Outlines of physiology and pathology; with a supplement to the physiology. Embracing an account of the most recent additions to that science / [William Pulteney Alison].

OUTLINES

OF

PHYSIOLOGY AND PATHOLOGY;

WITH

SUPPLEMENT.

BY

WILLIAM PULTENEY ALISON,

M. D., F. R. S. E.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, AND PROFESSOR OF THE INSTITUTES OF MEDICINE IN THE UNIVERSITY OF EDINBURGH.

WILLIAM BLACKWOOD & SONS, EDINBURGH; AND
T. CADELL, STRAND, LONDON.
MDCCCXXXVI.

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SUPPLEMENT

TO

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THE progress of some departments of Physiology of late years has been so rapid, that no one need be ashamed to acknowledge important deficiencies in what, a few years ago, he considered a fair outline of the Science.

Nor is it merely because many new facts have been ascertained, that I have thought it desirable to place in the hands of Students of Physiology, this Supplement to the Outlines formerly published. Some of the facts lately recorded, when taken in connection with previous observations, seem of such importance as to correct inferences, which had been generally drawn from facts formerly known; and to extend and modify the first principles of a science, which I continue to think is most properly taught *synthetically*, *i. e.* by laying down first principles in the outset, notwithstanding that some of these principles can be, as yet, expressed only in very general terms.

In drawing up this statement of what seem to me the most important recent additions to Physiology, I have endeavoured to make it intelligible without reference to the Outlines; but have always referred to the part of the Second Edition of that Work, where the subject of each Section of this Supplement is treated.

SUPPLEMENT

TO

OUTLINES OF PHYSIOLOGY.

I. OF VITAL ACTION IN GENERAL. P. 4, *et seq.*

CONTRACTIONS of living solids, as stated in the text, are essential to all other vital changes in the perfect state of all the higher animals. It is still supposed by many, that all those movements of fluids in the interior of organized bodies, which are peculiar and essential to the living state, are produced by these contractions; and it has been, even lately, supposed that these movements, modified by the structure of the solids containing the fluids, are sufficient to explain, according to the ordinary laws of chemistry, all those chemical changes which are essential to the nutrition of living animals; or if not, that the varied powers attributed to the Nervous System might supply the deficiency. The properties of Irritability and Sensibility, therefore, or, in more general terms, of Vital Contractility and of Nervous Agency, have been regarded as the only strictly vital powers to which physiological facts should be referred; and the different vital powers enumerated by BICHAT and others, are in fact only different modifications, and some of them only supposed modifications, of these powers.

But it may now be confidently asserted, that this generalization is premature and erroneous, especially in two points, 1. Because there are many movements peculiar to the fluids of living bodies, not referable to the impulse of their contracting solids; and 2. Because there are many chemical changes peculiar to living bodies which cannot be

explained by the movements of their fluids, by the structure of their solids, or by the influence of their nerves. And this makes it expedient to take a more general view of vital actions, of the conditions under which they take place, and the different powers which they seem to indicate, before treating specially of the vital contractions.

In taking this general view of the subject, we first set before us the *purposes of vital action*, *i. e.* the objects which are to be accomplished in the maintenance of organic structures, by means of the powers impressed by Nature on the different forms of organized matter, and of the arrangements observed by her in the formation of organized beings.

In the case of vegetables, the only objects appear to have been, to maintain the infinite variety, and provide for the successive reproduction, of those living structures themselves, in such forms and situations, and at such times, as are requisite for the different uses which vegetables serve to the various orders of animals, for their nutrition, their respiration, their protection, and the gratification of their varied wants, and capacities of enjoyment.

In the case of animals, it is obvious that those functions in which Mental Acts are essentially concerned, which establish the relations of external objects to a sentient being, and invest this being with more or less of voluntary power over the surrounding world, are the main objects of Nature in their construction and maintenance. These functions are all fitted to be *sources of pleasure* of various kinds, and in various degrees; and the provisions of Nature for the infinite extension and incessant reproduction of sentient pleasure, over the whole surface of the earth,—for the extension of the powers and capacities of enjoyment in the ascending scale of animals,—and for the higher mental operations and more varied occupations of the human race,—are evidently the main objects to which the laws and arrangements not only of animals, but of the whole organized creation, and of much of the inanimate world, are adapted.

We thus see the essential importance of the distinction drawn by BICHAT and others between the functions of Or-

ganic and those of Animal Life;—*i. e.* between those which take place, in the natural state, without the intervention or consciousness of Mind, and alike in animals as in vegetables; and those in which some act of mind is essentially concerned, and which constitute the characteristic of animals. But although this distinction should always be carefully kept in mind, yet it cannot be taken as the basis of an arrangement of Physiology, for the reason stated in the text, viz. that mental acts, and bodily changes connected with them, are not merely *superadded* to the organic life of animals, but are intimately connected or interwoven with it; forming, in the adult state of all but the very lowest animals, part of the conditions necessary to the maintenance of the quantity, and of the vital qualities, of the nourishing fluid on which all the organic life is dependent.

Another object in the constitution of animals is essentially different from any which the economy of vegetables includes, viz. the provision for repressing the numbers of the animal species themselves, which would otherwise become excessive, in proportion to the limited extent of the vegetable world, on which their nutrition is directly or indirectly dependent.*

Hence many tribes of animals are formed by nature to seek for gratification and find sustenance in the destruction of others; and their structure and economy, as well as their appetites and other mental acts, are adapted to this chief end of their existence. The same difficulty presses on the human species, and is the main cause of many of the evils with which all human societies have to contend, although it is only of late years that this principle has been distinctly recognised. The means which Nature has furnished, and the degree of efficacy which she has granted to human wisdom and foresight, for the correction of those evils, are beyond the province of Physiology, but may be noticed as the bond of connexion between this science and that of Political Economy.

* See ROGET's Bridgewater Treatise, vol. i. p. 45.

We next observe, that, for the attainment of these objects, the laws of Nature seem absolutely to require that certain general conditions shall in all cases be observed. By the knowledge of these *general conditions of vital action*, taken in connexion with that of the objects of such action, we are enabled in a great measure to understand the purpose of the different functions of animals and vegetables; and by observing the various circumstances and situations in which living bodies are placed, and the different objects they are intended to fulfil, we are enabled in many instances to perceive the use of the difference of the modes in which these functions are performed, and of the variety of structure provided for their performance, in different living beings.

When our knowledge of these points shall be reduced to ultimate facts or general principles, the Science of Physiology will have attained its highest perfection: and even in its present imperfect state, it is obvious, that there are certain laws of vital action which extend to the whole range of living beings, while others are imposed on particular classes or tribes by the special conditions of their own existence; and that a knowledge of both is required of the physiologist.

As all living bodies are essentially dependent on the continued process of Nutrition mentioned in the text, the conditions necessary to the maintenance of this process, must be regarded as the most general and fundamental Laws of Vitality; and although the terms Assimilation of Aliments, and Elaboration of Animal Fluids, convey as yet only a very general meaning, they will acquire precision as the science advances.

The following general laws or conditions of Nutrition, and therefore of all other Vital Actions, appear to have been fairly deduced from observation of all living beings.

1. In the order of things now established on earth, in by far the greater number, if not the whole of living beings, life springs only from life (*i. e.* from the previous life of others of the same species), and is maintained only by

the previous life of other organized beings; the materials of the nourishing fluid, both of animals and vegetables, being always in part furnished by previous organized structures; and these are always reduced to a fluid state by Water, taken in along with them.

The higher or more complex animals derive much of their nourishment from animals lower in the scale, and from vegetables; and the assimilation of the materials composing these may be said to begin in those simpler structures. Vegetables, as will afterwards appear, derive much of their nourishment from the carbonic acid of the atmosphere, and this is the only important exception to the law above stated; but much of that acid, in the present state of things, is furnished to the atmosphere by living animals, so that all the classes of living beings seem to aid in nourishing one another.*

2. The crude materials furnished from without are never applied directly to the nourishment of any organized structure, but are always combined with, and *assimilated by*, matters furnished by previous actions of the organized structure into which they are taken; and are thus fitted for its nourishment. This Assimilation, by means of a previously existing product of vital action, is a general law of the maintenance both of animals and plants, although Digestion, such as takes place in animals, will appear to be a secondary law, imposed by the peculiar conditions of their existence only.

This condition of the vitality of living bodies points out the use of various organs, and of many of the Secretions, and part of the use of the Circulation in the more complex structures. It explains also the necessity of various provisions included under the Function of Generation; viz. of those vital actions of the parent which furnish to the young animal or plant a supply of matter already assimilated to its nature, until, by this supply, its own organs are developed and fitted for their office; and as this condition

* See PROUT's Bridgewater Treatise, p. 470.

could not have been fulfilled in the beginning of the existence of the *first* individual of any species, it clearly shews that the origin of living beings on earth must have been given by a power superior to, and independent of, the laws on which their existence and continuance now depend.

3. Another essential condition of vitality is, the exposure of the nourishing fluid of each organized structure to the Air, in order that oxygen may act upon, and probably be absorbed into it. Hence arises the necessity of organs of Respiration, varying according to the circumstances of the different orders of living beings.

In the nourishing fluid, which has been acted on by the air, a substance is always found in large quantity, and nearly of the same nature in different plants and different animals, viz. of the nature of gum or sugar (*i. e.* a combination of carbon and water) in vegetables, and of albumen in animals, out of which, in the different parts of the organized frame, a great variety of deposits appear to be subsequently formed.

4. There is great variety in the different orders of living beings, and in many cases in the different states of the same, as to the rapidity with which the supply of nourishing fluid, and its exposure to the air, necessary to the maintenance of the vital actions, of which they are respectively susceptible, is kept up; and the general law is, that the more rapid the supply, and the more frequent its exposure to the air, the greater is the energy of vital action, and the variety of vital phenomena, and the more immediate the dependence of vitality on these conditions; therefore, the shorter the endurance of life when these are withheld. On the other hand, the slower the necessary supply of nourishment, and the slighter its exposure to air, the less energy and variety of vital action are in general observed, but the greater is the endurance or tenacity of life, *i. e.* the longer will the faculty of resuming vital action be preserved, after those conditions are withdrawn. These differences are seen in different animals, in different ages and states of the same animal, especially of those which hybernate, and still more

in the different states of vegetables, *e. g.* seeds or bulbs as compared with perfect plants. The knowledge of these principles explains the use and importance of many varieties in the arrangements of the organs of digestion and respiration in different classes and different states of animals, according, not merely to the circumstances in which they are placed, but to the energy of vital action which they are intended to exert.

5. All vital action is equally dependent on Heat, as on the other conditions now stated; is seen only within certain limits of temperature, and within these limits is excited by a rise and depressed by a fall of temperature; the amount of change being more effective in either way than the absolute degree of temperature which may be applied. The necessary limits of temperature vary in different classes and states of living beings, and the influence of heat resembles that of air in this respect, that the greater the amount and variety of vital action, the more immediate is its dependence on the maintenance of the usual temperature. Thus animals are more easily killed by cold than vegetables,—warm-blooded than cold-blooded animals,—those in a state of activity than those in a state of torpor,—perfect animals than eggs or pupæ,—perfect plants than seeds or bulbs; and nearly the same gradation is observed as to the injurious effects of excessive heat.

Hence the importance of the provisions of nature for the evolution and retention of heat in living bodies; and of varieties in those provisions, not only according to the circumstances in which animals are placed, but also according to the energy with which they are endowed.

6. Vital action is also subjected to a remarkable influence from Light, on which the power of vegetables to decompose the carbonic acid of the atmosphere, and to fix its carbon, and to acquire the green colour, is essentially dependent; and the effect of which on the colours of animals, and on the growth and development of young animals, though not so well understood, is illustrated by several facts. The ef-

fect of Electricity, and of several Poisons, on all vital action, is also to be regarded as an ultimate fact.

7. The fundamental vital action of assimilation and nutrition is in all cases attended with the formation of certain substances, which act as poisons if retained, and which are therefore uniformly *excreted* from the system, or at least from the nourishing fluid. The carbonic acid, thrown off from all living beings, the bile, urine, &c. of animals, and the proper juices, resins, and oils of vegetables, are of this kind; and the necessity of their expulsion explains the necessity of the different organs of Excretion.*

As the reception of nourishment into living bodies is connected with a continual process of deposition or Nutrition, so the excretions from the animal body are believed to be connected with a continual Absorption; and there must evidently be certain general laws which determine the noxious qualities, and the expulsion of certain compounds, as well as the nutritious qualities, and appropriation of others, although the same chemical elements are contained in both; but no such general principles have as yet been clearly ascertained.

8. It is a leading fact, in regard to the nutrition of all healthy living beings, that the substances formed by this vital action, although composed of the same elements which in the inanimate world form crystalline compounds, and although often assuming perfectly definite forms, *yet never take the form of crystals*, but rather those of membranes, fibres, and especially of globules and cells. The discovery by RASPAIL of a cellular texture, and of fluid contents of the cells, in many substances formed by vital action, such as starch, fat, albumen, &c. is important, as marking an important distinction between organic and inorganic matter. According to the observations of Dr PROUT, and of RASPAIL, even the minutest portions of all organized substances are composed of an intimate combination of carbon, oxygen, and hydrogen (with or without azote), and of certain

* See DECANDOLLE, Physiol. Veg. t. i. p. 217, and t. iii. p. 1347; and MACAIRE, Edin. Phil. Journ. 1833.

saline and earthy matters; and there must be something in the mode of combination, peculiar to the products of vital action, which prevents the ultimate atoms of any of these constituents from coalescing so as to form crystals, as in inorganic matters; or, as Dr PROUT expresses it, the vital action by which organized substances are formed, is attended with a principle of Self-Repulsion in the ultimate atoms of the elements which go to their formation. In the excretions, the chemical elements are disposed to form crystalline compounds; but whether this is because they have escaped from the dominion of vital laws, or whether they are excreted because they have become fitted for forming such compounds, is uncertain.

9. All vital action is only of temporary duration in any organized structure, and its gradual extinction by the mere progress of time, is always attended by the same kind of change in the condition of the structure, viz. a gradually increasing proportion of the *earthy* constituents of the solid textures; with which a slower and more languid motion of the fluids, and a gradual deficiency of the vital act of nutrition, are always connected.

The general facts now stated are observed *wherever living structures are maintained;* they appear to be the essential conditions, and characters, of the organic life of all living beings.

Our next and most important object is to inquire, what are the Vital Powers or Forces by which these vital actions are maintained? We cannot suppose that the power of contraction in living solids is essential to their production; *first*, Because in the commencement of the existence of living bodies, as will afterwards appear, vital actions take place, not only prior to the existence, but essential to the formation, of any contracting solids; and *secondly*, Because there is no evidence of contractions of living solids being concerned in the ordinary nutrition and growth of vegetables, nor indeed of the lowest of the beings commonly ranked as animals, such as the sponges. That we cannot suppose

the structure of the vessels or cavities in which the nutritious fluid is contained, to be the cause of the difference in the products formed from that fluid in the different parts of organized bodies, will appear from facts to be stated under the head of Nutrition and Secretion. That we are not entitled to suppose any agency of Nerves to be essentially concerned in producing these phenomena of organic life, will also appear from facts to be then stated; and especially from this leading fact, that all these phenomena take place in vegetables; where we not only cannot detect nervous matter, but have no reason to infer its existence, because the only phenomena which we *know* to be connected with it in animals, do not exist in plants.

But two strictly vital powers (according to the definition in the text, p. 2) appear to be indicated in all such vital actions, viz. 1. Vital affinity, by which the elements of the nutritious matter must be thrown into the peculiar combinations necessary for forming the organic compounds; and, 2. Some moving power peculiar to life, in all probability of the nature of Attraction and Repulsion, by which the particles of that matter must be moved in the directions necessary for these combinations.

That such a moving power, independent of impulse from the contractions of living solids, acts habitually on the fluids of living bodies, although not generally admitted, seems sufficiently established by the following facts.

1. Certain living bodies, such as sponges, the larvæ of Batraciæ, and many of the lower classes of animals, produce regular and uniform currents in the water in which they live, moving in definite directions; and in several instances it seems certain, that no movements of living solids are the causes of these currents; for in some, no movements, even of ciliæ, are seen to accompany the currents, and even where such movements exist, they often appear inadequate to their explanation.*

* Du Trochet in Ann. des Sciences Naturelles, t. xv. Grant in Edin. Phil. Journal, vols. xiii. and xiv. Sharpey, Edin. Med. and Surg. Journal, vol. xxxiv. Guillot in Journ. de Physiologie, t. xi. p. 183–4.

2. In certain of the fluids of many plants (milky juices, in which minute globules are suspended), uniform and regular movements have been accurately observed, to which the name of Rotation has been given in the case of the cellular plants, and that of Cyclose in the vascular plants; which are not only unattended with any visible contractions of the cells or vessels containing those fluids, but are of a nature which no contractions of these solids appear capable of producing,—particularly in the common case, where the currents in these fluids follow opposite directions in cells or vessels, which are so closely contiguous, that any movements of the parietes of one must necessarily extend to those of the others. And similar phenomena are distinctly seen within the translucent bodies of some of the lowest animals, particularly the Sertularia and Tubularia.*

3. It will afterwards appear that, in the fœtal state of animals, portions of the semifluid matter with which the embryo is connected, are moved in various directions before the heart acts, or any contractile vessels are formed.

4. It will also afterwards appear, that when the circulation in the minute or capillary vessels of animals is carefully observed, the globules of the blood are found to exhibit certain movements peculiar to the living state, but which cannot be ascribed to any contractions of the living solids; and the same observation applies to the *diffuse* circulation seen in many animals, low in the scale of beings, whose blood is not confined to vessels.

5. Many phenomena presented by the blood of animals, when drawn from their vessels (which will be afterwards considered), shew that its component parts have relations

Raspail, Bulletin des Sciences Naturelles, t. xii. p. 193, and Chimie Organique, § 764, *et seq.*, and § 582. Lister in Phil. Trans. 1834, p. 377, 379.

* See Schultze in Biblioth. Univ. t. xxxvi. Amici in Ann. de Chimie, t. xiii.; and Ann. des Sciences Naturelles, t. ii. Schultze, and Mirbel and Cassini, in do. t. xxii. Agardh in Bulletin des Sciences Naturelles, t. xi. Raspail, do. t. xii. Le Baillif, do. do. Meyer, po. t. xviii. Lister, in Phil. Trans. 1834, p. 365, 367, 377.

to each other, which soon cease after it is drawn from the body, and which can be designated only as vital attractions and repulsions.

6. Many of the phenomena of local determinations of blood in health and disease, to be afterwards mentioned, when compared with the known properties of the blood-vessels, seem clearly to indicate, that the flow of blood in the smaller vessels is influenced by causes independent of any contractions either of the heart or bloodvessels.

7. It will afterwards appear, that no cause can be assigned for the appropriation of certain of the materials contained in the compound blood, and the rejection of others, in each texture and organ of the body, unless we suppose that there are certain attractions and repulsions peculiar to the living state.

8. A similar observation may be applied to several facts to be afterwards mentioned as to Absorption in living bodies, and to the motion of the fluids in the lymphatic vessels.

Attempts have been made to refer all these movements, and changes of movement, in the fluids of living beings, to simply physical principles, particularly to the principle of Endosmose and Exosmose, *i. e.* to the tendency of different fluids to attract and penetrate each other unequally, through the intervention of a permeable membrane, and thereby to acquire motion in a definite direction. But although it is certain that this physical principle must act, in a certain degree, as a cause of motion in the fluids of living bodies, it is quite certain that the endosmose and exosmose often exist, even in organized structures, where no strictly vital changes follow; that several of the vital phenomena just enumerated take place where the conditions of the endosmose and exosmose are absent; and farther, that these and all other vital phenomena are liable to great and often sudden change and modification, from various stimuli and sedatives, from poisons, from injuries, and, in the case of animals, from affections of the nervous system,—which have no such effect on this or any other simply physical

principle; and therefore, we are entitled to set aside this explanation as inadequate.

The vital attractions and repulsions, which seem to be the chief causes of the phenomena in question, are probably in a great measure the effect of those vital affinities by which the composition of the fluids of living bodies is continually altered, and the solids nourished. But some of the facts to be afterwards considered will appear to indicate a principle of movement in the animal fluids, independent of chemical changes, as well as of mechanical impulse.

The vital powers thus illustrated, appear to be in operation wherever living structures are maintained; and in the nourishment and growth of vegetables, and of some of the very lowest animals, there is no distinct evidence of any other powers being concerned. But in other parts of the economy of all animals, there are indications of other vital powers; and in almost all, *contractions of living solids* are essential, even to the nutrition of the structure. The moving power afforded by such vital contractions, appears to be a necessary part of the economy in all but the very lowest orders of animals, for two distinct reasons, both resulting from the destined office of animal bodies as the residence of mental phenomena, and the instrument of mental powers. 1. The complexity of their structure, and necessary separation of their different organs, which are to be maintained by the same nourishing fluid, are so great, that the powers hitherto considered are inadequate to the extent of motion which that fluid must perform, and several even of their organic functions require the aid of vital contractions of solids, in order that they may be effectually performed. 2. The power of *spontaneous locomotion* is necessary, both for their own support, and for the ulterior purposes which they are to serve in the world; and this function, wherever it exists, is performed by means of vital contractions of organized solids. Hence certain laws of vital contractions, although apparently of less general existence among living beings, are yet, in the physiology of all the higher classes of animals, equally essential and fundamental principles.

The different modifications of what we call Nervous Agency, will afterwards appear to be the means by which the physical changes of living animals are placed in connexion with mental phenomena, and subjected to the control of mental acts; but it is not proved that they are essentially concerned in any of the strictly organic functions, and without proof it appears unphilosophical to assume any such principle.

Thus it appears, that the agency of four distinct vital powers in the economy of animals must be admitted, in order to explain the essential differences of the phenomena there seen, from those of inanimate matters,—viz. Vital Attraction and Repulsion, Vital Affinity, Vital Contractility, and Nervous Agency. Of these the two first, although the most general in their operation, are the least understood. In the adult state of all the higher animals, the vital contractions of certain of the living solids are essential to all the other changes, and demand our first consideration.

II. OF THE LAWS OF VITAL CONTRACTIONS. P. 6, *et seq.*

The accuracy of the observation of Prevost and Dumas on the zig-zag form taken by muscles when contracting, has been questioned by Raspail;* but his own description, that the contraction of a muscle, under the microscope, appears as "shortening of the fibres, accompanied by increase of diameter, which causes small swellings throughout their whole extent," is not materially different.

It is probably impossible to draw a distinct line among the varieties of vital contraction seen in different textures, but it is right to observe, that, besides the varieties termed Irritability and Tonicity in the text, there is an intermediate kind of vital contraction, *produced by a stimulus*, but *not* immediately or necessarily *alternating with relaxation*, to which the term Simple Contractility has been applied. This is

* Organic Chemistry, translated by Henderson, p. 260.

most distinctly seen in the contractions of the bladder and of the uterus; and is likewise, though in a slighter degree, perceptible in arteries, veins, and absorbents,—the chief vital power of which is tonicity.

The stiffening of voluntary muscles after death, noticed at p. 8, as the last exertion of their Tonic power of Contraction, differs essentially from the vital contractions which are excited by irritation. Thus, it is more directly excited by heat,—it is not excited by galvanism, nor mechanical irritation, nor does it take place until the excitability by the latter means is extinct;—and it is stopped or prevented by the application of salt, or other substances which excite the irritability of muscles. But that it is strictly a vital change, appears from the tendency to it being lost by muscular exertion, and recovered by rest; from its disappearing before putrefaction begins; and from its being totally prevented in some cases where death takes place from a sudden cause, powerfully affecting vital action of all kinds, but not altering the organization of the muscles, such, *e. g.* as lightning, mechanical suppression of the heart's action, or violent mental emotion.

In opposition to the obvious inference from the experiment by Dr Wilson Philip, quoted at p. 11, in which muscles continued irritable on repeated excitation of their own fibres, fully as long after their nerves had been cut as when they were entire, it has been stated by Mr J. W. Earle, that when muscles thus severed from the brain and spinal cord are stimulated until their irritability is exhausted, they cannot regain it; which he thinks sufficient to establish, in opposition to the facts stated in the text, the theory of a nervous influence flowing from the brain, and giving irritability to the muscles. But by the method of exhausting the irritability of the muscles, adopted by Mr Earle, inflammation and injury of their texture were produced;

* See Hunter on the Blood, &c. vol. i. p. 153, *et seq.*; and Prater, Exp. Inquiries in Chemical Physiology, p. 159, *et seq.*, 185, *et seq.*, and p. 216.

and in subsequent experiments by Dr J. Reid, where this source of fallacy was avoided, the irritability of muscles, of which the nerves had been divided, was apparently, if not absolutely, extinguished, by wetting them with a saline solution, and galvanising them strongly, and was afterwards completely regained, notwithstanding that their sensibility and voluntary power had been wholly destroyed;* which fact is quite in conformity with others stated in the text, and seems nearly decisive against the theory in question.

The opinion stated at p. 11, that it is only through the intervention of nerves that any stimuli act on muscles, is still held by many physiologists, who have abandoned the theory of a nervous influence affording supplies of irritability to the muscles; and Dr C. Henry† has adduced in support of it experiments, in which it appeared that narcotic poisons, which suspend the functions of the nervous system, have a similar power over the irritability of muscles, even when not applied to themselves. But this only proves, what is stated in the text, that the irritability of muscles is remarkably liable to diminution from causes acting on the nervous system, and depressing its powers: it does not take away the force of the fact, that the involuntary muscles, although very irritable, are not excitable by mechanical irritation acting on their nerves only. To which Dr Wilson Philip has added this remarkable fact, that even the voluntary muscles are not excitable by *chemical* irritations acting on their nerves only, although these irritations are quite effective when acting on their own fibres.‡ These facts seem hardly reconcilable with the supposition of all stimuli acting on muscles only through the intervention of nerves.§

* Fourth Report of British Association, p. 671.

† Edin. Med. and Surg. Journal, January 1832.

‡ Inquiry into the Nature of Sleep and Death, &c. p. 101.

§ Dr Henry seems to think, that because "the voluntary muscles, in all their natural and sympathetic contractions, are excited through the medium of nerve," there arises a presumption that "the same sequence of contraction to nervous change may be predicated of the natural actions

III. OF THE CIRCULATION IN GENERAL. P. 15.

As it is now known that a circuitous movement of the fluids takes place, not only in insects, as was first ascertained by CARUS, but in all the other articulated animals, and even in many of the zoophyta and in plants, we cannot regard the doctrine of CUVIER (stated at p. 16), as to the uniform connexion in animal structure, between a regular circulating system and a separate organ appropriated for respiration, as strictly correct. It must be admitted that a circulation exists in animals formerly thought to be nourished simply by *imbibition*, and where respiration takes place generally over the body. But such circulation as exists in the zoophyta and in plants, seems to be neither confined to vessels, nor necessarily dependent on contractions of vessels; and even in many of the articulated animals, a great part of the circulation appears to be *diffused* through the parenchyma. The observation of CUVIER is still applicable to that more perfect circulation where the circulating fluid is confined to vessels, and moved chiefly by the impulse of certain contractile parts of the vascular system, and is concentrated on its way to and from the parts which it nourishes, in order that it may be subjected to that impulse.*

IV. OF THE MOTIONS AND SOUNDS OF THE HEART. P. 21.

THIS subject has been lately much studied in experiments on animals in a state of complete stupor, but in

of the involuntary muscles." But the important distinction is here neglected, that the natural actions of the voluntary muscles are always excited by mental acts, and their excitement must therefore be expected to take place through nerves, " qui soli in corpore mentis sunt ministri ;" but the natural actions of the involuntary muscles have no connexion with mental acts, and are analogous to motions in vegetables where neither mental acts nor nerves exist ; and therefore, the presumption is rather that they will not be excited in the same manner.

* See ROGET's Bridgewater Treatise, vol. ii. p. 229, *et seq.*

which the circulation was maintained by artificial respiration; particularly by Dr C. Williams in London, and by a Committee of the British Association in Dublin.* The result of both sets of experiments is as follows:—The statement at p. 21, that the dull sound and chief impulse attend the contraction of the ventricles, and stroke of the apex of the heart against the parietes of the chest; and that the sharp sound, and weaker impulse, attend the diastole of the ventricles, and precede, by a short interval, the contraction of the auricles,—is confirmed. The cause of the sounds is ascertained (in opposition to the opinion lately stated by Magendie) to be in the heart itself, although they are rendered more audible by its coming into contact with the sternum and ribs. The first, or dull sound, appears to proceed partly from the contraction of the fibres of the ventricles, but chiefly from the impulse of the blood, moved by that contraction, on their irregular internal surface. The second, or sharp sound, having been found to be altered or suppressed by fixing one of the semi-lunar valves against the side of the aorta or pulmonary artery, seems certainly to depend (as was indeed previously conjectured)† on the sudden stroke, against those valves, of the columns of blood in these arteries, which, but for their intervention, would fall back into the ventricles at the moment of their diastole.

V. OF THE CAUSES OF THE MOTION OF THE BLOOD IN THE ARTERIES AND CAPILLARIES. P. 27, *et seq.*

It being ascertained, *first*, That although the heart's action, in warm-blooded animals, is essential to the support of the circulation in all parts of the vessels (p. 27), yet

* Williams on the Pathology and Diagnosis of Diseases of the Chest. 3d Edition, p. 170, *et seq.*

Transactions of the Medical Section of the British Association in Dublin Medical and Chemical Journal. September 1835.

† See Elliot de Corde; Edin. 1831, p. 53.

the numerous irregularities in the distribution of the blood to individual parts of the system, both in health and disease, demonstrate the flow of blood to be greatly influenced by causes applied at the extremities of the circulation (p. 30); and *secondly*, that arteries in the living body possess the power of Tonicity, and, in a slight degree, that of simple Contractility on irritation, but not the power of Irritability, or of contraction and expansion after the manner of the heart (p. 28); we may thus state the extreme difficulty of explaining the phenomena of the motion of the blood through the arteries and capillaries, on the supposition of the only moving powers peculiar to life, being the impulse from the heart and the pressure of the arterial coats.

The nature of the only contractile power which experiments authorize us to ascribe to arteries is such, that any *increase* of its agency can only be conceived to impede and retard the flow of blood through them, particularly if it be exerted, on the whole, more in the smaller than the larger vessels; and accordingly, in the experiments of WEDEMEYER (p. 28) it appeared distinctly that this retarding effect was produced by injecting such stimulating liquids as must have excited the contractile power of the arteries.

Hence it has been supposed (particularly by MAGENDIE and MAYO *) that all local determinations of blood are produced simply by a *relaxation* of arteries, and diminution of their pressure on the blood. And in several experiments made lately in Edinburgh on the contractile power of arteries leading to inflamed parts, as compared with those leading to sound parts, at different periods of the inflammation, and in different parts of their course, this relaxation, or diminution of contractile power (giving, of course, increased effect to the impulse from the heart), was the only alteration that could be perceived.†

But if we suppose this relaxation of arteries to be, not

* Outlines, p. 87.

† Fourth Report of British Association, &c. p. 674.

merely the accompaniment, but the sole cause, of such determinations, the following difficulties immediately present themselves.

1. If this be so, the only vital power concerned in moving the blood along the arteries is the impulse from the heart; and the action of the arteries, in the natural state, instead of being an auxiliary to overcome the obstacles to the free motion of the blood mentioned at p. 26, is a great additional obstacle,—so great, that wherever it is lessened in any part of the body, the blood makes its way into that part in unusual quantity. Now, the amount of those obstacles, and particularly the very moderate impetus with which the blood flows along the larger vessels (as shewn by the facility with which Sir C. BELL and others have found that its flow there can be arrested), strongly impress us with the belief, that, in the natural state, there must be some vital power concerned in *aiding*, not *opposing*, the impulse from the heart.

2. If the cause of all local determinations of blood be merely a relaxation of the arterial coats, then all irritations, healthy and morbid, applied at the extremities of arteries, which certainly excite such determinations, must act as *sedatives* on the contractile power of the arteries; which is not only the reverse of their action on more strictly irritable textures, but is the reverse of the effect which they have been often observed, in experiments, to produce and keep up, for a considerable time, on individual living arteries, to which they have been applied.

3. The explanation of the phenomena of such local determinations, given by this relaxation of the arteries, must in all cases be incomplete; because in different cases of the kind very different vital changes in the parts affected result,—in one, simple blushing or vascular congestion; in another, increase of secretion or nutrition; in another, serous effusion, without change of the solid textures; and in another, inflammatory effusion, with great condensation of the solids: and the same principle of simple arterial relaxation cannot embrace all these different changes.

4. In the case of Inflammation, we are certain that this principle is inadequate to the explanation, even of the changes in the motion of the blood along the vessels; because we know that there is in that case retarded movement or absolute stagnation in the vessels most affected, combined with accelerated movement and greatly increased transmission in the neighbouring vessels; and it is impossible to ascribe these two opposite changes in the movements of the blood to the same relaxation of the arterial coats.

Reflection on these difficulties naturally leads us to inquire whether there may not be some other, and more truly auxiliary cause, influencing the flow of blood in the small vessels, besides the impulse from the heart and contraction of the arteries. And this inquiry becomes more important when it is remembered, that a circulation of fluids is carried on in vegetables and the lowest animals (as already observed) without any perceptible aid from the contractions of solids; and certainly without any such peculiarity of structure as can explain the determinate direction of the movement; and that even in the lower of the animals that are provided with circulating vessels, a part of the circulation is still of the *diffused* kind, and apparently not referable to the contractions of these vessels.

Accordingly, the following facts, ascertained by microscopical observation of the capillary vessels of vertebrated animals (most of them confirmed by personal observation), seem to point out, almost with certainty, the habitual operation in this part of the animal economy of vital attractions and repulsions, distinct from any contractions of living solids.

1. The smallest arteries, wherever they are distinctly seen, appear quite motionless under the microscope, and the blood runs through them "as through tubes of glass."—(Haller.) In many cases it is, indeed, apparent that the blood moves in tracks or lines which have no perceptible coverings, and which are frequently changing; and Kaltenbrunner satisfied himself that in many parts of fishes, in the liver of reptiles, and in the spleen, even of warm-

blooded animals, the blood immediately after death quits these tracks, and is diffused through the parenchyma; which, if a correct observation, proves, both the absence of containing coats, and the vital nature of the attraction by which the globules are kept together.*

2. The blood passing through these motionless tubes or tracks, has sometimes been seen to continue its motion, more or less regularly, even in warm-blooded animals, and still more in cold-blooded, for many minutes after the heart has ceased to act, or the great vessels been tied; and yet, by certain applications to the vessels themselves, the motion at the part touched may be almost instantly stopped;† and, by certain injuries of the nervous system, much of the capillary circulation may be completely arrested;‡—the actions of the heart, in both these last cases, continuing unabated.

3. According to GUILLOT, within a short time after the blood, in such minute vessels, has come to a stand in an amputated limb, its motion may be restored by the application of heat, and go on for ten or fifteen minutes, regularly and forcibly.§

4. The *velocity* of the blood in these capillary vessels is subject to great and sudden variations; it is sometimes greater in a part of one of them than in other parts of the same, and often greater in one than in others, or *in the trunk, whence several arise*, which could not happen, in vessels the joint area of which increases as they subdivide, if the only cause of motion were an impulse *a tergo*;‖ and two minute currents may often be seen to meet and unite into a single stream, at right angles to their former course,

* Journal de Physiologie, t. viii. p. 85.

† WILSON PHILIP on Sleep and Death, p. 72. LEURET, in Journal des Progrès des Sciences Medicales, t. v.

‡ MARSHALL HALL on the Circulation, p. 123.

§ Journal de Physiologie, t. xi. p. 170.

‖ HALLER, Mem. sur le Mouvement du Sang, p. 56; and Exp. 62, 68, 72, 93, 262, &c. DOLLINGER, in Journal des Progrès, t. ix. p. 8. WILSON PHILIP on Sleep and Death, p. 71.

without retardation of their velocity, which appears also inexplicable on that supposition.

5. The *direction* of the blood in these vessels is also very liable to variation, especially after the influence of the heart on the part inspected has been in any way cut off; and it often exhibits irregularities not explicable by any conceivable changes in the pressure of the larger trunks.*

6. Long continued and regular *oscillations* of the globules of the blood in the minute vessels are often observed, particularly in animals with languid circulation, or even after apparent death; and as these are often seen in capillaries, when there is no motion in the larger vessels supplying them, or in lines of globules that have no communication with larger vessels, and as they often extend only to a part of these lines of globules, it appears certain that no contractions of vessels are concerned in producing them.†

7. The *derivation* of the blood, in all directions, towards a wounded vessel—certainly independent of the heart's action, because most remarkably observed after the heart has been cut out of the body—appears also to be frequently independent of contractions of vessels, for it takes place along vessels which do not appear filled with blood, and which do not contract during it, and has the effect of gradually filling vessels which had become empty, and of resolving coagula which had begun to form.‡

8. Globules of blood that have escaped from vessels and been effused on membranes, have been often observed to follow one another in determinate directions, even against the influence of gravity, for some time.§

* Haller, Mem. &c. p. 90; and Exp. 54, 93, 228, &c. Marshall Hall on the Circulation, p. 87. Kaltenbrunner, Exp. circa Statum Sang. in Inflamm. p. 5.

† Haller, Mem. &c. p. 88; and Exp. 225, 229, &c. Kaltenbrunner in Journal de Physiol. t. viii. p. 83–89. Dollinger in Journal des Progrès, t. ix. p. 22, *et seq.*

‡ Haller, Mem. &c. Exp. 179, 180, 182, 183, 224, 225, and p. 336. Andral. Precis d'Anat. Pathol. t. i. p. 27.

§ Haller, Mem. &c. Exp. 208, 214–16, 225, &c. Kaltenbrunner, Exp. &c. p. 66, 85.

9. When lymph has been effused from injured or inflamed vessels, files of globules of blood often gradually make their way through it, and the tracks which they follow become capillary vessels, nearly in the same way as the first globules of the blood arrange themselves on the germinal membrane of the chick in ovo, before the heart acts or the bloodvessels are formed.*

Even if it be granted that several of these observations may have been fallacious, there will remain enough uncontested to establish a proposition which is so strongly supported by the analogy of the lower animals, and the application of which to the difficulty above stated, as to the local determinations of blood in the higher animals, is so obvious and satisfactory.

The contracted condition of the arteries of a stump, or of the main artery of a limb which has been tied, when contrasted with the distended state of the collateral branches in the latter case (see p. 30), seems also inexplicable, if the blood, at any part of the arteries, is supposed to be moved only by a *vis a tergo*. And the microscopical observations of HALLER, and of KALTENBRUNNER, on the manner in which the blood deserts the branch of a vessel, so obstructed—*licet lumine hiet aperto*—and passes into anastomosing branches, clearly indicates the efficacy of some attracting power, modifying the effect of the *vis a tergo*.†

This would be strongly confirmed if it should be found that a fœtus, however imperfect, could exist without a heart, and alone in the womb. Such cases are stated to have occurred, but have not been reported with sufficient care to entitle us to rely on them.‡

That the peculiar chemical changes, which are wrought on the blood in all parts of the body, should be, to a certain

* HALLER, Mem. &c. Exp. 180. KALTENBRUNNER, Exp. &c. p. 22, *et seq.* GENDRIN, Hist. des Inflammations, § 1566, *et seq.* ROYER COLLARD, Essai d'une Système de Zoonomie, p. 80.

† HALLER, Mem., &c. Exp. 54. KALTENBRUNNER, l. c. § 20, 21.

‡ "Cohæret in *vix non omnibus* talibus monstris funiculus cum placenta infantis gemelli normalis."—ELBIN, *De Acephalis, &c.* p. 116.

degree, a cause of its movement (as stated at p. 33), is only what might be expected from facts ascertained by Reuss, Du Trochet, and others, as to motions produced in inanimate fluids by their chemical and electrical relations. And that the chemical changes on the blood at the lungs, or its reception of oxygen there, are really, as stated in the text, a powerful auxiliary cause of its motion through the lungs, will appear distinctly from the facts to be stated as to death by Asphyxia.

In so far as the movements of fluids through living bodies are determined by the chemical changes which they there undergo, they may be supposed to bear the same relation to the vital affinities, by which these changes are effected, as the phenomena of Endosmose and Exosmose bear to ordinary chemical attractions. And as the vital affinities obviously act with greater energy in individual parts of the body at some times than at others (*e. g.* at the lungs during inspiration, or at the stomach during digestion), we can understand how local determination of blood should be produced (by attraction rather than propulsion) by causes exciting the vital actions at the ends of the arteries. The increase of nutrition, secretion, or excretion, is in such cases, at least in the first instance, the cause, not the effect, of the increased flow of blood to the parts concerned; just as the excitement of vital action in a branch of a tree exclusively exposed to the sun is the cause, not the effect, of an exclusively increased flow of sap into it.

But some of the microscopical phenomena above noticed (as the oscillations, the derivation of blood to wounded vessels, the adhesion of globules to one another, &c.) seem to be unconnected with any chemical changes; and these have been ascribed, with some probability, to the existence, in the particles of the blood, of a certain degree of the same attractions and repulsions, and the same liability to the action of stimuli, as are found in the particles of the same matter constituting muscular fibres.

It is to be remembered, however, that although the powers of which we have now treated, appear to be of great ef-

ficacy in promoting the flow, and regulating the distribution of the blood in the capillaries, and therefore of great importance in Pathology, yet the experiments of MAGENDIE and of POISEUILLE (quoted at p. 27 of the text) shew that, in the healthy state, they are inadequate to the propulsion of masses of blood along the larger veins.

VI. OF THE COMPOSITION AND ORGANIC STRUCTURE OF THE BLOOD. P. 40, *et seq.*

THE question stated at p. 40 and 41, Whether, in the living body, the fibrin of the blood exists in the globules, as stated by PREVOST and DUMAS, DENIS, and other foreign writers, or is held in solution in the serum, as has been usually taught in this country, is now decided in favour of this last opinion, not merely by the observation of Dr BABINGTON, stated in the text (which is hardly decisive), but by the following facts.

1. It is stated by HALLER and others, that in cold-blooded animals, wounds of the small bloodvessels may be distinctly seen, under the microscope, to be closed up by a gelatinous matter (brouillard), which separates from the fluid part of the blood, and is obviously distinct from the globules.*

2. It has been found by MULLER, that the globules of blood just drawn from cold-blooded animals, may be caught on a fine filter unchanged; and that the fluid which passes through the filter subsequently coagulates.†

3. When the mixture of serum and colouring matter which is left after the fibrin of fresh-drawn blood has been removed by agitation, is examined with the microscope, the colouring matter is found to retain the form of globules, such as are seen in the fresh-drawn blood before it has coagulated, and these are seen to undergo the same changes

* Mem. sur le Mouvement du Sang, Exp. 157, 176, &c.; and p. 302. Also, GENDRIN, Histoire des Inflam. § 1441, 1480, *et seq.*

† Ann. des Sciences Naturelles, t. xxvii.

as the fresh globules, when mixed with water under the microscope. It is plain from these facts that the globules are passive in the act of coagulation, which depends on the consolidation of the fibrin, previously dissolved in the serum.

This peculiar constitution of the blood—a clear fluid in which many globules are suspended, and of which a portion becomes solid when it is drawn from the living body—is found in the nourishing fluid of almost all animals, and of many vegetables; and the principle which gives fluidity to the coagulating portion while it circulates in the living body, is evidently of primary importance in physiology.

It is important to observe, that in the clear fluid of the blood (or liquor sanguinis), before its separation into serum and fibrin, smaller globules, without the coloured envelope, have been discovered.*

It is highly probable, for reasons which will appear in treating of Nutrition and Secretion and of Absorption, that the blood is truly a more complex fluid than the analyses given at p. 42 and 43 indicate; and some experiments have been recorded to shew that the blood in different vessels may shew very considerable differences;† but the various compounds which are continually formed in it, must exist in so small quantity at any one time, as to be detected with difficulty; and when any complex process is performed on the constituents of the blood it is hardly possible to judge in what state the products, ultimately obtained, had pre-existed in the blood. This observation may be applied to some of the analyses of Denis, and to the three proximate principles, one thought to be identical with cholesterine, one called seroline, and the other a soap, obtained by Boudet from the substances previously described as the oil and the muco-extractive matter of the blood.‡

* See Muller in Ann. des Sciences Nat. 1834, t. i. p. 345.

† Stoker, in Trans. of Coll. of Physicians of Dublin, vol. i. p. 163.

‡ Journal de Pharmacie, 1833; and Edin. Med. Journal, vol. xli.

The proportions of the solid matter of the crassamentum and of that of the serum, to the whole blood, appear from the inquiries of DENIS, to vary remarkably in different individuals, the former in particular being greater in sanguine and robust than in phlegmatic and weakly persons; and greater in men than women; and a similar difference is found between the more active and more torpid states of animals, insomuch that in frogs the blood does not coagulate during winter.*

VII. OF THE COAGULATION AND VITAL PROPERTIES OF THE BLOOD. P. 45, *et seq.*

IT is still doubtful whether the irregular intestine movements in the particles of fresh drawn blood, mentioned at page 48, are truly vital motions; but there is now conclusive evidence, 1. That the fluidity of the blood in the living body is a vital phenomenon, and its ordinary coagulation not referable to any chemical or physical principle, but dependent on the loss of vitality; and 2. That although the fibrin is probably always deposited from the serum when blood is separated from the living body, yet the aggregation and contraction of the fibrin is a vital process, liable to alteration by causes which affect vital action, but cannot otherwise alter the constitution of the blood. This evidence may be stated thus:

I. Coagulation cannot be owing merely to Rest, because it cannot be prevented by agitation of the blood out of the body,† (although the coagulum formed, if the agitation be violent and continued, is diffused in minute particles through the fluid); nor can it be owing to Cold, for it is promoted by the temperature of 100°, out of the body; nor to exposure to Air, for it takes place rapidly in vacuo, or in a shut sac in the dead body; nor to escape of Carbonic

* MULLER, Ann. des Sciences Nat. t. xxvii.

† DAVY, Edin. Med. and Surg. Journ. vol. xxx.

Acid, for it can take place without perceptible evolution of gas,* and is not prevented from taking place when carbonic acid is agitated with and absorbed by the blood ;† and we can see no other physical change attending the emission of blood from the body to which to ascribe this effect. These facts lead us, therefore, to believe, that the coagulation is the effect of loss of vitality. And this is confirmed by observing, that blood will remain long fluid, although at rest, if inclosed in a living receptacle, especially an artery or vein, and be still capable of coagulation if withdrawn from that receptacle.‡

II. But if ordinary coagulation were nothing more than the death of the blood, we should never see blood drawn from the body remain apparently fluid, as we often do ; and the following facts appear sufficient to confirm the doctrine of Mr HUNTER, that the aggregation of the fibrin in coagulation is a truly vital process—a last exercise of its vital powers, similar to the stiffening of muscles.

1. If this aggregation be prevented by agitation, continued for some hours after blood is withdrawn, the blood has no power to form a firm coagulum afterwards, although its chemical properties remain unchanged until it putrifies.

2. The aggregation of the fibrin of the blood is affected by heat and cold nearly as all vital actions are ; it is accelerated by moderate heat, and permanently arrested by long continued cold (as by freezing blood for thirty-six hours), and also by raising its temperature rapidly to 140° (a heat short of that which coagulates its albumen).§

3. This process is *prevented* by various causes, which powerfully depress all vital action ; as we see in cases of sudden death from concussion, suffocation, mental emotion, muscular exertion, or the most violent poisons, or most ra-

* TURNER's Chemistry, p. 747.

† DAVY, *loco citato.*

‡ THACKRAH on the Blood, ch. v.

§ PRATER, Experimental Inquiries in Chemical Physiology, p. 12, 13.

pid diseases, when the blood remains permanently fluid, the whole fibrin being diffused in minute particles through the mass.

4. Coagulation of the blood in individual parts is *produced* in certain circumstances, even of the living body, by causes affecting vitality, but by which the physical properties of the blood must be unaffected, particularly if the parts to which it is going be in a state of gangrene,—or if the vessels containing it have been severely injured, as in contused or lacerated wounds,—or if it be kept *very long at rest* in any part, as in an aneurism, or an obstructed vessel. In such cases the coagulum formed is gradually decolorized, the colouring matter being either carried off by the still liquid portions of the blood, or absorbed in preference to the fibrin.

5. The coagulation of the blood and aggregation of the fibrin are much *altered* in different diseases. In typhoid diseases the aggregation of the coagulum is much diminished; and in inflammatory diseases there are several changes; the fibrin and globules have the peculiar tendency, explained in the text (p. 47), to separate from one another; the aggregation of the fibrin becomes in general unusually great, the proportion of fibrin in the whole blood is gradually increased;* and the albumen appears to attach itself in increased quantity to the fibrin;† and these changes in the blood drawn from the veins correspond perfectly to the changes, seen with the microscope to be gradually effected, in the blood of the inflamed vessels themselves.

All these modifications of the process of coagulation are from causes which powerfully affect vital action, but cannot be supposed to alter materially the physical or chemical properties of the blood; and several of these clearly indicate that the condition of the blood during life, as it is constantly dependent on an influence somehow communicated to it

* Scudamore on the Blood, p. 89, *et seq.* Whiteley, De Sanguine; Edinburgh, 1832.

† Gendrin, Op. Cit. § 1419.

from the living solids surrounding it, so is liable to great and sudden change from *impressions* made on those living solids; which is a principle of obvious and great importance both in Physiology and Pathology.

It does not appear possible to conceive a single property in the blood capable of comprehending all the phenomena which are peculiar to its living state. It has been supposed that the principle of attraction or aggregation among the particles of fibrin, on which the coagulation depends, is opposed during life by a principle of repulsion, which acts at shorter distances, and subsists for a shorter time after the blood is drawn; that both are truly vital properties, and that this last principle of Repulsion, or Vital Elasticity, is what keeps the fibrin fluid during life.*

This supposition does not seem more complex than the facts to be included under it require; and the properties thus attributed to the blood are strikingly analogous to those which are indicated by the vital actions of muscular fibres. Accordingly, in cases of the most sudden and violent deaths, the post-mortem contractions of the muscular fibres, as well as the aggregation of the fibrin of the blood, are nearly or entirely prevented; and these processes are somewhat similarly affected by various agents.† Indeed, all the effects produced on the coagulation of blood, by various agents which do not appear to affect its chemical constitution, seem in general to be compatible at least with the supposition that the precipitation of the fibrin depends on the loss of a peculiar vital power of Repulsion, acting only at very short distances, and easily destroyed; and that the subsequent aggregation of the coagulum depends on the predominance for a time of a stronger vital power of attraction.‡

One of these is, the effect of dilution with serum (*i. e.* with a weak saline solution) in promoting coagulation.

This last fact is obviously the chief cause (as stated at p. 46) why the blood last drawn from a fainting or dying

* See PRATER's Experimental Inquiry, &c. p. 214, *et seq.*

† Ib. § 5. ‡ Ib. § 2.

animal, being more serous, coagulates more rapidly than that first drawn, and therefore seldom shews the buffy coat in cases of inflammation. But the rapid and imperfect coagulation of blood drawn in such circumstances must also be partly ascribed to the shock by which its vitality is effected, nearly as by other causes of concussion or sudden death.*

The idea above stated of the vital properties concerned in the coagulation of the blood, is strikingly consonant to this leading fact, that when coagulation is unusually rapid (all the vital powers being speedily extinguished) it is also feeble, or the aggregation of the coagulum slight; and when it is slow (the vital properties departing very gradually) it is found to be, *cæteris paribus*, strong, *i. e.* the aggregation of the coagulum firm.

According to these views, it may be supposed that the properties of Irritability and Tonicity in muscles are only an increase and more perfect exemplification of the same vital powers of Attraction and Repulsion which exist in certain of the constituents of the blood; and it may be thought that the property of irritability may resolve itself into the more general principle of Vital Attraction and Repulsion.† But this can only, at present, be regarded as a conjecture, to prompt farther inquiry. And we cannot suppose the muscular fibres to consist simply of strings of globules, as some have supposed, because in the cold-blooded animals they are of much smaller diameter.‡

The important changes (alluded to in p. 48–9) which take place sometimes in portions of the blood itself, effused from vessels in the living body, but much more generally in portions of fibrin effused from inflamed surfaces and becoming organized, may also be held as clear indications of certain vital properties in the blood, which are modified

* See PRATER's Experimental Inquiries, &c. p. 118, *et seq.*

† See BLACK on Capillary Circulation and Inflammation, p. 87; and PRATER's Experimental Inquiries, &c. p. 19 and 214.

‡ See MULLER in Ann. des Sciences et Nat. 1834, p. 353.

and exalted by disease, although little progress has as yet been made in determining the precise nature of these properties. It is certain that the particles of fibrin which thus escape from the vessels and form preternatural membranes, are held together by an aggregating force, much greater than existed among them in the fluid blood; * that globules of blood effused into the dense but soft cellular substance thus formed, follow each other in tracks which soon become vessels; and that the lymph composing that substance is liable to conversion, perhaps into globules of blood, certainly either into pus, or other morbid products, in the event of disease continuing,—or into natural textures similar to the adjoining parts when health is restored; all which phenomena come clearly under the definition of Vital Action.†

The same observation may be applied to the masses of fibrin so frequently found in the heart and large vessels after death, which, when completely decolorized, have probably been gradually formed before life was extinct, and have often been found to exhibit variety of structure, and even similarity to the products of disease in other parts of the body;‡ and perhaps the formation of phlebolites in veins is by a process of the same nature.§

The part of the blood chiefly concerned in, and probably the only part essential to nutrition, is the Liquor Sanguinis; and of the use of the globules of the blood (which do not generally or necessarily leave the circulating vessels) we are quite ignorant.

* See HUNTER on the Blood, vol. ii. p. 61.

† See particularly GENDRIN, Hist. des Inflamm. &c. § 1566, *et seq.*; KALTENBRUNNER, Exp. circa Statum Sang. &c. p. 22, *et seq.*

‡ See *e. g.* ANDRAL and LOBSTEIN; and GRAVES, in Dublin Journal, 1835.

§ See Dr J. REID in Edin. Med. Journ. 1835.

VIII. OF NUTRITION AND SECRETION IN GENERAL, P. 49, *et seq.*

THE opinion stated at page 51 as doubtful, that these processes consist essentially in the evolution or separation, rather than the formation, out of the blood, of different products in different textures or organs, seems to derive support from two classes of observations lately made.

1. From the minute anatomical investigations of PROCHASCA,* DOLLINGER,† BRESCHET,‡ and others, and from the elaborate and satisfactory work of MULLER, "De Glandularum Secernentium Penitiori Structura," confirming the statement in the text (p. 51), that the capillary arteries in all textures terminate in veins, and allow of escape of any part of their contents only by transudation, so that the apparatus employed for nutrition and secretion in all parts of the body is essentially the same; and the process appears to be nothing more than the exudation of one set of particles in one place and others in other places, from the mass of circulating blood, or, as DU TROCHET expresses it, a Chemical Filtration.§

2. From the different chemical inquiries, since PREVOST and DUMAS, from which it has appeared that a greater va-

* Disquis. Anat. Physiol.

† De Vasis Sanguif. quæ villis intest. insunt.

‡ Recherches sur les Appareils tegumentaires, &c. Ann. des Sciences Nat. 1834, t. ii.

§ "Sanguis in omnibus organis per minima vasculorum sanguiferorum retia ex arteriis in venas transit. Quæ retia undique clausa sunt, solasque in advehentes arteriolas et venas revehentes aperiuntur." "Tenuissimi sanguinis in vasculis illis reticulatis rivuli vix spissioribus substantiæ limitibus tanquam parietibus continentur; membranæ enim propriæ hic nondum adsunt vasculis. Oriuntur iterum iterumque novi per substantiam rivuli, &c." "Substantiam ad rivulorum limites, parietum adinstar, spissiorum adesse, hoc equidem non videmus, sed licet suspicari. Verum ex spissori solummodo substantiæ limite parietes hice, nec vero membranis constare possunt, tamque parva intercedit inter rivulum continentemque substantiam differentia, ut substantia liberum cum sanguinis rivulis ineat commercium." MULLER, Op. Cit. lib. 15.

riety of products, than were formerly supposed, can be at least easily procured from the blood; and particularly from the observation as to urea, that it cannot be detected in the blood until its excretion by the kidneys is stopped, and then uniformly appears; from which it seems fair to infer, that other substances habitually deposited from the blood may really exist in it, but in so small quantity at any one time as to shew themselves only when their deposition is arrested.

The force of the objection, stated at page 53, to the doctrine of pre-existence in the blood of all the matters deposited from it, drawn from the nourishment of various textures by imbibition from a central cavity in the lowest animals, is in a great measure done away by these considerations, 1. That more of a circulating movement of fluids in these animals than was formerly supposed has now been ascertained; 2. That in those zoophyta where there is really no circulation, the structure is much simpler than higher in the scale, and there is little variety in the products formed from the nourishing fluid; 3. That in the central cavity of these animals, as there is a mixture of nourishment from without with the fluids of the whole internal surface of the animals, and pretty free exposure to the air, all the conditions necessary for effecting much chemical change on the ingesta, are present in a greater degree than in any one part of the internal cavities of the higher animals; and it may be supposed, therefore, that new products are formed in that cavity before the nourishment by imbibition begins.

Even in the lowest animals, therefore, and in plants, the internal cavities containing the fluids are probably the scene, not only of movements, but of many chemical changes, gradually effected by vital affinities, on their contents. And in the higher animals there is so much provision (noticed under the head of Absorption) for the very gradual incorporation of the crude ingesta with the blood, that the very complex nature of the blood itself may be easily understood.

At all events, we have strong grounds for believing that

all the *excretions* pre-exist, at least so far elaborated, in the circulating blood; and additional cases have been recorded to shew (as stated at page 52), that, in certain circumstances, these excretions may pass off *per aliena cola.**

We know that there are cases of Jaundice in which no obstruction exists in the passages for the descent of the bile, and none seems to be secreted at the liver. It is probable that in such cases the formation of the bile is merely multiplied in the blood, from the loss of vital power in its usual outlet, as happens to the urea when the kidneys are extirpated, or in Ischuria Renalis. It would appear also that in both cases the retained excretion acts very surely as a fatal poison; and it seems therefore probable that excretions *retained* in the blood are more injurious than those *reabsorbed* into it (as in the more common case of jaundice); † and some facts known as to the function of absorption support this supposition.

IX. OF ABSORPTION. P. 60, *et seq.*

It is important to observe the different objects accomplished by the vital act of Absorption in the animal economy, viz. the reception of foreign matter to be added to the nourishing fluid, and the continual removal of parts of the solid textures or fluid secretions of the body itself, probably with a view to excretion. Both objects are accomplished in all classes of animals, but it is not certain that the latter object is accomplished in vegetables.

It is important also to observe, that nutritive absorption is carried on in the cellular vegetables at least, and in many of the lower animals,—and that in the lower animals, absorption of the solid textures also takes place with great rapidity,—without any set of vessels being appropriated for

* See *e.g.* Cases by Dr Crampton in Dublin Hospital Reports, vol. v.; and by Dr Peebles, in Edin. Med. and Surg. Journ. 1835.

† See Edin. Medico-Chir. Trans. in Edin. Journ. 1835, p. 293.

the purpose. The absorption by the motionless spongioles, at the ends of the roots of vegetables, and by their rigid heartwood, described by Du Trochet,* and the absorption of the polypi at the extremities of the branches of Sertulariæ, described by Mr Lister,† are examples, carefully observed, of both kinds of this vital action carried on in this simple manner. In neither case could any contraction of solids be perceived; and in the latter case, it seems certain that no other movement than an alteration in the general direction of the current going on in the transparent stem of the creature, attended the absorption of the living structure at its extremity. These simplest cases of very active absorption, should be kept in mind in all inquiries as to the moving powers by which this function is performed.

The structure of the lymphatic and lacteal vessels, and of the glands, in the different classes of animals, and their connexion with the veins, have been carefully investigated of late years, especially by Lauth, Fohmann, Muller, and Panizza; and the result, particularly of the observations of the last author, makes it necessary to modify somewhat the statements made at p. 61–65.‡

1. The most important result of recent observations is, that the previous representations of lacteals and lymphatics commencing by open mouths from the surface of membranes, or the interstices of fibres, or substance of textures, are probably erroneous; no such openings having been seen on the villi of the intestines by the careful inspection of Meckel or Rudolphi, Dollinger or Lauth; nor on the surface of the skin by Breschet;§ and none having appeared in the finest injections of Fohmann or Panizza,

* L'Agent du Mouvement vital devoilè, &c. p. 103.

† Phil. Trans. 1834, p. 3, and pl. 9.

‡ See Breschet's Recherches, &c. in Ann. des Sciences Naturelles, 1834, t. ii. p. 204, *et seq.*; and Dr Allen Thomson's Account of the Works of Panizza, in Edin. Med. and Surg. Journ. Oct. 1835.

§ Ann. de Sciences Naturelles, 1834, p. 208.

by which the lacteals were the most completely filled, unless rupture could be traced.*

2. The notion of MAGENDIE and others, that communications may exist between the smallest arteries and the lymphatic vessels, appears to be satisfactorily refuted; the finest injections of PANIZZA coinciding with those of MASCAGNI (quoted at p. 64) in shewing, that the smallest lymphatics are never continuous with the smallest bloodvessels, and even that they are ten times as large.

3. The opinion of many anatomists, enumerated in the text, of communications existing between the lymphatics and many of the veins, is strongly opposed by the fact, that such communications between any vessels which are certainly lymphatics, and veins, were never found by PANIZZA in any of the Mammalia, excepting only in the great veins of the neck; and although this negative fact may not seem sufficient to set aside positive observations, yet as a source of fallacy is pointed out, in the circumstance of many small veins, leading from lymphatic glands, being easily filled by injections from the lymphatics, the positive observations of former anatomists may be distrusted.

In most birds and reptiles, however, it appears that there is a uniform communication between the lymphatic vessels of the lower extremities and the great veins of the pelvis; and in the frog a similar communication below the scapulæ. Where this communication exists, the lymphatics are enlarged into vesicles, and in the reptiles (where the lymphatic vessels are more numerous and larger than in any other animals), these vesicles pulsate like hearts.†

4. In the lymphatic glands, (which are nearly confined to the Mammalia, and may be thought to answer a similar purpose as these communications with the bloodvessels in

* The observations of the late Dr GORDON on the Lymphatics of the Skin were to the same purpose: "There is no appearance of distinct origin any where among the lymphatic vessels of the true skin, and I have never seen the smallest particles of the quicksilver escape from the outer surface in the most successful injection."—*Syst. of Anat.* p. 234.

† MULLER, Phil. Trans. 1833.

the lower tribes), the observations of PANIZZA go to confirm the observation made in the text, that intermixture of the contents of the lymphatics and bloodvessels must take place by transudation, because he found that mercury injected into the lymphatics passed very generally, without violence and without apparent rupture, into the veins, although with different facility in different animals.

That Mr ABERNETHY was mistaken, however, in the supposition that large cells or sacs exist in the mesenteric glands of the whale, appears from the observations of Dr J. REID, who found them to contain no distinct cells, and to be of the same structure as in other mammalia.*

If we consider it ascertained, that lymphatics and lacteals originate in a network of vessels, and have no open mouths, we must suppose that into them, as well as into veins, the matters they absorb enter by transudation, and cannot suppose that what is usually called capillary attraction, is (as stated at p. 63) concerned in filling them.† It has been stated that they are filled by a kind of Endosmose, *i. e.* by reason of the relations of the fluids exterior to them to the fluids within them. But the rapidity of the action of absorption in the living body, its cessation in the dead body, and the *selection*, particularly in the primæ viæ, of thin fluids by the veins, of chyle by the lacteals, and the rejection of the biliary matter in the natural course of digestion by both, are surely sufficient to distinguish this vital action from the endosmose and exosmose of dead matter.

It has been lately supposed by MOJON, that fibres, both longitudinal and transverse, can be seen in the coats of lymphatics, which may be thought to act on their contents in the same way as the intestines, or the dorsal vessel of

* Edin. Journal, 1835.

† Absorption seems every where to take place, as expressed by BRESCHET, "par l'imbibition prealable du tissu;" and if there be any orifices in the sides of the smallest lymphatic vessels, "ils doivent avoir une bien grande capillarité, puisqu' ils refusent passage au mercure." (Op. cit. p. 224.)

articulated animals; but the investigations of BRESCHET * seem to have decided this point in the negative.

Now, if we find the best anatomists think that the absorbent vessels have no open mouths, and can only be filled by transudation,—if we cannot find that they have any vital power but that of tonicity, or simple contractility like the arteries, which must obstruct rather than assist the entrance of foreign matters into them,—and if we consider farther what is stated above as to absorption in the lowest animals and plants,—we can hardly hesitate as to the conclusion, that the force by which this function is commenced, and so far carried on, and always supported, does not consist in contractions of the coats of the vessels concerned in it, but is rather to be referred to the head of Vital Attraction and Repulsion. It appears from these statements, that the *organ of absorption*, equally as of nutrition or secretion, *is not a vessel, but is the membranous or parenchymatous lining of a vessel, impervious to the mercury* by which the vessel itself may be filled and distended; and when we reflect on the *activity*, and on the *selecting power* (in certain instances), of such an absorbing apparatus, we must surely admit, that a principle of motion, independent of contraction, must be concerned in the function.

Additional confirmation of the doctrine of Venous Absorption, has been lately obtained by experiments by Dr HANDYSIDE,† in which saline matters, laid on the surface of the true skin, or of granulating sores, or of the mucous membrane of the primæ viæ, appeared so exclusively in the veins, that he has been led to suppose the function of the lacteals to be confined to the absorption of chyle, and that of lymphatics to the interstitial absorption of portions of the solid textures, which are taken up to make room for fresh deposits; while fluids within the body, and soluble foreign matters introduced into it, are taken up by veins only. These experiments hardly entitle us to set

* Ann. des Sciences Naturelles, 1834, p. 236.

† See Report of British Association in Dublin Journal, Sept. 1835.

aside the positive observations quoted in the text (p. 65) as to the absorption by the lacteals of other matters besides chyle; and we do not see how the known absorption of the substance of the brain, can be effected by any lymphatics yet demonstrated. But Dr HANDYSIDE's observations may be connected with the statement of Dr PROUT,* that the process of absorption of solid textures all over the body, is probably preceded by a solution or assimilation of these by means of a fluid (probably an acid fluid) thrown out by the bloodvessels; a process similar to digestion being, as he thinks, not confined to the primæ viæ. If this be so, it becomes probable that their absorption will be by the same system of vessels as the lacteals; and this speculation appears farther important, as furnishing a probable explanation, both of the general uniformity of the contents of the lymphatics, (stated by MAGENDIE and others as evidence against their absorbing power), and also of the innocence of excretions reabsorbed into the system from their outlets, as compared with excretions retained in the blood.†

In reference to the doctrine of the very compound nature of the blood, of the amount of chemical change probably effected by the vital affinities within the circulating vessels, and the comparatively simple nature of the process of Nutrition or Secretion, it is important to observe the careful provision made by Nature, in the arrangements of the absorbent system in all the higher animals, for the very gradual intermixture and incorporation of foreign matters with the circulating blood, and for the successive rejection of portions of these foreign substances from the blood, before they are applied to the purpose of nutrition. This is obvious when we attend to the division of the contents of the stomach and intestines into those immediately excreted, those taken up by the veins, and those taken up by the lacteals,—to the passage of the contents of the vena portæ through the liver, and of those of the lacteals through the mesenteric glands and thoracic duct, (in all which situations they are

* Bridgewater Treatise, p. 515, *et seq.* † See p. 36.

gradually mixed with the blood),—and the subsequent passage of both portions through the lungs and both sides of the heart, before they are admitted into the arteries. And of the importance of these provisions we cannot doubt, when we find that, although the blood of one animal may be perfectly fitted for affording aliment to another, yet its injection, in the entire state, into the veins of that other is injurious or fatal.

X. OF GLANDS AND THEIR SECRETION. P. 83, *et seq.*

THE numerous and elaborate investigations of the German anatomists, and particularly of MULLER, made by different modes of injection, in all classes of animals, and especially at the period of the first development, and simplest structure of glands, have amply confirmed the statements in the text (p. 51 and 83), that "a membrane on which many vessels ramify, seems all that is necessary to any kind of secretion; and that the varieties of secreting organs are only contrivances for conveniently packing a large extent of such a surface in a small compass;" and again, that the smallest divisions of all the glands, which have the secreting power, seem to be similar in structure to the mucous glands, *i. e.* to consist of a pulpy vascular substance surrounding a little membranous canal.* All these canals, in the case of most of the glands, appear to have been originally shoots from the excretory ducts, which are first formed in the fœtus; the membrane lining them is always continuous with the mucous membrane lining the excretory ducts; and whether these canals are straight or contorted, whether they are merely convoluted or subdivided, whether they are lessened or enlarged in calibre at their extremities, (all which varieties are seen in

* "Gravissimæ secretiones in simplicissimis membranis parantur, ut succus gastricus in homine." (MULLER.) "Tunica non ideo mucosa est, quia cryptas mucosas continet, imo potius cryptæ sunt mucosæ, quia ex tunica mucosa constant et efflorescunt." (WEBER.)

the same glands in different animals), their terminations are always blind; they are always much larger, even at their extremities, than the bloodvessels which encompass them, though in general smaller in the higher animals than the smallest lymphatics of the glands; and the secretion appears to go on over the whole extent of the membrane lining them, nor is it possible exactly to fix the limit where the mucous secretion of the excretory duct ends, and the proper secretion of each of its branches within the body of the gland begins. Thus the theory of RUYSCH, of inosculation of the arteries entering the glands with the smallest excretory ducts, is satisfactorily refuted; and the theory of MALPIGHI, of secretion taking place into sacs or crypts within the glands, is so far modified, that the cavities into which the secretions transude, are found to be the whole extent of the branches of the ducts within the substance of the glands, not their extremities exclusively.

According to KIERNAN, the smallest lobules set close together in the substance of the liver, have each a branch of the vena cava hepatica in its centre, around which a plexus of branches of the vena portæ, hepatic artery, and hepatic duct, ramify;* but in perfect accordance with MULLER'S description, he adds, that in all parts of this plexus, the blind extremities of the biliary ducts are found encompassed with numerous minute arteries and veins.† One part of each little lobule often appears paler than another, when there is less blood than usual in the gland, but there does not appear to be (as has been often supposed) any real distinction of the substance of the gland into grey and red matter.‡

In the secreting part of the kidneys, according to MULLER, there are minute cells, distinct from the uriniferous

* Phil. Trans. 1833, p. 710.

† Phil. Trans. 1833, p. 716, and Pl. xxiii. fig. 3.

‡ "Substantia canalium, seu tela glandulosa semper aut albida est, aut albido-grisea, aut albido-lutea." (MULLER, Op. Cit. lib. ix. § 29.); and KIERNAN, l. c. p. 749.

tubes, which serve as receptacles of blood, and which are not seen in any other glands.*

XI. OF THE SKIN AND ITS EXCRETIONS. P. 93.

According to the recent and elaborate investigations of Breschet, the structure of the integuments appears to be somewhat different from what has been generally supposed, in the following particulars:

1. The rete mucosum is only the newest and softest layer of the epidermis.

2. The epidermis is not strictly extra vascular, for absorbent vessels, although no bloodvessels, may be traced even to its outer layers; and the ducts which pour out the sweat, may be traced through its substance to its surface.

3. In the substance of the skin, the parts destined for the different functions enumerated at p. 94, appear to be more distinctly separated from each other than has been hitherto supposed; the organs secreting the mucus, which afterwards hardens into the epidermis, may be distinguished from those which secrete the sweat; and the latter send out spiral ducts which penetrate the epidermis, while the former glands emit straight ducts, which terminate on the surface of the true skin. The organs forming the colouring matter of the skin may even be distinguished from either of the other sets of glands; and all these parts are easily distinguished from the nervous papillæ, which are the organ of touch.

The substance of the epidermis appears to be merely condensed mucus, and the fact stated in the text, that either long and very slow imbibition or pressure is necessary to enable the lymphatics stated to exist in it to take up any foreign substance, can be explained only by its being destitute of vitality; and this appears farther to illustrate the truly vital nature of the process, by which living textures

* Lib. x. § 48–12.

absorb into the lymphatics, similarly situated in their substance. Greater diversity of structure and function are thus attributed to the minute parts of the skin than were formerly understood, but not greater than can be easily believed; since we are assured, by the microscopic observations of EHRENBERG, that the complexity of structure bears no proportion to the size of parts in animal bodies.*

An important observation has been lately made by DONNÉ, on the opposite chemical characters and electrical agency of the secretions of the skin, and of those of the internal membranes of the body. The secretions of the skin are acid, except at a few points, and those of the internal membranes, both serous and mucous, with the exception of the stomach, are alkaline; and accordingly, galvanic currents are easily established between the skin and the internal membranes, as also between the stomach and liver during digestion. But the chemical character of all the secretions appears to be easily changed, even by slight disease.†

XII. OF THE PHYSICAL AND VITAL PROPERTIES OF NERVOUS MATTER. P. 101, *et seq.*

THE minute structure of the Nervous System has been examined by EHRENBERG, whose skill in microscopical observations is undoubted. He agrees with former observers (*e. g.* with RASPAIL) in describing the nervous substance, on the minutest examination, as fibrous; and the fibres, in most parts of the brain and cerebellum, in the nerves of the special senses, and in the sympathetic nerve and ganglia, as studded with knots or globules; the cineritious nervous matter, and the ganglia, differing from the white nervous matter in being more vascular, in the fibres being less regularly arranged, and interspersed with a granular matter not disposed in fibres.

* See Recherches sur les Appareils Tegumentaires, &c. Ann. des Sciences Naturelles, 1834, t. ii.; and especially Pl. x. fig. 36.

† Id. lib. 1834, t. i. p. 125.

But at the base of the brain, in the crura cerebri, in the cerebral motor nerves, and in the spinal nerves, especially those of motion, he states that the fibres, (although continuous with some of those already described), become larger, free from knots or globules, and distinctly tubular, containing a soft, granular, not fibrous matter. But it is not certain whether this tubular nervous matter is common to all the parts of the system known to be destined for voluntary motion, nor whether it is confined to these parts.*

On the chemical nature of the nervous matter, the most important recent statement is that of Mr Couerbe, who, besides extracting from the brain, by the action of alcohol and of ether, a quantity of cholesterine, and a greater variety of oily matters than had been formerly distinguished, asserts that the proportion of Phosphorus in the nervous matter is liable to great variety, and that on its due proportion, the fitness of that matter for the maintenance of the mental functions essentially depends.† This observation, however, has not yet been confirmed by others.

Sir Charles Bell has given a description of the ascent of the fibres of the spinal cord, through the medulla oblongata and tuber annulare into the brain and cerebellum,‡ not differing from previous descriptions (see p. 104), excepting in these particulars: 1. That he has ascertained a decussation of the posterior part of the columns descending from the crura cerebri, immediately behind the known decussation of the anterior part of these columns.§ 2. That he describes a distinct transverse plate of fibres in the tuber annulare, passing forwards from the cerebellum, and separating the anterior from the posterior parts of these columns, and another vertical plate of fibres separating the right from the left column, which becomes deficient at the decussation. 3. That he describes columns descending from the corpora quadrigemina and olivaria, on each side, as in-

* See Dublin Journal, September 1834.

† See Ann. des Sciences Naturelles, 1834, t. ii. p. 249.

‡ Phil. Trans. 1834.

§ See particularly Pl. xx.

corporating themselves with those descending from the crura cerebri at this point of their decussation; and the columns descending from the cerebellum as passing down behind all the others without decussating.

He supposes the columns descending from the crura cerebri only, to be the origins of the anterior and posterior spinal nerves. But several of these statements are at variance with those of BELLINGERI; and the great complication, and known diversity of endowment, of the nervous fibres at this part of the system, still require farther elucidation.

Sir CHARLES BELL thinks there must be an arrangement of fibres higher than the obvious decussation, to explain the crossing of the effect of an injury of one hemisphere of the brain to the opposite cerebral, as well as the opposite spinal nerves, (see p. 112). Perhaps this last fact can only be explained by supposing the true origin, even of the cerebral nerves, to be from the columns of the spinal cord, and that the fibres which descend from the brain are really distinct from any of those which constitute the spinal cord and nerves, and act on them only at the point of obvious decussation, which seems to be the idea of Mr MAYO.*

An experiment of Dr WILSON PHILIP, in which the heart of an animal in a state of stupor, but breathing regularly, was found to be quite unaffected by any irritation or section of the semilunar ganglion and adjacent plexuses,† when taken in connexion with the facts stated at p. 112 to 114, as to the impression produced on the heart's action by any extensive injuries of the spinal cord, is important, as evi-

* Medical Gazette, 1834. The description by SERRES, of the decussation as first seen in the embryo, seems to favour this supposition: "Tous les faisceaux croises ne remontent pas de la moelle epiniere, quelques-uns descendent de la moelle alongée, et croisent leur marche avec les ascendans." And again, in the human body, "le croisement est formé par deux ordres de faisceaux, les uns descendent de la moelle alongée, les autres remontent de la moelle epiniere; les faisceaux ascendans et descendans ne communiquent pas entre eux."—Anat. Comp. Du Cerveau, p. 164, 165.

† Inquiry into the Nature of Sleep and Death, p. 92.

dence that the office of that ganglion is only to transmit and combine an influence, on the muscular parts which it supplies, of impressions made, or changes taking place, in the larger masses of the nervous system; not to originate any such influence in consequence of impressions made on itself. This is in accordance with the experience of other physiologists as to the effect of injuries of the ganglia, and is consistent with the speculation hazarded at p. 277 as to their use. But the observation of BRACHET,* that the heart's actions instantly cease when the great cardiac plexus is cut, is one so liable to fallacy, from the nature of the experiment, and so much at variance with other observations on injuries of the ganglia and plexus, that the inference attempted to be deduced from it may be distrusted.

The inefficacy, on the heart's action, of injuries of the ganglia, through which an influence must nevertheless be transmitted to the heart on many occasions from the larger masses of the nervous system, ought perhaps to be connected with the fact observed by various toxicologists, and lately illustrated by Dr C. HENRY,† that poisons which act, even with extreme rapidity, when applied to various surfaces of the body,—and which have been thought therefore to act through the nervous system,—are yet ineffectual when applied directly to nerves in their course. If this does not depend merely on more rapid absorption in the former case (which seems doubtful), it would seem that there are certain changes in the nervous substance (obviously distinct in this respect from those concerned in sensation or in voluntary action), which can be communicated upwards from the extremities of nerves, or downwards from the brain or spinal cord, but which cannot be excited by impressions on nerves in their course.

The effects of the section of the par-vagum on the functions of the stomach and of the lungs, mentioned at p. 114,

* Recherches Experimentales sur les Fonctions du Système Nerveux Ganglionnaire, &c. p. 125.

† Edin. Med. Journal, Jan. 1832.

are so important in reference to the question, whether secretion, or other organic functions, are *necessarily dependent* on an influence constantly flowing from the brain and spinal cord, or whether they are only subjected to *occasional influence and control* from changes in the nervous system, that they have been carefully studied by many physiologists.

With regard to the effect of that section on the stomach, there is nothing to be added to the statements at p. 56, which seem sufficient to shew, that the inference of the absolute dependence of secretion on nerves, or on the brain, cannot be legitimately deduced from that experiment; and, in fact, the secretion of the stomach, on which digestion depends, is of so peculiar a nature, so easily influenced by sensations or other mental acts, and so dependent on the application of a certain stimulus to the stomach (probably even, like the flow of saliva in the mouth, on the excitement of certain sensations there), that injury of the sentient nerves of the stomach and lungs appears evidently to be a cause very likely to derange it.

As to the effect of section of the par vagum on the bronchiæ and lungs, and the mode in which this operation produces death, the experiments of Brachet and of Swan have clearly shewn,

1. That section of the par vagum does not kill by arresting the natural action between the air and the blood, but by preventing the access of the air to the blood.

2. That after this operation, the sensations of the bronchiæ, even on mechanical irritation, are suspended; and, as a natural consequence, expectoration is suppressed.

3. That the air which enters the cells of the lungs by inspiration after this operation, is sometimes not duly expelled by expiration, but stagnates in the lungs and distends them. (Swan.)

4. That the frothy fluid which gathers in the cells of the lungs after this operation, does not depend on the lungs being congested and condensed by the blood stagnating there, but is observed also when such condensation is pre-

vented by bloodletting; and that the formation of this fluid must be at least a part of the cause which impedes the action of the air on the blood.*

From these facts it may be inferred, at least with probability, that the operation is fatal, either, as suggested in the text, by suspending the formation of the protecting mucus, and so exciting inflammatory effusion in the bronchiæ; or by suspending the movements of the minute bronchiæ, by which the ingress and egress of air to the cells is secured, and so causing stagnation of the blood; † or in both these ways; and, on either supposition, the effect of this injury of nerves on the arterialization of the blood may be said to be *indirect*, and dependent on the failure of sensations and of movements, in which an agency of nerves is allowed by all to be essentially concerned.

That the organic functions of the body, and especially of the lungs and stomach, must be liable to much influence and control from changes in the nervous system, is naturally to be expected, when it is remembered that these functions are very much under the control of sensations and other mental acts, and that it is only through the intervention of changes in the nervous system that such mental acts can produce these or any other bodily effects. But when, from such facts, we attempt to deduce an immediate and necessary dependence of the organic life on the nervous system, we carry the inference farther than the facts warrant, and are opposed by the facts and considerations stated at p. 55.

* See Recherches Experimentales, &c. sect. ii. chap. 2.; and Essay on the Connection between the Action of the Heart and Arteries and the functions of the Nervous System, p. 124.

† The observations of Sir C. Bell as to the contraction of the whole length of the trachea during expiration, and again as to the absence of muscular fibres in the trachea and bronchiæ of birds (where the air is rather drawn through the cells of the lungs, than received into them), favour this last supposition. (Phil. Trans. 1832, p. 301.) It is also to be observed, that after this operation the natural respiratory murmur is much altered.

XIII. OF THE ANIMAL FUNCTIONS IN GENERAL.

P. 118, *et seq.*

THE statement made at page 123, that, in the vertebrated animals, the greater development of the brain proper is in general proportioned to the greater intelligence of the animal, requires some correction.

That there is no such connexion of the intelligence of the animal with the *size* of the brain, appears quite obvious on comparing the very large brain of the cetacea, and especially of the dolphin, with the very small brain of the fish, or even with the smaller brain of man himself.

And although there is no other animal in which the *number* and *depth of the convolutions* are equal to those in man, yet the intelligence of animals certainly bears no regular proportion to these circumstances in the structure of the brain; for, in these respects, the monkey is inferior to the horse, the ass, or the seal, and all are greatly superior to the beaver, and to other animals of apparently equal or superior intelligence.

The degree of divergence of the crura cerebri, and size of the third ventricle and thalami, stated by SERRES * as the most constant mark of elevation in the scale of animals, is obviously unconnected either with the size of the whole brain, or with the complexity of the convolutions.

It is probable, therefore, that the great varieties of form as well as size observed in the convolutions and other parts of the brain in the different vertebrated animals, are determined by other conditions of their existence, rather than by the degree of intelligence which they are destined to possess. But this point will probably be satisfactorily elucidated by the inquiries in which Professor TIEDEMANN is now engaged.

There is still some difficulty as to the nerves and columns of the cerebro-spinal axis appropriated to voluntary motion, at least in the warm-blooded animals; because the doctrine

* Anat. Comp. du Cerveau, &c. p. 540.

of BELLINGERI, alluded to in the text (p. 127), which supposes a more minute division of the fibres constituting the spinal nerves, and of their endowments, than has been generally thought to exist,—and that the posterior roots are intended not merely for sensation, but for movements of *extension*, and the anterior for the motions of *flexion*,—is supported by two sets of facts, 1. By many pathological observations, which shew that the whole extensor muscles, and the whole flexor muscles, of the limbs, or of the lower limbs, are often simultaneously affected with spasm, especially in consequence of disease of the brain, which affords ground for thinking that they are connected at their origin; and 2. By repeated and well attested experiments, made both on warm-blooded and cold-blooded animals, in which it is stated that the power of extension in the lower limbs was lost, although that of flexion was retained, when the posterior roots of the nerves supplying them were cut; and again, that the power of extension was retained, although that of flexion was lost, when the anterior roots were cut.

These experiments coincide with those of other physiologists in shewing, that the power of sensation depends on the posterior roots of the nerves, but seem to indicate that it is not from the posterior columns, but rather from the interior grey substance of the cord, that the sensibility is derived.*

Dr MARSHALL HALL has lately described a variety of muscular movements seen on irritation of different parts of the surface of the body, after the brain, and even the medulla oblongata, had been destroyed, or separated from the spinal cord, as indications of what he calls the Reflex Function of the Spinal Cord. These movements are seen almost exclusively in cold-blooded, or in very young warm-blooded, animals; they are precisely similar to those excited by irritation of the same parts, when sensation is entire, and which we then regard as the effects of sensation, and refer to the head of Sympathetic or of Instinctive movements. Such

* See Edin. Med. and Surg. Journal, Oct. 1834, p. 404, *et seq.*

movements have been described by HALLER, WHYTT, LE GALLOIS, MAYO, and others, and are mentioned in the text (p. 129). They have generally been regarded as indications of some remains of sensibility in the animals so mutilated, and as illustrations of the remark at p. 130, that in cold-blooded and very young animals, the contents of the cranium are not so essentially necessary to sensation, and to the instinctive acts naturally linked with sensation, as in the adult warm-blooded animals. And the previous cessation of all voluntary motion, and even of the respiratory actions, in the cold-blooded, or very young warm-blooded animals, still susceptible of the movements in question, is not to be regarded as satisfactory evidence of their sensibility being extinct.

These movements are, in fact, precisely analogous to the phenomena described by FLOURENS, and admitted by CUVIER,* as proofs of the *continuance* of sensation when the brain and cerebellum have been removed, but the medulla oblongata remains; and it appears unnecessary, in consequence of the observation of such phenomena, to ascribe to the spinal cord, separated from the medulla oblongata, any powers distinct from those which are admitted to reside in it when that, its anterior termination, is entire.

XIV. OF RESPIRATION. P. 135, *et seq.*

IN regard to this, the most satisfactorily investigated of any of the functions of living animals, there are still some points doubtful, and a few on which additional information has lately been obtained.

* See Report to the Institute on the Experiments of FLOURENS, in Journal de Physiologie, t. ii.; and in the Recherches Physiologiques of FLOURENS. Dr MARSHALL HALL has himself admitted "general and instantaneous efforts or struggles" on the irritation of a sensitive nerve, in an entire animal, as "the signs of sensibility." (4th Report of British Association, p. 677.) Why is the same inference not admitted in the case before us? To urge, in reply, that in the case before us sensibility is extinct, would surely be a *petitio principii*.

The importance of the par vagum, as the chief, though not the sole medium of communication between the lungs and the sensorium, by which the effort of inspiration is excited, has been illustrated by the experiments of Dr MARSHALL HALL and Mr BROUGHTON,* in which the effort of inspiration was uniformly excited, not indeed by the usual modes of irritation, but by compression of that nerve. At the same time, the dependence of other sensations, and therefore of other sympathetic actions, on the same nerve, was shewn by the action of deglutition immediately following the effort of inspiration. These authors have not drawn this inference, but they consider "general and instantaneous struggles" on the irritation of a nerve, as evidence of *common* sensation; now, it seems at least equally reasonable to consider a particular effort of distant muscles, on peculiar irritation of a nerve, to be equally evidence of a *special* sensation, in the case where that effort is precisely such, as uniformly follows in the natural state a particular sensation felt in the part which that nerve supplies; which is exactly the case in question. And that this effort, on compression of the par vagum, is an evidence of sensation, appears farther from its being equally excited, after division of the nerve, on compression of its upper segment, as happens in regard to nerves of common sensation.†

The accuracy of the observation of MAGENDIE, as to the use of the recurrent nerves in holding open the glottis during inspiration (p. 143), has been illustrated by curious pathological observations, in which it has appeared that aneurisms or other tumors under the upper part of the sternum, by which that nerve must have been compressed, have been attended with occasional spasmodic closure of the glottis; the voluntary power of antagonising any spasmodic action of the muscles closing the glottis (and moved by the superior laryngeal nerves) having been then apparently lost.‡

The currents, formerly mentioned as visible in the fluids

* Fourth Report of British Association, p. 677. † Ib. p. 679.

‡ LEY in Medical Gazette, 1835. Edin. Medico-Chir. Trans. in Med. and Surg. Journal, 1835.

surrounding many aquatic animals, are evidently connected with their respiration; and it is a very curious observation, lately made by PURKINJE and VALENTIN, and confirmed by Dr SHARPEY, that similar currents may be seen on the mucous membrane of the air-passages in animals of all classes, if immersed in water within a very short time after death.* But as these are seen equally on the mucous membrane of the genital organs, it is probable that their immediate object is the movement and regular renewal of the mucus covering the membrane, rather than of the air passing over it. In this case, as well as in the case of the lower animals, the absolute dependence of the currents on the movements of ciliæ seen to attend them, appears to have been too hastily assumed by several authors.† In several instances at least, it seems certain that both the degree and kind of movement seen in such currents are inexplicable, by the vibrations of any ciliæ that have been described.

Several additional facts may be stated in confirmation of the opinion stated in the text (p. 149, *et seq.*), as most probable, as to the nature of the chemical change at the lungs, viz. that oxygen is absorbed directly into the blood, and carbonic acid given off entire.

1. While it has been ascertained by MARCET and MACAIRE,‡ that oxygen exists in arterial blood in greater quantity than in venous, it has also appeared from careful experiments, both by TIEDEMANN, GMELIN, and MISTERLICH, and by STROMEYER, that carbonic acid exists in greater quantity in venous blood than in arterial, because when both kinds of blood are admitted, over mercury, into vessels containing a small quantity of acetous acid, and the air surrounding the mercury exhausted, the venous blood gives off a larger quantity of gas than the arterial.§ And the absence of any appearance of carbonic acid, on the application of the air-pump in the same manner to pure blood over

* SHARPEY in Edin. New Phil. Journal, vol. xix. 1835.

† See RASPAIL, Chimie Organique, sect. 582, *et seq.*, and pl. 6, fig. 4.

‡ Mem. de la Societé de Physique, &c. de Geneve, t. v.

§ See Dublin Journal, Sept. 1834, p. 117.

mercury (which was observed by them as well as by Dr Davy and others), does not prove that carbonic acid may not exist in it, even in an uncombined state, since it has been found by Hoffman and by Williams, that blood artificially saturated with carbonic acid, is not made to part with it merely by the action of the air-pump, although it will do so on subsequent agitation with different gases.* This observation makes it unnecessary to resort to the supposition of the former authors, that the oxygen of the inspired air dislodges the carbonic acid from the blood, only by forming acetous acid at the lungs, which combines with the alkali of the blood.

2. That oxygen is really absorbed from the leaves into the nourishing juice of vegetables (instead of merely dissolving carbon from them), appears strongly indicated by two considerations; *first* that, on the latter supposition, the whole action of vegetables on the air, resolves itself into evolution of carbon on the one hand, and absorption of carbon on the other, which appears very unlikely; and *secondly* that, according to experiments by Du Trochet, air appears to be taken into the air-vessels of plants, and in its passage along them from the leaves, to be gradually deprived of its oxygen, without acquiring carbonic acid.†

3. From experiments "on low combustion" by Dr C. Williams, it appears that charcoal does not unite with oxygen, so as to form carbonic acid, below the temperature of 400°.‡

It may be added, that, according to Dr Prout's statement, the gradual formation, in the course of the circulation, of the gelatin of the animal textures, at the expense of the albumen of the blood, may be expected to be attended by an evolution of carbonic acid; while, on the other hand, the discharge of water at the lungs may be essential to the formation of the firm albumen of the blood out of the weaker and softer albumen of the chyle.§

* London Med. Journal, May 1833; and Medical Gazette, Oct. 1835.

† Ann. des Sciences Nat. t. xxv. ‡ Med. Gazette, Oct. 1835.

§ Bridgewater Treatise, p. 524.

XV. OF DEATH BY ASPHYXIA.

THE doctrine laid down in the text, as to the immediate cause of the cessation of the circulation and *organic* life in Asphyxia in the higher animals (p. 151, *et seq.*), was brought forward by Dr WILLIAMS of Liverpool,* before Dr KAY's experiments were published, although it has been more fully illustrated by the latter author. According to this doctrine, which seems well established, the circulation is arrested, because the blood, when it is no longer exposed to oxygen, soon fails to make its way through the lungs, and stagnates on the right side of the heart, even although the actions of this side continue longer than those of the left. But two questions remain, not yet so completely decided; 1. Whether the immediate cause of the cessation of sensation and of *animal* life, preceding the failure of circulation in asphyxia, is really, as stated in the text (p. 151), and by most authors since BICHAT, the influx of venous blood on the brain? and 2. Why does the blood fail to make its way through the lungs, when it no longer meets with oxygen there?

On the first question, Dr KAY's farther experiments, in his Treatise on Asphyxia (p. 193, *et seq.*), as they shew that large quantities of venous blood may be injected (slowly and gradually) into the carotid artery of an animal without affecting its sensibility, make it at least highly probable, that the sudden failure of animal life in asphyxia is owing to the rapid diminution in the *quantity* of the blood sent to the brain and nervous system, rather than to its venous *quality;* and therefore, that the animal life, as well as the organic, is extinguished by the failure of the circulation at the lungs, not by the circulation of venous blood.

As to the second question, it has been supposed by HALLER and others, that the blood stagnates in the lungs and right side of the heart, only because the mechanical actions of respiration have ceased, and the auxiliary power given

* Edin. Journal, vol. xix.

to the circulation through the lungs, by their alternate expansions and contractions, has been withdrawn. But in several experiments, made lately in Edinburgh, and reported to the British Association at Dublin, it appeared that, when an animal is confined in azote, until its breathing becomes laboured, and then, when the movements of its lungs are fuller and more forcible than natural, is killed instantaneously by concussion, the stagnation and accumulation of blood on the right side of the heart, characteristic of asphyxia, are still found.

It seems certain, therefore, that the auxiliary power to the circulation, the failure of which causes death by asphyxia, must act in the capillary vessels of the lungs themselves, and be given by the application of the oxygen to the blood there; and as it is impossible, for reasons formerly given, to conceive that this auxiliary power can be given by an excitement of the vital action in the capillaries themselves, this fact was formerly mentioned, as an indication of powers aiding the motion of the blood, but independent of contractions of the solids containing it; and is one of the strongest illustrations of the efficacy and importance of those powers.

This account of the changes in Asphyxia, is farther confirmed by the fact, that the failure of the supply of oxygen is equally fatal to the circulation in those animals, when there are no movements of the organs of respiration, which can be supposed to act as auxiliary powers; and is even fatal to the vitality in those animals which have no distinct circulation.

Agreeably to these principles, the statement in the text (p. 153), (also pretty generally credited since the time of Bichat), that venous blood admitted into the arteries is more injurious than the simple failure of the circulation, probably requires correction. It is probable that the reason why an animal, in a state of activity, is more easily killed by cutting off its supply of oxygen than one in a state of torpor, is, not that the textures of the former are more fully penetrated by venous blood, but that the vi-

tality of the former is in that degree which, according to the law formerly stated, requires a rapid supply of blood for its maintenance, while that of the latter is in the lower degree which requires a less supply, or less stimulating blood. And in fact, from Dr MARSHALL HALL's observations, it appears that, in warm-blooded hybernating animals, the circulation is never at a stand during the state of torpor, although the respiration is for a time quite suspended, and therefore, the small quantity of blood circulating must be venous.

It is important to observe also, as a confirmation of what is stated in p. 153 of the function of respiration being the intermediate link by which the circulation is put in dependence on the nervous system, that these warm-blooded, but hybernating animals, when possessing only that low degree of vitality which may be kept up for a time without respiration, are also in that state in which their circulation is independent of the nervous system; and it has been seen by Dr M. HALL to go on for nine hours after the gradual but complete destruction of the brain and spinal cord.*

The whole doctrine of the fatal changes in Asphyxia, is confirmed in the most satisfactory manner by the peculiarities of structure observed in warm-blooded diving animals, chiefly the Cetacea, whereby they are enabled to bear the occasional long continued privation of air without injury to their viscera from the consequent stagnation of blood; and without suspension of the flow of blood through the nervous system, and other textures, necessary for their vital action; notwithstanding that they are not provided, as the reptiles are, with any apparatus which can serve as a reservoir of air during their submersion.

This is accomplished, 1. By their having a very large quantity of blood; 2. By the large veins of these animals being dilated into sinuses near the heart, and those leading from the liver in particular being furnished with muscular fibres, whereby the blood stagnating on the right side of the heart

* Phil. Trans. 1832.

during submersion is securely lodged, and any injury to the liver prevented; 3. By the larger arteries of these animals, and particularly those going to the nervous system, being so constructed (chiefly by subdivision, convolution, and reunion), that they must contain a much larger quantity of blood, moving at a slower rate, than in other animals. They must therefore serve as a reservoir of arterial blood; and when the supply of such blood to them from the heart and lungs is suspended, their tonicity must necessarily force forward that which they already contain, so as to keep up the vital action in the nervous system, and in other organs, for a certain length of time.*

XVI. OF THE CONNECTION OF RESPIRATION WITH ANIMAL HEAT AND WITH MUSCULAR CONTRACTION. P. 154.

An additional fact, in proof of the connection of animal heat with the formation of that carbonic acid which is given off in respiration, is furnished by the observation of Dr Davy on the unusually high temperature of certain fishes, and at the same time on the peculiar structure of their gills, which shews that the action between their blood and the air must be unusually rapid.†

The expression of Cuvier, that the irritability of muscles, in different animals, is directly proportioned to the quantity of air they consume, has been shewn by Dr Marshall Hall‡ not to be strictly correct, the irritability of muscles being not only longer retained, but likewise more easily excited in cold-blooded than warm-blooded animals. But as the strength of muscular contraction, and the activity of all the vital actions dependent on that cause, are greatest in

* See Sharpey in Fourth Report of British Association, p. 682; Houston in Transactions of British Association, Dublin Journal, Sept. 1835; Kiernan in Phil. Trans. 1833, p. 738; and particularly Breschet in Ann. des Sciences Naturelles, 1834, t. ii, p. 376.

† Phil. Trans. 1835. ‡ Ib. 1832.

the animals that consume the greatest quantity of air, particularly in birds, it does not seem consonant to the usual language of physiologists, to use the general expression of Dr HALL, that the irritability of muscular fibres in animals is inversely as the quantity of their respiration. We should only say, that it is more enduring, and more readily manifested on irritation, as the quantity of air they consume, and the activity of muscular action of which they are capable, is less.

XVII. OF DIGESTION. P. 159, *et seq.*

THE varieties of this function in the different classes of animals, from its simplest form in polypi, where it seems to consist merely in the solution and immediate appropriation of part of the aliments, and immediate rejection of others, and where it may be performed by any of the living surfaces of the animal,* up to its most complex form in the warm-blooded animals, are daily more and more illustrated; but the most important additions to our information as to the more complex forms of digestion seem to be the following.

The observations of Dr BEAUMONT, in America, made in a case where perforation of the stomach from a wound which had healed allowed the interior of the stomach to be inspected, amply confirm the statements of the best recent authors (p. 170, *et seq.*) as to the erection of the villi after food has been taken,—as to the flow from them of an acid liquor at that time,—as to the diminution of this flow and prolongation of the act of digestion when the nervous system is strongly affected, whether by physical or mental causes,—and as to the effect of this acid liquor in dissolving aliments at the temperature of 100°, out of the body, although more slowly than in the stomach itself.†

* See ROGET's Bridgewater Treatise, vol. ii. p. 75, *et seq.*; also LISTER, Phil. Trans. 1834.

† Experiments and Observations on the Gastric Juice, &c., or JOHNSON's Medico-Chir. Review, Jan. 1835.

The accuracy of the observation of a second formation of acid, and action of it on the aliments, taking place in the cæcum (page 174), is confirmed by the observations of SCHULTZ; and the importance of this secondary or auxiliary digestion in the case of the digestion of vegetable food, is proved by him, not merely by the greater development of the cæcum, and the much stronger acidity of its contents (during digestion), in herbivorous than carnivorous animals, but also remarkably by the nearly rudimentary condition both of the cæcum and the three first stomachs of the young ruminantia, during the time they live on milk, and the development of all these when they begin to live on vegetable food.

As the acid formed in the cæcum, as well as that formed in the stomach, is neutralized by the bile, the cæcal digestion can only go on well during the intervals of the gastric; and this circumstance, in animals where the cæcum is large, and even to a certain degree in man, regulates the time when food is advantageously taken.

But as the bile is not mixed with the aliments in the stomach until their formation into chyme by the acid gastric juice is completed, so it would appear, from some observations of Dr SCHULTZ, that it is not admitted into the cæcum, when the digestion there is going on, until the acid has been thrown out and acted on the residuary aliments. During this time the cæcum is at rest, and the valve of the cæcum appears to be closed, as the pylorus is, and sometimes separates the acid contents of the cæcum from a considerable collection of bile at the end of the ileum. But when the fresh formation of chyme in the cæcum has been completed, its peristaltic motion is renewed, the bile enters, and the acidity of the contents of the great intestines gradually disappears.*

* See Edin. Med. and Surg. Journal, Oct. 1835.

XVIII. OF THE EXTERNAL SENSES. P. 179, *et seq.*

In this department of Physiology, little fresh information has been lately obtained.

The account commonly given of the dependence of the sense of Taste on the fifth nerve, and the statement in the text (p. 189), identifying much of what is usually termed Taste with the sense of Smell, would seem to be both set aside by the experiments of Panizza, in which it is alleged that section of the glossopharyngeus nerve, and of it only, seemed to destroy the sense of taste.* But as the result of experiments by Mr Mayo † was different, and as this nerve has never been described as supplying the papillæ near the tip of the tongue, we cannot rely on this observation.

On the subject of the correct vision of external objects by means of inverted images on the Retina, an observation by Mr Dick (veterinary surgeon, Edinburgh), deserves notice; viz. that the tractus opticus being curved upwards as well as outwards, those nervous fibres which lie lowest in the optic nerves, will have their roots in the highest part of the corpora quadrigemina. It is certain that the intimations as to the position of objects acquired by sight and by touch, correspond from the first; and we have perhaps a sufficient explanation of this fact, when we find that the impressions made on the *upper* part of the retina, are in fact impressions made on the *lower* part of the corpora quadrigemina, and *vice versa*. This speculation may probably be extended to other facts connected with the correspondence of visual with tactual impressions.

Several cases of Somnambulism or Extase have been lately recorded, similar to those mentioned at p. 210, in which it has been supposed that, during that state, either

* See Medical Gazette, September 1835.

† Do. October 1835; and Phys. Comm., No. 2.

transference of sensation from one organ to another has taken place, or else sensation has been felt in the usual organ, when the conditions commonly required for that purpose have been absent,—particularly vision has appeared perfect when the eyes seemed to be effectually bandaged;* and we can hardly doubt that these cases have been accurately observed, and faithfully recorded. If, indeed, all the information we obtained through the senses were either contained in our sensations, or logically deducible from the intimations of sense, or acquired by experience and association, we should think it certain—as we still think it possible—that some fallacy must exist in regard to these cases. But as we know that we ourselves, and other animals more remarkably, do obtain information as to the properties of external things, (*e. g.* as to their external existence, the nature of their primary qualities, their size and distance), which cannot be acquired in any of these ways, we must necessarily admit the principle of *intuition* as part of the cause of the knowledge we acquire by the senses; and if we admit this principle, we cannot judge how far it may extend; or whether, in any circumstances, information usually acquired only by certain senses, and under certain conditions, may be acquired by other senses, or under conditions somewhat varied.

If the description given by CUVIER† of the eyes of insects were correct,—that they consist only of a single homogeneous transparent medium, on the posterior surface of which the choroid and retina are spread,—we should be obliged to admit that these animals acquire such information, merely by the impinging of light on the retina, as we acquire only by the formation of distinct and inverted images there. But the inquiries of MULLER and of DUGES,‡

* See COLQUHOUN's Translation of Report of Committee of the Royal Academy of Medicine at Paris, on Animal Magnetism; and case of JANE RIDER, in American Journ. of Med. Science, 1834.

† Anat. Comp., Leç. xii. art. 13.

‡ See ROGET's Bridgewater Treatise, vol. ii. p. 486.

make it probable that the description of CUVIER and others was incorrect, and that the true retina in the insect is placed farther back, and has light transmitted and refracted upon it through different media, although by very minute apertures.

XIX. OF VOLUNTARY AND INSTINCTIVE MOTION.

P. 239, *et seq.*

THE strict appropriation of the nerves of the different sensations to their respective offices, the varieties even of common sensibility in different parts of the body, and the various instinctive, as well as strictly voluntary, movements connected with these different sensations, have been well illustrated by Sir C. BELL, in his Bridgewater Treatise. And many striking facts, as to the adaptation of the bones, of the muscles, and of the nerves, of different classes of animals to their respective situations, to the powers which they are to exert, and to the wants which the enjoyment of these powers (according to the general laws of all living bodies) necessarily impose on them, are stated with express reference to their final cause, both in his work and in that of Dr ROGET.*

The importance of the pharynx, and of the arches and velum pendulum of the palate, in producing definite and articulate sounds, has been also illustrated by Sir C. BELL,† as well as by the authors mentioned in the text (p. 250). And some facts collected by Dr MASON GOOD, show how much may be done by these organs of articulate voice, and by the lips, even when the tongue has been extirpated.‡

* Bridgewater Treatise, chap. 6–10. † Phil. Trans. 1832.

‡ Study of Medicine, vol. i. p. 472.

XX. OF THE USE OF THE GANGLIONIC NERVES. P. 275, *et seq.*

It having been stated, in an intelligent criticism on these Outlines, that the distinction drawn in p. 278 between the doctrine that "the ganglionic nerves *preside* over the involuntary motion and organic life," and the doctrine that these nerves "*bring the* organic functions *under the control of involuntary acts of mind*," is unintelligible, it seems worth while to add a short explanation.

The phrase to "preside over the organic functions" is vague and ambiguous, but in the minds of many who use it, clearly implies that these nerves, or an influence conveyed through them, are essential to the performance of the organic functions, against which theory various arguments were formerly stated.

On the other hand, when we say that the ganglionic nerves are intended in the natural state to bring the organs of organic life, and some of those of animal life also, under the control of involuntary acts of mind, we attribute to these nerves no agency different in kind from that which all physiologists ascribe to nerves, as the instruments of mental acts; we specify the kind of mental acts to which these nerves are subservient, (viz. sensations and emotions); and by reference to the facts stated at p. 275, *et seq.* we go so far in explaining, both the peculiarity of their distribution, and the complexity of their origin and course. Thus this doctrine is neither vague nor ambiguous, nor does it involve acquiescence in the improbable theory above stated. According to the one doctrine, the ganglionic nerves are sent to moving and secreting parts, to enable them to contract and to secrete; according to the other, they are sent thither to subject their contractions and secretions to occasional change, from certain involuntary mental acts, of which all animals are susceptible; and which are notoriously, and for useful purposes, endowed with the power of affecting the organic functions of the body, which the will cannot affect.

XXI. OF GENERATION.

MANY laborious investigations have lately been made on this subject; a considerable part of which, however, are rather anatomical than strictly physiological. The following seem the most important facts, in a physiological view, lately made known, and which evidently correct some of the statements in the Outlines, particularly at p. 294, *et seq.*

1. It appears at least probable, from several observations made by Dr LEE on the bodies of women who had died during menstruation, that, at every monthly period, an enlargement and rupture of one of the Graafian vesicles of the ovaria take place, simultaneously with an increased flow of blood to the lining membrane of the uterus; and if this be confirmed, it will follow that the changes then occurring are strikingly analogous to the first of those which take place in conception.*

2. The mode of formation of what has generally been called the Decidua Reflexa, has been much disputed, and different accounts given of it by BRESCHET, VELPEAU, GRANVILLE, and LEE. The case given by the last author seems the most decisive observation yet recorded;† and at all events, as this membrane appears to be formed from the flocculent matter thrown out into the uterus around, and under the influence of the ovum, the name Ovuline Decidua seems the most proper.

3. The strict analogy of the mode of growth of the embryo of the mammalia, before the formation of the placenta, to that of the embryo of the other vertebrated animals, and the correspondence of the umbilical vesicle, and vessels passing from it into the embryo, to the sac of the yolk and omphalo-mesenteric vessels in the chick, have been satisfactorily ascertained; the only essential difference being,

* Cyclopædia of Practical Medicine, art. Ovaria.

† Medico-Chir. Trans. vol. xviii.

that the sac of the yolk is ultimately received into the body of the chick, whereas the umbilical vesicle, as it shrinks and gives place to the umbilical vessels and placenta, recedes from the body of the embryo. Whether an allantoid membrane, continuous with the bladder, is also expanded on the surface of the ovum, for the exposure of the blood to air, as in other animals, during any part of this period, is doubtful. It is certain that, in many other respects besides the mode of its nutrition, particularly in the structure of its nervous system, and organs of circulation and respiration, the embryo of the mammalia in the early stages of gestation, is strikingly analogous to the permanent structure of the lower vertebrated animals.* But some of the analogies of structure in different classes of animals, on which much stress has been lately laid by comparative anatomists, seem to be fanciful, and some of the speculations connected with them may be distrusted.

It is here to be observed, that at this early period of the life of the embryo, it is absolutely dependent—as the chick in ovo is—on nourishment passing into the interior of the ovum; the albumen ovi, in the one case, gradually incorporating itself with the vitellus, and part of the matter secreted from the inner surface of the uterus, in the other, being gradually absorbed by the shoots of the shaggy chorion, at a time when these, according to the observations of Raspail and of Velpeau, are cellular, but not vascular.† There being no vessels provided for the commencement, nor even for some time for the support, of these movements of fluids, on which the nourishment of the embryo is dependent, they have been ascribed to the principle of Endosmose. But they take place in circumstances where no such movements occur, unless the embryo be alive; and therefore they ought to be referred to the head of Vital Attractions and Repulsions. Nor is there any other moving power to which we can ascribe the formation either of the blood or

* Tiedemann, Serres, Allen Thomson, &c.

† Embryologie Humaine, &c. p. 14.

bloodvessels, or of any parts of the embryo, on the germinal membrane.*

After the umbilical vesicle and omphalo-mesenteric vessels have shrunk, and the umbilical arteries and vein have appeared, and the shoots of the chorion, where they are in contact with the uterus, have thus become vascular, it seems now well ascertained that the vascular structure of the placenta is formed entirely from these fœtal vessels; and although the maternal blood is received into cells in the placenta, it is not poured into these cells by any considerable vessels passing across from the uterus, but exudes into them, at least for the most part, from the semilunar openings in the uterine sinuses, and stands in the same relation to the fœtal blood circulating in the placenta, as the air in the cells of the lungs to the blood of the pulmonary artery.†

In the case of many lower animals, as the mare and the cow, there appears to be no passage of blood from the uterus to the placenta, but vascular processes from the two surfaces are interwoven, though nowhere incorporated, with one another; and the nourishment must be effected by transudation from the one set of vessels to the other.

The placenta is in no case an organ for the *transfusion* of blood from the mother to the fœtus; but serves as a *reservoir* of nourishment, supplied by the mother, from which the vessels of the fœtus absorb certain matters, and form fœtal blood, specifically different from that of the adult.

Here again we see a manifest and powerful determination of the maternal blood to a particular spot, caused by the vital actions of the fœtus there going on (as is clearly shewn by the case of extra-uterine conception), and maintaining a copious transudation of portions of the blood, out of the vessels, *in one direction only;* which we judge to be inexpli-

* See particularly TIEDEMANN, Traité de Physiologie, § 265, 469; also DE BAER, in BRESCHET's Prepertoire, 1828; PREVOST and DUMAS, in Ann. des Sciences Naturelles, t. 2, 3, 12; ALLEN THOMSON in Edin. Phil. Journal, 1830.

† VELPEAU, Op. Cit. p. 70; RAMSBOTHAM in Medical Gazette, Jan. 1834; RIGBY in London Medical Journal, 1835.

cable by any conceivable alteration of the *propulsion* of the blood by those vital powers, which experiments authorize us to ascribe to the vessels of the mother; and regard therefore as an indication of a vital *attraction*, acting similarly to the endosmose of dead fluids, but producing such movement as the simply physical properties of animal fluids will not explain, and therefore strictly a vital power.

5. It is well ascertained by numerous observations, confirming those of KERGERADEC and Dr KENNEDY,* that the blood flowing through the plexus of vessels at the part of the uterus to which the placenta is attached, produces a peculiar sound at that part, which may often be recognized after the fourth or even third month, and easily distinguished from the sounds of the fœtal heart, which are also in general distinctly perceptible in the later months.

6. Sir A. COOPER seems to have ascertained that a function somewhat similar to that ascribed to the liver by Drs PROUT and LEE (see p. 301), is performed in the fœtus by the thymus gland; *i. e.* the formation of a fluid quite analogous to chyle, which passes by a lymphatic branch into the jugular vein, along with the thoracic duct. From this, as well as other facts now known, it appears how different a process the assimilation of blood in the body of the fœtus is from simple transfusion.

7. There is also good reason to believe that the general law of the necessity of excretion applies to the fœtal life, at least to its later stages, from some observations by PREVOST and DUMAS on the deposition of a coloured matter on the placenta during gestation; from the quantity of bilious matter (meconium) laid up in the great intestines of the fœtus and discharged after birth; and more particularly from the observation of Dr LEE and Dr PROUT, confirming previous statements by Mr HOWSHIP and Mr BILLARD, that, in cases where the urinary passages have been obstructed in the fœtus, they have been found distended with a fluid which has shewn, on analysis, a small but clearly recognised quantity

* Dublin Hospital Reports, vol. v.

of the usual constituents of urine.* From these facts, it is obvious that one use of the liquor amnii must be to serve as a receptacle for at least this excretion from the fœtus.

XXII OF THE DIFFERENT RACES OF MANKIND.

THE interest which has been attached to observations on this subject, depends very much on their tendency to elucidate the question, whether all the varieties of the human species can be reasonably supposed to have sprung from the same origin. CUVIER, although ranking them all as of one species (because the sexual intercourse between individuals of all varieties is prolific), evidently inclined to the opinion that at least three distinct races have been created, the Caucasian, or Mesobregmate—the Ethiopian, or Stenobregmate—and the Mongolian, or Platybregmate variety.

Of the first of these, the oval-shaped head, the high features and soft smooth hair and long beard, are more distinctive marks than the colour of the skin, which is generally but not universally fair.

The second is distinguished by the narrow or compressed head, the projecting jaws, depressed nose, and woolly hair, with which the black colour of the skin is very generally combined.

The third is characterized by the breadth of the skull, and breadth between the eyes, the projection outwards of the cheek bones, and obliquity of the orbits; the hair is lank, the beard short, and the skin usually olive-coloured or tawny.

The most characteristic examples of these races, in the European, the Negro, and the Calmuck or Chinese, are strikingly contrasted; and it is certain that arts, sciences, and civilization, have made the greatest progress in the first, have hardly advanced at all in the second, and only imperfectly in the third race. But when we compare all the actual varieties of the species with these standards, we

* Medico-Chir. Trans. vol. xix. p. 238.

find great difficulty in fixing the proper place of many, and may be led ultimately to think it as probable that they are all derived from one as from three original stocks.

Dr PRICHARD * has been lately at great pains to establish, that the variety of languages bears no relation to the supposed descent of the different tribes of men from these three roots; the greatest diversity, not only as to the derivation of individual words, but as to the whole structure of language, being found in nations belonging to the same family, *e. g.* in the Calmucks and Chinese; and again, the same language, or languages nearly akin to one another, being spoken by nations evidently belonging, according to their physical characters, to different families; as in the case of the Turks of the Caucasian race, and some of the tribes of Tartars, of the Mongolian.

Perhaps, however, too much reliance has been placed on this argument. We know that a great diversity of languages may arise in the same race of men, if thinly scattered over an extensive country, as in America; and we can easily understand how the same language may gradually become common to very different races, if one should be predominant in number or power. The analogies of languages are probably more important as marking the connexions of nations in the more advanced periods of their history, than as assisting us to trace their origin.

But independently of this consideration, there are two others which are strongly adverse to the theory of the triple origin of the human race:

First, The number of races which have been observed and distinctly described is so great, that if we adopt that theory, we shall be obliged to admit that from each original stock a number of distinct races have sprung, some of them differing from one another nearly as much as the original models; at least differing so much as to make it quite possible, that whatever causes produced these diversities, might explain all the differences observed at the present day;

* See Reports of British Association, 1832, p. 529.

and some of these races seem evidently intermediate links in series, by which all races may be connected. Thus the Hottentots have been described as partaking of the character of the Ethiopian and Mongolian race, and the Copts as partaking of those of the Ethiopian and Caucasian; and the Caffres of Southern, and the Tibboos of Northern Africa, differ from the general form of the Negro nearly as much as from the colour of the European. Among the Asiatic tribes there are similar varieties; the Hindoos, although referred to the Caucasian race, differ materially from any Europeans; and even among the Europeans, decidedly of the Caucasian family, there are peculiarities long hereditary and widely extended, *e. g.* that of the dark-haired melancholic Spaniard and the white-haired Swede or Prussian (whether sanguine or phlegmatic), which it may seem nearly as difficult to reduce to a common character, as those which separate the Calmucks and the Tartars.

Secondly, We know that in all animals in a state of domestication, more nearly approaching to the condition of the human species than any other in which animals are placed, distinctions of *breeds* are gradually formed, as obvious and complete as those which characterize the races of men, and transmitted through many generations nearly unimpaired, yet certainly not originating in specific differences.

In what manner the distinctions of breeds of animals and of races of men are produced, and then transmitted, we are by no means accurately informed; but in both cases it may be confidently asserted, 1. That the varieties are so numerous and well marked, that if we suppose each to have *retained* its characters unchanged throughout all generations, we must suppose not a few, but *many* distinct creations; and 2. That if we admit that each has *changed* its character gradually in the course of ages, so as to allow of the descent of the whole from a *few* original models, these gradual changes must have gone to so great an extent, as to authorize our believing that a single pair may have given origin to the whole.