ON

THE DEPENDENCE

OF

ANIMAL MOTION

ON

THE LAW OF GRAVITY.

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PREFACE.

In the following Pages, I have collected a body of evidence, which has satisfied my own mind, that Animal Motion is due to the one Great Law of Gravity.

I give it to the Public, that they may consider, and determine on it likewise.

HENRY WIGLESWORTH.

Swansea, January 9th, 1849.

ON THE DEPENDENCE OF ANIMAL MOTION ON THE LAW OF GRAVITY.

I.

It has been customary, from the first dawn of Physiology, to ascribe the phenomena of Life to an overruling "vital force," or "vital principle." Professors have taught it; their scholars have believed it—neither have understood it. A ready answer to every curious enquirer was this undefinable and unsearchable doctrine.

A better spirit, and a better school, has of late years arisen—one not easily satisfied with a dogma. Chemistry has joined hand with Physiology; and, singly and conjointly, they have demonstrated that a large part of animal motion is due to laws which act on matter, and that the conditions being the same, the actions are precisely similar, in and out of the body. Last year was an era, for in it was published the first course of lectures "On the Physical Phenomena of Living Beings."

But much remains to be done; as yet we know no more of the cause of muscular and nervous action—the strong citadels of "vital force," than did those ancient Physiologists, who taught and believed that the arteries were filled with air.

The strict right to hold the term "vital force," has, I believe, never been disputed; but disputed it assuredly should be. We are dealing with matter at rest and in motion, and can prove that, when investigated in this state, it has always been found governed by certain fixed and unchangeable laws. Till we get opposite proof that matter may be at rest and in motion, without being obedient to these laws, we have no right to assume it. It is true that these laws are difficult of investigation, and acting under circumstances apparently different to what we find them in the material world; but it will be more logical to attribute inertia and motion, of whatever character, to them, till their untenability is distinctly proved, than to deny their application. Let animal motion, therefore, be assigned to these laws, and our investigations directed to their discovery. Analogy supports us, and we shall be free from the charge of bare assumption.

In accordance with this view, I shall, in the following enquiry into the application of the One Great Law of Gravity to the production of Animal Motion, regard the body as a mere machine, obedient, in all respects, to Material laws. We shall find it directly active, through the medium of Atmospheric Pressure; the necessary vacuum being produced by the chemical changes. I have touched upon Muscular and Nervous action, Nutrition, Absorption, Secretion, and Animal Heat; how far I have satisfactorily explained them, I leave others to determine.

Before proceeding, I am desirous of drawing attention to the following *well-established* scientific truths, as they will form the ground-work of my remarks :—

1. That carbonic acid gas is continually discharged by the living body—that this discharge is considerably increased by cold and active motion, while it is considerably diminished by warmth and comparative rest.

2. That oxygen gas is continually received by the living body, the amount in like manner varying, according to the state of rest or activity, warmth or cold.

3. That carbonic acid gas is not received in the same form in which it is discharged, but formed within the body, how or where Physiologists have not determined.

4. That the chemical constitution of carbonic acid gas is two volumes of carbon vapour united with two volumes of oxygen, which new compound, instead of forming four volumes, is, *as an essential condition of its formation*, condensed into two.

5. That pressure will produce a closer approximation of the atoms or particles of gases.

6. That the mere approximation of particles will often promote the union of elements, and the production of a substance different from either; thus, when iron is minutely divided, so as to permit the close approximation of the oxygen of the air, the union of the two takes place so

rapidly, that the iron becomes red hot, and an oxide of iron is formed. We may say that the particles are thus mechanically brought within *combining distance.*

7. That the atmosphere presses on the surface of our bodies with the same force with which it presses on all other material substances, viz., 14.7 lbs. to the square inch. That, on the other hand, the tissues of the body press in the contrary direction, or on the atmosphere, with an equal force, so that the pressure and counter-pressure are necessarily exactly equal.

8. That wherever a vacuum is formed in a cavity communicating with fluid, such fluid will rush in and fill such vacuum simultaneously with its formation.

9. That where fluid is confined within a tube, open or distensible at each end, a certain amount of pressure at one extremity will instantaneously produce an equal amount of pressure at the other.

II.

Commencing with muscular action, I shall endeavour to establish the following position:—

That all motion in muscles is due to the pressure of the atmosphere on the surface of the muscle, a vacuum having been previously formed within it by the union and condensation of gases.

It is admitted by Physiologists, that active chemical changes, for which the presence of oxygen is necessary, take place within the substance of muscles. This will materially lessen the amount of proof required to support the view which is now propounded.

Dr. Carpenter, one of our highest authorities, and to whose works, as the standard ones of the day, I shall almost exclusively refer, speaks on this subject as follows, in his "Principles of Physiology," third edition, sec. 586 :—

"The doctrine now generally accepted as a Physiological truth, that the active exercise of the contractility of muscle is attended with a *waste* or *disintegration* of its tissue, rests upon a great variety of evidence. The increase of the demand for food, occasioned by muscular activity, is an indication that the nutritive operations are excited by it; and the purpose of these can scarcely be anything else than the reparation of the loss which the muscle has sustained. Again, it has been shewn, that the presence of oxygen is essential to the development of the contractile force; and there is evidence that, in this development, a chemical change is effected in the substance of the muscle, which is of a nature destructive to its integrity as an organised tissue;—for, in the first place, the researches of Helmholtz indicate such a change from the comparative results of chemical analysis of the muscle, before and after the violent excitement of its contractility. But it is still more decidedly shewn by the increase in the excretions, which is consequent upon muscular activity; *and especially by the augmentation of the carbonic acid** *set free from the respiratory organs, and by that of the Urea set free from the kidneys.* The amount of the latter, indeed, may be regarded *cæteris paribus*—as an approximative indication of the quantity of muscular tissue which has undergone disintegration, being increased or diminished in precise proportion to the degree of exertion to which the muscular system has been subjected. *It cannot but be regarded as a probable inference from these facts, that the development of the contractile force is in some way dependent upon the chemical change which seems to be so essential a condition of it.*"

There are some facts connected with these chemical changes which require to be brought prominently forward, especially as they appear to have been overlooked by Physiologists; and in order to do this minutely, we must first examine the constitution of urea, one of the compounds just referred to, and which appears to be more intimately, even than carbonic acid, connected with muscular action.

In Dr. Golding Bird's work on Urinary Deposits, sec. 59, we find it stated that the elements of urea "are so arranged that its composition exactly resembles that of carbonate of ammonia, minus two atoms of water;" or, to speak with more precision, that two atoms of carbonate of ammonia are equivalent to one atom of urea, plus two atoms of water.

* See Appendix I.

The following formula, with a formal alteration, I extract from his work :—

	C.	N.	H.	O	Carbon.	Nitrogen.	Hydrogen.	Oxygen.
2 atoms carbonic Acid					2			4
2 atoms ammonia						2	6	
					2	2	6	4
	C.	N.	H.	O				
1 atom urea	2	2	4	2				
2 atoms water			2	2	2	2	6	4

Nor can this view be regarded as simply theoretical, for urea is converted, with great readiness, into these identical substances—carbonic acid and ammonia. If boiled with a strong acid, as Dr. Golding Bird remarks, the acid unites with the ammonia, and liberates carbonic acid; while, on the other hand, if boiled with potass, this alkali unites with the carbonic acid, liberating the ammonia.

It seems, then, extremely probable, that urea is not directly formed by muscular action, but that the first chemical change is the production of ammonia, which substance is presented to carbonic acid in such a form as to produce urea and water, rather than the usual compound carbonate of ammonia: whether this combination takes place immediately in the muscle, or whether it is deferred till a later period, is uncertain.

We have, therefore, two substances which evidently appear to be the invariable products of muscular action—carbonic acid and ammonia; both are remarkable for the fact, that the vapours of each when united are condensed into half their bulk. This peculiarity in carbonic acid has been already referred to (paragraph 4, page 3), as also has its composition. Ammonia contains more volumes of vapour, its constitution being six volumes of hydrogen, and two of nitrogen, but the condensation is in the same ratio—eight volumes becoming four.

Now, it must be especially remembered, that, according to the received opinions of chemists, it is impossible for these gases to be formed without condensation. They know of no conditions under which carbon, hydrogen, nitrogen, and oxygen, the elements of muscle, can unite, and form carbonic acid and ammonia, without occupying half the space they previously occupied. Hence it follows, that, if these substances are formed, during muscular action, as sound evidence proves them to be, a vacuum must necessarily be formed also, unless the muscle is capable of diminishing in size, and filling the space occupied by the elements before their union.

For the sake of clearness I shall, in the following pages, refer to this condition of the muscle, *i.e.*, its impending diminution in size from the union and condensation of gases, and which, I conceive, is the necessary consequence of its chemical changes, as the *vacuous state*.

Any opinion as to the exact manner in which these compounds are formed in the muscle, must be conjectural. It would appear, however, pretty certain, that the combination takes place *within* the ultimate fibres, and even within the cells, in the "striped" form of muscle; probably they are immediately eliminated, and replaced by oxygen—it may be in accordance with the law of diffusion of gases.

Considering it, therefore, as proved that a *vacuous state* is the result of the chemical changes occurring in muscular tissue, we shall have no difficulty in explaining the cause of its motion.

As already stated (paragraph 7, page 3), the atmospheric pressure on the surface of the body, and the counter pressure, exerted by the solids and fluids from within, are equal—one force balances the other. Hence it follows, that any diminution of this pressure, either from within or from without, will render these equal forces unequal. Let a vacuum or partial vacuum be made within, and the balance of pressure will be in favour of the atmosphere, which immediately becomes an active force, pressing wherever such vacuum is localised. In the case of the muscles, with which we are directly concerned, the production of a vacuum in their interior, and consequent exertion of the atmospheric force, will, as they are free to move, produce an instantaneous diminution of their size. This diminution is exhibited chiefly in the direction of their length; hence arises the shortening of muscles and the motion, which, by means of their tendons, they communicate to any part to which they are attached. This simple pressure of the atmosphere, upon a muscle in a *vacuous state*, I believe to be the correct explanation of the "vital force" of contractility.

It might appear at first sight, that this diminution of muscles by pressure, is contrary to a well-known fact, that muscles *increase* in diameter, while they *diminish* in length; but a reference to another fact will prove that it is not so. There seems good reason for believing, that a part only of the fibres of a muscle are in action at one and the same time, the rest being inactive; and that, if a muscle is kept long contracted, a continual interchange of work, as it were, is taking place between the fibres—some passing from a state of contraction to relaxation, while in others the action is reversed, viz., passing from a state of relaxation to contraction. All the inactive fibres are, by the contraction of the adjacent ones, thrown into zigzag folds, for their extremities are approximated to a degree inconsistent with the maintenance of a straight line. Hence, although the *contracted* fibres are diminished in size, these necessarily occupy more room transversely than they did before. Partially unoccupied spaces are left between them, which, when a number are added together, will increase the diameter of the muscle, and satisfactorily account for the appearances which have been observed.

The extent of muscular motion will obviously depend upon the difference between the pressure and counter-pressure, or, in other words, upon the extent and completeness of the *vacuous state.* Taking the atmospheric pressure in round numbers, as 15lb. per square inch, and a vacuum be produced, so that the muscle shall only resist this pressure with a force of 12lb., there will be a clear force of 3lb. acting on the muscle, which will enable it to produce motion to the amount of that force, say raise a weight of 3lb. If, however, the muscle resists with a force of 13lb. or 14lb., or 14½lb. (a less complete or extensive vacuum having been formed), the muscular force gained will be only as 2lb., 1lb., or ½lb., respectively. There is no doubt that the force acts with an infinite greater delicacy than this, and that the pressure of the atmosphere will produce a motion when the counter-pressure is reduced in each of the ultimate cells of the muscle to the hundredth or thousandth of a grain, or even less—far exceeding in its adjustments the most delicate balance that was ever contrived. The muscles of the larynx will furnish an instance; and I have no doubt that, if examined and measured, the size of the ultimate fibres, and of the fibrillæ, will be found much more delicate than they are in the muscles which move the limbs in order to provide for this exquisite delicate action.

III.

Having now examined the general facts which support the position laid down at the commencement of the last Section, we shall proceed, in the present, to the consideration of some additional proofs. These have been selected without much regard to connectedness, the object being to obtain those most capable of establishing the truth, that such position does not altogether depend upon the hypothesis that a vacuum is formed in the muscle, and hence that the atmosphere *must* act; and to show that other conditions of muscular action exist which seem incapable of explanation, except by atmospheric force. Each we shall allude to is at present unexplained, and as each and all can be explained by this agency, they will not only prove peculiarly valuable, but extremely interesting.

The first I shall refer to is, that there are no muscles in the body out of reach of atmospheric pressure; aud it is remarkable, that, in the one instance in which we find them placed in a nearly immoveable cavity, we also find a channel communicating with the external air. I allude to the tympanic cavity, and the Eustachian tube. At present, Physiologists admit that the use of this tube is undetermined. I quote the annexed passage from Carpenter's Principles of Physiology, sec. 564, for proof:—

"As to the objects of the Eustachian tube, opinions have been much divided. From the experiments of Muller, it appears that it does not increase the intensity of sound, but that it prevents a certain degree of dulness, which would attend it if the cavity of the tympanum were completely closed; of this dulness we are conscious when any tumefaction of the fauces causes an occlusion of the extremity of the tube. It has been supposed, that, among other uses, this canal serves for the conduction of the speaker's voice to his ears, but this is certainly not the case in any considerable degree; for, when the Eustachian tubes are obstructed by disease, the patient hears his own voice well, though other sounds are indistinct; and it is easily shown that its transmission is chiefly accomplished in other ways. The common idea is, that it serves the same purpose with the hole in an ordinary drum, the effect of which is generally supposed to be the removal of the impediment to the vibrations of the membrane that would be offered by the complete inclosure of the air within. It does not appear, however, that any such impediment is really offered; and the effect of the hole in the drum seems rather to be the communication to the ear of the auditor of the sonorous vibrations of the contained air, which are thus transmitted directly through the atmosphere, instead of being weakened by transmission through the walls of the instrument. Hence there is no real analogy in the two cases. The principal object of the Eustachian tube (which is always found where there is a tympanic cavity), seems to be the maintenance of the equilibrium between the air within the tympanum and the external air, so as to prevent inordinate tension of the membrana tympani, which would be produced by too great or too little pressure on either side; and the effect of which would be imperfection of hearing. It has also the office of conveying away mucus secreted in the cavity of the tympanum, by means of cilia vibrating on its lining membrane; and the deafness consequent on occlusion of this tube is in part explicable by the accumulation which will then take place in the tympanum."

I cannot agree with even Dr. Carpenter's view of what "the principal object of the Eustachian tube seems to be,—the maintenance of the equilibrium between the air within the tympanum and the external air, so as to prevent inordinate tension of the membrana tympani, which would be produced by too great or too little pressure on either side;" for it does not seem possible that there can be any variation in the amount of internal pressure. The only variation in the amount of external pressure will depend upon differences of temperature, but this cause cannot affect the internal cavity, as it must be pretty equably maintained at the temperature of the body, 98 or 100, consequently its pressure will always be the same. Nor does it appear likely that an orifice, as a kind of safety-valve, is required to vary the amount of air within, according to the variation of *external* pressure, allowing some to escape, when the pressure is increased, and to enter, when the pressure is diminished, so as to maintain the same relative tension of the membrane; for the membrana tympani itself will regulate this, even in a closed cavity. If the air be cold and heavy, and disposed to press the tympanic membrane, it will from its low vitality, and almost complete isolation, likewise share in the temperature contract, and prevent any encroachment upon the cavity, or increase of tension from without, while, if the air be warmer and lighter, and its pressure upon the membrane diminished, the latter will again share in the change of temperature, relax, and give a greater surface to such pressure, thus preventing any enlargement of the cavity,* or increase of the tension from within; while, if the temperature of the contained air is slightly affected by the temperature of the membrane itself, the consequent alteration in the pressure will be precisely what is required to maintain the balance of tension, viz., diminution of resistance (from contraction) when the pressure is increased, and increase of resistance (from expansion) when the pressure is diminished.

* I may remind the reader, that the surface of the tympanic membrane is not a plane. It is concave without, and convex within; doubtless depending upon the difference in the amount of pressure, occasioned by the difference in the temperature.

Hence, I think we may fairly conclude, that there is no need whatever of an opening to regulate the pressure on the tympanic membrane.

With regard to the remaining point, "the office of conveying away mucus secreted in the cavity of the tympanum," I may dismiss this by saying—what Dr. Carpenter and all Physiologists are aware of—that this pressure is subsidiary to the use of the opening; the mucus is simply secreted because there is an opening—not an opening because there is a secretion of mucus. Unless the orifice is wanted for some special purpose, the cavity would be a closed one, and mucus is never secreted in closed cavities.

I think, therefore, from the foregoing quotation, and the remarks appended to it, it will be seen that, so far as present explanations go, the Eustachian tube seems of no use whatever; and that hearing ought to be just as perfect without one as with one.

The claim, therefore, which I put in for this apparently useless little tube, can meet with no opposition. I believe that it is required to enable the atmospheric force to act *directly* on the muscles of the tympanum. It is true that these muscles might play through the medium of the tympanic membrane, but not with that precision and delicacy which seems necessary for perfect hearing, more especially as they are, to some extent, surrounded by bone.*

* In conformity with the above view, we invariably find that muscles, which are required to act with power or precision, are brought close to the surface, so that they may receive the *direct* action of this force;—the gastrocnemii, glutæi, pectoral, biceps, and muscles of the larynx, are examples.

We can easily obtain facts which can satisfactorily support this. Let the atmospheric pressure be excluded by closing the tube, and what is the result ?—a certain degree of deafness, or dulness of hearing. In the extract from Dr. Carpenter, just quoted, he speaks of "the deafness consequent on occlusion of this tube."

For the sake of precision, we may remark, that the muscles act on a chain of little bones, which play upon the nerve of hearing, on the one hand, while the vibrations of the atmosphere act upon them on the other.

It is natural to conclude that, if their action on the nerve be faulty, as it is when the Eustachian tube is closed, something must be wrong with the muscles; and if, when the connection with the atmosphere is renewed, as is the normal action of the bones, we have still more reason to conclude that some relation exists between their perfect action and a free communication with the air; in short, between the muscles and the atmosphere. And if we pursue our enquiry, and ask, what is the mechanical influence of the atmosphere ? (for this is the only influence which can be exerted by it here), we can but refer to its pressure. This is enough; and from it, as a necessary result, flows the following conclusion: —that the direct pressure of the atmosphere is required for the perfect action of the tympanic muscles.

Another reason, of a very interesting character, for assigning muscular action to atmospheric agency, is to be found in the fact, that the force of a muscle is greatest at the commencement of its action, and gradually diminishes in power with the increasing shortening.

"It has been shown, by the experiments of Schwann, that the contractile force is greatest when the muscle is most extended; so that, with the same stimulus, it can overcome a greater resistance by its contraction, when it has been previously stretched to its full length, than it can when it has been already in part shortened by the exercise of its contractile force. The power diminishes progressively with the further shortening of the muscle, until at last no further contraction can be produced by any stimulus, the extreme limit having been reached. Hence it seems as if the contractile force of muscles differs completely from other forms of attraction, as those of gravitation, electricity, &c., since it is the universal law of *their* operation, that the force *increases*, in proportion to the decrease between the squares of the distances between the attracting bodies; whilst in the case of muscle, the force *decreases*, in proportion as the distance between the attracting particles *decreases*."—Manual of Physiology, by Dr. Carpenter, sec. 370.

This is a perfect description of the mode in which a piston descends into a vacuum, under the influence of atmospheric pressure; its force is greatest at the commencement of its descent, and gradually diminishes with the diminishing distance. The facts, as they stand side by side, seem to leave us no alternative but the conclusion, that both phenomena are attributable to one and the same cause.

Another reason, also, of a very satisfactory nature, is based upon the insufficient explanation which at present obtains of the dilatation of the heart.

"The diastole of the heart, according to Cruveilhier, has the rapidity and energy of an active movement; triumphing over pressure exercised upon the organ, so that the hand closed upon it is opened with violence. This is an observation of great importance; *but of the cause to which this active dilatation is due, no definite account can be given.* It may partly be explained, perhaps, by the elasticity of the tissue interwoven with muscular fibre in the substance of the heart; and this may be the cause of the first ventricular dilatation, the second being produced by the ingress of blood, occasioned by the auricular systole. *But the dilatation of the auricles appears to be much greater than can be accounted for by any vis-a-tergo, which, as will hereafter appear, is extremely small in the venous system, or by the elasticity of its substance;* for it was observed in this case* to be so great, that the right auricle seemed ready to burst, so great was its distension, and so thin were its walls."—Principles of Physiology, by Dr. Carpenter, sec. 719.

I shall show, presently, that not only is the contraction, but also the dilatation of the heart entirely occasioned by atmospheric pressure; and requesting my readers to consider it for a moment as proved, they will readily perceive that all difficulty will at once vanish in explaining its action. It dilates with the "rapidity and energy of an active movement," because such is its real character, and in so doing it

* A case which had fallen under the notice of Professor Cruveilhier, in which the heart was exterior to the chest, having escaped from it by a perforation in the superior part of the sturum.

draws the blood after it, as water follows the piston of a pump. Instead, therefore, of the action being due to the rushing of the blood into the auricles, or the *vis-a-tergo*, as it is termed, it is just the opposite, and the rushing is due to the action of the heart. Yet it is not improbable that, at the termination of the diastole, or dilatation, the force of the blood will somewhat increase the tension, in consequence of the sudden check to its acquired velocity, and the yielding nature of the cardiac walls.

Another reason in support of these views of muscular action, will be found in the fact, that when a muscle is contracted by the exercise of the will, it continues in that position till its antagonists are called upon to relax it. I am aware that Physiologists have ascribed this condition to the continued exercise of the will; but I believe this to be erroneous, and that it is due to the more simple cause of atmospheric pressure—an explanation we may remark entirely consistent with the modes of action of this force. Whenever (under all circumstances, and in every mode of application), the balance of pressure is restored, the loss of which brought this force into action, a state of equilibrium or inertia is produced in the body, to which motion has been given, and which will thus remain till again distinctly acted upon. Thus it is in the muscles: when the *vacuous state* has been induced, and the atmosphere has produced the consequent motion, they remain quiescent or "contracted," till an opposing force is brought to bear. That the will has uo influence in maintaining this state I think may be readily proved. Let the fingers be half flexed upon the hand—say in walking, and the arm be swung gently to and fro; if attention be drawn to them, they will feel like a dead weight, and the individual will feel that he has no share whatever in producing it. In fact, he may relax all the muscles of the arm, and let it hang, as it is popularly expressed, "as if it did not belong to him," still the fingers retain their half-flexed position, till, by a distinct effort of the will, they are again straightened. The same thing happens with regard to the position of a limb—say the arm; it will not move from that position, try as you may, till a *positive* force is brought to bear. Now I maintain, that if this be due to the "continued exercise of the will," the individual has the power of arresting it; in fact, this is implied by the expression; and, further, that the limb ought to fall to his side, under the influence of gravity, when so arrested: but at the same time I maintain, that no arrest of the will, no determination to cease the flexion will succeed, without the distinct action of the antagonist muscles. Hence, the continued state of flexion seems due to something out of the individual; the present explanation is unsatisfactory, while all difficulty ceases when the condition is ascribed to atmospheric pressure.

It is a clearly ascertained fact, that, when the irritability of muscles, *e.g.*, the capability of exciting contraction in them, has departed, after death, that the muscles *tear*, with a far less weight than they were previously able to *draw*. This has been ascribed to the "life" of the tissue, and considered as proof that the contractile force is greater than can be accounted for by its "simple cohesiveness."

This is another valuable piece of evidence—it is like every other fact connected with muscular action, consistent with the explanation which is here given of it. Indeed, if this explanation be correct, the facts above mentioned could not be otherwise, for the action, instead of being due to the drawing together of the fibres, is due to their being forced together, which would necessarily diminish the strain upon their cohesion, and give them a greater force than could their "simple cohesiveness." In truth, the source of motion is something *out* of the muscle—precisely what is indicated by the facts mentioned.

The irritability of muscles, although at one time, and perhaps even now, believed to be in some way dependent upon the nerves, has nevertheless been satisfactorily proved to be an inherent property,—"a vital endowment belonging to them in virtue of their peculiar structure;" and that they are no further dependent upon nervous influence, than that it is the channel by which they are usually called into play. This is another fact, also, in beautiful harmony with the present view. It likewise asserts, that the irritability of muscles is inherent, so far as disconnection with all other forms of tissue is concerned, and explains its simple dependence upon the production of a vacuum, and the consequent play of the atmospheric force.

A very interesting corroborative fact may likewise be drawn from the *structure* of muscles; we shall, in succeeding sections, advert to this at some length, at present, I shall only allude to the peculiar form of the cells in the striped form of muscle. These are square, or nearly so; and I call them peculiar, because cells in every other part of the body are round or oval, or approaching to such a figure. Now, it is readily demonstrable, that the square form is one which will offer the least resistance to pressure from without, upon the diminution of resistance from within. A round or oval shape, on the contrary, would oppose this pressure, on the same principle that an arch will bear without yielding a greater weight than a level surface. Were the cells, therefore, of this shape, they would want that ready susceptibility to diminution in size, under increase of pressure, which is so imperatively required, and which is furnished by a deviation from the usual form, and giving them that of the square.

We will add one more reason, and that shall be drawn from the general view of the subject, from the fact, which every student of Nature will assent to, that the great Author of Nature does nothing in vain. Were *He* to endow the animal body with a power of motion, perfectly independent of the atmospheric pressure, when all the conditions necessary for the action of such pressure are occurring within, *He* would be deviating from an uniform plan, and performing an unnecessary act; for this force, uniting the extremes of power and gentleness, is at hand ready for action; it is capable of producing all muscular motion in the human body, and is already called into operation, to resist the contrary pressure exerted by the solids and fluids, to keep the heads of bones in their sockets, and for several other purposes.

It appears, therefore, by way of recapitulation, that muscular motion may be assigned to atmospheric pressure—because a *vacuous state* is produced in muscles during their action, which must necessarily call this force into activity—

because there are no muscles in the body out of reach of atmospheric pressure—because the modes of action of the two forces are identical, each producing motion, with a gradually diminishing power; and each, so soon as the balance of pressure is restored, leaving the body moved at rest;—because the cause of muscular action is evidently something *out* of the muscle, and likewise out of the body, as its action is proved to be independent of all other forms of tissue—because the figure of the muscular cells is different to what is found in all other parts of the body, such difference rendering them more susceptible to pressure upon the formation of a *vacuous state*—because it satisfactorily accounts for the actions of the heart, hitherto unexplained;—and, lastly, because the use of a separate and independent force by the Deity, when one so perfect is at hand, and already in operation, would not be in accordance with His uniform workings.

IV.

We must now turn to the consideration of the means employed to produce the *vacuous state,* in muscular fibre, and shall commence with the following position:—

That muscular fibres in the animal body are thrown into motion by the simple touch or contact of some foreign substance, which by the compression it occasions produces in them the vacuous state.

Before proceeding to examine the effects of this pressure or compression in detail, we shall, in this Section, take a general view of its character.

"All muscular fibre, which has not lost its contractility, may be made to contract by a stimulus directly applied, and this stimulus may be of different kinds;—the simplest is the contact of a solid substance;" and it appears from various facts, that the amount of such contact required to produce muscular contraction, is extremely small, especially if the substance be pointed; thus it may be excited by simply touching the fibre. It is well known, also, that the peristaltic, or worm-like action of the intestinal tube, may be produced by a single prick with a needle; and under the microscope, an almost inconceivable amount of pressure has been seen to produce these effects.

"Mr. Bowman has observed, by means of the microscope, that a minute foreign body, such as a hair or a particle of dust, when it touches a fibre, will cause a contraction, which begins at the point of contact, and is limited to its immediate vicinity, so as to show plainly that it is caused by the mechanical irritation of the particle acting directly on the fibre." —Quain's Anatomy, by Sharpey and Quain, fifth edition, p. clxxxii.

But if such minute degrees of pressure are sufficient to occasion muscular action, it becomes a matter of enquiry, how far it may be affected by temperature; for if the *interior* of the body is affected by changes in temperature, and such variations produce the same effect in it as in the material world, we shall have a most delicate test of the influence of pressure on muscles.

"Our present knowledge of the temperature of the human body, under different circumstances, is chiefly due to the investigations of Dr. F. Davy. Much additional information may be expected, however, from enquiries which are at present in progress. Dr. Davy's observations have included 114 individuals of both sexes, of different ages, and among various races, in different latitudes, and under various temperatures; the external temperature, however, was in no instance very low, and the variations were by no means extreme. The mean of the ages of all the individuals was 27 years. The following is a general statement of the results, the temperature of the body being ascertained by a thermometer placed under the tongue:—

Temp. of air	60 deg.	Average temp. of body	98.28 deg.
,, ,,	69 ,,	,, ,,	98.15 ,,
,, ,,	78 ,,	,, ,,	98.85 ,,
,, ,,	79.5,,	,, ,,	99.21 ,,
,, ,,	80 ,,	,, ,,	99.67 ,,
,, ,,	82 ,,	,, ,,	99.9 ,,
Mean of all experiments	74 ,,	Mean of all experiments..	100. ,,
Highest temp. of air ..	82 ,,	Highest temp. of body ..	102. ,,
Lowest temp. of air ..	60 ,,	Lowest temp. of body ..	96.5 ,,

From this we see that the variations noted by Dr. Davy, which were evidently in part the consequences of variations in external temperature; but which were also partly attributable to individual peculiarities, amounted to $5\frac{1}{2}$ degrees; the lower extreme would probably undergo still further depression, if the inquiries were carried on in very cold climates."—Carpenter's Principles of Physiology, sec. 886.

The conclusion which these experiments seem to furnish, must, I think, be received with some caution, for they simply *prove*, that the heat of the *surface* varies with the temperature. It appears that the thermometer was placed beneath the tongue—a position where the external air, even if the mouth were closed, would have constant access; and where the interior is less than usually protected, for the fat, an excellent non-conductor of heat, and which in all other parts is placed, more or less abundantly, beneath the skin, is here almost, or altogether wanting. It is therefore next to certain that, if the interior temperature could be satisfactorily determined, the variation would be found considerably less than the above experiments would seem to indicate.

But, although the variation may be slight in amount, it will produce effects, and these may apparently be out of all proportion to the cause. Let us examine them, and see the conditions they will induce in muscular tissue.

Cold produces contraction, or the nearer approximation of the atoms or ultimate particles of substances; and this is proved by a large amount of evidence, to obtain in the living body, as in material substances; while warmth, on the other hand, occasions an increase in the distance between such atoms or particles. Under the former condition, the resistance to pressure is diminished—under the latter, it is increased.

Apply this to the contents of a muscular fibre, upon which a certain amount of pressure is bearing. The effect of a slight diminution in temperature will be two-fold;—firstly, it will draw its particles into closer contact; and, secondly, by diminishing the resistance, it will comparatively increase the pressure, which will again produce still closer contact; and as an approximation of particles promotes chemical union (paragraph 6, page 3), it follows that the union of elements, within the fibre, will be thereby promoted. A slight elevation of temperature will likewise produce two-fold results, but they will be the opposite of the preceding;—firstly, a repulsion between the particles, which will increase the distance between them; and, secondly, an increased resistance, which will comparatively diminish the pressure, and thus tend to a still fnrther separation—a state in which the disposition to chemical union is diminished.

Hence the effect of even slight changes in the temperature of a fibre, will be tantamount to an increase or diminution of pressure upon its contents, although the amount of such variation must necessarily be trivial.

If we pursue the subject, we shall find other conditions, which vary with the temperature, promoting the same results in the animal body. Cold, here, also, gives a greater density to the air, and hence an increased pressure, while warmth diminishes the density, and with it the pressure; and as such alterations are uniform over the whole surface, the solids and fluids of the system must necessarily be brought under varying degrees of compression.

This compression, again, must be of small amount; the difference between the pressure of the air, at different degrees of temperature, is not considerable;* and as this pressure must be uniformly distributed, the amount brought to bear upon the ultimate fibre of a muscle—the five-hundredth of an inch in diameter—must be almost immeasurably small.

From the preceding considerations, we draw the conclusion that variations in the temperature of the external air, will produce slight but appreciable variations in pressure upon the muscular fibre, and this, by its action, internally as well as externally.

But although these variations in pressure are extremely small, they ought to produce an increase or decrease in muscular contraction, if it be true that small degrees of pressure are sufficient, as alleged at the commencement of this Section; and this increase or decrease should be capable of proof in two ways:—

1st. By showing an increased discharge of carbonic acid, the formation of which is *essential* to muscular action, when the temperature is diminished (or the pressure increased), and *vice versâ*.

2d. By showing an increased action in the muscles, under the same circumstances, viz., when the temperature is diminished (or the pressure increased), and *vice versâ*.

The evidence in support of the first of these points I shall draw from Dr. Carpenter's "Manual of Physiology," sec. 691;

* See Appendix II.

the three first lines are from the "Principles," by the same Author, sec. 767 :—

"The amount of carbonic acid exhaled by warm-blooded animals, is greatly *increased* by external *cold*, and *diminished* by *heat*, as is shown in the results of such experiments as the following :—Small birds and mammals were enclosed in a limited quantity of air, for the space of an hour, at ordinary temperatures; the quantity of carbonic acid they produced was noted. The experiment was then repeated, at a temperature nearly approaching that of the body; and was performed a third time, at a temperature of about 32. The results were as follows :—

	Temp. 59.68. Grammes.	Temp. 86.106. Grammes.	Temp. about 32. Grammes.
A Canary	0.250	0.129	0.325
A Turtle Dove	0.684	0.366	0.974
Two Mice	0.498	0.268	0.531
A Guinea Pig	2.080	1.453	3.006

Hence it appears that the quantity of carbonic acid exhaled between 86 and 106 (the ordinary temperature of the body), is not much more than *half* of that which is exhaled between 59 and 68 (the ordinary temperature of the air); and is only about *two-fifths* of that which is given off at 32."

The second point is proved by the effects of temperature on the tonicity of muscles, by which is meant a constant moderate contraction, and which I consider due to a gradual and continuous formation of the *vacuous state* in a very slight degree, a condition which involves the gentle and equable pressure of the atmosphere. Now, in the words of Dr. Carpenter, "this property is *very greatly affected* by temperature, being diminished by warmth, and increased by cold;" or increased with an increase of pressure, and diminished by a reversal of that state.

Thus we obtain a complete and beautiful chain of evidence: first, from observation; second, from theory: let us briefly review it. Muscular action has been observed by the microscope, and in other ways, to be aroused by the contact of bodies, which, from their minuteness, must produce an almost inconceivable amount of pressure; hence, as the body is subject to varying through slight degrees of pressure, by changes in temperature, the latter becomes a test by which the correctness of this observation may be verified; and it is found to support it; for an increase in the amount of pressure bearing on the system (occasioned by depressions of temperature), is immediately evidenced by an increase in the products or waste of muscular action; while a diminished pressure (depending in like manner upon elevations of temperature), as directly decreases such waste; while, under the like changes, the muscles themselves indicate a varying amount of action, their tonicity or slow contraction increasing in the former, and decreasing in the latter state.

V.

It follows, from the considerations of the last Section, that there must be an intense disposition in the elements of muscle to combine and form carbonic acid gas; and also, if our surmise be correct, ammonia. If the simple contact of a particle of dust, or a very trivial amount of pressure, be sufficient, as has been demonstrated, the elements must always be, as it were, upon the threshold of union, so that the most trivial approximation of their elements is sufficient to determine it. Nor is such a condition confined to the human body. We often find substances, upon the verge of changes which a slight cause decides. I will refer to one, in the words of Professor Graham:—

"In certain circumstances, liquids can be cooled down several degrees below their usual freezing point, before they begin to congeal. Thus we may succeed, by taking certain precautions, in cooling a small quantity of water, in a glass tube, so low as the temperatures 8°, or even 5°, without its freezing; that is, 24° or 27° under its proper freezing point, 32°. The water must be cooled without the slightest agitation, and no sand or angular body be in contact with it; for the instant any solid body is dropped into water, cooled below its freezing point, or a tremor is communicated to it, congelation commences."—Elements of Chemistry, second edition, p. 42.

The same thing happens with solutions of salts cooled below the point at which crystals are usually formed; a slight mechanical impression being sufficient, under such circumstances, to determine immediate crystallization.

We might find other comparisons ;—the elements of gunpowder possess an intense disposition to unite, so that a single spark is sufficient to produce, in many barrels, that sudden and abundant formation of gas which is termed an explosion. A mere touch will occasion such explosion, in compounds of a different nature; and an analogous, though not identical condition, I conceive to obtain in muscular fibre, occasioning in it the formation of the *vacuous state*, from so simple a cause as gentle pressure.

There is little doubt that certain pathological conditions have an influence on the readiness with which the *vacuous state* is produced, increasing it under some conditions, and diminishing it in others; such states are known as excessive and defective irritability.

"Inordinate readiness or quickness of contraction constitutes mobility of muscle, a slight stimulus causing it to contract. *This often co-exists with want of power or completeness in the contractions.* It is exemplified in the irritable heart, which, although acting very frequently, does not expel its contents so vigorously as in health. It is seen in the quick nervous movements of irritable persons, who are at the same time weak. The bowels shew it in that irritable looseness, formerly called lientery, in which food is quickly passed, little altered; and it is instanced in the irritable bladder, which will not hold an ounce of urine." —Dr. Williams's "Principles of Medicine," second edition, sec. 113.

It can readily be demonstrated, that this condition of excessive irritability *must* be attended with a want of power. Take the heart for example—a certain amount of tension in its fibres (as we shall see hereafter) is necessary, in the normal condition, for the production of the *vacuous state;* but if the disposition to chemical combination, which is the cause of this state, be increased, a less amount of tension, or dilatation, will be required, the vacuum will be produced before the fibres have acquired their full size in dilating; thus, it will be less than ordinary; which, by restraining the full influence of the atmospheric force, must occasion "a want of power or completeness in the contractions."

On the other hand, when the tendency to the *vacuous state* is diminished, the full amount of tension will hardly produce it, so that the heart will beat slowly, and if this condition be extreme, with diminished power; which power will be in the ratio of the *vacuous state.* This will be the condition of defective irritability.

Tetanus and Hydrophobia are diseases in which the tendency to the formation of the *abnormal vacuous state* exists in its intensest degree. It is interesting in connection with the view we advocate of the cause of muscular motion, to observe, that the slightest contact of any substance with the body, produces these terrible muscular contractions. This is especially the case in Hydrophobia, where the least impression on the skin—the touch of a feather—or impulse of the gentlest current of air, are sufficient. The pressure here is probably exerted through the nerves.

We have not space to pursue the subject; but it will be readily seen by the Physician to be of much importance in Practical Medicine; and it will also occur to him, that variations in the degree of pressure will produce the same results as a varying tendency to chemical combination.

VI.

While dwelling upon the small amount of pressure required to produce muscular action, the attention almost naturally turns to the local pressure, which is continually being exerted upon the body, and the enquiry presents itself, How does this affect it? I believe that it has no influence, and my reasons for this view are as follows:—

Wherever fluids are free to move any local pressure in one part, instead of causing any compression in such part, will only cause their removal to another. Now, putting aside the fact that the body contains a much larger proportion of fluids than it does of solids, which will necessarily produce this disposition, there seems to be sufficient reason in the circumstance that, between the muscles and the integuments, a layer of oily fluid is always placed in healthy states of the system—between and around the muscles themselves, and in every little interstice, not otherwise occupied. This fluid is confined in cells, so as to keep it always in position, and prevent its gravitating, and becoming solid at the ordinary temperature of the air forms the fat.

It would be difficult to contrive an arrangement more calculated to prevent the tissues of the body suffering from external pressure of the ordinary character. All compression must be confined to the integuments, for the cells in which this fluid is confined, having from their structure some motion upon each other (especially beneath the integuments, where they "frequently present themselves in an isolated condition, disposed among the meshes of areolar tissue);" the effect of pressure in one part, will simply produce a bulging in another, without in any way compressing the subjacent muscles.

VII.

Hitherto our attention has been confined to the act of contraction in muscles; we must now examine one equally important—the act of relaxation. I shall endeavour to establish the following position respecting it:—

That the act of relaxation in muscles is produced by the influx of fluid into the fibre, as, drawn upon by an opposing one, it gradually passes from contraction to relaxation.

At page 19, it was asserted, and proof was adduced in support of such assertion, "that when a muscle is contracted by the exercise of the will, it continues in that position till its antagonist is called upon to relax it." Now, upon consideration, it will be seen, and the fact must at once have occurred to the scientific reader, that unless such opposing and *acting* muscle were more powerful than the contracted or *inactive* one—or, in other words, the vacuum formed be more complete, it must fail in its action, as it must draw in opposition to the atmospheric pressure. Suppose, for example, the pressure on *a*, the contracted muscle, to be 3, and the pressure on *b*, the antagonist, when the vacuum is fully formed, to be 3 likewise, there will be no motion; for like two pound weights put into each scale of a balance, one will balance the other. And even if the vacuum in *b* be equal to 4, the clear effective gain which could be expended in moving *a*, would only amount to 1, or *one-fourth* of the acting power; the remaining 3, or *three-fourths*, being spent in relaxing the muscle, or restoring it to its former position.

This is a great difficulty, and unless it can be obviated, and the whole force of a fibre in motion brought to bear upon the part to be moved, it is clear that muscular action must be both wasteful and inefficient. But a remedy of the most simple, and yet effective nature, is provided, acting in accordance with the principle referred to in paragraph 8, page 4. The coat of each muscular fibre is extremely delicate, and permeable to fluids, and there is a rushing stream of blood, brought by a capillary vessel close at hand. As soon as the *contracting* fibre begins to bear with any force upon the *contracted* one, and thus to produce a partial vacuum, or rather an impending vacuum in its interior, those molecules of the stream of fluid which are capable of penetrating the pores of the muscular coat, are urged into the interior by the atmospheric force, and follow, as it were, the *contracted* fibre during its relaxation. By this simple contrivance, all wasteful expenditure of force is prevented, while the muscular fibre is recharged, so to speak, with materials for another gaseous combination, constituting the act of its Nutrition.

VIII.

In the preceding Sections, we have taken a general view of the causes of muscular contraction and relaxation, and also of the means by which muscles are thrown into motion; we are therefore in a position to examine the different forms of muscular tissue, and investigate the application of these general principles to the varying conditions in which we find it. We shall commence with the lowest form of fibre—the plain or organic—as it is found in the intestinal tube.

The following description of this tissue is extracted from the last edition of Dr. Quain's Anatomy, p. clxxii; and it is introduced for the purpose of giving its structure and arrangement, from the highest and latest authority:—

"These plain or unstriped fibres are generally of a pale colour; their figure is for the most part flattened, though sometimes it might be rather said to be prismatic, and their diameter is from 1-2000th to 1-3000th of an inch. Under the microscope they have a peculiar soft aspect, without a stronglyshaded border, and they are marked at short intervals with oblong corpuscles, or nuclei, which give them a very characteristic appearance, especially after the application of acetic acid, which renders the corpuscles much more conspicuous. The substance of the fibres is translucent, but clouded, or even finely granular; and in the latter case, the granules are sometimes arranged in longitudinal lines. Mr. Bowman considers this last-mentioned appearance as indicative of an approximation towards the structure of the striped fibre; for he has observed the granules to be about the size of the elementary particles of voluntary muscle, already described. It is doubtful whether they have a sarcolemma, or special sheath.

"The plain fibres are for the most part disposed between the coats of the membranous viscera, as the stomach, intestines, and bladder, in the parietes of the air tubes, excretory ducts of glands, and the like. They are generally collected into larger or smaller fasciculi, 'or bundles,' in which they run parallel with each other; but the fasciculi, in many cases, cross one another, and interlace. It is not known how the fibres are fixed at their extremities, and when they are disposed in a circular manner round a cavity, as in the intestine, for example; it is uncertain whether a circle is formed by two or three fibres, each shorter than the whole circumference, or whether a single fibre performs the entire circuit. And further, whether, on the latter supposition, the fibre returns again into itself like a ring, or is continued round more than once in a spiral manner."

It appears from the above extract, that these fibres in the intestine are arranged in a circular manner, around its cavity; this, however, is only one layer, termed the *circular*, from this disposition; another is found running at right angles with the former, and hence in the direction of the length of the tube, in like manner termed the *longitudinal*.

The action of these plain fibres is peculiar; we may distinguish them by saying, that all their movements are slower, and more powerful, than those of the "striped" form of muscle; and that they call adjoining fibres into action by their own action. The following is a description of their movements in the intestine, which from its worm-like nature is termed *vermiform*, or *peristaltic* :—

"If we irritate a portion of the intestinal canal, the fasciculus which is stimulated will contract less suddenly 'than a striped fibre,' but ultimately to a greater amount; its relaxation will be less speedy, *and before it takes place*, other fasciculi in the neighbourhood begin to contract; their contraction propagates itself to others, and so on. In this manner, *successive contractions and relaxations* may be produced through a considerable part of the canal; a sort of wave of contraction being transmitted in the direction of its length, and being followed by relaxation."—Manual of Physiology, by Dr. Carpenter, sec. 352.

It is important to bear in mind the mode in which contraction is induced in these fibres. As we have said, they are the lowest forms of muscular tissue, and therefore we shall be prepared to find the means used, a rude copy of that which prevails in the higher and more perfect structure. The ordinary action is perfectly independent of the nervous system; this is proved by the fact, that it will continue after all connection with that system has been destroyed. It is excited "*by the contact of the aliment, or by that of the secretions mingled with it.*"

This contact of the aliment, however, must occasion in the muscular fibre a very trivial degree of compression; for not only is it in itself of soft consistence, and hence its powers of compression low, but the whole internal portion of the tube is coated with a layer of semi-fluid mucus, resembling in all respects the white of an egg, which readily yielding to pressure, will deaden the momentum on the intestinal wall itself. The same fact will also appear, when we consider the balance which exists between the internal pressure *upon* the atmosphere and its reaction, by which the solids and fluids hang as it were in perpect equilibrium, between the internal cavity and the external air, reminding us of the susceptibility to motion of the *whole*. Indeed, could we conceive the tissues of a solid elastic character, any pressure on the interior ought to be followed by a corresponding pressure externally, instead of allowing that yielding or compression which we consider necessary for motion, to take place; but "bodies possessing imperfect elasticity, as soft substances, oppose more effectually than any others the momenta of bodies in motion, *in consequence of their yielding, in a greater or less degree, to the force of collision,* without reacting upon it;

and thus opposing to the shock of the moving body a gradual resistance, instead of a sudden one, as in the case of perfectly hard substances."—Golding Bird on Natural Philosophy, sec. 75.

Beginning, then, with the contact of the aliment upon the intestinal wall, we will trace the actions to which it gives rise. It will be sufficient, after what has preceded, to state, that where the pressure* is equal to the effect, it results in the production of the *vacuous state*. This does not seem to be produced simultaneously throughout the fibre, but by successive, yet continuous combinations, each resulting in a partial vacuum—and leading, under the atmospheric pressure, to a slow diminution of its size; ceasing only with the state of rest or inertia produced by the arrest of any further combinations, and the consequent equalization of resistance and pressure. This is the contractile motion in the plain fibres of the intestinal tube.

The movement of relaxation is the next act of *this* fibre, and we shall find the general principle holds good, that it is due to the play of fibres drawing in the opposite direction to the contracted one; thus, if we suppose a circular fibre to have been contracted, which in the intestinal canal is, as we have seen, believed to form only a portion of a circle, I believe that it would be relaxed, or drawn out to its former

* It seems almost certain, that this pressure can only be exerted by the hard undigested portions of the food, the great mass of aliment gliding through the intestine, without exciting any movement. This is likewise consistent with the belief that the peristaltic movements occur only at intervals.

length, either by the contraction of the fibre or fibres, which make up the remaining portion of the circle, or by the contraction of a longitudinal one.

Let us explain these movements fully. We will suppose that, from a state of perfect rest, the contractile motion has been produced in a circular fibre or fibres, attended, of course, with a diminution in the calibre of the tube. This will necessarily produce a tension or pressure in those fibres which draw in the opposite direction—say the longitudinal ones—sufficient, I conceive, to produce in them the *vacuous state*. This will, in turn, determiue *their* action, which is that of enlarging the intestinal diameter; and hence a force bearing upon all opposing impediments. Such an impediment is the *contracted* circular fibre, which, as a consequent of its own action, it "relaxes," or restores to its former position.

But other circular fibres, in a state of rest, must also yield to permit this increase in diameter, which again consecutively occasions a state of tension—a *vacuous state*—a contraction. The preceding series of actions is repeated, and by them the longitudinal fibre is relaxed, and thus a series of alternate contractions and relaxations, in the longitudinal and circular layers, is propagated along the intestinal tube.

IX.

Let us now turn to the other, and most perfect form of muscular fibre in the human body; its structure is peculiar, and to it we desire to call particular attention.

" The fibres, although they differ somewhat in size individually, have the same average diameter in all the voluntary muscles—namely, about 1-400th of an inch ; and this holds good, whether the muscles be coarse or fine in their obvious texture. According to Mr. Bowman, their average size is somewhat greater in the male than in the female, being in the former 1-352, and in the latter 1-454, or more than a fourth smaller. When viewed by transmitted light, with a sufficiently high power of the microscope, the fibres, which are then clear and pellucid in their aspect, appear marked with very fine dark parallel lines passing across them directly or somewhat obliquely, at exceedingly short but regular intervals. The lines, as just mentioned, are dark, and the intervals between them light; their distance apart is about 1-9400th of an inch, and they are even closer together in parts of a muscle which happen to be contracted. This cross-striped appearance, which is most beautiful and characteristic, is found in all the voluntary muscles; but it is not altogether confined to them, for it is seen in the fibres of the heart, which is a strictly involuntary organ.

" As to the structure of the fibres, it has been ascertained that each is made up of a large number of extremely fine filaments or fibrils, inclosed in a tubular sheath. The proper sheath of the fibre, which was discovered nearly about the same time and independently, by Schwann and by Bowman, has been named by the latter ' Sarcolemma.' It consists of a transparent and apparently homogenous membrane, and, being comparatively tough and elastic, will sometimes remain entire when the included fibrils are ruptured by stretching the fibre. In this way its existence may be demonstrated, and it is especially well seen in fish and other animals which have large fibres, for in such instances it is thicker and stronger. It may also be shown by immersing a fibre in water, before irritability is extinguished; the fluid is in this case first imbibed by the fibre, and then, exciting contraction, is squeezed out of its substance, when it usually collects between the fibre and its sheath, and raises the membrane into vesicles or bullæ.

"When a fibril completely insulated is highly magnified, it is seen to consist of a single row of minute particles, connected together like a string of beads. These particles (named 'sarcous elements,' by Bowman), when viewed with a magnifying power of 400 or 600, appear like little dark quadrangular and generally rectangular bodies, with bright intervals between them, as if they were connected together by some pellucid substance; but on closer examination, provided the defining power of the instrument is good, a very faint dark line or shadow will be discovered passing across the fibril in the middle of each of the bright spaces, and sometimes, also, a bright border may be perceived on either side of the fibril, so that each of the rectangular dark bodies appears then to be surrounded with a bright area, having a similar quadrangular outline, and it may therefore be inferred that the pellucid substance incloses it on all sides. In short, it would seem that the elementary particles of which the fibril is made up, are little masses of pellucid substance, presenting a rectangular outline, and appearing dark in the centre. Their appearance, indeed, suggests the notion of minute vesicular bodies or cells, cohering in a linear series, the fainter transverse mark between being the lines of junction."—Dr. Quain's Anatomy, by Sharpey and Quain, fifth edition, vol. 1, p. clxvi.

We see, therefore, that the main difference between the fibres found in the intestine, and those now described, or the "plain" and "striped" form of muscle, consists in the cavity of the former being continuous from one extremity of the fibre to the other, while the cavity of the latter is divided and subdivided into a multitude of smaller cavities or cells, like the cells of the honeycomb.

The movements of the two are widely different. We have already seen that this is of a slow character in the plain fibres; they contract slowly, and they relax slowly. Now, the action of the striped ones is precisely the reverse; they contract quickly, and quickly relax, but the contraction of each fibre is less powerful or effective than is the contraction of the plain one—a fact in beautiful accordance with the mechanical law, that speed is always gained at the expense of power.

But although the motion of each fibre of the striped kind, when compared with the motion of a plain fibre of equal size, is less powerful in its character, yet we are accustomed to regard the striped form of muscles as more powerful than the plain. This is because, in each of the muscles, the number of fibres is much greater than in the plain bundles; and thus, in beholding the positive effects, we forget the comparative. The main character of the striped fibre consists in a capacity for quick motion, and power is gained by laying thousands side by side. When an immense amount of power is required, *without* quickness, we find the plain fibres used, as in the muscular structure of the uterus.

The necessity for this celerity of action will appear evident, when we consider the demands which are made upon the muscles formed of this structure; these are the muscles of the heart—those which are employed in moving the bones, and all others controlled by the will.

"The pulsations of the heart can sometimes be distinctly numbered in children, at more than two hundred in the minute, and as each contraction of the ventricles occupies only one-third of the time of the whole pulsation, it must be accomplished in the *six hundredth* of a minute, or *tenth* of a second. Again, it is certain that, by the movements of the tongue, and other organs of speech, fifteen hundred letters can be distinctly pronounced by some persons in a minute; each of these must require a separate contraction of muscular fibres, and the production and cessation of each of the sounds implies that each separate contraction must be followed by a relaxation of equal length; each contraction, therefore, must have been effected in the *three-thousandth* part of a minute, or in the *fiftieth* of a second. Haller calculated that, in the limbs of a dog at full speed, muscular contractions must take place in less than the *two-hundredth* of a second, for many minutes, at least, in succession. All these instances, however, are thrown into the shade by those which may be drawn from the class of insects. The rapidity of the vibrations of the wings may be estimated from the musical tone which they produce; it being easily ascertained, by experiments, what number of vibrations are required to produce any note in the scale. From these data, it appears to be the necessary result, that the wings of many insects strike the air *many hundred*, or even *many thousand* times in every second."—Principles of Physiology, by Dr. Carpenter, sec. 600.

The necessity for power in these muscles is equally obvious, for, as we have seen, they are employed in giving motion to the bones; great demands must, therefore, be made upon them, under the ever-varying circumstances of man's condition.

The problem, then, which it is necessary to solve, is,—How may the gaseous force be applied in the soft and yielding muscular texture, so as to combine great quickness of action with power?

We find a mechanism admirably suited in the structure described. Each muscle of the striped kind, may be regarded as an aggregation of millions of single and separate cells, each single one performing the same office as the cylinder of an atmospheric engine, while its walls may be compared to the piston. They are not aggregated together in an homogeneous manner, but form, in the first place, comparatively small collections; the ultimate muscular fibre, and the muscle, as already stated, is made up of these collections, laid side by side. The arrangement is extremely regular, and the interspaces between them are used, not only for the purpose of calling them into action, but affording a stream or channel by which the materials for their motion are supplied, and the waste of their motion removed.

No arrangement could be better adapted for producing instant motion in the *whole*, as it is given, to every *part* of the muscle, at the same moment, while the extreme amount of subdivision, by rendering the movement in each cell inconceivably small, renders the application of gaseous force compatible with the integrity of their delicate walls.

Millions of these delicate cylinders become vacuous;—millions of pistons sink under the pressure of the atmosphere, to produce a single motion, in one single muscle.

Let us compare for a moment with this exquisite action the results which must follow the production of a vacuum in larger cavities. We will suppose that a contraction of half an inch is required in two fibres, each two inches long, and each connected, by a slender thread or tendon, with a body to be moved; and that the texture of both fibre and tendon in each is the same. The cavity of one of these is entire, while the other is divided in the manner already described. A vacuum is suddenly produced in each to the same amount,—in the former, of course, in the single cavity; in the latter, an inconceivably minute one, in each single cell. Both will contract with the same force, but with widely-different results; the cellular (striped) fibre quickly, and uniformly like a well-acting spring, producing an easy, though it may be rapid motion, in the part to which it is attached, while its tender structure is uninjured. The one cavity, or noncellular (plain) fibre, on the other hand, contracts with abruptness and if of the same texture, as we have supposed, with a force incompatible with the maintenance of its integrity, lacerating its walls and rupturing its tendons. Hence it would be necessary to have a stronger or coarser texture in the case of the non-cellular fibre, but this would be attended with many and great disadvantages. It would greatly increase the bulk of the muscle; or, if the size were maintained, diminish its power, demand a more copious stream of blood, which would again add to the size, and prevent that free communication between the interior and the exterior, which is necessary for its action as well as for its nutrition.

X.

We have stated that each striped muscular fibre consists of a collection of cells, surrounded by a delicate membranous envelope. Within this envelope is likewise collected a portion of fluid, which seems to occupy the space between the fibrillæ (the long rows of cells). It is best seen when the fibre, or more properly speaking, the collection of cells forming the fibre, are contracted, when it is forced from between the cells, and collects between their circumference and their envelopes, in little globules or bullæ. I believe this fluid is required for several very important purposes.

In the first place, it appears to be the medium by which that minute amount of pressure is exerted on the cells, by which is produced in them the *vacuous state*. Fluid pressure is the most uniform of all, and by means of it, the least compression exerted at one extremity of a fibre, will instantaneously be communicated to every cell of which it consists.

Again, it appears to be selected for the purpose of directly conveying the atmospheric pressure to every cell, when the *vacuous state* has been produced; a provision which, in like manner, will enable this force to act with the most beautiful uniformity.

But, further, it is used to enable the muscular cells to enlarge in their transverse diameter, which will necessarily shorten or diminish their length. This is the grand object of muscular mechanism, for the extent of motion must obviously depend upon the capacity for shortening which the muscle possesses. We shall find the means used are of the most simple and beautiful nature.

"When the fibril is in a state of relaxation, the diameter of the cells is greatest in the longitudinal diameter; but when it is contracted, the fibril increases in diameter as it diminishes in length; so that the transverse diameter of each cell becomes equal to the longitudinal diameter, or even exceeds it. Thus the act of muscular contraction seems to consist in a change of form, in the cells of the ultimate fibrillæ, consequent upon an attraction between the walls of their two extremities."—Carpenter's Manual, sec. 336.

At page 10, it was stated that there seems good reason for believing that part only of the fibres of a muscle are in action at one and the same time, and that the contracted ones are in consequence thrown into zigzag folds; and this was the reason assigned why muscles *increase* in diameter, while they *diminish* in length, for "partially unoccupied spaces are left between them, which, when a number are added together, will increase the diameter of the muscle."

This appears to be the correct cause, not only for the increase in diameter, but also for the principal amount of shortening. When a fibre contracts, I believe an adjacent one is thrown into zigzag folds, and that in the partially-unoccupied spaces thereby occasioned, the fluid, which lay between the fibrillæ, insinuates itself, driving before it the membranous envelope, and forming little globules or bladders. The spaces left by the retreating fluid are immediately occupied by the yielding cells, which increasing in their transverse, at the expense of their longitudinal diameter, produce considerable shortening.

Nor is this change of position incompatible with the continual pressure of the atmosphere on the cells themselves, by means of this fluid. This may be proved by referring to a precisely similar action on a larger scale—the descent of the diaphragm. This muscle presses on the cavity of the chest, with the usual atmospheric force, or nearly so. It retreats during inspiration, forcing before it the abdomen and its contents, and allowing the chest to be lengthened, but at the same time maintaining its normal pressure; otherwise the integrity of the structures will be directly injured, as a variety of evidence proves.

Again, it seems pretty certain, that the pressure of the globules of fluid on the adjacent relaxed fibre, will arouse its action. This will necessarily cause a reflux of the fluid; by which the transverse diameter of the cells will be diminished to the extent they were previously increased. Thus the fibre will be lengthened, but as the extremities are prevented separating, from the contraction of adjoining ones, it must assume a wavy form, similar to the zigzag appearance already described, but not so extensive. This would be the state of partial relaxation, and it is one which remarkably accords with observation. Dr. Sharpey, in Quain and Sharpey's Anatomy, p. clxxviii., speaks on the subject as follows :—

" In the act of contracting, the fibre becomes shorter and thicker, but does not fall out of the straight line; on being subsequently relaxed, however, it is thrown into serpentine plicæ, *and remains so until its extremities, which had been brought nearer by contraction, are drawn out again by some stretching force.*"

Moreover, if the muscle remain long contracted, it is probable that this state of relaxation will become more complete from the influx of fluid into the cells, according to the laws of Endosmose, which would, under pressure, render it again susceptible of contraction, although to a lesser amount. In one or other, or both of these ways, may be explained that constant intercharge of action, which appears from observation to take place in a contracted muscle—which occasions the "muscular sound"—and which led Mr. Bowman to conclude, that " the sustained active contraction of a muscle, is an act-compounded of an infinite number of partial and momentary contractions, incessantly changing their place, and engaging new portions in succession."

The last object which this fluid seems to serve, is that of aiding the true relaxation of the whole muscle (as described in page 36). It is within the membranous envelope, and therefore in a position immediately to rush into the interior of the cells, when they are drawn upon by an opposing one; while its place will be immediately re-supplied from the blood.

It therefore appears that this minute amount of fluid is used as a medium by which the *vacuous state* is produced, and the atmospheric force brought to bear upon the cells, —as the essential cause of their *shortening*—of the constant interchange of action between the fibres—and, lastly, as the promoter of their relaxation.

XI.

We are now prepared to examine in detail the actions of the striped muscular fibres, in some special organ or organs, as we have previously done those of the plain, in the intestinal tube. These fibres are found under very opposite circumstances; we meet with them in the heart, an organ uncontrolled by the will, and which has thus led the muscles which compose it to be styled the *involuntary*—but principally in the muscles, which are goverued by the will, hence styled the *voluntary*, and which are used in giving movement to the bones, and other organs of motion. Another and more important difference consists in the mode in which the muscles, under these opposite circumstances, are excited to action. Notwithstanding the similarity of their structure, we find the heart contracting, from simple tension, while the muscles of the limbs require to be subjected to nervous influence.

According to the plan hitherto pursued, we shall, first, briefly consider the simplest of these actions, viz., that of the heart; and that we may do it satisfactorily, a brief glance at its structure and position will be required.

The heart is divided into four cavities, two of which are destined to receive blood, one *from* the general system, and the other *from* the lungs, called the auricles; and two to propel it, one *to* the lungs, and the other *to* the system; and these are termed ventricles.

Two of these cavities contract, and two dilate together, and the contraction of one pair occurs during the dilatation of the other; thus, while the two cavities which receive the blood are dilating, those which propel it are contracting; and, on the contrary, while those which propel it are dilating, those which receive it are, in their turn, contracting.

The muscular fibres of this organ interlace one with the other, and are arranged in a very intricate manner, so as to render a positive demonstration of their course impracticable; sufficient, however, is known of them to warrant the belief, that one set is the antagonist of another set; thus, in the auricles, the fibres of which are distinct from the ventricles, the arrangement immediately reminds us of that which exists in the intestinal tube; for we have *looped*, or circular ones, embracing the auricles, and attached to the fibrous rings which unite the auricles and ventricles; and we have *transverse*, or longitudinal ones, passing in precisely the opposite direction; and in those little sacs, which jut from the auricles, termed auricular appendages, this arrangement is most distinct, for we find circular fibres, named *annular*, entirely embracing them, and by far the more numerous, while a lesser number run at right angles, or in a longitudinal direction. In the ventricles, the arrangement is much more intricate than it is in the auricles; but still we find the same general distinction prevailing, the bulk of the fibres running in a circular, or spiral direction, while a few run crosswise, or longitudinally. The arrangement, likewise, of the fibres, here, is of a kind demanding special attention (as we shall see hereafter), for it appears that most of the fibres run from the fibrous rings, which unite the auricles with the ventricles, and return thither, or to those tendinous cords which are affixed to the valves, named columnæ carneæ, changing their position, with a peculiar turn at the apex of the heart, where they pass from the interior to the exterior of the ventricles, or *vice versâ*.

"Having reached the interior of the ventricles, they pass up to form the walls, the septum and the musculi papillares of those cavities, and are ultimately fixed to the auriculo ventricular tendinous rings, either at once or through the intervention of the larger chordæ tendineæ. In consequence of the preceding arrangement, some anatomists have represented the fibres of the ventricles as consisting of a middle layer, incomplete at the apex, and of an external and internal layer, *which are continuous with each other at the apex*, through the aperture there left in the middle layer. According to Dr. John Reid, even the intermediate fibres, or the middle layer, have a similar arrangement to those which cover them."—-Quain's Anatomy, by Sharpey and Quain, fifth edition, p. 1121.

The thoracic cavity, which contains the heart, is perfectly air tight, every portion of it being occupied with the various structures. The heart is situated nearly in the centre; and the bag which directly covers it, is closely and almost completely surrounded with the lungs, which here assume its shape. The portions uncovered with these air bags are, firstly, a part of the anterior face, which is brought into direct contact with the walls of the chest; and, secondly, a portion of the base which is *attached* to the muscle, which separates the chest and abdomen, viz., the *diaphragm*. The heart lies loosely in this bag, being attached only at one extremity, so that it possesses the utmost freedom of motion.

Upon closer examination, we see that great care is exhibited to spread a uniform pressure over every part of this organ. Every space which directly surrounds it, and which is unoccupied by the viscera of the chest, is filled with fluid fat; and it is remarkable that, in great degrees of emaciation, when the fat disappears in almost every other part of the body, it remains here intact, proving that its presence is *essentially* required. Two things become apparent from this disposition; 1. that the heart must be extremely sensible to all variations in pressure; and, 2. That the atmosphere, by means of the lungs, is brought into direct contact with the greater part of its surface: indeed, this force will act upon it with greater facility than on any other muscles of the body, as the membranous substance of the lungs through which it acts is incomparably finer than the integuments of the surface.

If we remember the mode in which the air enters the chest, we shall at once see how perfectly the pressure will bear upon this organ under any diminution of its size. This is entirely due to the enlargement of its cavity, which, by producing a vacuum, or rather a tendency to a vacuum, in the air cells, causes the external air to rush in to maintain the equilibrium. Now it will be readily seen, that any diminution in the size of the heart will be tantamount to a similar enlargement of this cavity, which will necessarily cause an instant rush of air* and pressure upon that surface which is surrounded with the atmosphere of the lungs. The remaining surfaces will be as instantaneously affected—in one direction by the direct pressure of the walls of the chest, in the other, by that of the diaphragm, which, through the abdomen, is directly susceptible to external pressure. It is not improbable—indeed almost certain—from what is

* These actions, though described consecutively, will, of course, occur simultaneously—the enlargement of the cavity of the chest, and entrance of air, are one and the same act, and can only be separated in words.

known of the admirable perfection of animal structure, that the force of motion, although coming through several channels to several parts of the surface, is perfectly uniform over the whole. The pressure of the atmosphere in the lungs is less than the surface pressure, arising from its higher temperature; but this will be compensated by a diminution in the force of the latter, as the result of its playing through yielding structures.

We have an interesting proof of the closeness of the communication between the action of the heart and the external air, in the fact that each beat has been observed to occasion a slight increase or decrease in the air of the lungs. The same observation is likewise a delicate test of the diminution in size of the heart during contraction; for if, like the voluntary muscles, it increased in diameter, while it diminished in length, no amount of air, however small, could make entrance or exit as the consequence of its action.

We are now in possession of all the facts necessary to explain the action of the heart, which, I trust, I shall be able to do in a satisfactory manner; and I may remark, that the views which are found in the Physiological works of the present day, respecting the exciting cause of this action, are in entire accordance with the position we shall endeavour to establish, viz., that it depends upon the entrance of blood.

It is true these works ascribe it to the peculiar stimulus of the blood, while I believe it due to its simple mechanical pressure; but the fact, upon which no disagreement exists, still remains, that the correct explanation of the action will, in some way or other, be found in the blood itself. Dr. Carpenter speaks of it thus:—

"The transmission of blood from the auricle into the ventricle, by the contraction of the former, *is the stimulus which most effectually excites the latter to contraction.*"

And again:—"The auricle, now free to dilate, is *distended by the flow of blood,* from the veins that open into it, and this flow *stimulates* it to renewed contraction."

Our explanation commences with the auricles. We will suppose that both are on the verge of contracting, in which state there is very considerable distension, so much so, that, as stated at page 18, "the right auricle seemed ready to burst, so great was its distension, and so thin were its walls." Now, I conceive, and from the facts already adduced, I believe it will be admitted, that this state of tension must produce in these fibres the *vacuous state.* The surrounding atmosphere, which is so delicately brought to its surface, immediately acts, and pressing in all directions, forces the contained blood from its cavities. Relaxation immediately follows, and this I should attribute to the action of opposing fibres, which have been stretched or compressed by the auricular contraction—the superficial transverse in the auricles, and the longitudinal in the auricular appendages, seem as if they would answer this end. The *vacuous state* is thus again produced—again the atmosphere plays upon the surface—the auricles dilate; and this action is so nicely adjusted, that the balance of pressure is restored the moment the auricles attain their normal size. The blood, during the dilatation, rushes in through the mouths of the veins, and follows the enlarging cavity, as water follows the piston of a pump, and the amount of tension required to renew the action in the contractile fibres is gained, partly, by the sudden check to the momentum of the blood, which the discontinuance of motion in the dilating fibres must occasion; and partly by the tension which the dilating fibres themselves will occasion in their antagonists, the contractile.

While the muscles are contracting, the ventricles are dilating, or producing a vacuum in their interior; this will necessarily be followed by the influx of blood into their cavities, such being the place where the least resistance is offered to its flow.

It therefore becomes an interesting question—of what use is the contractile movement in the auricles? If this explanation be correct, the formation of the vacuum in their interior is amply sufficient to load their cavities with blood; and this must follow if we adopt the present view that the organ vitally dilates. Hence, we must look further for the reason; and we may do so in the assurance that we shall find an important one, for it is the very foundation of science, that the Almighty does nothing in vain.

This, it appears to me, we shall find in the tension which the ventricles require, as the *sine quâ non* of their action. The momentum of the blood is utterly inadequate, although no doubt operating, for not more than two ounces will gain the required velocity, in these important cavities, while the auricles, whose walls are much thinner, receive that of the whole venous system; hence something must be added to increase it, and this is effected by the contractile force of the auricles, which I believe to be the correct and simple explanation of the movement, and the true object served by these cavities.

The remarkable arrangement of the fibres in the ventricles gives an almost demonstrative support to this view. We stated in the description of them, that the fibres of the exterior communicate with the fibres of the interior, at the apex of that organ; and that this is believed to be the arrangement of the middle layer also, by which it results that a portion of each fibre is brought to the interior of the ventricle, while the remainder lies more or less externally.

A more interesting fact than this could not be desired, nor one more consistent with our present explanation. A portion of every fibre is brought to the spot where it will receive, in the greatest degree, the impetus necessary for its action. Nor is it by any means necessary that the whole fibre should be brought into contact for efficient action; for we have already seen that each contains a minute portion of fluid, which, from its simple physical properties, must cause the slightest impression on one part to be conveyed, at the same moment, through the whole of the fibre; thus must the blood, when it distends the walls of the ventricles, produce compression on a part of every fibre, at one and the same instant, which will be followed by complete contraction, and propulsion of the contents of the cavity.

Relaxation will depend upon the opposing action, probably, of the longitudinal fibres, which are stretched, during the contraction of the spiral or circular, sufficient to occasion in them the *vacuous state*; and this is so adjusted, that they commence their action when the atmosphere ceases to act on those which diminish the cavity.

But there seems reason to believe, that the tension produced by the blood is not the sole cause of the ventricular action, but that it is of a compound nature; for if we admit the above explanation of relaxation—the same which has been given elsewhere, we must likewise allow that the relaxing fibres, when fully contracted, will in their turn stretch the contracting fibres, *or some of them*, sufficient to determine their action in return, or prove an important auxiliary to it. Leaving, however, supposition, there are some decided facts, which would be otherwise inexplicable; for the heart, when removed from the body, will continue to beat for some time, *after all tension from the blood has ceased*, the relaxations following the contractions with great regularity—a state which I would ascribe to the *vacuous state*, being alternately produced by the fibres at the time in action; thus, supposing the contractile fibres in action, they would produce the *vacuous state* in the dilating fibres, or some of them, when they had arrived at their limits of contraction, while, if the *dilating* ones were playing, they would, in their turn, arouse the *vacuous state* in the contracting, or some of them, when their limit of action had, in the same way, been reached. Indeed, I conceive that the movement is precisely similar to the vermiform, or peristaltic action of the intestines. We have seen (page 42), that when this is excited in one part, a series of contractions and relaxations will be propagated along the tube. If these actions were condensed or concentrated, we should have the actions exhibited by the heart, while, if the actions of the heart were unravelled, we should have the movements of the intestinal tube.

This analysis, therefore, of the tension, which produces the *vacuous state* in the ventricles, furnishes us with three distinct causes:—1. The force of auricular contraction. 2. The acquired velocity of the blood. 3. The action of the antagonist fibres. The two latter causes must likewise act in the auricles; and the latter one only is the cause of relaxation.

XII.

In the present Section, we shall apply the preceding views to the explanation of the circulation of the blood, which, even at the present time, is not fully and satisfactorily accounted for. We have already referred (page 18) to the insufficient explanation which at present is given of the heart's action; where we stated that it was proved by observation to possess an active dilating power, of which, however, in the words of Dr. Carpenter, "*no definite account can be given.*" And I stated that I should show that it was due, as well as its contractile power, to the atmospheric force. My readers must judge how far I have succeeded;—indeed, if the views which have been here propounded be correct, this force is as essentially required for the dilatation or relaxation of a muscle, as it is for its contraction, *no muscle or muscular fibre possessing an inherent power of relaxation.*

Turning from the heart to the arterial circulation, we have no difficulty in admitting, that it is as well explained by *assuming* a peculiar contractile force, as by attributing it to the action of the atmosphere; and here, therefore, we have only to refer to the reasons for considering the former dependent upon the latter; but the venous circulation is still surrounded with difficulties, according to every previous explanation. These we shall shortly review.

"One of these is the suction power attributed to the heart, acting as a *vis a fronte,* in drawing the blood towards it;" but it is a cause not generally received; and the reason appears to be, that its admission, under the present view of muscular action, requires the existence of an elastic or active dilating property in the auricles to be assumed, which their structure does not prove them to possess.

Again, the movement of the muscles has been considered one of the most powerful causes, for, under such circumstances, "a portion of the veins of the part will undergo compression; and as the blood is prevented by the valves in the veins from being driven back into the small vessels, it is necessarily forced on towards the heart. As each set of muscles is relaxed, the veins compressed by it fill out again—to be again compressed by the renewal of the force." But a great obstacle to this view is, that the circulation is carried on with *great,* though not equal rapidity, when the muscles are at rest, as in sleep, or the recumbent position. Besides, if no alteration in the size of a muscle take place during its action, as is insisted, it is difficult to perceive how compression can have much effect. Contraction ought only to increase the diameter of the vessels, at the expense of their length, not compress them ; and I consider that the increased activity of the circulation during muscular exercise, may be explained by attributing it to the increased chemical changes, without assuming any compressing power in the muscles; nevertheless, it has probably some influence, although by no means an important one.

The chief cause, however, is considered by Dr. Carpenter to be the *vis-a-tago* of the arterial blood. He says, that " The movement of the blood through the veins, is *chiefly* effected by the *vis-a-tago*, or propulsive force, which results from the action of the heart and arteries, and from the additional power generated in the capillary vessels ;" but this view is likewise insufficient to remove the difficulties; for the force of the heart's action is so much diminished in the capillary vessels, that they lose all pulsating power ; and in passing through this extensive system, the friction which the blood must meet with is so great, that it seems difficult to conceive that sufficient force remains to put in motion the whole mass of venous blood, especially when we remember that the current not only rapidly increases as it approaches the heart, but that it is then conveyed, in direct resistance to the force of gravity, the vena cava, or large abdominal vein, having no valves. Nor do I consider that any proof is afforded of any motor power being generated in the capillaries, dependent upon an affinity between the blood and the tissues. " The arterial blood, containing oxygen, with which it is ready to part, and being prepared to receive in exchange the carbonic acid which the tissues set free," causing " the arterial blood, which enters the systemic capillaries on one side, to drive before it, and expel on the other side of the network the blood which has become venous whilst traversing it ;" for this interchange appears to take place, not between the blood and tissue, but the gases of the blood and tissue, and according to the law of mutual diffusion, by which is understood the replacement of one gas by another, in proportions depending upon their density, and which appears to be unattended with movement, except in the gases themselves ; nor does the development of a motor force seem *possible ;* for (according to the law of diffusion) if an attraction exists between the oxygen and tissue, a contrary and similar attraction must likewise exist between the carbonic acid and the blood, one drawing *to*, the other *from ;* which forces being equal to each other, can produce no movement —no more, I believe, than could be occasioned in a balance by the replacement of a pound of gold for a pound of silver.

Some sufficient explanation of the venous current appears, therefore, still to be wanting, for none of the reasons at present adduced, are entitled to be regarded as more than subsidiary causes.

But every difficulty connected with the subject disappears when the heart is regarded as an atmospheric engine, and its office that of forcibly pumping the blood from one set of tubes—the veins—into another set of tubes—the arteries.

In order fully to explain it, let us for a moment refer to the action of a common forcing-pump.

A metallic cylinder, of greater or less diameter, is closed at the bottom, with the exception of a valve opening upwards. This valve, by means of a tube, is in direct communication with the water externally. The piston is solid, and moves freely in the tube, while from the side and bottom of the cylinder, a tube departs, with a valve opening outwards. When the piston is elevated, a vacuum is formed, causing the water to rush in and fill the cylinder, which when the piston descends, is ejected through the tube, from the peculiar arrangement of the valves. The flow is maintained by the continual ascent and descent of the piston, alternately enlarging and diminishing the cavity.

The action of the heart is similar, in principle, to this, but the mechanism is incomparably superior. Instead of the increase and decrease of the cavity being effected by a small portion only of the surface, the rest remaining fixed as in the pump, motion is given to every part at one and the same instant; thus the walls of the cavity become the piston: but the beauty of the mechanism appears more distinctly, when we remember that no complicated machinery exists between the pistons and the cylinders, where the vacuum is formed, but that the walls comprise both—thus giving the most effective motion, with the least possible waste of room, and materials, or loss of power from friction. It has long been a desideratum with engineers, to apply the power gained in a steam or atmospheric engine, directly to the part designed to be moved, without the intervention of the beam and crank, or other machinery, but hitherto without success; and now, for the first time, we can point to an engine where the long-sought desideratum is to be found—a model for their studies.

But the most wonderful fact connected with this wondrous mechanism is, that the fluid which it moves is the source of its motion—that simple compression on its ten thousand times ten thousand cylinders produces the vacuum in their interior, which enables the atmosphere to act upon their surface.

Let us suppose that we can put this engine into play (considering each wall a double cylinder and piston). A little pressure—and a vacuum forms, in textures finer than the cobweb—a powerful force bears upon the surface—the piston sinks—not with the sharp clank of machinery, but a dull, distant, booming sound; while a jet of blood is spouted through the arterial tube. Another vacuum then forms in the opposing cylinder, its piston falls—raising in its descent the one just worked, while the blood rushes into the vacant cavity, from the venous tube with which it communicates.

In each fibre we see a copy of this action—a heart in miniature; the same movements occurring as are exhibited on the larger scale in the heart's action. The cavities are the ultimate cells—the blood flows in from a capillary—the opposing cylinder is an opposing fibre.

And thus the strokes of the piston succeed each other, at an average rate of ten thousand per hour, from the first moment to the last of life, without disorder, and without weariness. All difficulty in explaining the venous circulation is at an end; for to the very verge of the venous system must the active force exerted by the auricular pumps be felt. Simple, yet complex—delicate, yet powerful—clear, yet incomprehensible, is the atmospheric engine of the human body.

XIII.

The structure of the muscular fibres which put in motion the various bones, is of a precisely similar character to that already examined in the heart, although it appears to be more finished, if we may use the word; our attention will therefore be solely directed to the means which are used for producing its movements; and we will commence with a review of those employed in the structures already examined.

We shall find the same principles applied throughout, in a gradually advancing form; they are—*a motor force, acting upon a fibre, through the medium of fluid pressure.*

The intestine furnishes us with this application in its most simple form; here the fibres are thinly spread over the entire surface, and covered with a layer of semifluid mucus, unconfined, except by the folds of the membrane, while the force consists in the momentum produced by the occasional and limited pressure of the aliment, especially the harder portions; the movements are thus only produced at intervals. The auricular action is a considerable step in advance; here the fibres are collected together, and their united movement effected by a simultaneous pressure of fluid on the entire surface, while the force consists in the acquired velocity which the fluid acquires in rushing into a vacuum. But a much more complete application of the same principles we find in the ventricles—concentration is carried to a greater extent—a portion of every fibre is brought to the spot where the impulse is received—while the force consists in the propulsion of fluid by a special movement against the fibres.

In passing from these to more rapid and complex movements, as those of the limbs, the Physiologist will be prepared to find the same principles worked out more perfectly. He will expect to see pressure, aroused by a more active force, brought to bear upon the muscular fibre, and that fluid—here, as elsewhere, will be the medium of its action; especially when he remembers the similarity of structure between the muscular fibres of the heart and limbs, rendering any great deviation from the usual course unnecessary, and improbable.

The conditions, then, which will be sufficient for producing muscular action in the limbs, will be these;—Fluid pressure on the fibre, and the connection of this pressure with the seat of volition—the brain, where a motor force of varying activity must be generated, under the control of the will.

The connection between the fibre and the brain, and through it with the will, is effected by the nerves, which likewise are the instruments by which the necessary pressure is effected. Each nerve consists of thousands of fluid columns, of the most exquisite delicacy, separate one from the other, though bound up in bundles; they are conducted to every muscle, where they divide and subdivide, till from a mass of tubes they form collections of three and four, which again are divided; till at last a single tube of exquisite delicacy is found in contact with many of the muscular fibres, and according to some late observations,* pierces it, again dividing and subdividing among the cells.

Thus is the fluid pressure brought to the threshold of each fibre, or the great majority—the reverse of what obtained in the ventricles; for here a portion of the fibre was brought to the fluid It is delightful to dwell upon the beauty of the mechanism, and its almost unapproachable adaptation to the requirements of the muscle. Compression, to be effective in such yielding structures, must be direct, as provided for here. The arrangement, indeed, very much reminds us of the mechanical division of substances promoting chemical union, as alluded to in paragraph 6, page 3. If the oxygen, under the circumstances mentioned, were to act upon the iron in a mass, it will, from its partial action, require a considerable time to form the oxide; whereas, when it is brought to every particle of the iron, this change is almost instantly effected. So, if the minute amount of fluid pressure which each nerve possesses, were simply brought to the surface of the muscle, the action must be confined to a few fibres, of course, with small effect. It would not do to bring a portion only of each fibre to the fluid, and then turn it elsewhere, as in the ventricles, for the effective shortening of the muscle requires that the longitudinal direction be preserved throughout. This

* Quain's Anatomy, by Sharpey and Quain, p. ccxvii.

difficulty might certainly be met by increasing the surface; but then an increased quantity of fluid will be required, which will not simply occupy room, but demand a much more powerful force to throw it into motion.

The advantage, therefore, which is gained by bringing the columns into direct contact with the fibre, is not merely quickness of action, but the least possible use of material—of space—and of motor force; and to these advantages may be added the admirable provision by which the muscle is made, to a certain extent, self-acting; by which we mean that alternate contraction and relaxation of fibres, more than once alluded to, which occurs during prolonged contraction. We must remember that the nervous tubes are not distributed to every fibre, but pretty uniformly through the muscle, regular interspaces being left between them. Each tube is surrounded, as it were, with a little island of fibres, as the lamellæ surround the Haversian canals in bone; and the immediate effect of its pressure is simply to excite contraction in the fibre or fibres immediately adjacent, while these propagate it to the next, and so on, as described at page 52. By this provision, less fluid is required than would be were the tubes conveyed to *every* fibre, or if it were confined to one surface; and, we may again add, less material—less space—less motor force.

We must now turn to the consideration of the force which is generated in the brain, and by which the impulse is given to the fluid contained within the nerve tubes; but first, let us recal to memory the structure of the brain, and the connection of the nerve tubes with its substance.

The brain consists of a fibrous portion, *i.e.*, of nerve tubes, which enter its substance from the spine, the same which are found in communication with the muscles, and a *vesicular* portion, in which all the active powers of the nervous system are said to reside. The latter is composed of vesicles or cells, and principally occupies the surface of the brain; it is excessively vascular.

The following extracts give a description of each:—1. Of the nerve fibre. 2. Of the cells.

(1). The nervous fibre, in its most complete form, is distinctly tubular. It is composed externally of a very delicate transparent membrane, which is apparently quite homogeneous; this is obviously analogous to the myolemma of the muscular fibre, and serves, like it, to isolate the contained substance most completely from surrounding structures. This membranous tube is not penetrated by blood-vessels, *nor does it branch or anastomose with others; and there is reason to believe it to be continuous from the origin to the termination of the nervous trunk.* Within the tube is a hollow cylinder, of a material known as the *white substance of Schwann*, which differs in composition and refracting power from the matter that occupies the centre of the tube, and of which the outer and inner boundaries are marked out by two distinct lines. And the centre or axis of the tube is occupied by a transparent substance, which is termed the *axis cylinder. There is reason to believe that this last is the essential component of the nervous fibre;* and that the hollow cylinder which surrounds it, serves like the external investment, chiefly for its complete isolation. The whole of the matter contained in the tubular sheath is extremely soft, *yielding to very slight pressure.*

(2). The second primary element of the nervous system, without which the fibrous portion would seem to be totally inoperative, is composed of nucleated cells, containing a finely granular substance, and lying somewhat loosely in the midst of a minute plexus of blood-vessls. Their normal form may be regarded as globular (hence they have been termed *nerve*, or *ganglion globules*); but this is liable to alteration from the compression they suffer, so that they may become oval or polygonal. The most remarkable change of form, however, which they undergo, is by an extension into one or more long processes, giving them a *caudate*, or a *stellate* aspect. These processes, according to Messrs. Todd and Bowman, are composed of a finely-granular substance, resembling that of the interior of the vesicle, with which they seem to be distinctly continuous. They are very liable to break off near the vesicle; but if traced to a distance, they are found to divide and subdivide, and at last to give off some extremely fine transparent fibres, which seem to interlace with those of other stellate cells, and which may, perhaps (though this is at present only a surmise), become continuous with the axis cylinders of the nerve tubes.—Carpenter's Manual of Physiology, sections 370 and 378.

Two circumstances, of much importance, we find to exist from the above extracts, and to which we desire to call attention; the first is, that the nerve tube is continuous from one end to the other—there is no branching or ramifying, as is the case with the blood vessels. This is a fact as well established as any in the anatomy of structures, and leads to the inference, that each tube acts by itself. The second is, the direct connection which is believed to exist between some of the cells (the stellate) and these tubes, the cavity of one communicating with the cavity of the other; for, although the fact is not so well established as the previous one, it is nevertheless important; as proving the close relation which undoubtedly exists between them. Other facts, also, point to this conclusion. It is well known that the tubes in the brain, where they do not actually communicate, come into direct contact with the cells, their coats become thin, and their diameter increases, evidently a formation, which must render the one susceptible to all the changes which may take place in the other. Again, the observation has been lately made, that nerves, in passing through the spinal ganglia of the skate, not merely *come into* contact with the cells of which it is composed, but pass through them, entering at one of the poles, and leaving it at the other.* These various observations taken together are such as to leave little doubt that the connection between the tubes and vesicles, is an extremely close one—that there is great reason to believe that the cavities of each frequently communicate; and that where this is not the actual arrangement, the connection is one which approximates to it.

It has been stated, in one of the above extracts, that with-

* Quain and Sharpey's Anatomy, p. ccix.

out the vesicular or cellular portion of the brain, the fibrous would be totally inoperate, and many facts prove that all the nervous power exists in the former; for it is instantly destroyed by death, while the tubular portions still retain the power of conducting impressions; and it is likewise instantaneously destroyed by the arrest of the current of blood. Thus we have in the brain a cell or cells intensely active, connected, by a series of tubes, with every part of the body.

Another point must be borne in mind—the skull is a closed cavity, surrounded almost entirely by bone, so that no increase can be made in any portion of its contents, without another portion being driven from it, nor decrease, without some portion being added; and we must also remember, that the communication between the brain and muscles is one of an instantaneous character. No sooner does the will call the brain to action, than the action is performed.

What, then, are the phenomena of muscular action dependent on the will?

A sudden force in a cell contained in a closed cavity, and a sudden movement in a part distant from that cavity, the medium of communication being a delicate tube of fluid.

We have already enquired into the movement, and hence have only to examine the character of the force. Would not the mind of the mechanic, with such a problem before him for solution, instinctively dwell on gaseous force; and would he not explain the distant movement, as due to the pressure of fluid which must be driven from the cavity by the generation of such force?

And why should we look further for an explanation? Sound philosophy prohibits our searching for causes which are not requisite to explain the phenomena; while it may be readily shewn that this hypothesis is most in accordance with what we know of Physics, and perfectly capable of explaining the action. Moreover, if gaseous force be the true cause of muscular action, we shall be led, by analogy, to admit the probability of its causing other movements in the system.

I would therefore explain the action of the nerves on the muscles as follows, selecting for our description a single nerve cell, nerve tube, and muscular fibre, which will equally answer for the whole:—

In the nerve cell, the movement is gained by the union of compounds, with a slight impulse—say the oxygen of the blood (which *is essential* for nervous action) with the hydrogen of the tissues. This is determined by an act of volition, which (as it unquestionably possesses a material influence) we may liken to the cause of muscular action—gentle pressure. Before this force, the moveable fluid in the adjacent nerve tube will be driven—the brain case preventing the enlargement of the cell; and as its impetus will be felt chiefly, if not entirely, at its extremity, the muscular fibre will be at once affected, resulting in its movement.

This sudden impulse will be followed by as sudden a recoil; for a condensation occurring in the union of the supposed gases, three volumes (one of oxygen and two of hydrogen) being condensed into two, a partial vacuum must be formed. This will be equalised in two ways;—first, by the restoration of the fluid in the nerve tube to its former position, under the atmospheric pressure; and, second, by the influx of blood from a neighbouring capillary. Thus will cellular NUTRITION be explained in the brain, and similarly to what we have seen it in the muscles; and if sufficient evidence has been adduced to render the latter explanation satisfactory, it will go far to demonstrate the correctness of the former.

I said just now, that the impulse given to the fluid of the nerve tube will be felt "chiefly, if not entirely, at the extremities;" and the reason is this:—Within the spine and brain, the tube, like the cell, is incapable of enlarging, for it is enclosed in a bony cavity; within the nerve, it appears to be incapable, also, for it is packed up most closely with others; every little interspace is filled with fibres, while bands of fibrous tissue* are mingled with them, apparently for the express purpose of maintaining the integrity of the tube; and even after it has divided and subdivided, to a very considerable extent, till not more than three or four tubes remain, it will still be in the same position; and not till it is entirely separated from all connection with others, which is just at its termination, and where it is no longer

* These bands are regarded by some anatomists as the organic, or grey nervous fibre; but "the grey or gelatinous fibres described by Remak, and (following him) by Muller and others, as essentially constituting the organic system of nerves, are now generally admitted not to be entitled to the designation of nerve fibres, but to be a form of simple fibrous tissue." We shall find reasons in support of this view, first promulgated by some able Continental anatomists, as we proceed.

surrounded by its special sheath, shall we find a capacity for enlarging under pressure. As the muscular fibre will thus be affected, simultaneously with the cranial impulse, the promptness of nervous action is as satisfactorily explained, as it could be even by electricity.

XIV.

The mechanism provided for the action of the brain is precisely what these views of nervous action require;—its bony case preventing the exercise of pressure, in any other channel than through the nerves which leave its cavity. These should be regarded as a multitude of tubes, rising into its substance from without, and susceptible of the most delicate internal changes.

I am aware that it is customary to regard the skull as formed for no other purpose but the protection of the brain; but this view is hardly so satisfactory as could be desired, for we observe the presence of bone, where little or no protection is required, as where there is full exposure to danger. Above the eye, for example, the brain is hardly in harm's way, but it is separated by a sheet of bone—the roof of the orbit; and inferiorly, there must be less protection required than above and at the sides; but still the integrity of the case is maintained—the base of the skull. Indeed, design seems more conspicuous in surrounding the brain with an immovable case, than in simply affording protection, as the former view wants the inconsistencies which attend the latter.

The arrangement of the cells in the brain likewise deserves attention; they are brought, on all sides, into direct contact with the skull, a provision which, by opposing their enlargement under pressure, will enable them to act upon the nerve tubes with increased effect. This arrangement deserves the more notice, as it is peculiar; the vesicular matter elsewhere being always found in the *interior* of the ganglia, while the fibrous portion occupies the surface. This latter fact likewise, in its turn, gives support; for the cells, when deprived of all extraneous investment, such as bone, must meet with more protection, and be enabled to act more delicately within than without the ganglia.

XV.

The facts derived from experiment and Pathology, likewise give their support, for all the uses of a nerve are destroyed by tying it, which will, of course, prevent the action of the fluid columns below the point where the ligature has been applied. It is also known that a muscle, which has not lost its irritability, may be instantaneously thrown into contraction, by simply pinching, with a pair of forceps, the nerve proceeding to it. This will necessarily produce a slight amount of pressure on each nervous tubule, by which it will be transferred to the muscular fibre; and positively proves that *the gentle pressure of a nerve is sufficient to produce muscular contractions.*

In fractures of the spine, all sensation and voluntary motion are destroyed below the seat of fracture, resulting from the obstruction to the rise and fall of fluid, which the compression of the bone must occasion. Effusions into the brain produce apoplexy and palsy, partly from a similar obstruction, and partly from the fluid being driven from the brain, and so retained, by the internal pressure; thus cutting off all communication between the tube and cell. Compression, from fracture of the skull—numbness, from pressure—are interesting examples, to which a multitude of others might be added.

XVI.

The phenomena of Sensation likewise furnishes us with some valuable corroborative evidence in support of these views of nervous action.

"All beings of a truly animal nature possess, there is good reason to believe, a *consciousness* of their own existence, first derived from a *feeling* of some of the corporeal changes taking place within themselves; and also a greater or less amount of *sensibility* to the condition of external things. *This consciousness of what is taking place within and around the individual, is all derived from impressions made upon its afferent nervous fibres; which being conveyed by them to the central sensorium, are there felt.* Of the mode in which the impression, hitherto a change of a physical character, is there made to act upon the *mind*, we are absolutely ignorant; we only know the fact. Although we commonly refer our various sensations to the parts at which the impressions are made,—as, for instance, when we say that we have a pain in the hands, or an ache in the leg,—we *really* use incorrect language; for, though we may refer our sensations to the parts where the impression is first made on the nerves, they are really *felt* in the brain."—Manual of Physiology, by Dr. Carpenter, sec. 930.

From this we perceive, that sensation is manifested in conducting impressions *to* the brain, as volition is exhibited in conducting impressions *from* it; and it is a remarkable fact, that *mechanical pressure* is invariably used to occasion the impression at the extremity of the nerve;—there is only one apparent exception. However varied may be the sense, the principle is not varied; there is only a modification of the mechanism. In common sensation, the simple pressure of a body with which the surface comes in contact—even a current of air is sufficient; in the nose, the contact of invisible particles—in the ear, a certain tremulousness, communicated to fluid by the vibrations of the air—in the tongue, the contact of the food—in the eye, compression, excited in a way to be hereafter described, by the rays of light;—all, in fact, though differing in their results as widely as one sense can differ from another, aroused by the varying degrees of an external impression. Moreover, the same structure and arrangement is observed in the nerves distributed to these organs of sensation, as in those which pass to the muscles; tubes of fluid being thrown between them and the brain.

With these facts before our minds, let us throw aside all preconceived ideas; let us simply dwell upon the coincidence unvaryingly exhibited between pressure on the surface, and the cognizance of it in the brain; let us remember that a delicate tube of fluid, with no exception, unites the two—that pressure upon one end will be simultaneously exerted at the other; and then see whether reason or judgment will permit the search after any, than the obvious explanation.

I need hardly dwell upon it. I consider that the pressure at the extremity of the nerve, elevates the fluid in the tube, which here also communicates with a cell in the brain—that this elevation produces an almost inconceivable amount of yielding in its contents, followed by the formation and condensation of gases—and that the impulse derived therefrom informs the soul of the impression; drives the fluid back to its former position, while a fresh supply of material flows in from the blood.

I stated that there was one apparent exception to this rule,—it is found in the eye, where sensation appears to be occasioned by the rays of light, which, as far as we know, are immaterial—but it is only apparent. The tubes which unite the eye and brain, spread out in the globe of the former, and a layer of cells cover them. I conceive that the rays of light, which are well known to be extremely active in promoting decomposition, occasion the gaseous combination in these cells, and that the impulse thereby occasioned produces the chain of actions in the brain, which (as just described) is effected by external pressure in the other sensations. And whenever these cells are found at the extremities of nerves, I believe they fulfil the same office, gently detonating from the pressure, and magnifying the impressions made on them.

We can hardly help being reminded, as we consider the phenomena of sensation, of the admirable precision with which the nervous tubes must act. The touch of the finest hair at the furthest extremity of the body, is recognised simultaneously in the brain; and the wind cannot pass us without recording its flight. But these may almost be considered crude examples, when compared with others;—the contact of invisible and imponderable particles with the nose, or the gentle and inappreciable tremulousness communicated to the nerves of hearing by the vibrations of the air. It must be admitted, that these are inconceivable degrees of pressure; and it will be easy to question the capability of their conveyance to the brain, by tubes whose walls are of a yielding nature, however closely they may be packed; but so long as experiment proves that a muscle *after death* can be entirely contracted by gentle pressure on its nerve, though meeting with the friction of the thousand fibres, among which such minute pressure is spread, we must admit that what may appear inconceivable is nevertheless possible.

It must not, however, be considered absolutely necessary that the pressure, or more properly speaking, the exact displacement of fluid, should be accurately conveyed to the brain, in order to produce *an effect*, for, in the words of Dr. Golding Bird, "liquids transmit forces acting upon them equally in all directions," by which it results that pressure exerted on one extremity of the tube, must likewise be exerted on the other. Still this exact conveyance is essential to produce any *effective compression* in the cerebral cells; and if the principle be established that this compression is required—that the exact pressure exerted on the surface is to be exerted on the brain, which (among other reasons) the various means employed to catch the slightest impression, indicate; then, however difficult may be the conception, we may rest assured that the mechanism acts with complete precision.

XVII.

We have, in preceding Sections, endeavoured to show that the movement of the muscular fibre in the limbs is due to the simple pressure of fluid in a nervous tube, aroused itself by pressure in the brain;—a view not only supported by the structure of the nerves, but by analogy; and the facts just recorded give a character of certainty to the idea, inasmuch as they show the dependence of sensation in the brain on a similar kind of pressure exerted by the nervous tubes, which has, in its turn, been aroused by *external pressure.* The phenomena are, in fact, reversed; before, we had only one end of the chain, leaving us in doubt about the other; but now this is let down, while the other is drawn up, enabling us to discover the character of the former, which we find answering to the conjecture formed; for sensation and volition, although appearing to be diverse acts, are only so in direction; the one conveying an impression *to*, the other *from* the brain, by the same mechanism; hence we may prove each by each.

But other, and more interesting facts remain, confirming—we had almost said, demonstrating, the correctness of our position, and uniting the above views yet more closely. There are a large number of movements in the animal body normally and directly produced by *pressure on the nerves;* they are termed Reflex; the impression and the action do not appear to be in the exact ratio of cause and effect, for the former is magnified in its course, reminding us of the combination of mechanical powers, between a moved and moving body, by which a few pounds will move a weight of tons.

No fact is more certain in Physiology, than that these actions are invariably excited by the contact of some body with a nervous surface—thus the irritation of the nostrils produces sneezing—the contact of the food with the throat, swallowing*—the pressure of the fæces on the rectum, de-

* So distinctly is this the case in swallowing, that it is impossible to perform this action without the contact of fluid or solid. This may be proved by swallowing the saliva repeatedly till none remains in the mouth; it will then be found *impossible* to repeat the movement till a fresh supply be secreted.

fæcation—irritants in the stomach, sickness—and so on. The arrangement required for their production is very simple, viz., a nervous tube, passing from the surface where the impression is made, to a collection of nervous cells, from which another tube proceeds directly to the muscles.

"Every such action involves the following series of changes:—In the first place, an impression is made upon the extremity of a nerve, *by some external agent, just as when sensation is to be produced.* Secondly, this impression is transmitted by a nervous trunk to the spinal cord in Vertebrata, or to some ganglionic mass which answers to it in the Invertebrata. But instead of being communicated by its means to the mind, and becoming a sensation, it immediately and necessarily executes a motor impulse, which is reflected back as it were to certain muscles, and, by their contraction, gives rise to a movement."—Carpenter's Manual, sec. 394.

The following is the explanation which I offer:—By the pressure or impression made upon the tube, a small amount of compression is produced in the cell with which it is immediately connected; this arouses a motor force in the way already described, and causes a propulsion of the nervous fluid in the tube proceeding to the muscle, by which its *vacuous state* is immediately produced, and, as a consequence, its action. Thus the phenomena combine, as it were, the *material* acts of sensation and volition—the *immaterial*, or those which communicate the impression *to*, and receive it *from*, the mind, are wanting. From their consistency with the previous views, they bind them together, and give to them considerable support.

One word with regard to the muscular movements, as aroused by these actions. Relaxation directly follows contraction, which I attribute to the tension produced in the opposing muscle by the sudden contraction (as seen in the heart and elsewhere), and by which its action is determined. The movements of shivering from cold are analogous. In this condition the tendency to the *vacuous state* is so much increased (see page 27) that the slightest impression produces sudden and alternate contractions and relaxations.

XVIII.

In addition to the nervous system of the brain and spinal cord—the cerebro-spinal, as it is termed, there is another, which is deprived of all bony covering, termed the ganglionic, or sympathetic, and which is principally, though not entirely, situated in the chest and abdomen, especially the latter cavity, where the great centres of this system are situated.

" In front of the vertebral column, there is a series of ganglia on each side; communicating, on the one hand, with the spinal nerves, as they issue from the vertebral canal; and also connecting themselves with the two large semilunar ganglia, which lie amidst the abdominal viscera, as well as with a series of ganglia, that is found near the base of the heart. In the head, also, there are numerous scattered ganglia, which evidently belong to the same system, having numerous communications with the cephalic nerves, and being also connected with the chain of ganglia in the neck. The branches proceeding from this series of ganglia are distributed, not to the skin and muscles (like those of the cerebro-spinal system), but to the organs of digestion and secretion, to the heart and lungs, and particularly to the walls of the blood-vessels, on which they form a plexus, whose branches probably accompany their minutest ramifications. The peculiar connection of this system of nerves with the organs of vegetative life, has caused it to receive the designation of the Nervous System of Organic Life; the cerebro-spinal system being termed the Nervous System of Animal Life."—Carpenter's Manual, sec. 926.

The nervous trunks which connect and proceed from these scattered ganglia, have generally been held to consist of two classes of fibres—the tubular, and grey or gelatinous; the former being similar in character to those found elsewhere, but of smaller size, a few of the ordinary size being mingled with them. Latterly, however, great doubt has been thrown upon the correctness of these views, by Volkmann and Bidder, two eminent Continental anatomists, who deny that the grey fibre is a nervous fibre at all, but that it is simply used to envelope or protect the true nerve fibre; they regard the small tubular fibres as the true fibres of this system, and refer their origin to the ganglia, while the larger they attribute to the cerebro-spinal system. Moreover, they find that the structure of these smaller fibres are not precisely similar to the larger, as they are destitute of the double contour, and their contents appear to be homogeneous.

We shall adopt their opinion, as it is one upheld by much authority, and most in accordance with these views of nervous action, which render imperative a tubular structure for the manifestation of the phenomena.

At present the use of the sympathetic system is not clearly understood; but from the distribution of the nerves,* it appears as if the viscera, and other parts of the body, were brought by it into mutual relation, so as to ensure conformity of action. This is distinctly apparent during disease, where functional derangement of one organ will give rise to a similar state in another; it is said in the language of the Physician to sympathise with it, and hence the name given to the system.

Let us take an example. Inflammation of the uterus will produce sickness of the stomach, and increased action of the heart—for with both of these organs, it has considerable nervous connections; the rational explanation of this, may I think, be as follows:†—

The increased quantity of blood and fibrinous deposit, which inflammation will produce in the tissues of the uterus, give

* It should be remembered, that the fine fibres are not confined to the sympathetic system, but largely interwoven with the cerebro-spinal. Indeed, Dr. Carpenter states, the only distinction to be—"that the large tubular fibres *predominate* in the latter, and the fine homogeneous fibres in the former."

† The reader is not to look upon this explanation as anything more than a rough sketch of the manner in which the sympathetic system may act. The subject is too extensive to be fully discussed.

rise to considerable tension. This, by pressure upon the nerves, will, in accordance with the present views, occasion pain. But it will do more—it will produce a certain amount of pressure on the ganglia, which are the centres of the uterine nerves; for under the tension the contents of the nerve tube will yield, and that in a degree propor tioned to the amount of tension. This pressure, so long as it continues, will give rise to an increased gaseous formation in the nerve cells, by which the tubes, in communication with them, and which pass to the heart and stomach, will be affected, causing an increased nervous pressure upon each. In each it will be evidenced by an increased disposition to the formation of the *vacuous state* in their muscular fibres; for if a pressure equal to 6 produces it in the normal state, a pressure equal to 9 will occasion it more readily. The movements of the stomach are, therefore, excited by causes, which in the ordinary state would be unequal. Sickness follows—while the heart, abruptly contracting, upon smaller quantities of blood than usual, will occasion a sharper and *quicker* circulation; making up, in fact, by an increased number of beats for the diminished quantity of blood injected with each.

Thus, as the result of inflammation of the uterus, we have sickness, and a quick and sharp pulse; and these effects are well adapted for the removal of the disease—the current of blood is diminished in calibre, and increased in quickness, rendering it more capable of passing through an obstructed organ, while the sickness, by prohibiting the introduction of food, opposes the most effectual barrier to the formation of fresh blood.

The remedies which experience has found useful are those which reasoning would teach us to expect. Bleeding and purges, by diminishing the general fullness of the system, while leeches and fomentations alleviate the local tension. In proportion, as this is effected, the contents of the nervous tubes return to their former position—the pressure on the ganglionic cells diminishes—while the muscular fibres of the heart and stomach lose their increased susceptibility to action. The sickness is removed, and the circulation becomes quiet.

It will, doubtless, have struck the reader how similar these movements are to the reflex already considered; indeed, both in its arrangement, and the protective nature of its actions, the sympathetic may be rightly termed the Reflex system of the viscera; and from what we know of diseased states, we cannot doubt that in the healthy condition, it enables one viscus to conform itself to the condition of the other, as truly as the pupil does to the rays of light.

XIX.

The mechanism of the sympathetic system is not suited for quick and prompt movements, nor is it required by the purposes which it serves. The ganglia as already remarked, are not surrounded by bone, nor do the contents of the tubes appear to be of diverse densities; arrangements which will equally prevent that suddenness of action which is so characteristic of the centres and nerves of the cerebro-spinal system. But though not so sudden, it is probable that the action will be more continuous and powerful, resembling in this particular the organic or plain muscular fibres, in which the vacuum appears to be formed by "successive yet continuous combinations."

This system, however, presents a structure well suited for the slow action of the gaseous force; for in the first place it seems probable that the cavities are directly continuous with the tubes, diminishing the resistance which would be otherwise offered to the propulsion of their contents, and which would rather direct the pressure to their walls. The cells occupy the interior of the ganglion—are bound to each other by the tubes, many of which "take a circuitous course among the nerve cells, round which they make various turnings and windings;" and the whole is invested with a dense membrane, which "sends processes inwards, through the interior mass, dividing it, as it were, into lobules.". A round or oval shape is likewise given to the entire mass, as also to the separate cells; the walls of the latter are stronger than those found in the brain, and they are moreover enclosed in capsules, "*composed of smaller cells*, closely adherent to each other and the contained cell;" an architectural arrangement well adapted for maintaining their complete integrity. Indeed, the whole is of a character well suited to oppose their increase during the gaseous discharge, and also the action of the atmosphere upon the consequent occurrence of the vacuum within; while the nutrition of the cell is satisfactorily provided for, as we have seen it effected elsewhere.

It further seems probable that the grey or gelatinous fibres which especially prevail among the nerves of this system, and which (as we have seen) have been considered its true nerve fibre, have for their office the protection of the tubes; they are of a tenacious consistence, and seem adapted to prevent the yielding of the walls. The fact that they are likewise found in the cerebro-spinal system, to a small extent, when the nerves which comprise it leave the protection of the spine, gives some additional support to this idea.

XX.

Although we have spoken of nervous action, both in the spinal and sympathetic systems, as if it were irregular in its performance, aroused only at variable intervals, yet from what we know of the various actions of life, and especially of muscular action, there seems little reason to doubt that it is incessantly occurring in a slight degree, and that there is a state of the nerves, as well as of the muscles, which might correctly be termed "tonic," by which they are kept to their bearing, and rendered more directly susceptible to all changes in pressure.

We can hardly leave this subject of nervous action—the grand Master Piece of the Almighty's workmanship—without dwelling for a moment on its wonders. It is, on the one hand, the medium by which external impressions influence the soul; and, on the other, the channel by which the soul operates on the body. Let us remember that the latter has no intrinsic feeling—that, in itself, it is a mere dead piece of mechanism; and, divested of all connection with any fancied "vitality," we shall be more fitted to comprehend this incomprehensible system. A tube of fluid is the simple mechanism by which every action is performed—every external impression received; and this is worked out by the Deity, with a skill which language has not words to describe. All the movements of the body, however gentle—however powerful—however complex—from the tremendous exertion of brute force to the magic touch by which the pupil answers to the rays of light, are dependent upon varying degrees of pressure brought to bear upon the muscles, through millions of these tubes. From this source spring those intricate combinations, which, by merely modifying the air in its passage from the lungs, result in the beautiful variations of language; and on them are dependent the changes of tone, and the melody of song. Spreading far and wide—studding every portion of the human surface, so that the point of a needle shall not find one vacant spot, they rise with every breath which touches them, and sink as they leave their tale. But facts like these, incomprehensible though they be, pass from our sight as we dwell on other and mightier manifestations; as the idea glances through the mind, that every sense is dependent on the selfsame cause—pressure—still simple pressure, *brought to the measure and the rule.* We know not how to admit it, but cannot contradict it. Our judgment assents, but our mind gropes to comprehend.

That every sound which pours delight upon the ear—every scene which gives fascination to the eye, or object that gratifies the smell, or yields pleasure to the taste;—that each and all should carry their creations to the soul, born by the *touch* of invisible things; may be written—may be spoken—but may not be understood.

XXI.

We have, when speaking of muscular and nervous action, had occasion to refer to the function of NUTRITION, and shall, in the present Section, enter more fully upon it; premising, however, that we apply the term in a restricted sense, not to "the whole series of processes, by which the fluid alimentary materials—prepared by the Digestive process, introduced into the system by Absorption, and carried into its penetralia by the Circulation—are converted into organised tissue," but to the mechanical process, in which materials are continually supplied to tissues *already formed*, which are wasting, as a consequence of their actions, and demanding renewal.

It is well known that the ultimate constitution of all tissues, of an active character, is cellular, made up of little bags or cells, containing certain ingredients; and the activity consists in some change or alteration, by which their contents are continually removed, and as continually renewed.

We think that, generally speaking, very undefined ideas are held, when this active process of cellular nutrition is referred to, and as the clear conception of the circumstances attending an action is necessary, in order to explain it, we shall say a few words on this subject, and then endeavour to apply the knowledge we shall gain.

It must be kept in mind, that modern Physiology supposes that a waste, or disintegration of tissue, is the necessary result of its action—that no muscular or nervous act* is performed without the loss or death, as it is termed, of a certain amount of material.

"Many circumstances lead to the conclusion, that, *like all other tissues actively concerned in the vital operation*, Nervous matter is subject to a waste or disintegration, which bears an exact proportion to the activity of its operations; or, in other words, that every act of the nervous system involves the death and decay of a certain amount of nervous matter, *the replacement of which will be requisite, in order to maintain the system in a state fit for action.*"

Again:—

"Every act of contraction may be said to involve the death of a certain amount of muscular tissue; and the products of decomposition, which consist of the elements of muscular fibre united with the oxygen of the arterial blood, are carried off by the venous current. On the other hand, the muscular substance is repaired by an act of nutrition, at

* I refer to these actions, because, from their activity, they must prove the best examples of the nutritive function.

the expense of the fibrin, supplied to it by the circulating fluid."—Carpenter's Manual of Physiology, Sections 384 and 362.

In addition to these facts, let us remember that the activity of each of these tissues resides in the cells—that the action of the muscular cells is undoubtedly consistent with the integrity of their walls—that this almost certainly holds good with regard to the nervous tissues likewise, and we shall find ourselves furnished with the following conclusion:—*That each and every act of muscular and nervous action, is attended with a diminution of the contents of each cell; and each and every act of renewal, with an increase or addition to those contents.*

I think it will be at once admitted, that the cells of both these tissues, and, indeed, of all others, are fully capable of retaining their contents, without loss; they are simple membranous bags, and their object the same as would be served everywhere by such structures. To suppose otherwise, would argue imperfection in their formation.

Now, if we apply our knowledge of the condition required for effecting a change or movement (such as is occurring in these cells), out of the body, to the explanation of those of a similar character occurring within, and which the right use of our reason demands, we shall at once perceive that some motor force must be exerted upon every cell, before an increase or decrease can take place in its contents. It is irrational to believe that a cell, full of material, can in any way suddenly change the amount of its contents, without the exertion of a force from within or from without, as it is to believe that a glass full of water, at rest or in a state of inertia, can empty itself, in part or entirely.

We have seen that evidence, of a most unquestionable character, goes to prove that a partial vacuum is formed in the interior of each muscular cell, *as the condition of its action;* that the walls then collapse, under the atmospheric pressure; and that the cell is refilled under the same pressure, when a stretching force is applied to the walls.

The nutritive material is therefore actually pumped into the cell. In the same way we saw reason to believe that the nervous cell was partially emptied and again refilled, only that from the bony protection which these cells receive, they can undergo no diminution in size when the vacuum is formed.

Now, if this be the correct explanation of nutrition in the muscular and nervous tissues, we may safely conclude that it occurs throughout the body on a similar type—a diminished resistance from within being equalized by the pouring in of materials from without, under the atmospheric pressure. The round or oval shape which obtains among cells, and their peculiar packing, by which the external pressure must be distributed amongst a collection rather than a few, will prove the necessary impediment to the collapse of the walls under the atmospheric force.

We may take the fat cells beneath the integuments as an example of this latter form of cellular nutrition. Now, I consider, and I shall give the reasons presently, that carbonic acid is continually being formed in their interior, and that the partial vacuum thereby occasioned, causes a fresh amount of oleaginous material to be pumped in from the blood, while the carbonic acid is replaced by oxygen, according to the law of *mutual diffusion*.

Further and interesting confirmation of these views may be found in the processes of *nutritive absorption*, by which the chyle is removed from the intestinal cavity, and mingled with the blood. The *lacteals* by which this is effected spring from the intestinal villi, where they appear to "form loops by anastomosis with each other, so that there is no proper free extremity in any case," they run along the intestinal walls, and unite with each other in the mesentery, where they form convoluted loops, known as the mesenteric glands.

"After emerging from the mesenteric glands, the lacteal trunks converge, with occasional union, until they discharge their contents into the *receptaculum chyli*. From the receptaculum chyli arises the *thoracic duct*, which passes upwards in front of the spine, receiving other lymphatic trunks in its course, to terminate at the junction of the left subclavian and jugular veins, where it delivers its contents into the sanguiferous system."—(Carpenter).

The loops formed by the lacteals, where they arise in the Villi, are imbedded in a mass of cells, which grow or are nourished at the expense of the chyle, in which the Villi are steeped during digestion. When filled, their contents are said to be yielded up by bursting or deliquescence; and subsequently absorbed by the lacteals.

Now, in every one of these Villi, we have a vacuum continually being formed, of a similar character to that which occasions the filling of the muscular, nerve, and fat cells, under the atmospheric pressure. This is produced by the action of the right auricular pump of the Heart, with which (as we have seen by the above sketch of the lacteal system) they are in direct communication, through the medium of the veins. It is true that the vacuum will be a partial one, and even slight in its character; but this is not only required, but expressly arranged, for the chyliferous vessels are subjected to numerous turnings and windings in the mesenteric glands, by which the flow of their contents will be considerably arrested.

Under the diminished resistance which this vacuum will occasion, the chyle will be forced through the walls of the intestinal Villi, by the external pressure.* The movements will be as follows:—

Firstly, under the gentle, and perhaps equable influence of the heart's action, a portion of the chyle in the lacteal roots will pass forward; its place will be immediately supplied by the influx of another portion, which previously surrounded the roots. This will diminish the pressure on the cells; or, what is the same thing, remove a portion of *their* resistance to external pressure; which then, assuming the character of an active force, will drive the chyle from the exterior of the Villi to the interior of the cells.

* This is necessarily the pressure of the air in the *intestinal canal.*

Thus we have an additional and strong reason for believing that cells are invariably filled by some active force; for we see in this process of absorption the same principle carried out, although the means are varied; the resistance is removed from the *exterior* of the cell wall, instead of the *interior*, followed necessarily by the same results.

But other, and, perhaps, the most interesting evidence, remains behind. It will be found in the change of form, which the cells of the epidermic tissues undergo, in passing from the basement membrane to the surface. Take the skin for example:—

"In their progress from the internal to the external surface of the epidermis," Dr. Carpenter remarks, "the cells undergo a series of well-marked changes. When we examine the innermost layer, we find it soft and granular, consisting of *nuclei* in various stages of development into cells, held together by a tenacious semifluid substance. Passing outwards, we find the cells more completely formed; at first nearly spherical in shape, but becoming polygonal where they are flattened against one another. As we proceed further towards the surface, we perceive that the cells are gradually more and more flattened, until they become mere horny scales, their cavity being obliterated; their origin is indicated, however, by the nucleus in the centre of each. This flattening appears to result from the gradual desiccation or drying up of the contents of the cells, which results from their exposure to the air."

In reference to the same change of form, Dr. Sharpey states—"A little higher up in the mass, the cells are enlarged—they have a globular figure, and are filled with soft matter; they next become flattened, but still retain their round or oval outline; then the continued flattening causes their opposite sides to meet and cohere, except where separated by the nucleus, and they are at length converted into thin scales, which form the uppermost layers. While they are undergoing this change of figure, their substance becomes more firm and solid, *and their chemical nature is more or less altered;* for the cell membrane of the softer and more deep seated cells may be dissolved by acetic acid, which is not the case with those nearer the surface."

These various changes can be most satisfactorily accounted for by considering that a progressively-increasing vacuum is formed in the cell cavity, as it passes to the surface. So long as the cell is in contact with the semifluid substance surrounding it, this condensation is immediately balanced by the influx of the latter; but when all access to fresh nutriment is prevented, the continuance of the process will be attended with a gradually-diminishing resistance to external pressure. At first, the regular contour of the cell will be pretty well preserved, but as the vacuum becomes more complete, so will the influence of the atmosphere be more decided, and the cell will undergo an extension in the direction of least resistance. This on the skin, and elsewhere, is found to be in the lateral direction, where room is afforded from the packing not being of a very close character; hence they become flattened, and by a continually-increasing vacuum being produced, as they approach the surface, the atmosphere brings the two surfaces into close contact, forming "mere horny scales."

The various forms which the epithelium assumes in various parts of the surface, may be explained in the same manner. If, for example, during the development of the cells, they meet with considerable pressure upon their sides, or, what will amount to the same thing, if their free extremity be the point of least resistance, they will become lengthened, rather than flattened, forming the true "cylinder" or "columnar" epithelium. And the modifications of this epithelium, by which, on a convex surface, the free extremity becomes enlarged, at the expense of the attached one, or by which, on a concave surface, the reverse arrangement obtains, can be thus most correctly explained; for it will be at once apparent that, under the former condition, (where the epithelium rests upon a convex, or, as is seen in the intestinal Villi, on a round surface), the base, or attached end, must form a smaller circle than the opposing or free end; hence the space for the cell will be contracted at the base, while it will be expanded at the summit;—conditions to which the cell is exactly adjusted, under the equal pressure of the atmosphere, one end becoming narrow, at the base where the resistance is greatest, and the other broad, at the summit where the resistance is least.

These views appear to lose the last shred of conjecture, when we remember that the atmosphere in the intestinal canal exerts a less degree of pressure than does the external atmosphere, in consequence of its higher temperature; so that the internal pressure which is exerted between the tissues and the air, instead of being equal and contrary, as it is upon the surface, is in favour of the solids and fluids. We have seen that this gives a convex surface, internally, to the *membrana tympani;* and the same disposition must be present in the intestinal canal, and all other free surfaces maintained at the temperature of the body. In a forthcoming Section, it will be shown that this arrangement is adequate to explain many acts of SECRETION, as it must prove a force driving everything moveable into the interior; and such will be the position of the epithelium cells, directly their connection with the basement membrane is destroyed; which will dispose them to fall into the cavity, as they would into a partial vacuum, unless resisted by a superior force.*

Some corroborative evidence, which must not be lost sight of, will be found in the ciliary motion, which takes place on many of the epithelium cells in the interior. This must doubtless be attributed to the cause which produces muscular action, and all other *movements*—the pressure of the atmosphere upon the surface; and will, of course, render necessary the presence of a vacuum, either in the cell or in

* I may here remark, that Dr. Carpenter's idea that the flattening of the cells on the skin "appears to result from the gradual desiccation or drying up of the contents of the cells, which results from their exposure to the air," seems insufficient to explain the phenomena; for, in the first place, the flattening commences before the cells reach the point of exposure; and, secondly, the same series of changes occur in the interior of the body, where there is no exposure at all.

the cilia*, probably the former, as the latter supposition presupposes the existence of a complex organism in each filament, which anatomy has not shewn them to possess†;

* Although the movements of the cilia will, probably, be found to be the simplest of those actions which are dependant on atmospheric pressure, yet, till observers are agreed upon the exact character of the movement, and also of the ciliary structure, any explanation of its true mechanism must be too conjectural to be of value.

† In the last Edition of Quain's Anatomy, Dr. Sharpey has given a variety of reasons for attributing the ciliary movement to the causes which occasion muscular action; and as the opinions of this eminent Physiologist, particularly on this subject, are of considerable value, I have extracted the passage, which I do the more readily, as it will add importance to the views which are expressed in the text.

"It must be confessed that the nature and source of the power by which the cilia acts are as yet unknown; but whatever doubt may hang over this question, it is plain that each ciliated cell is individually endowed with the faculty of producing motion, and that it possesses in itself whatever organic apparatus and whatever physical or vital property may be necessary for that end; for single epithelium cells are seen to exhibit the phenomenon long after they have been completely insulated.

"Without professing to offer a satisfactory solution of a question beset with so much difficulty, it seems, nevertheless, not unreasonable to consider the ciliary motion as being probably a manifestation of that property on which the more conspicuous motions of animals are known to depend, namely, vital contractility; and this view has at least the advantage of referring the phenomenon to the operation of a vital property, already recognized as a source of moving power in the animal body.

"It is true that nothing resembling a muscular apparatus, in the ordinary sense of the term, has been discovered to be connected with the cilia, nor is it necessary to suppose the existence of any such; for it must be remembered, that vital contractility is not limited to a tissue strictly defined in its appreciable structure. The anatomical characters of voluntary muscle differ widely from those of most involuntary contractile textures, although the movements must in both cases be referred to the same principle. The heart of the embryo beats, while yet but a mass of cells united, to all appearance, by amorphous matter, in which no fibres while the evidence already adduced goes to show that a vacuum already exists in the cell.

are seen; yet no one would doubt that its motions depend then on the same property as at a later period, when its structure is fully developed.

"In its persistence after systemic death, and in parts separated from the rest of the body, the ciliary motion agrees with the motion of certain muscular structures—as the heart for example; and the agreement extends even to the regular or rythmic character of the motion in these circumstances. It is true, the one endures much longer than the other; but the difference appears to be one only of degree, for differences of the same kind are known to prevail among muscles themselves. No one, for instance, doubts that the auricle of the heart is muscular, because it beats longer after death than the ventricle; nor, because a frog's heart continues to act much longer time than a quadruped's, is it inferred that its motion depends on a power of a different nature. And the view here taken of the nature of the ciliary motion derives strength from the consideration that the phenomenon lasts longest in cold-blooded animals, in which vital contractility also is of longest endurance. It must be allowed, unless we distrust the observations of very competent enquirers, that narcotic substances do not in general affect the cilia, while they are generally admitted to alter or extinguish muscular actions. At the same time there remains some ambiguity on this head; my own observations do not agree on all points with those referred to, and Ehrenberg states that strychnia produces the same effect on the cilia of infusoria as on muscular organs. Something, moreover, may depend on the facility or difficulty with which the tissues permit the narcotic fluid to penetrate, which circumstance must needs affect the rapidity and extent of its operation. In the effect of opium on the heart there is a great difference, according as the narcotic is applied to its outer or inner surface; and to this must be added that the effect of narcotics has not been carefully tried on all contractile tissues. Again, we see differences in the mode in which the cilia themselves are affected by the same agent; thus fresh water instantly arrests their motion in certain cases, while it has no such effect in others. The discovery lately announced, that vibrating cilia exists on the ova of certain cryptogamic vegetables, may, perhaps, be deemed a strong argument on the opposite side; but it is by no means proved that the sensible motions of plants (such, at least, as are not purely physical), and those of animals, do not depend on one common vital property."

I may just add, that there seems little reason to doubt that the movement in plants here referred to, is due to the same cause as animal motion—atmospheric pressure.

XXII.

I do not see any satisfactory reason why we should not proceed a step further, and search for an explanation of the "vital" endowment of cells (by which they select one material in preference to another), on physical principles. If our evidence be so far trustworthy, and the conclusion is received, that a vacuum is formed in each cell, by which certain materials rush into its interior, under the atmospheric pressure, we shall find it difficult to believe, that the remaining process is given over to "Vitality."

In the first place let us remember, that the uses of the cell are of a strictly physical nature; they perform (as before remarked) the same office in the body as would be served by little bags out of the body; and, as used in nutrition and secretion, take up materials from a free surface, and give them to the blood; or take them from the blood and give them to a free surface. In the intestinal Villi they may aptly be likened to a number of little membranous buckets, which are attached to its interior, and which, in the manner shown, are successively filled with the chyle; loosing as they do so their hold upon the wall, and passing towards the root of the lacteals, where their contents are yielded up to be pumped upwards to the blood. They perform, in fact, the office of *a vessel holding or carrying materials.*

For the manner in which these little vessels are filled, the following requirements are necessary, which seem perfectly adequate to explain the action:—

An adaptation between the pores of the cell wall, and the molecules which the cell contains, by which they are traversed by such molecules, and by them only.

It will be easy to bring a large amount of evidence, derived from analogy, in support of this idea; and it will obviously divide itself under two heads:—First, the proof of variations in the shape or size of the molecules; and, secondly, the proof that the cell wall is variously adapted to these variations.

Direct observation as to the size and shape of the ultimate particles of substances, or the "elements," as they are termed, is of course wanting, their minute size defying the searching power of the best instruments; but there is unquestionable evidence that they are of diverse characters; and this opinion is drawn from the differences which have been observed in their weight. The atomic theory, for the sake of convenience, supposes that the ultimate particles are of the same size and shape, but differing in weight; but it most accords with observation on the aggregation of elements, to suppose that the particles are of equal weight, but diverse in shape, size, or in the *number collected together.*

But it does not appear necessary to proceed to the consideration of the size and shape of the *ultimate* elements, as there is no proof that the materials which are poured into the cells are ultimately divided; but, on the contrary, that they have been already aggregated into masses, which may be *distinctly proved to be of diverse shapes.* Let us take the saline matters as an example. Chloride of Sodium, Phosphate of Ammonia, Uric Acid, &c., form crystals in masses—different in shape and size; and all are introduced into cells, as is proved in the clearest manner, by their being found there. Now, Chemistry concludes that there are *single atoms* of all these substances, as the smallest particle which can be subjected to analysis, proves its possession of the properties of the entire mass, and the microscope almost demonstrates that these atoms must, as is the case with their aggregations, be *diverse in shape;* for, *as far as it can go*, it detects one from the other, by this character alone.

Moreover, the same conclusion seems indicated in the fact, that the contents of the cells are of a dissimilar character, which irresistibly tends to the belief, especially in connection with the above observations, that the molecules which enter them, are dissimilar likewise.

But does the examination of the blood—the source of these ingredients, afford any evidence that the molecules are of diverse shapes? We will reply by a review of facts. In Harvey's time, Physiologists believed that it was a homogeneous fluid, and any idea that it contained different-sized globules, would have been treated as an hypothesis. The microscope, however, has since been turned to its examination, and revealed the fact that it consists of a liquid, in which float cells of different shapes and sizes. Could we, in the same manner, pierce the constitution of the serum, we should doubtless be furnished with the same results.

If this position, then, be considered satisfactory, we shall find no difficulty in giving an almost complete certainty to the second head—that the pores of the cell walls are adapted to the size of these molecules.

It needs but a little attention to see how the principle involved in this view is carried out, in all the constructions of Nature; indeed, it is so self-evident, that the remarks may, perhaps, appear unnecessary.

Wherever two bodies come designedly into contact with each other, we always find an adaptation existing between them, and that of a character so marked and distinct, that, through the infinity of similar substances, however near they may approach it, yet the adaptation fails to be true and complete. We need not proceed beyond the human body, for satisfactory and convincing proofs. Look, first, at the skeleton;—here we find adaptation, even in the solid bones, most strikingly exhibited. One bone is capable of taking no other position than that originally intended—the head of each is exactly and precisely adapted to a cavity in the other; and in the bones of the skull we find a series of very fine projections, precisely filling a series of very fine indentations, in those with which it articulates; and in the jaw, the fangs of the teeth find sockets, which completely encase them.

We need do no more than refer to the openings adapted for blood-vessels, nerves, or the grooves which are designed to receive blood-vessels or tendons, although they strikingly exhibit the application of our principle;—in the exact and perfect packing of the great cavities with their contents, it meets us; in the mechanism by which the food is compelled to pass *over*, instead of *into*, the tracheal orifice; in the sphincters, which shut or open to the passage of their contents; in the mutual fitness of the finest capillaries for the blood, as exhibited in the greater or less contraction of the globules, when any obstruction arises to their passage; in the arrangement by which the blood pours out its superfluous water* in the kidney; and in many other instances, too numerous to mention. One we will conclude with; and that, perhaps, the most marked of all—it is the exquisite adaptation of the fœtus to the passages of the mother, so as to take up the least practicable amount of space during its expulsion. It takes the direction of greatest room, while a position is given to it, by which it occupies the least during the whole process.

These examples of the mechanical adaptation of one part with another, are (as have been seen) not confined to those which the naked eye detects—the microscope has informed us of them, and proved their existence in textures, which would otherwise seem homogeneous; and why should we consider that the limit of this exact adaptation is bounded by the power of our microscopes? Let us magnify this process of cell development in our minds; and consider each

* This beautiful instance of mechanical adaptation we shall more particularly refer to in the Section on Secretion.

cell enlarged to the diameter of a foot, or more, and each capillary increased in the same ratio;—remember that channels, of a certain size, connect the cavities of each; and that the stream in the capillary consists of yielding globules, variable in shape or size, some precisely adapted to the connecting channel, while others, and the greater number, are opposed to it. Let a piston be adapted to the cell, and raised, drawing the globules towards it; and will it excite our wonder to see those precisely adapted to the channels selecting them, and passing into their interior, while others unadapted, pass on in their course? No stoppage or blocking up could be effected by the latter, as the stronger force which drives the current onwards, would prevent all tarrying.

And if we search elsewhere for an explanation, we immediately lose sight of that consistency, which everywhere indicates One Omnipotent Mind. He gives the same laws to an atom as to a world. He acts on the small scale as on the large—in the hidden recesses of the system, as in those which blaze directly in the sight. It is easy to confound ourselves, if we forget that minutiæ and magnitude are both alike to Him!

It does not seem necessary for a great variety of shapes to be given to the globules—a spherical one might readily be modified to meet every requirement; slight changes being sufficient to produce the oval, pyriform, egg, and disk shapes, with a vast variety of others.

A considerable support is given to the present explanation, and the hypothesis, as it now stands, deprived of much of its conjectural character, by a consideration of the evidence which we have adduced in explanation of muscular action. I do not wish to claim for this evidence any higher position than it deserves; but I think it will be readily admitted to be as sound as most which is known in Physiology. This, (it will be remembered) explained Relaxation thus—(See Section VII):—" As soon as the contracting fibre begins to bear with any force upon the contracted one, and thus to produce a partial vacuum, or rather an impending vacuum in its interior, those molecules of the stream of fluid, which are capable of penetrating the pores of the muscular coat, are urged into the interior by the atmospheric force, and follow, as it were, the contracted fibre during its relaxation."

Now, it is perfectly clear that this action is a mechanical one, and equally clear that the pores of the myolemma must only admit one class of elements, otherwise the various materials contained in the blood would unquestionably enter, and the muscular fibre would be found, on analysis, to vary in its chemical constitution; sometimes comprising one class of elements, at other times a different class. But as analysis contradicts this, and proves that the composition is invariably the same, so we conclude that the same elements invariably choose its pores, implying *that* mutual adaptation which we have conjectured to exist.

The same thing may be said with regard to the nutrition of the nerve cells. As they always admit, under the atmospheric pressure, the same elements, so it seems must the structure of the orifices be expressly adapted for such admission.

If this evidence, then, be received as established, it will absolutely prove the principle of cellular nutrition, which we have advocated to be correct. To the best of my knowledge, no explanation as to the reason why one cell chooses one material in preference to another, has yet been offered; certainly none which is consistent with all other actions of the body, which this one claims as its chief recommendation.

It will be easy to carry out this conclusion, so as to explain the different peculiarities in the cellular contents. We can easily suppose, of which, indeed, there is absolute proof*, that one cell may have its pores adapted for the passage of oil, and another, in the same organ, for the passage of saline matter; and if these cells yield their contents as they do in secreting organs, we shall have oil globules and saline matter in the secretion; or the same pores may be adapted to two different substances—say phosphoric acid, and ammonia, which, when they are blended so intimately as will be the case when they meet in the same cell, will give rise to phosphate of ammonia. Whether any further explanation be required to explain what is understood by the elaboration of nutriment *in the cell*, must be matter of

* "Both in animals and plants the individual cells, which are associated together on the same secreting surface, may differ from each other in the nature of their contents. Thus Dr. Meckel states, that in the liver of mollusca he found some cells containing biliary matter, and others containing fat; and in the recent soft part of the epidermis and its appendages, it is quite common to see cells filled with pigment mixed with others which are colourless."—Quain and Sharpey's Anatomy, p. ccc.

conjecture. I am inclined to think that the view here given is quite sufficient for the purpose; for we must not forget that the elements appear to possess tendencies to union, foreign to what they are presented out of the body; and that, when materials which were previously scattered, are aggregated together in one cell, they will exhibit properties different to what they before manifested—just as the blood globules are colourless, in small amounts, and red when collected together.

XXIII.

The attachment of the cell to the *basement membrane itself*, whilst the ingredients, which it carries are being pumped into its interior, seems due, moreover, to the same universal law. They seem to adhere by the force of atmospheric pressure, and loose their hold by the removal of that force. The boy who raises a brick or stone in the street, by pressing a piece of wet leather tightly on the surface, furnishes us with an exact example of the principle. Let us suppose that one of the chyle cells, in a half filled state, is in close contact with the internal walls of the Villi, so as to exclude the air from between the surfaces; to this it will remain attached, because the external pressure is greater than is resisted by the interior, considering the large extent of attached surface, which offers no resistance. At each stroke of the right auricular pump further additions will be forcibly made to the contents, which must add to the resistance, and therefore slightly break the adhesion, till, by a continuance of the process, the resistance from within equalizes *on all sides* the pressure from without, the cell becomes completely loosened, and at the next stroke of the pump falls away.

XXIV.

The evidence which has been collected in the previous pages, in support of the true cause of Animal Motion, is capable of throwing a considerable amount of light on the hitherto abstruse function of SECRETION. This is most intimately connected with the subject of NUTRITION, as just considered—indeed, it is very difficult to separate them; and thus an enquiry into its nature must either give additional support to the views which have been expressed, or contradict them.

" Secretion is very closely allied to nutrition. In the one process, as in the other, materials are selected from the general mass of blood and appropriated by solid textures; but in the function of nutrition, or assimilation, the appropriated matter is destined, for a time, to constitute part of the texture or organ, whereas, in secretion, it is immediately discharged at a free surface. * * * It has thus been common with physiological writers to designate the selection and deposition of material which takes place in nutrition, by the term "nutritive secretion," whilst the function which we have here to consider, is named simply "secretion," or sometimes, when necessary for the sake of distinction, "excretive secretion."—Dr. Quain's Anatomy, by Sharpey and Quain, p. ccxcvii.

The great object of this function is to remove certain materials from the blood, either to be cast out of the body as useless—as the urine, or to be used for some purpose after separation is effected, as is mucus, bile, saliva, &c. The process is necessarily carried on within the body.

The essential structure of a secreting apparatus is very simple; it may be said, "substantially to consist of a simple membrane, supporting a layer of secreting cells on one of its surfaces, whilst finely-ramified blood-vessels are spread over the other;" or it may even be found of a still simpler character, the cells being absent, while the membrane and blood-vessels alone remain.

The membrane is of a very delicate texture, and extremely permeable to fluids; and this is the case with the blood-vessels, which run beneath it—they are distributed over the entire surface, and their branches are of the finest character.

Now, if we conceive both membrane and blood-vessels involuted and convoluted in a great variety of directions, we shall have every form of glandular structure—the surface will be increased at the smallest expense of space—the true object of this peculiar arrangement.

Let us consider, as has been our custom, the simplest apparatus first. In this (as just remarked) the blood is brought to the interior surface of the body, from which it is separated by a mere film of membrane, and through which its contents pass.

But here, as elsewhere, it is imperative that some motor force be exerted before any transmission of fluid can occur, and which is capable of overcoming the obstruction offered by the membrane. If this is withheld, so must the transmission; for the pressure exerted by the blood-vessels of the animal structure, is conceived to be exactly met by the pressure of the surrounding medium. (Paragraph 7, page 4).

This force in these least complex forms of secretion, is of an extremely simple nature; it depends upon the difference of density between the air of the interior and the air of the exterior, by which a power will be gained in the exact ratio of the difference. This, by pressing upon the blood-vessels, will force from them a portion of their contents, when they arrive at the internal surface. It is correctly exemplified in the pneumatic experiment, by which mercury is driven through the pores of wood. If a piece of wood, excavated into a cup upon its upper surface, be screwed upon the top of the receiver of an air-pump, and filled with mercury, it will, upon exhausting the receiver, be forced by the external atmosphere through the pores of the wood, in the form of a metallic shower.

This I conceive to be the explanation of the simplest act of secretion; and with the superaddition of cells, it doubtless takes a principal, if not an entire share in the formation of every secretion within the body. It is quite sufficient to explain the secretion of the serous membranes—the passage of albumen, water, whether pure or mixed with saline matter, from the interior to the exterior of the blood vessels. The most interesting example of this action, on a large scale, is to be found in the kidney, in which an apparatus is especially provided for the passage of simple water from the system, while the solid parts of the secretion are removed by a distinct and separate process. This consists of nothing but a knot of capillary vessels, uncovered either by membrane or by cells. The Malpighian bodies, as these convoluted blood vessels are termed, are situated at the bottom of each of the uriniferous tubes, which expands into a flask-shaped cup or capsule to receive it. They are formed by an artery, which first pierces the capsule, and then divides and sub-divides into vessels much larger than itself; which, when the division has reached its limit, begin to coalesce—unite as they before divided, till one vessel is again formed, which leaves the capsule by piercing it in the same manner as it did upon its entry.

"It would, indeed," Mr. Bowman remarks, "be difficult to conceive a disposition of parts more calculated to favour the escape of water from the blood, than that of the Malpighian body. A large artery breaks up in a very direct manner into a number of minute branches, each of which suddenly opens into an assemblage of vessels of far greater aggregate capacity than itself, and from which there is but one narrow exit. Hence must arise a very abrupt retardation in the velocity of the current of blood. *The vessels in which this delay occurs are uncovered by any structure.* They lie bare in a cell from which there is but one outlet—the orifice of the tube."

On these tufts, then, so admirably fitted for the temporary congestion of the blood, a slight amount of atmospheric force is brought to bear. Under its pressure the water gently leaves the vessels, an explanation which gives a beautiful completeness to the whole of this exquisitely simple mechanism.*

The "exhalation" of the water from the surface of the lungs, which occurs at every respiration, I consider to be an analogous phenomenon, dependent upon the internal pressure being diminished by the higher temperature. It is quite impossible that simple exhalation or transudation. *without pressure,* can explain this, for, as Dr. Carpenter remarks, "The blood parts in the lungs with a very large amount of moisture, for the inspired air is always *saturated with fluid* as soon as it reaches the air cells; and as it is heated at the same time to about 98°, it thus receives a considerable

* The present explanation is, that the water physically transudes through the coats of the vessels, without any reference to pressure; but this is quite inadequate to explain the rapidity with which the water may be separated from the kidneys, if a large draught of it be taken; nor is the explanation consistent with the law of equal and contrary pressure, which has hitherto been considered applicable to the internal, as well as to the external surface, for then no greater pressure should be exerted by the fluid than is exerted by the air—an obstacle to its transmission. The explanation in the text is physical transudation *under pressure.*

addition, even if it were previously charged with as much as it could contain at a lower temperature."

This exhalation, moreover, must necessarily be constantly occurring during the intervals of respiration, as when the chest is expanded, and by its collection at these intervals; and the pressure which it will then exert on the whole internal surface of the lungs, seems sufficient to occasion the inspiratory act.*

But as already remarked at the commencement of this Section, we find cells playing an important part in the secretory process—indeed the glands are everywhere furnished with them; and their position is always the same, viz., spread out on the *basement membrane*, which intervenes between them and the blood.

There is nothing to add, to what has been already ad-advanced, regarding the principle on which the cells are filled, and the reason why the cells of one gland select one material in preference to another; but we shall find, by taking a review of the process, that the views already propounded are distinctly demonstrated.

Firstly, we obtain, we may say, conclusive proof that the process of cell development (or the filling of the cell), is attended with the exertion of a motor force; and further, that such force consists in the production of a vacuum, on its

* I have no hesitation in expressing my belief, that the reflex action of the respiratory muscles is occasioned by the mechanical pressure of the aqueous vapour upon the nerves; this will supply a satisfactory cause, which has long been wanting, while it has the merit of being in entire accordance with all that is known of the "stimuli" of reflex action.

exterior, for the secreting surfaces to which the cells are attached, are the walls of one immense partially-exhausted cavity. However diversified the glands may be in structure, or however contrary may be the secretion which they select, they all agree in this one condition of their interior surface; and, as we dwell upon it, there seems to spring up an increase of probability that the pores in the cell walls are adapted to the molecules which enter them; for if we find a porous surface, which in its entire extent pours out molecules from the blood, as the result of an active force, and perceive here and there collections of cells adherent to the membrane, each drawing off peculiar molecules, which considerably vary the one from the other, and all from those which pass in the intervening space, we seem almost driven to the belief, that the reason must lie in the more or less ready exit which the molecules find, as they search for an escape beneath this pressure.

But we find a most interesting chain of evidence, *clearly and distinctly confirming the dependence of secretion on the pneumatic force* in the fact, that an express provision exists in most glands for exhausting the secreting cavity.* It appears under various forms, and of different degrees of complexity; but the principle varies not with the modifications. It either clears the cavity of the secretion already formed, thus permitting its renewal, or aids in pumping it directly from the blood, under circumstances demanding a quick and sudden supply.

* By this I mean the interior of the gland, whatever may be its shape.

It must be obvious, that, where secretions are being poured upon a surface, in consequence of its presenting a diminished resistance, that if their efflux be prevented, a limit to such condition must speedily occur, arising from the resistance from within, which will be caused by their collection, equalling, or even exceeding the pressure from without. An apparatus is therefore required to remove the fluid as it collects, in order to maintain the normal conditions of resistance and pressure. Such an apparatus is known in the liver as the gall bladder, in the kidneys as the urinary bladder. Let us see how each fulfil their office.

But first, it will be well to enquire, whether the present explanation of the uses of these organs is, or is not, consistent and satisfactory? The gall bladder is regarded as a "receptacle or reservoir for such bile as is not immediately required in digestion." (Quain). Let us see how its internal structure accords with this idea.

"The mucous coat which is generally strongly tinged of a yellowish brown colour with bile, is elevated upon its inner surface into *innumerable ridges*, which, uniting together into meshes, leave between them *depressions of different sizes and of various polygonal forms*. This structure gives to the interior of the gall bladder an areolar aspect. * * These areolar intervals become smaller towards the fundus and neck of the gall bladder; and at the bottom of the larger ones, other minute depressions, rendered visible by a lens, apparently lead into numerous mucous recesses or follicles."—Quain's Anatomy, by Sharpey and Quain, vol. 2, p. 1075.

Now, if this organ be simply intended for a vessel or receptacle for bile, it seems difficult to account for this structure. One would naturally look for a smooth surface, and expect to find the organ well adapted for the discharge of the contents, which are temporarily stored in its cavity. But, on the contrary, the whole arrangement points to its retention.

First, there are depressions "bounded by *innumerable ridges*;" and at the bottom of the larger ones, other minute depressions, "which apparently lead into numerous mucous recesses or follicles," forming, in fact, so many barriers to the movement of fluid; indeed, it seems almost questionable whether any pressure, consistent with the integrity of the cavity, could *entirely* expel the fluid from this net of obstructions. The same fact meets us more distinctly as we proceed in our examination; another obstruction is met with at the neck of the gall bladder.

"This, which is gradually narrowed" (I quote from the same high authority), "forms two curves upon itself like an S, and then, *having become much constricted*, and changing its general direction altogether, bends downwards, and terminates in the cystic duct:" but, perhaps, the most marked impediment occurs in this latter tube—indeed, it is so self-evident as to put aside all doubts upon the subject. "In its interior," Mr. Quain goes on to say, "the mucous membrane is elevated in a singular manner into a series of crescentic folds, which are arranged in an oblique direction, and succeed close to each other, so as to present very much *the appearance of a continuous spiral valve.* When distended, the outer surface of the duct appears to be *indented in the situation of these folds, and dilated or swollen in the intervals*, so as to present an irregularly sacculated or twisted appearance."

Thus there are two serious objections to the idea, that the gall bladder is only a receptacle for bile;—the complexity of its structure, which is inconsistent with so simple an office; and the marked provision at the neck of the bladder for impeding the exit of the bile. This cannot be for the purpose of preventing its flow into the intestine, for a valvular arrangement already exists, by which this is prevented; indeed, the filling of the gall bladder is at present explained, by the obstruction which the bile meets with in passing onward, and which must prove an efficient safeguard to its emptying. Hence there seems reason to conclude that it performs some action which essentially involves an impediment to the discharge of its contents.

This, I believe, is an occasional peristaltic movement,* by

* The gall bladder is not generally accounted muscular, still, however, its contractility is inferred. This accords with the view which has been propounded in these pages respecting animal motion. It is not necessary that a structure be muscular, in order to be contractile; it is only necessary that a slight vacuum be formed in its fibres, and that the atmosphere may in some way reach its surface. Many contractile movements in the body may be thus explained, and amongst them the fibrous coat of the gall bladder. The following is Mr. Quain's account of it:—"The cellular coat is strong, and consists of bands of dense shining white fibres, which interlace in all directions. These fibres resemble those of cellular tissue; and as a matter of inference only, they are supposed to possess con-

which the ducts and their ramifications will be cleared of the bile, which has collected in them, as by a pump. It is probable that the repletion of these ducts, which would prevent the further separation of bile, first produces by its pressure a contraction of their coats; this drives the bile wherever the obstacle to its progress is least—into the gall bladder. Its presence excites the alternate dilatation and contraction of this cavity, just as the blood excites it in the heart. With each dilatation a partial vacuum will be produced, which will be filled by an influx of bile; and it will be forced, with each contraction, into the cells, and towards the fundus of the organ—that being the position of least resistance; instead of being driven from the cavity, as the blood is driven from the heart. It is probable that the action, instead of being simultaneous, travels gradually from the neck to the opposite extremity.

When the bile ducts are thus drained of their secretion, the action is arrested, and the gall bladder will remain motionless, till a supply of bile has again exuded, sufficient to excite the action. When, during digestion, the bile is required in the intestine, it will only be necessary for an equable and momentary contraction to be exerted by reflex action, so as to fill the common duct; after which the duct will slowly empty the gall bladder, as a siphon does a cask.

tractility. In recently-killed quadrupeds, the gall bladder *contracts on the application of a stimulus*; and in the larger species, such as the ox, *muscular fibres* of the plain variety have been found in this coat." Kirkes states that the gall bladder contains "contractile fibro-cellular tissue, with which is mixed organic muscular fibres."

The anatomist will remember, that the shape of the duct is analogous to the siphon.

The Bladder is another organ, which has been supposed to perform no other office than a simple receptacle for the urine. Let ns again see how its structure accords with such a view.

Its coat contains two layers of muscular fibres—one, externally, the longitudinal; the other, internally, the circular. It has been generally believed that this coat was merely designed for aiding the bladder in the propulsion of its contents; but I believe that it performs no such office, *for the two layers of fibres cannot contract simultaneously*; the one is the antagonist of the other—the one dilates, the other contracts the cavity. Indeed, the arrangement is precisely similar to what we find in the intestinal tube, the movements of which we have already considered; and we may, with just as much reason, deny the peristaltic action of any portion of the latter, while we give it to another portion, as we may deny the peristaltic action of the bladder, with such proofs of its structure, while we accord it to the alimentary canal.

Moreover, it is difficult to see the necessity of a muscular coat to aid in the expulsion of the urine. When the sphincter relaxes, the abdominal muscles are thrown into momentary contraction, by which the urine is forced along the urethra. This latter tube then acts, like the gall bladder duct, as a siphon, to which it likewise accords in shape, and thus empties the bladder. The muscular movements of the organ would therefore be superfluous; and thus we may say, that as they neither act, so neither are they required.

We are thus led to attribute the same uses to this coat, as we found served by the contractile tissue of the gall bladder, that of occasionally emptying the ureters, pelvis, and uriniferous tubes of the kidneys, so as to maintain the activity of the secreting process. The mechanism is the same, and so are the actions. When the ureters become distended, a peristaltic action is excited in their coats, by which they drive the urine through the valves, at their extremity, into the bladder—an alternate dilatation and contraction* will directly be excited, by which all the urine in the tubes will be pumped into this organ.

The Vesiculæ Seminales are the last of these receptacles which we shall mention. A double purpose is assigned to them;—first, that of elaborating a peculiar secretion; secondly, acting as a reservoir for semen.† The former function appears to be of a very conjectural character, and therefore we shall decline its discussion—the latter appears to be untenable. In accordance with our previous plan, we shall enquire how far its structure supports the function which has been assigned to it.

* The dilatation, which appears to be excited by the longitudinal fibres, must greatly preponderate over the contraction, and this is provided for by the small number of circular fibres.

† The seminal vesicles serve as receptacles, or reservoirs, for the semen, as is easily proved by a microscopic examination of their contents; but, besides this, it is *supposed by some*, that they secrete a peculiar fluid, which is incorporated with the semen."—(Quain).

"The sacculated appearance of the vesiculæ seminales is owing to their peculiar formation; each consists of a tube, coiled on itself, *in a complicated manner*, and firmly held in that condition by a very dense fibrous tissue. When unrolled, this tube is found to be from four to six inches long, and about the width of a quill. Its posterior extremity is closed, so that it forms a long *cul-de-sac*. But there are generally, if not always, several longer or shorter branches or diverticula developed from it, which also end *by closed extremities*. Its anterior extremity, which forms the fore part of the vesicula, becomes *straight and narrowed*, and ends opposite the base of the prostate, by uniting on its inner side, at a very acute angle, with the narrow termination of the corresponding vas deferens, to form a single canal, which is the common seminal or ejaculatory duct. * * * * * The mucous membrane is pale, or has a dirty brownish-white colour. It is traversed by *multitudes of fine rugæ*, which form an areolar structure, resembling that seen *in the gall bladder*, but composed of much finer meshes."—Quain's Anatomy, by Sharpey and Quain, p. 1249.

It seems difficult to perceive, as we read this description, any sufficient reason for considering the function of the vesiculæ seminales as merely that of a reservoir. All the remarks, which were suggested by an examination of the gall bladder, hold with still greater force to these organs. In addition to the meshes, which must clog and restrain all motion of the fluid, we find branches of various size, with closed extremities, jutting from the side of the tube, which is moreover twisted in a very complicated manner, while the orifice again is narrowed. It seems, indeed, as we consider it, that the main object of such a structure must be to keep the fluid as much as possible in one position, and prevent its movements in any direction. If it were merely designed for a reservoir, analogy would lead us to expect a simple cavity, with some contractile tissue connected with its orifice, as is exhibited in the common bile duct, and the sphincters generally, and not such a difficult piece of mechanism. *This* plainly tells us that it is destined to perform a much higher office.

Such a structure is admirably adapted for filling these receptacles with fluid, by an alternately dilating and contracting movement. Let us suppose that this commences at the orifice, and travels onward; the fluid which enters it would first fill the nearest meshes and branches; subsequent charges would pass further into the tube, and so on, till the whole were filled, the greatest practicable obstructions being offered to the reflux during contraction, which, as I have said, with regard to the bladder, is probably less marked than the dilatation.

The purpose of this action appears to be of a higher character than is served by the gall and urinary bladder; and this is indicated as well by its structure as other important considerations. It appears to pump the secretion directly from the glands when required during sexual intercourse, and not simply to relieve the vas deferens, and tubes of the testes, from its accumulation.

A variety of evidence may be collected to show that the secretion of the testes is performed only at intervals. Putting out of view the consideration that a constant action of these glands would argue imperfection in structure, and waste of materials (as the secretion is only required at occasional and irregular periods), we find sufficient reason in the fact, that the covering of the testes (the scrotum) in man is obviously designed to forbid such action.

Let us first consider the relation which exists between the more important glands, in which the secretion is *continually eliminated*, and the atmosphere. We find, as was the case with the muscles, every means adopted to enable this force to act truly and constantly on their surface; and even under some circumstances, a muscular power is superadded, leaving them never free from pressure. Thus the liver is compressed alternately by the diaphragm and abdominal muscles, in addition to the atmospheric pressure, which is exerted on its *under* surface. The kidneys are surrounded by an accumulation of fluid fat,* as is the case with the heart, and obviously with the same intention; so are the intestines, where it forms the mesentery; the effects of which arrangement will obviously be, to enable the pressure to be carried without waste, to the whole external surface of those glands, which it surrounds.

Now, in the testes we find a departure from this plan; and an apparatus is provided, by which the force is brought

* It is a well recognised fact, that the fat appears to accumulate irregularly in the system, without any well assigned reasons, choosing to surround some organs, and neglecting others. The true cause now appears evident.

to bear on them only at intervals. This is the dartos, a contractile tissue, which is situated beneath the scrotum, and completely surrounds these glands; forming, in short, a case or capsule, in order to receive them. When the demand is made for their secretion, a vacuum appears to be produced in it, as in muscular tissue, which will directly cause a compression on all sides, while the cremaster muscle fixes them against the pubis, so as to produce still further compression. This action is attended with the well-known corrugation of the scrotum.*

The force which is therefore brought to bear is greater than could be effected by the different degrees of resistance and pressure which ordinarily obtains between the interior and exterior of the gland—a distinct vacuum being produced; and I may be permitted a moment's digression to remark, that this very fact goes to establish the principle of Secretion; for to whatever cause the contractile movement of the dartos be attributed, we must regard it as a force brought to bear in all directions on the gland, simultaneously with the call made for its secretion.

But no sooner has the gland furnished the amount of semen which copulation demands, than the compressing force is taken off the testes—they sink in the loose tissue of the scrotum—become freely moveable—and exhibit a condition, as much opposed as possible to the action of the atmosphere upon their surface.

To return, now, to the uses of the seminal vesicles. It

* I am not sure, however, that the corrugation of the scrotum is attended with this action.

seems next to certain that their movements are, in part, excited through the medium of the nerves, the impression being afforded by the friction excited on the glans during connection;—that the alternate contraction and dilatation thus occasioned pumps the semen from the testes, just as a common pump raises water from a well;—and that, when the vesiculæ are fully charged, "a reflex contraction of the muscles surrounding the vesiculæ seminales" is produced, which discharges their contents into the urethra, and "from this they are expelled, with some degree of force, and with a kind of spasmodic action,* by its own compressor muscles."

It seems probable, from what we know of analogous actions, that the presence of the semen also assists in the movements of the vesiculæ seminales, and that the discharge of its contents is in a great measure to be attributed to the equal and uniform pressure which the repletion of these organs will produce, and which will excite a reflex action in the adjacent muscles. When small quantities of semen are discharged, a stronger impression must be made upon the nerves, which will then principally effect the movement.

To sum up. The evidence which is furnished by these structures, strikingly supports what has been here advanced,

* It appears to me that the spasmodic action is excited thus:—During erection, the spongy portion of the urethra will be closed, from the pressure exerted upon it; the semen, therefore, which is driven into the membranous portion, collects and uniformly distends the compressor muscles;—these immediately contracting, drive the semen along the urethra, which forcibly opens a passage as it proceeds. This seems to account for the intensity of the sensation.

regarding the principle of secretion. It distinctly proves its connection, not with vital actions, but *an active force bearing on the external surface of the gland;* and adds confirmation to the opinion, that a partial vacuum is invariably produced within.

But we may add other examples, and they seem to get more convincing as we proceed. If we examine the mechanism by which the saliva is introduced into the mouth, during eating, we shall find ourselves furnished with proof surpassed by none advanced, in interest and beauty.

The movements of the jaws, during mastication, are attended with the formation of a partial vacuum in the interior of the mouth, caused by the alternate enlargement of the cavity. This will necessarily take off the pressure from the ducts, which open into the mouth, and thus enable them the more readily to pour forth their contents under an external force.

It will be remembered, that the duct of the parotid gland opens in the cheek on a level with the second molar tooth, and the sublingual, and submaxillary glands, under the tongue; so that when the air rushes into the mouth, during mastication (to which it will have ready access posteriorly), it will be intercepted in its endeavour to regain its lost pressure on the ducts—in the former case, by the food between the jaws, which will temporarily divide the mouth into an internal and external cavity; and in the latter, by the tongue, which will again partially divide it into an upper and lower cavity;—arrangements which must obviously tend to maintain a pretty perfect vacuum on the interior of these temporary cavities, and hence on the interior of the secreting surface.

Moreover, we find the salivary glands so situate, that they shall be compressed by muscular force, when their action is required ; thus giving to them an additional amount of pressure.

Now, if these facts be true, we ought to find the mouth recharged with saliva after every act of swallowing—for this is attended with the formation of a more perfect vacuum ; one, indeed, which cannot be restored till after the action is completed ;—and this is found to be the case. Let us take a glimpse at the movement.

So soon as the food is sufficiently masticated, the top of the tongue is applied to the roof of the mouth, and afterwards the whole upper surface of this organ. It retreats, as it were, from the anterior part of the mouth, forcing the food and saliva before it, and leaving a vacuum behind it ; acting, in fact, for the moment, like the piston of a pump. The food is swallowed, and the mouth is simultaneously refilled with saliva.

We can have the most clear evidence of the correctness of this view. Any one, indeed, can perform the experiment on himself, while he reads the description. Nor is experimental evidence of a different, though equally decided character, wanting ; for if the contents of the mouth (say saliva) are swallowed, the finger being placed between the teeth, so as to prevent a vacuum being formed during the process, hardly any saliva will be found to enter the mouth. Very delicate attention can scarcely detect it.

Thus we get distinct possession of the fact, *that the rapid formation of saliva coincides with the removal of pressure from the interior of the gland.*

We pass now to the examination of the last evidence, which we consider necessary to bring. It is furnished by the secretion of milk.

First, examine the means provided for bringing pressure to the external surface of the gland. It does not present a simple surface, like the liver, or kidney, or testes, but is divided and subdivided into an immense number of lobules*, between which the adipose tissue penetrates, and thus the secreting portions of the gland are provided externally with a very complete amount of fluid pressure.

Again, there appears to be evidence of a special degree of compression bearing on these glands during their action, of a similar character to the contractile movement, which embraces the testes.

The mother, when about to nurse her infant, feels a peculiar movement, or perhaps we might say, a peculiar "something" in the breasts, which is termed the *draught.* The physiologist attributes it to the rushing of blood into the glands; but surely the circulation is not so extraordinarily increased as to occasion so marked a feeling. And is there any proof that an increased rapidity of circulation will give rise to a feeling of this character?

It is most in accordance with what we have learned of the cause of glandular action, to consider that a certain amount

* I believe the true reason why a gland is divided into lobules, is to bring the external pressure directly to every portion.

of contractile tissue surrounds, or is interwoven with, the lobules; that a slight amount of pressure is exerted on it, through the medium of the nerves, acting in reply to the commands of the Mind, by which a vacuum is produced, and hence a movement.*

Some further evidence supports this idea. Firstly, the general structure and arrangement of the breasts and testes, are *evidently upon the same type*, and the latter organs possess it. Moreover, the lobules seem surrounded or constructed of tissue, not much unlike that of the dartos, for we find each thus described by Quain :—" The substance of the lobules (of the breasts) especially as contrasted with the adjacent fat, is of a pale reddish cream colour, and rather firm." And with reference to the testes :—" Immediately beneath the skin of the scrotum, there is found a thin layer of a peculiar loose reddish tissue, endowed with contractility, and named the *dartos* ;"—showing some analogy in appearance, if nothing beyond.

But I do not consider that the phenomenon termed the *draught* is confined to the mammæ. It has simply seemed an isolated fact, because of the extent of the movement. It is felt, under the sexual stimulus, in the testes ;—in the sides of the jaw (the parotid gland), under the idea of acidity in the mouth ;—in the lachymal gland, under emotion ; all, apparently, due to the cause to which the mammary move-

* Our views of movements have, as I before hinted, been much limited, by considering that muscular fibres were essential to their production. Now, on the other hand, we can hardly draw the limit with accuracy. The inference of contraction must often be drawn, by the proof or probabilities of an action occurring.

ment has been attributed—a pressure on, or squeezing of, the gland, by a slight contractile movement in the tissue of each lobule; depending, firstly, upon pressure in the brain, which is then conveyed through the nerves.*

We now come to the means employed for removing the pressure on the interior surface of the gland; our last, and what must be regarded, from its ready demonstration, as the most important piece of evidence. *It is the vacuum produced in the child's mouth by sucking.* This is the cause universally acknowledged. An alternately dilating and contracting movement is produced by the muscles of the mouth, necessarily attended with a partial exhaustion; and which causes the milk to rush into its cavity, under atmospheric and special pressure, and from which it passes into the stomach at intervals.

The complete dependence of this secretion on a vacuum is moreover exhibited in the fact, that if it be not formed,

* When glandular secretion is excited by local irritation, it is doubtless through the nerves—a reflex action—the pressure having been transmitted to the nearest ganglion, and there magnified. The secretion of gastric juice may, I think, be attributed to the local pressure of the food—the ordinary pressure being insufficient to produce it, except, perhaps, in a very slight degree. It seems also not unlikely, that the pressure of the food in the mouth, during mastication, will excite the secretion by reflex action. I account for the fact that the secretion is not poured out in larger quantities than the wants of the system require, by the simple consideration that the materials do not exist in sufficient abundance in the blood to form it. Did they, there would be a want of adaptation between the supply and demand, which is nowhere visible. The same explanation I would offer for the small quantity of semen which the testes are capable of secreting, and for the precise amount of material which each gland furnishes.

neither will the milk. It is true that the breasts become slightly distended after labour, if the child be withheld; for the blood is then highly charged with the molecules, and the difference between the internal and external pressure is quite sufficient to fill the ducts; but in a few days it disappears, and no further secretion is established. Thus we are taught to apply the child to the breast soon after delivery, rather than wait the appearance of milk; for the vacuum which the infant's sucking produces is all that is wanting to promote the flow. The child, when it is born, demands the breast, but the breast equally demands the child.

We may, then, in concluding this Section, say of this secretion, as we did of the secretion of saliva—that it gives us "distinct possession of the fact, *that the rapid formation of milk coincides with the removal of pressure from the interior of the gland;* while it furnishes us with evidence, almost as distinct, of a direct force being simultaneously exerted on the outer surface.

XXV.

We might almost take the fact, which is admitted at the end of the last Section,—the connection of the secretion of milk with the formation of a vacuum in the interior of the gland, as the basis of an argument in support of these views of secretion; and enquire why we should look for an explanation of this process internally, on a different type to that which is there exhibited; but it will be sufficient to let it stand by itself, in beautiful harmony with all which have preceded it; and let them, together, furnish the Conclusions which we shall presently draw. I may just add, that the mouth of the infant fulfils the same action for the mammæ, as the gall bladder does for the liver—the urinary bladder for the kidney—the vesiculæ seminales for the testes—the adult mouth for the salivary glands; and that the simple difference between them is to be found in the fact, that the gall and urinary bladder unite the offices of pump and cavity; while the remainder, which secrete the saliva, milk, and semen, exhibit the pump and cavity, as two distinct and separate organs.

In taking a review of the whole function of Secretion, or the motion of the Special Fluids, we become aware of the fact, that it is as much a mechanical process as the movement of the blood;—each of the organs which have been specially referred to, are analogous in principle and office to the Heart, although the structure is of an inferior character. And it is not a little support to the views which have been propounded, to find that they refer the movements of all the fluids to *one and the same cause*, although necessarily exhibited in various degrees of perfection.

It seems surprising that Physiologists have not seemed to consider, that a considerable force was needed to explain the movements of the Secretions; while at the same time they have laboured to remove all difficulties in the Circulation, although this function is provided with an active moving power. But we have, certainly, no more right to expect a spontaneous circulation of fluids, through the ducts of glands, than we have a similar circulation of blood through the blood-vessels. If a force be imperative in the one case, so is it imperative in the other. More especially does this become apparent, whenever the fluid is conveyed in direct opposition to the force of gravity, as the semen in the *vas deferens*. If this secretion does not rise in obedience to a partial exhaustion of the cavity of the Vesiculæ Seminales, we may fairly insist that some equally satisfactory force be found to show why it does rise.

The passage of the Secretions from the blood-vessels to the gland ducts, appears to find a resemblance in the capillary circulation. We may, indeed, liken the venous current, taken by itself, to a gland on the scale of the whole system, hurrying to discharge its excretion at the lungs. It commences in tubes of the utmost delicacy and minuteness; many, if not all of which, are unadapted for giving a passage to *more than a single row of the blood globules.* These coalesce into larger and still larger tubes, and their accumulating contents are drawn towards the Heart, by the alternate dilatation and contraction of this cavity. Each gland is a miniature copy of this action; its molecules enter the cells, through pores, which are incapable of giving a passage to *more than a single row.* These find their way into the ultimate ramifications of the ducts, which coalesce iuto larger and still larger tubes, and their accumulating contents are drawn each to its reservoir, by the alternate dilatation and contraction of the cavity. Thus we find the Secreting process to be the type of the Venous Circulation.

I have already expressed an opinion that the Secreting power of glands is limited by the amount of its own peculiar molecules contained in the blood*; and this is essential to our idea of perfection in this mechanism. It seems hardly necessary to dwell upon it; but it is a subject which seems admitted in one view, and neglected in another. For example:—It is sufficiently recognised that the amount of chloride of sodium (common salt) received into the system, is regulated in accordance with its requirements; but the consequent is forgotten, that only so much can be thrown out, as hydrochloric acid (in the gastric juice), as bears a proportion to what has been received; so that, if an excess of food be taken, and not of salt, there must be an insufficient supply of gastric juice to digest it; and so with all other secretions. Indeed, this is one of the first principles of mechanism. No structure is made of stronger materials than its actions absolutely require;—the contrary implies waste, which perfection avoids. And if, with this knowledge, its powers are taxed to an extent to which it is manifestly unequal, it can excite no surprise that it is found insufficient for the work. A ship preparing for a voyage, is

* The capillaries of the lungs, which pass to the auricle, may be regarded as ducts or vessels, carrying the oxygen which has been separated or secreted from the air; and it is hardly necessary to state, that their power of conveying this gas to the system is limited by the amount which they meet with in the lungs.

stored with a variable amount of the provisions which experience recommends. If one be used in preference to another, and to an unanticipated extent, it necessarily fails. These illustrations may seem simple, but they have the advantage of giving my meaning correct expression, and hence their homeliness will probably be excused.

The great rapidity with which Secretions are occasionally poured from their glands, recalling to memory the quickness of nervous action, has often excited surprise. The eyes will become instantaneously suffused with water upon the occurrence of any irritation, and the mouth will as rapidly fill with saliva. To say that this may be explained by Endosmose, is to say that Endosmose, *in* and *out* of the body, are two different things; for all that we have learned of this phenomenon, obliges us to admit, that it is an action of a slow, rather than of a rapid character.

But I think it would be difficult to find an explanation more satisfactory than that to which our evidence points. Instantaneous pressure called into play upon the external surface of the gland, obliging every molecule to seek the path of least resistance—and as those *only* will pass, the pores of which are adapted to the passage, we shall have an immediate collection pouring from the gland, presenting variable properties, and which our senses lead us to distinguish as tears, saliva, mucus.

We may further direct attention to the fact, that, under great increase of temperature, the secretions are invariably diminished. It is well known, that the bile is with difficulty secreted in hot climates—hence liver complaints;

that much less urine is passed in Summer than in Winter; and, likewise, that the perspiration is considerably greater in the former than the latter season;—it is referred to the relaxing influence of the Heat, on the animal surface; while at the same time it is stated, that the perspiration prevents the heat of such surface being raised. The explanation which I offer appears to me less contradictory;—it attributes the increase of external secretion in Summer to diminution of pressure, depending on the increase of temperature; and its diminution, in Winter, to the increase of pressure, resulting from the fall of temperature;—as a consequence of which, the fluids escape, with varying degrees of readiness, at the different surfaces.

Upon a complete review of the evidence adduced, the main points of which will at once occur to the mind of the Physiological reader, I do not see how we can escape from the following conclusions respecting this great process:—

1. That Secretion consists essentially in the escape of fluid from a porous membranous surface, the molecules of which it consists being adapted to the pores.

2. That its essential cause is the difference, in the degree of pressure, bearing on the surfaces of the membrane.

3. That its activity is in the exact ratio of this difference.

4. That in the most simple forms of Secretion, the variations in the amount of internal and external pressure,

result from the differences in the internal and external temperature.

5. That in the more complex forms, mechanical pressure is brought, in addition to the external surface of the gland, especially if its secretion be rapidly required;—the action of the *dartos* on the testes is an example.

6. That, under the same circumstances, the resistance is likewise diminished internally, by the partial exhaustion of the secreting cavity;—the vacuum made by the infant at the breast is an example.

7. Secretion, therefore, is a mechanical process; and every gland a piece of mechanism.

XXVI.

In the Section on NUTRITION, we endeavoured to explain the manner in which the digested food passed from the cavity of the stomach into the blood-vessels, and to show the action of the cells in the process, up to a certain point;—but there is another mode in which materials introduced from without are received into the blood, and which is generally known as the process of ABSORPTION.

Dr. Carpenter distinguishes the substances, which are introduced by the agency of cells, as being in a finely-divided state, but not in one of perfect solution; while those which are absorbed by the blood-vessels, are entirely dissolved.

This view is entirely consistent with the explanation which I offer, that the former materials are unadapted to the pores of the blood-vessels, while the latter find a ready passage through them.

The following is Dr. Carpenter's account of the mode in which these soluble substances are introduced into the blood:—

" This passage of substances, in a state of perfect solution, from the stomach into the blood-vessels, is probably due to the operation of that peculiar modification of Capillary Attraction, which is called ENDOSMOSE. When two fluids differing in density are separated by a thin animal or vegetable membrane, there is a tendency to mutual admixture through the pores of the membrane; but the less dense fluid will transude with much greater facility than the more dense; and consequently there will be a considerable increase on the side of the dense fluid; whilst very little of this, in comparison, will have passed towards the less dense. When one of the fluids is contained in a sac or cavity, the flow of the other to it is termed Endosmose, or flow-inwards; whilst the contrary current is termed Exosmose, or flow-outwards. Thus if the cœcum of a fowl, filled with syrup or gum-water, be tied to the end of a tube, and be immersed in pure water, the latter will penetrate the cœcum by Endosmose, and will so increase the volume of its contents, as to cause the fluid to rise to a considerable height in the attached tube. On the other hand, a small proportion of the gum or syrup will find its way into the surrounding fluid by Exosmose. But if the cœcum were filled with water, and were immersed in a solution of gum or sugar, it would soon be nearly emptied—the Exosmose being much stronger than the Endosmose.

" Now it seems to be in this manner, that substances contained in the cavity of the stomach, and perfectly dissolved by its fluids, are received into the blood-vessels; for as the blood is the fluid of greater density, it will have a tendency to draw towards it, by Endosmose, the saline and other matters, which are in a state of perfect solution in the stomach."—Carpenter's Manual of Physiology, Sec. 491-2.

There are two very serious objections to this explanation. Firstly, the want of perfection which is apparent in the process. Nutriment cannot be received into the blood, without a certain portion of its own nutriment being lost. Secondly, the character of the movement. Experiment informs us that Endosmose is a very slow action; while Absorption is often known to be extremely rapid.

I shall add little to the first head, as I think it carries its own refutation. It does away with all that beautiful simplicity and perfection, which everywhere characterises the Almighty's workmanship, and nowhere more than here. Dr. Carpenter seems aware of the difficulty, for he is silent on the point. He speaks of the passage of the ingredients, from the cavity to the blood, or " flow-inwards ;" but declines the examination of the necessary result—the passage from the blood to the cavity, or "flow-outwards."

Mitchelet speaks more strongly still; and one may readily be excused from following a Professor of such high repute. He refers more especially to the nutrition of cells, but the remarks will equally apply.

"We can now, by the aid of the phenomenon of Endosmose alone, effected entirely under the dominion of physical forces, explain the mechanism of cell life;—we can tell how the materials necessary for nutrition are able to penetrate the cell, whilst others are eliminated."—Lectures by Pereira, p. 10.

I would simply ask, Shall Bile pass from the blood to return to it—or Urea to return to it—or Chyle leave the digestive cavity, to leak? If it be so, Secretion is no longer an effective process of Separation, or Nutrition an effective process of Absorption.

With regard to the second head, I consider that Endosmose must be rejected, in explaining Absorption; for, as I before said, it must then be "a different process *in* to what it is *out* of the body." Its action, compared with Absorption, is extremely slow; indeed, the time which a full meal would require to be absorbed by Endosmose, would be a speculation of some interest.

There is no necessity for enlarging on this subject; for it is well known that water, alcohol, and various poisons, may be absorbed in a very short space of time; indeed, in a period which may often be counted by seconds.

The correct explanation of Absorption in the body, generally, will, I think, be found in the pneumatic force of the Heart. It must not merely draw the blood from the extremities of the veins, but also all substances in juxtaposition with the veins, which are capable of traversing its pores.

But a different mode appears to be adopted for removing materials from the Digestive cavity, which is explained in the following Section.

XXVII.

There is a peculiarity in the Venous Circulation of the abdomen, which is anything but satisfactorily accounted for. The blood from the stomach, spleen, pancreas, and intestines, instead of passing direct to the heart, after it has traversed these several organs (as is the case with the blood from the general system), is passed through the liver. The various veins pass from small branches into larger trunks; and these again unite into one large vessel—the Portal vein, which, entering the fissure of the liver, is distributed to its substance, dividing and subdividing like an artery.

By what means does this blood circulate? We cannot stop, as many works are found to do, with the bare mention of the fact, but must endeavour to get an explanation. It seems almost superfluous to assert, in the present advanced state of Physiological science, that blood cannot possibly circulate through an organ, unless propelled by an active force;—that, unless the Heart continues to beat, the circulation in the Brain, Stomach, Kidneys, Limbs, &c., will come to a stand;—that such a force is found in every gradation of the Animal Kingdom—from the Lord of the Creation, to the worm, creeping at his feet. But still, though surprising, attention must be recalled to the fact. That it will be necessary to maintain it, I do not believe.

Let us keep this first principle in mind; and look for a force sufficient to maintain the circulation in the Liver. The action of the heart will explain the venous circulation, or the circulation *from* the liver; but it is inadequate to explain the Arterial circulation, or the circulation *to* the liver. The current in the Portal Vein cannot explain it; indeed, without some active movement (see Section XII), it is impossible that the blood can be collected in this vein, from the various viscera of the abdomen, much less that it will, when collected, possess the power of establishing a circulation, which, as far as we know, requires as much as any viscus, supplied with all the power of the left ventricle.

We find this force in the Spleen. Look at the proofs.

"The Spleen* essentially consists of a fibrous membrane, which constitutes its exterior envelope, and which sends processes in all directions across its interior, so as to divide it into a number of minute cavities of irregular form. *These cavities communicate freely with each other, and with the splenic vein;* and they are lined by a continuation of the

* I shall only speak here of what is termed the "cellated" structure of the spleen, as it is with this that we have principally to do. It appears that the spleen may be regarded "as an organ of duplex character, and probably of double function."—Carpenter.

lining membrane of the latter. The small veins which collect the blood from the capillaries of the organ, convey it into these cavities, from which it is conveyed away by the splenic vein."—Carpenter.

This fibrous membrane is "whitish in colour, and is composed of interlaced bundles of cellular tissue mixed with a fine elastic tissue. In addition to these, there are found, especially in the bullock's spleen, pale soft fibres, *apparently plain or unstriped muscular fibres*,* resembling those of the middle coat of arteries.

"It is owing to this structure, *endowed perhaps with vital contractility, as well as mere elasticity*, that the organ is capable of such great and sudden alterations in size. * *

"The spleen varies in magnitude more than any other organ in the body; *and this not only in different subjects, but in the same individual, under different conditions—sometimes appearing shrunk, and at others being much distended.*"—Extracts from Quain and Sharpey's Anatomy—Article, "Spleen."

The minute cavities, or lacunæ, formed by the fibrous membrane, "may be readily injected from the splenic vein with either air or liquid, provided they are not filled with coagulated blood; *and they are so distensible, that the organ may be made to dilate to many times its original size, with very little force.* According to Dr. Evans,† the cavities of

* Professor Kolliker is of opinion that the Spleen is a "muscular organ," and Dr. Sharpey has made some observations which confirm this conclusion.—See Quain and Sharpey's Anatomy, p. 1087.

† Lancet, April 6, 1844.

the spleen *never contain anything but blood*, which 'differs in no respect from venous blood taken out of any other part of the portal system.' * * * The *cellated structure* may be considered as a multilocular reservoir, capable of great distension, and lined with a continuation of the inner membrane of the vein, receiving blood, on the one hand, from the veins of the interior of the organ, and transmitting it onward to the Vena Portæ; and, on the other hand, *acting as a reservoir for the venous blood* of the abdomen, when from any cause, its passage into the Vena Cava is obstructed." The theory which best accounts "for its *cellated* structure is that which regards it as a sort of *diverticulum or reservoir*, which may serve to relieve the Portal Venous system."—Carpenter.

What, then, are the facts which this account furnishes? To state them briefly—

The Spleen is a *contractile and distensible organ*; and, therefore, an organ possessing a MOTOR FORCE;—its cavity is divided into an infinitude of smaller cavities, connected with each other, and with the splenic vein; and it possesses a free communication with all the veins of the digestive cavity.

Connect this with the fact, that blood, resembling in all respects the Portal blood, is found in its cavities, of which, therefore, it has been said to be the "reservoir;" and that the Liver imperatively requires an organ to circulate the blood—brought by the Portal vein.

The conclusion is forced upon us,—that the use of the *cellated* structure of the Spleen, *is to effect the circulation of Blood in the Liver.*

Let us call to mind, for a moment, the relation of the vein, which leaves this organ, and the arrangement of those vessels which enter the vein, and we shall then perceive how completely adapted is the structure of the *whole apparatus*, for the function which has been assigned to it.

The vein runs along a groove, situated in the upper border of the pancreas, and is bound down, as it were, to this organ, by receiving veins direct from its structure. The gastric and mesenteric veins open into it, and a glance at any diagram of this system, will shew that they sweep into the splenic vein, at an acute angle, first running, for a short distance, almost parallel, whilst its union with the superior mesenteric, to form the portal vein, exhibits a similar arrangement likewise.

The effect of this disposition will be :—Firstly, the prevention of the enlargement of the Splenic Vein, under pressure. Secondly, that a current of blood from the spleen will pass, in a direct course, towards the liver, instead of retrograding into any of the supplying streams.

The circulation I would describe as follows; commencing with the distension of the moving organ—the Spleen; first however, premising, that the analogy of muscular movements throughout the body, warrants the belief that some of its fibres are the antagonists of the others;—a certain number dilating, while the rest contract the organ.

Under dilatation, therefore (undoubtedly of a slow character), the blood will be drawn, into the cavities of the spleen, from the whole digestive system, and from its *parenchymatous* structure, producing a greater or less degree of distension. When this distension is sufficiently effected, contraction commences, by which the blood will be driven along the splenic vein directly towards the Liver.

Observe the admirable provision for enabling this circulation to be of a continuous character. The main current of blood, as it leaves the spleen, carries along with it the blood from all the veins which open into it; thus maintaining a flow, as well in contraction as in dilatation. This is in accordance with a well-known fact in hydraulics, and is dependent on the friction between the fluid particles of the main stream, and the mouths of its tributaries. Dr. Golding Bird thus refers to it:—

"The friction of fluid particles is illustrated by an experiment of Bernouilli: he found that water, in passing rapidly from the narrow to the wide end of a conical tube (as is imitated in the "Splenic Vein,") would empty a vessel filled with water, and communicating with it by *a small lateral tube*."

Hence, both in dilatation and contraction, the blood slowly courses onwards; and a completeness is given to the Portal Circulation. It is strikingly adapted for obstructing a too rapid rise into the system of the materials supplied from without, which is effected, in the Lacteals, by their convolutions. The Moving Organ slowly pumps such materials into the blood-vessels, and is the true cause of ABSORPTION, from the Digestive Cavity.

XXVIII.

If I have succeeded in planting the conviction, which a consideration of the evidence already handled, has left in my own mind, into the minds of my readers, I shall find many supporters of the following position:—

THAT PHYSIOLOGY IS NOT RIGHTLY A SEPARATE SCIENCE; BUT THAT THE STUDY OF THE ANIMAL KINGDOM BELONGS TO PHYSICS.

Mathematicians of Cambridge! This is the reply to the *cui bono* taunt of your Science.

The subject, however, is not exhausted. I can hardly say that I have fully explained, as I hope to explain, a single function; but the additional evidence will come under heads, to be more fully considered in the Second Part.

END OF PART I.

NOTE.

The Appendix will appear with the Second Part.

ERRATA.

Page 60, line 10, for "muscles" read "auricles."
Page 65, line 13, for "*vis-a-tago*" read "*vis-a-tergo*."

PART II.

ON

THE DEPENDENCE

OF

ANIMAL MOTION

ON

THE LAW OF GRAVITY.

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PART II.

I.

There are a series of vessels in the Animal Body, which are regarded as specially designed for the purposes of absorption—the "Absorbents or Lymphatics." They were believed by Hunter to remove excrementitious matter from the system; but, as an analysis of their contents—the lymph, proved it to be a highly nutrient fluid; and as it was poured in to the blood-vessels, instead of being removed from the system, as excrementitious matter invariably is; Dr. Carpenter, and some other Physiologists, have rejected this idea, and regard the lymphatics as designed to absorb nutrient materials dispersed through the various tissues.

"Absorption by the lacteals has been commonly described as nutritive absorption, because materials for nutrition are, by its means, conveyed into the blood; while absorption by the lymphatics has been more generally named *interstitial absorption*, and regarded merely as the means, by which the several parts of the body *are cleared of their waste and excrementitious materials*, whether during growth or in common nutritive repair. But it is most probable that the latter purpose is effected by the absorption by blood-vessels; and that the lymphatics are, in this again, essentially similar to the lacteals; that they absorb and elaborate organisable principles, capable of being employed for further purposes in the economy."—Kirkes's Physiology, p. 252.

Dr. Carpenter remarks—

"It was formerly supposed (and the doctrine was particularly inculcated by the celebrated John Hunter) that the office of the lymphatic system is to take up and remove all the effete matter that is to be cast out of the body, being no longer fit for its nutrition. *But for such a supposition there is no adequate foundation.*"—Manual, sec. 502.

I quite agree with Dr. Carpenter, that the lymphatics carry a nutritious fluid, and think that he has shown most distinctly in his works, that there is not one particle of creditable evidence in support of the idea, that they carry excrementitious matter; but I think he fails to be equally happy in explaining the source of these nutrient materials, or perhaps I should say in suggesting the source, as he writes with considerable diffidence upon the subject:—

"With regard to the source of the matters absorbed by the lymphatics, it is difficult to speak with certainty. Their contents bear a close resemblance to the fluid element of the blood, or 'liquor sanguinis' in a state of dilution; and it is very probable that they partly consist of the *residual fluid*, which, having escaped from the blood-vessels into the tissues, has furnished the latter with the materials of their nutrition, and is now to be returned to the former. But they may include, also, those particles of the solid frame work, which have lost their vital powers, and which are not fit therefore to be retained as components of the living system, but which have not undergone a degree of decay which prevents them from serving, like matter derived from the dead bodies of *other* animals, as a material for reconstruction, when it has been again subjected to the organising process."—Carpenter's Manual, sec. 501.

Dr. Kirkes adopts the idea of Dr. Carpenter, but states that, "on the whole, it is most probable that the lymph is derived from the liquor sanguinis." He expresses the doubts which surround the subject in stronger language than does the latter Physiologist. "The *real nature* of the substances, absorbed by the lymphatic vessels, and the mode in which the absorption is effected, are still among the *enigmas of physiology*."

I must express my dissent from each of these views; not because they are unsupported by evidence, but as it seems, contrary to much. We know of no means by which *dead* matter may be rendered organisable except it be subjected to the *digestive* process; and it is not attempted to be shown, that any such process is effected in the lymphatic glands. Moreover, the idea that any waste or residuum occurs in the nutrition of the textures, is repugnant to the idea of perfection

If a single molecule escapes beyond what is required by the wants of any texture, or in other words, if there be not an *exact* correspondence between the supply and the demand; then the absolute perfection of animal mechanism is at an end. Hence, therefore, in commencing an inquiry into the uses of the lymphatics, we shall not be assuming much to say, *that no satisfactory evidence of their function at present exists.*

II.

All will be ready to admit that however uncertain is the use of the lymphatics, they must, nevertheless, fill some very important office; that their function is no subsidiary one in the animal economy. We find them *minutely distributed* through almost every tissue of the body. Dr. Sharpey says, "The Lymphatic vessels are found, in nearly all the textures and organs which receive blood; the exceptions are few, and with the progress of discovery may yet possibly disappear."

Their anatomy almost obliges us to believe, that they are an essential part of the vascular system. They correspond most closely with the structure and arrangement of the veins. Commencing in vessels, of minute diameters, they gradually increase in size, till they find a termination; and, although we shall see reason to believe, that the entire system does not end in one main trunk—*the thoracic duct;* yet as a considerable portion of it does, the analogy is maintained. They are likewise furnished, in certain parts of their course, with valves which are wanting in other positions, and this arrangement again corresponds with what is found existing in the veins.

Indeed the only absolute distinction between the veins and lymphatics consists in the latter being convoluted in their course at variable intervals forming the "Lymphatic Glands."

It is not generally considered that the lymphatics are continuous with the capillaries; but many observers have maintained it. It is well known that a mercurial injection of the lymphatics will pass into the veins; and this is explained by *assuming* that a rupture of the vessels occurs; but in the absence of positive demonstration of rupture, I do not see why we should adopt this conclusion. It is true, that this alone would be insufficient to justify a belief in their continuity, if good evidence of their function forbad it; but when we find, as I think we shall, a considerable amount of evidence pointing almost irresistibly to this belief, such demonstration becomes a valuable corroborative fact. Moreover, we must remember, that so soon as the current of liquid is arrested in these vessels, as after death, they will contract, nearly, to obliteration;* and this will be especially marked in the smaller vessels; a fact, which, taken in connection with the probably minute size of those which communicate, may not unnaturally account for their continuity, yet remaining to be distinctly and satisfactorily proved by observation. A fact mentioned in Dr. Carpenter's "Principles of Physiology," seems considerably to support the idea of continuity. He says,—

* "That the lymphatics are endowed with vital contractility is shown by the effect of mechanical irritation applied to the thoracic duct, as well as by the general shrinking and emptying of the lacteal and lymphatic vessels, on their exposure to the contact of cold air, in the bodies of animals opened immediately after death."—Quain and Sharpey's Anatomy, p. cclxiv.

" Some observations have been recently made by Bidder, on the amount of liquid which flows through the thoracic duct into the venous system ; and if any inference can be fairly drawn from the measurement of the quantity delivered in the course of a few minutes, it would appear that the total amount thus transmitted in one day is nearly, or quite equal, to the entire mass of the blood. At any rate, it so far exceeds the amount of liquid ingested, that *we must believe a large portion of it to be derived from the circulating current,* having been withdrawn from it for a time, to be again delivered into its stream, after having undergone the requisite elaboration."

Now, as I shall presently show, evidence of a very trustworthy character compels us to believe, 1. That the thoracic duct carries only a portion of the lymphatic fluids; and, 2. That no fluid current from the blood to the lymph can take place in the lymphatic glands. Hence, upon the establishment of these points; and also, of the correctness of Bidder's experiments by corroborative evidence, we shall find it difficult to withhold our assent to the almost certain continuity of the capillaries and lymphatic vessels, and the passage of liquor sanguinis from the former to the latter ; for otherwise, we shall have to explain, by some means foreign to what the analogies of anatomy supply, the source from which these vessels derive and yield to the blood, a mass of fluids strongly resembling the liquor sanguinis, and amounting in one day to many times the whole quantity of blood ; and which is clearly *not derived* from that fluid *in the lymphatic glands.*

The fact just mentioned, that the lymph resembles, in all respects, the liquor sanguinis of the blood, is deserving of much more than a passing remark. Dr. Sharpey says, that "the liquid part bears a strong resemblance, in its physical and chemical constitution, to the plasma of the blood; and, accordingly, lymph fresh drawn from the vessels coagulates after a few minutes' exposure, and separates, after a time, into clot and serum." Other physiologists hold the same opinion; indeed, it is a universally recognised fact. Hence, therefore, keeping in mind the large quantity of it which is constantly thrown into the blood, we think it impossible to disbelieve that it is directly derived from the latter; and consider that such a position may strictly be claimed for it, till it be clearly shown to be inconsistent with facts.

So far, therefore, the evidence which may be fairly admitted seems to be this:—that the lymphatic is a vascular system, the general structure and arrangement of which corresponds with the venous; and hence it must have *possession* (used or unused) of the power of *circulating* its contents—that it unquestionably mingles its fluids with the venous blood—and that a communication of some kind has been established by injection between its commencing vessels and the capillaries. Further, experiment proves that the quantity of lymph which is poured into the blood so "far exceeds the amount of liquid ingested, that we must believe a large portion of it to be derived from the circulating current;" and, lastly, we have the established fact, that its contents precisely resemble the liquor sanguinis, or the fluid of this current.

III.

I stated just now, that evidence of a very trustworthy character went to show "that the thoracic duct carries only a portion of the lymphatic fluids;" and this I shall now proceed to do. Our proof will rest (1.) Upon the facts furnished by a comparison of the lymphatic system, taken as a whole, with other vascular systems. (2.) On the peculiar termination of the thoracic duct. (3.) On the additional terminations which have been observed by anatomists; and (4.) On a consideration of the object which the thoracic duct seems intended to fulfil.

(1.) In comparing the anatomy of the lymphatic with other vascular systems, a striking point of difference at once presents itself, viz.—the want of that gradual enlargement in the diameter of its vessels, as they unite and form one single trunk, which is so characteristic of the venous and other systems. We find the lymphatics at their origin as minutely distributed as the veins, indeed the larger branches exceed the latter in number, though they fall short of them in size—their ultimate ramifications, however, on the other hand, *exceed* the vascular system in size. Dr. Sharpey says, "Even the most superficial and finest network is composed of vessels which are larger than the sanguiferous capillaries."

But this system, with a vascular origin so extensive, does not exhibit that gradual union of smaller into larger vessels, and then again into vessels of increased magnitude, till they find a termination in one great trunk, the mode which may be proved to be adopted in all cases where unquestionable evidence exists of their course and termination; on the contrary, they seem to enlarge comparatively slightly, quite out of proportion to such origin; and the duct in which they are said to terminate is a little tube, about a quarter of an inch in diameter, or less,* utterly incompetent to be regarded as the single outlet to a system of vessels, whose aggregate capacity *must* require a trunk of very much larger diameter.

This is a point of much importance; for it is plain that if the supposed outlet be shown to be out of proportion to the entire system, we must at once reject it. The adoption of it, under such circumstances, is the adoption of a fact at absolute variance with the plan upon which all vascular systems are constructed in the Animal Kingdom: and not merely is it opposed to the type of their venous and arterial circulations, but it meets with contradiction in the arrangements of every duct of the myriads of glands with which their mechanism abounds.

Hence, the point clearly at issue is, whether the lymphatic system is formed upon a plan opposed by, and inconsistent with, all others; or whether anatomists have erred in regarding the *thoracic duct* as its single termination. I need not say that the latter conclusion is the only one which seems admissible.

* "These vessels which are named 'lymphatics,' from the nature of their contents, and 'absorbents,' on account of their reputed office, take their rise in nearly all parts of the body, and after a longer or shorter course, discharge themselves into the great veins of the neck; the greater number of them previously joining into a main trunk, named the thoracic duct."— Quain and Sharpey's Anatomy, p. lxxxv.

(2.) Another inconsistency, as inexplicable as the last, (if the thoracic duct be regarded as themain trunk of the lymphatics), is found in the termination of this vessel in the neck. It courses upwards, and empties itself into the great veins of the neck, most commonly at the junction of the left internal jugular and subclavian veins; but it "often divides into two or three branches, which, in some instances, terminate separately in the great veins, but, in other cases, unite fast into a common trunk." (Quain.) Now, if the thoracic duct be charged with nutritious matter, in order to be mingled with the mass of blood, it might find a shorter course and readier access than is here given to it. It runs through its entire length, parallel with a vein, which seems to present greater facilities for its discharge, and which, at length, it leaves to mount to a smaller current. It passes the heart, although its object is to arrive at it. This is inconsistent, and is again a fact at variance with commonly recognised truths. The shortest and readiest means of access are everywhere adopted in the Creator's works.

(3). From the negative evidence which we have obtained in showing that the function assigned to the thoracic duct is irreconcileable with the general arrangements of Nature, we proceed to the examination of more positive evidence—that which leads to the belief, that the lymphatic system pours its contents into the veins, by other channels.

In the edition of Quain's Anatomy, lately published by Sharpey and Quain, the following remarks occur, under the head of " Alleged additional terminations (of lymphatics) in man," p. cclxvii.

" The alleged terminations of lymphatics in various veins of the abdomen, described by Lippi, as occurring in Man and Mammalia, have not been met with by those who have since been most engaged in the prosecution of this department of anatomical research, and, accordingly, his observations have generally been either rejected as erroneous, or held to refer to deviations from the normal condition.* But while such (extra glandular) terminations in other veins than those of the neck, have not been generally admitted, several anatomists of much authority have maintained that *the lacteals and lymphatics open naturally into veins within the lymphatic glands.* This latter opinion, which has been strenuously advocated by Fohmann in particular, is based on a fact well known to every one conversant with the injection of the vessels in question, namely, that the quicksilver usually employed for that purpose, when it has entered a gland *by the inferent lymphatics, is apt to pass into branches of veins within the gland*, and thus finds its way into the large venous trunks in the neighbourhood, in place of issuing by the efferent lymphatic vessels. But, although it of course cannot be doubted that, in such cases, the mercury gets from the lymphatics into the veins, no one has yet been able to perceive

* " In a recent communication inserted in *Müller's Archiv. for* 1848, p. 173, Dr. Muhn of Heidelberg, maintains the regular existence of these abdominal terminations, and refers to three instances, which he met with himself. In two of these the lymphatics opened into the renal veins, and in the other into the vena cava."

the precise mode in which the transmission takes place, and, looking to the circumstances in which it chiefly occurs, it seems to be more probably owing to rupture of contiguous Lymphatics, and veins within the gland, than to a natural communication between the two classes of vessels in that situation." *

From this extract we perceive, that the regular termination of the lymphatics in the abdominal veins, is maintained by some anatomists, and has very recently been insisted on. A fact which I have said in reference to the lymphatics and capillaries, if opposed to a complete knowledge of the function of these vessels, might be regarded as abnormal; but when a consideration of such function obliges us to look for additional communication, the state of things becomes changed, and the observations become pre-stamped, as it were, with Truth. But leaving this point, we find, further, that "several anatomists, *of much authority,* have maintained that the lacteals and lymphatics open naturally into veins, within the lymphatic glands," observations, to the correctness of which the previous considerations likewise lend their support. And when we find, in addition, that all authorities unite in the assertion, that the relation between the blood vessels and the lymphatics in the glands, is of a very intimate nature, the probabilities that an actual communication exists become still

* The Comparative Anatomy of the Lymphatic System strongly favours the idea of a more frequent communication with the venous system, than is generally supposed ; for in all the lower vertebrata—Fishes, Reptiles and Birds, the former communicates with the latter at several points.—H. W.

further heightened. Dr. Sharpey, in reference to this latter point (although doubting that an inosculation of vessels exists) thus writes :—

"The plexiform branches of lymphatics within the glands must evidently be collectively more capacious than the afferent or efferent vessels, with which they are continuous, and hence *the lymph or chyle must move more slowly through them*; and while thus detained or delayed in the gland, *it is brought into close relation with the blood of the numerous capillaries distributed on the lymphatic plexus,* and is thus placed in the most favourable condition for receiving matters from that fluid, or *for yielding up something to the sanguiferous system.*"—Quain and Sharpey's Anatomy, p. cclxvi.

It appears to me that all difficulty, connected with the communications in the glands, between the blood-vessels and lymphatics, disappears, if we regard the channels of junction as small and numerous, forming a kind of microscopic sieve, rather than few and large ; a view which, when we remember that the blood and lymph are brought into contact, over a great extent of surface, appears highly probable, and sufficiently reconciles the opposing facts that globules of mercury will pass from the lymphatics into the veins, although no openings have been detected. Indeed we may get proof from the converse, and say, that if mercury *does* pass into the veins, while no distinct openings for its passage appear, then the only conclusion, free from inconsistencies, is, that it finds its way through porous channels.

(4). The true object, which is served by the thoracic duct, seems clear, if we dwell upon the fact that a necessity evidently exists for a communication between the abdomen and vessels of the neck, for this passage of the lymph, and especially the chyle, into this great division of the venous system. Let us bear in mind that the stream of venous blood consists of two main divisions, each emptying itself seperately into the heart. One, which terminates in the *inferior cava,* and collects the blood from the abdomen and lower extremities, has every facility, from its connection with the digestive viscera, of receiving, more or less *directly,* nutritious materials from without. The other, terminating in the superior cava, formed by the blood of the head and upper extremities, has no such facility; its blood is returned from parts where neither digestion nor absorption of nutriment is normally effected. In order, therefore, to assimilate the character of the blood, in each of these main currents, a supply of nutriment is obviously required from the abdomen to the superior cava, and this is effected by the thoracic duct, which empties its contents at an angle, where the vein from the *head* meets the vein from the *superior extremities,* indicating that its fluids are conveyed, to be mingled with the blood of these two streams.

The evidence, therefore, which we have collected to show "that the thoracic duct carries only a portion of the lymphatic fluids" is this. That its size, compared with the entire lymphatic system, is such as to find much opposition in analogy to be regarded as the single outlet—that its course is irreconcileable with the idea, that it carries the lymph to the entire mass of blood; that additional terminations to the lymphatic system have been discovered in the abdomen, and within the lymphatic glands; and that the purpose of this vessel to the neck, is the conveyance of chyle and lymph to the upper segment of the venous current.

IV.

The next point which we have to establish in connection with this subject is,—"That no fluid current from the blood to the lymph can take place in the lymphatic glands." This depends upon a very simple cause.

In Part I. of this work I stated that the movement of the chyle was due as the movement of the blood to the action of the heart. I shall presently dwell more fully on this point, but in the meantime request my readers to assume its correctness.

It is one of the laws of Gravity, that its power of attraction decreases, in proportion, as the distance between the bodies attracted increases, and *vice versâ*; from which it follows that a falling solid or fluid body will move more rapidly, and therefore with a greater momentum, at a distance of three feet from the earth, than at a distance of six feet. Now, as the heart's action is dependent upon this law, we shall necessarily find the movements of the fluids, which it puts in motion, governed by it likewise, and therefore they will all move more rapidly, the nearer they approach the heart, and more slowly at a distance from it.

Every lymphatic gland must furnish us with an illustration of this point; for although the lymph and blood vessels are intimately mingled within them, and hence we might, at first sight, think they were equally distant from the heart; yet the blood vessels are actually nearer, as the lymphatics are subjected to convolutions in the glands, through several of which they sometimes pass, which must considerably increase their length. Their distance, therefore, from the heart, at any one point, must be much greater than an adjoining blood vessel.

The heart, thus acting less powerfully on the lymph than on the blood, its motion must be slower than the motion of the latter—an effect which will be still further promoted in the lymphatic glands, by the increase of friction at the different angles or turnings, which very distinctly diminishes the onward movement of fluids. Thus the force of the current, in each porous blood vessel, must greatly preponderate over a current of lymph on its exterior, of the same magnitude; and hence the amount of the latter, which is drawn into, and mingled with, the current of blood, must greatly outbalance the disposition to any contrary movement.*

V.

We must now pass to the consideration of the evidence which corroborates the fact observed by Bidder, that a much

* That the current in the lymphatic glands is from the lymph to the blood, derives confirmation from the fact, that such a current unquestionably takes place in the mesenteric glands during digestion ; for as it passes them, the amount of oil which the chyle contains has been observed to disappear ; a fact which seems to admit of no other explanation

larger amount of fluid is poured into the thoracic duct, than is ingested—the same evidence will show that a circulation of lymph as complete as the ordinary circulation must be present in the animal body.

To do this satisfactorily, we must previously endeavour to clear up one or two points connected with the circulation of blood; they involve a glance at the uses of the valves; and prove that the venous current is continuous, and not momentarily arrested at each contraction of the auricles.

It is the opinion generally received among Physiologists, that the uses of the valves are to prevent the reflux of the blood under the pressure of muscles, (see p. 64) and this opinion seems simply based upon the fact that they are only found in the limbs, where muscles are in continual action.* I have already expressed an opinion (page 65) that the compression of muscles can have little effect on the circulation in the way mentioned; and I may here add that, if this be the true uses of the valves, we ought most assuredly to find them in the abdomen, as in the limbs; where, if veins are influenced by muscular action, a constant pressure must be in operation, from the alternate action of the diaphragm and abdominal muscles—the peristaltic action of the intestines—the movements of the psoas and iliacus, &c. How powerful must be the influence thus exerted, in fast running, for example; the circulation of the *inferior cava* must be constantly

* "The chief influence that the veins have in the circulation is effected with the help of the *valves*, which are placed in all veins that are subject to local pressure from the muscles, between or near which they run."—Kirkes.

arrested, at the very time when it requires to be especially free.

But, perhaps, more convincing evidence that this is not the purpose served by the valves, is found in the fact that they are wanting in the lungs. Now, it cannot be said of these organs, as of the muscles, that they increase in one direction, while they diminish in another; on the contrary, during expiration, the entire lung diminishes in size, presses, and drives the air from its cavity; and must likewise press upon the blood contained in its vessels. Here, therefore, if anywhere, should valves be found, in order to prevent the reflux which must occur to an enormous extent, if local pressure impedes the circulation. Their absence furnishes us with a good test of the correctness of present opinions.*

I believe the valves are simply designed to meet the

* The reason why no reflux of blood occurs during expiration will, I think, be found in the following extract from Dr. Golding Bird's work on Natural Philosophy, sec. 199. I am obliged slightly to alter a word or two, as the text refers to a diagram in the original.

"When fluids pass through a tube or channel, whose section is greater at one part than another, the velocity of the liquid is necessarily greater in the narrow than in the wide parts, as the same quantity must pass through every part in the same time. Thus, if in a tube, water be allowed to run through in a stream, so as to keep it constantly full, its velocity at the narrow will be much greater than when traversing the wide parts. For the same reason, when water flows through a funnel its velocity is much greater when passing through the tube, than when traversing through the wider part of the instrument."

Bearing in mind that the blood is rushing to the heart, in obedience to the force of gravity uniformly acting; it will be readily seen that the simple effect of inspiration and expiration will be to quicken or diminish the velocity of the blood, according as the vessels are rendered narrower or wider by these actions.

variations in the rate of the venous current, to which it is liable in sudden alterations of the *positions of the body*. In sitting down, for example, or running, the rate of the venous circulation, in the lower extremities, must be instantly diminished, because two angles are directly formed in the venous tubes, at each of which, from the increase of the friction between the blood and veins, the blood will directly move more slowly, and, rebounding, give a retrograde tendency to that beyond; the effect of which is instantaneously prevented by the closure of the valves.

Accordingly, we find that valves are only found in those vessels where these abrupt variations in the rate of the current can occur. In the great cavities of the body—the cranial, thoracic, and abdominal—they are wanting; for no greater change in position than a slight and gradual curve—and, hence, no more than a very *slight* impediment to the stream—can be produced.

The hydraulic law, which is here illustrated, is thus referred to by Dr. Golding Bird:—

"Friction is found to take place between solids and liquids. A stream of water is always more rapid in the centre than at the sides, as, being deeper there, the current flows on the surface of lower strata of fluid; whilst, in the shallower portions of the river, the water is exposed to the friction of the rough and unequal bottom. In the escape-pipe of a fountain, a similar fact is observed; *for if it be bent abruptly, and not with a regular and gradual curve, the passage of fluid becomes much obstructed.*"—Natural Philosophy, sec. 201.

The next point to be considered is the means by which the venous current is rendered continuous, and all reflux from the auricle, during contraction, prevented. I think I am not mistaken in saying that physiologists have been in the habit of considering that a momentary arrest to the progress of the blood, in the veins, accompanies each auricular contraction, attended by a partial reflux from the cavity;* but, if this be true, how is it that the ventricle becomes filled with the contents of the auricle at each contraction? To allow for a reflux, the auricle should be more capacious than the ventricle (especially when we remember that the latter cavity is actually *distended*, not simply filled, by the contraction of the former); but the exact converse is the fact,—the ventricle is more capacious than the auricle; and hence, in place of a reflux, an addition to the entire contents of the auricle is required before the cavity of the ventricle can be efficiently loaded with blood. This, therefore, is a capital objection to the probability—nay, possibility—of any reflux occurring.

Moreover, such a view is out of harmony with the idea that animal mechanism is without a flaw. We cannot deliberately say this of a movement which, designed to effect propulsion, fails in giving it completeness. If the blood

* "The effect of this contraction of the auricles is to propel *nearly the whole* of their blood into the ventricles. The reflux of the blood into the great veins is hindered by the simultaneous contraction of the muscular coats, with which they are provided for some distance before their entrance into the auricles; a contraction which, however, is not so complete but that a small quantity of blood does regurgitate, *i. e.* flows backwards into the veins, at each auricular contraction."—Kirkes's Hand-Book of Physiology, p. 79.

regurgitates at each contraction of the auricles, and is compelled to trace and retrace the ground over which it has just flowed, then it is a movement less perfect than could be effected by man.

In explaining the action of the heart, the influence of the hydraulic law referred to, in the description of the portal circulation (p. 155), appears to have been overlooked. I think we shall find it sufficient to remove every difficulty, while it shows us that the venous circulation must be a continuous current.

In accordance with this, whenever friction takes place between fluids in free motion, the particles of one will, under certain circumstances, be carried forwards by the particles of the other—the smaller current by the larger. Now, we have seen, in a previous section (Sec. XI, Part I.), that, as the auricle of the heart contracts, the ventricle dilates, forming a vacuum in its interior; a movement which is attended with a rush of blood into its cavity, "such being the place where the least resistance is offered to its flow." Now, it is impossible for this current to pass from the auricle without a considerable amount of friction between the particles leaving the cavity and those which are entering its mouths, from which it will result that a portion of the latter will be swept by the former into the ventricle, forming the surplus quantity of blood required to produce full distension of this cavity. No reflux can take place, because the contents of the auricle are drained, as it were, by the constantly increasing vacuum which forms in the ventricle during its dilatation.

Hence, we see, that the action of the heart is perfect. The last remaining difficulty disappears; and also, that in obedience to this simple law, the blood in the veins courses onwards in one continuous stream.

We are now to apply the information we have gained to the explanation of the motion of the lymph, in the animal body. It has been already stated, that the thoracic duct empties itself at the angle formed by the union of the internal jugular and subclavian veins; and it is obvious that this again must depend upon the friction between the fluid particles of blood and lymph. Dr. Golding Bird, in his "Natural Philosophy," adduces it as one of the instances of "this hydraulic arrangement in animal structures."

Now, as the termination of this vessel is very near the heart, its contents will meet with very considerable friction from the blood in the adjoining veins, where the rush is of a very rapid character.* And, when we remember that this forcible current is continuous, ceaselessly moving through the whole of life, we shall find ourselves unable to combat the assertion, that a stream of lymph must, at this point, continuously, and pretty rapidly be mingled with the blood.

The design likewise manifested in the prevention of reflux, adds a persuasive to the adoption of this view. Had the flow been less continuous, we should not have found the thoracic

* The reason should be kept in mind—the attraction of gravity increases, as the distance from the source of attraction diminishes. The heart is here the indirect source; the blood is close to it, and hence the force of attraction in this position must be strong.

duct furnished with so many valves.* Their presence evidently shows a susceptibility to those conditions, which would oppose a ceaseless onward movement in this vessel.

We may apply this reasoning further. It is applicable to all the other terminations of the lymphatic system. Whereever a small lymphatic vessel joins a venous trunk, there must be a continuous current of lymph from the former into the latter ; and in each of the innumerable glands with which this system abounds, a slow current of communication must be in untiring motion.

These facts are quite at variance with the idea that the lymphatics absorb a few materials scattered through the body —they prove to us that a current of liquor sanguinis is constantly entering the venous system at almost every point. The experiment of Bidder is confirmed, and its truth demonstrated. Indeed we may say that the conclusion which it furnished errs in not going far enough, rather than going too far. From observations on the current in the thoracic duct only, it was inferred that it might furnish a daily stream, equalling or exceeding the entire mass of blood; but when we know that this is only *one* stream to *one* (and that the smaller)

* As the thoracic duct is principally situated in the chest and abdomen, the presence of valves in its interior, might, at the first glance, be regarded as a fact at variance with the opinions expressed of the uses of these structures. But it rather confirms them ; for the thoracic duct mounts into the neck, and thus its current is liable to be affected by its various *positions* ; and also, by the varying force of the venous current in unison, with the *position* of the upper extremities. Immediately, therefore, we find valves placed upon the vessel, although situate where, under other circumstances, they would be absent.

division of the venous system; that numerous, perhaps innumerable channels of communication exist, we know not by what numbers rightly to multiply the amount which is daily furnished. Many thousand times the mass of blood passes through the heart in a single day—many hundred times its mass of lymph probably enters the venous current in the same period.

It is almost surprising that we have not been looking out for such an active circulation, considering that it exists in the lower divisions of the vertebrate kingdom. The lymphatic system in Reptiles is well known to be provided with pulsating sacs, termed hearts, beating about sixty times in a minute,* and furnished with valves, that an efficient onward current may be secured. If such be the case with the cold-blooded animals, whose sanguiferous system is so inferior to the mammalia, and the lymphatic much less developed; surely it must be still more active in the higher divisions, where both arrive at their highest degree of development. To conceive the contrary, would involve us in a maze of difficulties, from which we should find it hard to get free.

VI.

A summary glance at the facts with which we are now furnished, will perhaps enable us to discover at what they are pointing.

* If the lymphatic and sanguiferous system are not continuous, where can the lymph be obtained to keep in this active motion, four hearts, the number with which the frog is provided, untiringly, day and night?

(1). We have evidence that the lymphatic system of vessels possesses numerous communications with the venous system—and that at these points the fluid current is from the lymph to the blood vessels.

(2). That a large quantity of lymph (a fluid closely resembling liquor sanguinis) is daily poured into the blood vessels, out of all proportion to the waste of the body, or food or liquid received, during the same period.

It seems scarcely possible as our minds receive this evidence to see any other conclusion than that the lymph is derived directly from the blood, and that a current of liquor sanguinis, free from red corpuscles, is incessantly circulating through the lymphatic system.

A junction, therefore, must exist in the capillaries by channels sufficiently minute as to exclude the larger globules.*

In this way, and I believe this way only, may the various facts, which an inquiry into this system has thus far furnished us with, be reconciled ; and when we see the need for such a circulation, we shall be the more readily convinced that this must be the correct explanation.

Firstly, let us dwell for a moment on the effect which such

* We have before said, that a communication has been established between these vessels by injection—of the correctness of which the facts furnished in the text scarcely permit us to doubt,—moreover we may add the negative fact, that anatomists who deny this communication, are far from being clear as to the mode in which the lymphatics commence. Dr. Sharpey, in speaking of the deep origin of the lymphatics, says, "that the precise mode in which it takes place is not known."

a current must produce. A continual stirring or intermingling of the blood,—numberless minutely divided streams are continuously and incessantly leaving, and as continuously and incessantly entering the columns of blood,—from which must result a constant interchange of the fluids of one part with the fluids of another.

And such an arrangement is obviously required. The blood is a fluid in which various substances of different densities are mechanically intermingled. As such it is undergoing perpetual and irregular changes—some of its materials are removed, in one direction—others in another. From two different viscera the blood can never return alike; and each of these currents will again be at variance with the streams into which they run. Hence the necessity for the apparatus we find in continual motion by which fresh liquor sanguinis is continually remixed with the altered blood.

And new blood (if we may so term the chyme) is thus cautiously and thoroughly mingled with the old, and by the same system of vessels. It is well known that the lymphatics of the abdomen, the lacteals, as they are here termed, are in the intervals of digestion filled with lymph; and the opinion seems to be generally entertained that, during digestion, they contain nothing but chyle. But argument seems wanting in support of such a view. Its adoption requires us to believe, that the function of the abdominal lymphatics is in abeyance during digestion, that these vessels are suddenly diverted from one office to fulfil another; of which there is no proof on the one hand, nor does it tally with analogy on the other. Moreover the chyme is semi-fluid, and consistent, incapable, we may say, of circulating; while the chyle is liquid and possesses this power, leading us, not unnaturally, to infer some dilution. From what we have learned of the continuous circulation of lymph, it seems pretty certain, that the fluid in the lacteals during digestion is a mixture of lymph and chyme. Its composition is in precise accordance with such an idea, while it is agreeable with all other facts.

It will not be difficult to explain the mode of such an admixture. Let us suppose that a current of lymph continues to circulate in the interior of each intestinal villus, during digestion, while the chyme coats its outer surface, and that channels of communication unite the two,* the latter will be gradually drawn or pumped from the exterior to the interior of the villi, where it will slowly and intimately mingle with the lymph, and appear as the fluid known as the chyle.

* I am aware that Physiologists of the present day are disposed to question the existence of orifices on the extremities of the villi, and channels of communication for the admittance of chyme into the lacteals, but I cannot divest myself of the opinion that the views of Lieberkuhn, Cruikshank, and John Hunter, three of the most eminent observers, are correct. An opposite opinion seems to have been entertained from the fact that the lacteals seem imbedded in a multitude of cells, which are thought to elaborate the chyle and transmit it to their interior; but there is no proof whatever that any elaboration takes place in the intestinal villi, or that the chyle, supposing it to be subject to this process, presents any characteristics which may not be accounted for on the supposition of dilution, as maintained in the text. Moreover, we shall find reason to believe that the cells are *within* rather than *without* the lacteal vessels, and if the probability of this can be shown, it will, by nullifying the views at present held, remove a main objection to those of the celebrated Physiologists mentioned. The communication by orifices has the recommendation of simplicity, and of being most in accordance with the works of the Creator, who invariably effects his ends by the least

This is very carefully received into the blood. The surface over which it is spread comprises 150 glands or more, and through their pores it percolates in numberless invisible streams; but a certain portion is doubtless passed into the blood within the intestinal villi, where the union between the capillaries and lacteals is very intimate. So much so, that it has been thought (with other considerations) to countenance the idea, that the chyle was secreted from the blood vessels, or, in other words, that a current existed from them to the lacteals. Each intestinal villus may, I think, rightly be likened to a lymphatic gland, slightly modified, the afferent lymphatics being represented by the channels of communication from the blood vessels (which are elsewhere gathered into larger vessels, and then pass to a lymphatic gland), and from the orifices on the external surface of the villi—while the efferent are represented by the "lacteals" leaving the villus. The plexus with its close communication with the capillaries may be likened to the plexus of lymphatics in their proper glands, possessing, like them, a free communication by pores.

As soon, therefore, as the chyme has entered the lacteal plexus of the villi, it will, as chyle, be acted upon by the preponderating force of the blood in the capillaries for the reasons which have already appeared in explaining the current

complex means. It is to be expected that such orifices, or channels, as they not improbably are, should be most difficult of demonstration; for like all other vessels endowed with the contractile power, which the lymphatics possess, they will contract to obliteration so soon as the current in their channels is arrested, while their size, even when dilated, is probably extremely minute; another great difficulty, in the way of demonstration.

in the lymphatic glands; and under which a large portion of its contents, (bearing a relation to their consistence), will slowly pass into and become mingled with the blood. A considerable portion however must pass onwards, to be subjected to a similar action in the "mesenteric glands."*

VII.

I need hardly tell the Physiologist, that the main difficulties, connected with the lymphatic system, have yet to be cleared away—I allude to the fibrin-elaborating function supposed to be effected in the glands, and the development of lymph corpuscles, which seems here to take place—I have purposely abstained from alluding to them hitherto, that the subject might not be uselessly encumbered; nor must we now change

* If this view be held to be the most consistent with facts, it may yet be a subject of controversy whether the veins of the intestinal villi in vertebrata, absorb *directly* any materials from without, or whether they are not in the first place received into a lymphatic plexus, and subsequently into the capillaries. The thin soluble substances would, under such circumstances, rapidly pass into the blood, while the "finely divided" portions must make a longer round. Thus we should have beneath the absorbing digestive surface, a slow moving current, licking up all materials capable of traversing its pores, which it more or less directly passes to the sanguiferous system, itself principally put in motion by the stronger current of blood spread on the external surface of the villus, and with which it directly communicates. Thus the moving organ of the circulation becomes, though not in the way generally considered, the "true cause of absorption." Such an idea meets with considerable support from observation, for "the recent experiments of Mr. Fenwick (Lancet, Jan. Feb. 1845) show that the lacteals will not absorb alimentary matter from any part of the intestinal canal, *in* which the blood is not circulating."—Carpenter.

our plan. If we hope to unravel this rather intricate subject, we must patiently untie each knot as we proceed.

We shall find ourselves furnished with information, which will throw much light upon this subject, if we previously inquire into the nature, uses, and modes of action of the blood globules. "These are of two kinds, the red, and the colourless; the former are not spherical, as the name 'globules' by which they have been so generally designated, would seem to imply, but flattened or disk-shaped. Those of the human blood have a nearly circular outline like a piece of coin; and most of them also present a shallow cup-like depression or dimple on both surfaces; their usual figure is therefore that of biconcave disks." * The colourless globules are "of a rounded and slightly flattened figure, rather larger in Man and Mammalia than the red disks." The red globules are by far the most numerous, and both kinds are swept along in the liquor sanguinis, to every tissue of the body.

The red globules firstly require our attention. Dr. Carpenter says, and apparently with much reason, "that they are actively (but not exclusively) concerned as *carriers* of oxygen from the lungs to the tissues, and of carbonic acid from the tissues to the lungs; and that they have no other direct concern in the functions of nutrition, than the fulfilment of this duty."—Manual, sec. 223.

If such be their functions, we must look for some mechanism

* Quain and Sharpey's Anatomy, p. lxv.

by which they may part with their carbonic acid in the lungs, and become laden with oxygen; and on the contrary, part with their oxygen to the tissues, and become laden with carbonic acid, for we have no right to believe that cells *in*, any more than *out*, of the body, can dispense with the process of filling and emptying, when engaged in the conveyance of materials.

In the lungs, where the globules discharge their carbonic acid, and become in their turn charged with oxygen, the mechanism can most satisfactorily be studied.

As soon as inspiration commences the red globules in the lungs will be acted upon by two forces; one, the ordinary force, by which they are drawn towards the heart; the other, the special, or additional, by which they will be drawn towards the commencing vacuum in the lungs; for being freely moveable in the blood, they must share, with the atmosphere, the disposition to fill the vacant space.

The force by which they are drawn to the surface of the lungs will exceed that which leads them to the heart, for, as before mentioned, the attractive force of Gravity is greater, in proportion as the distance between the bodies attracted, is less; the globules are close to the vacuum which forms in the lungs, while they are distant from the vacuum forming in the left auricle.

Thus simultaneously, with commencing inspiration, the red globules will bear, with a pretty considerable force, against the surfaces of the blood-vessels, which are in juxta-position with the cavity of the lungs, and they will arrange themselves with their flat surfaces towards the cavity; as the centre of gravity, which is the centre of the globule, always finds itself nearest to the attracting mass, when nothing interferes with the freedom of motion.

But the surface of the air-cells gradually increases during inspiration, and as this enlargement is unshared by the red globule, a gliding movement of the surface of the former will take place upon the latter; and, as the force by which the globule is borne against the lung surface, continues during the entire movement, the latter, at its termination, will be found adherent to the membrane, for when two surfaces are closely pressed together, with a slight motion between them, they will adhere.

The necessity for this adhesion becomes apparent when we remember that, as soon as the inspiratory movement ceases, the force by which the globules are drawn to the lung surface ceases also, and the second or ordinary force, by which they are drawn to the heart, comes into play; hence they will instantly leave the surface of the lungs, whereas a momentary delay is required in order to receive their charge of oxygen, and deliver their charge of carbonic acid. This interchange of gases can only be effected at this moment, for, according to the law of diffusion, the gases must be under equal pressure, which state only obtains at the *acmé* of inspiration.

The stretching of the lung surface probably opens a multitude of pores, which again close, as it collapses. The red globules are doubtless provided with similar pores, and thus, at this moment, a free channel of entrance and exit will be afforded to the gases, which their instantaneous diffusion requires. Such pores in the globules will not be inconsistent with the complete retention of their contents, all loss being guarded against by the rapidity of their movement.

It will be difficult to match the simplicity and beauty of the mechanism by which, so soon as the required interchange is effected, the adherent globules lose their attachment. The cohesion is destroyed by the shrinking of the membranous surface of the lung, which movement being unshared in by the red corpuscle,* it becomes peeled, as it were, from its connection. The force by which it was borne against the surface, has also ceased to act, and therefore every obstacle to its onward movement.

Thus, millions of globules, which an instant before, forcibly adhered to the surface of the air-cells, have lost their hold, and each is swept to the tissues with its load of Life.

A slight modification of the mechanism will exist in the tissues, where the red globules become laden with carbonic acid in lieu of oxygen, and this modification is adapted to the altered circumstances in which they act. Two forces will, however, be still found in operation. Let us take a fat cell, for example, and suppose a vacuum be formed in it by the formation of carbonic acid, the globules in the neighbourhood will be instantly arrested and applied, with their flat surfaces to the cell, in accordance with the laws which have already been explained.

No sliding motion between these surfaces can occur, because the fat cell from its shape retains its size without

* The attachment and separation of the red globule from the surface of the lungs I consider to be analogous to the attachment and separation of the placenta from the uterus.

alteration (notwithstanding the diminution in the density of its contents) when the vacuum is formed. Nor does it appear to be required, for the vacuum being formed at once and directly, and not continuously, as in the lungs, and the red globule being simultaneously applied to the surface, the interchange may be at the same moment effected, provided that channels of communication exist (which we may fairly assume), for the cavity of each will be directly continuous, and the contained gases under the same degree of pressure.

But the vacuum in the cell still continues, and into it the oleaginous globules of the blood will be forced under atmospheric pressure; the motion must be slower than the preceding one, hardly, if at all, commencing before the completion of the former, for the movement of fluids, under equal forces, is much more tardy than that of gases. As the cell gradually fills, the force by which the red globule is attached, gradually diminishes, till at last it is out-balanced by that which draws it to the heart. It then separates from the membranous surface, and passes with its load of excretion to the lungs.

In this way, the mode by which the red globules part with their oxygen, and receive carbonic acid in exchange, may be satisfactorily explained in all cells, the size of which does not vary, with the formation of the vacuum in their interior.

In the muscular fibre, the plan adopted seems a copy of what has been examined in the lungs.

It has been seen, that when the *vacuous state* has been induced in this fibre, the atmosphere directly bears upon the moveable cell wall; but it must also bear, and with equal force, upon all other moveable cells in the vicinity, and hence upon the red blood globules. These latter will, therefore, be brought to bear, with a certain degree of force, upon the delicate *myolemma*, or covering of the muscular fibre. It does not appear, however, that they can directly interchange their oxygen, for the carbonic acid, which has just been formed for their cavities, are probably not directly continuous, for a reason which will be given; and, till this occurs, the pressure upon the two gases must be unequal, that on the carbonic acid gradually increasing with the diminishing size of the cells.

By what provision, then, are the globules attached to the myolemma? for if they be not, before the motion in the muscular cells have ceased, the force of the heart's action will carry them onward, before the interchange of gases has been effected.

We have seen, p. 51, that during the contraction of a fibre, the fluid, which was previously insinuated between the fibrillæ, is forced to the surface, and causes the *myolemma* to be gathered into little bladders in this situation. At some parts of the circumference of a fibre, this, as is there shown, will occupy the otherwise vacant spaces which the zig-zag folds must form ; but where the delicate capillary comes into contact, these bullæ must, during contraction, gradually project into its interior, and gradually retreat during relaxation, a movement precisely similar to the enlargement, and diminution, of the air-cells in the lungs.

The effect upon the biconcave blood disks must also be similar. The gliding of the delicate myolemma and capillary coat, will attach the globule, so that when the pressure is equalised by the inertia of the muscular cells, the gases are capable of exchanging by mutual diffusion—and we may assume, that only *at this moment* channels of communication * exist between the interior of the myolemma and the cavity of the globule; because the action *must* be perfect, and perfection as much demands that a movement shall not take place *before*, as that it shall not take place *after*, the exact period required. Relaxation, either partial or complete, immediately follows: accompanied in either case with the retreat of the bullæ. The former movement is simply attended with a change of place in the fluid by which "the transverse diameter of the cells will be diminished to the extent they were previously increased,"—(p. 52) the latter by the free influx of fluid. This neutralises the force which *in complete* relaxation would otherwise bear upon the red globules; and as in the lungs, their surfaces do not share in the movements of the *myolemma*,† they become detached, the single force of the heart

* We may fairly assume, also, these channels of communication, because so far as our reason points, no material substances (gases not excepted) can pass from one vessel into another without *real* not *ideal* orifices of entrance and exit. I have already said that I regard the attachment, and separation of the red globule, to be analogous to the attachment and separation of the placenta; I may further liken the adaptation of the orifices for the transmission of the gases in the former, to the like adaptation, for the passage of blood in the latter.

† It seems by no means improbable, that the *myolemma* may possess a contractile power, excited by the pressure or "stimulus" of the fluid forced from between the fibrillæ; and that while its enlargement is of a *passive* character, *as far as its own tissue is concerned*, its diminution in size is *active*.

comes into operation, and under it they seek an outlet for their load of excretion.

Hence the red globules are true "carriers" of their contents; and *in* the body, as *out* of the body, require to go through similar processes of emptying and filling. Each cell may be compared to a little membranous boat, sailing through the liquor sanguinis, unlading carbonic acid and lading oxygen in the lungs; and the converse, viz., lading carbonic acid, and unlading oxygen in the tissues.

VIII.

We now pass to the mode in which the white corpuscle receives, and parts with, the materials which it carries. The facts, which we have just examined, are calculated to throw considerable doubt upon the generally received opinion, that they lose their contents by rupture or deliquescence; they would rather lead us to expect some analogy between their action and the red corpuscles.

But this analogy cannot be complete; the latter globules undergo no change in size during their action, because they receive the same volume of carbonic acid, for the same volume of oxygen, and *vice versâ*; but as there is no reason to believe that the white corpuscles receive anything in exchange, they must diminish in size, when they part with their contents, and lost their character of globules.

We have then to inquire, not only into the mode in which this emptying of the white corpuscle is effected, and by what it is determined, but also to look for some mechanism by which it is refilled. We must try and trace this small unnoticed corpuscle by our reason, as we cannot by our sight, from the spot where it has yielded up its load to the reservoir, where it is again refilled.

Although the white and red corpuscles are generally spoken of as if they were indiscriminately mingled with the blood, yet in the circulating current they have been observed to occupy different positions. "In the capillaries of the Frog's foot," Dr. Carpenter observes, "they occupy the exterior of the current, where the motion of the fluid is slow, whilst the *red* corpuscles move rapidly through the centre of the tube.* The colourless corpuscles, indeed, often show a disposition to adhere to the walls of the vessels, which is manifestly increased on the application of an irritant. Hence the idea naturally arises, that (to use the words of Mr. Wharton Jones) there is some reciprocal relation between the colourless corpuscles, and the parts outside the vessels in the process of nutrition."

* This separation of the red globules from the white, and their occupancy *en masse* of the centre of the current, must be governed by some law. Their higher density would almost lead us to look for a reversal of this position ; and to see the heavy red globule, more frequently in the slow current, which, for the increased friction, must always be found near the walls of the vessel. May not the fact as it stands, be due to that peculiar attraction which I think we may safely call magnetic, by which the red globules adhere in piles, when removed from the vessels. There can be no question of the existence of this property in the living body ; and it must act in two ways—Firstly, it will oblige the red globules to cut the liquor sanguinis with their thin edges ; any tendency to present their broad surface to the current which the numerous eddies in it might occasion, being resisted by the attraction of those before and also of those behind. Secondly, it must effectually separate the different globules, by collecting into one stream those only which possess this mutual attraction.

This disposition of the white corpuscles to connect themselves with the walls of the vessels, which observation unquestionably proves, is a very valuable piece of evidence in support of the views which are here sought to be established. Indeed our attention may be simply directed to an examination of the mechanism of this connection if such high authorities as Dr. Carpenter and Mr. Wharton Jones, are convinced that some relation exists in nutrition "between the colourless corpuscles and the parts outside the vessels."

It is plain, from what has preceded, that the white corpuscles occupy a position in the blood-vessels which must render them extremely sensitive to those variations of pressure which the constantly recurring vacua in the tissues must occasion, and it is equally plain that they are too large of themselves to pass through any pores in the blood-vessels upon the occurrence of such pressure; its simple effect must be to arrest their progress, and press them against the walls of the vessels, in accordance with the laws which we have already examined, when treating of the red globules; but here the similarity ends; the colourless globules, unlike the red, have to yield, in place of exchanging their contents; and, as there are, doubtless, a sufficient number of orifices by which this can be freely and effectively done, the pressure upon their surface must produce an immediate discharge of the fibrinous material which they carry into the surrounding blood, and which may either enter the tissues or occupy the place of some other material, which has done so more speedily, from its passing the pores of that particular spot with greater facility. No attachment or adhesion between their walls and the walls of the blood-vessels, as in the case of the red globules, can occur, because this can only be effected between two *smooth* surfaces, while the coats of the white corpuscles are *rough* or granulated.

The globule thus emptied must shrink into a small point or spherule, and in this state pass onwards with the current. In this state also it will readily pass with the liquor sanguinis, into the lymphatic system, for, coursing along the sides, in preference to the centre of the vessel, where, as we have said, the movement is slow, it must be carried with the issuing stream.

It will be arrested in its movement at the first lymphatic gland, for here, as we have shown, the motion of the liquor sanguinis or lymph becomes diminished; and hence the spherule, which it carries. The greater number of these will be carried with the preponderating current towards the blood-vessels through which the liquor sanguinis seems to pass through a multitude of small pores. These, if sufficiently small, will obstruct the passage of the spherules, and cause them to collect * just as leaves, branches, and other matters in a stream collect at a point, where the water is slightly dammed. This will, however, not be sufficiently close but

* This state of things is precisely what observation has detected in the lymphatic glands. "We find," says Dr. Carpenter, "it (the epithelium) composed within the gland, *of numerous layers of spherical nucleated cells;* of which the superficial ones are easily detached, and appear to be identical with the cells that are found floating in the chyle." Dr. Sharpey says, "according to that anatomist (Mr. Goodsir), the lymphatics within the gland lay aside all but their internal coat and epithelium, and the latter, in place of forming a thin lining of flat, transparent scales, as in the extra-glandular lymphatics, acquires an opaque granular aspect, and is converted into a *thick-irregular layer* of spherical nucleated corpuscles measuring on an average $\frac{1}{5000}$th of an inch in diameter."

that the stream of liquor sanguinis can find its way; the tubercles of the spherules preventing too near a contact, while at the same time they must tend to hold these globules in temporary connection.

But the lymphatic force, (if we may so understand the strength of the current in the lymphatics, in opposition to the current in the blood-vessels, or venous force,) though overcome is not destroyed; it must prove a power continually removing the pressure from the surface of these globules, and hence, by occasioning a slow continuous vacuum in their interior, enable them to be filled* with those materials which are capable of traversing their pores; as they enlarge they will gradually separate themselves from their connection, and be carried onwards by the larger current.

It is most probable that albumen is pumped into their interior, and it is not a far-fetched idea to suppose that it is introduced in such a finely divided stream, as to give it afterwards the character of fibrin, just as a brittle rod of glass can be transformed into a waving life-like plume, by drawing it into threads of extremely minute diameter. Whether this alone is sufficient, the judgment of each person can decide, but physical changes are capable of producing such an utter transformation of matter, that we may fairly require that a large amount of results be attributed to them, in Animal Mechanism.

* Several examples of this mode of filling cells will be found in section XXI, Part I, which is especially devoted to the subject. The reader must bear in mind, that I have there taken, by way of illustration, the generally received views of "nutritive absorption," but which he will see from the text, I do not entirely coincide with.

In this way I would explain the source of the corpuscles, which are well known to abound in the lymphatic and mesenteric glands,* and also why "the lymph and chyle contain a greater proportion of fibrin, and are consequently more perfectly coagulable after passing these glands"—(Sharpey.) They abound then with colourless corpuscles containing fibrin, refilled with which they seek a re-entrance to the mass of blood. It is most probable from what has previously appeared, that all which are filled below the heart, allowing a certain number for the neck, pass more or less directly to the venous current.

Although the white corpuscles are occasionally emptied in the tissues, yet this process must be principally effected in the lungs, where the exchange of the carbonic acid of the red globules, for oxygen, likewise takes place on the grand scale. At every inspiration, the white corpuscles of the lungs must be simultaneously emptied, from the disposition of the fibrinous fluid they contain to fill the gradually increasing vacuum in the chest, and thus we can satisfactorily account for the increase of fibrin in arterial blood, or that which has traversed the lungs.

It is not only, or perhaps even principally, in the lymphatic glands, that the white corpuscles of the blood are filled. There are several organs in the body which appear to be specially set apart for this purpose. I refer to the vascular glands, the spleen, supra-renal capsules, thymus, and thyroid glands.

* The cells, which have been observed, in the intestinal villi, have no doubt a similar origin—hence they are *within* the lymphatic plexus, where they are filled in the manner which has been previously described.

In the Manual of Physiology, by Dr. Carpenter, and also in the Principles of the same author, he has given many reasons for the belief—original on his part—that these glands are used for the elaboration of fibrin, by the development of white corpuscles, which draw the materials on which they grow, from the albuminous matter of the blood. The following is the summing up of this able Physiologist.

"The spleen, the supral-renal capsules, the thymus gland, and the thyroid gland, all seem to share in the preparation of the nutritive materials of the blood, along with the ordinary glandulæ (the lymphatic and mesenteric glands) of the absorbent system. In fact, we may regard them all as, together, constituting an apparatus which is precisely analogous to that of the ordinary glands, but of which the elementary parts are scattered through the body, instead of being collected into one compact structure. Thus, if we could imagine any tubular gland, such as the kidney or the testis, to be unravelled, and its convoluted tubuli to be spread through the system, yet all discharging their contents by a common outlet, we should have no unapt representation of the lymphatic portion of the absorbent system. Its function appears to be, to separate the crude albuminous matter from the blood, to subject it to an elaborating action performed by the epithelium cells lining the tubes, and then to pour forth this elaborated product—not as an excretion to be carried out of the body, but (in conjunction with that which has been newly taken in by the lacteal portion of the system, and which has undergone elaboration by its glandulæ) into the blood-vessels which are to convey it to the different parts of the body where it is to be appropriated. The four bodies mentioned above appear to be, so far as their glandular function is concerned, appendages to this system."—Carpenter's Principles of Physiology, third edition, sec. 690.

It will be out of place to enter into a close description of the anatomy of these bodies; they all abound in lymphatics, in blood-vessels, and in globules. It may shortly be said of them, that they consist of tubes, either straight (supra-renal capsules), or convoluted (Malpighian bodies of spleen), or of vesicles (thymus and thyroid glands), all of which, with the exception of the Malpighian bodies of the spleen, appear to have no orifices of outlet; their cavities contain a fluid, together with very small globules, and others more fully developed; while the exterior of the tubes or vesicles are covered with a very minute plexus of blood-vessels.

One is naturally struck with the marked resemblance in general type, which these organs bear to a common lymphatic gland. It, as we have seen, consists of a convoluted tube or tubes, containing a fluid, furnished with minute spherical corpuscles, and others more fully developed; while, on the exterior, blood-vessels are minutely distributed. The important difference consists in the fact, that the tubes and vesicles of the vascular glands are described as closed.

It is difficult to believe that this is really correct. Surrounded on all sides by fluid currents, filled with corpuscles, which are actively needed in various parts of the body, and resembling in other respects, both structural and functional, the lymphatic glands, through which a continuous stream is passing; the persuasion seems forced upon the mind, that these cavities are not so isolated as their complete closure would oblige us to admit; but rather that they are dilatations or enlargements between the blood-vessels on the one hand and the lymphatics on the other, where the motion of the liquor sanguinis is arrested, in order that the empty little corpuscles may be refilled.*

IX.

It is a well ascertained fact that the union of the carbon and hydrogen of the food with the oxygen absorbed by the lungs, is the cause of the warmth of animals, although the

* It is interesting to observe, in connection with the idea, that the vascular glands receive empty corpuscles from the blood and fill them, how abundantly they are loaded with small cells or molecules. Dr. Carpenter, in speaking of the supra-renal capsules, says "the interspaces of the venous plexus are filled with a sort of pulp, *consisting of minute spherules, averaging about* 10,000 *of an inch in diameter, but varying from nearly twice that size to less than half.* These bodies appear to be the *nuclei* of cells, the full development of which is checked; but in the ruminant animals, and occasionally in the human subject, *the cells are more or less developed, and then resemble the ordinary lymph corpuscles in size and appearance.*" Again, with regard to the thymus gland, "the cavities of the follicles contain a fluid, in which a number of corpuscles are found, giving it a granular appearance. *These corpuscles are for the most part in the condition of nuclei*; but fully developed cells are found among them, *at the period when the function of this body seems most active.*" Further, in speaking of the peculiar parenchyma of the spleen, "this seems to be made up of reticulations of blood-vessels, and lymphatics, with a larger quantity *of minute globules or incipient cells,* of about half the diameter of blood corpuscles, which lie in the meshes of the capillary net-work, and which seem to be in intimate connection with the lym-

precise mode in which the union takes place has been hitherto undetermined.

The condensation occurring in the formation of carbonic acid, appears to afford the desired information, and to point out the *most probable source of animal heat.*

Heat is always evolved in the condensation of gases, while cold is the invariable result of their expansion; indeed, this is a rule applicable also to the various conditions of matter.

When, therefore, under the required conditions, the union of oxygen and carbon takes place in the animal body, a certain amount of heat is evolved, simultaneously with the condensation; a fact which remarkably tallies with the increased production of heat, which invariably attends an increased production of carbonic acid. To this evolution, therefore, animal heat seems to be due; and it is a process which is incessantly occurring in the organism. It would be difficult experimentally to prove this fact, because carbon in the form of gas has not yet been formed, and hence, we are unable to determine the amount of heat, which it evolves in condensation; but it is better to assume an explanation most in accordance with ascertained facts, than to use a term like combustion, which gives no information.

I think there can be little doubt that the union of the carbon and oxygen occurs in the interior of the fat cells

phatics;" and lastly in regard to the thyroid, "its vesicles are surrounded, like the vesicles of the true glands, with a minute capillary plexus, and in the fluid they contain, numerous corpuscles are found suspended, *which appear to be cell-nuclei, in a state of more or less advanced development.*"

themselves, as I have already briefly stated; and I have also shown the mode in which, I conceive, the fat cells are refilled both with oxygen and hydro-carbon, and the mode also in which the carbonic acid is removed. An intense disposition in these elements to unite, in all probability, exists; but it is not easy to see the *immediate* cause of their union. Perhaps it may be the constant movement of the cells upon each other; as this must be unceasingly occurring.

Each fat cell, therefore, is to be regarded as a little heating apparatus, acting independently. The fat seems never to be re-introduced into the blood after deposition, as many believe, but is burned *in the cell*, from whence its products are removed as soon as formed, while it is simultaneously re-charged with fuel.

X.

In the first section of this work, I stated that we should find the Law of Gravity directly active in the animal body, through the medium of atmospheric pressure. This force, it is well known, is due to the mutual attraction existing between the earth and the air, and hence the movements which it produces are directly attributable to Gravity.

This force is in ceaseless operation during the whole period of life; indeed, we cannot conceive the existence of life without movement. "The idea of life," remarks Dr. Carpenter, "in its simplest and most correct acceptation, is that of vital action; and obviously, therefore, involves that of *change*. We do not consider any being as *alive* which is not undergoing some continual alteration that may be rendered perceptible to the senses."

My idea of a living organism is this: a structure, into which certain materials are introduced through different channels, which have the power, under certain conditions, of combination and condensation. That they are brought, under such conditions, within the animal mechanism; the movements of which are then produced by the attraction of the earth acting through the atmosphere on the organic surface.

The object, therefore, served by the food and the oxygen of the air, is simply the production of motion within the organism, and that matter only is truly excrementitious which is incapable of producing further movement.

In describing the various functions, I have assumed that the process of growth is completed, and that the body is fully formed; but we cannot doubt that the successive stages of "development" are likewise effected by the same grand law.

The egg, which during incubation has furnished us with so much knowledge on this subject, gives us this proof; for we find it provided with an air chamber at one extremity; the purpose of which obviously is to provide the atmospheric pressure, which is essential to the first movement and subsequent development of the future being.

I do not refer to the views which are held regarding the uses of this chamber; for I think I shall be interpreting rightly such opinions when I say that their authors do not consider them sufficiently satisfactory.

It is pleasing to peruse the opinions of mathematicians regarding the primary motion of the universe at large, carried out on the no less marvellous universe of the egg. They suppose that two forces are in operation, the centrifugal and centripetal, by one of which the body would move off in a right line, by the other, at right angles with that line, and that their combination produces the curve.

Now, when incubation commences in the egg, two forces must necessarily come into play. The temperature is raised to about 100 degrees, which gives each molecule a tendency to separate from each other, in accordance with the ordinary effects of heat, while the air which undergoes a more complete expansion must act upon each molecule in direct opposition. As the air becomes more rapidly heated than the semi-fluid contents of the egg, it will be the force first brought to bear, and thus we have absolute proof that the *first movement* must be due to the Law of Gravity, and that movement one of propulsion.

Here we must leave the subject with two remarks. Firstly, that if the Law of Gravity be not capable solely and alone of producing animal motion, then it is an imperfect law. Its applicability to matter only reaches to a certain point, and there it fails. This is not the wisdom of God. Secondly, that it is easy to conceive how cells may be divided and sub-divided, materials added, and form given to a structure, by variations and combinations of the force of Gravity, for man completes his most exquisite pieces of mechanism with the aid of this force alone. But, however physiologists may be able hereafter to explain every step of the process of "generation" they will never be able to comprehend the incomprehensible wisdom which can so modify the force of Gravity, as it shall act as truly in the simple division of a cell invisible to the eye, while millions of other movements depending upon it, and upon each other, are at the same time in play, as in producing the harmonious motions of the planetary masses.

I cannot say that I have much fear for these pages. I am throwing them before men of intelligence, who, while they sift the doctrine and expose the errors they contain, will yet sternly maintain the truths. I cannot wish for more. If I have succeeded in establishing the leading fact, that Animal Motion is due to the one Great Law of Gravity, and that we are to bring every process to it for an explanation, instead of fettering our minds with ideas of "Vitality," I am content. The building has yet to be completed, and many hands may find employment at the work. These pages simply lay the FOUNDATION STONE.

APPENDIX.

I.

" The connection between muscular exertion and the exhalation of Carbonic Acid, is most remarkably shown in Insects; in which animals we may witness the rapid transition between the opposite conditions of extreme muscular exertion, and tranquil repose; and in which the effects of these upon the respiratory process are not marked by that exhalation of carbonic acid, which is required in warm blooded animals simply for the maintenance of a fixed temperature. Thus, a Humble Bee has been found to produce one-third of a cubic inch of carbonic acid, in the course of a single hour, during which its whole body was in a state of constant movement, from the excitement resulting from its capture; and yet during the whole twenty-four hours of the succeeding day, which it passed in a state of comparative rest, the quantity of carbonic acid generated by it was absolutely less."—Carpenter's Principles of Physiology, sec. 767, f.

II.

When the text was printed, it was my intention to have given here a short table of the difference between the pressure of the air at different degrees of temperature. But as it is not a matter of importance, I may as well refer the reader to those works which treat upon the subject.

III.

At page 92 it was hinted in passing, that the action of the nervous tubes might to some extent depend upon the diverse densities of the fluid. It seems not improbable that this may partly explain the correctness of their action. If the interior column be less dense than the exterior, it will under compression move more rapidly, which motion will likewise be assisted by the fact, that its friction upon the semi-fluid material which surrounds it, will be less than the friction of the latter against its membranous coating. By this arrangement, the internal column will carry along with it, as it were, the particles of the external one, they will occupy its place as it passes in advance, and thus prevent during compression, any dilatation of the tube.

IV.

As far as I am aware the uses of the ventricles of the brain are unexplained. I believe they contain air, and are required to produce the atmospheric pressure, without which it would be impossible for the blood to circulate. They perform the same office for the brain as the air-chamber does for the egg.

THE END.

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