiments similar in their nature to those of Mr. Bell have been performed by Mr. Mayo, but with a different result, so far as the respective powers of the nerves is concerned, and their relation to the faculties of perception and voluntary motion. Comment. p. 107, et seq.; and part 2, p. 2, et seq. It is to be regretted that such discrepancies should arise, but where truth is the only object of the inquirer, they will be in no long time reconciled by multiplying our observations and experiments. Since the above was written two papers of Mr. Bell's have appeared in the Phil. Trans. for 1823, p. 166, and 289, in which he has made a beautiful application of his principle to illustrate the respective offices of the different nerves that are sent to the eye and the parts immediately connected with it. The acute mind of J. Hunter enabled him to detect the general principle of the specific powers of different nerves when sent to the same organ, Animal Economy, p. 262, but the merit of demonstrating the truth of the principle, and of its successful application, rests with Mr. Bell.

[4] Dr. Philip, in Quart. Journ. v. xiv. p. 93.

appendages, and the nervous, properly so called, including the nerves, the plexuses, and the ganglia.[5] The sensorial organs are the exclusive seat of the powers of perception and volition, and of the intellectual faculties, while the office of the nerves is to serve as media of communication between the common centre and the organs of sense and motion.[6]

[5] Instit. Physiol. § 198.

[6] What is stated in the text is, perhaps, all that we are warranted in concluding from the facts which are at present clearly ascertained; but it is almost impossible to proceed so far without forming some conjectures, or proposing some queries respecting what remains still to be discovered. Mr. Bell's first set of nerves, i. e. the fifth cranial and the spinal, possess the function of communicating both perception and volition, and as they arise from double roots, it is not unreasonable to infer, that the two roots serve respectively for the two powers. But these nerves are distributed both to the voluntary muscles, and to the skin and the internal surfaces; has any anatomical difference been detected between the nerve of the two orders of parts, the one possessing the double function, the other merely serving for the transmission of perception? Mr. Bell's second division of nerves, the irregular, are not capable of communicating either perception or volition, but serve to transmit the nervous influence from one part to another; it includes what he names the respiratory nerves and the intercostal system, and is connected in an indirect manner only with the brain, while it is principally to these nerves that the ganglia are attached. Are these nerves capable of transmitting their influence in both directions? Is it probable, that besides their ordinary office, these nerves, on certain occasions, are capable of conveying perceptions, and that the ganglia are the parts to which the perceptions are referred, for example, the perceptions of internal diseases? Besides these two classes of nerves, we have a third kind, those appropriated for specific perceptions, as the optic, the olfactory, &c.; What is there peculiar in their ana-

The great superiority of the intellectual faculties of man over those of other animals, has induced anatomists to investigate whether there be any thing in his anatomical structure which would seem to account for this superiority. The great size of the human brain, compared to that of other animals, was noticed by the ancients, and Aristotle laid it down as a general principle, that the faculties which were referred to this organ were in proportion to its size, compared with that of the whole body. This rule holds good with respect to many of the domestic animals, which were the best known to the ancients, and upon which we may presume that their observations were made. For example, the brain of a man, according to the calculation of Monro, is four times that of an ox, although, upon an average, the body of the ox is six times the size of the human body.[7] But there are many exceptions to this rule. It has been found by the accurate researches of the modern anatomists, that in some of the mammalia, the proportion of the

tomical structure? Do not the mere *perceptive* parts of the first class belong to this division? Have not the nerves of the surfaces more analogy, at least in their functions, with these nerves, than with those which transmit volition? What relation do the nerves that convey simple sensations of pleasure and pain bear to the other parts of the nervous system? The further consideration of many of these points obviously belongs to a subsequent part of the work. In order to avoid circumlocution it might be convenient to give the names of simply sensitive, perceptive, and motive, to the three kinds of nerves, according as they respectively serve for the offices of the mere transmission of nervous influence, for perception, and for voluntary motion.

[7] On the Nervous System, p. 25.

size of the brain to the body is equal to that of the human subject,[8] and that there are certain species of birds in which the proportional size is even greater. It further appears that some animals, which are remarkable for the comparative perfection of their sensorial powers, have the brain below the average size, as the horse and the elephant. In man, the ratio of the weight of the brain to that of the whole body has been stated at an average at about $\frac{1}{28}$, in the dog it is about $\frac{1}{160}$, in the horse $\frac{1}{400}$, in the elephant $\frac{1}{500}$ only, while, on the contrary, in several of the small singing birds, and particularly in the canary, the brain is above the average of man, being as much as $\frac{1}{14}$,[9] and Ebel mentions a kind of simia, where the proportion is even $\frac{1}{11}$.[1]

But an observation has been made by Sœmmering, to which hitherto no exception has been found, that the perfection of the sensitive functions does not depend upon the absolute size of the brain, nor upon its proportion to the body at large, but upon the proportion between the size of the brain and the aggregated bulk of the nerves that proceed from it;[2] or, according to Blumenbach's nomencla-

[8] Buffon, v. ix. p. 247, says that the brain of the seal is larger in proportion to the body of the animal than that of the human subject.

[9] Cuvier, Leç. d'Anat. Comp. t. ii. p. 149, et seq. See also Lawrence's Lect. p. 191.

[1] Observ. Neurol. ex Anat. Comp. in Ludwig, Scrip. Neur. t. iii. p. 150.

[2] Corp. Hum. Fab. t. iv. § 92; de Basi Enceph. p. 14. Lawrence's Lectures, p. 192, et seq. Blumenbach's Comp. Anat. by Lawrence, p. 292.

ture, between the sensorial and the nervous organs. As an illustration of this position the example of the horse is cited ; the absolute size of the brain of the horse is only about half the size of the human brain, while the mass of the nerves of the horse at their origin, is no less than ten times larger than that of man. And as we pursue our researches into comparative anatomy, we find that we are able, in most cases at least, to trace a correspondence between the perfection of the respective functions and the physical condition of the organs. Most of the inferior animals have larger nerves, and possess some of the nervous functions in a much more acute state than man, but man decidedly excels them all in the comparative size of the brain and in the perfection of his intellectual functions.

Ever since the time of Willis the proportion of the cerebrum to the cerebellum is a point that has been attended to by anatomists, as marking a difference in the degree of perfection of the nervous system, and it has been asserted, that the proportion of the cerebrum to the cerebellum is greater in man than in any other animal. But although this holds good in most instances, it appears from Cuvier that there are some exceptions, and, as is often the case in comparative anatomy, we find that in those points where man differs most from other animals, there are some species of simiæ which resemble the human subject. In man the weight of the cerebrum is to that of the cerebellum as nine to one, in the dog as eight to one, in the horse as seven to one, in the cat as six to one, in the sheep as five to one, while, on the contrary, in a certain species of monkey, the proportion is as much as fourteen to one.[3] It may seem somewhat remarkable that although, in the higher classes of animals, we find a general ratio between the perfection of the nervous system and the large size of the cerebrum, compared with that of the cerebellum, yet when we descend lower in the scale, we meet with some cases in which the cerebellum is entirely wanting. In these cases, however, the general form of the brain differs so much from that of man, and the animals that resemble him, that it is not easy to say what are the parts most analogous to each other, or to what portions of the brain the different appellations ought to be applied.

Another comparison has been instituted, which gives a more constant superiority to the human subject, the proportionate size of the cerebrum to the medulla oblongata, although, in this instance, as well as in the former, the simiæ are found to be more analogous to man than to any other animals.[4]

All these comparative observations are deserving of attention, but we may remark concerning them, that we might, *a priori,* expect the powers of the nervous system to depend as much, at least, upon the perfection of its organization, as upon its mere bulk, or upon the proportion between the size of its dif-

3 Leç. d'Anat. Comp. t. ii. p. 152, et seq. Blumenbach's Comp. Anat. by Lawrence, p. 312.

4 Leç. d'Anat. Comp. t. ii. p. 152, et seq. The dolphin forms the only exception to this rule among the animals upon which observations have been made.

ferent parts. It does not appear that any very accurate comparative observations have been made upon the minute structure of the brain in the different classes of animals, nor perhaps ought we to expect to be able to discern any visible difference between them; but it is well known that the greatest variety exists in their general figure and anatomical structure, and although we are very frequently unable to trace the connexion between the configuration of parts and their respective functions, we may reasonably infer that such a connexion actually exists.

The only further observations that I shall offer on the comparative uses of the different parts of the nervous system are concerning the appropriate office of the ganglia. From the mode in which they are composed, it appeared a natural conclusion, that one office which they perform is to produce a more complete connexion and sympathy between the sensations of distant parts; but this, as far as appears, might have been accomplished by the simple union of the nervous filaments, as occurs in the plexuses, without the additional apparatus which we observe in the ganglia: some eminent anatomists, as Winslow, Willis, and Vieussens, supposed them to be small brains, or independent sources of nervous power and central spots, to which perceptions are referred. This opinion is embraced by Richerand[4] and by Cuvier,[5]

[4] Physiologie, t. i. p. 108.

[5] Leç. d'Anat. Comp. Intr. p. 26. The author remarks that the ganglia are larger and more numerous when the brain is deficient in size.

and a doctrine essentially similar is maintained by Bichat,[5] who conceives them to be the nervous centres of the organic functions. Lancisi had a fanciful notion that they promoted the flow of the animal spirits along the nerves, by a kind of muscular action;[6] Johnstone supposed that their use is to render the organs which derive their nerves from them independent of the will,[7] and it has been lately conjectured that their office is to recruit the nerves that pass through them, or to add to their substance, in the same manner that the cortical part of the brain has been conceived to generate the medulla. Dr. Philip considers the ganglia as secondary sources of nervous influence, the specific office of which is to receive supplies of it from all parts of the brain and spinal cord, and transmit this collected influence to the organs where it is required.[8] But the difficulty which has been already alluded to occurs in this hypothesis, that this combination of nervous influence, so far as we know, might have been accomplished by the mere union of the nervous filaments.[9] Upon the whole I apprehend we must acknowledge, that the specific office of the ganglia has not been discovered.

5 Anat. Gen. t. i. p. 200; t. ii. p. 405.

6 Morgagni, Advers. Anat. pars 5, p. 113.

7 Essay on the Ganglia, p. 19.

8 Enquiry, p. 170, et seq.; Phil. Trans. for 1815, p. 436; Quart. Journ. v. xiii. p. 266.

9 For a concise view of the various opinions that have been entertained upon this subject, see Sœmmering, Corp. Hum. Fab. § 161.

§ 4. *Connexion between the Muscular and Nervous Systems.*

Having considered the properties and uses of the nervous system, I proceed to inquire into the nature of the connexion between the nerves and the muscles, or, more generally, between sensation and motion. After Haller had clearly established the difference between the two essential properties of animal life, contractility and sensibility, and pointed out their mode of action, it was universally admitted that the former of them is the immediate cause of motion and the latter of sensation. It was obvious that motion was, in many cases, necessarily accompanied by sensation, but the question then arose, whether it was so in all instances. The solution of this question was attended with many difficulties, and eventually gave rise to one of the most animated controversies that ever took place in physiology, and which, although no longer pursued with any degree of acrimony, subsists to the present day.

The point at issue may be thus stated. When a stimulant acts upon a muscular fibre, so as to produce contraction, does it act immediately upon the fibre itself, or does it not always act through the intervention of a nerve? The nerves are the organs of sensation, when, therefore, a muscle receives the impression of a stimulant, is not this impression always, in the first instance, received upon the nervous matter distributed through the muscle, and the impression then transferred from the nerve to the muscular fibre? Haller and his disciples thought the intervention of the nerve not to be necessary, but supposed that the irritability of the muscle, as they termed it, was, in many cases at least, alone concerned, and that consequently stimulants were capable of acting upon the fibre itself. His opponents, of whom Whytt was one of the most active and zealous, supported the contrary doctrine, and maintained that the muscular fibre is merely an organ of motion, that it is incapable of receiving impressions from external objects, and never contracts, except through the intervention of the nervous power.[1] This hypothesis was embraced by many of the French writers, particularly by Senac, who became one of its most able defenders, as well as by the colleagues of Whytt, Cullen and Monro; and it was the doctrine generally embraced in the University of Edinburgh, then approaching to its most splendid period of reputation.[2] The term irritability, which had been originally used by Glisson in a physiological sense, and had been adopted by Haller, was employed by the two parties in a somewhat different meaning, a circumstance

[1] On Vital and Involuntary Motions, sect. 1, p. 10, et alibi.

[2] Willis, who was one of the first that minutely attended to the operations of the nervous system, conceived that all motion depends upon, or originates in, the nerves, and that the nerves of the voluntary and involuntary organs are derived from the cerebrum and cerebellum respectively. Mayow, Boerhaave, and other eminent physiologists of the latter part of the seventeenth and the beginning of the eighteenth century, adopted Willis's hypothesis, and it may be considered as the prevailing opinion until the time of Haller.

which it is necessary to bear in mind in examining the merits of the controversy. By Haller it is intended to express the power inherent in the muscular fibre (its vis incita) of being excited to contraction on the application of a stimulant; according to Whytt it means the faculty which the muscular fibre possesses of receiving impressions transmitted to it from the nerves. The experiments and arguments that have been adduced in this controversy, in support of the two opposite opinions, have filled many volumes; I shall only have it in my power at present to notice a few of the leading facts that have been stated, and the general scope of the reasoning that has been employed.[3]

The great argument of Haller was an appeal to the anatomical structure and obvious powers of the animal body. He made a distinction between the contractile and the sensitive organs, and he endeavoured to point out to what parts of the system these

[3] A well-digested and correct state of the hypotheses that prevailed on this subject before the time of Haller, and of his own doctrines, will be found in a report made to the French Institute on the experiments of Legallois by Percy, Humboldt, and Hallé. See Ed. Med. Journ. v. x. p. 207; and Philip, in Quarterly Journ. v. xiii. p. 98; also Dr. Cooke's introduction to his valuable treatise on nervous diseases.—M. Cabanis, an eloquent and acute writer, and the nature of whose work might be supposed to lead him to a degree of logical accuracy, has, notwithstanding, chosen to regard this controversy as merely a verbal one, but I believe that I shall be justified in stating that he has completely misunderstood the nature of the argument. See Rapports du Phys. et Mor. de l'homme, t. i. p. 90, 91, (2e edit.)

terms were respectively applicable.[4] He pointed out some which were extremely contractile, but were only scantily supplied with nerves, and it was found generally, that the contractility of parts did not bear any ratio to their sensibility, or to the quantity of nerves sent to them. The example of the heart was adduced in proof of this position. Indeed so little sensitive is this organ, not merely in its sound, but even in its morbid state, that there are many well-known instances, where it is found to have undergone great alteration in its structure, to have had its parts condensed by the effect of inflammation, or even destroyed by suppuration, and yet no pain has been felt, and nothing unusual has been experienced except a sense of oppression, and this not depending so much upon the condition of the heart itself, as upon the difficulty which it had in transmitting the blood along the arteries.

In the second place it was urged as a powerful argument in favour of the Hallerian doctrine, that muscular parts remain contractile for a long time after they are removed from the body, and when their communication with the brain is of course destroyed.[5] This is especially the case with respect to the heart, which in frogs and in cold-blooded animals generally, for many hours after its separation from the body, will still contract on the application of a stimulant; but how, it is asked, can the nervous system be con-

[4] Mem. sur les Part. Sens. et Irrit. Opera Minera, t. i. p. 239.

[5] Haller sur les Part. Sens. et Irrit. t. i. p. 48, et seq

cerned in this case, since the centre of the sensibility is removed?

A third set of facts, which were supposed to be still more in favour of his doctrine, was brought forwards by Haller, consisting of the accounts of fœtuses that were born with imperfect brains, or even altogether without heads, and yet had grown to their full size in the uterus, and after birth exhibited many marks of contractility, being capable of moving their limbs upon the application of stimuli, of evacuating the contents of the bladder and intestines, and, in short, of exercising the accustomed functions, as far as was consistent with their imperfect organization. Indeed it would appear that, if we could have supplied the materials for digestion, life might have been prolonged to an indefinite period. This point has been lately insisted upon by Dr. Philip, in his essay on the connexion between the muscular and the nervous powers. From some experiments that were performed by Legallois on this subject, it seemed to be proved, that the motion of the heart was independent of the brain, yet that there was a necessary connexion between the heart and the spinal cord.[6] But the experiments of Dr. Philip have demonstrated, that the spinal cord is no more necessary than the brain for the motion of the heart. He found that the total destruction both of the brain and the spinal cord did not prevent the contraction of the heart from proceeding in its ordinary manner, provided its other

[6] Sur le Principe de la Vie, p. 138, et seq. et alibi.

connexions with the system were duly maintained; he likewise adduces cases, apparently well authenticated, of fœtuses born in a state of maturity, where the spinal cord, as well as the brain, was totally wanting.[7]

In the fourth place, the Hallerian doctrine has been supposed by some physiologists to receive very powerful support from the constitution of the lower classes of animals, such as the Actiniæ, and others of the larger zoophytes, which are of considerable size, and are endowed with a great degree of contractile power, yet in which the most minute researches have not been able to detect a nervous system. The limbs of these animals contract upon the application of a stimulant, and of course the contraction is brought about without the intervention of nerves, but as it appears to be precisely similar to that of animals that have a nervous system, so we may conclude that in both cases the process is conducted in the same manner, and according to the same general laws.

A fifth argument in favour of the Hallerian doctrine has been derived from the order of time in which the different parts of the body come into existence and acquire their full powers. By minute observations made upon the fœtus, and more especially upon the chick in ovo, during the earliest periods of their existence, we learn that the formation of the heart precedes that of the brain; that the first

[7] Philip's Enq. p. 62. Lawrence, in Med. Chir. Trans. v. v. p. 168, et seq.

distinct indication of life is a small beating point, the punctum saliens, as it has been called,[8] which afterwards becomes the muscular substance of the ventricles, from this the large vessels gradually expand, and it is not until after some time that the brain becomes organized.[9] The successive periods at which the functions of the parts respectively commence their actions agree with these observations. The contractions of the heart proceed with perfect regularity long before we can trace any sign of the operations of the nervous system, and we find a great variety of involuntary muscular motions performed in the most perfect manner immediately after birth, while it seems necessary that a considerable length of time should elapse before any of the nervous functions are capable of being exercised.[1]

Besides the above arguments, all of which seem to be derived from legitimate grounds of reasoning, other considerations have been adduced in favour of the Hallerian doctrine, which are either in themselves of less weight, or have been since superseded by more correct scientific investigations. These, it will not be necessary to notice, except indeed that derived from the analogy of vegetables, which Haller himself

[8] Harvey de Generatione, Exerc. 17.

[9] From the recent observations of Sir E. Home we learn, that the rudiments of the brain and spinal cord are visible before the heart, but it may be argued, that the nervous system is at this period incapable of performing any of its functions. Phil. Trans. for 1822, p. 342.

[1] Haller, El. Phys. iv. 4. 28.

seems to have regarded as of considerable force. Plants, he said, exhibit evident marks of irritability, they possess spontaneous motion, and obey the operation of stimulants, yet they are without nerves, so that we have here a case in which irritability exists independent of a nervous system. But this is certainly an attempt to explain what is obscure by something which is still more so; we know less of vegetable than of animal life, and when we speak of the irritability of vegetables, we employ a metaphorical expression to denote a quality with the nature of which we are totally unacquainted.

I must not omit to mention an argument, that would have great weight in favour of the independent contractility of the muscular fibre, were the fact on which it is founded fully established. I allude to some experiments which were performed in Italy a few years ago, on the application of galvanism to the fibrin of the blood immediately after coagulation, in which it was stated that a contraction was produced in this substance similar to that of the muscular fibre.* The strong analogy which exists between the properties of the two kinds of fibrin would no doubt go far to demonstrate, that if contraction could be produced without the intervention of nerves in the one case, it might be so likewise in the other. Unfortunately, however, the fact in question is not sufficiently authenticated; the experiment, when performed in this country, has not succeeded, and although a negative experiment is not to be considered as of itself of equal force with a

It might be stated that the Galvanic influence compensated for the deficiency of nervous structure positive one, yet, upon the whole, the preponderance of evidence seems to be against the contractility of the fibrin of the blood.[2]

Powerful and direct as the arguments may seem which were adduced by Haller and his disciples, in support of the doctrine of the independent action of the muscular fibre, the neurologists have not been remiss in replying to them, and they have further supported their side of the question by arguments, which, by many physiologists, have been even thought more convincing. In opposition to Haller they have likewise appealed to the anatomical structure of the body, and have particularly insisted upon the general diffusion of nervous matter through the substance of the muscles. Although we are not able to trace the nerves to their extreme ramifications, yet it is contended that they are in fact distributed to each individual fibre, for when we insert into a muscular part the finest point of a needle, if it be only sufficient to produce a contraction, it will excite a corresponding sensation. Indeed some of the ablest defenders of this side of the question, and particularly Smith, seem disposed to rest the issue of the question upon this sole point, whether it be possible to discover any part which is contractile, and which is, at the same time, not furnished with nerves. It seems,

[2] See also Berzelius on the progress of animal chemistry, p 20, where we are informed that the apparent contraction of fibrin upon the application of electricity depends upon the shrinking of the body connected with its coagulation; it is not improbable that the process may be promoted by electricity.

however, impossible to put it to the test of direct experiment, for although we might have it in our power to obtain the muscular fibre in a separate state, and might even, by the diagnostic characters pointed out by Fontana, accurately distinguish between the muscular and the nervous filaments, yet in employing the mechanical means necessary for their separation, all their vital properties would be unavoidably extinguished. With respect to the heart and the other parts which are scantily supplied with nerves, and yet possess a great degree of contractility, it was said that in these cases, the quantity of nerve, although small, is in proportion to the nature of the stimulant, and the degree of effect required, and, in short, that the nerves of the heart are adequate to the action of the organ. Then it is argued that the heart is furnished with nerves, although small, and if they are not employed in producing its contraction, for what purpose are they destined?

With respect to the power which individual muscles possess of retaining their contractility when separated from the body, the neurologists certainly appear to have been much puzzled to account for the phenomena, and they were reduced to the necessity of adopting the opinion, which appears to involve a gross inconsistency, that the sentient principle is divisible,[3] or rather that there is no sensorium com-

[3] Whytt on Sensibility and Irritability, part 2, § 2. On Vital and Involuntary Motions, § 14. We may remark how directly the opinions of this learned physiologist lead to materialism, t the very time that he is arguing against this doctrine; a

mune. This inconsistency they could only escape by adopting the Stahlian hypothesis of the soul being co-extended with the body,[4] as they termed it, or that the different parts of the body were able individually to perceive the effect of stimuli applied to them. Senac explicitly states it as his opinion, that the nerves and spinal cord possess all the properties of the brain, only in a less degree, and that they are capable, for a limited time, of performing all its functions.[5] According to the hypothesis of the animal spirits, which was then generally adopted, he conceives that the spinal cord and even the nerves are capable of generating these spirits, and, in short, that the whole nervous system is, to a certain extent, the seat of perception.[6] The same kind of reasoning

striking example of the inconsistencies into which we fall when we attempt to investigate topics that are beyond our comprehension, and a powerful motive for exercising the utmost candour towards those who differ from us on such abstruse points. Mr. Herbert Mayo, who is in general remarkable for the correctness of his phraseology, may be cited as an illustration of the same sentiment. In his observations upon this much agitated question, he uses the expression, "mind and matter are logically distinct substances," thus controverting his own position in the very enunciation of it. The candid and philosophical reflections with which he concludes the chapter cannot be too highly commended. Anat. Comment. p. 8, 9.

[4] See Stuart de Motu Muscul. § 5.

[5] This appears to be the opinion likewise of Scarpa; speaking of the nerves generally, he says that they possess a power independent of the brain, and cites the cases of acephalous fœtuses in proof of this point. Tabulæ Neur. § 22.

[6] Sur le Cœur, liv. 4. c. viii.

was applied to repel the arguments that were drawn from the existence of fœtuses without heads, in which case it was said that the seat of sensibility was only in part removed, and that what was left in the spinal cord, the nerves, and the ganglia, was still competent to carry on all the functions which are exercised by these imperfect beings.

In connexion with this argument it has been observed that the size of the brain and the spinal cord, in the different classes of animals, bears no proportion to each other; on the contrary, as we descend to the less perfectly organized classes, the brain diminishes, and at length entirely disappears, while the spinal cord is increased in size, as if for the purpose of supplying the place of the brain. These facts are well known, but the inference to be drawn from them is not so clear. Before we can form an hypothesis to account for the action of the nervous system, derived from observations on the comparative anatomy of the inferior animals, we must previously make ourselves acquainted with the nature of their functions, which we have many reasons for supposing to be very different from those of the human species. The acephalous fœtuses, which have been employed to show the independence of the muscles upon the nervous system, have been also brought forward to prove the independence of the nerves upon the brain, for it is said that, in these cases, the nerves are perfectly formed, or even sometimes larger than natural, as if intended to supply the deficiency of the brain. But here again, although we may admit the anatomical facts, the conclusion does not necessarily follow from them, for in these instances there is no unequivocal evidence of the existence of any functions, which the Hallerians conceive to be exclusively attached to the brain as distinct from the nerves, and although the nerves exhibited their natural appearance, and seemed to possess a perfect anatomical structure, we have no means of ascertaining in what degree they were capable of exercising their appropriate functions.

The uniformity of the action of the different fibres of which the muscle consists, or what has been termed the general consent of all its parts, has been regarded by some of the neurologists as an argument of much weight in favour of their doctrine. If only one fibre be irritated, the whole muscle instantly contracts, but how can this contraction be propagated from fibre to fibre except by means of nerves? This argument, however, implies a knowledge of the intimate nature of muscular contractility which we are not entitled to assume, and it can only be regarded as endeavouring to explain a difficulty by the adoption of an hypothetical principle, which itself stands in need of proof.

The argument in favour of the Hallerian hypothesis, drawn from the absence of nerves in the zoophytes, was answered by asserting that we are not sufficiently acquainted with the structure of these animals to make it the foundation of our reasoning, and it was even alleged that the same argument might be employed to prove the existence of contractility without the muscular fibres; for a proper fibrous structure can no more be perceived in these animals than a nervous system. In the same manner it was stated that the order of time in which the different parts are formed, was a subject involved in too much obscurity to permit us to employ it in our reasoning. It was said that the heart might be sooner visible than the brain, in consequence of its motion, of its consistence, and its other sensible properties, but that it is impossible for us to decide which of the parts is actually first brought into existence.[7]

So far I have attempted to give an abstract of the manner in which the neurologists replied to the reasoning of the Hallerians; they have also employed direct arguments in support of their opinions, which must now be stated. But before I enter upon this subject I may remark concerning the heart, which, as it appears, has been appealed to by the supporters of both sides of the question, as a proof of their respective doctrines, that the structure of the nerves of this organ has itself been a subject of controversy among anatomists, and has given rise to much learned discussion and minute investigations. According to Sœmmering[8] the nerves sent to the heart are much smaller in proportion to the size of the organ than to any other muscle, and it was even contended by Behrens,[9] that these nerves are not destined for

[7] See note in p. 297.

[8] Corp. Hum. Fab. t. iii. § 32.

[9] Dissert. qua demon. Cor Nervis carere.

the muscular part of the organ, but for its large vessels, while, on the contrary, we have the no less respectable authority of Scarpa [1] in support of the opinion that the heart is furnished with nerves in the same manner with the other muscles of the body.[2] Until the anatomical question respecting the nerves of the heart be decided, it will be impossible to build any physiological hypothesis upon them, but the comparatively insensitive state of the heart is admitted by every one, and may be adduced as an objection to the doctrine of the neurologists, even were the existence of its nerves fully established.

The argument which was brought forwards by the neurologists with the most confidence was derived from experiments made with artificial stimulants, the object of which was to show that in a great number of the most unequivocal cases they act upon the muscles through the intervention of the nerves. The experiments of Smith are some of the earliest, as well as the most decisive that were performed on this subject,[3] and he found that exactly the same effects were produced, whether the stimulant was applied to

[1] Tabulæ Neurologicæ Card. Nerv. &c. These may be considered as, in all respects, probably the best anatomical plates that were ever published. They are admirably expressive of the subject, without the gaudiness of the French engravers, who appear to aim principally at effect, or the tameness of the English, who seem to think of little else except economy.

[2] See also Senac's work on the Heart, liv. i. ch. 7, where we have a minute detail of the observations of various anatomists and physiologists previous to his publication, and liv. iii. ch. 8, § 5.

[3] From a MS. copy of Cullen's Lectures on Physiology, of which I am in possession, it appears that it was chiefly upon the

the fibres themselves, or to the nerve that is distributed through them.[4] This he observed to obtain with respect to all the different kinds of stimulants, mechanical as well as chemical, and in these cases it is easy to detach the nerve from the surrounding parts, so that the application may be made to it in the most unexceptionable manner. The experiments that have been more lately made with galvanism tend to the same conclusion. Here, when a muscle is completely separated from the body, except by the intervention of a nerve, contractions are excited by transmitting the electric influence through the nerve, and it is always found that if a trunk of a nerve form part of a circuit, it is not the muscles in the neighbourhood of the nerve that are excited, but those to which the nerve is ultimately distributed, however distant they may be.

By reversing the nature of the experiment the reverse effect was produced. If instead of applying a stimulant we make use of a sedative, such as opium or laurel water, and immerse the nerve in it, the muscles to which the nerve is sent lose their contractility as entirely as if the sedative had been applied to the muscular fibres themselves. Facts of this kind are so well substantiated as to admit of no contradiction, and they undoubtedly seem to disprove one part of Haller's doctrine, that opium and other narcotics, when they affect the muscular power, operate through the intervention of the blood-vessels

authority of Smith's experiments, that he derived his hypothesis of the identity of the muscular and nervous fibres.

4 De Motu Musculari, passim.

and not of the nerves. Now it is said that we have certain evidence of the intervention of the nervous power in a great variety of cases, it is therefore a reasonable and fair inference that the same intervention exists in all cases, and that muscular contraction never ensues from the application of a stimulant except through the medium of a nerve.

But the facts which have been generally thought the most decisive against Haller are those in which we observe the muscular power to be affected by mental operations, such as the passions, and this is observed to be especially the case with respect to the heart, the organ which had been selected by the Hallerians, as one that acted independently of the nervous power, and was even thought to be entirely without nerves. And besides the observations that have been made upon the body, while in a state of health, numerous pathological occurrences daily present themselves to us, where the contractility of the muscles is obviously influenced by the state of the nervous system, or by agents which can only operate through its means. Other considerations of less weight, or founded upon less certain grounds, have been urged by the neurologists, but what I have stated seems to me to constitute the most important part of their reasoning, and so convincing did it appear, that their hypothesis has been continually gaining ground, until it came to be almost universally adopted.[5]

[5] See Dr. Alison's remarks in the Quarterly Journal, v. ix. p. 106; also the Report made to the French Institute, referred to above, p. 293. Cuvier says, that the objections to Haller's doc-

The opinions of physiologists on this disputed question were much influenced in favour of the hypothesis of the neurologists by the very elaborate and ingenious train of experiments which were performed by Legallois, to which I have already had occasion to refer. Haller had shown that the heart can continue its action when it is detached from the nerves that proceed to it directly from the brain, and had thence concluded that it is not under the influence of the nervous system. Legallois, however, endeavoured to prove, that if, besides the removal of the brain, we also destroy the spinal cord, the motion of the heart immediately ceases, and thus, by establishing, as he conceived, a necessary connexion between the action of the heart and the spinal cord, he controverted the argument that had been deduced by Haller in favour of the independent contractility of the muscles. The experiments by which Legallois supported his opinion were apparently so direct and unequivocal, as to gain very general assent to his doctrine, and were supposed completely to subvert that of Haller, when the subject was taken up by Dr. Philip, who, by repeating and modifying the experiments of Legallois, detected a source of fallacy in them, which, as far as this question is concerned, entirely destroys their value, and subverts the conclusion which had been drawn from them. Legallois had announced that when the spinal cord is destroyed, every part of the body loses its contractility, but Dr. Philip discovered

trine are every day becoming more apparent. Leç. d'Anatomie comp. Intr. p. 22.

that this is only the case when the cord is destroyed suddenly; on the contrary, when the destruction is effected slowly and gradually, he found that the heart was capable of continuing its contraction,[6] and hence he infers the correctness of Haller's doctrine of the inherent contractility of the muscular fibre. And, indeed, if we allow the accuracy of Dr. Philip's results, respecting which there appears no reason to entertain any doubt, they afford us the most direct confirmation of Haller's doctrine of muscular contractility being a faculty which exists independently of nervous sensibility.[7]

Besides establishing the general fact, that the heart retains its contractility after it has been separated both from the brain and the spinal cord, Dr. Philip performed some further experiments for the

[6] Experimental Enquiry, p. 88 and 97.

[7] In order to obtain a clear and comprehensive knowledge of the experiments and reasoning which were adduced in the course of this discussion, which may be considered as among the most curious and important that ever engaged the attention of physiologists, the following works should be carefully perused. Legallois' work, "Experiences sur le Principe de la Vie," which contains his original experiments; Dr. Philip's two papers in the Phil. Trans. for 1815, in which he urges his objections to Legallois' conclusions, and states the main facts on which he founds his own opinion; his "Experimental Enquiry into the Laws of the Vital Functions;" and his series of papers in the Quarterly Journal, v. xiii. and xiv. consisting of his general conclusions and a digested summary of his doctrines. A very correct and elegant summary of the experiments of M. Legallois and Dr. Philip, and the conclusions which may be deduced from them, is given by Dr. Roget in the supplement to the Encyclopædia Britannica, article "Physiology."

purpose of proving that the contractile power of the muscles is independent of the nervous influence. The experiments consisted in employing the corresponding muscles of the two extremities of an animal, the nerves of one of which were divided, while those of the other remained entire. Both the sets of muscles were then thrown into strong contractions by the direct application of a stimulant, when it was found that the extremity in which the nerves were left entire lost its contractile power as soon or sooner than that in which the nerves were divided.[8] This result appears fully to justify Dr. Philip's conclusion, that contractility is an inherent power of the muscles, and does not depend upon any thing which is conveyed to them through the media of the nerves.[9] About the same time when Dr. Philip was engaged in his experiments to prove the independent contractility of the heart, some valuable observations upon the same subject were made by Mr. Clift, which led him to the same conclusion. They were made on the carp, and it was found the heart of this animal is capable of supporting its contractility for some hours after its complete separation from the brain and spinal cord.[1]

In concluding the review of this controversy we

[8] Experimental Inquiry, p. 99. [9] Phil. Trans. for 1815, p. 89.

[1] Phil. Trans. for 1815. The important and decisive experiments of Mr. Brodie, which have been already referred to, although performed with a somewhat different object, bear very directly upon this point. Phil. Trans. for 1811, p. 36, et seq. See also Mr. Mayo's Comment. p. 16.

may observe, that, on arguing upon this subject, writers in general have not sufficiently attended to a point which was very explicitly stated by Haller himself, that the question is not whether the nerve generally intervenes between the action of the stimulant and the contraction of the muscle, but whether its intervention can, in any instance, be dispensed with.[2] I have already had occasion to describe the process by which volition is propagated from the brain down the nerve to the muscle, and in all these cases it is sufficiently obvious that the action of the nerve is an essential part of the process. And this is not only the case in all voluntary motions, but also in all those motions, either voluntary or involuntary, where the stimulant is not directly applied to the part that is ultimately moved. Here the transmission of the effect is always through the nerve, and it appears, moreover, that in many instances it is impossible to produce the same effect in any way, except by means of the nerve. A great number, however, of the most important actions of the body are performed in consequence of the direct application of stimulants to the part that is to be moved. Of this description are the vital functions generally, where different substances, partly as it appears by their mechanical bulk, and partly by certain specific properties, cause the con-

[2] Whytt, a writer of considerable acuteness and information, argues upon the principle, that as the intervention of the nervous system is necessary for the performance of certain muscular motions, it must be so in all cases. See the Essay on Vital and Involuntary Motions, passim.

traction of the muscular fibres to which they are applied. These operations are most of them involuntary, apparently placed out of the direct influence of the nervous system, and seem to be examples of the excitement of muscular contraction in consequence of the direct application of a stimulus. We accordingly find that this question has been much embarrassed by not attending to the difference between the voluntary and the involuntary muscles, a difference which was seldom adverted to by the earlier physiologists, although not totally unknown to them. This difference will be more fully explained when we discuss more particularly the nature of volition; but I may remark in this place, that although there are certain muscles that act habitually in both ways, and others which may, on certain occasions, act in a manner different from that which appears to be their ordinary mode, yet that there is a decided difference between the two classes, and that this distinction depends, in a considerable degree, upon the quantity of nerve that is sent to them, and the source whence the nerves are derived. The organs of voluntary motion are more plentifully supplied with nerves, and they are derived more or less directly from the brain and the spinal cord, while the involuntary muscles have fewer nerves, and those, for the most part, proceeding immediately from the intercostal nerve, or from some of the numerous ganglia with which it is connected.[3] The ordinary motions of these parts are

[3] Bichat, Anat. Gen. t. ii. p. 405; see also Johnstone on the Ganglia, sect. 2.

not only independent of volition, but they are without consciousness, which always attends the contractions of the voluntary muscles, and they may be generally said to want those characteristic circumstances which mark the intervention of the nervous system, or which indicate it to have any connexion with or co-operation in the effect produced.

In discussing this question we are therefore entirely to discard the consideration of the voluntary muscles, and to disregard all the experiments that have been performed upon them, as it is admitted on all hands that they are placed under the direct control of the nervous system.[4] The only subjects of controversy

[4] Dr. Philip, with his usual clearness and sagacity, has pointed out the difference which exists between the voluntary and the involuntary muscles with respect to the cause which stimulates them into action. This, in the former, he states to be the act of volition *alone;* but this limitation, I conceive, to be liable to certain exceptions, although it is obvious that the will is their appropriate and ordinary stimulant. See "Enquiry," p. 101. If we refer to the classification of the nerves which is stated above, we should say that the voluntary muscles possess both perceptive and motive nerves, the involuntary muscles possess no motive nerves, and it is probable that their nerves are principally the simply sensitive, with a small proportion however of the perceptive. This consideration may lead to the conjecture, that the electric fluid is not capable of acting upon the nerves which serve for the purpose of simple sensation, but that both the perceptive and the motive nerves are under its influence. Is it not probable that the small degree of perception which the involuntary muscles possess is not employed in their ordinary action, but serves only to convey to us the knowledge of any unnatural or morbid state of the parts, and that the ganglia are the centres to which these perceptions are referred?

are the involuntary muscles, and here the fact seems to be, that we have on one side the most decisive proof that these muscles, such for example as the heart, can act when entirely cut off from all connexion with the nervous system, while, on the contrary, we have no less decisive proof that they are capable of being influenced by causes which can only operate through the intervention of the nerves. This apparent difficulty may be removed, as Dr. Philip has clearly shown, by making a distinction between the ordinary and the extraordinary action of these parts. Under ordinary circumstances the heart contracts merely by the stimulus of the blood, in consequence of the inherent contractility of its fibres, but upon certain occasions it is liable to feel the influence of passions and other mental emotions, which can only be conveyed to it through the intervention of the nerves.[5] What determines the operation of these extraordinary circumstances, and how they are connected with the involuntary muscles, will be the subject of future consideration; but in the mean time the facts must be admitted, and they seem to offer an easy explanation of the difficulty which has so long embarrassed this question.

By not attending to this distinction between the voluntary and involuntary muscles, the greatest part of the experiments on the artificial application of stimulants, which had been regarded as among the most decisive arguments in favour of the doctrine of the

[5] Exper. Enq. p. 79.

neurologists, will be found to be totally irrelevant to the question. In the experiments of this kind the application has generally been made to the voluntary muscles, which are not properly the subject of the controversy. If, on the contrary, the involuntary muscles be employed, we shall obtain results that favour the Hallerian hypothesis. From obvious causes it is not so easy to perform experiments on the involuntary as on the voluntary muscles, but upon the whole it seems tolerably well established that the nerves which supply these parts are not affected by the application of artificial stimuli, or rather will not admit of the transmission of the effect to distinct muscles, in the same manner with the nerves that go to the involuntary muscles. The experiments with the galvanic apparatus, which are the most easily performed, lead to this conclusion, either that the involuntary muscles are absolutely incapable of being stimulated by the application of electricity to the nerves, or at least in so small a degree as bears no proportion to the size of the nerves, or to the contractility of the part when the stimulus is applied directly to its substance.[6]

[6] For the effect of galvanism on the involuntary muscles, see my Essay on Galvanism, p. 49; Bichat, sur la Vie et la Mort, p. 120: Bichat, indeed, states the results of his experiments in a general way only, yet we may regard his statement as sufficient to prove the different effect produced on the two kinds of muscles; also Dr. Paris's observations on the effect of electricity on the action of the heart, Med. Juris. v. ii. p. 32. I must, however, observe that Dr. Park, arguing from the same premises, is led to form an opposite conclusion. Quart. Journ. v. ii. p. 233. Because

The final result of the controversy appears therefore to be in favour of the doctrine of Haller, although in admitting it we are obliged to introduce certain modifications, which did not enter into his original hypothesis.[7]

§ 5. *Arrangement of the Functions.*

These observations upon the connexion which subsists between the action of the muscles and the nerves will enable us to decide upon the plan which we ought to adopt in the arrangement of the functions. The older physiologists, and many of the most eminent among the moderns, have classed them under the three divisions of animal, vital, and natural; the first signifying those which are specifically attached to animal existence, such as immediately depend upon sensibility and contractility, including the various forms of sensation and spontaneous motion; the vital, denoting those that are directly concerned in the continuance of life, such as the action of the heart and lungs; and the third class, the natural, intended to

it has been found in these experiments that the involuntary muscles are very slightly affected by the galvanic influence, he contends that there is no specific difference between them and the voluntary muscles. But in answer to the objection of this intelligent physiologist, I reply, that the extremely delicate sensibility of the one class, and the small share of it possessed by the other, bears no proportion to the quantity of nerves respectively sent to them, and can only be explained upon the supposition of some essential difference in the properties of the nerves themselves, or in their relation to the muscles.

7 Philip's Enquiry, p. 103.

express those which are not necessary for the immediate continuance of life, but which serve to maintain the body in its proper condition, and to support all the other functions; in this class are placed digestion, secretion, and absorption. There is a foundation for this division, but it is in some respects faulty, especially in there not being any precise limits between the two latter classes, the vital and the natural; and with respect to the nomenclature of the whole it is obviously incorrect. In the first place, every one of the functions may be strictly called animal, for in every one of them the operation either of sensibility or of contractility is immediately concerned. The animal functions are also strictly entitled to the appellation of vital, for not only are they directly essential to the support of life, but they are likewise the most characteristic of its presence; with respect to the term natural, as applied to any description of functions, it is clearly without any specific meaning.

Several new classifications of the functions have been lately proposed in France by physiologists, whose works, although of various characters, have each of them obtained a considerable share of reputation; of these the most important are Dumas, Richerand, Bichat, and Cuvier. The first of these writers, whose productions are more distinguished by the popularity of their style than by scientific research, divides the functions into four classes; in the first he places those which serve to connect the individual with external objects, under which he includes nearly the same operations, which in the old arrangement were styled the animal functions, spontaneous motion, sensation, and the action of the external senses. In the second class he places the functions which maintain the body in its natural condition, considering it as a compound of solids and fluids, possessing the properties of cohesion, consistency, and temperature; here are placed the circulation, respiration, and the faculty of generating heat. The third class contains those functions, the object of which is to preserve the composition of the body, and the properties immediately connected with it; among these are digestion, assimilation, sanguification, secretion, and absorption. In the fourth class are placed the functions which refer to the connexion that subsists between different individuals, as well those which serve for the continuance of the species, as those which connect mankind together by their moral agencies. Considered as a physiological arrangement, the plan of Dumas is decidedly bad; the second and third classes are separated by a distinction which is not easy to comprehend, and the functions which are placed together in the last class, whatever connexion they may have in a metaphysical sense, have no physical connexion with each other.

Richerand's method is much more simple; he divides the functions into two classes only; those that are subservient to the preservation of the individual, and those that are subservient to the preservation of the species. But this arrangement is not derived from any natural principle of classification, and it presents the practical objection of throwing almost all the functions into one of the divisions. Bichat, in this as in most other cases, has disregarded, in a great measure, the opinions of his predecessors, and founded his system upon a new principle, which appears more philosophical, although not without considerable objections. He places all the functions in two classes, to which he gives the names of animal and organic, the first coinciding nearly with the animal functions of the older physiologists; the organic embracing both their vital and natural functions. The author himself characterizes his animal functions as those which distinguish the living animal from all other beings, those which enable him to maintain his connexion with the external world, originating from, or immediately consisting in, the faculties of sensation and locomotion. The organic functions are those which serve for the support of the body individually, without any reference to surrounding objects, which are performed by certain organs that are similar, or at least analogous to what we find in every being that is possessed of organization. This division is preferable to the old one, in being more simple, and in not attempting to discriminate between the vital and natural functions, which are so closely connected together. But I conceive that it is not unexceptionable, either with respect to the actual nature of the functions, or to the names by which they are designated. All the functions are properly animal, for they are all sufficiently distinct, both in their prime cause and their phenomena, from any which are exercised by vegetables, and they are all entitled to the name of organic, for the brain and muscles are as properly organs of sensation and motion as the heart and lungs are of the circulation and the respiration.

The arrangement of Cuvier may be regarded as compounded of those of Richerand and Bichat. He divides the functions into three classes—animal, vital, and generative; the first coinciding with the functions so named by Bichat, and the second being also similar to his organic functions, except in separating those that are subservient to generation. This method labours under the same objections with that of Bichat, although it is perhaps somewhat improved by the separation of the generative functions, which could not with propriety be included under the organic, according to the definition which he gives of them.[8]

Besides the objections that have been urged to these different arrangements, considered individually, there is one that attaches to them generally, as well as to all the others that have been proposed, that they do not make a sufficient distinction between the functions immediately resulting from the action of the nervous system, as forming one of the constituents of the body, and the mental or intellectual operations. The brain is indeed the instrument of the mind, or the medium through which the mind acts, yet it does not necessarily follow that the mind is to be regarded as a quality or property of the brain. But whatever

[8] It may be worthy of observation that Cuvier, who is so eminently distinguished for his sagacity in classification and arrangement, should have described sensibility and muscular contractility as both of them functions of the nerves. Regne Anim. p. 33.

may be our opinion upon this point, we may without hesitation assume that the functions which result from the immediate action of the brain and nerves are altogether so different from those of the mind or intellect, that notwithstanding the connexion which subsists between them, they should be placed in distinct classes. Perhaps one cause why writers on physiology have not made separate classes of these two orders of functions may be, that they supposed the former of them only to be the proper objects of their attention, and that the mental or intellectual powers belonged exclusively to the science of metaphysics. This, however, is no ground for employing a defective arrangement, and it may be farther remarked, that although the object of metaphysics be very different from that of physiology, yet the operations of mind and body are so intimately connected together, and the one part of our frame has so much power over the other, that no system of physiology can be perfect, in which the mental faculties are entirely disregarded.

The mode of viewing the subject which appears to me the most correct, is to regard contractility and sensibility as the two primary attributes of animal life, each equally characteristic of it and peculiar to it, and each performed by its appropriate organs. The functions depend upon the exercise of these powers, and although probably in all cases they are both of them exercised, yet generally one of them seems to be the principal agent, or the prime origin of the ensuing operation; we may consequently divide them into the contractile and sensitive functions, or those which more directly belong to contractility and to sensibility, and which of course serve respectively for motion and sensation, and to these two classes, according to the remarks that have been made above, must be added the third class of the intellectual functions.

The contractile functions must be considered in the first place, as being the most independent, and what may be regarded as the prime cause of the others, for we have seen that the muscular system can act without the intervention of the nervous, but that the nervous is directly dependant upon the muscular. The heart and the involuntary muscles continue to contract, and the limbs remain sensible to the impression of stimulants, after the brain and spinal cord are totally destroyed, but if the circulation and the respiration be stopped, the nervous power is immediately extinguished.

Among the contractile functions, the essence of which consists in motion, the first in point of importance, and the one which may be regarded as the most necessary to the direct support of life, and to the indirect maintenance of all the rest, is the circulation. In all animals, whose functions and mode of existence bear any considerable analogy or resemblance to the human subject, we observe that the fluid which serves for the support of the system, and from which the materials for its nutrition are immediately derived, is kept in continual motion. In the more perfect animals the great source of motion in the circulating fluid is the heart, but as we descend in the scale of organization, the heart becomes of less relative importance, and a part of its power is supplied by the action of the great vessels, until at length we arrive at an order of animals, where its power is entirely superseded by that of the large arteries, while in the lowest tribes, which possess any thing analogous to a sanguiferous system, the large arteries themselves are not to be distinguished, nor is there any regular progression of the fluids, and we can observe nothing more than a general oscillatory motion in them, which appears to be equally extended through all parts of the body. It must, however, be observed, that the perfection of the circulating system does not hold an exact ratio with that of the organization generally. Many insects that have various distinct organs, and a variety of delicate functions, have no circulating system,[9] and the office of the heart and arteries is supplied by parts which act upon a different principle.

Next to the circulation, in point of importance, comes the respiration, the function by which the fluid that is carried along the vessels is adapted for the purposes of life, by a certain change which it experiences from the action of the air. This function, in some form or other, is probably exercised by all classes of animals. The same kind of complicated mechanism, which exists in the higher orders, is not indeed found in a great number of the inferior tribes, but in them we may trace something which is to be regarded

[9] Lamark, Hist. des Anim. sans Verteb. Intr. p. 149.

as a substitute for it, or as supplying its place, by enabling the air to produce its appropriate action on the nutritive fluid. After these two functions, one by which the blood is carried to all parts of the body, and the other by which it acquires its vital properties, we come to those of calorification, secretion, digestion, including assimilation and sanguification, and absorption, functions which serve, in some way or other, for the continuance of the action of the animal machine, and which preserve all its parts in their proper condition, without, however, being essential to the immediate support of life. In this class we may place the function of generation, which, although one of the most inexplicable of all the operations that are performed by the animal powers, and acting in a specific manner, of which we have no other example, may be considered as essentially consisting in secretion.

Although I have spoken of some of these functions as being more essential to life than the others, still, when we take a general survey of the animal œconomy, we find that they are all of them intimately connected together, acting, as it were, in a kind of circle. The circulation has been stated to be the prime cause of all the rest, for it is that which carries to every part of the body the fluid which endows it with its vital properties and its appropriate powers. The heart, however, would be incapable of fulfilling its functions were not its fibres furnished with a regular supply of blood, which has been carried through the lungs, and is acted upon by the air in the process of respiration, which transmission of blood through the lungs is itself effected by the action of the heart. If we conceive the heart and lungs, or some organ equivalent to them, to be in action, so that the blood may receive its proper alteration from the air, and may be carried to all parts of the body, we have the essentials of life, and are in possession of those functions by which existence may be maintained, and the individual preserved, for a certain length of time, in a perfect condition. Both the solids and the fluids of the body are, however, in a constant state of change. One essential office of respiration is to discharge a quantity of carbonaceous matter from the lungs, and the blood, after it has passed through the vessels, is incapable of continuing its office without a supply of fresh materials. The heart itself, the great source of all the motions of the body, and the centre of the circulating system, would lose its powers, were not its substance gradually renewed, and the waste that is thus going forwards from various causes counteracted by the functions of digestion and assimilation. But for the performance of these functions it is necessary that certain substances should be separated from the mass of blood, constituting the process of secretion, and, after the future nourishment is prepared and elaborated, absorption is necessary to carry it to its proper receptacles. All these functions are therefore as necessary for the continued support of the body, as the circulation and respiration are for its momentary existence, but they are so in a less direct manner, and are connected more through the medium of certain mechanical and physical changes, than in consequence of any changes which seem immediately connected with the specific powers of the system.

These remarks on the relative importance of the different functions must be understood only as they regard man and those animals that the most nearly resemble him in their organization. As we examine the various classes with respect to each other, taking in the whole of the scale, from the most perfect to those that are the least so, we observe a very different order to prevail respecting the connexion between the functions and their relative degrees of importance. Beginning from the human species, and descending by a gradual progression, we find that the intellectual functions are the first that disappear, the sensitive also become fewer in number or more contracted in their operations, and, according to the opinion of the most learned naturalists, many of the lowest tribes of animals are entirely without a nervous system, and in consequence are deprived of the powers that depend upon it. Among the contractile functions, the circulation, which we have considered to be the most important in man, is the one which is first found to be wanting among the lower animals; the organs of respiration, secretion, absorption, and generation, gradually become less and less distinct, until at length they can be no longer traced, while the last that remain are those of digestion, although there is a considerable number of animals in whom no distinct apparatus even for digestion can be detected. The power of producing their species is also absolutely essential to animal existence, but the manner in which this is accomplished in the most simple animals is totally different from the function of generation as exercised by the more complicated, and in the former no distinct organ for this purpose can be discovered. According to Lamark, who has minutely attended to the gradations of animal existence, the organs of the several functions, when considered with respect to their relative necessity for the absolute support of life, and the universality of their occurrence, will stand in the following order; the organs of digestion, those of respiration, of motion, of generation, of perception, "sentiment," and lastly of the circulation.[1]

The sensitive functions may be divided into two classes; first, those which originate in the action of the external agents upon the nervous system, and, secondly, those of a reverse kind, which depend upon the re-action of the nervous system upon these agents. In the first of these divisions are included what we call the external senses, the sight, hearing, taste, smell and touch, and in the same division must be placed the sensation of hunger, that of temperature, and some others, which have not been correctly discriminated from general feeling, but which possess specific characters. In the second class, those functions which depend upon the re-action of the nervous system on external bodies, we must place volition, and to the same class we may also refer instinct, association, sympathy, habit, and some other faculties of a similar kind, which appear to hold, as it were, an

[1] Anim. sans Verteb. t. i. p. 360.

intermediate rank between the corporeal actions and those of a purely intellectual nature. As the functions which compose the first of these classes may be all referred to a species of perception, so the latter may be considered as more or less analogous to volition; in the former, the effect upon the nervous system, whatever it be, is propagated upwards from the extremities to the centre, in the latter it proceeds in the opposite direction, from the centre to the extremities of the body.

The intellectual functions form the third class; these are a less direct object of physiology than the two former, yet many of them are so closely connected with the physical changes of the body as to require some degree of notice in a system which professes to give a complete view of the animal œconomy. These, although intimately, and, as it would appear, necessarily connected with the nervous system, are, at the same time, so different in their phenomena and their characters from any of the properties of matter, that I conceive we are warranted in the conclusion that they originate from an essentially different source, and are of an essentially different nature. Whatever hypothesis, however, we may adopt upon the subject, it is obvious that they possess the power of acting upon matter, and that they exercise a very extensive influence over the animal body, and so far as this influence extends, it will fall under our department to investigate its nature and to trace its effects. Among these intellectual operations, which possess a decided action upon the corporeal frame, we must place the passions, and in the same class we may regard that compound of mental and physical influences, from which results what we call temperament and character. We hence proceed to functions of a more purely intellectual kind, which, as they recede from the corporeal, and advance towards the mental part of our frame, are less within our province, and belong more to the moralist or the metaphysician.

CHAP. V.

OF THE CIRCULATION.

§ 1. *Introductory Observations.*

I HAVE now described in succession the principal ingredients of which the body is composed; the membranous matter, the bones, the muscles, and the nervous substance; and I have likewise given an account of the two general properties which distinguish animals from all other beings, contractility and sensibility. I must now proceed to the different functions which are individually exercised by the particular organs of the body, which, all of them, consist either in motions brought about by the contraction of the muscular fibres, in sensations produced by the appropriate actions of the nervous system, or in peculiar affections of certain parts of this system, which are connected with the various intellectual operations. According to the arrangement which I proposed in the last chapter, I shall begin with the contractile functions, those which more immediately depend upon the contractility of the muscular fibre, because they seem to be the most essential to the existence of the animal frame, and because we conceive ourselves to be better acquainted with their nature, and regard them as being more analogous to the other powers of matter, than the functions which depend upon the operations of the nervous system.

I have already made some remarks upon the connexion of the functions with each other, and upon their relative importance to the support of animal existence, and we are led to conclude, that in the higher orders of organized beings, the circulation of the blood seems to be, as it were, the main spring of all the rest, that from which they derive their origin, and which is the most essential to the well-being of the whole. Respiration, in the most perfect animals, is, indeed, as essential to their existence as the circulation, but, if we may be allowed the expression, it is only incidentally necessary, inasmuch as by respiration we produce that change in the blood which gives the heart its power of contraction. It appears to follow as a direct consequence of Mr. Brodie's interesting experiments, that if the blood had its specific change induced upon it by any other means, or were it exposed to the action of the air in any other manner than by passing through the lungs, all the functions would go on as at present without interruption,[2] whereas, if the circulation be impeded or suspended, every part of the system, and every one of the functions, immediately feel the effect. This observation is, however, strictly applicable only to a part, although that a large part, of the animal kingdom. As I have remarked above, there is a numerous class, and that possessed of a considerably complicated

[2] Phil. Trans. for 1811, p. 36, et seq.

organization, which has no circulation of the blood, and yet in these the nutritive fluid is acted upon by the air in a way which may be regarded as analogous to respiration. But the general structure of these animals, and the nature of their functions, bear so little analogy to that of man, as not to allow of their being compared to each other, or considered as merely occupying different gradations in the same scale.

With respect to the other contractile functions, I may remark that, however necessary a certain temperature may be to the existence of what are called the warm-blooded animals, who, being generally immersed in a medium colder than themselves, require some apparatus for generating or evolving caloric, in order to supply this deficiency, yet this may, in like manner, be regarded as rather incidental than essential, and what is more dependant upon the peculiar circumstances in which they are placed, than upon any thing necessarily connected with the support of life. The functions of digestion, absorption, and secretion, are evidently still less subservient to mere existence, their object being either to supply materials for the growth and mechanical support of the body, or to mould and fashion its form, while generation is obviously unconnected with the life of the individual, and is only useful as a means of perpetuating the species.

If we take into consideration the relative importance of the contractile and sensitive functions, or of the heart and the brain, as being the respective centres of each, so far as regards mere animal existence, we shall also be led to decide in favour of the former. In the higher orders of animals indeed, where there is the greatest number, and the most perfect development of the organs and functions, the brain and the heart may, at the first view, appear to be equally essential, not only to the continuance of their full powers, but even of life itself. Upon a more accurate examination of the subject, however, we shall find that the heart is the centre, not of the contractile powers alone, but of the whole of the corporeal frame, a conclusion to which we are led, both by anatomical researches, and by the nature of the powers and functions respectively exercised by these parts.

When we attempt to trace the progress of an organized being, from its earliest stage of existence to its full maturity, the first appearance that we observe of any arrangement of parts consists in the rude sketch of what is afterwards to become the organs of circulation. We are informed by Harvey, who accurately observed the gradual development of the different parts of the embryo in the chick during incubation, that the first appearance of distinct organization was a beating point, punctum saliens, as he expresses it,[3] which was the rudiment of the future

[3] Quarto itaque die si inspexeris........ punctum sanguineum saliens emicat. De Gener. Exer. p. 17. Haller observed the pulsation of the heart at a considerably earlier period; see Comment. de Form. Cord. in Op. Min. t. ii. p. 101, and Comment. de Form. Pulli, c. 9. "Deinde hora 42 et cor vidi et aortam, et motum vertiginosum quasi, sagittæque similem, sanguinis rubigi-

heart, and which preceded the formation of the other parts of the body, being visible for some time before he could discern any trace of the brain. The existence of acephalous fœtuses affords a further confirmation of the same opinion; as these beings are absolutely without brains, so it is certain that they cannot possess any share of those powers which are derived solely from this organ; yet they have grown to their full size in the uterus, and have even lived for some time after they have been expelled from it, and their death has appeared to be owing, not to any physical impossibility to the continuance of life, but to their not being able to effect those changes which are, as it were, incidentally necessary to the continuance of an existence of any considerable duration. For example, a regular supply of nutritious matter is essential to the support of life; this can only be supplied by the introduction of food into the stomach, and food can only be received into the stomach by the act of deglutition, but this act, at least in the higher order of animals, cannot be performed without the intervention of the nervous system.

Then with respect to the dependance of these two parts upon each other, the view which has been taken of the nature of their powers, and the manner in which they are exercised, lead us to the same opinion. The very existence of the brain, as composing part of the substance of the body, necessarily implies the

nosi ex corde sursum projecti, iterumque relabentis." Op. Min. t. ii. p. 269. See note in p. 297.

conveyance of the blood or some analogous fluid for its formation and support, while, on the contrary, it does not appear that the mechanical contraction of the heart, or the means, whatever they may be, by which the fluid is carried to the brain, is necessarily connected with the exercise of any sensitive function. These considerations, and others of a similar kind, both anatomical and physiological, all conduce to the conclusion, that we must regard the heart as the centre of the whole corporeal frame, the fountain of life, which is designed to pour out its vital streams to every part of the system, and to unite the various functions, however different in their nature and operations, into one harmonious whole.[4]

§ 2. *Description of the Heart and its Appendages.*

The organs of circulation may be divided into three parts, as connected with their structure and their uses, the heart, the arteries, and the veins. The heart is a hollow muscle, composed of masses of strong longitudinal fibres, forming an irregular cone, and leaving an internal cavity. The outside of the heart

[4] Sœmmering has adduced various considerations, which appear conclusive as to the point, that the nervous system is not necessary to the mere continuance of life. Corp. Hum. Fab. t. iv. § 87. The same doctrine is the necessary deduction from the decisive experiments of Mr. Brodie and of Dr. Philip, which have been already referred to, and is confirmed by some experiments that are related in the posthumous work of my much respected preceptor, Dr. Marshall, Anatomy of the Brain, p. 249, et seq.; and likewise by those of Mr. Mayo, Comment. p. 16.

is covered with a firm membrane, and the internal cavity is lined with the same substance, the muscular part is copiously supplied with blood-vessels, but its nerves are considered as few in number in proportion to its bulk.[5] It is suspended from its base by the great blood-vessels, which form the main trunks of the sanguiferous system, and it is enclosed in a membranous bag called the pericardium; it is situated in the left side of the fore-part of the thorax, resting upon the diaphragm. The interior of the heart is unequally divided, by a strong muscular septum, into two distinct cavities, called ventricles, which have no direct communication with each other; there are also two membranous bags at the base of the heart, called auricles, forming in all four separate cells, each of the auricles communicating with its corresponding ventricle, but the auricles, as well as the ventricles, having no direct communication with each other. Although the auricles may be considered as membranous bodies compared with ventricles, yet they are furnished with numerous fibres, and possess the power of contraction.

In describing the different parts of the heart,[6] it is customary to speak of its right and left sides, and of the right and left auricle and ventricle, but it is well known that the terms are not correctly applicable to the situation of these cavities in the human body,

[5] Vide Supra, p. 304.

[6] Perhaps one of the most ample and correct anatomical descriptions of the heart will be found in Bichat's Anat. Des. t. iv. p. 87, et seq.; see also Boyer, Anat. t. iv. p. 277.

which, as far as its situation is concerned, are more accurately designated by the words anterior and posterior. The terms right and left were originally employed by the ancients in consequence of their dissecting brute animals, in which the heart is placed differently from what it is in the human subject, and corresponds generally with the terms that were employed. This affords one proof, among many others of a similar nature, that when Galen and his successors described the anatomy of man, their descriptions were borrowed, at least in a great measure, from the different species of simiæ, which, in consequence of the superstition and prejudices of the age, they were obliged to substitute for the human body.[7] With respect to the names which we attach to the cavities of the heart, perhaps, upon the whole, the most unexceptionable terms, and those which are the least likely to lead to any erroneous conceptions, are pulmonic and aortic, those which are usually called right being immediately subservient to the pulmonic, and those called left to the aortic circulation.

The use of the heart, as forming a part of the circulating system, is to receive the blood from the veins and to propel it again through the arteries; this is accomplished by the contraction of its fibres, by which the cavities of the heart are diminished in size, and their contents necessarily forced out. The simple diminution of the cavities, and the mere pressing out of the blood, would not, however, be sufficient for the

[7] Haller, El. Phys. t. iv. 2. 3.

purpose of the circulation; for it is not only necessary that the blood be moved, but that it be moved in the right direction. For this purpose the heart is furnished with an elaborate mechanism of valves, which are attached to the orifices of the ventricles and the mouths of the arteries, and which are so constructed, that when the heart contracts, and the blood is forced out, the current is necessarily propelled in the proper direction.

When the blood leaves the heart it is sent with considerable force into the large trunks of the arteries; these vessels soon begin to ramify in different directions to all parts of the body, until at length they are reduced to vessels too small to be traced by the eye or even by the microscope. The arteries, which perform this office of conveying the blood from the heart, are flexible, elastic tubes,[8] principally composed of membranous matter formed into distinct layers, and composing what have been called the coats of the arteries. Of these membranous coats anatomists usually describe two, as possessing a sufficiently determinate structure to be easily distinguished from each other; the outer one partaking more of the nature of the cellular texture, and therefore called the cellular coat;[9] and an

[8] It may be necessary to observe that, according to the observations of the most accurate anatomists, the arteries are not perfectly cylindrical, but conical, the narrower end of the cone being situated towards the heart. Hunter on the Blood, p. 168, et seq.

[9] Some anatomists have been disposed to regard this outer coat as merely a continuation of the cellular substance which is continued over all the body, and connects together its different parts, and have therefore conceived it to be not essential to the existence of

inner membrane, white, firm, and smooth, possessing more of the physical properties of tendon. In consequence of the erroneous notions which formerly prevailed on the subject of the white parts of the body, to which I have already alluded, this latter was named by the older writers the nervous coat, a name which has still been applied to it by some of the moderns, but it is sufficiently designated by the name of the interior or innermost coat.

Between these membranous coats is situated a stratum of transverse fibres, which have been termed the muscular coat: this has been supposed, like other muscular parts, to possess a contractile power, and to give the artery the capacity of alternately contracting and relaxing, thus assisting the heart in the propulsion of the blood.[1] To this alternate change in the capacity of the arteries the pulse has been commonly ascribed, and the sense of pulsation which the artery gives to the finger, when applied to certain parts of the surface of the body, has been supposed a sufficient proof of the existence of the arterial dilatation, but this point will be considered more fully hereafter. The mouths of the two great arteries, which receive the

the proper arterial structure, an opinion which I am disposed to consider as correct, but I have thought it desirable to employ, for the present, the ordinary phraseology. See some judicious observations in a Review of Beclard's Additions to Bichat; Ed. Med. Journ. t. xviii. p. 258.

[1] The nature of these transverse fibres, and the question whether they are properly entitled to the appellation of muscular, and possess a proper contractile power, will be considered hereafter.

blood as it is projected respectively from the two ventricles of the heart, are each of them furnished with a system of valves, by means of which, when the blood once enters the arteries, it cannot return into the heart, but is necessarily forced towards the extremities.

When the blood has been transmitted by the arteries over all parts of the body, it is returned again to the heart by the veins, being first received by their minute extremities, and carried from smaller to larger branches, contrary to what takes place in the arteries, until at length it arrives at the large trunks, and is poured from them into the heart. The veins are membranous tubes like the arteries, but they differ from these in possessing a less firm texture, in being nearly without the transverse fibres, and in having a number of valves in different parts of their course; whereas the arteries have no valves except at their commencement.

After this brief and general sketch of the organs of circulation, the next object will be to trace the blood through its whole progress, beginning at one part of the circuit, and following it until it arrives again at the same point. But before I enter upon the description, I must observe that the blood in fact makes two circulations before it absolutely completes its course, being, between the two, brought back again to the heart, although not to the same part of this organ. This double circuit depends upon the circumstance, that by the circulation of the blood two distinct objects are obtained; by one of them the blood is sent into the lungs, and is there exposed to the action of the atmospheric air, by which its properties are changed, and it is adapted to the support of life. The blood, having thus acquired its specific vital properties, is returned to the heart, and is again sent out from this organ, along another set of vessels, to all parts of the body, except to the cells of the lungs, through which it had been transmitted in its former circuit. These two circuits have been distinguished by different appellations; from the extent of their course they have sometimes been called respectively the lesser and the greater circulation; or perhaps more appropriately, from the parts to which they are sent, the first has been called the pulmonic and the latter the aortic or systemic circulation.[2] The organs of the circulation have also been divided into the arterial and the venous parts, as depending upon the structure of the vessels and the mechanical purposes which they respectively serve. They have likewise been divided into the parts containing the red and the black blood, a division which proceeds more upon physiological than upon anatomical principles, and does not entirely coincide with the former. We shall find it convenient to use each of these divisions on certain occasions, employing one or other of them

[2] These are the terms employed by Dr. Barclay in his "New Anatomical Nomenclature," p. 176, a work which is justly entitled to the praise of ingenuity, but I think the proposed alterations are most of them unnecessary, and on that account very undesirable. The partial adoption of a new language in any department of science tends to embarrass the memory, and the general adoption of it would have the serious objection of rendering the old standard authors, in a great measure, unintelligible.

according to the objects in view, or the particular point which we wish to illustrate.

In tracing the progress of the blood through the heart and along the arteries and veins, I shall begin with that part where it is returned by the systemic veins, or those which belong to the greater or general circulation, into the right auricle of the heart. From the right auricle it is poured into the right ventricle; when the ventricle becomes distended to a certain extent, its fibres contract, and its cavity being thus considerably diminished, a proportionate quantity of the fluid which it contains is expelled. There is a valve, or set of valves, which, from its figure, as consisting of three principal divisions, has been called tricuspid, attached to the passage between the auricle and the ventricle, and so constructed, that, by the contraction of the ventricle, it closes up this orifice, and prevents the blood from returning into the auricle, so that it is necessarily sent forwards into the pulmonary artery, which likewise opens into the right ventricle. The pulmonary artery carries the blood through the lungs, in a way which will be more particularly described hereafter, along the lesser or pulmonic circulation; and, after it has undergone its appropriate change from the action of the air, it is returned into the left auricle by the pulmonary veins The same mechanical process occurs in the left side of the heart as I have just described with respect to the right; the ventricle contracts, a valve at its mouth, which, from its consisting of two principal divisions, is called the mitral valve, prevents the blood from returning into the auricle, and it is accordingly propelled into the aorta, the great systemic artery. When the blood has once entered the artery, it is prevented from flowing back into the heart by a set of valves called sigmoid or semilunar, placed at the mouth of the vessel, so that any motion which is afterwards impressed upon it, after it once enters the aorta, either by the succeeding portions of blood sent from the heart, by the action of the vessels themselves, or by any extraneous cause, must all have the effect of carrying the blood forwards from the heart into the veins, then from the smaller veins into the vena cava, the main trunk of the systemic veins, and finally depositing it in the right auricle.

§ 3. *History of the Discovery of the Circulation.*

A slight and casual observation of the phenomena of the living body was sufficient to prove that the blood is perpetually in motion, but the nature of this motion, or the course which it pursues, was unknown to the ancients.[3] They had many chimerical and unfounded opinions upon the subject, which it is not necessary to detail, although some of them were sanctioned by high authorities. As a specimen of their notions it may be sufficient to state, that they considered the principal office of the arteries to be that of conveying air or some kind of spirits to and from the heart, while the veins carried the blood; that the

[3] For an account of the opinions of the older anatomists on this subject, see Senac's Treatise on the Heart, Introd. p. 68, et seq.

fluids moved along the vessels in one direction during the day, and in the contrary direction during the hours of sleep, and various doctrines of a similar kind were maintained, either derived from incorrect or imperfect observations, or founded totally upon mere unauthorized hypotheses.

Some approaches to the true theory of the circulation were made by Servetus, or Servede, the celebrated victim of Calvinistic intolerance, and afterwards by the Italian anatomists, Colombo and Cesalpini, who flourished in the sixteenth century. It appears that they had each of them a pretty correct idea of the passage of the blood through the lungs, along the lesser circulation, and were even aware of its being acted upon by the air, during this part of its course, but, in other respects, their view of the subject was completely erroneous.[4] The honour of the grand discovery of the circulation, the greatest that was ever made in anatomy or physiology, is due to our illustrious countryman, Harvey. He completed the discovery about the year 1620, but, with a rare degree of philosophical forbearance, he spent eight years in digesting and maturing his ideas, when they were at length given to the world in a short tract, written with remarkable clearness and perspicuity, which is well characterized by Aikin, "as one of the most admirable examples of a series of arguments, deduced from observation and experiment, that ever appeared on any subject."[5]

[4] Haller, El. Phys. iv. 4. 17; Sabatier, Anat. t. ii. p. 255.

[5] See Harvey de Motu Cordis et Sanguinis Circulo.

The manner in which this discovery was received by the public forms a curious and interesting occurrence in the history of philosophy. Harvey, for some time, scarcely made a single convert, and an excessive clamour was excited against him, for having called in question the revered authority of the ancients. He fortunately lived in a country which had been favoured with the light of the reformation, otherwise it is not impossible that he might have shared the fate of Galileo, for some of his antagonists, when they found themselves foiled in argument, did not scruple to raise against him the weapons of superstition and prejudice, insinuating that his new doctrines would tend to subvert the credit of the scriptures, and thus undermine the foundations of religion and morality. After some time, however, it was found that Harvey's theory was true, and his opponents then commenced a different plan of attack. They asserted that the doctrine of the circulation, which had been brought forwards by him as a new discovery, was well known to the ancients, and passages were quoted and warped in a thousand ways to prove the allegation. It is asserted that, for some years, he even suffered in his professional practice from the prejudice excited against him; but by degrees the merits of his discovery began to be appreciated, and he lived long enough to witness the triumph of truth over the cavils of ignorance.

Harvey's doctrine of the circulation is now so universally admitted, that it might seem unnecessary to adduce any formal train of reasoning in its support. It may, however, be useful to review the nature of the arguments that were employed, as many of them consist of curious matters of fact, and throw considerable light upon the structure and properties of the sanguiferous system. If we open the chest of a cold-blooded animal, and bring the heart into view, we may observe its alternate contraction and dilation proceed with great regularity.[6] For a short space of time the heart lies at rest, and suffers itself to be distended with blood, then it is suddenly seen to rise up on its basis, to shorten its fibres, and to expel its contents. During this process it strikes the ribs, and produces what we call the beating of the heart, which does not depend, as is sometimes imagined, upon the distention of the heart from the blood which is received into it, but, according to the ingenious suggestion of W. Hunter, upon the injection of the blood into the curve of the aorta tending, to a certain degree, to straighten this part, and to raise up the apex of the heart by which it is brought into contact with the ribs.[7]

The passage of the blood along the arteries and the veins was first demonstrated to the eye by the experiment of Malpighi,[8] who, by applying the micro-

[6] Harvey, Exer. 1, cap. 2.

[7] J. Hunter on the Blood, p. 146, note. Harvey was well aware that the striking of the heart against the ribs did not depend upon its distention, p. 30.

[8] Malpighi informs us that he saw the circulation of the blood, by means of the microscope, in the membranous part of the lungs

scope to the web of a frog's foot, or other transparent membranous part, enabled us to behold the interesting spectacle of the arteries rapidly projecting the blood in successive waves towards their extremities, where it was received by the veins and returned in a uniform stream by their trunks. It must, however, be acknowledged, that this experiment, although a peculiarly beautiful one, can scarcely be regarded as proving more than the mere passage of the blood through the arteries and the veins, and the circumstance of the pulsation being confined to the former of these vessels, for the rapidity of its motion, and the interlacing of the vessels with each other, scarcely permit the eye to detect the exact progress which it follows.

From an inspection of the mechanism of the valves, we perceive that it is impossible for the blood to return from the ventricle into the auricle, because, when this fluid endeavours to escape, the first effect is to raise up the valve which was floating upon its surface, and to apply it closely to the passage which leads from the ventricle to the auricle. There is, however, no obstacle to the entrance of the blood into the arteries, and we accordingly find that they become

and the mesentery. See his Second Epistle "de pulmonibus," addressed to Borelli; it is dated 1661; see also Boerhaave, Prælect. ab Hallero, notæ ad § 160. Leeuwenhoek first saw the circulation by the microscope, as it seems, in the year 1698; he observed it in a bat's wing, a tad-pole, and a fish's tail; Hoole's Leeuwenhoek, p. 90, et seq.; also Epistolæ, p. 49, where, in a letter to Heinsius, Oct. 1698, he describes his observations on the circulation in microscopic eels.

distended with blood. From various causes, which will be more particularly examined hereafter, the artery then contracts, but the valves which are at its mouth are so constructed as to prevent the blood from getting back into the heart, so that it must be necessarily carried forwards into the minute branches of the arterial system.

The curious operation called transfusion proves the course of the circulation to be from the arteries into the veins. In this operation, which seems to have been invented, or at least perfected by Lower about the year 1660, the artery of one animal is connected by a tube with the vein of another animal, when we find that the first is gradually emptied of its blood, while the second is brought into a state of plethora. If an opening be made at the same time in the veins of the second animal, the blood originally belonging to it will escape, and thus the fluid in the vessels generally will be changed. At the time when these experiments were made diseases were commonly supposed to depend upon some morbid qualities residing in the blood, and as the operation of transfusion held out a method of changing this fluid at pleasure, it was hailed as a most important means of restoring the health, and, repugnant as it appears to the feelings, some individuals actually submitted to have the blood of lambs or calves transmitted into their vessels, for the purpose of being cured of certain diseases, or having their vigour renovated when it was exhausted by old age.

It does not belong to my present object to notice the operation, except so far as it may illustrate the theory of the circulation, otherwise it would be amusing to recount the extravagant expectations that were formed respecting its probable advantages. Lower himself, who was a man of science, and possessed of a clear and philosophical turn of mind, seemed to regard the discovery as a new era in the healing art,[9] and it was warmly patronized by other learned persons, who might be supposed less apt to be biassed in its favour. But the first experiments of the kind that were performed upon the human subject ended fatally, and although the advocates for the practice endeavoured to show that these unfortunate events were not necessarily connected with the act of transfusion, yet it excited so much alarm, and appeared altogether so disgusting and shocking an operation, that it was prohibited in France by an act of the legislature, and everywhere soon fell into complete neglect.[1]

[9] De Corde, c. 4; Boerhaave, Præl. ab Haller, not. ad § 160.

[1] The following papers give an account of some of the first experiments that were performed on this subject. Phil. Trans. No. 19, p. 352, (1666.) A general notice of the fact of transfusion having been performed before the Royal Society in London and at Oxford. No. 20, p. 353. A more full detail of the experiment. No. 25, p. 449, (1767.) An account of further experiments. No. 26, p. 479. The operation is performed at Paris. No. 27, p. 490. The operation is performed at Pisa, by Fracassi. No. 28, p. 517. Account of Bond's case at Paris, the first human subject on whom the operation was performed; it ended fatally. No. 30, p. 557. The operation was performed in London on a human subject by Lower and King. No. 32, p. 617, (1668.) Denys performs the operation at Paris on a maniac; the disorder is supposed to

A fifth argument that was advanced as a proof of the circulation being from the arteries into the veins is derived from the effects of wounds of the vessels. It was observed that when an artery was cut or divided, the part nearest to the heart projected a

be relieved; the operation is repeated and ends fatally. No. 36, p. 710. A particular account of the above case. No. 54, p. 1075, (1669.) Further particulars of the case. See also Senac's Treatise on the Heart, Intr. p. 92.—From this time the operation appears to have been entirely laid aside, until it was again introduced by Dr. Blundell, who has given us a minute detail of his experiments, and of the method of performing the operation. He has established the important point, that the blood of an animal of the same species may be safely and easily transfused, but that if the blood of a different kind of animal be employed, great disorder of the functions is occasioned, and death generally ensues. Med. Chir. Trans. v. ix. p. 56. The experiment was tried upon the human subject, and, so far as the operation was concerned, with success. Ibid. v. x. p. 296.—The curious fact, that the transfusion of the blood of an animal of a different species proves fatal, has been since observed by MM. Prevost and Dumas; they once mention Dr. Blundell's name, but no one would suspect, from the perusal of their memoir, that he had anticipated them in the most important of their conclusions. Bibl. Univ. t. xvii. p. 215. The results of the experiments of Dr. Blundell, and of MM. Prevost and Dumas, would appear to be scarcely consistent with the following statement by M. Magendie; Physiol. t. ii. p. 342. "J'ai eu occasion d'en (expériences) faire un certain nombre, et je n'ai jamais vu que l'introduction du sang d'un animal dans les veines d'un autre eût des inconvéniences graves, même quand on augmente beaucoup, par ce moyen, la quantité de sang."—Numerous references to cases or treatises on transfusion may be found in Plouquet, "Chirurgia Infusoria et Transfusoria;" we may remark, however, that here, as well as in other parts of this learned performance, the value of the work is diminished in consequence of subjects being incorporated, which have rather a technical or verbal, than a real connexion.

stream of blood, and that comparatively little fluid was poured out from the other end, while the reverse was observed to take place with respect to the veins when they were wounded; here the flow of blood was from the part more remote from the heart. Although this statement is in the main true, and is naturally explained by the course which the blood is known to follow, yet it must be considered rather as affording an illustration of the subject, than as any very direct proof.

A much more decisive argument is offered by the effect of ligatures placed upon the vessels; here it was observed that if the artery be tied, so that the stream of blood along it be interrupted, the part between the heart and the ligature becomes turgid, while the part beyond the ligature is comparatively emptied of blood. A ligature upon a vein has exactly the contrary effect; here the part between the commencement of the vessel and the ligature is rendered turgid, while the part between the ligature and the heart becomes flaccid.

Two other arguments in proof of the circulation have been adduced, even by writers of the first eminence, the power which we have of filling all the vessels of the body by injecting a fluid into one of them, and the fact well known to physiologists, that when certain medicinal substances are introduced into the veins, they are carried into the general circulation, and are found to exercise their specific action upon certain glands or other organs of the body, in the same manner as if they had been received into the stomach by the mouth.[2] These two circumstances, however, can afford only a general proof of the motion of the blood through the vessels, and of their mere communication with each other, without showing the nature of the blood's motion, or the manner in which this communication is effected.

All the circumstances which have been enumerated, the inspection of the heart of a cold-blooded animal, the application of a microscope to a transparent membrane, the mechanism of the valves, the operation of transfusion, the effect of wounds of the vessels and the action of ligatures, when taken in connexion with each other, may be considered as proving very decisively that the course of the blood is from the heart along the arteries, and through the veins back to the heart; but it still remains to prove in what manner the systemic and the pulmonic circulations are related to each other. This is very satisfactorily demonstrated by the mechanism of the heart and especially that of its valves. We find that there is no direct passage between the right and left sides of the heart, that when the blood is in either of the auricles, it must be transmitted into the corresponding ventricle, that the tricuspid and mitral valves will not permit it to return into the auricles, and, therefore, that the pulmonic ventricle, when it contracts, must necessarily force the blood into the pulmonary artery, while the aortic ventricle can propel it only into the aorta.

Sœmmering, Hum. Corp. Fab. t. v. § 40.

In speaking of the proofs of the circulation I have hitherto taken no notice of a train of phenomena which daily offer themselves to our notice, and which are generally regarded as affording very direct and decisive proofs of the course of the blood. I allude to the appearances which are frequently found in the dissection of subjects who have died of diseases of the sanguiferous system. These, however, are rather to be considered as illustrations of the true theory, or as confirmations of it, than as actual proofs, and although they may occasionally assist us in investigating the nature of the uses which the blood serves in the animal œconomy, yet, as they belong more to pathology than to physiology, it would be scarcely consistent with the object of this work to enter into any minute detail of them.

I shall merely state in general terms, that when an obstruction occurs to the passage of the blood along any part of its course, a turgescence is produced behind the obstruction, just in the same manner as from the application of a ligature. We occasionally observe individuals in whom we have reason to suppose that the blood does not experience its specific change by the action of the air in the lungs, and, on examining their bodies after death, we find that from some malconformation of the heart or its appendages, the blood had been transmitted immediately from the right to the left side of this organ, without passing through the pulmonary vessels. It not unfrequently happens that a great arterial or venous trunk becomes obliterated by some accident, or may have been deficient in the original formation of the body, and we then find that the branches are increased to an unusual size to supply the deficiency, and that they are given off in such situations as to correspond with the theory which has been laid down. These examples may serve as specimens of the nature of the illustrations of the true theory of the circulation, which are afforded by morbid anatomy and pathology.

We may now be considered as having established the general fact of the circulation and the path which the blood pursues, but there are several circumstances connected with this function which require to be more minutely examined, either as having formerly been the subject of controversy, as points about which a difference of opinion still exists, or as tending to illustrate some of the operations of the animal œconomy, and to explain the uses of its different parts. In the first place I shall mention the circumstances of a more mechanical nature, connected with the structure and organization of the heart and its appendages, directly affecting its motion or its action upon the blood, considered merely as an hydraulic machine. I shall afterwards notice some points that are more immediately connected with its action as a vital organ, particularly those that depend upon its contractility. Lastly I shall give an account of various circumstances connected with the arteries and the veins, which I have hitherto not noticed, or adverted to only in an indirect or cursory manner. In pursuance of this plan it will be my object to intrude as little as possible upon the province of the anatomist, and to make use of the facts deduced from his science only so far as they immediately lead to any important physiological conclusions.

§ 4. *Mechanism of the Heart.*[3]

We have seen that the substance of the heart is composed of the parietes of two cavities, called ventricles, to which two others are attached, called auricles, making in all four cavities, through which the blood is progressively carried from one to the other in succession. In describing the heart it has been a point warmly contested by anatomists what is the relative size of these cavities; whether they have all exactly the same capacity, or whether they differ from each other in this respect. As to the auricles, perhaps the point can scarcely be determined with perfect accuracy, as it is not easy to assign the precise limits where the large veins may be said to terminate and the auricles to commence. It is, however, generally admitted that the right auricle is considerably more capacious than the left, and Haller assigns their proportions as about seven to five.[4] The limits of the ventricles are better defined, both in consequence of their more compact form, and of the valves which are attached to both their orifices; yet it is remarkable

[3] For the most ample account of every thing that respects the mechanism and structure of the heart, the reader may be referred to the learned work of Senac, liv. 1 and 3, a writer no less to be admired for the extent of his information, than for the candour with which he comments upon the opinions of others.

[4] El. Phys. iv. 2. 17.

that very different opinions have been entertained respecting their size, a point which one should have supposed might have been easily subjected to a direct and precise experiment. Most of the older anatomists have described the right ventricle as considerably larger than the left, and even Haller admits that this difference of size exists,[5] but there seems to be good reason for doubting the correctness of the opinion. Sabatier examined this point very accurately, and his conclusions have been since generally acquiesced in. He admits that when we examine the heart after death, the right, or pulmonic cavities, are frequently found much more capacious than the aortic, but he conceives that this difference did not exist during life, and that it is occasioned by the manner in which the circulation terminates in the last moments of existence. From causes which will be more particularly described hereafter, in the act of death the blood necessarily becomes accumulated in the pulmonic cavities of the heart, while it is, from the same cause, almost entirely expelled from the aortic cavities; hence the latter become contracted, while the former are proportionably dilated, and the comparative weakness of the muscles of the pulmonic ventricle still further contributes to produce this effect, and permits the dilatation to take place. It appears, therefore, that we are to regard the two ventricles of the heart,

[5] El. Phys. iv. 3. 3; Lower, p. 36, endeavoured to prove that the capacity of the ventricles is equal, but his opinion was not generally adopted.

during the life of the animal, as possessing nearly equal capacities.[6]

But whatever may be the fact respecting the size of the two ventricles, they differ very obviously from each other in their form and in the strength of the masses of muscular fibres which compose their sides. The left ventricle, or that which communicates directly with the systemic circulation, is much stronger than the other: it lies more nearly in the centre of the heart, and gives the general form to the organ, while the right, or pulmonary ventricle, lies, as it were, upon the systemic, like an appendage attached to the heart, and has much thinner and weaker parietes. Accordingly, when we divide the heart by a transverse incision across the two ventricles, we find that the section of the left ventricle is nearly a circle, while that of the right exhibits a semilunar figure. This difference in the strength of the ventricles is necessarily connected with the offices which they respectively perform, or with the degree of force which they exercise in the parts of the circulation to which they are immediately destined. The right or pulmonic ventricle has merely to propel the blood through the lungs, while the left has to transmit it to every other part of the body. The resistance which the blood has to overcome in its passage through the vessels depends upon a combination of several circumstances, which will be considered hereafter, but there can be no doubt of the general fact respecting the difference of

[6] Sabatier, Anat. t. ii. p. 241, and t. iii. p. 373; Mem. Acad. for 1774.

resistance opposed to the blood at its entrance into the two circulations, and, by ascertaining the relative quantity of fibres which belong to each of the cavities, we might form an estimate of the amount of the force respectively exercised by them. The same difference of strength exists in the auricles as in the ventricles, the right being considerably weaker than the left; Haller conceives that the former possesses only one-third the strength of the latter.[7]

Another subject, which has given rise to much discussion, is the exact order of succession in which the different parts of the heart contract. It is obvious that each auricle must contract before its corresponding ventricle, but it has been questioned whether any of these events are synchronous, or whether they do not each of them occur in succession. It is now, however, very generally admitted, that the parts of the same description contract precisely at the same point of time, as the two auricles and the two ventricles; that the contraction of the auricles exactly alternates with that of the ventricles, and that the contraction of the arteries is synchronous with that of the auricles. This opinion was warmly contested, about the middle of the last century, by Lancisi and Nicholls, who had each of them some peculiar notions on this subject, which, however, it is not necessary to particularize, as they are now entirely discarded.

Every part of the blood passes through all the four cavities of the heart in the course of a complete cir-

[7] El. Phys. iv. 3. 2.

culation; it therefore necessarily follows, that if all the cavities contract an equal number of times during the same interval, they must project the same quantity of blood, and consequently, if they differ in size, that a portion of the contents of the larger of them will not be expelled. With respect to the ventricles it appears that their size is nearly equal, and that, at each contraction, they are nearly emptied; but it is probable that this is not the case with the auricles, and if the right be so much larger than the left, as has been stated above, it is obviously impossible that this can be the case. Indeed, from the form of the auricles it would appear that the contraction of their fibres cannot entirely obliterate their cavities. It is also probable that when the auricles contract, a part of their contents will be forced back into the mouths of the great veins, as there is no valve situated between the vein and the auricle by which the reflux of the blood can be prevented.[8]

Calculations have been formed of the length of time which the heart occupies in performing its motion, and of the quantity of blood which is expelled by each contraction. This quantity of course varies in different individuals, and in the same individual at different times, and there are practical difficulties which prevent us from arriving at complete certainty on these points. Professor Blumenbach's estimate may be taken as a fair average; according to this the

[8] Haller, El. Phys. iv. 4. 10. For some interesting observations on the mechanism of the heart, see Home's Lect. on Comp. Anat. p. 47.

heart is supposed to expel two ounces of blood at each contraction; the whole mass of blood is reckoned at 33lbs.[9] and the number of pulsations are taken at seventy-five in a minute. Proceeding upon these data we shall find that the blood shall complete its circulation, or that the whole of it shall have passed through the heart in about two minutes and a half, and that a mass of fluid equal to the blood would be carried through the heart twenty-four times in an hour. It must, however, be observed that the different portions of blood complete the circulation in very different periods of time, partly depending upon the length of the course which they have to follow, and partly upon the degree of resistance which they meet with. When the blood is sent into the aorta it soon begins to pass into the different arterial branches that are connected with the great trunk; a part circulates only through the muscles of the heart, another portion takes a longer circuit through the chest, and others through such as are more extended, until a part of the blood is carried to those organs that are most remote from the heart.

It has been stated that the auricles and ventricles are filled and emptied alternately, so that there is no period in which the whole of the heart is either full or empty, but, as the substance of the ventricles is much more considerable than that of the auricles,

9 Mr. Good has collected the various estimates which have been formed of the amount of the whole mass of blood; he concludes the most probable quantity to be between thirty and forty pounds. Study of Med. v. ii. p. 11.

and as the former belong more particularly to the heart itself, the terms systole and diastole of the heart are applied to the contraction and dilatation of the ventricles respectively, and are of course reversed with respect to the auricles. I have before observed that the pulsation of the heart, which we feel when the hand is placed upon the ribs, depends not upon any increase of bulk which the ventricles experience by the injection of the blood into them, but upon the effort which they make to expel their contents, or rather by the injection of the blood into the arteries; the beat of the heart therefore occurs during the systole of the ventricles, and is contemporary with the diastole of the auricles.

In describing the structure of the heart it has been already stated that, in the adult and perfect state of this organ, there is no direct communication between its right and left sides, i. e. between the two auricles and the two ventricles. As a general principle, no point in anatomy is better established than this fact, and it is one which admits of an easy and satisfactory proof. With respect to the auricles, however, the position must be taken with some limitations, as it appears that the passage between them, which exists in the fœtus, called the foramen ovale, which I shall have occasion to describe more fully hereafter, is sometimes not entirely closed even long after the period of infancy.[1] With respect to the ventricles,

[1] Sabatier says, it always remains open; see also Blumenbach's Comparative Anatomy, by Lawrence, § 159, note (C).

however, the fact may be stated without exception, and yet such was the force of prejudice, and the bigoted attachment to hypothesis, that men of learning were not wanting who affirmed the existence of certain passages or pores between the ventricles, which they asserted that they were able to detect, and even professed to demonstrate to others.[2] In this, as well as in many similar cases, it is not easy to decide, whether such persons were the dupes of their own credulity or wished to impose upon others, but it must tend to impress upon our minds the important truth, that even the most direct testimony is always to be received with distrust, when the facts related are themselves incredible.

There is a still more remarkable example of the bigoted attachment to certain opinions, and, especially, when they were sanctioned by the authority of the ancients, connected with this part of the subject, which it may not be uninstructive to relate. Vesalius, the great restorer of anatomy after the dark ages, perceived that the description of the vessels of the heart which was left by Galen, did not correspond to what he found in the human subject, but resembled this part in apes and monkeys; from this

[2] Haller, El. Phys. iv. 3. 13. Sœmmering, Corp. Hum. Fab. t. v. § 27; Sabatier, Anat. t. ii. p. 235. The existence of such passages was first announced by Vieussens and Thebesius; they were received by Ruysch, Lancisi, and others of the first eminence; they were afterwards called in question by Duverney, and finally disproved by Senac and Sabatier. See Sabatier, Anatomie, t. iii. p. 410.

circumstance he very naturally concluded that Galen used these animals in his dissections. A learned French anatomist and professor, Du Bois, better known under his latinized name of Sylvius, who was a warm advocate for the ancients, and a violent antagonist of Vesalius, in his zeal to repel the accusation, seriously maintained the position, that the human form had undergone a change in its structure since the age of Galen, and that formerly the vessels were distributed as he described them.[3]

The heart, as I remarked above, is enclosed in a membranous bag or pouch, called, from its situation, the pericardium ; it is lined with a serous membrane, which, like other parts of the same structure, has a serous fluid perpetually discharged from its surface. This fluid, the liquor pericardii, is sometimes found to exist in considerable quantity, to the amount of several ounces ; so large a quantity, however, is evidently the effect of disease, but it is a question, about which there have been very warm and even very acrimonious disputes, whether any perceptible quantity of the liquor pericardii exists during life and in the state of health.[4] Perhaps the question can scarcely yet be considered as completely decided ; if we argue from the analogy of what occurs in other close cavities that are furnished with a serous membrane, we should

[3] Haller, El. Phys. iv. 2. 7. Du Bois extends this argument to the intermaxillary bone. Lawrence's Lectures, p. 174.

[4] Haller, El. Phys. iv. 1. 19, 20. Sœmmering, Corp. Hum. Fab. t. v. § 6. Sabatier, Anat. t. ii. p. 217. Bell's Anat. v. ii. p. 52, et seq.

conclude that, in the natural state of the parts, there is no fluid present, but that as fast as it is discharged by one set of vessels it is taken up by another. When, however, these actions do not correspond, when either the discharge is too rapid or the removal too slow, an accumulation must take place.

A circumstance respecting the mechanical structure of the heart, which may be worth noticing, is the difference of its size in different individuals; this difference is indeed much more considerable than might have been suspected, for we are informed by anatomists, that the heart is, in some individuals, double the size it is in others, and this has not been observed to bear any proportion to the general bulk of the body. The heart of the fœtus always bears a greater proportion to the whole body than that of the adult; as the growth advances this disproportion is diminished, but it is not entirely removed until the body attains its full size. It has also been observed, that the proportion between the parts of the heart is different in the fœtus from what it is in the adult; the auricles are larger than the ventricles, and the aortic side of the heart generally is larger than the pulmonic; but these points will be considered more particularly in the account of the fœtal circulation.

The heart is essentially a muscular organ, and when it contracts it acts, like other muscular organs, by shortening its fibres, and in this way it diminishes the capacity of its cavities. The general fact is demonstrable to the eye, when we lay open the thorax of a cold-blooded animal, and may be directly inferred both from the structure of the valves and from the actual effects which are produced. There has, however, been much controversy upon this subject, although one that is apparently so clear and perspicuous; the great point in dispute is, whether the ventricles are diminished in both their dimensions, whether, by the act of contraction, their sides are not brought nearer together without their being diminished in length, or even whether the heart is not actually lengthened during its systole. This opinion, which at one time had very powerful advocates, seems to have arisen partly from some hypothetical notions about the pulsation of the heart, or the manner in which it is enabled to strike the ribs during its systole; but by ocular examination, by direct experiments made upon the heart, and by considering the manner in which the valves perform their office, it is now generally admitted that the cavities of the ventricles are diminished in every direction.[5]

With respect to the striking of the heart against the ribs, this, like almost every other circumstance connected with its mechanism, has given rise to considerable discussion, although the cause which has been already assigned for it,[6] the sudden injection of blood into the aorta, and its consequent effort to straighten its curvature, will probably be considered as sufficient to explain the phenomena. Other causes, however, have been pointed out, either as

[5] Sabatier, Anat. t. ii. p. 229; Bichat, Anat. Descrip. t. iv. p. 113.

[6] See p. 346.

more satisfactory, or at least as conspiring with this to produce the effect; among others, Sabatier has endeavoured to prove that, when the ventricles contract, a portion of the blood that was contained in them is necessarily forced behind the valves into the auricles, and in this way contributes to push the heart forwards against the fore-part of the thorax.[7]

While the future animal is still retained in the uterus of its mother, it is obviously placed under very different circumstances, with respect to all surrounding agents, from what it is after that period. It is entirely secluded from the air, which is afterwards so essential to its existence, it is incapable of receiving any nourishment through the usual course of the digestive organs, it is constantly immersed in a high temperature, and has no opportunity of employing either the muscles of loco-motion, or any of those that are connected with the exercise of the external senses. In short, it may then be regarded as forming a part of the mother; from her it derives its nourishment, its heat, and all those powers necessary for the support of that kind of life which it possesses. Still, however, to a certain extent, its organs are developed, and acquire the form and properties which they are afterwards to possess, although with certain modifications and adaptations to present circumstances, so as to afford a most remarkable example of what has been called prospective contrivance,[8] or that adjustment of means to ends, which respects not merely

[7] Anatomie, t. ii. p. 230. [8] Paley's Natural Theology.

the present condition of the being, but that which it is afterwards to assume. This species of adjustment is more peculiarly remarkable with respect to the circulation of the blood, for we have it directed in a different course from that which it afterwards pursues, we have even vessels employed which are obliterated after birth, while the whole is so connected with the mother, that her system supplies what is deficient in that of the embryo, and completely ministers to all its wants. Both as constituting in itself a very interesting part of physiology, and as tending to illustrate the functions of the adult, we may consider the fœtal circulation as a subject well deserving of our attention.[9]

The most important point in which the state of the fœtus differs from the same animal after birth is its seclusion from the atmospheric air, from which it follows, in the first place, that the lungs are incapable of exercising their appropriate functions, or of inducing the specific change in the state of the blood, and secondly, that this change of the blood, so far as it is required for fœtal existence, must be effected by the mother. In order to accomplish this purpose, a large part of the blood, which in the animal after birth is carried through the lungs, forming the lesser or pulmonic circulation, is in the fœtus diverted from this channel, and passes directly into the left, or aortic side of the heart. This diversion in the course of the blood is a necessary consequence of the state of

[9] Senac, liv. 3. ch. 9, 10, 11.

the fœtus, both with respect to its posture and the relative development of its organs, and is likewise an expedient adapted to the circumstances in which it is situated. In order to supply the place of the lungs, or to execute the office which they are incapable of performing, the blood is carried into the placenta, where it is so acted upon by the blood of the mother, as to experience a change analogous to that which, after birth, it undergoes in the lungs. It is carried to this organ and brought back from it by a set of vessels which exist only before birth, and which are necessarily destroyed by the act of leaving the uterus, at the very instant when they become no longer of any use.

But in appending the supplementary or extraneous set of vessels to the ordinary circulating system, it happens that the blood, after it has experienced its specific change, is returned to the fœtus in the course of the venous part of the circulation. It is, however, obviously unnecessary for it to complete its course in this direction, and probably by so doing it would lose some part of that specific quality which it was the office of the placenta to impart to it. On this account a passage exists between the two auricles, called the foramen ovale, through which a part of the blood that is brought into the pulmonic auricle is directly conveyed into the systemic auricle, without passing into the pulmonic ventricle, as well as two additional vessels or ducts, which belong exclusively to the fœtal circulation. By one of these a part of the blood is immediately brought back to the heart, soon after its return from the placenta, without passing through any part of the venous circulation, which it would otherwise have to perform in order to arrive at that organ; by the other, a part of the blood which has arrived at the right or pulmonic ventricle, and is propelled into the mouth of the pulmonic artery, is carried directly into the aorta, thus escaping the whole of the pulmonic circulation, as well as the passage through the left or aortic cavities of the heart.

The first of these temporary or supplementary vessels, as they may be styled, is named the ductus venosus; the second, the ductus arteriosus; names, which it may be remarked, are rather anatomical than physiological, being derived from the parts to which they are connected, not from the offices which they perform. In fact they are both to be regarded as arterial vessels, for their purpose is to convey, by a short track, the blood, after it has been changed in the placenta, to the main trunk of the aortic system. In short, the mechanism of the fœtal heart all tends to one object, to reduce what in the adult is a double organ, or at least an organ consisting of two sets of parts, which, although placed in contact, serve two different purposes, to a single organ, or to an organ, the parts of which may conspire to one object only. The fœtus is in the state of those animals that are without lungs, and therefore before birth only so much blood is sent to them as may be sufficient for their growth, and may prepare them for being employed after the animal acquires the capacity of breathing when it leaves the uterus.

In the course of this work many opportunities will occur of illustrating the nature of the functions by the difference between the adult and the fœtal organs; at present we shall only further remark, that although we are fully acquainted with the nature of the difference in the mechanical structure of the parts, and the change which they experience at birth, we are perhaps scarcely able, in every period, to trace out the actual or immediate cause by which the change is effected. We, however, understand it sufficiently to perceive that it is accomplished by a system of contrivances at once simple and effectual. The first act of inspiration, which is immediately connected with the introduction of the infant into its new state of existence, necessarily causes the blood to pass through the pulmonic vessels in greater quantity than before birth, and we may infer that this new direction which the blood then assumes produces all the changes in the vessels, expanding those parts which were before small and only imperfectly pervious, while it diverts the blood from those that are no longer useful, and which, therefore, by their own elasticity, shrink up and finally become obliterated.

All the peculiarities of the fœtal circulation evidently tend to one object, to bring the blood, after it has been changed in the placenta, more directly into the systemic artery, but it is not so evident what is the use of each particular part of the additional apparatus. It may be asked, why may not the whole change have been effected by means of the foramen ovale or the ductus arteriosus alone, as the object of both of them appears to be precisely the same? Part of the difficulty which has attached to this subject is certainly owing to the incorrect notions which, until lately, were entertained respecting the use of the lungs and the function of respiration. Although the older writers had some imperfect idea of the connexion between the passage of the blood through the lungs and a chemical change in the composition of the air, yet the chief object of respiration was conceived to be mechanical, and of course there could be no correct conception of the use of the placenta as a substitute for the lungs. Whatever purposes the pulmonic circulation may be supposed to serve, it is obvious that it is unnecessary for the fœtal state, but that it becomes necessary the very moment that the animal, by leaving the uterus, acquires an independent existence, and must therefore perform by its own organs those functions which were before exercised by the organs of the mother. Yet we may readily conceive the difficulty there would be in effecting this change, without, at the same time, producing a great derangement in the mechanical structure of the parts concerned, and that it would be desirable to employ every method for rendering this transformation as easy as possible. Perhaps, then, it may be regarded as a sufficient reason for the present construction of the body, that this change would be more readily effected by having the current of blood divided into two streams, instead of the whole of it going through one channel, so that the use of the ductus arteriosus, in addition to the foramen ovale, is simply to facilitate the transmutation from the fœtal to the respiratory state, by increasing the number of parts concerned, and therefore rendering a less mechanical change necessary in any one direction.

Although I think it quite useless to advert to many of the speculations and discussions that have taken place on the subject of the fœtal circulation, there is one hypothesis that may be mentioned, principally on account of the individual by whom it was advanced. Sabatier, whose name has been so frequently referred to as one of the most accurate and judicious of the anatomists who have particularly directed their attention to the investigation of the structure and mechanism of the heart, has taken much pains to prove that the object of the foramen ovale is not simply to permit a part of the blood to pass directly from the pulmonic to the aortic auricle, but to keep the two currents of blood that proceed from the two great branches of the vena cava separate from each other, to send that part of it which comes from the vena cava superior into the pulmonic ventricle, while the blood from the vena cava inferior is transmitted through the foramen ovale into the aortic ventricle.[1] But in spite of the authority by which the hypothesis is supported, I must acknowledge that I think it very fanciful and altogether inadmissible.

It has been stated by those who have minutely attended to the gradual development of the parts in the fœtal state, that in the first stages of existence

[1] Anat. t. ii. p. 224, and t. iii. p. 387; Mem. Acad. for 1774, p. 198.

the two sides of the heart are nearly of the same strength and form, but by degrees the aortic ventricle, in consequence of the greater force which it has to exert, acquires its superior strength and thickness. This is analogous to other well-known facts in the animal œconomy, where a muscular part acquires additional strength by action, a provision which is attended with the most beneficial results, and is one of the most beautiful examples of the adaptation of means to ends with which we are acquainted: the efficient cause of this change will be considered hereafter.

The great importance of the circulation, considered as the regulator of the whole animal machine, and the connexion which it has with all the other functions, renders it desirable for us to view it in all its relations, and to examine it under every different circumstance in which it presents itself to our inspection. It may, on this account, be useful to make some observations upon the comparative anatomy or the organs of circulation, at least in those animals whose general structure is so similar or analogous to the human, as to enable us to establish a comparison between them. Of the two great divisions into which the whole animal kingdom has been arranged by Cuvier, the animals with and without vertebræ, I shall refer only to the former, as the structure and œconomy of the latter differ so materially from man, as not to throw any light upon the subject now under consideration. We have seen that an essential part of the circulation in the human frame is the circumstance of its being double, or of the blood being carried twice through the heart in each complete circuit, once for the purpose of its being acted upon by the air which is received into the lungs, the other, in order to transmit it, when thus acted upon, to all parts of the body.

The animals possessed of vertebræ have been divided into the four classes of mammalia, birds, reptiles or amphibia, and fish. The circulatory organs of all the animals that belong to the two first classes are essentially the same with those in man; they consist of a heart divided into two ventricles, to which two auricles are appended; to these the arteries and veins are so attached, that the blood pursues its complete course along the two circulations, as in the human subject, the differences depending only upon some variations in the shape or position of the parts, the order and disposition of the vessels, or the structure of the valves.

The third class, the reptiles or amphibia, although they differ from the other vertebrated animals sufficiently to be placed in a separate division, differ also considerably among themselves, so as to be defined almost as much by negative, as by positive characters. The organs of the circulation are less uniform in this than in the other classes, and it is perhaps not a little remarkable, considering the great attention which has been bestowed upon comparative anatomy, and the facility with which animals of this description may be procured, that anatomists do not altogegether coincide in the account which they give of the circulatory organs of the same animal. Without descending, however, to minute particulars, which would be foreign to my object, it will be sufficient to state as a general fact, that the heart of the amphibia consists either of only one ventricle, or of two ventricles which freely communicate with each other, so as, physiologically considered, to be equivalent to one. The number of auricles depends upon the construction of the ventricular part of the heart; where there is a single ventricle there is but one auricle, and where there is a double ventricle there are two auricles. In all these animals, however, there is only a single large artery proceeding from the heart, which serves both for the pulmonic and the systemic circulation, and the essential peculiarity of the circulatory organs of these animals consists in the pulmonic circulation being merely an appendage to the systemic circulation. After the great artery, which may be regarded as analogous to the aorta of the mammalia and birds, has left the heart, it divides into two main branches, by one of which a part of the blood is carried to the lungs, while the other part goes through the arteries that are distributed over all the parts of the body. These two portions of blood are united in the heart, and, after being mixed together, are again expelled through the great artery. It appears, therefore, that whatever be the office of the lungs, a part only of the blood is transmitted through them during each circuit, and that although there are two sets of vessels, yet that each portion of the blood passes through one circulation only each time that it leaves the heart.[2]

The circulation of the blood is effected in fishes, the fourth great class of the vertebrated animals, in a different and more simple manner. Here the heart consists of only one auricle and one ventricle; all the blood, as it returns from the veins, is brought back into the auricle, is poured from this into the ventricle, and from the ventricle into the great artery, which carries it to the branchiæ or gills, the organ which in fish corresponds to the lungs of breathing animals. The single heart in fish may therefore be considered as corresponding to the pulmonic heart of the mammalia and birds. After the blood has traversed the gills, and undergone the specific change from the action of the air that is suspended or dissolved in the water in which the animals are immersed, it is again collected into a main arterial trunk; from this it is sent to all parts of the body, and is again brought back into the auricle by the veins. Here every part of the blood is exposed to the action of the air through each circuit, but in consequence of the smaller proportion of blood in fishes, and the more scanty supply of air, the mutual action of the air and the blood, or the degree of effect produced by this action, is much inferior to what it is in animals furnished with lungs.[3]

While I am upon this part of the subject it may be necessary to make an observation upon the terms

[2] Blumbenbach's Comp. Anat. by Lawrence, p. 241, with note (E) containing the references to Cuvier.

[3] Monro on Fishes, p. 14, et seq.

single and double circulation, which I have frequently had occasion to employ, and which have not always been used by authors in the same sense. According to what I conceive to be the most correct acceptation of the phrase double circulation, it should be applied to that construction of the heart and its appendages, where there are two cavities which do not communicate with each other, except through the medium of one of the circuits, as in the two first classes of animals. By a single circulation, on the contrary, we should understand that which consists of a heart with only two cavities, where the blood, when it has once left this organ, does not return to it until it has completed its passage through all the vessels, as is the case in fishes. The amphibia will hold a middle place between the two, part of the blood being carried through only one circuit, while another part of it goes through both the lungs and the body generally.

This concise sketch of the comparative anatomy of the circulatory organs proves to us both the importance of the function, and its intimate connexion with respiration, so much so as to justify the assertion, that a primary object of the circulation is to propel the blood through the lungs or some equivalent organ, where it may receive the influence of the air, and have a certain specific change wrought upon it, which is a necessary requisite to the performance of its other offices. It will also appear from these remarks, that the use of the heart is entirely mechanical, that it is an apparatus by which the blood is propelled along a series of vessels, by means of the alternate contraction and dilatation of a muscular receptacle. The mechanism of the heart is all directed to this object, that is, to the propulsion of the fluid with the due degree of force and in the proper direction, and upon this principle we may explain the action of all its separate cavities, valves, and other appendages, and show how any variation which occurs with respect to them in the different classes of animals are adapted to the general structure and functions of the individual. Where the quantity of blood is large in proportion to the size of the body, and where it is necessary that it should receive the full influence of the air, there are two complete circulations, and of course two muscular bags to send it along the two sets of vessels, the relative strength of which is proportionate to the resistance which they have to overcome. Where the quantity of blood is smaller, and where a less degree of change is induced upon it by the air, we have, according to circumstances, either only a single circulation, or one of an intermediate kind, and we find that the mechanism of the heart and its absolute power are always exactly adapted to the circumstances of each particular case. We have here only one principal muscular cavity, or, if there be a greater number, they have a free communication with each other, while we find that the number of auricles, whether one or more, is in like manner adapted to the number and disposition of the great veins, so as the most readily to receive the blood from them, and convey it into the ventricle in the manner which best admits of their effectually performing the alternate actions of contraction and dilatation.

§ 5. *Vital Properties and Actions of the Heart.*

Having now made ourselves acquainted with the mechanism of the heart, we shall proceed, in the next place, to consider its vital properties, which in this, as in every other part of the body, are two, contractility and sensibility. I have already had occasion to enter upon the question, how far the heart possesses proper sensibility, and we have seen that on this point a great difference of opinion still exists, among those who might be supposed the best qualified to judge concerning it, both with respect to the actual facts, and to the inferences that may be deduced from them. What I am disposed to regard as the most probable conclusion, in the present state of our knowledge, is, that the heart has but a small share of sensibility; that when it acts in its ordinary manner, it produces no sensation; that it is not under the control of the will; that the nerves distributed to it are less numerous than those which are sent to other parts containing the same number of muscular fibres, and that, both from their origin and their texture, they are more analogous to the nerves which supply the muscular coats of the viscera, than to those which are distributed over the proper muscles.

Still, however, the heart is supplied with nerves, although perhaps scantily, and we are naturally led to inquire what is their use. To this question we must seek for a reply rather from a consideration of the uses of the nervous system generally, than from any individual facts which we have it in our power to adduce on this particular topic. The uses of the nervous system have been stated above to be two; to connect us with the external world, and to unite the different parts of the system with each other, so as to form it into one connected whole. The first of these objects is effected through the medium of the organs of the external senses only, and does not apply to the heart; we are therefore to look to the second to explain the point now under consideration. We may suppose that one important use which the nerves of the heart serve, as well as those of all the other internal viscera, is to indicate to us any injury or disease of the organs, of which, as they are removed from our view, we might be unconscious, were not the parts endued with the faculty of feeling pain. To this explanation, as applied to the heart, an objection may indeed be urged, that it not unfrequently becomes the subject of disease, and even in a very considerable degree, without our experiencing any direct sensation of pain, so that we become sensible of the morbid condition of the organ more from some indirect circumstances connected with the state of the circulation, or of some of the other functions, than from any indication of it depending upon the sensation excited in the heart itself. On the other hand, however, we are aware that there are certain affections of the heart, as induced either by injuries or disease, in which it is sensible to pain or uneasiness, and perhaps all that we are able to say, in the present case, is, that the degree of sensation which the heart possesses is in proportion to the quantity of nerves sent to it, and is adequate to the wants of the system.

There is another use which may be assigned to the nerves of the heart, although it may belong more to the moral than to the physical part of our frame. We have already had occasion to remark, that although the heart in its ordinary action is independent of the nervous system, yet that on certain occasions it is liable to be influenced through it. This is especially the case with respect to mental emotions, which frequently produce or are attended by some change in the state of the circulation, either quickening or retarding the action of the heart, or affecting the quantity of blood propelled by each pulsation. Hence, according to the nature of the emotion, a greater or less quantity of blood will be sent to the surface of the body, and in no part will the effect of this change be more apparent than in the face. The countenance, therefore, as indicated by the state of the circulation, becomes the index of the mental emotions, and as it is not under the control of the will, it frequently points out what is actually passing in the mind, in a manner which can neither be falsified nor concealed. There are many important effects which are produced upon the various functions, as well contractile as sensitive, by an occasional increase or diminution of the circulation, as indirectly effected by the nerves of the heart; but these will be considered more fully hereafter.

With respect to the other vital function of the heart, its contractility, there are not the same difficulties as with respect to its sensibility, since its most obvious and characteristic property is its constant motion and the readiness with which it obeys the action of various stimulants. The contractility of the muscular fibres of the heart is indeed the main spring of the animal machine, considering it as an apparatus adapted for the purpose of spontaneous motion; for the direct office of the heart, and the immediate effect of its contraction, is to propel the blood through the two circulations, in one of which it undergoes a certain necessary change, while in the other it is sent, after being changed, to all parts of the body, giving to each of them their specific powers and capacities for action. Many causes have been assigned for the power by which this great machine is primarily moved. The ancients, who could conceive of no proper mechanical cause for so great an effect, had recourse to many mysterious agencies and fanciful hypotheses. They imagined that the heart contained a species of innate fire, or a kind of ethereal or subtile spirit, which produced its motion, but they did not condescend to give any minute description of the manner in which the subtile fluid acted. The learned Dutch Professor, Sylvius, who is noted as the founder of the chemical sect in medicine, accounted for the motion of the heart in a way which was perhaps more intelligible, although certainly not more correct. He ascribed it to an effervescence excited by a mixture of the different kinds of blood, one of which possessed an acid and the other an alkaline nature, and many other equally absurd ideas prevailed until the publication of Senac's valuable treatise on the heart.[4]

This judicious physiologist properly attributed the power by which the circulation is carried on entirely to muscular contractility, residing principally in the heart, and showed that by the blood being poured into its cavities, and causing a certain degree of distention of its fibres, the heart is stimulated to contraction, and by this act expels its contents.[5] The stimulating cause being thus removed, the heart relaxes, but, in the mean time, a portion of blood has been propelled along the arteries and veins into the auricles, and is ready to be poured into the ventricles immediately upon their relaxation. They therefore again become distended, are again excited to contract, and again propel the fluid along the vessels, and this alternation proceeds as long as life continues. It is frequently observed that, even in human contrivances, the most important effects are produced by the most simple means, and the observation applies to the present case. Yet simple and intelligible as is the general doctrine of the circulation, there are many interesting and diffi-

[4] For a sketch of the opinions of his predecessors and for a full exposition of his own doctrine, see Senac, liv. 4. c. 7. 8. 9.

[5] Among the physiologists who attributed the contraction of the heart to the stimulus of the blood, some ascribed it to certain specific properties in this fluid, which acted upon the fibre, others to their distention only. Those who adopted the former opinion were much puzzled to account for the different effects of the two kinds of blood in the two ventricles.

cult points concerning it, which we are not yet able to answer or comprehend to our entire satisfaction.[6]

The most important subject connected with the motion of the heart, and one which has given rise to much controversy, is the inquiry into the cause of its regularity and constancy. What, it has been asked, can enable the heart to proceed for years together, without fatigue, pulsating at equal intervals, and propelling the same quantity of blood? The hypotheses that have been advanced to answer this question are, as usual, very numerous. Willis, who may be regarded as the first physiologist that was fully sensible of the importance of the nervous system in the animal œconomy, advanced an opinion, which, for some time, was very generally received, that the

[6] Some experiments are stated to have been made by Mr. Brodie, which appear to oppose the doctrine of Senac. He emptied the heart of its blood and found that it still contracted and relaxed alternately, hence he concludes that the action depends upon some influence transmitted to it, in the same manner as with respect to the diaphragm, and not upon the blood in its cavities. See Cooke on Nervous Diseases, Introd. p. 61. Whatever comes from Mr. Brodie's pen must be received with attention, but until the experiments are given more in detail, it would be premature to speculate upon the consequences that may be deduced from them. We have likewise a similar statement made by Mr. Mayo, Comment. p. 16, from which he concludes that the alternations of contraction and relaxation in the heart depend upon something in its structure; in Mr. Mayo's experiment the effect was produced after the organ was separated from the brain and spinal cord. May we refer it to the elasticity of the heart, causing an alternation of action like the vibrations of a metallic rod? Can this elasticity and the action resulting from it stimulate the muscular fibres?

nerves of the heart, as well as of all the organs of the body which are in constant motion, are derived from a different part of the brain from the nerves of those organs which are only called into occasional action. It must be admitted that there are certain facts which countenance the idea, that the voluntary and involuntary muscles derive their nerves from different sources, but it is doubtful how far this will apply to the heart, and even were it proved, although it might tend to generalize the fact, still it would not solve the difficulty. Willis's doctrine, however, had many adherents; among others, Lancisi, who published an elaborate treatise upon the heart, adopted it, and laboured to remove all the objections that had been urged against it, and in short it remained, for a long time, the most approved hypothesis with the mechanical physiologists, and with those who were the most conversant with the anatomical structure of the body. Many other opinions were occasionally started, which it will not be necessary to notice, but there is one which has had so extensive an influence over the science of physiology, that it must not be passed over in silence; I refer to the doctrine of Stahl, who ascribed the regularity of the heart's motion to the *anima*, or soul, which resides in man, and superintends his actions, and which, knowing the fatal effects that would ensue from the interruption of so important a function, is careful always to preserve it in a proper state of action.[7]

[7] The last defender of the pure Stahlian doctrine appears to have been Nicholls; see his treatise "De Anima Medica." But

The doctrine of a superintending intelligent principle, residing in the body, seems to have originated from Vanhelmont, who gave it the name of *archeus ;* Stahl refined upon Vanhelmont's notion, and applied it to many parts of the animal œconomy, under the appellation of *anima*, and it has borne a distinguishing share in the hypotheses of many learned physiologists, down to the present time, under the title of the vital principle, spirit of animation, and various other names. An agent of this kind, although not so distinctly brought into view as by Stahl and some of his immediate followers, forms a leading feature in the writings of John Hunter;[8] and many of Darwin's

even as modified by Whytt and those physiologists who have been designated by the title of semi-animists, it involves us in the greatest inconsistencies. Whytt maintains the identity of the vital and the sentient principle, or of life and intellect, and argues that a considerable part of the actions of the body are effected by mental operations, of which we are essentially inconscious. On Vital and Involuntary Motions, § 11.

[8] Although it is an ungracious task to dwell upon the errors of great men, yet the very circumstance of their celebrity is a reason why we should be more especially put upon our guard against the mistakes into which they may have occasionally fallen. This is perhaps in no one instance more necessary than with respect to John Hunter. After giving his reasons for dissenting from the explanations that had been previously given of the alternate action of the heart, he subjoins his own. "The alternate contraction and relaxation of the heart constitutes a part of the circulation; and the whole takes place in consequence of a necessity, the constitution demanding it, and becoming the stimulus; it is rather therefore the want of repletion which makes a negative impression on the constitution, and becomes the stimulus, than the immediate impression of something applied to the heart." On the Blood, p. 149.

speculations, when divested of their poetical garb, can only be referred to the same class. But I have no hesitation in saying, that nothing can be more contrary to the spirit of true philosophy, than to assume the existence of an intelligent agent, of which we are entirely unconscious, or to adduce, as the cause of certain effects in the body, a power of which we have no knowledge or intimation, except as a commodious method of solving the present difficulty.

The Stahlian doctrine, in all its ramifications, and under every form in which it has been exhibited, seems to have originated, partly from the want of an accurate discrimination between the physical and the final causes of the operations of the body, and partly from an indistinct conception of the difference between the agency of the first great cause of all things, and the secondary or physical cause, into which alone it is the province of natural philosophy to inquire. It affords no explanation of the physical cause, or the nature of any phenomenon, to say that the Supreme Being has thought fit to order it so; the object of our researches is to examine, to which of the great laws that are impressed upon matter, the action in question can be referred. But we are always unwilling to confess our ignorance, and are never satisfied without attempting to explain every thing that passes before us. In the present instance the only answer that we can give to the proposed question is one that is in itself, in some measure, an acknowledgment of our imperfect acquaintance with the subject. We know that distention excites a muscle to contract, and that when a muscle has contracted relaxation ensues. In the present case, therefore, we can only say, that in the formation of our body the degree of contractility bestowed upon the heart, the quantity of distention which it receives from the blood, the size and texture of the arteries which are to transmit the blood, and the quantity of resistance which it has to overcome, are all so nicely balanced, that each particular action is retained in due subjection to the rest, and contributes to form one harmonious whole.

Admitting the justice of this remark, which is indeed merely the expression of an obvious matter of fact, it remains for us to inquire, what is the specific mechanism by which this action is accomplished, or whether we can observe any thing peculiar in the structure of the heart, or the disposition of its parts, which would seem to adapt it to this state of continued action. It must be confessed that on this point we have but little assistance from the researches of anatomy, at least there are no facts with which we are acquainted that can afford us a satisfactory solution of the difficulty. I have already adverted to the nerves of the heart, which, both with respect to their quantity, their origin, and perhaps also their mode of distribution, seem to differ from the nerves of the voluntary muscles. And with respect to the muscular fibres of the heart, it has been observed, that both in their form and their mode of connexion with each other, they likewise differ from the voluntary muscles. Instead of an assemblage of long and comparatively straight fibres, disposed in the form of separate bundles, each of them enclosed in a sheath of cellular substance, and the whole furnished with a coating of the same, the muscular fibres of the heart are disposed in an irregular manner, they are not divided into distinct parcels, and they have but little cellular substance attached to them.[9] This peculiar form and composition of the muscular part of the heart seems, however, to refer rather to its mechanical action than to any peculiarity in its vital powers. The long and straight lacerti of the muscles of voluntary motion are fitted to produce the contraction of these parts in one direction only, whereas the irregular interlacing of the fibres of the heart obviously serve to promote the contraction of this organ in every direction, so as to diminish its size in all its dimensions. There is no muscular part which is possessed of the same kind of action with the heart, and which is possessed of nearly the same degree of power, so as to require an equal quantity of muscular fibres to be concentrated in one point; but it would appear that a similar structure of the muscular matter exists in the stomach and the bladder, and that some approach to it will be found in all those hollow muscles, where the contractile force is exercised in more than one direction. This structure, however, throws no light upon the nature of the vital powers of the heart, nor does it, in any degree, tend to explain the difficulty in which this point seems to be involved.

The regularity of the heart's motion, or the perfectly uniform manner in which the different steps of

9 Sœmmering, Hum. Corp. Fab. t. v. § 29.

the process follow each other, proceeding in the same order and occupying the same space of time, is a circumstance still more worthy of our admiration than its constancy, and was accordingly regarded by the earlier anatomists and physiologists as something almost beyond the powers of the human mind to comprehend or explain. The mathematicians were not, however, wanting in their attempts to solve the problem, and Bellini, who in the application of his general principles was appalled by no difficulties, endeavoured to account for it by supposing that the auricles and ventricles of the heart were to be regarded as antagonist muscles so arranged, that when one set of them contracted the other necessarily relaxed. Baglivi, whose premature death must excuse many of his errors, formed an ingenious, although fanciful hypothesis, according to which he accounted for the phenomena in question upon the idea that the alternate contraction and relaxation of the ventricles proceeded from a corresponding alternation of pressure on certain parts of the nervous system, by which their energy, and consequently the contractile power of the muscles, was alternately repressed and excited. A mechanical theory something like this was maintained by Boerhaave, but it will not be necessary to examine it in detail, for this, and some other refined speculations of the same description, were completely overthrown by Haller. He reduced the regularity of the motion of the heart to the simple effect of stimuli, acting successively upon a series of contractile organs, the mechanism of which is so arranged, that each part, when relaxed, receives the stimulus from the contiguous parts, is then roused to contraction, and by this act transmits the stimulating body to the next cavity, which is then in a state of relaxation and prepared for its reception.

§ 6. *Action and Properties of the Blood-vessels.*

We are now to proceed to the third of the topics which I pointed out as requiring our attention, the structure and actions of the arteries and veins. We have hitherto considered the blood-vessels merely in the light of tubes appended to the heart, receiving the fluid that is propelled into them by this organ, and transmitting it from one set of cavities to the other. We must now regard them in a further point of view, not simply as mechanical agents, acting like the parts of an hydraulic machine, but as vital agents, endued with the properties of living matter, and forming parts of an organized system. Although nothing can appear more reasonable than this doctrine in the abstract, yet there are few topics in physiology which have been the subject of more controversy than the exact nature of the vital powers of the blood-vessels, and the consequent share which they have in the circulation of the blood. The mechanical physiologists, as might be expected from their system, regarded them as mere tubes, subject to no laws but those of mechanical impulse, and even some of the latest and most esteemed of the moderns do not admit them to possess either contractility or sensibility.

As to the latter quality, it is generally agreed that the blood-vessels possess no proper sensibility in their healthy and natural state, being, in this respect, analogous to many other parts of the body which are principally composed of membranous matter, and are not subject to the control of the will. How far the blood-vessels resemble the other membranous parts in the capacity of feeling pain, while suffering from inflammation or any other morbid state, is a question which appears to have been but little attended to; analogy would induce us to suppose that they possessed sensibility under these circumstances, and I know of no facts which would lead us to a contrary conclusion.

But the great controversy respecting the vital powers of the blood-vessels has been concerning their contractility, or rather that of the arteries, and this question has been resolved into another, which is intimately and necessarily connected with it, whether the arteries possess any muscular fibres, or whether the transverse fibres, which have been usually called the muscular coat, are justly entitled to that denomination. That the larger arteries actually possess a certain assemblage of fibres, placed in a transverse direction, is admitted by the concurrent testimony of all anatomists, but there has been a great difference of opinion respecting their nature and properties, or the effect which they produce in the animal œconomy.

The contractile power of the arteries was very warmly discussed by the anatomists of the last century, and was one of those points on which Haller exercised all his talents, both anatomical and physiological. To a certain extent he was the advocate for the muscularity of the arteries; he regarded the transverse fibres as clearly muscular; he, indeed, admitted that he could not exhibit their contraction by the application of the appropriate stimuli, but he adduced a variety of considerations to prove that the contraction of the arteries was an important agent in the circulation of the blood.[1] The contractility of the arteries was warmly advocated by Whytt[2] and Senac,[3] and may be regarded as forming the basis of Cullen's celebrated hypothesis of the action of the vessels. This hypothesis, which had so much influence over the general doctrines of pathology, as to introduce a totally new theory of some of the most important parts of the science, although not invented,[4] was so far modified and arranged by Cullen, as to possess a merit far superior to that of mere originality. The Galenic doctrine of the humoral pathology had been already

[1] El. Phys. ii. 1. 7; ii. 1. 13; iv. 4. 32; vi. 1. 39; Memoires sur la Nature Sens. et Irrit. des Part. du Corps Animal, mem. 2, sect. 11; his treatises "De Sanguinis Motu," "De Part. Corp. Hum. Sent. et Irrit." and "Responsio," in Opera Minora, t. i. On no topic is the candour and caution of this great man more apparent than in what relates to the muscularity of the arteries.

[2] Whytt's works, On the Circulation of the Fluids in the very small vessels of Animals; and Observations on Sensibility and Irritability, part ii. sect. 1.

[3] Traité du Cœur, liv. 5, ch. 3.

[4] The doctrine of the vital action of the vessels was perhaps first distinctly announced by Stahl, and was very explicitly stated by Gorter, but so combined with erroneous opinions as to lose much of its value. See Med. Compend. t. i. tract. 17.

called in question by Baglivi and others, and the hypothesis of the solidists had begun to make some progress in public estimation. But it was rather from the weakness of the old theory than from the consistency or force of the new one, that this change of opinion is to be ascribed, for it must be acknowledged that the opposers of the humoralists had been much more successful in controverting their antagonists than in proving the superiority of their own doctrines. Cullen was not satisfied with completing the destruction of the ancient edifice, but aspired to the honour of erecting a new one in its place, and consequently he did not think it sufficient merely to state in general terms that the vital actions of the body, and the changes which it experiences, depend upon the solids, but he proceeded to point out the agents by which these changes are effected, and these he determines to be the minute ramifications of the arterial system, or, as they have been named from their size, the capillary vessels.

The action of the capillary vessels has, since the time of Cullen, been generally regarded as the great agent in all the vital actions of the body, either physiological or pathological, by which any permanent changes are produced in its form or composition. This point we shall have abundant opportunities of illustrating in the account of the different functions as they will successively fall under our review, and with respect to the circulation in particular, there seems every reason to conclude, that it is very materially influenced by the capillaries, not merely as one of the means employed to propel the blood along the vessels in its ordinary course, but as the power by which all the subordinate changes in the state of the circulation are principally or entirely effected.

With respect to the share which the arteries themselves have in the motion of the blood, this acute physiologist remarks, that if the motion of the blood depended entirely upon the impetus impressed on it by the heart, its force and velocity in the different parts of the body must, at all times, bear the same ratio to each other. If the quantity and velocity of the blood in any two corresponding parts, as in the two arms, for example, be the same, provided the arteries be mere membranous tubes, the momentum of the blood in the two arms must always be precisely similar, and this will continue to be the case, whatever be the strength or velocity of the heart's motion, and however these may vary at different times.

But we do not observe this constant ratio to exist; on the contrary, the relative momentum of the blood in the different parts of the body is always varying, so that the quantity of blood and the velocity with which it moves are perpetually altered, both from the effect of external stimuli, and from a number of internal causes. This variable state of the momentum of the arterial system may be regarded as a decisive proof of the vital action of the vessels, and it may be considered as almost a necessary consequence, that this action must consist in alternate contraction and dilatation, and it is at least highly probable, that the contraction is effected by a muscular structure.[6]

An elaborate set of experiments was performed by Hunter, the object of which was to distinguish between the elastic and the muscular power of the arteries, in which the existence of this latter appeared to be fully established. The experiments proceeded upon the principle that the arteries possess both a proper contractile and an elastic power, that the former of these ceases with life, but that the latter remains as long as the parts retain their structure and composition. An animal was bled to death, by which the arteries are brought into a state of complete contraction; portions of them were then taken and slit open longitudinally, and were distended by a given weight in the transverse direction. After some time the weight was removed, and they were then left at liberty to resume their natural size, when it was found that they had undergone a certain degree of contraction, but less than that which they possessed before the experiment. They were measured in their three states, of ordinary contraction, of greatest distention, and of medium contraction; it was argued that the difference between their original size and the greatest distention of which they were capable, was the measure of the sum of their muscular and elastic contraction, while the degree in which they recovered themselves after the distention was the measure of their elastic force only, because after death this power alone could be supposed to remain in them, the muscular

[6] Thomson's Lectures on Inflammation, p. 66.

power necessarily ceasing with the cessation of life. By the weights that were employed, and by the degree of distention that was produced, Hunter endeavoured to estimate the exact relation which these forces bore to each other in the different vessels, or in the different parts of the vessels upon which he performed his experiments.[7]

Although I conceive there are various circumstances which will prevent us from arriving at any great degree of accuracy in the estimates that are deduced from experiments of this description, yet the principle upon which they were performed I believe to be correct, and they may be considered as certainly establishing the general fact, that the arteries possess a proper contractile, as well as an elastic power. We may also be allowed to depend upon them so far as to admit that they afford evidence of the general distribution of these respective forces; viz. that the larger arteries possess a greater proportion of the elastic, and the smaller of the muscular power, a conclusion which coincides with Cullen's speculations.

A number of experiments, essentially resembling those of Hunter, and which appear to have been performed with great care, were made by the late Dr. Parry, and have been lately repeated and extended by his son; from these, as well as from various considerations connected with the subject, they arrive at

[7] Hunter on the Blood, p. 124, et seq.; see Hewson's Exp. Enq. v. ii. p. 14, for a confirmation of the principles upon which the above experiments were conducted.

a conclusion which, in its most important features, may be regarded as coinciding with that which we have obtained from other sources.[8] He, indeed, conceives that the fibres which compose the middle coat of the artery are not properly muscular, because he supposes they cannot be made to contract upon the application of those stimulants which produce contraction in proper muscles; but he admits of a certain vital action in these parts, allowing a considerable degree of contraction of the arteries, necessarily connected with the life of the part, and which is a distinct property from what the vessels possess as elastic tubes; to this power he gives the name of tonicity.[9]

But although so many circumstances, both physiological and pathological, tended to favour the doctrine of the contractility of the vessels, still it remained defective in its most important point, that the transverse fibres could not be made to contract upon the application of stimulants. It had, indeed, been generally assumed, from their appearance and texture, that these fibres were muscular, but Haller's experiments were unfavourable to this opinion,[1] and it was strenuously opposed by Bichat, both upon anatomical and physical consider-

[8] See Dr. Hastings's remarks on Dr. Parry's experiments, in his Treatise on the Mucous Membrane, p. 20, 36, &c.

[9] On the Arterial Pulse, p. 52, et seq.

[1] Albinus, describing the anatomical appearance of the parts, without any regard to physiological hypotheses, says, that the transverse fibres of the large arteries do not resemble either muscle or tendon. Acad. Annot. lib. 4, c. 8, p. 32, 33.

ations.[2] We are also informed by Berzelius that the chemical analysis of these transverse fibres differs from that of the proper muscles,[3] and the same statement is made by Dr. Young.[4] But the objection, although proceeding from such high authority, will probably not be thought absolutely decisive, when we reflect upon the decided marks of contractility which is manifested by many of the lower classes of animals, as the Actiniæ, the substance of which is at least as unlike that of the muscles of warm-blooded animals, as the transverse fibres of the arteries can be conceived to be.[5]

[2] Anat. Gen. t. 1, p. 272, et seq. Nysten endeavoured to prove the correctness of Bichat's opinion, by showing that the aorta is not sensible to the stimulus of galvanism. Recherches de Physiol. p. 325, et seq. It must, however, be remarked, that the aorta was not the part of the arterial system where contractility is supposed to reside, and that we have experiments which lead to the contrary conclusion, although, perhaps, not so decisive as Nysten's. We must remark also, that Bichat supposes the contraction of the capillaries to be independent of the heart. Sur la Vie et Mort, p. 134. Lerminier, the writer of the article "Circulation," in the Dict. des Scienc. Med. argues, that the circular fibres are not muscular, nor the arteries contractile; but he restricts the term *artery* to the larger vessels, and supposes the capillaries to be distinct from either the arteries or veins; it does not, however, appear to me that any advantage is gained by this innovation, and it is not pretended that any precise limit can be fixed where this change takes place.

[3] View of Animal Chemistry, p. 25. [4] Med. Lit. p. 501.

[5] Dr. Young appears almost to afford a reply to his own objection, for at the termination of the paragraph he maintains that a part may be muscular which does not contain fibrin, and he adduces the case of the crystalline lens in support of his opinion. He sup-

But all the arguments of a general nature that have been adduced in this question are of little weight compared to the direct experiments which have been performed since the time of Haller, and which appear unequivocally to prove that the arteries possess a power in every respect analogous to, or rather identical with, that of the proper muscular fibre. Experiments of this description were performed by Verschuir shortly after those of Haller; they are related with much apparent candour and accuracy, and seem to prove very decidedly that the larger arteries contract upon the application of the ordinary stimulants,[6] but, from the high and almost overwhelming authority of Haller, they appear to have been but little attended to, and to have made scarcely any impression on the opinions of physiologists.

The results of Verchuir's experiments have been amply confirmed by the successive investigations of Dr. Philip, Dr. Thomson, and Dr. Hastings. Dr. Philip placed the web of the frog's foot in the microscope, and distinctly saw the capillaries contract upon the application of those stimulants which produce the

poses these fibres possess considerable elasticity. Med. Lit. p. 502. Dr. Jones, who, it may be presumed, had made himself familiar with the appearance of these parts, very decidedly pronounces them to be muscular; at the same time he says that they possess considerable elasticity. Treatise on Hæm. p. 2. Sir Everard Home observes, that the thin membranous coat of the hydatid possesses contractility, although its structure is so different from that of the muscles. Lect. on Compar. Anat. p. 30.

[6] De Arter. & Ven. Vi irrit. p. 20. 25. and 81.

contraction of the muscular fibre.[7] The same effect is described as having been the result of Dr. Thomson's experiments;[8] and it would appear that, in both these cases, the appearances were so obvious and decisive, as to admit of no question as to their reality, and scarcely of any doubt as to the cause producing them. This conclusion has been recently confirmed by Dr. Hastings, and he has farther extended his experiments to the large arterial trunks, and even to the veins,[9] and has observed in them, as well as in the arteries, the most clear evidence of their contraction upon the application of various stimulants, both chemical and mechanical.[1]

And besides the direct result of experiment, there are other circumstances stated by Dr. Philip, which are scarcely less decisive in favour of the contractility of the arteries. He informs us that he distinctly observed the circulation in the smaller vessels to continue for some time after the heart was removed from the body,[2] and the same observation was made by Dr. Hastings,[3] an effect which must necessarily have depended upon the action of the vessels themselves. Dr. Philip further informs us that the motion of the

[7] On Febrile Diseases, (3d ed.) v. ii. p. 17, et seq.; Med. Chir. Trans. v. xii. p. 401, et seq.

[8] Lectures on Inflammation, p. 83.

[9] The contraction of the veins was observed by Verschuir.

[1] Treatise on the Mucous Membrane, Introd. p. 24—28, 50—58, 61—64.

[2] Enquiry, ex. 24, 62, 63. Quart. Journ. xiii. 107.

[3] Introd. p. 51.

blood in the capillaries is influenced by stimulants applied to the central parts of the nervous system,[4] which must depend upon the capillaries possessing a proper contractile power, similar to that of the muscular fibre.[5]

It appears, therefore, that we are fully warranted in the conclusion that the arteries possess a proper contractile power, and it is to be presumed that this power resides in their transverse fibres. It appears likewise to be established, that this contractile power is principally seated in the capillary arteries, while the large trunks and the veins, although not destitute of it, possess it in a less degree. Indeed every fact with which we are acquainted respecting the mechanism and functions of the sanguiferous system, lead us to the same conclusion, that the large arteries are to be regarded as canals transmitting the blood from the heart, where it receives its great impulse, into the smaller branches, and that it is principally in these

[4] Enquiry, p. 291, 292.

[5] An indirect argument of considerable importance in favour of the contractile power of the arteries may, I think, be drawn from the occurrence of cases in which the heart has been either altogether wanting or completely defective in its structure, of which a well-marked instance is recorded by Mr. Brodie, Phil. Trans. for 1809, p. 161, et seq. We have a decisive instance of the same kind mentioned by Hewson, Exp. Enq. v. ii. p. 15. We may conclude, in these cases, that the arteries must have been the *compensating* organs; and it is more agreeable to the general principles of the animal œconomy to suppose that they were endued with an additional portion of their natural power, than that a completely new function was imparted to them.

smaller branches that it exercises its various functions. We are therefore to consider the large trunks in the light of a mechanical or hydraulic system, and the capillaries as physiological or vital organs. It may be remarked that this distinction, which is of the greatest importance, has seldom been clearly laid down by physiologists,[6] and that to this neglect may be attributed at least a part of that ambiguity which has so long prevailed respecting the properties of the arterial system.

With respect to the properties of the veins there is much less difference of opinion than with respect to the arteries. It is generally admitted that the veins exhibit the transverse fibres in small quantity only, or are nearly destitute of that part which is analogous to the middle coat of the arteries, and are consequently to be regarded as little more than mere elastic tubes. Their office is to return the blood to the heart after it has completed all its functions in the different organs, and has either expended that ingredient which gives it the power of acting upon the constituents of the body, or has acquired some addition which renders it inert or noxious. The action of the veins is therefore entirely mechanical, and the blood is transmitted by them to the auricles upon hydraulic principles, in a way which will be considered hereafter. With respect to their structure, so far as their physiological properties are concerned, it is only

[6] From this censure I must except the anonymous author in the Edin. Med. Journ. to whose judicious papers I have so frequently had occasion to refer.

necessary to remark, that all the large veins which pass along the muscles are furnished with valves so placed as to prevent the regurgitation of the blood towards the commencement of the vessels; the use of these valves, and the reason why they are confined to certain veins alone, will be further considered when we treat of the powers by which the circulation is effected.

Before we quit this part of our subject it will be necessary to make some observations upon the peculiar affection of the arteries which is denominated the pulse. It is well known that if the finger be applied with a moderate degree of force to certain of the arteries, a degree of pressure or increased resistance is experienced by the finger during the instant of the systole of the heart, or that period when the heart propels its contents into the arterial trunks. This constitutes the pulse; and until lately it was always conceived to depend upon the dilatation of the vessels produced by the increased quantity of blood that was projected into them during the contraction of the ventricle. The acuteness of Bichat led him to doubt whether the dilatation of the artery actually existed, or even if this were the case, whether it was sufficient to account for the phenomena, and he was disposed to refer it rather to a change in the position of the artery, produced by the sudden impetus of the jet of blood into it, analogous to the effect on the neck of the aorta, by which the apex of the heart is raised up and made to strike on the thorax. Still, however, the bulk of physiologists continued to attribute the pulse to the dilatation of the artery, until Dr. Parry instituted an experimental inquiry into the subject, the result of which was to show that not the smallest dilatation can be perceived in the larger arteries when they are laid bare during life, nor does he believe that there is any degree of displacement or unbending of the artery, which can account for the effect that is produced upon the finger.[7] He ascribes the pulse to "impulse of distention from the systole of the left ventricle given by the blood, as it passes through any part of an artery contracted within its natural diameter." This view of the subject appears to me to be correct; at the same time we may remark that it does not fundamentally alter our idea of the nature of the pulse, or the relation which its phenomena bear to the other parts of the system, or to the use that is made of it in pathology. According to this doctrine we must regard the artery as an elastic and distensible tube, which is at all times filled, although with the contained fluid not in an equally condensed state, and that the effect produced on the finger depends upon the amount of this condensation, or upon the pressure which it exercises upon the vessel as determined by the degree in which it is capable of being compressed. Upon this principle we can easily account for the different conditions of the pulse, and the relation which each of them bears to the action of the heart, whether

[7] We are, however, informed by Dr. Hastings, that the alternate dilatation and contraction of the larger arteries was sufficiently obvious to himself and his friends; it is implied, although not so stated, that this alternate action corresponded to or was connected with the pulse. Treatise on the Mucous Membrane, p. 31, note.

it expels a larger or a smaller quantity of blood, whether it is sent out of the ventricle with a greater or less force, whether the contraction be performed in a longer or shorter space of time, and other points of this nature, the consideration of which belongs more especially to the science of pathology.[8]

§ 7. *Efficient Causes of the Circulation.*

In considering this subject, we shall enquire first, into the nature and operation of these causes, and, secondly, into their amount or the degree of their effect. The causes themselves may be arranged under the heads of vital and mechanical, those which belong to the organs as parts of the living body, and those which they possess when regarded merely as a system of elastic tubes or membranous cavities. The vital powers of the constitution may be all reduced to the single effect of muscular contractility, by which, as has been so often remarked, the size of the cavity is diminished, and the contents necessarily pressed out. This is a real source of power in which motion is generated which did not previously exist, and the ventricles of the heart are the main spring from which this force proceeds. A similar source of motion appears likewise to originate in the arteries, especially in their capillary extremities, by which they produce an actual addition to the motion of the sanguiferous system, subsidiary to that of the heart. The power of the arteries is, however, entirely subservient to that of the heart,

[8] For some judicious observations upon this subject see an article in the Journal of Medical Science, No. 38.

and much inferior to it in quantity, for we find that the pulsations of every part of the arterial system, although situated at such various distances from the centre, are all synchronous, and contemporary with the contraction of the ventricles. Hence it appears that the impulse which is given to the blood in the trunks of the great arteries is propagated to the remotest parts of the system, and that they are immediately brought into their state of distention.

It was a point which was much contested by the earlier physiologists, especially by those of the mechanical sect, whether the elasticity of the vessels promoted the flow of the blood through them. As a question concerning the mere quantity of power, we apprehend there can be no doubt of its insufficiency, because it is a principle which is now generally admitted, that simple elasticity is not a source of power, but only a means of distributing the power generated by other causes in a new direction. Although, therefore, the present constitution of the blood-vessels is much more convenient and more conducive to the well-being of the animal œconomy than a set of rigid tubes would have been, their membranous coats are not to be considered as, in any degree, contributing to the actual power by which the blood is propelled along them.

Among the mechanical causes which promote the circulation of the blood there is one which is generally supposed to be considerably efficacious, especially in the venous part of the circulation, the pressure which the muscles, during their contraction, exercise upon the veins. Whenever a muscle contracts, so as to have its ends brought nearer to each other, its belly is proportionably increased in thickness, and it is evident that in this change of form all the tubes that pass through the muscles, or in contact with them, will be compressed. When this compression is exercised upon an artery, its operation will be rather to retard than to promote the flow of the blood, in consequence of its diminishing the capacity of the vessels, and rendering them less able to yield to the distending force of the heart; but in the veins any effect of this kind will be much more than compensated, in consequence of the numerous valves which these vessels contain. As the veins are less firm or dense than the arteries, and are generally so situated as to be more subject to be acted upon by the muscles, it follows that the effect of external pressure will be experienced in a greater degree than by the arteries, and therefore that the balance will be considerably in favour of the transmission of the blood towards the heart. The use of these valves is sufficiently obvious from their form, and it is still further proved by the circumstance, that those veins alone are furnished with them which accompany the muscles, especially those of the extremities, while the veins belonging to the internal viscera, and those connected with the system of the hepatic circulation, are not provided with them.

There is a mechanical cause which has been pointed out as contributing to the circulation of the blood, to which the name of derivation is applied. When the ventricles have contracted, and have expelled their contents into the arteries, they relax, and by this means become again increased, and consequently a vacuum would be produced in them, were it not that they are immediately filled by the flowing in of the blood from the contiguous auricles. The fluid in this case is supposed to leave the auricle, not from the muscular contraction of this organ, but from its necessarily flowing into that part where there is the least pressure. The effect of derivation, as influencing the circulation of the blood, is a circumstance which seems to have escaped the older physiologists; it is mentioned, although obscurely, by Haller, but was first brought forwards in a clear and definite manner by Dr. Wilson, in a short essay, which contains some ingenious speculations, connected with much of what is incorrect, both as to matter of fact and the inferences that are deduced from them. After remarking upon the insufficiency of the force of the heart to propel the blood through the whole course of the circulation, he goes so far as to state that the heart is not the organ of the motion of the blood, and that it even acquires no actual addition of motion in passing through the heart. The sole, or at least by far the most important power of the heart, according to this author, is its faculty of absorbing the blood from the veins, which it does upon the principle of an exhausting machine; by throwing out the blood from its cavities a space is left into which the contents of the veins are immediately discharged, because there is the least resistance in this direction.[9]

The views which had been thus imperfectly opened

[9] Wilson's Enquiry, p. 9, 11, 16, 35, et alibi.

by Dr. Wilson have since been more fully disclosed by Dr. Carson. In an elaborate dissertation, in which he very fully discusses the nature and extent of the powers of the circulating system, he so far coincides with the generally received opinion, as to conceive that the contractile force of the heart transmits the blood through the arteries, and even into the extremities of the veins, but that this *vis à tergo*, as it is termed, would not be competent to complete its return into the right auricle. To accomplish this object Dr. Carson has recourse to what he terms the power of suction, which, on the principle suggested by Dr. Wilson, impels the venous blood into the cavities of the heart, to fill up the vacuum which would otherwise be formed there. This diminution of resistance to the entrance of the venous blood into the heart is brought about in two ways: it is supposed that the construction or disposition of the muscular fibres of the ventricles is of that kind, that when they relax, the organ is necessarily dilated, a circumstance which depends upon the fibres being twisted, so as to give to the parietes of both the ventricles a shape somewhat resembling that of the figure 8. A still more powerful agency, however, he conceives to be derived from the action of the lungs. These, it is supposed, are always in a state of forced distention, and would consequently collapse by the pressure of the atmosphere, were they not contained in a rigid case which secludes them from its operation. In one part, however, where the membranes of the heart are connected with the pleura, the walls of the thorax are merely membranous, and are therefore subject to the influence of this external pressure, which, acting through the intervention of the pericardium, keeps its cavities in a state of dilatation, and, as the external surface of the heart is always in contact with the internal surface of the pericardium, the auricles must also of course be kept distended, except when they contract by their vital energy. Hence, what may be termed the natural condition of both the auricles and ventricles is a state of dilatation, so that immediately after each of them have completed their systole, and relaxation ensues, they become again dilated, and necessarily receive the blood which is contiguous to them in the veins, as the valves will prevent the return of that which has once passed through them.[1] I conceive the general conclusions of Dr. Carson to be fully established, so far as respects the inadequacy of the contraction of the heart and arteries to return the blood to the right auricle, and also the actual existence of the principle of derivation, by which the venous blood is poured into the auricle and ventricle, because it meets with less resistance in this quarter than in any other direction. I am, however, disposed to doubt the validity of some parts of his reasoning, while there are certain positions which appear to be decidedly incorrect. In the latter predicament I consider the alleged condition of the lungs, as being always retained in a state of forced distention, a point which I shall discuss more at large when I treat upon the function of

[1] Carson's Inq. pp. 97, 108, 117, et alibi.

respiration, while among the doubtful positions may be placed the supposed dilatation of the heart by the mere relaxation of its fibres. If we conceive of the relaxation of a set of fibres of this description, as forming the parietes of a double cavity, it will have the effect of leaving this cavity in what may be termed a passive state, so as readily to yield to any distending force from without; but this is merely a negative effect, and will not account for the entrance of the fluid into it without some other more active principle. This I conceive to be a mechanical cause depending upon the structure of the heart, as consisting of a substance which possesses a great degree of elasticity, so that when its cavity is diminished by the contraction of the muscular fibres, as soon as the contractile power ceases to operate, the elastic force comes into play, and tends to bring it back to its former shape. We may form a clear idea of the operation of this supposed principle by regarding the heart in two ways, first, as consisting of a flexible and inelastic bag similar to a moistened bladder; and, afterwards, as composed of a bag of similar dimensions formed of caoutchouc. We may imagine that each of these has the same apparatus of muscular fibres; when the first of them is filled with blood, the muscular fibres contract, reduce its size, thereby expelling its contents, and leave the bag in a collapsed state; whereas, in the second case, after the fibres have contracted and expelled the blood, the elastic nature of the organ causes it to resume its rounded form, and to leave a cavity nearly as considerable as before the operation. This cavity will of course be immediately filled by any fluid which is in contact with it; and in the case of the heart the blood that is in the auricles, without any action on their part, will immediately flow into the ventricles, and will itself be succeeded in the auricles, by an equal quantity of blood from the veins. We have, perhaps, no very decisive experiments or observations to prove that this is actually the state of things with respect to the heart, but it appears very probable that it is so, at least to a certain extent, and therefore I do not hesitate to enumerate derivation, or, as it might be more correctly and less hypothetically termed, the elasticity of the heart itself, as among those causes which assist in the circulation of the blood.[2]

Contrary, however, to what appears to be the opinion of those who have treated on this subject, I cannot admit that the principle of derivation is an actual source of power, or that there is any generation of motion produced by it. Like other forces which depend upon elasticity, it is to be regarded solely as giving a new direction to a power previously existing, the origin of which, in this case, is the

[2] See Dr. Hastings's observations upon Dr. Carson's hypothesis, Treatise on the Mucous Membrane, p. 8, et seq., and Philip in Med. Chir. Trans. xii. 397, et seq. It is to this elasticity of the heart that we may refer a certain degree of re-action which it appears to exert during its diastole, and which M. Magendie observes is something more than a mere passive operation. Physiol. t. ii. p. 329. The same opinion, as has been stated above, p. 173, was maintained by Bichat.

contractility of the ventricles. It may indeed be regarded as a very beautiful contrivance for equalising the force employed in the circulation, and for transferring, as it were, part of it from the commencement to the termination of the passage of the blood along the vessels; but I apprehend that precisely as much power as is gained by the influx of the blood from the auricles into the ventricles, must have been previously exercised by the muscles of the ventricle in overcoming the increased resistance caused by the increased elasticity of its substance. We may conclude therefore that the efficient causes of the circulation of the blood are the contractility of the muscular fibres of the heart, that of the capillary arteries, and external pressure upon the veins, principally produced by the contraction of the muscles.

After considering the causes or powers which promote the flow of the blood along the vessels, it will be necessary to take a cursory view of those which tend to retard it. These are very numerous, and some of them very considerable; they may be all referred to the head of mechanical causes, because any circumstance which tends to diminish the vital energy of the heart or arteries acts merely in a negative manner, diminishing the effect of their contractility. The most important of the mechanical causes which retard the motion of the blood are the following; the physical composition and structure of the vessels, as being imperfectly elastic, flexible, pursuing a winding course, ramifying into branches which go off at considerable angles from the trunks, the branches occasionally uniting, by which the streams of blood meet each other in opposite directions, and the circumstance of the sum of the areas of all the branches being greater than that of the main trunk. The resistance which the blood already in the vessels opposes to the entrance of any fresh quantity is the cause of a prodigious expenditure of power, while the nature of the blood itself, as being a thick and tenacious fluid, its adhesion to the sides of the vessels, and the friction which it must experience in passing along so extensive a system of tubes, afford other causes of the loss of power, the amount of which must be very great. Those who are versed in the laws of hydraulics will perceive the reality of all these causes of retardation, and will be aware that they operate independently of the vital power of the organs, acting upon them as they would on a system of tubes possessed of the same physical properties, but without any of those functions which are peculiar to the living animal.

When we consider how much the powers of hydraulics are concerned in the circulation, we cannot be surprised that the mechanical physiologists, who applied their science to many points of the animal œconomy to which it had so little relation, should have bestowed unusual attention upon every circumstance connected with the action of the heart, and the motion of the blood along the vessels. We shall accordingly find that some of their most elaborate calculations were directed to this subject, and that they bestowed upon it no less attention than upon the cause and nature of muscular contraction. Upon the whole, the result of their labours has been almost as unfortunate in this as in the former case, for although the data are less obscure, as far as concerns the nature of the powers employed, yet we are totally unable to ascertain the amount both of the powers which promote and those which retard the circulation of the blood.

With respect to mathematical reasoning in general, it must be admitted that, when it is cautiously applied, it has enabled us to arrive at physiological truths, which we perhaps could not have attained by any other method, and which are beyond the reach of actual observation. But when we call in the aid of mathematics to assist us in our researches, it is of the utmost importance to ascertain that our data be well founded, and that we are not misled by false analogies, or by the mis-application of principles which may be in themselves correct. But the mechanical physiologists fell into the fatal errors of assuming principles which were incorrect, of adopting data which were of doubtful authority, and of applying them in an incorrect manner.

To relate all the theories, hypotheses, and calculations that have been formed upon the subject of the circulation, would be an idle expense of time and labour, but it may be proper to give an account of some of the attempts that have been made to estimate the force of the heart, because they were conceived by men eminent for their learning and ability, and afford an excellent specimen of the method of reasoning that was fashionable at the time when they flourished.

Borelli, proceeding upon his hypothesis, that the power of muscles is in proportion to their weight, estimated that the force of the heart is equal to the enormous sum of 180,000lb. Keill perceived the extravagance of Borelli's calculation, and attempted to arrive at the truth by a more complicated process. He set out by the position that the force of the heart produces two effects; it expels a quantity of blood from its cavities, and communicates motion to the contents of the arteries. He first attempted to estimate the quantity of blood thrown out of the heart by each of its contractions; and by taking the diameter of the aorta, he could then calculate the velocity with which it passes along this vessel. He found the quantity of blood to be about two ounces, the area of the great vessel to be about three-fourths of a square inch, and he conceived that the actual contraction of the ventricle would occupy about the two-hundredth part of a minute. Hence he found that the blood sent into the aorta would compose a cylinder of about eight inches in length, and be driven along with a velocity of 156 feet in a minute. In producing this velocity the heart has not only to expel the blood, but to overcome all the resistances in the vessels, and the next step was to ascertain their amount. For this purpose he opened a living animal, and laid bare the iliac artery and vein. Now he argued that all the blood which passes through any artery must be returned by the corresponding vein in the same time, but with a different degree of force; the arterial blood has to overcome all the resistances which occur in the course of the circulation, the venous blood possesses only the force which remains after the resistances have been overcome. He opened the iliac vein and received all the blood that flowed out in ten minutes, and afterwards he opened the artery and received the blood that flowed out during the same length of time, when he found that the quantity of blood which he obtained from the artery was to that from the vein as seven and a half to three, or as two and a half to one. He therefore concluded that the first of these numbers may be regarded as the measure of the velocity which the blood receives from the full force of the heart; and the second, the velocity with which it moves after it has overcome all the obstacles which it meets with in its passage through the vessels. Proceeding upon this principle, the velocity in the aorta, without the resistances, are estimated at 390 feet in a minute, or nearly six feet and a half in a second. From this datum, by means of the Newtonian theorem, he estimates the force which is necessary to move a given column of blood, with a known velocity in a given time, and this he determines to be five ounces and a half, or about half a million of times less than the calculation of Borelli.

We cannot but give the merit of ingenuity to Keill's reasoning, but it is obviously incorrect in many particulars. In the first place, a great mass of resistance is opposed to the blood at its entrance into the aorta, which must have been overcome before it arrives at the iliac artery, on which Keill made the experiment. In the second place, the quantity of blood which flows from a divided vessel is no measure of what passes through it at other times, as on the principle of derivation, blood will be sent from all the neighbouring vessels to a part where the resistance is removed. In the third place, we are not certain that the same quantity of blood is returned by what are called the corresponding veins; and it is also probable that, in Keill's experiments, a greater quantity than ordinary would pass off by the veins, in consequence of the anastomoses that veins have with each other. But it is unnecessary to dwell any longer upon these objections, enough having been said to show that the estimate does not afford even an approximation to truth.

The only other calculation of this kind which I shall notice, is that of Hales. Hales was remarkable for the purity and integrity of his character, and his ardour in philosophical researches, and he bestowed a large part of his time on experimental inquiries into the nature of vegetable and animal bodies. He had too much candour to be blindly devoted to any sect, but the genius of the age in which he lived was so decidedly in favour of the employment of mathematical reasoning in every department of philosophy, that he entered very fully into all the views of his predecessors respecting the mechanical powers of the sanguiferous system. He attempted to estimate the relative force of the arteries and veins by inserting tubes into the great vessels near the heart, and observing the comparative height to which the blood was impelled into them. This he found to vary in different experiments, but it was always considerably greater in the arteries than in the veins, upon an average, as about ten to one. In order to ascertain the absolute force of the heart, Hales inserted tubes into the aorta, soon after it leaves the left ventricle, when he found the column of blood raised in the tube to be of such a height, that by comparing it with the cavity from which it was projected, and taking into account the time and the area of the vessel, the force of the heart would be about 50lb.[3] Perhaps Hales's estimate may not be very remote from the truth, yet there are many points in which it is defective, even regarding the heart as an hydraulic machine; and it is obvious, that when we consider contractility as a variable power, depending upon a number of causes connected with life, which it is impossible to appreciate, we shall be convinced of the futility of such calculations. Hales's works, however, contain much important information, the direct result of experiment, which will always render them a valuable magazine of facts for the philosophical inquirer into the actions of the sanguiferous system.

§ 7. *Of Inflammation.*

Before I quit the subject of the circulation it may be proper to offer a few remarks on the nature of inflammation, a point indeed more immediately connected with pathology, but yet of considerable im-

[3] Statical Essays, v. ii. p. 38, 40.

portance considered in its physiological relations. The phenomena of local inflammation, to which I mean principally to confine my observations, vary according to the structure and functions of the part affected, yet there are some circumstances which may be considered as common to all cases, of which the four following are considered the most essential,—redness, heat, pain, and swelling.[4] It is generally agreed that the capillaries are the immediate seat of the inflammatory action, and that when any change occurs in the large vessels, it is to be attributed to a secondary or consequential operation, originating from the affection of the minute arteries. All the four symptoms mentioned above,—redness, heat, pain, and swelling, may be attributed to one primary cause, an increase in the size of the minute vessels, by which they are enabled to admit of more than the ordinary quantity of blood to be received into them, but a great diversity of opinion has prevailed concerning the mode in which the blood is conveyed or retained there, or concerning what has been termed the proximate cause of inflammation.

After the vital power of the vessels had been established by Stahl and his disciples, the phenomena of

[4] Cullen's First Lines, § 235; Thomson's Lectures on Inflammation, p. 42; see also, J. Hunter's Treatise on the Blood, c. 3, a portion of this celebrated work which cannot be too carefully studied, both for its valuable observations and for its profound insight into the operations of the animal œconomy; yet even here we have to lament the metaphysical subtilties in which the author is occasionally involved.

local inflammation were referred to this action, and as all the natural operations of the arterial system seemed to be augmented during this state, so it followed, almost as an obvious consequence, that inflammation essentially consists in increased action of the capillaries.[5] A strong objection to this supposition was, however, quickly perceived, that the effect of the vital action of the vessels is contraction, while, as we observed above, the very essence of inflammation consists in an increase of the capacity of the vessels. But the ingenuity of the physiologists was not baffled by this difficulty; various means were invented to accommodate the theory to the facts, depending upon the presence of some obstacle which the blood was conceived to meet with in its passage through the vessels, which, at the same time, was not incompatible with their increased diameter. Boerhaave attributed the obstruction to a change in the texture of the blood itself, by which it became more thick and viscid, acquiring, what he called, a state of lentor, and to this lentor he added the further hypothesis, that the increased action of the arteries forces the larger particles of the blood into vessels, which, in their natural condition, are too small to receive them. This constitutes the *error loci* of the mathematical physiologists, and by the lentor and *error loci* were the phenomena of local inflammation explained

[5] For a clear and candid statement of the opinions which have been entertained upon the state of the blood-vessels in inflammation, I may refer the reader to Dr. Thomson's Lectures on Inflammation, p. 61—75; and Hastings's Treatise on the Mucous Membrane of the Lungs, p. 67, et seq.

by Boerhaave and his numerous disciples,[6] until this, like most of their other speculations, was assailed by the powerful genius of Cullen. In place of the mechanical doctrine of Boerhaave, Cullen substituted his favourite hypothesis of spasm;[7] for he admitted the increased action of the vessels in local inflammation, while he was aware, that without some counteracting circumstance, this increased action would produce effects totally inconsistent with the actual phenomena.[8] We shall, however, find, upon due reflection, that the spasm of Cullen is equally unfounded, and perhaps even less intelligible, than the lentor and *error loci* of the Boerhaavians; and accordingly his explanation has been generally regarded as incomplete, yet since his time no regular attempt has been made to reconcile the increased action of the vessels with their enlarged diameters.

In this dilemma a totally different view of the question has been taken, according to which local inflammation is to be attributed to a diminished action of the capillaries. This hypothesis, which appears to have been originally proposed by Vacca, an Italian physiologist, about the middle of the last century, was first brought forwards in a clear and consistent form by Mr. Allen, who, for some years,

[6] Aphor. 110, 122, 370, et seq. cum Sweiten. Comment.

[7] First Lines, § 244, et seq.

[8] Haller was quite aware that an increased action of the arteries must have a tendency to diminish their capacity, and employed this consideration as an argument against the muscularity of the capillaries.

lectured in Edinburgh on the animal œconomy; it was adopted by Dr. Wilson Philip,[9] who performed a series of experiments in support of it, and has been since embraced by Dr. Parr,[1] Dr. Thomson,[2] and Dr. Hastings.[3]

According to Mr. Allen's hypothesis, the redness, heat, pain, and tumour, are to be ascribed to the increased quantity of blood which the vessels contain in consequence of their relaxed state; the symptoms, therefore, which had been usually attributed to excessive action he ascribes to this partial stagnation of the fluids, together with a kind of struggle between the

[9] Wilson on Febrile Diseases, v. iii. p. 15—73.

[1] Dictionary, Article "Inflammation."

[2] Lectures on Inflammation, p. 70.

[3] On the Mucous Membrane, p. 71, et seq. Dr. Thomson and Dr. Hastings likewise supported the hypothesis by numerous experiments. Although Philip, Thomson, and Hastings, agree in the main point, that inflammation essentially consists in diminished action of the capillary arteries, they differ respecting the actual state of the vessels. Dr. Philip supposed that the constant effect of inflammation is to dilate the vessels, and to diminish the velocity of their contents. Treatise on Febrile Diseases, v. iii. p. 15, et seq.; also Preface to 4th ed. p. vii.; and Med. Chir. Trans. v. xii. p. 407. Dr. Thomson concludes that the velocity is sometimes increased and sometimes diminished; Lect. on Inflammation, p. 89; while Dr. Hastings adopts the opinion of Dr. Philip, that in the proper inflammatory state, the velocity of the fluids is always retarded; on the Mucous Membrane, p. 91, et alibi. It may be worth noticing, that the article "Inflammation," in the Dict. des Scien. Med., written by Boyer in 1818, contains no account of either the hypothesis or experiments of the English physiologists: inflammation is referred, according to the old doctrine, to the increase of vital action.

loss of power in the part, and the unusual stimulus to which it is thus subjected. Although this doctrine of the proximate cause of inflammation may, at first view, seem to counteract our established notions upon the subject, yet it will, I think, be found upon examination to be more consistent in all its parts, and to accord better with the various phenomena both of pathology and of physiology, than any of the speculations which had preceded it, derived from the principle of increased action combined with obstruction. And indeed if we take into account that the exact seat of the inflammatory action is not visible to the eye, and that, according to the hypothesis of Dr. Hastings, the state of active inflammation consists in an increased action of the larger arteries, while the capillaries are in their natural state,[4] it will approximate this hypothesis, at least as to all pathological and practical consequences, pretty nearly to the old doctrine. But I must add, that I think we are not arrived at that degree of knowledge on the subject which will enable us to form a decisive opinion respecting it. Waving, therefore, the regular discussion of the hypothesis, I shall conclude by a few observations, which may be of some use to those who are inclined to investigate the proximate cause of inflammation with more minuteness.

Although we may conceive that the phenomena of inflammation are as readily explicable upon the hypothesis of diminished as of increased contractility of the capillaries, yet it appears to me that both the

[4] On the Mucous Membrane, p. 106.

exciting causes of this affection and the treatment coincide more with the idea of excessive than of defective action. All those circumstances which we are usually in the habit of considering as stimulants excite inflammation; and where the same effect is brought about by sedatives, or by agents of a more doubtful operation, still we can generally perceive the existence of what has been termed re-action, which is the immediate precursor of the change in the state of the circulation. In the same way the remedies for inflammation appear to me more adapted to remove or relieve an excess than a defect of vital energy, as for this purpose, except under peculiar circumstances, we always apply either direct or indirect sedatives, and find stimulants to be as injurious as the others are beneficial. From these considerations I am induced to recur to the former idea of increased action being the proximate cause of inflammation, or at least as being essential to it, and to inquire whether there be no correct method of combining a state of increased action with distention of the vessels. This, it is obvious, must be accomplished by obstruction in some form or other, either arising from the nature of the fluid, or from the difficulty with which it leaves the vessels after it has entered into them. Now, although we may agree with Cullen that Boerhaave adduced no sufficient proof of the existence of his lentor and *error loci*, yet it does not follow that no alteration in the condition of the blood exists. May we not conceive, that by the inflammatory action the proportion of fibrin is increased, or that the fibrin already pre-

the increase of fibrin should precede the inflammation sent acquires a greater tendency to coagulate? That the solid contents generally of the blood are augmented, either by an increased quantity being thrown into the vessels, or a portion of the more fluid part being removed? May not some new arrangement take place with respect to the globules, so that they may coalesce or be more strongly attracted together? Or, without having recourse to any speculations of this kind, may we not conceive it possible, that if the minute arteries are contracting more vigorously than ordinary, their relaxation will be proportionably great, so as to allow of the different parts of the blood, as the fibrin and the globules, to be admitted into vessels which are generally impervious to them, and which, when once entered, from the *vis à tergo* on the one hand, and from the decreasing diameter of the vessels as they divide into small branches on the other, are forcibly detained, and produce all those symptoms which seem to originate from mechanical obstruction? These considerations are thrown out rather to show what may be conceived as a possible occurrence, than from the idea of our possessing any evidence of their actual existence. They may, however, show that we have not a sufficiently intimate acquaintance with the subject to enable us to decide peremptorily respecting the proximate cause of inflammation.

CHAP. VI.

OF THE BLOOD.

§ 1. *Remarks on the Progress of Animal Chemistry.*

After the description of the course which the blood follows in the circulation, and of the mechanism by which its motion is produced and regulated, I must now proceed to consider those functions by which its physical and chemical properties are changed, so as to become subservient to the growth and nutrition of the body. The blood is, however, a fluid of a very compound nature, the different constituents of which are possessed of peculiar properties, and are retained in a state of combination, in many respects, unlike that of any other substance with which we are acquainted. It will therefore be desirable, before we proceed to the other functions, to give some account of the nature and properties of the blood; to this I shall prefix a few remarks upon the history and present state of animal chemistry.

During the earlier part of the eighteenth century, when chemistry first began to assume a scientific form, and when the experimentalist proposed to himself some definite and intelligible object of inquiry, the analysis of animal and vegetable substances naturally attracted a share of his attention. The mode of examination which was then adopted was indeed little calculated to throw any light upon the subject, for it consisted almost entirely in submitting the substances to the process of distillation at a high temperature, by which their primary compounds or proximate principles were entirely destroyed, and either converted into new compounds, which did not previously exist, or resolved into their ultimate elements. Many of these latter were of a gaseous or volatile nature, and from the operator being, at that period, ignorant of the properties, or even of the existence of such bodies, they were suffered to escape, and were totally disregarded, while the solid or fluid products that were obtained were, in all cases, nearly similar to each other, and conveyed no idea of the nature of the substance from which they were procured. They principally consisted of a carbonaceous residuum, with a quantity of empyreumatic oil and ammonia, which two latter substances were generated during the process.

The first radical improvement in animal analysis consisted in substituting the action of various re-agents for this method of destructive distillation, an improvement for which we are principally indebted to the French, and especially to some members of the Royal Academy of Sciences, who, about the middle of the last century, were led to investigate the composition of organized bodies, and soon perceived the little benefit that had been derived from the former method of proceeding. One of the most active of those who were engaged in this new pursuit was Rouelle; he subjected the substances which he wished to analyze to the action of alcohol, acids, alkalies, and other powerful re-agents; he noticed the effect of simple exposure to the atmosphere, he examined the changes produced by temperature and moisture, and, at the same time, carefully watched the progress of spontaneous decomposition. Neumann of Berlin, and some other of the German chemists, proceeded upon the same plan with Rouelle and his associates, and, at a somewhat later period, Macquer and Baumé materially contributed to our knowledge of this department of chemistry by their publications. Hitherto it had been little attended to in this country; Hales indeed made the important discovery, that a permanent gas was obtained from various animal substances by distillation,[5] but it was an insulated fact, the nature of which was not understood, so that it led to no further improvement; and it was not until the establishment of the pneumatic chemistry, as it has been called, for which we are so much indebted to Black, Cavendish, and Priestley,[6] that the nature of the results could be

[5] He informs us, that he obtained "a considerable quantity of permanent air" from blood, fat, and various other animal substances, and more especially from urinary calculi. Stat. Ess. v. i. p. 173, 193, (1769.) Scheele's Experiments on Calculus were published in 1776; see Marcet on Calculous Disorders, p. 63.

[6] It was from the latter of these philosophers that I received my first instruction in chemistry, and I cannot mention his name without offering to his memory my grateful tribute of respect and admiration. His merit as a chemical discoverer of the first order is so generally acknowledged, that it may appear almost unnecessary to enlarge upon it, yet I believe that few persons who have not particularly attended to the subject are aware of the full extent

understood, or the true method of analysis be clearly comprehended. The experiments of Priestley, in which he obtained azotic gas by treating animal substances with nitric acid,[7] constituted a very important advance in our knowledge, but we must admit that it was chiefly by the labours of the French, and especially by those of Fourcroy and Berthollet,[8] that the foundation was laid of the correct information which we at

of our obligation. Those who are disposed to investigate this point should peruse his original six volumes of experiments, and compare the information which they contain with the chemical publications which immediately preceded them. In originality, in quickness of apprehension, and in diligence, he has probably never been surpassed; but I conceive that his judgment was by no means equal to his genius, for, although we are astonished with the variety and extent of his discoveries, we are not unfrequently surprised at the futility of his hypotheses and the weakness of the arguments by which he defends them. So far as I am capable of forming an opinion, the same character will apply to his other publications. Of his strictly theological works I do not profess to judge; but two of his most elaborate performances, which were intended for general use, the "Disquisitions," in which he attempts to identify the phenomena of mind with those of matter, and the "Letters to a Philosophical Unbeliever," the object of which is to prove that the credibility of supernatural events rests upon the same grounds with that of the ordinary operations of nature, while they abound with ingenious remarks and original conceptions, appear to me to be both of them founded upon fallacious principles.

7 Priestley's experiments, in which he procured "phlogisticated air" by the action of nitric acid on animal substances, were published in 1775. Experiments on Air, v. ii. p. 145, et seq. (Original series of six volumes.) Fourcroy says that Berthollet *discovered* azote in animal substances in 1784. System, v. ix. p. 42.

8 See Berthollet's Memoirs on the Analysis of Animal Substances; Mem. Acad. for 1785; also Journ. de Phys. t. xxviii. p. 272, et t. xxix. 389.

present possess on this subject. Latterly, indeed, the English have had at least an equal share in promoting the science of animal chemistry; I have already had occasion repeatedly to refer to the labours of Mr. Hatchett, and, in addition to his name, we may select those of Wollaston, Pearson, Marcet, Henry, Prout, Brande, J. Davy, Thomson, and Ure. In France, those who are at present the most distinguished in this department are Vauquelin, Guy-Lussac, Thenard, Chevreul, Proust, and Bouillon la Grange; and we must not omit to acknowledge the great obligation under which we lie to Sweden, formerly in the person of Scheele, and now in that of Berzelius, whose genius and assiduity have rendered him almost equally illustrious in every branch of chemistry.[9]

The method at present employed in the examination of animal substances may be considered as combining three distinct sets of operations. The first consists in noticing the effect of external agents upon the substance in question, and observing its spontaneous changes; the second depends upon the application of re-agents, which are used either in the way of tests, to indicate the existence of particular elements or primary compounds, or as menstrua, which, by their specific affinities, may separate the elements or primary compounds from each other; while the

[9] For a sketch of the progress of animal chemistry, see Fourcroy's System, v. ix. p. 33—56; and Berzelius's essay expressly on this subject. It is much to be regretted that these writers should have entirely omitted to give any particular references to the experiments or opinions which they detail.

third set of operations are to be regarded as, in some measure, a return to the original plan of destructive distillation, but with this very essential difference, that, in the modern analysis, we carefully collect all the gaseous and volatile matter, and by ascertaining its nature and the amount of its elements, we estimate the nature and amount of the compounds into which they previously entered as constituents. We procure, for example, a certain quantity of carbonic acid gas and of water, and we know the proportion of carbon, oxygen, and hydrogen, which they respectively contain; the azote remains uncombined and is collected in the gaseous state, so that we are able to ascertain the amount and relative proportion of the elements which entered into the constitution of the substance analyzed. This method of discovering the nature of a body, by resolving it into other bodies of known composition, was first practised, although in rather a rude manner, by Fourcroy and Vauquelin; it was afterwards very much improved by MM. Gay-Lussac and Thenard, and still more so by Prof. Berzelius and Dr. Prout, from whose science and skill it has arrived at a very great degree of perfection.[1]

[1] For an account of the modern analysis of animal substances, see Thenard's "Traité," t. 4; Children's Translation of the same, containing much valuable additional matter; an Essay by Berzelius, in the 4th and 5th vols. of Annals of Philosophy; Dr. Ure's paper in Phil. Trans. for 1822; and the various papers of Dr. Prout, in the Med. Chir. Trans. and the Annals of Philosophy.

§ 2. *Nature and Properties of the Blood.*

I shall now revert to our more immediate object, the nature and properties of the blood. After describing this fluid in its entire state, I shall give an account of the primary compounds, and proximate principles, into which it may be separated, either by its spontaneous changes, or by the application of re-agents. In the next place I shall notice some of the alterations that are brought about in the blood by the natural functions of the system or by the effects of disease, and I shall conclude by some observations upon the opinions that have been successively entertained respecting its various constituents.

Blood, when first drawn from the vessels, is an adhesive fluid, of an homogeneous consistence, of a specific gravity of about 1·050, in man and the more perfect animals of a red colour, of a slightly saline taste, and, in the human subject, of the temperature of about 98°. Soon after it leaves the vessels, if it be suffered to remain at rest, it begins to coagulate; and as this process advances, it will be found to separate into two distinct parts, so that we at length obtain a red mass floating in a yellowish fluid: the red part is called the clot or crassamentum, and the fluid part the serum. In venous blood, which is generally employed in these experiments, the average period of coagulation is said to be about seven minutes. The proportion of the two constituents has been variously estimated; it is not easy to obtain any accurate result, because the separation is by no means complete, a portion of the serum always remaining attached to the clot; and by attending to the state of the blood, we find that the proportion varies considerably in different individuals, and even in the same individual at different times. It has been stated, as a general average, that the crassamentum amounts to about one-third of the weight of the serum, and perhaps this may not be far from the truth.

As the fluid of warm-blooded animals, when first drawn from the vessels, possesses a temperature considerably greater than that of the atmosphere, it has been made the subject of experimental inquiry, what is the rate of the cooling of the blood, compared to that of water raised to the same temperature. The greater viscidity of the blood must necessarily tend to retard the escape of heat from it, but, besides this, it has been conceived that the coagulation of the fibrin, like other processes in which a fluid is converted into a solid, should cause the absolute extrication of caloric. Fourcroy relates an experiment, in which, during the formation of the clot, the thermometer rose no less than 11°,[2] but as the particulars were not mentioned, and as the result appeared to be in contradiction to some facts adduced by Hunter[3] and others, the conclusion was not generally admitted. Fourcroy's experiment has, however, been confirmed by some that have been lately performed by Gordon, in which the effect of coagulation in evolving caloric

[2] Ann. de Chimie, t. vii. p. 147, from 20° to 25° R.

[3] Hunter on the Blood, p. 27.

was rendered most evident, by moving the thermometer, during the formation of the clot, first into the coagulated, and afterwards into the fluid part of the blood, when he found that by this means he could detect a difference of 6°, and that the difference remained perceptible for 20 minutes after the process had commenced. In repeating the experiment upon blood, drawn from a person labouring under inflammatory fever, the rise of the thermometer was no less than 12°.[4] I conceive, therefore, that this point may be considered as established, that during the coagulation of fibrin a quantity of caloric is extricated, thus proving that fibrin has a less capacity for heat in its coagulated than in its uncoagulated state.

§ 3. *Fibrin.*

The crassamentum, when removed from the serum, generally appears under the form of a soft solid, of such consistence as to bear cutting with a knife: it frequently assumes a fibrous appearance, and when it has been coagulated under particular circumstances, it may be converted into an irregular net-work, consisting altogether of fibres, of a greater or less degree of fineness, according to the manner in which the process has been conducted. The best method of exhibiting this fibrous appearance is to stir the blood, as it flows from the vessel, with a bunch of twigs, or to receive it into a bottle, and shake it during its coagulation, but it must be observed, that if the motion be too considerable, the clot is altogether pre-

[4] Annals of Philosophy, v. iv. p. 139.

vented from forming. The coagulum which has been produced in the usual manner, while the blood is at rest, may also be deprived of its red colour by repeated ablution in water, thus showing that the colouring matter is only mechanically mixed with the fibrin, and not retained there by any chemical affinity. When the fibrin is thus procured in a pure state, it is found to be a solid of considerable consistence, elastic and tenacious, and in its general aspect, as well as in its chemical relations, very similar to the pure muscular fibre, although we have reason to suppose that it differs from it in its minute organization. It has been designated by several names—coagulable lymph, gluten, fibre of the blood, and fibrin; this last appellation was given to it by the French chemists, and will be adopted in this work, as being the most characteristic and appropriate.

It is obviously upon the fibrin that the formation and separation of the clot depends, thus producing what has been termed the spontaneous coagulation of the blood, in opposition to the other kinds of coagulation, which are effected by the application of heat, or of some chemical re-agent. A change so singular in its nature could not but excite great attention among physiologists, and numerous observations and experiments have been made to account for its occurrence, or to discover what circumstances tended to promote or retard it. The two most obvious circumstances which might be supposed to operate, as constituting the chief difference between the condition of the blood while in the vessels, and after it is discharged from them, are rest and exposure to air. I have already alluded to the effect of agitating the blood; it is well known, that if the fluid, as it is discharged from the vessels, be briskly stirred about for some time, the process of coagulation is entirely prevented from taking place, either in consequence of a more complete union of its parts with each other, which prevents their future separation, or from the fibrin, after it has been for some time discharged from the blood, losing this peculiar property, by which its particles are attracted together. It is probable that both these causes may operate, but I am not acquainted with any facts which can enable us to determine which of them has the most powerful effect.

With respect to the influence of air upon the spontaneous coagulation of the blood, we have not yet arrived at any very positive conclusion. Many experiments were performed on this subject by Hewson, and although they are not sufficiently decisive, nor always uniform in their results, yet they lead to the opinion, that the presence of air promotes coagulation.[5] Hunter opposed the doctrine of Hewson,[6] but his experiments and arguments go no further than to show, that air is not essential to the process; a point which was fully admitted by Hewson, and which follows immediately from his experiments. This circumstance is proved by the fact, that coagulation not unfrequently takes place in the vessels or cavities of the body, where the blood must be com-

[5] Experimental Enquiries, p. 20. [6] On the Blood, p. 22.

pletely excluded from the air; and indeed this change has been found to exist, to a certain degree, during life, as the polypous concretions that are occasionally found in different cavities of the body, and which from the previous symptoms, as well as from their appearance and texture, must, at least in some cases, have existed before death, are chiefly composed of fibrin. Many of the wonderful stories that are recorded, and sometimes on very good authority, of worms being found in the chambers of the heart, the arch of the aorta, the sinuses of the brain, or the large veins, must be explained upon the supposition, that the spectators have been deceived by portions of coagulated fibrin, in the form of long strings, possessing somewhat of the shape and form of worms, to which a lively imagination and a fondness for the marvellous have added the other properties of these animals.

Besides the action of the atmosphere generally on the coagulation of the fibrin, experiments have been made upon the effect of its constituent parts taken separately, and also of other gases; but although the examination has been pursued with considerable diligence, the results are not so decisive as might perhaps have been expected. It must, however, be confessed, that in performing experiments of this kind, much manual dexterity is requisite, and that there are a variety of circumstances, connected with the state of the atmosphere itself, the manner in which the blood flows from the body, the kind of vessel in which it is received, the temperature and other conditions of the gas in question, and the manner in which it is brought into contact with the blood, all of which may produce a notable difference in the result. We find, therefore, that although some respectable authors would lead us to suppose that oxygen retards the progress of coagulation, and that it is promoted by carbonic acid and some other of the unrespirable gases, yet we are informed by Sir H. Davy, that he could not perceive any difference in the period of the coagulation of venous blood when it was exposed to azote, to nitrous gas, to oxygen, to nitrous oxide, to carbonic acid, to hydro-carbon, or to atmospheric air.[7]

I have mentioned above that the spontaneous coagulation of the fibrin is prevented by sufficient agitation, and the same effect is produced by the addition of certain neutral salts. Hewson, to whom we are principally indebted for these facts, found that the sulphate and muriate of soda and the nitrate of potash were among the most powerful salts in this respect, so much so, that if we add to a portion of blood rather less than one-twentieth of its weight of any one of them the coagulation does not take place. On what this depends we are entirely ignorant; it would not appear to be upon any tendency in the salt employed to dissolve the fibrin, because the neutral salts do not possess this property, at the same time that potash, which is the proper solvent of fibrin, has less power in retarding

7 Researches on Nitrous Oxide, p. 380.

its coagulation. Besides the neutral salts, the mere dilution of blood with a sufficient quantity of water will effectually prevent its spontaneous coagulation. We are informed by Crawford, that when blood is mixed with twelve times its bulk of water, no coagulation was observed to take place for several hours,[8] an effect which may perhaps be explained merely by the particles being removed to so great a distance from each other, as to be placed beyond the reach of their mutual attraction.

Among the changes which attend the coagulation of the fibrin, I may remark that its specific gravity is said to be increased by this process; but this is a point which it must be difficult to ascertain, in consequence of the serum and the red globules which are always mixed with the clot, besides that the firmness of the clot, and consequently its specific gravity, differ very much in different cases. Haller, as it would appear upon the authority of Jurin, states that the specific gravity of the crassamentum is 1·126,[9] the serum being only 1·030;[1] but the fact which was long since observed by Boyle, that the crassamentum floats in the serum, so as to preserve the surface of the two nearly on a level, would seem to show that they cannot differ much in their specific gravity.

Much has been written about what is termed the halitus of the blood, or the vapour which arises from it when it is first drawn from the body. Plenk, who has paid the most attention to it, calls it gas

[8] On Animal Heat, p. 278. [9] El. Phys. v. 2, 5. [1] v. 3, 1.

animale sanguinis; he conceives it to be composed of hydrogen and carbon, and that it produces many important effects in the animal œconomy.[2] But I believe that this opinion is altogether unfounded, as the halitus is nothing more than the aqueous vapour, which necessarily arises from a fluid considerably warmer than the air in contact with it, and which, during its evaporation, carries up a very minute quantity of animal, and perhaps even of saline matter.[3]

The cause of the coagulation of the fibrin has never been satisfactorily explained: it is a phenomenon which does not exactly resemble any other with which we are acquainted, and the operation of external agents upon it is not so well marked as to enable us to refer it to any general operation of the physical properties of matter. What renders the subject more difficult is, that there are some circumstances which affect the coagulation of the blood in a manner that we are quite unable to explain. Many causes of sudden death have this effect; lightning and electricity; a blow upon the stomach, or injury to the brain; the bites of venomous animals, such as the viper and the rattle-snake; some acrid vegetable poisons, as laurel-water; also excessive exercise, and even violent mental emotions, when they produce the sudden extinction of life, prevent the usual coagulation of the blood from taking place.[4]

A singular coincidence has been observed in these

[2] Hydrologia, p. 42. [3] Fourcroy, Syst. v. ix. p. 185.

[4] Hunter on the Blood, p. 26.

cases between the want of coagulability in the fibrin of the blood, and the diminution of contractility in the muscles after death. They are all found in a state of relaxation, incapable of being excited by the accustomed stimuli; and it has been further observed that the body is disposed to run rapidly into the state of decomposition. These facts appear to identify, at least to a certain degree, the property of muscular contraction with that of the coagulation of the fibrin, and this identity is further supported by considering that the chemical composition of fibrin is similar to that of muscle. For the knowledge of this relation between the coagulation of the blood and the contraction of the muscles we are principally indebted to Hunter, who noticed it with much attention, and built upon it some of his favourite physiological speculations. It is indeed probable that we may trace to this source his celebrated hypothesis of the life of the blood, a doctrine which is founded upon the principle that a fluid is capable of organization, and that it may possess functions either identical with, or very similar to those which are the most characteristic of the living animal solid. According to this hypothesis the blood is supposed not merely to be the substance which gives life to the animal, by carrying to all parts what is necessary for their support and preservation, but that it is properly itself an organized living body, and even the peculiar seat in which the vitality of the whole system resides.[5] The question of the life of the blood cannot be fully examined, until we are

[5] Hunter on the Blood, p. 76.

further advanced in our view of the animal œconomy, and especially, until I have endeavoured to define the manner in which the term life ought to be employed. But I may remark, that even were the Hunterian doctrine of the life of the blood to be fully established, it would not offer any explanation of the cause of its coagulation ; for the same difficulty still remains, in what manner the presence of life operates so as to produce either the coagulation of the blood or the contraction of the muscles.

Perhaps the most obvious and consistent view of the subject is that the fibrin has a natural disposition to assume the solid form, when no circumstance prevents it from exercising this inherent tendency, As it is gradually added to the blood, particle by particle, while this fluid is in a state of agitation in the vessels, it has no opportunity of concreting ; but when it is suffered to lie at rest, either within or without the vessels, it is then able to exercise its natural tendency. In this respect the coagulation of the fibrin of the blood is very analogous to the formation of organized solids in general, which only exercise their property of concreting or coalescing under certain circumstances, and when those causes, either chemical or mechanical, which would tend to prevent the operation, are not in action. Upon this principle we shall be induced to regard the coagulation of the blood as analogous rather to the operation by which the muscular fibre is originally formed, than to that by which its contractile power is afterwards occasionally called into action ; for, notwithstanding the relations pointed out by Hunter, we shall find that the operations are essentially different in two very important particulars—in the causes which produce them, and in the subsequent state of the parts. The causes of muscular contraction, as we have already had occasion to observe, are exclusively stimulants of various kinds, but it does not appear that any one of these, numerous and various as they are, has the smallest effect in promoting the coagulation of the blood. With respect to the subsequent state of the parts, in the muscle contraction is always succeeded by relaxation, whereas nothing at all resembling this ever occurs in the blood; the fibrin, when once formed, remains unchanged as long as it retains its chemical composition. Upon the whole, therefore, although we must acknowledge the validity of the facts pointed out by Hunter, we are at present scarcely prepared to form them into a consistent theory; and we must content ourselves with the simple statement, that the fibrin of the blood and the muscular fibre possess, the former the property of coagulation, and the latter that of contraction, which are acted upon in the same manner by various circumstances, although we are not able, in these cases, to perceive the relation of cause and effect.[6]

[6] Upon the same principle which induced me to notice John Hunter's hypothesis of the action of the heart, I shall quote his opinion respecting the coagulation of the blood. "My opinion is, that it coagulates from an impression: that is, its fluidity under such circumstances being improper, or no longer necessary, it coagulates to answer now the necessary purpose of solidity." On the Blood, p. 25.

Before I dismiss the subject of the coagulation of the fibrin, I must advert to a circumstance which is of great importance in the practice of medicine, that the nature and appearance of the coagulum vary very much according to the state of the body at the time when the blood is drawn. The most important of these variations consists in what is called the size, or buffy coat of the blood, a term employed to denote that state of the crassamentum, when the upper part of it contains no red particles, but exhibits a layer of a buff-coloured substance, lying on the top of the red clot. This buffy coat is generally formed when the system is labouring under inflammatory fever, and when, according to the modern doctrines of pathology, there is supposed to be an increased action of the arteries. The immediate cause of this appearance in the crassamentum is obvious; the globules, or other matter which give it the red colour, begin to subside before the coagulation is completed, so that the upper part of the clot is left without them. The remote cause of the buffy coat is not yet ascertained, although many experiments have been made to discover it. Hewson thought that the fibrin became specifically lighter, and, of course, the red particles comparatively heavier, whence they would be disposed to sink to the lower part of the clot; he also thought that the blood coagulated more slowly.[7] Hunter was inclined to account for the appearance by the firmer coagulation of the fibrin, as it were, squeezing out the red particles: but this would scarcely explain why the

7 Experimental Enquiries, p. 39, 59, et alibi.

upper part of the clot alone is left without them. Hey's opinion is perhaps better founded, that by the increased action of the vessels the different constituents of the blood are more intimately mixed together,[9] while Dr. Davy opposes the opinion of Hewson as to the fact of the slower coagulation of inflamed blood.[1] From some experiments that were performed on the composition of the buffy coat by Mr. Dowler, it appears that it contains a very large proportion of serum,[2] and this, by diminishing its viscidity, will more readily allow of the subsidence of the red particles. It is, however, not improbable that Hunter's opinion is in part correct, for we find that the clot of inflamed blood obviously possesses a firmer texture than in its ordinary state, so that sometimes, in consequence of the contraction of the clot, after it has begun to form, the surface has a depression in the centre, forming what is called the cupped state of the coagulum. And here we have another analogy between the blood and the muscles; for there are several circumstances which lead us to conclude, that the force of muscular contraction through the system generally is increased in inflammatory fever.[3]

9 Observations on the Blood, p. 10, 19, et alibi.

1 Phil. Trans. for 1822, p. 271.

2 Med. Chir. Trans. v. xii. p. 91. Hewson conceived that the buffy coat is composed of fibrin, but that in inflammation the fibrin acquires a thinner consistence, p. 34, 45, et alibi.

3 It may not be uninteresting to peruse the account which Sydenham gives of the buffy coat of the crassamentum of the blood in

It is probably upon the fibrin that the property which the blood possesses of repairing the injuries of the solids principally depends, a property which affords us one of the most interesting examples of the resources of the animal œconomy, while, at the same time, it very forcibly illustrates the slow progress of medical information, when the mind has once received an impulse in a wrong direction. Every one who is acquainted with the history of surgery is acquainted with the sympathetic powder, which, about the middle of the seventeenth century, engaged the notice and received the sanction of the most learned men of the age. This celebrated remedy derived its virtues not from its composition, but from the mode of its application, for it was not to be applied to the wound, but to the weapon by which the wound was inflicted; the wound was ordered to be merely closed up, and was taken no further care of.[4] Most men of sense, indeed, ridiculed the proposal, but after being fully tried, it was found that the sympathetic mode of treating wounds was more successful than those plans which proceeded upon what were considered scientific principles; and it continued to gain ground in the public estimation, until at length some innovator ventured to try the experiment of closing up the wound with-

pleurisy; his observations will be commonly found to be correct, although his hypotheses are too often fallacious. Observ. circa Morb. Acut. Hist. § 6, c. 3.

[4] See "A late Discourse, &c." by Sir K. Digby, a treatise which admirably exemplifies the mode of philosophizing that was fashionable in the earlier part of the seventeenth century.

out applying the sympathetic powder to the sword. Wiseman, who wrote about fifty or sixty years after the introduction of this mysterious operation by Sir Kenelm Digby, in describing the importance of keeping the divided parts in close union, says, "for here nature will truly act her part, by the application of blood and nourishment to both sides indifferently, and finish the *coalitus* without your further assistance. And this is that which gives such credit to the sympathetic powder."[5]

Although we are now well acquainted with the general facts respecting the re-union of divided parts, yet there is much obscurity concerning the exact mode of the operation. We find that when two newly cut surfaces, which were not previously connected, are laid in close apposition, and the air carefully excluded, they will unite, and when the operation is performed under favourable circumstances, the trace of the wound is scarcely perceptible, either in the structure or functions of the part. What will appear more wonderful is, that parts of different structures are capable of contracting this close union, the arteries, veins, and even the nerves, becoming connected, each to each; and to add still further to the marvellous aspect of these operations, we cannot avoid giving our assent to the fact, that portions of the body, which had been entirely cut off, or even of a different body, if speedily applied to a recently divided surface, will unite and retain their functions.[6]

[5] Chir. Treat. b. 5, c. 1, p. 342.

[6] J. Hunter caused the spurs of a young cock to adhere to the

Although, in a great majority of instances, we shall find ourselves in the right, if we disbelieve the wonderful tales that we find in the works of the older writers, yet our scepticism may be carried too far; and accordingly it is now admitted, that the operations of Taliacotius, or Tagliacozzi, a name which could not until lately be admitted into a serious discussion, were founded upon correct principles. It appears, indeed, that a process of an analogous nature had been long practised in India, and has been introduced into this country, with complete success, by Mr. Carpue.

As I have already stated, we have reason to believe that the fibrin is the intermedium by which the process is effected, yet I confess that I know of no rational method in which we can explain the successive steps of the operation. We may, indeed, conceive of the divided end of an artery, which belongs to the cut surface nearest the heart, discharging a portion of its fibrin, which may coagulate, and form a basis, or nidus, as it has been termed, through which the current of blood may afterwards form a new channel; but in what way this stream is to discover the ends of the arteries of the other surface, by what power it is to enter them, how these insulated

comb of another cock, and the testicles were found to become united to the internal cavities of other animals. He remarks, "The most extraordinary of all the circumstances respecting union, is by removing a part of one body and afterwards uniting it to some other part of another, where, on one side, there can be no assistance given to the union, as the divided or separated part is hardly able to do more than preserve its own living principle, and accept of the union." Treatise on the Blood, p. 208.

parts are to propel their blood into the veins, and lastly, how the veins of the divided part are to transmit their contents into the veins of the body, are questions that at present, I apprehend, we are not able to answer.[7]

A curious series of facts relative to the coagulation of the blood, and to the mode in which portions of coagulum acquire an organized structure, has been lately brought forwards by Sir Everard Home, principally derived from the microscopical observations of Mr. Bauer. It is stated that a quantity of carbonic acid is always present in the blood, that, during its coagulation, this acid is extricated, and that by its extrication it forms linear passages or tubes in the substance of the blood, into which the vessels of contiguous parts are elongated, and which become the rudiments of the future arteries. It would appear that the serum is the nidus in which these tubes are formed, as they are said to be altogether independent of the globules, which are supposed to be the more immediate constituents of the fibrin. It is not expressly stated whether the tubes are themselves converted into blood-vessels, or whether they only afford spaces in which vessels are formed, nor does it seem

[7] It may be amusing to observe the uses which are ascribed to the crassamentum of the blood by Plenk, a writer of extensive information and general accuracy. The uses that he points out are three; 1. (to employ his own words) "sanguini ruborem conciliat;" 2. by the gravity of its metallic ingredients, it irritates the heart and arteries more effectually than the lighter particles of serum; 3. at the same time it imparts motion to the lighter particles of the serum.

quite clear what connexion the spontaneous coagulation of the fibrin has with the production of the tubes. Without calling in question the accuracy of Mr. Bauer's observations, it may be remarked, concerning the hypothesis that is deduced from them, that the formation of regular tubes could not be the result of the extrication of gas in a viscid fluid, the formation of the tubes must therefore be the result of a tendency in the fluid in question to assume an organic arrangement. The observations of Mr. Bauer are valuable, as showing the mechanical structure which the blood exhibits, during the process by which it becomes organized, but, I apprehend, they throw no further light upon the operation.[8] We cannot be surprised that so subtile a property as that by which the blood is enabled to acquire an organic arrangement should be destroyed by its being subjected to the action of the air-pump.[9]

With respect to the chemical properties of fibrin

[8] Phil. Trans. for 1818, p. 181, et seq.; for 1820, p. 2.

[9] Since the above was written Dr. Davy has published the result of some experiments on the subject, from which, as well as from other considerations, he concludes that Sir Everard Home's hypothesis is so far unfounded as respects the existence of carbonic acid in the blood. Dr. Davy remarks, that the gas observed by Mr. Bauer is probably azote, implying his belief that gas of some kind or other is contained in the blood. His experiments would, indeed, lead to the opinion, that there is no gas in recent blood. Phil. Trans. for 1823, p. 506. But, on the contrary, the existence of carbonic acid in blood, or at least the fact that carbonic acid may be disengaged from blood, by merely removing the atmospheric pressure, seems to be established by Vogel, Ann. Phil. v. vii. p. 57; and by Mr. Brande, Phil. Trans. for 1818, p. 181.

it is sufficient to remark, that they appear exactly to resemble those of the muscular fibre; it is acted upon in the same manner by nitric acid and the other reagents, so as to be fully entitled to the appellation of liquid flesh, which was bestowed upon it by the older physiologists. For the first correct account of the chemical relations of fibrin we are indebted to Fourcroy, Vauquelin, and Berthollet; it has been lately examined by Berzelius, whose experiments may be regarded as the most correct that we possess upon the subject.[1]

§ 4. *Red Particles.*

After having described the fibrin, I proceed to the other constituent of the crassamentum, the red particles or globules.[2] These, from the singularity of their appearance and organization, have attracted an unusual share of attention, and have been the subject of almost innumerable observations and experiments. Soon after the microscope was introduced into anatomical researches, these peculiar bodies were observed by Malpighi, and they were afterwards more minutely examined by Leeuwenhoek. They were at first described simply as globules floating in the serum, and giving the blood its red colour, but as observations were multiplied, errors and absurdities were advanced in an almost equal proportion.

[1] Med. Chir. Trans. v. iii. p. 200.

[2] I have retained the ordinary description of the clot, as composed of fibrin and the red globules, although some late experiments, of which an account is given below, throw a degree of doubt upon its accuracy.

Leeuwenhoek himself invented a fanciful hypothesis, which had a long and powerful influence over the most enlightened physiologists, that the red particles of the blood were composed of a series of globular bodies descending in regular gradations; each of the red particles was supposed to be made up of six particles of serum, a particle of serum of six particles of lymph, &c.[3] This hypothesis, for which there really does not appear to be the slightest foundation, was so suited to the mechanical genius of the age, that it was generally adopted without any examination of its truth, received as the basis of many learned speculations in that and the succeeding age, and even formed a leading feature in the pathological speculations of Boerhaave. The futility of the doctrine was exposed by Lancisi and Senac, but it still maintained its ground until the time of Haller. When objects can only be detected by highly magnifying lenses, it is so extremely difficult to avoid being misled by ocular deceptions, that all descriptions of this kind are to be received with the greatest caution. The necessity of this caution is sufficiently proved by the discordant accounts, respecting the red globules of the blood, that have been given by different observers,

[3] For an account of Leeuwenhoek's supposed discoveries respecting the constitution of the globules, see Martine, in Ed. Med. Ess. v. ii. p. 74, &c.; the whole of this Essay is well worth perusing, as a curious specimen of the mode of prosecuting physiological inquiries, that was pursued by the learned mathematicians about a century ago. The earliest of Leeuwenhoek's papers appears to be in the Phil. Trans. for 1674, No. 23.

who could have no apparent motive for intentionally imposing upon their readers.

Next to the observations of Leeuwenhoek, those of Hewson were the most elaborate, and had at least the appearance of great accuracy. He describes the red particles as consisting of a solid centre, surrounded by a vesicle filled with a fluid. He informs us, that by adding water to them they swell out, the surrounding vesicle becomes thinner, and at length bursts, and leaves no trace behind; the whole substance of the particles is soluble in water, and imparts to it their red colour. It appears also that those animals which have white or rather colourless blood, possess particles which are supposed to be similar in their general form and organization to those of the red-blooded animals; we are likewise informed that they are of different sizes in different animals, and that their size bears no ratio to that of the animal from which they are taken, as, for example, they are said to be of the same size in the ox and the mouse, larger in birds, and to be the largest in the skate; in birds, amphibia, and in insects, they are elliptical, but, excepting in their form, they resemble those of the human subject. We have an account of some singular changes that are produced upon their colour and form by various chemical re-agents, especially by some saline bodies. Alkalies and acids, when moderately strong, first corrugate the vesicle, and afterwards seem to dissolve or decompose the whole of the globules; nitre has the property of considerably heightening the colour, so as to convert it to a bright red, while, on the contrary, there are some other substances by which their colour is destroyed without changing their form.[4]

Hunter, who made many observations on these bodies, differs essentially in his account of them from Hewson. He is silent respecting the central nucleus and the investing vesicle; there are some animals in which he could not discover any globules, as the silk-worm and the lobster, and he never observed them, in any case, to assume the elliptical form, described by Hewson. He does not regard them as properly solid bodies, but as liquids possessing a central attraction, which determines their figure; a circumstance that might identify them with substances of an oily nature, were it not stated, on the other hand, that the globules are miscible with water, and that they always preserve the same size, and are never disposed to run together or coalesce.[5]

The globules of the blood have been also examined by the Abbé Torré, by Monro, and more lately by Dr. Young. Torré supposed them to be flattened annular bodies, or like rings composed of a number

4 Phil. Trans. for 1773, p. 303, et seq.; Tab. 12. Also Exper. Enq. v. iii. ch. 1.

5 On the Blood, p. 40, et seq. Mr. Bauer, however, found that when the investing vesicle is removed, the nuclei are disposed to unite together, and upon this property the formation of the muscular fibre depends. Phil. Trans. for 1818, p. 176. MM. Prevost and Dumas, in like manner, suppose that the crassamentum of the blood, in the act of its spontaneous coagulation, is formed by the central globules adhering together in the form of fibres. Ann. Chim. et Phys. t. xxiii. p. 51.

of separate parts cemented together;[6] to Monro they exhibited the appearance of circular flattened bodies like coins with a dark spot in the centre, which he conceived was not owing to a perforation, as Torré had imagined, but only to a depression.[7] Some remarks were published on the subject by Cavallo, who conceived that all these appearances were deceptive, depending upon the peculiar modification of the rays of light, as affected by the form of the particle, and he concludes that they are in fact simple spheres.[8] But the account which Dr. Young has recently given of these bodies, to a certain extent, coincides with the description of Hewson. He remarks that if the globules be viewed by a strong light, they will appear like simple transparent spheres, but that if we examine them by a confined and diversified light, we shall be better able to ascertain their real figure and structure. The particles of the blood of the skate, from their size and distinctness, are the most proper for this kind of examination, and he found their form to be like that of an almond, but less pointed and a little flattened; they consist of an external envelope containing a central nucleus. This

[6] Phil. Trans. for 1765, p. 252, et seq.

[7] We must not omit noticing the result of Prof. Amici's examination of the globules, as given us in the Edin. Med. and Surg. Journ. vol. xv. p. 120. It may afford curious matter for speculation to those who place much confidence in microscopical observations; it is, however, proper to observe that the statement does come directly from the author himself.

[8] Medical Properties of Factitious Airs, p. 237, et seq.

nucleus is independent of the substance with which it is surrounded, for when this latter has been removed or destroyed, the nucleus still appears to retain its original form. The nucleus is much smaller than the part surrounding it, being only about one-third the length and one-half the breadth of the whole particle. We are informed that the entire particle of the human blood is not larger than the central part of the particle of the skate. Dr. Young found that the particle in the human subject is flattened, and has a depression in the centre, somewhat as was described by Monro, but much less in degree.[9]

There has been as much difference of opinion respecting the size, as the form of these particles. Without recurring to the older and less correct observations, it may be sufficient to give those of Dr. Young, Captain Kater, and Mr. Bauer; Dr. Young[1] and Captain Kater[2] both agree that the particles of the human blood are between $\frac{1}{4000}$ and $\frac{1}{6000}$ of an inch in diameter, or taking the medium, $\frac{1}{5000}$ of an inch. Mr. Bauer supposes them to be considerably larger; in their entire state he estimates them at $\frac{1}{1700}$ of an inch, and even when they have lost their external part, the nucleus is said to be $\frac{1}{2000}$ of an inch in diameter;[3] it is not stated whether the observations

9 Med. Liter. p. 545. Mr. Bauer has observed that the form of the globules of the skate is oval during the life of the animal, but becomes flattened after its death; this circumstance may perhaps, in some measure, tend to reconcile the discordant statements that we meet with on this subject. Phil. Trans. for 1818, p. 174.

1 Med Liter. p. 555. 2 Phil. Trans. for 1818, p. 187.

3 Phil. Trans. for 1818, p. 173.

of Dr. Young and Captain Kater were made upon the entire globule, or upon the central part only. These estimates unfortunately differ so much from each other, as to throw a considerable doubt upon their correctness; but, upon the whole, I feel disposed to assent to the statements of Dr. Young and Captain Kater, from their coincidence with each other, although made with a different kind of apparatus.[4]

The composition and chemical properties of these bodies still remain the subject of controversy, for although they have engaged the attention of some of the most acute of the modern chemists, the results which they have obtained are so discordant, that we cannot deduce any consistent or decided conclusion from them. This, in some measure, depends upon the difficulty which there is in procuring them in a separate state; there appears to be no method of detaching them from the serum in which they are enveloped, without at the same time dissolving at least their external covering, and perhaps affecting the consistence of the whole. The experiments of Berzelius are the most elaborate, and probably those on which we ought to place the most confidence; but I think it may be objected to them, that according to the method which he adopted, the external part of the globule would be removed, while the nucleus would be necessarily mixed with some of the other ingredients of the blood. His result is, that these particles do not materially differ from the other parts

[4] Cavallo states that the particles varied in size, the larger being ·0003 and the smaller ·0004 of an inch in diameter, p. 249.

of the blood, except in their colour, and in the circumstance of a quantity of the red oxide of iron being found among their ashes after combustion.[5]

The discovery of iron in the blood appears to have been originally made by Menghini,[6] and has been

[5] Med. Chir. v. iii. p. 212, et seq.

[6] Menghini's Memoir (Bonon. Comment. t. ii. pars 2, p. 244, et seq.) gives an account of a prodigious number of experiments which he performed on the blood of various animals, as well as other animal substances. Most of the experiments are related only in a summary manner, but some of them are detailed with more minuteness. In the first series he used five ounces (2400 grains) of the blood of a dog, and by calcination he obtained 24 grains of residuum, to which he applied a magnet, and found very nearly the whole to be attracted by it. The calx consisted of two kinds of particles; the one, "splendidiores," which were the most easily attracted; the other, "colore ad crocum martis vergentes," also magnetic, but less so than the former. p. 245, 246. As the Bolonese Commentaries may not be accessible to every one, I shall transcribe an experiment that was made upon human blood. "Humani enim sanguinis globulos unius libræ vehementissimo igne dimidiam horam vexavimus: vexatos ebullire primum aliquantisper, tum ex improviso flammulam emittere conspeximus. Erat hæc cærulea ad instar earum rerum sulphurearum, quæ solitæ sunt adhiberi ad transmutandum ferrum in chalybem. Metus fuit ne emissa flammula, cum materiam omnem absumeret, curiositatem quoque nostram eluderet. Quapropter subsidentem in vase materiem illico supra porphyritem Lellius effudit. Hæc granorum 28 ponderis inventa est. Hanc postea cum supra eburneum planum extendissem, in grandiuscula quædam corpora, inter quæ unum eminuit figura subrotundum, magnitudine ceteris præcellens, granum parvuli ciceris adæquans, offendi. Periclitari hinc, volui an ad cultrum magneticum omnia accederent; accesserunt autem super ea velocitate, qua solet ferrum purissimum. Porro corpusculum illud, quo ceteris magnitudine præstabat, discissum et fractum, intus cavum, nitentibus lineis distinctum, figura, duritie, et colore

fully confirmed by late observers, although they have differed very much both concerning the amount of the iron and the state in which it exists. Menghini himself, and some of the earlier chemists, seem to have very much over-rated it; Fourcroy's account does not enable us to ascertain the quantity of it in the blood with any degree of accuracy,[7] but we are informed by Berzelius, that the colouring matter of the blood, separated from the other part, leaves one-eightieth of an incumbustible residuum, of which rather more than one-half is an oxide of iron.[8]

We have hitherto been unable to ascertain in what state this iron exists; it would appear not to be in the form of any of the known salts of this metal, because, before the blood has been calcined, we cannot detect the iron by the tests which usually indicate its presence, yet its solubility in the serum, on the other hand, would seem to favour the opinion of its possessing something analogous to the saline state. Berzelius has performed many experiments on this point, but his conclusion is merely a negative one, that he has

ubicunque simillimum fuso ferro, mediocris lentis auxilio cognovimus." p. 260, 261. On this subject it will only be necessary to remark that Menghini's paper affords one instance out of many others, written at that period, where we may be assured that the facts as stated cannot be true, yet where it is not easy to assign the sourse of fallacy, or to determine in what degree the experimentalist was himself deceived or wished to deceive others. Plenk, Hydrol. p. 38, says that Rhades discovered the iron in the blood. In 25 pounds of blood, the average quantity in a man, he found two drachms of oxide of iron. Vauquelin says that Lemery discovered it. Ann. Chim. et Phys. t. i, p. 9.

[7] System, t. ix. p. 207. [8] Med. Chir. Trans. v. iii. p. 215.

been unable to combine the serum with any salt of iron, so as to produce a compound similar to the colouring matter of the blood.[9]

The existence of iron in the globules of the blood is, however, clearly proved; whatever difficulty there may be in determining the state in which it exists, and from the property which iron possesses of colouring the substances with which it is united, it has generally been supposed that the iron gives the blood its red colour. We are not, I apprehend, in possession of any facts by which this opinion can be either decisively proved or disproved, but I think it may be admitted as a probable presumption. It has indeed been opposed by some writers of high authority; of these one of the first, both in point of time and of respectability, is Wells.[1] But his experiments seem to me only to prove that the colour of the blood is not occasioned by any salt of iron, or of iron in such a state as to be affected by the ordinary tests, which is admitted to be the case. Mr. Brande has also attempted to prove that the colour of the blood does not depend upon iron,[2] because he found the indications of the presence of iron to be as considerable in the parts of the blood that are without colour as in the globules themselves; and indeed his results

9 Med. Chir. Trans. v. iii. p. 221. Fourcroy and Vauquelin announced, as the result of direct experiment, that the iron of the blood was in the state of sub-phosphate, but Vauquelin has since retracted this opinion. Fourcroy's System, v. ix. p. 207, 208.

1 Phil. Trans. for 1797, p. 416, et seq.

2 Phil. Trans. for 1812, p. 90, et seq.

would rather tend to prove that the quantity of iron in the blood is too minute to produce any effect in it, than to explain its action on the different component parts of this fluid. But with respect to the quantity of iron, I may remark that they are at variance with the later and apparently more elaborate experiments of Berzelius.

A series of experiments have been lately performed on the colouring matter of the blood by M. Vauquelin, which he regards as confirming the conclusion of Mr. Brande. They consisted in digesting the crassamentum in diluted sulphuric acid, which dissolves a portion of the albumen and fibrin, together with the colouring matter, which last is thrown down separately from the solution by ammonia. The colouring matter subsides from the fluid, and, by sufficient washing, may be obtained, as it is supposed, in a pure state; if it be then diffused through water it produces a fluid of a purplish colour, in which the presence of iron is not indicated by the usual tests, while they readily detect it in the fluid from which the colouring matter has subsided.[3] But I apprehend that this cannot be considered as deciding the point; for we may observe that after the crassamentum has been subjected to the action of these re-agents, the constitution and chemical properties of its component parts will be considerably altered, so as not to afford us any certain indication of their previous state. Besides, although the precipitate that is formed upon the addition of the ammonia be a substance of a purple

3 Ann. Chim. et Phys. t. i. p. 9.

colour, we do not seem to have any evidence that it is composed of the red particles as they naturally exist in the blood; and we may further remark that the experiments of M. Vauquelin differ essentially from those of Mr. Brande, inasmuch as Mr. Brande's results tend to prove the almost total absence of iron in the blood.

One of the most remarkable properties of the red globules is the change which is effected in their colour by the action of the different gases. Lower noticed the greater brightness of the upper and external part of the clot, when exposed to the air, and attributed it to its proper cause,[4] but the mathematical doctrines, which were so prevalent about this period, led to a different explanation of the fact. Cigna of Turin, about 60 years ago, revived the doctrine of Lower, and confirmed it by some ingenious experiments, but it is remarkable that he afterwards almost abandoned his own hypothesis.[5] The subject was then taken up by Priestley, and he not only fully established the point, that the bright red colour of the external part of the crassamentum depends upon the action of the atmosphere, but he proved that it is owing to the oxygenous part of the air alone, and that carbonic acid and azote have precisely the contrary effect, reducing bright scarlet blood to the purple colour.[6] We have every reason to suppose that it is the red particles of the crassamentum on which the air more

[4] De Corde, c. iii. p. 178.

[5] Priestley on Air, v. iii. p. 357, et seq.

[6] Ibid. p. 363, et seq.

particularly acts,[7] and as these would appear to differ in their chemical constitution from the other constituents of the blood, principally in the iron which they contain, it seems a natural conclusion that the iron, however minute in quantity, is the agent by means of which the blood acts upon the atmosphere. Of this, however, it is admitted that we have no decisive proof, and that until we know in what state the iron exists in the blood, we can form no more than a conjecture on this point.

Both from the microscopical observations and the experiments of various kinds that have been made upon the red particles, it would seem probable that their colouring matter is principally or entirely seated in their external covering, and that the central nucleus itself is without colour. From this circumstance it had been generally supposed that the particles were soluble in water, but Dr. Young informs us that it is merely the envelope, which contains the colouring matter, that is so, and he points out a method by which the central nucleus may be procured, retaining its perfect form in water, after the red part has been dissolved.[8]

Besides the ordinary globules of the blood, Mr. Bauer has given us an account of another species of globules, which would appear to be essentially different

7 We learn from Berzelius, that "blood, in which the colouring matter is still contained, absorbs oxygen gas very quickly, when out of the body and shaken in atmospheric air ;. . . . on the other hand, serum, when destitute of colouring matter, does not change the atmospheric air before it begins to putrify." View of Animal Chemistry, p. 36.

8 Med. Lit. p. 347.

from the former, both in their nature and properties, and in the relation which they bear to the other constituents of the blood. He first detected them in the serum, where he observed them to be actually generated while the fluid was under examination. Minute spots made their appearance, which gradually increased in bulk, until some of them attained the size of the globules of the blood when deprived of their colouring matter. We are informed that these globules are generated in serum after it has been removed from the vessels for some days; it would appear, therefore, to be independent of any property in the fluid that is connected with vitality, or its tendency to organization. Similar appearances were detected in pus, which is stated to be, in the first instance, an homogeneous fluid, and that globules gradually make their appearance in it. It does not appear that the serum is actually composed of these globules, but that they are formed from its constituents. Sir Everard Home gave them the name of lymph globules.[9]

Mr. Bauer afterwards observed these lymph globules in the coagula of an aneurysm, where they exist along with the ordinary blood globules, the lymph globules being in greater proportion in the older coagula, until it appeared that the oldest were almost entirely composed of them. The globules in these coagula were $\frac{1}{2800}$ of an inch in diameter, and were supposed to be the same that had been previously observed in the serum. The buffy coat of inflamed blood is said to consist almost entirely of these lymph

9 Phil. Trans. for 1819, p. 2, et seq.

globules, a circumstance which appears somewhat inconsistent with the fact previously noticed, of their being found in greater abundance in old coagula, and with a remark which is subsequently made, that in tumours, the firmer and older parts are principally composed of the lymph globules, and the more recent principally of the ordinary blood globules. From some of Mr. Bauer's observations it might be inferred that these lymph globules are merely the ordinary blood globules deprived of their envelope of colouring matter, but there are other facts stated which seem incompatible with this idea.[1]

It is not impossible that these lymph globules may be the origin of Leeuwenhoek's descending series, and it affords us a striking illustration of the mode in which microscopical observations may be warped and accommodated to a preconceived theory, even by a person of skill, science, and integrity.

§ 5. *Serum.*

After this account of the crassamentum of the blood, we now proceed to the serum, or the fluid part which is left after the separation of the clot, in consequence of the spontaneous coagulation of the fibrin. Serum is a transparent, homogeneous liquid, of a light straw colour, a saline taste, and an adhesive consistence. Its specific gravity varies in different subjects, but it is always greater than that of water; the average is probably about 1.025.[2] It converts blue vegetable colours to green, thus proving that it contains a quan-

[1] Phil. Trans. for 1820, p. 2, et seq.

[2] Marcet, in Med. Chir. Trans. v. iii. p. 363.

tity of uncombined alkali, and besides this it is found to hold in solution various earthy and neutral salts. Its most remarkable and characteristic property is its coagulation by heat: we find that when the serum of the blood is exposed to a temperature of 160°,[3] it becomes white and opake, and acquires a firm consistence. In this state it exactly resembles the white of the egg, when hardened by boiling, and is found to be essentially the same with this substance, whence it has obtained the name of albumen. Although the whole of the serum appears to be converted into a solid mass by the process of coagulation, yet, if the albumen be cut into small pieces, and placed in the mouth of a funnel, a fluid drains from it, which is called the serosity of the blood. The separation of the serosity may be further promoted by washing the coagulated albumen in hot water, and after this process has been continued for a sufficient length of time, the albumen is left pure, united only with a quantity of water; this, by the application of a moderate heat, may be expelled, when the substance assumes the appearance of a semitransparent solid, both in its physical and chemical properties similar to the harder varieties of membrane, except that it does not exhibit any appearance of an organized structure.

[3] Dr. Thomson quotes Cullen as stating that the coagulation of albumen takes place at 165°, but without any particular reference; Cullen, in his "Institutions," p. 206, fixes the degree at 156. Fourcroy, in his "System," v. ix. p. 190, fixes it at 75 C. i. e. 167 F.; while in Ann. Chim. t. vii. p. 156, he names 55 R. i. e. 155·75 F.; 160 is the degree assigned by Henry, Children, Brande, and Ure. MM. Prevost and Dumas, in some recent experiments, state the degree to be "autour de 70 Cent." i.e. 158 F. Ann. Chim. et Phys. t. xxiii. p. 53.

The coagulation of albumen is an operation which must be regarded as essentially different from the spontaneous coagulation of the fibrin, although they are both designated by the same appellation, inasmuch as they are produced by totally different means; the one by mere rest, which has no effect upon the other, while, on the contrary, heat, which does not produce the concretion of the fibrin, immediately converts albumen into the solid form. The texture also of the coagulum is different in the two cases; the fibrin has a tendency to arrange its particles in a specific manner, so as to present the appearance of an organized body, but nothing of this kind takes place with respect to the albumen; it simply concretes into a mass of an uniform consistence, in which the albumen remains united to the water that previously held it in solution.

Besides heat, there are other agents which are said to coagulate albumen; alcohol, acids, metallic salts, and tan, and, according to the curious discovery of Mr. Brande, it may be also effected by the negative wire in the interrupted galvanic circle. But I apprehend, that in these cases, although the whole or a part of the albumen is converted into the solid form, the operation is very different from that of heat. Some of these agents, as alcohol, and perhaps the stronger mineral acids, produce their effect by abstracting a portion of the water which held the albumen in solution, while tan and the metallic salts unite with the albumen and form a compound which is insoluble in water, and consequently separates from the fluid, thus producing an effect, which should rather be styled precipitation than coagulation.

Albumen which has been coagulated by heat, to which I shall at present confine my remarks, differs in many respects from albumen before it has undergone this change. Besides its conversion from the fluid to the solid form, many of its physical properties are altered, and it is differently acted upon by chemical re-agents, especially by water, in which it is no longer soluble. The efficient cause of the coagulation of albumen is a question that has been frequently discussed, but hitherto, I conceive, without much success. We are informed that the specific gravity of albumen is not affected by the process, so that we may conclude it does not depend upon condensation; nothing is added to it, and nothing is abstracted from it; for although Scheele endeavoured to prove that there was a fixation of the matter of heat, and Fourcroy, in his vague way, attributes it to the absorption of oxygen, we do not find that there is any foundation for this opinion. We learn indeed from the experiments of Carradori, that the coagulation takes place as readily when the contact of air is prevented, as when the albumen is exposed to the atmosphere; and he likewise informs us that the substance experiences no change of bulk during the operation.[4] We can only, therefore, account for it upon the supposition of some change which the figure or nature of its particles have experienced, by which their relation to each other is altered, without their being brought nearer together, or, as far as we can perceive, being arranged in any specific manner analogous to organi-

[4] Ann. Chim. t. xxix.

zation. But we are unable to explain what is the nature of this new relation, or what are the means by which it is effected.

Dr. Thomson supposes that the process of coagulation is analogous to the change which takes place in a solution of silicated potash, when it is saturated with muriatic acid, where the acid gradually detaches the potash from the silex, which being thus left at liberty, unites with a portion of the water and forms a gelatinous mass.[5] But it may be objected to Dr. Thomson's hypothesis, that the case he adduces is one of an interchange of chemical elements, which in fact produces a precipitation, but which is brought about so slowly, that the precipitated body becomes united to a portion of water, instead of being thrown down separately, as it would be under ordinary circumstances. With respect to albumen, however, we have no evidence that any chemical change takes place among its constituent parts, or that any thing analogous to precipitation exists.

Mr. Brande employs a method of explaining the phenomenon which is somewhat different. He regards liquid albumen as a solution of solid albumen in alkali, and upon this principle he endeavours to show how the action of the galvanic apparatus, of alcohol, and of acids, coagulate albumen by abstracting the alkali.[6] But I think I have proved by direct experiment, that the quantity of alkali in albumen is much too minute to retain it in solution, and that the alkali may be neutralized and the albumen still retain its fluid

[5] System. v. v. p. 409. [6] Phil. Trans. for 1809, p. 373, et seq.

form;[7] besides I do not perceive how this explanation applies to the action of caloric. Upon the whole I am disposed to regard the coagulation of albumen by heat as an effect entirely sui generis, as one which at present we are not able to refer to any general principle.

The most important chemical properties of albumen, while in its liquid form, are its solubility in water, and the precipitates which it forms with the mineral acids, tan, and a variety of metallic salts. Of the acids, the muriatic is supposed to combine with it the most readily, and is therefore employed as one of the most delicate tests of its presence in a substance where we suspect it to exist. Tan forms with albumen a dense precipitate, of a tough consistence, and insoluble in water. A variety of the metallic salts precipitate albumen, and, like the acids, serve as very delicate tests of its presence; of these probably the corrosive sublimate, or the bichloride of mercury, is the most delicate, and at the same time the most discriminative, as it appears to have no action upon any other of the animal substances which enter into the composition of the albuminous fluids.

The chemical relations of coagulated albumen, as I have already remarked, differ materially from those of this substance before coagulation. It then becomes completely insoluble in water, unless by heating the water under strong compression it be raised to a temperature considerably above the boiling point, and then it would appear that the albumen is rather

[7] Med. Chir. Trans. v. ii. p. 173, 174.

decomposed than dissolved. Coagulated albumen is partially soluble in sulphuric acid, but one of its most interesting chemical relations is that which was described by Mr. Hatchett, where he found that by digesting it for some time in diluted nitric acid, it is converted into a substance which possesses the physical and chemical properties of jelly. If the nitric acid be applied in a concentrated state, and the action be assisted by heat, the albumen is dissolved, and the fluid assumes a yellow colour, which is changed into a deep orange by the addition of ammonia, a change which may be employed as a test of the presence of albumen in the solution.[8] The caustic mineral alkalies act readily upon solid albumen, and form with it a saponaceous fluid, which may be substituted for some of the coarser kinds of soap in various manufacturing processes.

§ 6. *Serosity.*

The portion of the serum which remains fluid after the albumen has been coagulated by heat, and which may be obtained either by washing the coagulum with water, or by simply suffering the fluid part to drain from it, is called the serosity. Compared with the other constituents of the blood, it exists only in small quantity, and is generally so connected with the albumen, that it was some time before it attracted any attention; and partly on this account, and partly

[8] Phil. Trans. for 1800, p. 385; for a good view of the chemical relations of albumen, both in its uncoagulated and in its coagulated state, see Thomson's System, v. iv. p. 406.

from its being a compound substance, consisting of several ingredients, it was not until very lately that any correct notions were entertained respecting it. Its existence as a substance distinct from the albumen appears to have been first announced by Butt, in a thesis published at Edinburgh in 1760;[9] its properties were still further developed by Cullen in his "Institutions,"[1] and it afterwards became the subject of more minute chemical examination in France, by Fourcroy and Vauquelin,[2] and by Parmentier and Deyeux.[3] The most important point which the French chemists stated, as the result of their experiments, is the discovery of a quantity of gelatine, or animal jelly, in the serosity, which was said to constitute the bulk of the animal matter contained in it, and to which it owed its specific properties. The account which was given of this substance, especially by Parmentier and Deyeux, was so much in detail, and altogether bore so much the appearance of accuracy, that every one acquiesced in their statement, and not only was jelly always considered by systematic chemists to be one of the constituents of the blood, but means were pointed out for ascertaining its proportion, and many important physiological speculations were derived from its supposed agency in the animal œconomy.[4]

9 De Spontanea Separatione Sanguinis, p. 53, et seq.

1 § 247—253.

2 Ann. Chim. t. vi. p. 181; and t. vii. p. 157.

3 Journ. de Phys. t. xliv. p. 439, 9.

4 Sprengel, a writer of extensive information, and generally of

During a course of experiments on the nature of the animal fluids, in which I was engaged in the years 1805 and 1806, I examined the serosity of the blood, but was unable to detect the smallest quantity of jelly in it, or in any other of the albuminous fluids, so that I was led to conclude that jelly never exists as a constituent of the blood.[5] This discovery has been since amply confirmed by Prof. Berzelius, Dr. Marcet, and Mr. Brande, so that notwithstanding the high authority and the weight of experiment by which the contrary opinion appeared to be supported, we may certainly decide that the blood contains no gelatine. At the same time, however, that I made the above experiments, I found that the serosity contains a quantity of animal matter, and that this matter is not albumen, but it was difficult to ascertain any other than negative characters for it, as it is always united with a quantity of soda, and with a variety of other salts, from which I know of no method of separating it, without its being, at the same time, decomposed. To this substance Dr. Marcet has applied the name of muco-extractive matter;[6] I have preferred styling it the uncoagulable matter of the blood, as a term expressive of its most characteristic and distinctive property. So far as I have been able to ascertain its nature, it is not coagu-

great accuracy, describes the jelly of the blood in the following terms; "namque gelatina, quam continet (serum) in frigore duntaxat coit, nequaquam vero in æstu." Inst. Med. t. i. p. 381.

[5] Med. Chir. Trans. v. i. p. 71, et seq.

[6] Ibid. v. ii. p. 364.

lable by heat, or by any other means; it is not affected by corrosive sublimate, or by tan, which are the appropriate tests of albumen and of jelly respectively, but it is copiously precipitated by the muriate of tin, and still more so by the acetate of lead. These reagents have indeed been supposed to act rather upon the salts contained in the serosity than upon the animal matter, but the presence of this latter may be unexceptionably indicated by nitrate of silver; this does not produce any immediate change upon it, and if it be kept in the dark, remains for a long time without alteration, but upon exposure to light, communicates to it a black colour, as it does to animal matter generally.

Although, I think, there is sufficient proof of the existence of this peculiar substance in the serosity, and of its being a proximate animal principle different from any other of the constituents of the blood, yet a contrary opinion has been maintained by Mr. Brande. Mr. Brande principally resting upon the fact, that when the serosity is exposed to the action of the galvanic apparatus, a quantity of coagulum, apparently similar to albumen, is collected round the negative wire, supposes the animal matter in this part of the blood to be merely albumen, held in solution by an alkali. But I think, from the manner in which he performed his experiment, it will be found, that the serosity upon which he operated was in an impure state, and that it still contained a quantity of albumen, which was separated by the action of the apparatus. Besides, I found by direct experiment, that if the alkali in the serosity be neutralized, and even if there be an excess of acid, still there is no tendency to coagulation manifested by the animal matter which it contains, under circumstances which would immediately have produced this effect in an equally concentrated solution of albumen.

Prof. Berzelius entertains a different opinion respecting the nature of the animal matter in serosity. He says, "it is clear that Dr. Marcet's extractive matter is the impure lactate of soda."[7] It would seem that he conceives, that a part of the soda of the blood, which had been supposed by other physiologists to be in a caustic state, is in combination with the lactic acid, and that this salt is united to a portion of animal matter. I do not, however, find that he has distinctly stated what are the properties of this animal matter, which is combined with the lactate of soda, whether he considers it to be a portion of the albumen which remains attached to it, or something of a specific nature. Although, therefore, we may admit, upon his authority, the existence of the lactic acid, or of the lactate of soda in the blood, I do not think that this affords any objection to the proofs that have been adduced by Dr. Marcet and myself of the uncoagulable matter as forming one of its essential constituents.

The only remaining ingredients of the blood are the various salts that are found in it, which, although

7 Med. Chir. Trans. v. iii. p. 231; Progress of Animal Chem. p. 16.

they might in one sense be regarded as extraneous substances, yet they are so constantly present, and so nearly in the same proportion under all circumstances, that we must regard them as an essential part of it. They appear to have been first distinctly noticed by Guglielmini;[8] they were examined with considerable skill and accuracy by Rouelle,[9] and since his time have been frequently made the subject of examination, but for the latest and most correct account of them we are indebted to Dr. Marcet and Prof. Berzelius.

Dr. Marcet, after coagulating the albumen and washing out the serosity from it, evaporated the solution thus obtained, and incinerated the residuum, by which means he obtained the saline matter in a separate state; it was found to amount to rather more than 9 grains in 1000 grains of serum. Of these 9 grains about 6½ were muriate of soda, combined with a small quantity of muriate of potash, about 1½ of the subcarbonate of soda, with minute quantities of the sulphate of potash, and the phosphates of lime, iron, and magnesia.[1] There is reason to suppose that the soda which Dr. Marcet obtained in the state of a subcarbonate exists in the blood in the caustic state. Prof. Berzelius's analysis of the salts of the serum agrees with Dr. Marcet's very nearly in the quantity of the muriates, which he estimates at 6 grains from 1000 grains of the serum; the absolute amount of

[8] Opera, l. ii. de Sanguinis Natura, sect. 52.

[9] Journ. de Medecine, t. xlvi. p. 65, et seq. (1776.)

[1] Med. Chir. Trans. v. ii. p. 370.

the soda we cannot ascertain, as he only gives it as existing in the form of an impure lactate, and with respect to the other salts, the sulphate of potash and the earthy phosphates, he supposes that they did not previously exist in the blood, but were generated during the process of combustion.[2] The difference of opinion which thus appears between two chemists of so much eminence renders it very desirable that the experiments should be carefully repeated, that the existence of the lactic acid may be confirmed, and a further examination made of the animal matter which forms the basis of the serosity.[3]

Since we find that a certain quantity of saline matter is constantly present in the blood, as well as in all the other albuminous fluids, we are naturally led to conclude, that these salts perform some useful purpose in the animal œconomy, yet we are at a loss to say what this purpose can be. It has been conjectured that they may stimulate the nerves of the heart, and thus contribute to the contraction of its muscular fibres,[4] that they may aid in the operation of the secreting organs, or that they may contribute to the process of digestion, but these suppositions are all gratuitous.

[2] Med. Chir. Trans. v. iii. p. 231. See also Dr. Prout, on the salts in albumen ovi, in Phil. Trans. for 1822, p. 385.

[3] Sir Everard Home, in dissecting an aneurysmal tumour, found a mass of crystals, which were analyzed by Mr. Faraday, and are stated to have been "sulphate of lime, with muriate and phosphate of soda," which, it is added, are "salts usually met with in the blood." Phil. Trans. for 1820, p. 8.

[4] Whytt's Works, p, 26.

Sulphur has been enumerated among the constituents of the blood, but its existence would appear to be rather problematical. The only direct proofs that have been brought of its presence is the effect of serum in tarnishing silver, when heated in contact with it, and the fact mentioned by Vogel, that when serum is beginning to decompose, a gas exhales from it which has the property of blackening the acetate of lead.[5]

Although the animal substances which enter into the composition of the blood possess properties, both chemical and physical, which are sufficiently characteristic to distinguish them from each other, yet they may all be resolved into the same ultimate elements, —carbon, oxygen, hydrogen, and azote. MM. Gay Lussac and Thenard have endeavoured to ascertain the respective proportion in which these elements exist in albumen, in fibrin, and in jelly, but the results can scarcely be regarded as more than approximations to the truth. They are as follows:

	Albumen.	Fibrin.	Jelly.
Carbon	52·883	53·36	47·881
Oxygen	23·872	19·685	27·207
Hydrogen	7·54	7·021	7·914
Azote	15·705	19·934	16·998 [6]

[5] Ann. Chim. t. lxxxvii. p. 215. Berzelius objects to the opinion that sulphur is a constituent of the blood, but admits that it is so of the albumen; View of Animal Chemistry, p. 17. This statement appears so singular, that I suspect there must be some inaccuracy in the translation.

[6] Thenard, Chim. t. iii. p. 523, 528, 534. Notwithstanding the ingenuity of the process which was employed by MM. Gay

§ 7. *Different States of the Blood.*

We have a few experiments, but those not very satisfactory, upon the relative composition of the blood in the different periods of life, and in different morbid conditions of the body. From these we may be perhaps authorized to draw the inference, that the proportion of azote increases as age advances, and as coinciding with this opinion, that there is more fibrin in the blood of the adult than in that of the infant. Fourcroy informs us that he found the blood of the fœtus to contain no fibrin, but in its stead a gelatinous substance, which was not reddened by the contact of the air, and also that there were no phosphoric salts in it.[5] We may probably admit the general fact, but the imperfect state of animal chemistry when these experiments were performed will not allow us to place implicit confidence in the statement. Pathologists have minutely described the different appearances which the blood exhibits in different diseases, and the alteration which takes place in its

Lussac and Thenard, and their known skill and address in conducting experiments, it appears that the method which they adopted does not admit of perfect accuracy, and we accordingly find that every subsequent attempt to discover the ultimate elements of organized substances differs more or less from those that have preceded it. For the objections to this process (which appear to be valid) the essays of Prof. Berzelius in Ann. Phil. v. iv. p. 402, et seq. and of Mr. Daniell in Children's Thenard, p. 358, et seq. may be consulted.

[5] Ann. Chim. t. vii. p. 162.

physical properties, but we have scarcely a single fact on which we can rely, that indicates any decided difference in its chemical constitution.[6]

There is, however, one change of great importance, which the blood experiences in its passage along the circulation, that from the arterial to the venous state. The most obvious character which distinguishes these two kinds of blood is their colour, which, in the large trunks of the systemic arteries, is a bright scarlet, and in those of the corresponding veins a purplish red. With respect to the other circumstances in which they differ, it is commonly stated that venous blood coagulates more slowly than arterial, and that it contains less fibrin, but that its specific gravity is greater; I conceive, however, that these points are not very accurately ascertained.

Although the relative temperature of arterial and venous blood is a matter of fact, which one should have supposed might have been easily learned by a simple experiment, yet it seems to be still undetermined, at least we have precisely opposite accounts given by those who have professed to relate the results of their own experiments. Upon the whole the weight of authority seems to be in favour of the temperature of the arterial being greater than that of the venous blood. Dr. Davy, in his experiments, which are the latest and probably the most correct that we

[6] Berzelius has mentioned some minute circumstances in which human blood differs from that of the ox, and from these is led to the conclusion that the latter, notwithstanding the nature of its food, contains more azote than the former. Med. Chir. Trans. v. iii. p. 229.

possess upon the subject, always found the temperature of the blood in the large arteries to be a degree or a degree and a half higher than in the corresponding veins.[7]

A still more important difference is the one which Dr. Crawford laboured so much to establish, the greater capacity for heat of the arterial than of the venous blood; this he estimates to be in the proportion of eleven and a half to ten, and it was upon this fact that he founded his theory of animal heat; but as this subject will necessarily claim our attention in a subsequent part of the work, I shall defer for the present the farther consideration of these experiments, and of the value which is to be attached to them. The change of the blood from its venous to its arterial state being effected during its passage through the lungs, when it is exposed to the

[7] Phil. Trans. for 1814, p. 596, 597. Crawford, p. 273, says that the arterial blood which he employed in his experiments was 102°, the venous 99½°, and it may be inferred that this difference existed while the two fluids remained in their respective vessels. Plenk says, generally, that the temperature of the blood is about 96°, and that the arterial blood is warmer than the venous. Hydrologia, p. 32. The results of the experiments of Mr. Coleman and Sir A. Cooper were, that the venous blood had a temperature superior to that of arterial blood; but this apparent discrepancy may perhaps be reconciled by the statement subsequently made, that although, when the heart was first examined, the blood in the right ventricle was the warmer of the two, yet after remaining for some time exposed to the air, the relative temperature of the two was altered, in consequence, as was supposed, of the greater specific heat of the arterial blood. Coleman on Respiration, p. 36.

action of the atmosphere, is so intimately connected with the subject of respiration, that we shall be more able to estimate the nature and amount of this change when we have inquired into the nature of this function.

Many calculations have been formed of the total quantity of blood in the body, but as the data upon which they have proceeded are extremely uncertain, so the conclusions have been widely different, and, of course, the greatest part of them remote from the truth. Perhaps, upon the whole, the estimate which would seem the nearest approximation is that of Haller, who supposes that the blood may constitute about one-fifth of the weight of the adult body, the proportion of the fluids being greater in youth and diminishing as age advances. A body weighing one hundred and fifty pounds would therefore contain about thirty pounds of blood, and of this it is supposed that three-fourths or more are in the veins, and one-fourth only in the arteries.[8]

The great importance of the blood in the animal œconomy, as the source whence all the parts of the body derive their immediate support, and new matter is obtained to repair the waste occasioned by the exercise of our various functions, induced the earlier physiologists to regard this fluid as the prime seat and direct cause of all diseases. This doctrine is tacitly assumed by Hippocrates, and was strenuously insisted upon and greatly amplified by Galen, who laid it down as the basis of all his elaborate hypotheses, that changes in the state of the blood were the cause of

[8] El. Phys. v. 1, 3.

every deviation from health, and likewise, that an original difference in the nature of the fluids gives rise to those different conditions of the constitution called temperaments. This doctrine, which obtained the name of the humoral pathology, was generally adopted by Galen's successors, and maintained its ground, without ever being called in question, for many centuries. Even after the chemists had subverted most of Galen's dogmas, and had produced a total revolution in medical practice, the blood was still regarded as the origin of diseases, and they were ascribed to an acid, an alkaline, a watery, a saline, a putrid, or some other imaginary condition of the fluids. The mathematical physiologists were likewise, for the most part, humoralists, and ascribed diseases to some change in the condition of the particles of the blood, connected with their weight, size, viscidity, or other qualities, which might be supposed to affect their motion along the vessels.

The first regular opposition which was made to the humoral pathology was by Baglivi, who maintained, in a very unequivocal manner, that disease more frequently originates in the solids than in the fluids, and that any change in these latter is to be regarded as an effect and not as a cause.[9] The premature death of Baglivi prevented him from extending and maturing his ideas in the way which might have been expected from the early promise of his talents,

9 The doctrines of Baglivi will be found in his treatise "De Fibra Motrice," a work which exhibits a powerful and original genius, but still struggling with the shackles of authority.

and it must be admitted that some of his prominent doctrines are fanciful and unfounded. From this time, however, the solids were more or less regarded as influencing the state of the constitution, until the humoral pathology received its most formidable attack from Cullen. Since his time the doctrine of solidism may be considered as forming the prevailing tenets of the medical schools, and is the one to which we usually have recourse for our explanation of the phenomena of the animal œconomy, which are supposed to be connected with its morbid changes. Upon the whole, I conceive, there can be little doubt, that both our original temperaments and our subsequent diseases are more affected by the condition and properties of the muscles and the nerves, than by any physical or chemical differences in the nature of the blood; yet, I apprehend, that the solidists have gone too far in asserting that there is no original difference in the state of the fluids. When we consider the various sources from which the chyle is derived, we might rather suppose that it would vary in its properties in different cases, than that it should possess a constant uniformity, nor are we in possession of any experiments sufficiently decisive to authorise us in asserting that this absolute uniformity exists. This question will, however, be discussed more fully when we come to the functions of digestion and assimilation.

§ 8. *Successive discoveries respecting the Blood.*

From the great number of individuals who have treated of the blood, pathologists, physiologists, and chemists, and from the erroneous opinions which were so long entertained respecting it, it necessarily follows that very different accounts have been given of the nature of its component parts, and that the same substance has frequently received different denominations. It will therefore be desirable to give a brief sketch of what may be called the natural history of the blood, to notice the successive discoveries that have been made concerning it, and to point out the differences that have existed in the opinions of some of our most distinguished writers on this subject.[1] It will not be requisite for our present purpose to trace the progress of opinion beyond the period of Galen; more especially, as there is some reason to conclude that his doctrines were fundamentally the same with those of his predecessors. He supposed that the blood essentially consisted of four parts, to which he gave the names of gore or blood properly so called, phlegm, bile, and black bile.[2] It is not very easy to determine what substances he intended to designate under these terms, nor, except as a matter of mere curiosity, is it a question of any importance, as we know that his opinions are erro-

[1] I shall here beg leave to refer to an essay published in the first volume of the Med. Chir. Trans., where some points respecting the natural history of the blood are discussed more at large than is consistent with the nature of the present work.

[2] For a learned account of the opinions of the ancients and the earlier of the moderns respecting the constitution of the blood, I may refer to an essay by Martine, Ed. Med. Essays, vol. ii. p. 67.

neous. It is probable, however, that under the denomination of αιμα, which has been translated cruor or sanguis, he proposed to designate the crassamentum or clot, that the term φλεγμα was applied to the serum, and that of black bile to the red globules at the lower part of the clot, which, in consequence of being secluded from the air, had acquired a dark colour. With respect to the κολη or bile, as one of the constituents of the blood, it seems probable that its existence was assumed, rather because this substance was separated by the liver, and was therefore supposed to have been previously in the blood, than from there being any distinct part to which the name could be appropriated.

Erroneous as these opinions were it does not appear that any material correction of them took place until the time of Harvey, who, about the middle of the seventeenth century, described the spontaneous separation of the blood into the crassamentum and the serum.[3] He is also supposed to have been the first who gave an account of the coagulation of the albumen by heat, but I conceive that his claim to the discovery is very dubious, and am more disposed to give the merit of it to Lower, who describes it in a much more correct and unequivocal manner.[4] At the same time Malpighi materially advanced our knowledge respecting the nature of the crassamentum, which he found to consist of a white fibrous substance, united to a red colouring matter; he also saw the red particles, which were afterwards so minutely examined by

[3] De Generatio, Ex. 52. [4] De Corde, p. 6.

Leeuwenhoek, and he likewise, to a certain extent, understood the nature of the buffy coat which forms upon the blood drawn during inflammatory fever. I have already had occasion to give an account of the fanciful speculations which Leeuwenhoek formed respecting the red globules, as well as of the discovery of iron in the blood, and the discussion respecting its state of combination, and the effect which it has in producing the red colour of the blood. After much discussion respecting the structure of the red particles, Dr. Young appears to have at length decided this point by showing that the colour of the blood is produced by a vesicle which surrounds a colourless globule, while the still later observations of Mr. Bauer, to which may be added those of MM. Prevost and Dumas, render it probable that these central globules compose the fibrin.

We are indebted to Senac for some advance in our knowledge of the serum, which he distinctly remarks as being similar to the white of the egg, and, at the same time, points out the circumstances in which it differs from jelly. As far as I have been able to learn, it was he who brought into general use the terms serosity and coagulable lymph, the first being applied to the serum generally, and the latter to the crassamentum, the simple term lymph having been before given to the serum. About the middle of the last century there was very considerable confusion in the names applied to the different parts of the blood, partly from imperfect notions of the substances that were described, and partly from different writers describing the same substance under different names, or applying the same name to different substances. This was the case with the word serosity,[5] which was first applied to the serum at large, but was afterwards restricted by Cullen to that part of it which is not coagulated by heat.[6] Cullen appears to have been the first who introduced the term gluten,[7] as applied to what had been before called by Senac the coagulable lymph, and by Guglielmini, with more propriety, the fibre of the blood.

It was about this time that we first hear of the jelly or gelatine of the blood, but both the terms employed, and the properties by which it was designated, are so vague, that it is impossible to draw any correct conclusion from them as to what the writers intended to describe. The jelly of the blood was, however, in the year 1790, explicitly announced by MM. Fourcroy and Vauquelin, and four years afterwards a very detailed account of it was given by MM. Parmentier and Deyeux, in which the method of pro-

[5] The earliest work in which I have met with the term "serosité" is in an essay of Lemery's in Mem. Acad. for 1729, on the Action of Borax on the Animal Fluids. It was applied by him to the serum; it was used in the same sense by Senac and by Rouelle; Journ. de Med. t. xl. p. 68, 1773. By some of the French physiologists it is applied to the different species of dropsical fluids, or any aqueous secretion or excretion from the blood; see an elaborate article, "Serosité," by M. Villermé, in Dict. des Scien. Med.

[6] There has been the same diversity in the use of the word cruor; the ancients appear to have understood by it the red blood generally, and it is used in this sense by Guglielmini, de Sang. § 44; Haller, El. Phys. v. 2. 8, 9, and Cullen, Inst. § 249, apply it to the crassamentum, while Blumenbach restricts it to the red globules; Inst. by Elliotson, p. 6.

[7] Institutions, § 4. c. ii.

curing it and its distinctive properties were laid down with a degree of minuteness that appeared to remove all doubt on the subject. I have had occasion, however, to point out the error that has prevailed on this subject, and, at the same time, to observe, that although we can have no doubt about the non-existence of jelly in the serosity, yet we have been unable to ascertain exactly what is the nature of the animal matter which enters into its composition.[8]

[8] A very interesting series of observations and experiments have been lately made upon the blood by MM. Prevost and Dumas, which present us with an idea of its nature and constitution that is, in some respects, different from the one generally adopted. They regard the blood as essentially composed of serum, holding in suspension a quantity of red particles, which consist of central colourless globules enclosed in a coloured vesicle. When the fluid is drawn from the vessels, the central globules, in consequence, as it may be inferred, of the loss of their envelope, are attracted together, and disposed to arrange themselves in lines or fibres, thus forming the basis of the clot or crassamentum. These fibres mechanically entangle in the network which they form a quantity of the serum and of the colouring matter, which, by simple draining, or by sufficient ablution in water, may be removed from them. What we then procure is pure fibrin; they therefore identify this substance with the central globule, and the clot generally with the entire particle. I may observe that Berzelius always considers the fibrin and the red globules as distinct proximate principles; Progress of Animal Chem. p. 16, 23, 46. The colouring matter is said to be a compound of a peculiar animal substance and the peroxide of iron: water possesses the property of breaking down these vesicles and detaching them from their nuclei, but does not dissolve them. The authors do not appear to have examined the nature of this animal matter, nor to have made any particular observations upon the state or quantity of the iron. They have not entered into any detail of the chemical relations of the albu-

The general conclusions that we may form respecting the nature of the blood are, that it is a compound fluid, consisting of several ingredients of various physical and chemical properties, dissolved, or at least

men, they only state that various re-agents act upon it in the same manner as upon fibrin. The existence of the uncoagulable matter is recognized and is characterized as a substance soluble both in water and in alcohol, and precipitated by the salts of lead; they suppose, with Berzelius, that it is combined with lactate of soda.

But the most important part of the researches of these gentlemen consists in some very valuable experiments upon the proportional quantity of globules contained in the blood of different kinds of animals, and of different parts of the sanguiferous system in the same animal. As they appear to bear an exact ratio with the temperature of the animal, I shall defer giving a particular account of them until we arrive at the subject of animal heat. I shall only anticipate so far as to state, that the higher is the natural temperature of the animal, the greater is the proportion of particles in the blood, and that arterial contains a greater proportion of them than venous blood. They afterwards entered into a very curious investigation respecting the changes which the blood experiences in animals who have had the kidneys extirpated; the result was, that notwithstanding the loss of these organs, the urea was formed and might be detected in the blood; but I shall postpone the farther consideration of this subject to the chapter on secretion. Ann. Chim. et Phys. t. xxiii. p. 50, et seq. and p. 90, et seq.

The authors do not refer to any of Sir Everard Home's papers on the blood, a circumstance which is to be regretted, as it would have been interesting to have learned how far their microscopical observations agreed with those of Mr. Bauer. We may, however, remark upon the coincidence between their account of the fibrin and Mr. Bauer's description of the origin of the muscular fibre. It would scarcely be supposed, from the perusal of these papers, that many of the most curious particulars respecting the globules of the blood which they have detailed, had been observed long before by Hewson.

suspended, in a large quantity of water. Of these the fibrin and the colouring matter are disposed to unite, to separate partially from the water, and to form the crassamentum or clot, to which the iron is also attached. The albumen, the uncoagulable matter, and the salts, remain in a state of solution in the water, and compose the serum; by heat the albumen is rendered solid, and may, in this way, be detached from the serosity, which consists of a portion of water holding in solution the uncoagulable matter and the salts. By slow evaporation part of the salts may be procured in the crystalline form, but the whole of the saline matter can only be obtained by calcining the residuum after evaporation, when the animal matter is consumed, and the neutral and earthy salts left behind, although probably in a different state of combination from what they originally possessed.

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