SMITH (N.R.)

physiological essay on digestion.

Dr Dunglison,

from his obliged friend

Joseph Coolidge

Boston,

Feby 2.d 1826.

Bof

A

Dr. Dunglison.

PHYSIOLOGICAL ESSAY

ON

DIGESTION,

BY NATHAN R. SMITH, M. D.

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It is no small part of Science to be well acquainted with its real boundaries; but it is necessary also to know what it is which truly exists within these boundaries, and what it is which is only fabled to exist.

PREFACE TO BROWN ON CAUSE AND EFFECT.

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TO those whose talents and candour elevate them above the local and personal partialities and prejudices, which characterise too many of our profession, this Essay is respectfully inscribed, by one who relies only upon their unbiassed judgment, and whatever merits the work may contain.

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PREFACE.

I WILL not, in the cant language of authors, enumerate the embarrassments under which this small work has been prepared for the press. It would not, however, have been, at this time, submitted to the public but by the advice of intelligent friends.

The reader will discover that I am far from being orthodox in my opinions, but as there is nothing revealed on the subject of Physiology, I trust that I shall not be denounced as a heretic in medicine; certainly not, till this Essay has received a fair and candid examination.

I flatter myself that I shall at least call the attention to some important defects in the present mode of reasoning on these subjects. That I have a higher object in view, however, in presenting the following pages, I will not deny.

N. R. S.

INTRODUCTION.

IT is a common remark of those who compare the Physical and the Physiological Sciences, that while the former are rapidly rising upon the sure foundation of correct reasoning, and receiving additions, which must be co-existent with truth itself, the latter on the other hand, from time to time, exhibit the downfall of beautiful superstructures, which had been erected upon fallacious principles.

The circumstances which render the progress of Physiology so uncertain, appear to me to be the following: 1st, A disregard to the marked distinction which exists between these sciences; and 2d, The not determining with sufficient accuracy the real boundaries of the latter.

The employment of bare hypothesis, in place of fact, has indeed been for the most part abandoned, and physiologists now uniformly endeavour to pursue the inductive method, and to furnish themselves by observation or experiment, with less perishable data. It is too often the case, however, that the very first inference drawn from these facts is fallacious, and that, from not regarding the essential difference between the causes or properties, which produce the effects or phenomena of one science, and those which operate in another.

When we confine our inquiries to any particular science, with the laws of which we are acquainted, and reason from an effect to a cause, which in its operations we know to be controlled by those laws, there need be no fallacy. But if we enter another field of science, the laws of which we have not investigated, and reason from an effect to a cause, we shall almost certainly fall into an error, for we unconsciously attribute the effect to a cause with the operations of which we are familiar. Thus, if we combine two inorganic substances, and after a time, observe that a remarkable change has taken place in their properties, we at once, and with propriety, attribute it to the influence of chemical affinities; for we know that such substances are exclusively under the control of physical properties. But if we convey certain substances into the stomach of an animal, a polypus for instance, and upon removing them after a considerable time, discover that they have become very much changed, having received new chemical and sensible properties, it is altogether unwarrantable to infer that these changes are wholly the effect of chemical influence; for, as the stomach of this and of every other animal is indued with certain vital properties, and as we are not yet fully acquainted with their laws or uniform habits of action, we can not say how much, in this instance, is to be attributed to their influence.

That similar effects, then, in any particular science are referrible to the same cause, is generally true; but that similar effects in two distinct sciences, the cause being known in one, are referrible to the same cause, is, in principle, false.

The other circumstance mentioned as having retarded the progress of physiology, is the not determining, with sufficient accuracy, the limits of our inquiries.

In physics, philosophers have long ago abandoned the pursuit of final causes, and have contented themselves with observing and recording the uniform laws by which the properties of matter are exerted. In the physiological sciences, although it may have been done in principle, it has rarely been done in practice.

Undoubtedly, there is in the first attempts which are made in any science, much difficulty in ascertaining what are the proper objects of our inquiry, and what are beyond the reach of human intellect. In the present improved state, however, of the moral and physical sciences, there need be much less difficulty than formerly. The moral sciences have made us acquainted with the powers of our minds, and, in some measure, enabled us to distinguish the proper objects of their exercise. The physical sciences, although, as already stated, they have, from our not observing natural distinctions, been eminently prejudicial to physiological science, have nevertheless, disciplined our minds to the true inductive mode of reasoning, and taught us to distinguish, in those sciences, the attainable objects.

These two sciences in some respects, bear an interesting analogy to each other. In both, there are discovered certain properties of matter which, variously modified and combined, produce all the phenomena which we observe.* In Physics, we know the essential nature of these properties to be beyond the reach of our faculties; we must presume, therefore, those which correspond to them in the physiological sciences to be equally so.

How much more happily then are we circumstanced, than were the earlier physiologists, who for the want of these simple truths, exhausted the efforts of many great minds upon subjects which the author of nature has rendered incomprehensible. For centuries the efforts of the human mind have been lost; occasionally indeed a transcendant genius has intuitively fallen upon an

* This analogy probably led to the discovery of the vital properties.

important truth, but without being conscious of the mental process that led to it, and, of course, without being able to establish any uniform principles of reasoning, which should confirm his own discoveries and lead on to others; hence the world has been but little benefitted by those flashes of truth, which found and left it in darkness.

Haller has, perhaps, been the greatest benefactor of physiological science, inasmuch as he was the first who introduced any thing like systematic reasoning, and taught, though not perspicuously, the distinction between the physical and vital laws, and also pointed out the proper objects of our inquiry. Although, in these respects, his system was by no means complete, yet he accomplished so much, that others were able to perceive the tendency of his principles and to continue the work which he had begun. His suggestions have led to the establishment of truths which he did not anticipate, and Bichat and others have been much more explicit on these subjects.

We may, therefore, after the lapse of so many centuries of fruitless exertion, consider ourselves in the possession of the true magnet and chart which should guide us in our physiological investigations. With these facilities, we cannot but anticipate with delight the intellectual conquests, which, in this department of science, the human mind is destined to achieve. But error in reasoning is like the fabled Hydra, and when one fruitful source of it is destroyed, another springs up in our way. Thus, it is necessary, as we advance, that we should be perpetually on our guard and make frequent application of the tests of truth. This strife with error must prevail while there remains one truth to be discovered.

These remarks are sufficiently exemplified in the writings of the physiologists of the present day. Their success has certainly been much less than, from the influence of the present improved mode of reasoning on these subjects, we might have anticipated. Their writings contain many plausible and ingenious suppositions, which, when they are subjected to the rigid but just ordeal, are "burnt and purged away." They do, indeed, begin upon the true principles of inductive reasoning, but on following them a few steps, we very often discover a sophistry, into which they are led by a particular set of pre-conceived notions, which, perhaps unconsciously, they are endeavouring to establish. Anticipations in science have rarely proved correct, and yet we are exceedingly prone to make them. The slow and tedious process by which we arrive at truth, as we are conducted by facts, is unsatisfactory to most minds, and the imagination is allured by the hope of some brilliant discovery, into regions, where it cannot be followed by reason.

The arranging, in their proper order, those phenomena which constitute a science, may be compared to the labour of builders, who have prepared, ready to their hands, all the parts of a regular and spacious edifice. They may be entirely unacquainted with the design of the master builder, and unable to predict any thing with regard to the form which the work is to assume; and yet, by carefully observing the form of each stone in relation to others, and arranging them in different orders, the proper place of each part will be ascertained. As the work proceeds, it will obviously become less difficult, and at last, the design of the master builder begins to be developed, and the symmetry of the whole, together with the beauty of each part, disappoints and astonishes the beholder.

In the same manner, should we employ the phenomena which are discovered in nature, not presuming, from the form of a single stone or a pillar, to anticipate the great designs of the Creator, nor endeavouring to combine the materials according to our own fancy; but patiently observing the adaptation of one part to another, and changing their order until an obvious relation is produced. Should an error occur, which cannot at first be observed, it will soon be discovered by the difficulty of applying the remaining parts. If, in the progress of the work, for want of the proper fact, we interpose a presumption of our own, the symmetry of the whole is destroyed, and although we may continue to build, yet the plan of the great Designer never appears. If, also, we are too intent upon a plan of our own imagining, we shall be almost certain to produce an incongruity in the arrangement. In fine, it is not our business to create the materials of the sciences, but to arrange and study them. It is a humble employment, then, in which even they are engaged, who are capable of the utmost stretch of human intellect.

It may not be improper in this place, to remark for a moment, upon the sources from which our materials are to be drawn.

Experiments upon living animals, with a view to multiplying facts in relation to life, appear to characterise the researches of the present day. That these under some circumstances, and performed with proper precaution may yield useful matter, is undeniable; but it is necessary to their success that we should, to a certain extent, be acquainted with the properties of life, and the laws by which the exertion of them is governed; otherwise, we do not know what effects they may be capable of accomplishing, and are in danger of attributing the results of our experiments to causes with which we are more familiar. The little progress which has been made in Physiology by the multitude of these experiments, confirms what I have stated. We perceive that experimentors, who may even essentially differ in opinion, find no difficulty in substantiating their favourite notions, by the same series of experiments. We cannot, by them, accurately determine, how much is the effect of physical, and how much of vital properties, until we know something more of the powers of the latter. It would, therefore, be more profitable, if we were to direct our attention to inquiring what degree of control they exert over the phenomena of life, rather than to inquiring how much influence the chemical affinities have over the same.

Arguments, drawn from analogy, are, when not carried too far, as I have already stated, often exceedingly useful, particularly on entering a new field of inquiry. Indeed such are the relations which the different departments of science bear to each other, the moral, the physical, and the physiological, that one cannot comprehend the scope of any one of them without taking a general view of the whole. The existence of those properties and laws which control inanimate matter, undoubtedly suggested to Bichat and others the existence of corresponding vital principles. The analogy of plants with animals may be made much more particular, for these two orders of beings are classed in the same department of science, and are subject, in part, to the same laws.

Descriptive Anatomy, human and comparative, has long since arrived at a considerable degree of perfection. Every part of the human body has been scrutinised with the closest attention, and it appears that in this department but little remains to be done. It is almost unnecessary to say, that a constant adherence to anatomical facts, is essential to correct reasoning in physiology.

Pathology also furnishes us with important data, for the consequences which result from the defect of any organ and of its particular function, may in many instances suggest to us the importance and the particular use of such organ, and the nature of its function.

We must not, however, place too much reliance upon arguments drawn from any *one* of these sources; but if our opinions, so far from being pre-conceived, are suggested by facts, if they are illustrated by the general relation which exists between different sciences and between parts of the same, if they are supported by what we know of the laws of life, and if they harmonise with the general anatomy of the system, and with the particular structure of those organs which are principally concerned, we may fairly conclude that we stand upon the rock of truth.

Before entering upon the immediate subject of our inquiries, I think fit to to make a few remarks on the relation existing between the physical and the physiological sciences, that the general analogy which connects them, and the characteristics which distinguish them, may be borne in mind.

ESSAY, &c.

PRELIMINARY OBSERVATIONS.

Definition of Life.

THE material productions of Nature are, by Naturalists, divided into two great classes: Organic and Inorganic. Organic bodies are those which possess an internal structure capable of receiving and assimilating to their own tissues, inorganic matter,* and of thus increasing and renewing themselves from within. Inorganic bodies are those which may be said to accumulate, but not to grow; for they increase by the addition to their external surface, of matter already assimilated. The first class comprises animals and vegetables, the second, the mineral kingdom.

Organized bodies are said to possess life, a term which has been used with great variety of signification at different periods and by different individuals. The ancients, who in their physiological investigations, always sought final causes, observing the uniformity with which the functions of the different organs are performed, and the harmonious relations existing between them, were led

* Animals feed on substances which *were* organized, but after their death, although they retain for a time their structure, yet they obey only the laws of inorganic matter.

to ascribe organic phenomena to an ethereal principle, which they supposed to reside in the centre of the system, and to control it with an unerring intelligence. This is at the present day the vulgar idea of life, and is such as the mind naturally adopts, when not disciplined to the true mode of reasoning on these subjects. It continued among men of science, variously modified, but in principle the same, till a late period in the history of medicine.

Modern physiologists have wisely abandoned the pursuit of a final cause or source of animation, and have been contented with observing the phenomena manifested by the several organs in the discharge of their proper offices or functions, the totality of which they regard as constituting life. Life, therefore, is to be regarded rather as a result than a cause.

Every species of matter, whether animate or inanimate, exists in its present state and exhibits its peculiar properties by virtue of certain natural powers of which we know nothing but by the effects which they produce. These are always exercised in a uniform manner, and by careful observation we determine the laws by which they are governed. Neither the most simple substances nor the most complex are ever exempt from their control; were their influence but for a moment withheld, all nature would fall into one chaotic mass.

Those powers which constitute the class of inorganic beings are the attraction of gravitation, the attraction of cohesion, chemical attraction, electrical influence, &c. To produce a higher state of existence other properties are superadded, and vegetable life is the result. Animal life exhibits a still greater number of vivifying principles. No portion of matter, therefore, can be said to be absolutely dead, though trans-migrating from one state of existence to another.* Vegetables and animals exhibit so many phenomena in common, that they have both been considered as possessing life, in the common acceptation of that term.

Distinction between the Physical and the Physiological Sciences.

Those properties upon which the existence of inorganic bodies depends, are denominated physical, as also the sciences which treat of them. In contradistinction to these, the sciences which treat of the properties of organized beings are termed physiological.

Sensibility and contractility are in the physiological sciences what attraction, elasticity, &c. are in the physical. From the numerous modifications and combinations of these, result the func-

* See Burns, Bichat, &c.

tions which are the subjects of physiological inquiry.

Sensibility and contractility are of two kinds: organic and animal: These preside over different kinds of life; the first over organic, the second over animal life. Organic life is the result of those functions which are only concerned in the growth, preservation and restoration of the structure of the individual being, without establishing any perception of bodies exterior to it. These functions are digestion, respiration, circulation, exhalation, absorption, secretion, nutrition, calorification. They are employed in assimilating foreign substances to the structure of the plant or animal, promoting its growth or renewing its decaying parts and removing whatever has become useless or hurtful. It is this species of life alone which belongs to vegetables, and by the functions which constitute it, they exist without any consciousess of impressions from surrounding objects. Far otherwise is it with animals, that move from place to place, and sustain an interesting and necessary relation with surrounding beings. By means of the senses, the moral faculties take cognizance of whatever influences the animal frame, and seek or shun it as it gives pleasure or pain.

Both animals and vegetables possess another class of functions by which they reproduce their kind and preserve the species.

The organs which are concerned in performing the functions of organic life, are the alimentary canal and its appendages, the lungs, the heart, arteries and veins, the exhalents, the absorbents, the secernents and the nutrients.

The functions of animal life are performed by the organs of the senses, by the brain, the nerves, the muscles, the bones and the organs of the voice.

The function of digestion, which we are to consider, is perhaps the most important and interesting of those which constitute organic life.

Digestion of Plants.

Every living being, whether animal or vegetable, possesses an apparatus by which it elects and assimilates its appropriate food. This, in the simplest forms of life, is itself equally simple. Plants derive their nourishment from the earth, by means of absorbing tubes contained in their roots. These are continuous with the sap vessels or blood vessels of the plant, from which the circulating mass is conveyed to the leaves, there to be acted upon by air and light, and thence to be re-circulated for the purposes of nutrition, secretion, &c.

In what part of the circle it is, that the most important changes are effected, is not demonstrable. It is undoubtedly, however, accomplished somewhere in its course by the peculiar action of these tubes upon their contents.

Digestion of Animals.

The most imperfect animals are scarcely distinguishable from vegetables, the definitions of Naturalists having often proved fallacious. The most infallible criterion, however, is their mode of digestion. Plants feed upon inorganic matter, and in the manner abovementioned. Animals are nourished from the animal or vegetable kingdom, and receive their aliment into an internal cavity, where such parts as are susceptible of animalization, are elected and assimilated.

As the structure of some animals differs so little from that of vegetables, we may fairly infer that their digestion differs but little except in the above particular; thus the polypus has no digestive apparatus which we can discern, except the cavity into which its food is conveyed and from which it is afterwards disgorged. May we not presume then, from this easy gradation of structure, that a corresponding one of function exists, and that the digestion of the polypus after its food is introduced, differs but little from that of vegetables? As we rise to the higher classes of animals, we find the digestive apparatus becoming more complicated. That, which was a mere sack in some of the lower animals, with a single orifice, and which in others was little else than a straight tube, becomes a convoluted canal, many times the length of the body, expanding near its superior extremity to form the stomach, and having appended to it many subsidiary organs indispensable to its functions.

Theories concerning Digestion.

The earliest writers on this subject, were of opinion, that the change effected on the aliment in the stomach was little else than the result of putrefaction. Unsophisticated in the logic of the natural philosophers and chemists, they supposed the food to undergo the same change which warmth and moisture effect upon it out of the body. But when the physical sciences had made some progress, and men of learning were habituated to reason almost exclusively on physical principles, the mechanism of the system was alone talked of, and the stomach wrought mechanically upon its contents. As chemistry began to assume the character of a science, it also, was very prolific in physiological explanations, and digestion was wholly chemical.

It were ridiculous in us, as it has appeared in many writers on this subject, to inflict, like Falstaff, a new stab on these dead theories, for the sake of making the victory, in part, our own. As shown in our introduction, they are obviously inconsistent with principles of medical logic, now well established. We enter the field armed with our sling and stone, (common sense and truth,) against a far more formidable adversary, in the prime of life, mailed in all the sophistry of the age, and backed by universal approbation.

We shall now proceed to give an account of what has been regarded for many years past, as the true explanation of the process of digestion; further to inquire what are the objections which may be started against it; and finally, whether there is any more rational mode of accounting for its phenomena.

ESSAY, &c.

CHAPTER I.

PHENOMENA AND THEORY OF DIGESTION.

PREVIOUS to detailing the theory in question, I shall briefly enumerate the facts, which, in relation to this subject, may be considered as well established by observation or experiment. This is the more necessary, as in elementary physiological works, they are not well distinguished from what is questionable, or matter of mere assumption. We shall find the latter to be frequently employed as indisputable facts, having been considered as the property of physiologists by long possession, but without any well established title.

Phenomena of Digestion.

The first change which aliment undergoes is comminution by the teeth, and admixture with the secretions of the mouth, which are mucus and saliva. The former is the common secretion of all mucous surfaces; the latter, which is much more abundant, is furnished by appropriate glands. It is an exceedingly bland fluid, slightly viscous and chemically constituted of four parts of water and one of albumen, in which are dissolved phosphates of soda, lime, and ammonia, and a small quantity of common salt. It possesses an antiseptic property, and has an affinity for oxygen, which are all the remarkable chemical properties known to belong to it.

The secretion of saliva is much increased by the presence of food in the mouth. The action of the teeth, the tongue, &c. diffuses the saliva through the whole mass of food, which then loses, in some degree, its sensible properties. During mastication, a small quantity of air is involved by the viscid property of this fluid. If solid food be swallowed without admixture with the saliva, and trituration by the teeth, its digestion is slow and imperfect.

The food is next conveyed by deglutition, into the stomach. It is not necessary that we should describe the mechanism of this function.

The accumulation of food in the stomach immediately appeases the sensation of hunger, and in a short time the vigour and activity of the body are restored. These effects do not take place in proportion to the distension of the organ, food that is highly nutrient satisfying in less quantity.

According to Magendie, food remains in the stomach about one hour before any change takes place, except that which arises from mixture with the fluids, which, during this time are copiously poured into the organ. The stomach, then contracts upon its contents, and from this time there is discovered in the pyloric extremity, a fluid supposed to be the result of digestion, and denominated chyme. A lamina of the same is found covering the mass in the greater curvature of the stomach. Wilson Philip states "that this process takes place *only* on, or near the surface of the stomach, and that in proportion as the food there situated undergoes the necessary change, it is, by the muscular powers of the stomach, moved onwards towards the pylorus, making room for that which next succeeds, till the whole contents of the stomach have undergone this process, the digested contents being regularly discharged into the duodenum as they arrive at the pylorus, till most, and in some animals, all the contents of the stomach are thus removed into that intestine."

From the moment that food is conveyed into the stomach, there begins to be exhaled from the mucous surface of the organ, mucus, and a fluid resembling very much, in its sensible and chemical properties, saliva. These properties must however, be very variable or very obscure, since of the many experimenters who have carefully analyzed this fluid, scarce any two give the same results. Some have positively declared it to be acid, others with equal confidence have ascribed to it alkaline properties. A third class discovering nothing remarkable in it, have presumed it to be nothing more than the residue of former digestions, mingled with saliva and a similar secretion from the stomach.

From two circumstances it has been inferred, and universally believed, that this fluid possesses an extraordinary solvent power. Mr. Hunter discovered, that in persons dying suddenly, and in whom the function of digestion was progressing vigorously, there occasionally took place an erosion of the coats of the stomach. The same appearances have since been observed, and the fact is unquestionable. The other circumstance is the digestion of food contained in hollow balls and tubes, pierced with holes, and introduced into the stomach, as occured in the experiments of Spallanzani, Reaumur, &c. Spallanzani and other experimenters assert that the same results, though not with the same facility, attended their experiments on this fluid out of the stomach. The more recent experiments of Montegre and others, performed with more accuracy and precision than chemical analysis was then capable of, show that these results were fallacious.

The aliment, while in the stomach, is subjected to gentle pressure by the walls of that organ. It is also agitated by the motions of respiration. The heat of the stomach during digestion varies from 98° to 104°.

After some hours have elapsed from the time that aliment is taken into the stomach, it is found to have lost, in a great measure, its sensible properties, and to be converted into a substance called chyme. This is described as "a homogeneous, pultaceous, greyish mass, of a sweetish insipid taste and slightly acid." Its properties are by no means uniform, as they vary with the food employed, and accurate observers assert that there are as many kinds of it as of aliments.

Animal food is more easily converted into chyme than vegetable.

The time required for the completion of digestion in the stomach is from three to five hours.

It has been acertained by the numerous and accurate experiments of W. Philip, that if the eighth pair of nerves be divided, the process of digestion is immediately and completely suspended. Violent affections of the mind and of the body produce the same effect.

In cases of sudden death, when the process of digestion is vigorously going on, food does not continue to be converted into chyme, although the stomach may be eroded.

In digestion, the chyme does not accumulate in any quantity at the pyloric end of the stomach, for its presence excites a contraction of that portion of the organ at the same time that the pyloric sphincter yields, and the contents are conveyed into the duodenum, which also contracting propels it along the whole tract of the intestines. Chyme is not changed in its properties, until it reaches the ductus communis choledochus. Here, being combined with the hepatic and pancreatic secretions, it acquires new characteristics, assuming a bitter taste and yellow colour. After this, there will always be found precipitated upon the valvulæ conniventes, a greyish coat adhering to the mucous membrane. This has been thought to be the elements of that fluid, which is discovered in the lacteals, called chyle, the elaboration of which has been regarded as the most important result of digestion.

These phenomena are observed in the two superior thirds of the small intestines.

As the chyme varies exceedingly in the properties which it exhibits on entering the duodenum, so the changes, which are effected in the small intestines, vary with the nature of the aliment. Food, which has passed the stomach without change, undergoes but little in the intestines.

It is in the small intestines that digestion is completed, and it is from them that those minute absorbing vessels, which we call lacteals, arise. They are seen in the mesenteries of animals killed soon after eating, filled with a milky fluid, which they convey to the thoracic duct, which transmits it to the left subclavian vein.

Although the chyle of the intestines is spoken of by physiologists, yet nothing is found in them more nearly resembling it than the greyish precipitate described above. The chyle, which has uniformly been the subject of experiment and description, is that of the lacteals and thoracic duct. Magendie gives the following account of it. "If the animal from which the chyle is extracted had eaten of fatty vegetable or animal substances, the fluid drawn from the thoracic duct will be of a white milky appearance, rather heavier than distilled water, of a spermatic odour, stimulating the tongue, a little saltish, and perceptibly alkaline. Soon after passing out from the vessel in which it was lodged, the chyle runs into a mass, and acquires a consistence almost solid; after some time, it separates into three parts, one solid, which is found at the bottom of the vessel, another fluid, which is found above it, and a third which forms a sort of pellicle on the surface of the fluid; the chyle at the same time assumes a bright reddish tint. When it consists of aliments which do not contain fat, its general properties are the same; but instead of being white and opaque, it is semi-transparent, and the pellicle formed on the surface is less distinctly marked than in the first kind of chyle.

Of the three parts into which chyle separates, when left to itself, that on the surface, of an opaque white colour, is a fatty substance; the coagulated or solid part is formed of fibrine and a little red colouring matter; the fluid is analogous to the serum of the blood. The proportion of these three parts varies according to the nature of the aliments. There are various chyles, for example, that of sugar, which contains but very little fibrine, and that of flesh, which contains much more. The same remark applies to the fatty part, which is extremely abundant when the aliments contain fat or oil, while this is hardly distinguishable when the aliments are destitute of fat. The same salts which are found in the blood exist also in the chyle."

Theory of Digestion by the Gastric Juice

In the foregoing sketch, I have not endeavoured to enumerate all the minutiæ relating to the process of digestion, but rather to present to the mind of the reader, in as concise a manner as possible, those important and undeniable facts, which bear upon the question about to be agitated ; for more particular information I refer him to the elementary authors. In the following pages, I shall adduce other circumstances, both *pro* and *con*, perhaps more equivocal, but shall endeavour to give them the precise weight which their character merits. I shall now proceed to give a concise account of the theory of digestion at present received.

Chesselden was the first who suggested that digestion might be accomplished by a solvent fluid secreted for that purpose, and he conjectured that it was the saliva. Reaumur and Spallanzani were led to a similar anticipation by observing the effects of chemical solvents in their laboratories, for chemistry at that time was becoming the fashionable science and they were devoted to its pursuits. With a view to disprove the mechanical theory and to confirm their own anticipations, they introduced into the stomach, as stated above, hollow balls and tubes pierced with holes and charged with aliment. Their contents, upon their being removed from the stomach at the end of some hours, were found to have undergone the same change as the mass in which they were involved. From that time, this experiment has been regarded as a complete confutation of all the former hypotheses on this subject, and a demonstration that digestion is effected by the solvent power of a fluid, exhaled from the stomach, denominated the gastric juice. This theory was confirmed by the erosions of the stomach, first observed by Hunter, as mentioned above. The failures of those who have attempted artificial digestion, by means of the gastric fluid, out of the stomach, have not shaken the confidence of physiologists in the powers of this agent.

The belief, therefore, which prevails is, that upon the introduction of food into the stomach, this organ becomes a centre of fluxion, and that there is then exhaled from its surface the fluid mentioned above, which, by a chemical agency, converts the alimentary substances into the homogeneous mass called chyme. There is some difference of opinion with regard to the aid which it receives from the stomach. Some believe that the vital powers of this organ are necessary to digestion, even after the exhalation of this fluid; others, that the process is entirely chemical. Magendie asserts that the gastric juice requires no aid but such as it receives from the precise degree of heat, moisture and motion, to which the aliment is subjected in the stomach. W. Philip believes that the vital powers of the stomach are only employed in the elaboration of this fluid, which acts on the food independently of them.

The conversion of aliment in the stomach into chyme is regarded as far the most important part of digestion, and as the only office of that organ. The chyme, as is generally supposed, is then conveyed into the duodenum to undergo another chemical change, by the agency of the hepatic and pancreatic secretions, the result of which is the precipitation of the chyle from the mass of the chyme. I say this is generally believed, for there is some difference of opinion on this point, as it is asserted by some that these secretions serve no other purpose than to qualify the residuum, and to stimulate the intestines, while the chyle is elected immediately from the chyme by the lacteals.

Such is the theory of digestion at present universally received, and which has long since ceased to be questioned. So confident are men of science in its imperishable nature, that all our systems of physiology and pathology are founded upon it as their corner stone. We are to recollect, however, that the above experiments and deductions were made at a time when the rules of logic, by which we are guided in speculations of this kind, were less rigid than they now are. We are to consider, also, that at that time, the progress of science had shown the previous theories to be altogether inadequate to the explanation of the phenomena of digestion, that the application of chemistry in its improved state to physiological inquiries was then universal, that, consequently, the ingenious theory in question, offered, as it was, by men of high character and great influence, soon came to be generally received, and consequently, ceased to be the subject of that spirited inquiry, which results from difference of opinion.

ESSAY, &c.

CHAPTER II.

OBJECTIONS TO THE ABOVE THEORY.

BUT are the evidences in favour of digestion, by the solvent powers of the gastric fluid, sufficient to check further inquiry on this subject? Is the mode of reasoning, which has led to this opinion conformable to just principles of logic? Does the theory, like that of Harvey, rest upon numerous and unequivocal facts? Are the phenomena of digestion satisfactorily explained by the supposition, and does it harmonize with the general anatomy, physiology and pathology of the system.

Objections to the reasoning employed.

I object to the mode of reasoning employed on this subject, as one which has been productive of infinite perplexity and error, and which has been uniformly found inadequate to the explanation of physiological phenomena.

It appears that Chesselden, Reaumur and Spallanzani were led to anticipate the existence of a powerful solvent in the stomach, from comparing the changes which the food undergoes in that organ, with results obtained in their laboratories.

This was evidently predicting the means by which Nature should accomplish her ends, in one department of science, by observing what she has done in another, in which the properties of matter, and the laws by which they are exerted, are entirely different.

It is only in the character of humble enquirers, who submit themselves to the guidance of such facts as they may discover, that men have extended the boundaries of knowledge.

Were it impossible that the changes effected on the aliment in the stomach, should be accomplished in any other way than by a mechanical or chemical process, their conclusion would be just, for there could be no fallacy in reasoning from an effect to a cause, the operations of which we are acquainted with. But knowing the distinction which exists between the physical and physiological sciences, it is not only possible, but even probable, that this change is wrought by a cause, the operations of which, we are but little acquainted with. It is incumbent on those, therefore, who maintain the above position, not merely to show the effect, but to demonstrate unequivocally that the gastric fluid possesses such properties as are capable of producing the effects attributed to it, or to show that they can be accomplished in no other way.

The chemical powers attributed to the gastric fluid are certainly very extraordinary. It is said, not merely to dissolve rapidly the great variety of aliments which are taken into the stomach, but to enter into combination with them, and to produce a third substance. Should we not, therefore, expect to discover something equally extraordinary in the *properties* of so powerful an agent? But I have already stated, that this is so far from being the case, that scarce any two persons give the same account of its chemical constitution. Its analysis therefore, yields nothing to support the theory in question.

It was very naturally supposed, by those who attributed these extraordinary powers to the gastric fluid, that they would be manifested, in some degree at least, when removed from the stomach; accordingly, artificial digestion was attempted, the auxiliary circumstances of heat, motion, &c. being imitated as nearly as possible. Had this experiment unequivocally succeeded, it would have been a necessary conclusion, that the solution was effected by some property inappreciable by the ordinary chemical tests. It is obvious, however, that the experimenter might, in this process, very easily deceive himself. Chyme is a substance by no means possessing uniform characteristic properties, but is exceedingly variable; by what criterion therefore, should he ascertain that he had accomplished artifical digestion, since we can only say of chyme, that it is a "homogeneous, pultaceous, greyish mass, of a sweetish, insipid taste, and slightly acid?" It certainly would be no difficult matter to produce a compound, having these properties, by admixture and maceration of aliments, sufficiently comminuted, aided by gentle heat and motion. It does not appear that these experimenters accomplished any thing more, although their report of the result was received as unequivocal evidence of the solvent powers of the gastric fluid.

The experiments of Montegre, performed, as we may presume from the reputation of the author, with all the nicety and precision of modern chemistry, tend to show, that the gastric juice, out of the stomach, exerts none of those extraordinary chemical powers which are attributed to it, when in that organ, but that it is as inert as water. Allowing that his experiments are not conclusive, it is certainly very surprising that a fluid, which, after death, is supposed to corrode the stomach, *without* the aid of a uniform temperature of 98° and of motion, should, to the scrutiny of an able chemist, yield no evidence of its possessing any solvent powers whatever.

Magendie, who nevertheless supports the position of digestion by the gastric fluid, does not believe that artificial digestion has ever been accomplished, and regards the experiments of Montegre as satisfactory proof, that out of the stomach, the gastric fluid is perfectly inert.

It is stated in the foregoing chapter that there are two undeniable facts which contributed greatly to establis the opinion in question: first, The erosion of the coats of the stomach after death, observed by Hunter, and second, The digestion of food contained in hollow balls, and introduced into the stomach.

With regard to the first of these phenomena, it appears to me, that the advocates of the theory of solution are not aware of the dilemma to which they are reduced, in case it be admitted as an effect of the gastric juice. Magendie, in accounting for the inertness of the gastric juice out of the stomach, attributes it to the absence of certain circumstances, which cannot be accurately imitated. The principal of these are uniform heat and motion. They, it is obvious, are dependent upon vitality, and must cease with it. If, therefore, it be asserted by any, that this fluid acts thus upon the dead stomach, with an energy equal to that of the most powerful acids, it is certainly incumbent on them to exhibit its effects out of that organ, since there can be no reason why they should not be unequivocally demonstrable. If the negative argument be resorted to, that these effects can be produced in no other way, I consider the assertion to be altogether unwarrantably, and the mode of reasoning to be, on physiological subjects, altogether inadmissible. The changes which take place in the human body, in the article of death, have often been observed with astonishment. So numerous and so striking are they, that much caution is required to distinguish them from those changes which are the result of disease. We know very well, that if we would discover, after death, the morbid evidences of a slight degree of inflammation, the examination must be promptly made, or those evidences disappear. We know, too, that absorption is performed with great energy in the article of death, and I believe after apparent death, or after the animal functions have ceased to be performed. We have reason, also, to think, that appearances, resembling those of ulceration, have taken place after apparent death.

These *post mortem* changes are, in a general way, accounted for, on the supposition that the organic functions, particularly those of the capillaries and absorbents, continue to be performed after the animal functions have ceased.

Who, therefore, will undertake to say, that the abovementioned erosions of the stomach are effected by a fluid, secreted by that organ, and possessing remarkable chemical properties, because it is impossible that they can be accomplished in any other way? We know, very well, the extreme vascularity of the stomach and that its capillary vessels, and particularly its absorbents, act with unparallelled energy. We know also, that different portions of the stomach derive their vitality from different portions of the nervous system. Is it not possible, therefore, to conceive that one part of the stomach, having lost its vitality, may be acted upon by another which retains it? It is by no means incumbent upon me to make out, even the probability of this supposition, the bare possibility of it is obviously sufficient for my purpose.

Another objection to the common explanation of this phenomenon is, that in all those instances in which it has been observed, the food, taken into the organ a short time previous to death, has been found untouched, however extensive the erosion of the stomach may have been. It is certainly very singular, that this fluid should produce no effect on the aliment, which, to prepare it for its action, has been comminuted by the teeth, mixed with the saliva, macerated by the fluids of the stomach, and finally imbued with the gastric juice itself; and that it should turn with energy upon the stomach, which has been influenced by none of those circumstances, and which is moreover protected by an epithelium, or cuticle, and its own mucous secretions.

The experiment of introducing hollow balls or tubes, filled with aliment into the stomach, was first performed with a view to disprove the supposition of the mechanical action of the walls of the stomach. In this respect, it has completely succeeded; but it has been also regarded as unequivocal evidence of digestion by the gastric juice, although it by no means suggested that theory. Let us candidly examine the circumstances of this experiment, and see whether the above inference necessarily follows. The balls or tubes, employed for this purpose, must have been small, and pierced with holes, either numerous or large, in order to favour the free access of the gastric juice. On the introduction of the balls into the stomach, the progress of the experiment is entirely hidden from our view, and we know nothing of it, but from the result, which, on removal of the balls, is found to be a partial digestion of the aliment contained in them. If it can be shown, then, that it is impossible, that the phenomenon can be accomplished in any other way, the conclusion is just; but this, as in the instance above, is obviously absurd. I hope to make it appear not only possible, but even probable, that the villi of the stomach act immediately on the aliment, and effect the changes that have been attributed to the gastric juice. It is enough for my present purpose that such a thing is obviously possible, and being so, the phenomenon in question is no longer proof of the solvent powers of the gastric juice ; for the villi of the stomach, being probably capable of elongation, and attached to a very moveable membrane, would be capable of seeking the aliment contained in the stomach and of coming in contact with that which is inclosed in the balls ; and it is to be observed that it is only that portion, which is in the neighbourhood of the holes, that is found changed.

It is proved by Wilson Philip and others, that it is only the surface of the alimentary mass, in contact with the walls of the stomach, that is changed by the gastric juice : therefore, only that part of the aliment in the balls, which is reached by the surface of the stomach, can be acted upon, according to either supposition. Hence, the fact is as much in favour of the one position as the other, and proves nothing.

A fact, to which I have already alluded, calculated to throw much light upon this obscure subject, is that mentioned by Philip, and also by Magendie. They both state, that, in digestion, no change takes place in the alimentary mass, (except such as maceration effects,) until certain motions are observed to take place in the stomach, by which its inner surface is closely applied to the surface of the aliment; that then, the first changes which are discovered, are on the surface of the food in contact with the mucous membrane of the stomach. A thin layer of aliment is found to be converted into the substance which is called chyme. This is then conveyed, by the motions of the stomach, to the pyloric extremity of the organ and a new surface is presented to the surface of the stomach to undergo the same change. To those who advocate the theory of solution, the above must oppose no small difficulty. The fluid, which, we are told, begins to be poured out soon after the food enters the stomach, and which, in some instances, is found in considerable quantity in that organ, must certainly pervade the whole mass of the aliment; why, then, should its effects be confined to the surface of the alimentary mass? and why should they be simultaneous with certain motions of the stomach? It may be said, indeed, that this fluid acts with such surprising energy, that, at the instant of its secretion, it seizes on the first particle of food with which it comes in contact and that thus, it never permeates the mass of food. This resort, however, would, perhaps, cost our opponents more than they are aware. It would amount to an absolute denial of the action of the gastric juice upon the dead stomach: for, how could it act upon that organ after death, unless it were accumulated in it, and that, when there is food present upon which it has not acted? It would also follow from this supposition, that the secretion of the gastric juice does not commence until the very moment that digestion is found to take place. We might also fairly infer, that its secretion would never take place except food were present and that then it would never remain uncombined in the stomach for a moment. It would, therefore, be utterly impossible to prove the existence of such a fluid, and in advocating it, the abettors of the theory of solution would abandon all the arguments usually employed, and gain nothing but a supposition.

Those who believe that the gastric fluid can only act when aided by certain vital powers residing in the walls of the stomach, and account in this way, for digestion taking place only on the surface of the mass, in contact with the stomach, are in the same dilemma. On such a supposition, there could be no more proof adduced of the existence of the gastric juice than of the existence of the nervous fluid of which so much was once said. We see, indeed, the surface of the aliment coming in contact with the surface of the stomach, and a remarkable change effected upon it: but shall we say, that this is effected by a fluid possessing remarkable chemical properties poured out at that instant and enabled to act by the vital powers of the stomach? If its powers are exerted in no other way, and at no other time, what evidence have our senses, that there exists any such fluid, except the effect that is produced upon the aliment? I have already shown that this reasoning would be inadmissible.

The promptitude with which digestion is interrupted, by the division of the eighth pair of nerves, is a sufficient evidence that there is something more than a mere chemical process going on in the stomach, for, according to the commonly received hypothesis, the gastric juice, already secreted, should continue to operate upon the aliment.

Having noticed the arguments which are usually employed in support of the doctrine of digestion by the solvent powers of the gastric juice, we now proceed to the consideration of circumstances not immediately suggested by them.

Changes not wrought upon large volumes of matter.

Our systems of physiology teach us, that the change which the food undergoes in the stomach, is by far, the most important step in the process of assimilation or the transition of matter from the control of physical laws to those of vitality. We are told, that the gastric juice, dissolving and uniting with the nutrient portions of the food, forms a new and peculiar substance, which must be considered as highly animalised, since no one considers that an equally great change is wrought upon it in the small intestines, or in its passage to the mass of the blood; nor is it supposed that any change which it may undergo in the blood vessels, is equally important with that which takes place in the stomach.

If this opinion be correct, digestion is the only instance in the animal economy in which an important change is wrought upon a large volume of matter in any of the large reservoirs. All the changes, which are effected upon the aliment after its mingling with the blood, are wrought in the capillary vessels of the lungs and the nutrient vessels of each tissue to which it is appropriated. In fine, all the changes which are wrought upon the blood, either for more perfect animalization, secretion, exhalation, or any other purpose, are wrought in the capillary tissues, where the circulating fluids, becoming exceedingly attenuated, are subjected to the peculiar action of the vessels which convey them. No changes are wrought upon the blood in the heart or in any of the great blood vessels.

Glandular structures appear to be little else than congeries of minute vessels, for the purpose of increasing the surface with which the contained fluids are in contact and thus subjecting them more fully to their peculiar action.

Is it not surprising, therefore, that so great a change can be effected upon a large mass in the stomach, when, to accomplish changes much less important, it is necessary, that the matter acted upon should be made to pass through vessels indefinitely multiplied? It will presently be seen that, by the hypothesis which I offer, this difficulty is not incurred, since, by that, the more important part of digestion itself is effected in the capillary vessels.

Variable properties of chyme.

The want of uniformity in the sensible and chemical properties of chyme must oppose no small difficulty to the advocates of the theory in question. Magendie and others inform us, that, in these respects, it is so variable, that there are as many kinds of chyme as of aliment, it being sometimes acid, sometimes alkaline, sometimes of one colour and consistence, and sometimes of another. If the conversion of aliment into chyme be so important a step in the process of animalization, should we not expect to discover some uniform property or quality, which shall give evidence of its having become in a great degree assimilated? We do, indeed, discover evidences of that kind in the chyle, but never till it has passed through the capillary vessels. The properties of chyme, therefore, although they are very different from those of the aliment from which it is formed, suggest no such important change, as that said to be effected by the gastric juice; on the other hand, they are nothing more than might result from comminution, admixture and maceration, of the contents of the stomach. The comminution however, as I shall by and by show, may be such as cannot be completely effected by art.

Chyle not found in the intestines.

Writers speak with great confidence of the formation, in the small intestines, of a fluid denominated Chyle. The most common opinion with regard to its formation is, that the chyme, having already undergone the most important change, is, on being conveyed into the small intestines, acted upon by the bile in a chemical way and that chyle is precipitated from the mass. Others, indeed, give different accounts of this process, but all speak of the formation of chyle in the small intestines with as much confidence as if it were uniformly found in that part of the alimentary canal, and, taken from it, had often been made the subject of experiments by which its chemical and animal properties had been ascertained. I will hazard the assertion, however, that there was never a particle of what was *ascertained* to be chyle, obtained from any other source than that of the lacteals or thoracic duct. The existence of chyle in the intestines has been presumed from the belief that the lacteals have no power to effect a change on their contents, and that consequently they were taken up precisely as they are found in them. If this be the case, the conclusion is palpably false, for who can say that the lacteals have no peculiar actions by which their contents are wrought upon?

In speaking of the changes which the chyme undergoes in the small intestines, Magendie states (page 226,) that "If it consist of animal or vegetable substances, containing oil, there will bes een to form, here and there upon its surface, irregular filaments flattened or rounded, which attach themselves to the valvulæ conniventes, and *appear to be imperfectly formed chyle.* I hope the reader will recollect that this substance is, what *appears* to be, *imperfectly* formed *chyle*, and not what has ever been *demonstrated* to be chyle, which is also found in the lacteals. In another place (page 228,) the same author remarks "From what has been said, it will seem that the chyme in the small intestines is divided into two parts. The one is attached to the walls, and is the chyme in an imperfect state; the other is destined to be pushed into the large intestines, and at last entirely rejected." Without having adduced a single experiment or other circumstance, to show that this substance, which, in one place he speaks of as only appearing to be imperfectly formed chyle, and which, in another, he mentions as chyle in an imperfect state, is actually the same substance which is found in the lacteals, he makes this final statement, (p. 229,) "Thus is accomplished that most important part of digestion, the production of chyle." Who will not pronounce the above conclusion to be altogether unwarrantable? *I think it may be with safety asserted, that no animal properties can be demonstrated in any substance which has not been subjected to the action of capillary vessels.*

Objections from the Anatomy of parts concerned.

The theory of digestion, by the solvent powers of the gastric juice, does not well explain the uses of certain anatomical structures met with in the digestive organs.

The villi or papillæ of the stomach are, in their structure, essentially the same as those of the small intestines. No material difference in their organization can be pointed out, and yet we are told, that their offices are diametrically opposite, that of the villi of the stomach being to exhale the gastric juice, while those of the intestines absorb the chyle.

Bichat noticed this incongruity, and says, "It is difficult to conceive how an organ, every where nearly similar, can perform, in different parts, functions so different. I say nearly similar, for we shall see that these papillæ exhibit differences of size, length &c. without having any of texture or structure.

That the thoracic duct is the only medium of communication between the digestive organs and the mass of the blood, is the necessary belief of those who favour the opinion in question. It is conjectured by some that a small quantity of nutriment is taken up by the lymphatics, but these terminate in the thoracic duct.

This vessel is of extremely small size, although the common trunk of the lacteals and lymphatics. Its coats are so exceedingly thin and delicate that when collapsed it is scarely discernible. Its length is very great, as it extends from its origin in the abdomen, quite through the thorax to the left subclavian vein. The track which it pursues is obliquely across the spine, beneath a branch of the vena azygos and beneath the aorta. Having gained a situation upon the left of the spine, it mounts upward in the neck for a short distance beneath the carotid artery and then curves downward to the place of its termination.

If the functions of this little vessel are what they are supposed to be, it certainly performs an office of vastly more importance than that of any duct in the system. We should expect, therefore, from observing the provisions of nature in less important structures, to find this organ guarded with peculiar care, and with appendages capable of performing a vicarious office.

If the proper artery of any part is obliterated, we find that nature has anticipated the evil by furnishing collateral branches. The same is true of the nerves. The organs of the senses are wonderfully protected and exist in pairs. How obvious and how ingenuous is the guardian care of nature, in the protection of the brain and spinal marrow! The brain too, consists of hemispheres, and the spinal marrow sends off its nerves in pairs, and these pairs are constantly interchanging filaments. The organs most essential to life receive their nervous influence from both the brain and spinal marrow and by a great many origins. These nerves also exist in pairs and are wonderfully protected in their courses.

The great vessels, the trachea, the œsophagus and the intestines, from their size, their structure and other circumstances, are but little liable to obstructions.

The digestive organs rarely have their functions completely suspended, and when, from disease, this does take place, the absorbents are supposed to feed on the reservoirs of fat. The lungs are organs whose functions cannot be for a moment suspended; they, therefore, are carefully protected and exist in pairs, the functions of one of which may be interrupted without materially affecting those of the other.

The biliary and pancreatic ducts may be compared to the thoracic, but in case they are obstructed (which occasionally takes place,) the fluids which they convey may safely be withheld for a time, or are taken up by the absorbents and appear in every part of the system.

How different from all these are the circumstances of the thoracic duct! Of all those organs, which are properly called *ducts*, I believe it is the longest, and of course, in this respect, the most exposed to obstruction. Its parietes too, are the most delicate and unresisting. It passes in the neighhourhood of parts, which, in certain diseases, would be exceedingly liable to press upon and obliterate it. It has no fellow nor is there any organ which can assume its functions. It is said, indeed, that in some instances, there are branches given off from it in the thorax which enter the subclavian vein at different points; if, however, these were important, we should expect to find them uniformly existing. What appears most like a provision of nature to protect its functions, is the division of the duct in the thorax into several small branches and their reunion.

The experiment of tying this duct in the living animal, I consider to be neither conclusive for nor against the opinion which I oppose. In many instances where it has been tied, the animal has experienced no inconvenience. In others it has proved fatal. The former result may, however, be accounted for by the existence of the collateral branches mentioned above, and the latter, by the presumption that the performance of so difficult an experiment would, in many instances, be attended with injury of some essential organ.

It has been long thought by many, that the functions attributed to the liver, by the present theory of digestion, do not correspond with its magnitude, its structure and its pathological importance. It is present in nearly all animals. The ancients supposed it to be the organ of sanguification. Many modern physiologists have suggested that it might effect some change on the blood besides that of the elimination of bile.

Dr. Rush, in an essay on the functions of the liver and spleen, has the following: "The design of the liver I believe to be, to receive the blood from every part of the body, in order to subject that part of it, which has not been completely animalized or divested of its chylous properties, to a secretory process, and after, to pour the product of this secretion into the duodenum, to be absorbed or otherwise taken up by the lacteals and conveyed with the chyle from the stomach into the blood-vessels, in order to be completely converted into red blood, for the purpose of serving the various and important uses for which that fluid is intended in the human body."

Dr. James Johnson, in his Treatise on Tropical Climates, says, that "This immense gland is the the largest in the human frame; for neither the brain, heart, spleen nor kidnies, can be at all compared with it; and the lungs, though occupying a larger extent when inflated, yet, if condensed to equal solidity, would fall short in size and weight. Now, since nature, throughout her works, has seldom been accused of supererogation, we may safely conclude, that the importance of this organ's function, in the animal economy, is commensurate with its magnitude."

The cases of malformation related by Mr. Abernethy and Mr. Lawrence, in which the vena portæ terminated, without division, in the vena cava, are highly interesting. They render it certain that bile *may* be secreted from arterial blood. The fact will be again spoken of in answer to an objection, which it apparently opposes to opinions soon to be advanced.

Nutrient effects of aliment.

It appears to me that the theory in question does not satisfactorily account for the speedy restoration of strength and vigour, which one experiences on taking food, who has fasted till weak and faint. I know that many are ready to reply, with the convenient and magic word sympathy. We are told that the stomach, by its nerves, gives notice to the system of the good things it has in preparation, and that the anticipation of them rouses its dormant energies. That stimuli and medicinal substances partially produce their effects through the medium of the nerves, I do not deny. That the sapid properties of food have a similar influence, in a small degree, is equally probable. The stimulus of distension may also have an effect of this kind, though it cannot produce the invigorating effects which we experience so soon after taking food, for the effect is not in proportion to the quantity, but to the nutritious qualities of the aliment. That feeling of renewed strength, which every one experiences on breaking a long fast, as it appears to me, is not well accounted for but on the supposition that some portion of it soon enters the circulation. We are told, however, that the process of assimilation does not commence until more than an hour after the food is received, and that during that time, and for some time after, so far from receiving, the digestive organs are copiously imparting.

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ESSAY, &c.

CHAPTER III.

DIGESTION BY THE VEINS OF THE STOMACH AND INTESTINES, AND BY THE LIVER.

AFTER perusing the foregoing remarks, I trust that the reader will not be astonished at the title of this chapter. The objections to a false hypothesis, if they are founded in fact and properly urged, must involve some of those circumstances which contribute to the explanation of whatever phenomena are the subjects of our inquiry. We will, therefore, commit ourselves to the guidance of such facts as have been mentioned above, and such others as do notrequire to be tortured into an accordance with our sentiments.

Agency of the Mucous Membrane.

In the foregoing chapter it appears, that no important change is effected on the aliment until it comes in contact with the mucous membrane of the stomach. I trust that I have also shown the existence of a gastric liquor possessing the powers so long attributed to it, to be a fallacy. As I have before stated, we see the aliment come in contact with the walls of the stomach, and an important change immediately taking place upon it. If I understand the relation between cause and effect, we are not to interpose a new link in the chain of sequents, for the sake of aiding the imagination, or for completing our preconceived notions. The presumption is, then, until some new intervening cause can be pointed out, that the change in question is effected by immediate agency of the mucous membrane of the stomach. If we go beyond this, and assert that the change is effected by a particular portion, or set of vessels, contained in this organ, we require further proof. My proposition is, that the first step in the process of digestion is effected by capillary veins, originating in the villi of the stomach with absorbing extremities, and terminating in the great branches of the vena portæ. The arguments in favour of it are drawn from the results of certain experiments, from the nature of the change effected on the aliment, from the analogy of plants with animals and from the anatomy and pathology of the digestive organs.

Absorption by the Veins.

Much credit is due to those who have revived and so ably maintained the ancient doctrine of absorption by the venous extremities. It is very evident that so important a fact, for I no longer consider it questionable, must have an important bearing on other physiological questions. To them I acknowledge myself indebted for the most important circumstances, which led to the opinion I entertain with regard to digestion. If there are any who, after scrutinizing the experiments of Magendie and his pupils, are disposed to doubt on this subject, I refer them to a series of experiments detailed in the Philadelphia Journal of the Medical and Physical sciences, as performed, apparently with great labour and accuracy, by Messrs. Lawrence and Coates. I refer them, also, to the experiments and opinions of a great majority of those whose circumstances have given them an opportunity to investigate the subject experimentally, for their own satisfaction. It will be found, that in many of these experiments, certain fluids, introduced into the stomach, were conveyed into the mass of the blood, although every communication of the absorbents with the blood vessels was cut off by tying the thoracic duct and the lymphatic trunks which enter the subclavian vein. All that can be expected from experiments upon the living system is certainly accomplished in these instances, and their results are as conclusive as the nature of the vital laws will admit. If any reliance is to be placed upon arguments drawn from such a source, they certainly furnish irresistible proof, that at least some substances are taken into the circulation by the medium of the veins of the stomach.

If any, however, are so fastidious as still to doubt, let them produce equally strong evidence that the *lympatics* do actually perform the function so long attributed to them. We should not suffer the revered names of Monro, Hunter, Cruikshank and others, to cast a spell over this subject, and to deter us from free inquiry. Let the physiologist adduce, from the writings of these men, evidence that the lymphatics absorb, equally strong with that upon which Magendie and others have founded their opinions.

I would not be understood to deny the function which the lymphatic and lacteal absorbents are supposed to perform. It would seem, however, that the lymphatics performed some other office. Their contents, so far from appearing to be excrementitious matters, consist of a fluid which is uniform in its sensible and chemical properties and which appears to be highly animalized, and resembling the most important constituent principle of the blood.

If the lymphatics exert no influence on their contents, but exist merely for the purpose of taking up that which has become useless, and conveying it into the mass of the blood, why, since nature accomplishes her ends by the simplest means, do not the absorbent branches terminate in the nearest veins, instead of traversing so great a distance to reach the left subclavian?

From the length of these vessels, which is greatly increased by the convolutions which probably take place in the lymphatic glands, from the nature of their contents and other circumstances, we are led to inquire whether they are not concerned in the important process of animalization, receiving certain parts of the blood from the capillary arteries, which they exalt to a higher degree of organic life. The manner in which their contents combine with the chyle in the thoracic duct, harmonises with the supposition; for we may rationally suppose that they exert an influence on the partially animalized, though comparatively crude fluid, introduced by the lacteals. By the theory at present received, the only fluid that nourishes the system, is here mingled with the refuse matters.

Different substances absorbed.

It being proved that certain coloured fluids are taken up by the veins of the stomach, we may fairly presume that the sudden, and hitherto unaccountable, disappearance of water, alcohol, &c. from the stomach, and its sudden appearance, under some circumstances, in the blood and the urine, is owing to its being thus absorbed. This phenomenon has been heretofore explained by attributing extraordinary energy of action to the absorbents of the stomach, or by supposing the existence of a direct communication between the stomach and bladder. Who, however, on being made acquainted with the fact that the veins of the stomach do absorb, and that they are incomparably more numerous than the absorbents, will not account for it by the supposition that these organs are principally concerned?

But if the veins absorb water, alcohol, and the prussiate of potash, it is certain that they will take up those simple aliments which are soluble in water. A solution of gelatin, for instance, must be presumed to be as readily taken up as the prussiate of potash, and so also all the fluid parts of animal substances. Many other substances are also capable of yielding their nutrient parts to water, and may also be taken up.

Again, if the veins of the stomach absorb any fluid substances except mere water, we do no violence to reason to say that they will, in some way or other, take up such substances as are insoluble, so far as we know, by any thing contained in the stomach. Most physiologists believe, that certain medicinal substances which are insoluble are, nevertheless, taken into the circulation by the absorbents. The fact appears to me to be supported by abundant proof. We know, also, very well, that the bones and other solid parts of the system, are sometimes taken up by either the absorbents or veins, with surprising rapidity. In some instances, the most intractable substances, when lodged in the flesh, have been absorbed and removed. * Plants, by their roots, decompose the soils on which they feed, and take up substances which are the most insoluble in nature.

How it is that these phenomena are accomplished, we cannot say. Perhaps they may be taken up in a state of extreme comminution, and perhaps they may be resolved, by the powers of life, into their ultimate, or in some instances, their proximate principles.

That our food, solid and fluid, should be taken up by the veins of the stomach with sufficient rapidity to be subservient to digestion, is certainly not absurd. Vascular and nervous as the stomach is, we are to presume that its vital energies, in this respect, are far superior to those of any other tissues in the system. The surface too, which it presents to the food, when we consider its folds and its papillæ, must be very great.

The villi of the intestines are known to peform absorption with great energy, and that of a substance which does not appear to be altogether fluid. The villi of the stomach are certainly more numerous than those of the intestines and, a priori, we might fairly presume, from their anatomical

* I would observe that this analogy is not objectionable on the ground that it is drawn from another kingdom. The organic functions are common to both plants and animals.

structure, that they performed the same function with greater energy. But by the prevailing theory of solution by the gastric juice, their function is entirely different, a circumstance noticed by Bichat as mentioned above.

Sensible properties of chyme.

The sensible properties of the chyme I have already mentioned as furnishing arguments against the opinion in question. They are not less important in the support which they yield our hypothesis. On the supposition that the villi of the stomach act upon the aliment as suggested above, electing from it such principles as are subservient to nutrition, the appearances of the chyme are precisely what we should expect. The sensible properties of chyme suggested to some of the older anatomists the idea that the aliment was, in the stomach, attacked by a myriad of small worms which reduced it to the uniform pulpy mass of this substance.*

That this may be effected by a myriad of absorbing mouths, the changes which take place in the aliment render not improbable. An extremely minute mechanical division would of course be

* Perhaps they were as near the truth as those who consider the process to be performed by a chemical agent, for neither the existence of these wonderful worms, nor of the not less wonderful fluid can be proved, and if we reason from the effect alone, we should with more propriety ascribe it to the former.

effected upon it, and perhaps many substances would be decomposed, some of their proximate principles being taken up. A change in the sensible and chemical properties equivalent to what we actually discover would of course take place. We should also expect that the chyme would be variable in its appearance as the food employed varies.

The manner in which different portions of aliment are successively changed is also much more satisfactorily explained by our hypothesis, for that only can be acted upon which is in contact with the surface of the stomach. This, as stated by W. Philip, by the peculiar action of the muscular coat of the stomach, is made to give place to another layer and the digested portion is conveyed to the pyloric extremity. The appropriate motions of the stomach are constantly evolving the central parts of the mass and bringing them in contact with the surface.

Effects of division of the eighth pair of nerves.

The results of the experiments of W. Philip harmonize much more perfectly with the opinion which we offer, than with the theory of solution. In mentioning the phenomena which result from the division of the eighth pair of nerves, we could not but admire how the process of digestion should be immediately suspended, even although the food in the stomach be imbued with the gastric liquor; particularly, as it is stated by the advocates of the latter theory, that the gastric fluid does, after death, corrode the walls of the stomach. If the most important influence exerted on the food is by the absorbing powers of the villi, and if the villi receive their power to act, as they undoubtedly do, from the eighth pair of nerves, the difficulty is at once solved.

Vegetables and Animals compared.

I have already suggested an analogy between the digestion of Plants and of Animals. This comparison may with propriety be carried much farther.

Vegetable life is under the control of the same ultimate properties which preside over the organic life of animals. Organic *sensibility* and *contractility*, variously modified, produce in both the functions of assimilation, circulation, nutrition, secretion, &c. Our logic therefore, does not forbid the employment of analogies drawn from the vegetable kingdom, the functions of which are less complex.

One of the characteristic differences between plants and animals is, that the former are fixed to the earth by their roots, the radicles of which take from the soil whatever affords nutriment to the plant and convey it immediately into the circulation, where it becomes assimilated to the different textures of the plant. Animals on the other hand, must have locomotion, and to render this consistent with digestion, the animal is furnished with an internal cavity capable of receiving those substances which yield aliment, though in a much more concentrated state than they are presented to the roots of plants.

Plants, it has been often observed, do, in their general structure as well as in their organic properties, bear a rude resemblance to animals. They have a cuticle, a cutis and a skeleton. Their sap vessels too, perform a circulation more simple, indeed, but not unlike that of the blood vessels of animals, and their leaves are obviously their lungs, in which their blood is attenuated and exposed to the action of the air. They have also their secernents and exhalents. But to what in the animal economy shall we compare the radicles of vegetables? I think it will appear that there is no forced resemblance between them and the villi of the stomach. The roots of plants, indeed, are of comparatively great length and necessarily so, for plants having no prehensive apparatus by which their proper aliment is elected and brought in contact with their roots, the latter must go in quest of it, ramifying to a considerable extent from the stalk of the plant and drawing nourishment from a great volume of soil. Hence also the roots of plants are constantly elongating and multiplying. The radicles of vegetables are indefinitely small and numerous, much more so than one would imagine, who has merely examined such as are torn from the earth. If they are raised with a large volume of earth and this be carefully washed away, they will be found as minute and numerous as the villi of the stomach. The *radicles* of the stomach are very differently circumstanced. The substances from which they are to elect food are, in a much more concentrated state, conveyed into the cavity of the organ, and by its muscular motions successively applied to its surface. Yet they probably have, in some degree, the power of seeking their food by elongating themselves, and by the mobility of the surface to which they are attached.

By the prevailing theory of digestion, however, an action, by no means analogous to that of the radicles of plants, is attributed to the villi of the stomach, for these last are supposed merely to exhale a fluid which acts chemically upon the contents of the organ. Thus too the digestion of an animal must be less under the control of vital or organic properties than that of the humblest plant; for in the latter, nothing appears to be left to the agency of physical properties, but all to be accomplished by the vital or organic powers. The radicles of plants are endued with the surprising power of acting upon the most intractible substances and conveying them into the circulation, thus silex is detected in the textures of some plants.

We should expect, in rising from the lower to the higher grades of existence, to find the functions more purely and exclusively under the control of those laws which constitute life, but if the above theory be true, the laws of inorganic matter have, so far as we can ascertain, more influence over the animal than the vegetable functions. Is it not then more rational to infer from the analogy of structure which I have pointed out, a corresponding one of action, and that the villi of the stomach are endued with the power of electing and absorbing alimentary substances?

If the above analogy does not carry with it conviction, yet I am persuaded that to those who take a comprehensive view of the works of nature and the relations which the different orders of beings bear to each other, it will at least be a corroborating circumstance.

Digestion of the inferior animals.

The phenomena attending the digestion performed by some of the inferior animals are highly interesting, and as the process in them may be presumed to be more simple, they may assist to elucidate our subject. I have already spoken of the digestion of the polypus. From its being properly classed with animals it has been presumed that its digestion is performed in a similar way, and that the substances taken into its cavity are acted upon by a gastric juice secreted for that purpose ; yet no one has ever demonstrated this fluid, or that the digestion of the polypus is effected by it.

This animal is, by its structure, scarcely distinguished from vegetables. The only circumstance, which characterises it as an animal, is the fact that instead of deriving its nourishment by means of roots from the earth, it takes it up from an internal cavity into which it is conveyed. If then we are to presume any thing with regard to the digestion of this animal, is it not more rational to suppose that the digestion of a being which only differs from a vegetable in the nature of its food, and in its taking it up by an internal instead of an external surface, will perform this function nearly in the same manner with a vegetable ? This supposition furnishes us with a beautiful and easy gradation of function, as well as of structure, from the vegetable to the animal kingdom.

But there are animals which may be regarded as of a still lower order than the polypus, and which do not even possess the internal cavity of that animal, but take up their nourishment by means of a kind of placental surface. Does any one believe that they perform digestion by means of a gastric juice?

While amusing myself with angling, I once took a trout which had swallowed one of its own species of half its own length. The tail was still out of its mouth, and only the head and a small part of the body were in the stomach, and these parts were considerably digested. The appearance which it presented was something different from what I had anticipated on the supposition that digestion is performed by the gastric juice. It appeared as if a considerable part of the substance of the fish had been eaten away, while that which remained upon the bones was altogether unchanged. In the stomach was nothing like chyme nor chyle, that I could discover, but merely a frothy mucus. I had at that time no favourite theory to support, and even then, the circumstance staggered very, much, my belief in the commonly received theory of digestion.*

Assimilation effected by capillary vessels.

But I fancy that my opponents are impatient to inquire of me in what manner the changes of as-

* In the writings of Sir William Jones is to be found an account of an animal named the Pangolin, the stomach of which was frequently examined by the author, and never any thing but mineral substances found in it. The singular circumstance led him to query whether the animal did not derive its nourishment from this source, and by a process in the stomach similar to that performed by the roots of plants.

similation are wrought, and what apparatus is appropriated to that function, in case the process is so little effected in the cavity of the stomach. I reply, that it is accomplished in the capillary and other vessels, through which the aliment is made to pass before it is added to the circulating mass of the blood.

It is obvious that whatever is absorbed by the veins of the stomach will be first conveyed through the tissue of vessels indefinitely minute, which may be said to compose the villi of the organ; next through the branches which ramify in the cellular coat, through the long tortuous vessels of sensible magnitude meandering upon the suface of the stomach and between the laminæ of the peritoneum.

This, however, is not the most interesting part of its course. On reaching the trunk of the vena portarum, along which it is slowly propelled, probably by the action of that vessel, it is conveyed to the great fissure of the liver, to be diffused through that large and important viscus, having first been mingled with the returning blood of the spleen, the pancreas, the omentum, &c. It is then again subjected to the action of a congeries of capillaries constituting the substance of the liver.

Here, then, is an ample opportunity for all those changes to be wrought, which may be deemed necessary for the conversion of aliment into blood.

This supposition too, accords well with what we observe in the performance of other functions, by which changes are wrought in the properties of circulating fluids, viz. that they are accomplished not upon lage volumes and in large reservoirs, but upon attenuated streams and in minute vessels.

I have heretofore objected to the prevailing theory, that it does not appreciate the importance of the liver nor attribute to it an office corresponding to its magnitude, structure and pathological consequence. The hypothesis which we offer exalts it to the highest degree of physiological importance, regarding it as, perhaps, the most efficient organ concerned in animalization.

Circulation of the vena portarum.

Anatomists have heretofore had no settled opinions with regard to the end for which the blood is circulated through the vena portarum. Some have supposed it to be merely preparatory to the secretion of bile; others, that it is for the purpose of preserving an equilibrium in the circulation of the blood. With regard to the first opinion, the secretion of bile does not appear to be a matter of sufficient importance to account for all this appropriation of apparatus. The latter does not better comport with what we observe of the means which nature employs to accomplish her ends.

That a system of vessels is furnished merely for the purpose of keeping a portion of the blood for a time out of the general circulation, is too much like the belief that the spleen is packed into the abdomen just to fill up the vacant space and to make all snug, and for the same reason that a traveller tucks an old coat into his trunk. These minor objects are undoubtedly accomplished in the animal *economy*, but never at such an expense of means. Such a wonderful harmony pervades the whole, that these circumstances must be presumed to follow in the train of those which are more important.

In some respects the circulation through the abdominal vessels is even more interesting than that through the lungs. Their contents are made twice to pass through capillary vessels, the trunk of the vena portarum performing between them an office something like that of a heart.

Phenomena of the Fœtal circulation.

A reference to the phenomena of the fœtal circulation will, as I think, tend to confirm what I have advanced with regard to the functions of the liver and the vena portarum, and also to display a beautiful correspondence between the changes which at the time of birth, take place in this circulation and that of the lungs, that, so far as I know, has not been heretofore pointed out.

It is well known that previous to birth the liver predominates over all the other viscera. It is undoubtedly thus early developed for important purposes, performing, at that time, some indispensable office in the fœtal system. It is conjectured by some that at that time it exerts an influence on the blood which is subsequently assumed by the lungs. The blood which is received from the maternal system is conveyed to the liver by the umbilical vein. It there enters the portal sinus, a part of it being taken up by the ductus venosus, while a large proportion is conveyed by the ramifications of the vena portarum through the glandular structure of this organ. It is not in vain that the fœtal blood is thus circulated through the liver, it is undoubtedly for the purpose of its being wrought upon by the capillaries of that gland, and it is for this purpose that the liver is thus early perfected.

In the fœtal state the system of the vena portæ is organized at the same time with the digestive organs, but, like the vasuclar system of the lungs, it is in a great degree dormant, the quantity of blood which it conveys being merely sufficient for their nutrition. The moment, however, that digestion begins to take place after birth, these organs become centres of fluxion, and the vena portarum becomes charged with blood possessing peculiar properties which, in some way or other, it receives in the stomach, intestines &c.

At birth the circulation by the umbilical vein immediately ceases, and its place, as soon as digestion commences, is supplied by that of the blood of the vena portarum. Must we not infer, then, that this last is circulated through the liver for the same purposes as the former? for it is conveyed in the same vessels, and we cannot suppose that they are at once endued with a new action, entirely different from that which they had before performed and which is merely for the purpose of furnishing bile, a fluid which is not needed in the fœtal state, as no digestion then takes place.

Here, then, as well as in the lungs, we see a surprising change taking place in the circulation at the time of birth, not so promptly, indeed, as that of the lungs, nor so immediately essential to the continuance of life, but not less so in its ultimate effects. We have every reason to think that, as is the case often in the lungs, so also in the portal system an imperfect establishment of its circulation, either from organic defect in the liver or some other organ, is often the cause of infantile diseases marked with indigestion and imperfect assimilation and which often terminate fatally. Who, that has been engaged in extensive practice, has not witnessed the frequent occurrence of infantile jaundice, and that too taking place as soon as digestion has commenced, and in children born apparently healthy.

An interesting case of the kind has recently occurred in my own practice. The birth of the child was supposed to be premature by three or four weeks, and occasioned by a rupture of the membranes and discharge of water, at first unattended with pains. The child, when born, was of the natural complexion and apparently well; but as soon as it began to take the milk of the mother the skin began to assume a deep yellow hue, the abdomen swelled and digestion appeared to go on imperfectly. The bowels were obstinately costive. By the use of the blue pill, accompanied with other laxatives, the health was in a measure restored, the skin assuming a healthy complexion and the digestive organs performing their functions more perfectly. Still, however, all was not right; the bowels remained tumid, and the costiveness such as to require the occasional use of a cathartic. The birth of the child occurred in the spring. These symptoms continued during the summer and following winter; the child grew, however, and was thought by the parents to be otherwise well. At the age of one year, by my advice, the child was weaned. This change appeared, for a time, to be favourable to its health. It was found necessary, however, to restrict it to the most simple and easily assimilated articles.

Cows milk it could never take. The article which appeared to be most suitable was arrow root, and this was for a time almost exclusively used.

In the summer, however, bowel complaints began to prevail among children. It is to be observed also, that at Burlington, Vt., where the case occurred and in its neighbourhood, intermittent fevers occasionally happen, and in the summer most diseases exhibit a bilious character. At this time the little sufferer became afflicted with a kind of lintery, its food appearing to glide through the alimentary canal without having undergone any change, and accompanied with a copious secretion of mucus. The treatment which I found successful in similar cases proved unavailing. I then directed that it should be removed into the country remote from the atmosphere of the lake; in this also we were disappointed. At this time, and when we had given up all confidence in medicine, and almost entirely our hope of its recovery, the mother suggested to me that the child had manifested a desire for the breast. I immediately advised that a healthy wet nurse should be procured, but the child would not suffer itself to be taken by any woman but its mother. I then directed that it should be fed with the milk of a healthy woman, whose child was six months of age and perfectly healthy. Immediately every thing was changed; the diarrhœa ceased; the restlessness, which had been extreme, was succeeded by refreshing sleep; the appetite improved and the child took eagerly of the breast milk, although it would not swallow a drop of cows milk.

The little patient gradually recovered health and strength, but whenever any other food was substituted, the same symptoms recurred.

On the approach of cool weather, however, the breast milk was gradually withdrawn, and the digestion of the most simple aliments appeared to go on in the regular and healthy manner, and the child was restored to perfect health.

I know very well, that these causes are not unaccountable upon the supposition that digestion is effected by the gastric juice, and that the most important function of the liver is the secretion of bile; but are they thus accounted for in an equally satisfactory way? and does the explanation harmonize equally well with what we discover in the anatomy of these organs? We have shown, that the secretion of bile is probably effected from the arterial blood of the liver. The circulation of this, we know, is perfect in the fœtal state, and that no great change is effected in it at birth; but as a new circulation by the vena portarum occurs, and as by our hypothesis, the blood conveyed by it is charged with alimentary matters which may be presumed to produce a new excitement in the liver, it is easy to conceive, that any organic defect in the liver or an imperfect preparatory assimilation in the branches of the vena portarum, which originate in the stomach and intestines, may derange all the functions of the organ. Pathological and physiological phenomena harmonize beautifully with the supposition, and certainly furnish us with very plausible arguments. That they may be duly appreciated and receive their proper weight, is all that I ask.

In conclusion of this section, I would point out an interesting analogy which follows from our hypothesis, between what may be called digestion in the fœtal state, and digestion after birth. It is pretty well ascertained and if I mistake not, generally believed, that the fœtus must be nourished from the blood of the mother, by means of the absorbing powers of the capillary veins in which the umbilical vein terminates, and which appear to constitute a considerable part of the fœtal portion of the placenta. By our hypothesis the function at birth, is transferred to the capillary veins of the vena portæ, which as above, appears to perform precisely the same office in relation to the liver, that the umbilical vein does before birth. Let it be carefully noticed too, that *they are the opposite extremes of the same continuous vessel.*

Another circumstance which strengthens the analogy between the nourishment of the fœtus and digestion after birth is, that as a part of the nutriment in the latter case is, without circulating through the liver, as I shall by and by more particularly state, conveyed by the thoracic duct into the venous blood, to be circulated through the lungs before it serves the purposes of nutrition; so also in the former, a portion of the fœtal blood, by the ductus venosus, immediately enters the heart, and as it has in the placenta undergone that change which after birth is effected by the lungs, it is immediately fit for nutrition and is distributed for that purpose.

In considering this subject, we are very naturally led to inquire whether the blood of the vena portæ exhibits any properties which would favour the belief that the veins of the stomach and intestines do largely absorb alimentary substances. The unbiassed observations of many, furnish us with a satisfactory reply. Dr. Rush, who quotes authorities in support of the assertion, tells us that the blood of the vena portæ is more crude than the blood of other veins, that it coagulates more slowly, and that it undergoes putrefaction less readily; circumstances which decidedly indicate a less degree of animalization. I believe it is commonly thought by physiologists, that the blood of this system differs more from arterial blood than that of other veins.

Haller asserts that he detected oil in the blood of the vena portæ. We may have the more confidence in his observation, as he was not at the time searching for appearances of this kind for the purpose of substantiating a favourite hypothesis.

The experiments of Flandrin, as quoted by Magendie, are decidedly in point. "In the horse, the substances contained both in the large and small intestines, are generally mixed with a large quantity of liquid, which is more or less abundant, as we approach towards the rectum; it is ábsorbed, as it passes over this part of the intestinal canal. Now Flandrin ascertained, that the fluid contained in the lacteal vessels did not possess any odour analogous to that of this intestinal fluid: but on the other hand, that the venous blood of the small intestines had sensibly an herbaceous taste; that of the cœcum had a sharp and slightly urinous taste; that of the colon possessed the same character in a more remarkable degree. The blood in the other parts of the body presented nothing of the kind. A half pound of assafœtida dissolved in an equal quantity of honey, was given to a horse; the animal was afterwards fed in the usual way, and killed in about sixteen hours. The odour of the assafœtida was very distinct in the veins of the stomach, small intestines, and cœcum; it was not remarkable in the arterial blood nor the lymph." p. 307.

It appears to me surprising that the result of these experiments, which are of such a decided character, did not suggest the hypothesis which, I think I may say without vanity, we have found at least very plausible, upon other grounds.

It is not to be supposed, however, that, under ordinary circumstances, the presence of aliment in the vena portæ is to be easily demonstrated. We must recollect that, during digestion, the circulation in the exceedingly vascular organs concerned, is much increased, that the alimentary matters are also mingled with the returning blood of the spleen, pancreas, &c., and that hence, it must be greatly diluted.

Another inference which may be drawn from the above experiments is, that the portion of the aliment which is found in the vena portæ is in a much more crude state than that of the thoracic duct.

Objections to the hypothesis of Absorption.

The arguments which would seem to oppose the opinions which I have endeavoured in the foregoing pages to substantiate, are many of them the same which favour the theory of solution by the gastric fluid. I have already attempted a refutation of them. Other difficulties which appear to arise from the structure of the organs concerned in this function, I have endeavoured to solve. There are some, however, which present themselves, of which we have not found occasion to speak.

It might, perhaps, be thought that if the veins of the stomach, in addition to their transmitting the blood which they receive from the arteries, do also absorb and convey so great a volume of aliment, they should be found in greater number, in proportion to the arteries, than in other organs.

In reply, it is only necessary to call to mind that the arteries of the stomach during digestion are exhaling as great a volume of fluid as is absorbed by the veins. Here we may notice, too, in the supposition, an interesting correspondence of action which supports the doctrine of venous absorption.

That the arteries of many organs copiously exhale, no one doubts. That the veins absorb is not so demonstrable; but when we take into consideration the relation between the arteries and veins, that the arteries convey the blood *from* the centre of the system, and that the veins *return* it, absorption is certainly the function which, in the veins, corresponds to exhalation of the arteries.

That we shall be able to prove or disprove the function which I attribute to the villi of the stomach, by comparing the weight of the food taken in, with what remains at the time that it begins to be transmitted to the intestines, I think very doubtful. Its consistence is, as we have stated, entirely changed, and its volume and weight are so much increased by the exhaled fluids, that the removal of its more nutrient parts might not be appreciable.

The cases of mal-formation, related by Abertheny and Lawrence, would, at first view, operate against our hypothesis. I think, however, that they are capable of an explanation which gives it additional weight. It will be remembered that the hepatic artery, in these cases, was much larger than usual. Now it is obvious, that the whole mass of blood must have been in a short time subjected to the action of the liver. Usually the aliment is conveyed by the vena portæ to be acted upon by the liver before it is added to the mass of the blood. In the above cases there is this difference, that the aliment must first have been mingled with the mass of the blood, after having undergone the action of the absorbing veins of the stomach and intestines, before it was acted upon by the liver.

These cases, then, bear a striking resemblance to those of mal-formation of the heart, in which life is for a long time supported, although but a small proportion of blood passes through the lungs. The spleen and the pancreas would, probably, under such circumstances, vicariously assist the liver.

I presume that it will be inferred, from what has already been said, that I by no means deny *in toto* the functions attributed to the lacteals. Their office is, undoubtedly, to take up a portion of the aliment, to subject it to their own action and that of the mesenteric glands, and finally to pour it into the mass of the blood. They probably absorb that part of our food which is capable of being converted into some one of the constituent parts of the blood, without undergoing the action of the liver. What part of this fluid it is which is furnished by the lacteals, and what by the liver, I am unable to say. It has been supposed by some, that the latter organ was concerned in elaborating the red globules. That it assists in completing some one or more of the constituent parts of the blood, I think will follow from what has been said.

The argument against the theory of solution and that in favour of immediate absorption, will reciprocally aid each other. If the objections against the former are founded in truth, they render the latter the more probable; and if the latter hypothesis is supported by analogy and by fact, the objections to the former are the stronger. I have to request of the reader that he will view this attempt in all its parts, and estimate their united weight.

In conclusion of this essay I would observe, that I am aware of the caution with which objections to a long established system are received.

It was said by a distinguished logician, Dr. Johnson, that "After a system is well established upon positive evidence, a few partial objections ought not to shake it. The human mind is so limited that it cannot take in all the parts of a subject, so that there may be objections raised against any thing."

We cannot, indeed, be too well guarded against perplexing sophistries of this kind, but if the reader will take the trouble to review the objections which have been adduced against the theory of digestion by the gastric juice, he will discover that so far from being partial, they have a very general bearing upon the system in question, and that they are intended to meet the arguments upon which it chiefly relies. It is to be recollected too, that this system is by no means founded upon positive evidence, but such as is, at best, equivocal.

ERRATA

14, 2d line from the end (the physiological, &c.) read (see 4) (the physiological, &c.)

34, 17th line from the bottom, for [illegible], read chyle

36, 5th line, for *lymphetics*, read *lymphatics*.

ERRATA.

Page 7. 2d line, for *and* (the physiological, &c.) read *with* (the physiological, &c.)

,, 54. 7th line from the bottom, for *chyme,* read *chyle.*

,, 66. 5th line, for *lympatics,* read *lymphatics.*