

An inaugural experimental dissertation on digestion : submitted to the examination of the Rev. John Ewing, S.T.P. provost ; the trustees and medical faculty of the University of Pennsylvania, on the 17th day of May, 1796 ; for the degree of Doctor of Medicine / by John Wilson, A.M. of Pennsylvania, member of the Philadedhhia [sic] Medical Sosiety [sic].

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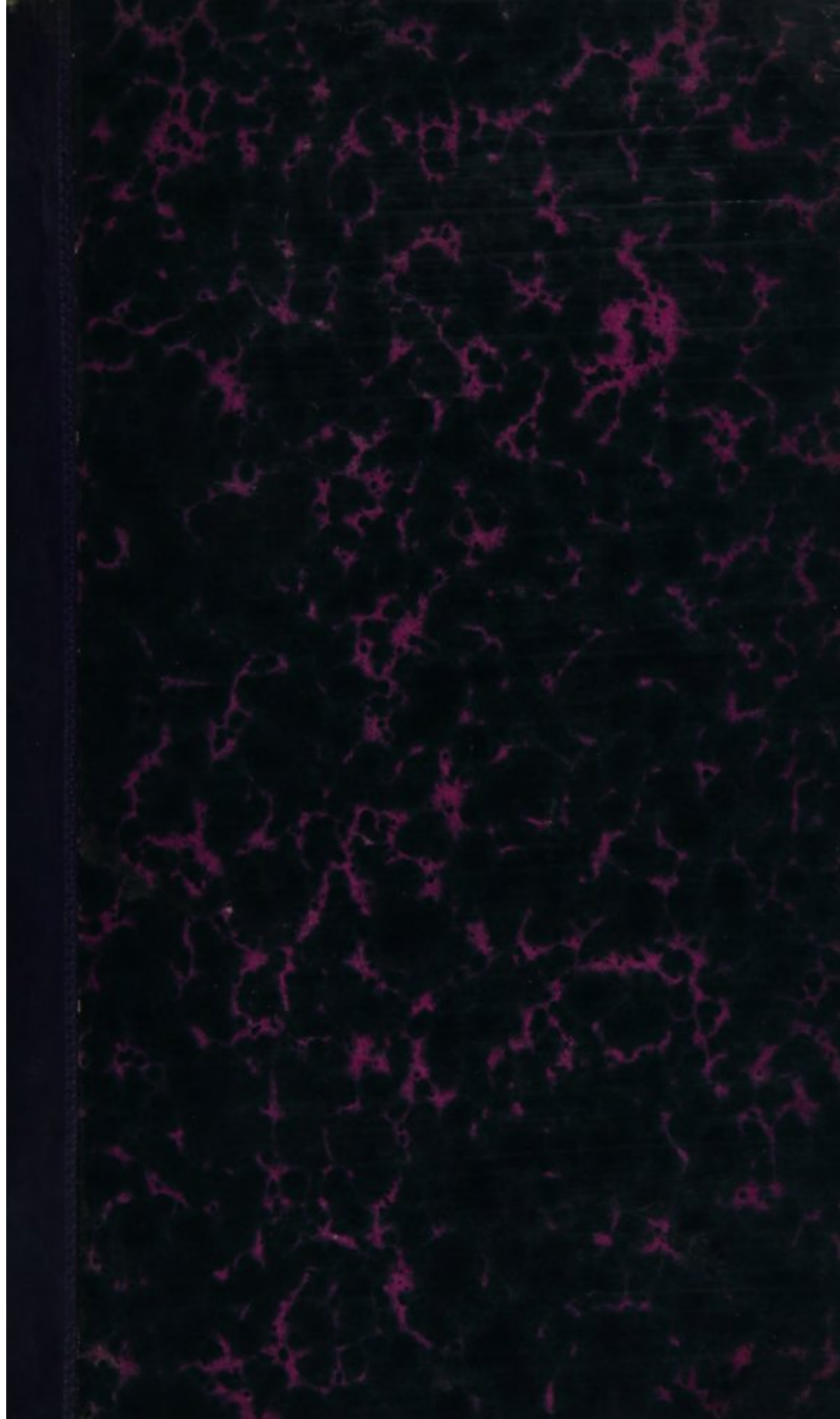
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AN
Inaugural Experimental Dissertation
ON
DIGESTION,
SUBMITTED TO THE EXAMINATION OF THE
REV. JOHN EWING, S. T. P. Provost;
THE
TRUSTEES AND MEDICAL FACULTY
OF THE
UNIVERSITY OF PENNSYLVANIA,

On the 17th day of May, 1796.

FOR THE DEGREE OF DOCTOR OF MEDICINE.

By John Wilson, A. M. of Pennsylvania,
MEMBER OF THE PHILADELPHIA MEDICAL SOCIETY.

Aggrediar, non tam perficiendi, quam experiendi voluntate.
CIC. ORAT. ad BRUT.

PHILADELPHIA,

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TO

Caspar Wistar, M. D.

ADJUNCT-PROFESSOR OF ANATOMY, SURGERY,
AND MIDWIFERY,

IN THE

UNIVERSITY OF PENNSYLVANIA;

T H I S D I S S E R T A T I O N

IS RESPECTFULLY INSCRIBED,

AS A SMALL TRIBUTE OF GRATITUDE AND ESTEEM,

BY HIS MUCH OBLIGED FRIEND AND PUPIL,

JOHN WILSON.

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Introduction.

SINCE the works of a Fordyce, a Hunter, and, above all, the ingenious Spallanzani have appeared on this subject, a further investigation seemed unnecessary ; but as physiologists were not settled in their opinions, and as some useful experiments have heretofore been neglected, which might have a tendency to throw some light on so interesting a subject, this small attempt may not prove unacceptable.

But notwithstanding this, I am laid under an obligation of an higher nature ; as the laws of this Seminary require that every candidate for medical honours should exhibit before the Trustees and Faculty, some evidence of his improvement in the principles of that art which has been the ground of his studies ; therefore, necessity, not choice, prompts the unexperienced Student to attempt the investigation of something new, or take the tedious round of dull repetition—The former ensuring upon his unde

fended head the piercing shaft of criticism, while the latter is sure to create disgust.

In a subject so difficult and obscure as the animal œconomy, it is not surprising, that philosophers should be so often embarrassed; and that instead of certainty, they should frequently be obliged to determine her operations by a seeming probability, or by a vague and distant analogy.

All animated beings seem to be subject to certain laws calculated for the purpose of their œconomy. These laws dispose them to certain modes of action, which occasion a diminution either of their fluids, or solids, or both.

Possibly they may remain in a dormant state for some time without sustaining any diminution or loss of substance. But when a loss is sustained, it is necessary, that it should be supplied by the addition of some new matter. If this matter is different in its qualities from the matter of the body which is lost, it is necessary that it should undergo some change, and be converted into a substance endued with the same qualities, as that which was lost.

Besides the continual waste which animals are suffering, there is a certain period of their lives in

which they are acquiring new parts, and encreasing those already formed. In consequence of this, it is necessary that food should be employed to supply matter for this formation and increase.

For this purpose, there is a certain process in the organs of digestion of living animals, through which food has to pass, before it can be animalized and get into the general system of vessels. It must, therefore, be presumed, that food has to undergo some change, in consequence of its delay in those organs.

Although the Organs of Digestion in different animals are so various, it is most probable, that those changes which take place in the food of different animals are similar ; their mode of life only rendering a different apparatus necessary.

But our present enquiry is not about the situation and structure of those organs, or the quantity or quality of food best adapted to them ; it is about the mode of assimilating those substances to an animal nature, whereby a fresh growth of parts may be maintained, or a constant loss of substance renewed.

A N

INAUGURAL DISSERTATION

O N

D I G E S T I O N.

THE human frame, like every other machine, would necessarily wear out by the mutual attrition of its parts, without a constant renovation. For this purpose we are endued with the peculiar appetites of *hunger* and *thirst*. The disagreeable sensations arising from a non-indulgence of these, and the extreme pleasure produced by their gratification, stimulate us to take in alimentary substances; and these having gone through the process of *digestion*, are converted into fit nutriment to support the various parts of the body.

The substances best adapted to this purpose are taken from the animal and vegetable kingdoms. These, under various forms and modifications, con-

stitute the proper food for the body. But the mode of operation in the process of Digestion, has been a subject of considerable altercation among physiologists.

Some physiologists have asserted that the stomach operates in digestion principally by a triturating power. Another opinion concerning Digestion was, that it is effected by fermentation.

Since the experiments of the ingenious Spallanzani, mechanical agency, by trituration, in the process of Digestion, has no longer any weight among philosophers. It remains, then, to consider what *chemical* change really does take place, whereby such heterogeneous substances are reduced to one simple homogeneous matter, called *chyle*. This change has been ascribed either to fermentation, or solution, or both.

The doctrine of *fermentation* was much in vogue about the middle of the last century, when an explanation of the various functions of the human body was sought for in fermentation. It appears that Boerhaave first opposed the generally received opinion of fermentation in the animal functions, but still admitted a limited degree of it in the act of Digestion. He supposes that nothing prevents a complete fer-

mentation of the food taking place in the stomach, but its short continuance there. Some modern physicians, viz. Pringle and M'Bride, think this limitation too great, and maintain that a compleat fermentation really does take place in the process of Digestion. But let us take notice that their experiments were made out of the body, and their conclusions drawn from its being produced in this situation by adding saliva to alimentary substances. Dr. Rush also appears to conclude in favour of this latter opinion from the like experiments, as well as those made in his own stomach.

Indeed, when we consider the circumstances of the food during Digestion, there certainly is a strong presumption in favour of this doctrine. But notwithstanding this, and the nearly convincing arguments of such ingenious advocates, I am led to adopt a contrary opinion from my own experiments, and those of others. Besides, the continual influx of fresh humours into the stomach, and the short continuance of alimentary substances there, strongly induce me to believe that it is not a fermentative process.

The different species or stages of fermentation, as described by chemists, are the *vinous*, *acetous*, and *putrefactive*. The putrefactive stage of fermentation, few, or none, now believe to have any agency in

Digestion. On the contrary, from a variety of experiments made to ascertain this fact, I found that putrid aliment is incapable of being converted into chyle before it is sweetened by the action of the gastric juice upon it. Those who argue in favour of the vinous and acetous stages of fermentation, maintain their opinion from particular phenomena frequently attending the process of Digestion; such as acid eructations and vomitings, the acid taste of the gastric juice, and internal coat of the stomach, &c.

That such appearances frequently do take place cannot be denied; but whether they are the natural effects of the digestive process, or unnatural incidental occurrences, owing to a morbid state of those organs, or to an improper quality or quantity of food, is a matter of contention. To the latter opinion I am strongly inclined, both from experiments and otherwise.

To discover whether the quality of food has any influence on the digestive organs, I fed a number of dogs, for some time, on animal food alone. They were then killed about four hours after eating fresh animal substances, at which time Digestion was considerably advanced; but neither the gastric juice of those animals, or the contents of their stomachs shewed any signs of acidity when touched with an

infusion of blue vegetables. According to those experiments, animal substances which seem to be the most natural food of those animals, do not produce the least acidity in the digestive organs in a healthy state, and still digestion was completely performed. But as uniformly as I fed them with vegetable, or with animal and vegetable substances together, I found an acid to exist in the stomach and duodenum of those animals. I also made a number of other experiments differently modified, and they uniformly rendered similar results.

Mr. Hunter very properly observes, " That fermentation can go on in the stomach, there is no doubt ; but when this happens, it arises from the powers of digestion being defective." *Animal Economy*, p. 161.

And again, the same author says, " That it may be admitted as an axiom, that two processes cannot go on at the same time, in the same part of any substance ; therefore, neither vegetable nor animal substances can undergo their spontaneous changes, while digestion is going on in them ; a process superior in power to that of fermentation." P. 163.

Having found, by the former experiments, that an acid is not necessarily produced in digestion, I made a number of experiments out of the body with water,

saliva, and the gastric juice, in different alimentary mixtures, to find at what time fermentation really does begin.

Experiment I. Equal parts of boiled pork, parsnip and leavened bread, were put into two vials. In one vial water alone was added to these substances; in the other equal parts of saliva, and water. These mixtures were submitted to a sand heat equal to the human body. No fermentation was discovered till between four and five hours. Air-bubbles first appeared in the vial containing saliva.

Experiment II. Leavened bread and boiled pork were put into two vials. In one vial water was added, and in the other saliva. These were submitted to a heat equal to the human body; but in neither of these could fermentation be discovered till towards five hours, either by the escape of air-bubbles, or by an infusion of blue vegetables.

Experiment III. Unleavened bread, roasted beef and potatoe in equal quantity were put into two vials. In one vial water, and the other saliva, and water were added to these mixtures. They were then submitted to a sand heat between 96° and 112° of Fahrenheit's thermometer. A small degree of fermentation was perceived at the end of four hours. The reason

of fermentation beginning sooner in this experiment, than in the former, may be accounted for from the heat being something greater than that of the human body.—The vials containing water alone in the foregoing experiments, were used only by way of comparison, and the chief difference appeared to consist in the escape of air-bubbles, and not in the change of colour produced by an infusion of blue vegetables.

Experiment IV. I obtained a quantity of pure gastric juice from my own stomach. This I mixed with equal quantities of saliva and water in similar mixtures with the former experiments, and submitted them to a heat equal to the human body. They remained much longer than any of the former experiments before they shewed any signs of fermentation. The gastric juice, in those experiments, shewed as strong antizymic powers as it had done, in former experiments, antiseptic; and both powerfully oppose the doctrine of fermentation, either putrefactive or acetous, in the process of digestion.

Finding that little difference took place, in the former experiments, between alimentary mixtures with water and those with saliva, I was induced to believe that any other fluid of the body of equal viscosity (the gastric juice excepted) would shew signs of fermentation with any alimentary matters, as soon

as with faliva. I therefore made a number of experiments with the serum of blood alone, and mixed with water, in different preparations, under similar circumstances with the former experiments; but was never able to discover any difference, either by the escape of air-bubbles, or when touched with an infusion of blue vegetables.

I am, therefore, of opinion with Dr. Fordyce, in opposition to Dr. M'Bride, that the faliva has no fermenting principle; and he very justly observes, that " Dr. M'Bride grounds his opinion on experiments made with pieces of meat and water mixed together alone, and pieces of meat, water and faliva mixed together in similar vials; upon letting them stand, air-bubbles were found in the vial, in which the meat, water and faliva were contained, before there were any found in the vial, in which the pieces of meat and water were alone contained. The result was the same when bread and water; and bread, water and faliva were compared: and also, when bread, meat and water; and bread, meat, water and faliva were compared. But no deduction can be made from these experiments, by which the power of the faliva to induce fermentation can be grounded; because the faliva giving viscosity to the water would prevent vapours from rising in small and imperceptible bubbles; and

would retain them until they became more numerous, and until they united together so as to become more sensible." *Fordyce on Digestion*, p. 53 & 54.

It may be asked, at what time does Digestion begin to take place? From a number of experiments made on dogs to ascertain this point, I found it to be immediately after food had been taken into the stomach. Now we know that fermentation cannot take place in any known circumstances out of the body in less than four or five hours, when a ferment is not added, in which time the whole process of digestion is completed. That fermentation may take place towards the latter end of digestion, will not be denied; but this is far from being a necessary consequence to digestion, as Dr. Rush supposes: it is nothing more than an accidental circumstance necessarily taking place from food being too long retained in the stomach, when the power of the digestive organs has been too feeble to assimilate it, before this natural change takes place.

To put the subject beyond all possibility of controversy, I instituted a number of experiments heretofore untried, and which were as disagreeable as new.

Experiment I. Having breakfasted upon coffee, leavened bread and butter, and a little boiled ham,

by way of relish—four hours afterwards, I took an emetic, which brought up the contents of my stomach. These I submitted to distillation and re-distillation, several times repeated; but I could not discover the smallest appearance of spirit of wine throughout any part of the process.

Experiment II. Having dined upon roasted beef, leavened bread, potatoes and water—three hours after I took three grains of emetic tartar, and threw up the contents of my stomach. This appeared acid to the taste, and imparted a slight red colour to a blue infusion of vegetables, as Dr. Rush very justly remarks in his experiments; but after submitting these to distillation, as in the former experiment, I could not produce any signs of spirit of wine.

These experiments, though extremely disagreeable I repeated at a time when I enjoyed the most perfect state of health, and with exactly the same issue.

Experiment III. But lest it should be thought that the experiments made on my own stomach were not sufficient to establish the doctrine, I obtained the favour of a healthy man in the House of Employment to assist me. He dined heartily on leavened bread, boiled pork, parsnip and potatoe, and a little water

by way of drink. Three hours after he discharged the contents of his stomach by an emetic.

Experiment IV. The same man having dined upon leavened bread, beef-stake and molasses beer—four hours afterwards he took an emetic, and threw up the contents of his stomach. The two last experiments were treated by distillation, similar to those made on my own stomach, and with exactly the same result.

These experiments appeared sufficient to disprove the doctrine of fermentation in the process of Digestion. Nevertheless, if the spirit of wine should accidentally appear in distilling the contents of the stomach, I could not subscribe to the doctrine of fermentation necessarily having any agency in Digestion; because the digestive organs may be so slightly debilitated, or be under so small a degree of morbid excitement, as scarcely or not at all to be perceived; and under these circumstances fermentation of a vinous nature might readily take place. This is a state, every person is very sensible, frequently takes place in his stomach; but who, under such circumstances, would ever suppose this to be either natural or necessary to Digestion?

In this manner I would account for an acid being produced in the stomach, when an acetous acid is generated there. But, is the acid most commonly found in the stomach, the pure acetous acid? Is it not more properly called the phosphoric acid, which is evolved in the time of digestion, or otherwise the carbonaceous matter of the saccharine part of the alimentary mixture strongly attracting pure oxigene, forming thereby carbonic acid, so frequently found in the stomach?*

Now, seeing we so constantly observe that fermentation, combustion, and elective attraction produce new and different changes in bodies independent of each other; may we not consequently look for other changes to take place in the properties of bodies entirely different from either? This we see in the process of Digestion, whereby two or more substances are decomposed, and re-combined in another

* After having written the above pages, I met with the following Letter de M. Reynior, a M. de la Metherie.

The writer of this letter informs M. de la Metherie, that professor Struve and M. Maquart, have discovered that the phosphoric acid and volatile alkali are the two essential constituent parts of the gastric juice; and M. Struve has composed a liquor which acts on alimentary matters, in the same manner as the gastric juice.

Journal de la Physique.

manner by a process *sui generis*, and thereby forming a *tertium quid*, possessing properties widely different from its constituent principles. And, as Mr. Hunter observes, "The process of Digestion differs from every other natural operation in the change of bodies. It is by no means fermentation, though it may somewhat resemble it. For fermentation is a spontaneous process, and is that natural succession of changes, by which vegetable and animal matter is reduced to earth; therefore, must be widely different from Digestion, which converts both animal and vegetable substances into chyle; in the formation of which, there cannot be a decomposition similar to fermentation.

"Digestion is very different from chymical solution, which is only an union of bodies by elective attraction, not a real change of the substances themselves, but of their properties. But digestion is an assimilating process, and in this respect is somewhat similar in its action to *morbid* poisons. It is a species of generation, two substances making a third; but the curious circumstance is its converting both vegetable and animal matter into the same kind of substance or compound, which no chymical process can effect."

A question may still be asked, whence is the acid principle derived which exists in the secretions, par-

ticularly in the urine, if a fermentative process is not admitted in the act of Digestion? To this I answer, that all the secretory organs of the living body appear to possess a power peculiar to themselves. And is it not an immutable law in animal bodies, that each gland can secrete no other, but its own proper fluid, which is wholly, or in part, fabricated in the very gland itself, by an animal process, which it there undergoes? But why one gland possesses the power of secreting a saltish, another a bitterish, or another an acid principle, is a difficulty with which the present state of Physiology has not made us acquainted. Nevertheless, we know that all the secretions of the body are produced from the peculiar matter of blood specifically acted upon by the various secretory organs. They cannot be produced by any other substances or powers, either within or without the body,

In endeavouring to explain the *modus operandi* in the process of Digestion, we should constantly remember this one principle, that nothing can be created or annihilated in all the processes and operations of nature or art. Nevertheless, we might almost be led to a contrary conclusion from observing the great difference in appearance between aliment before and after the process of Digestion.

Aliment contains all the elements of chyle as well before as after Digestion, but in a different state of combination. Mr. Boyle has proved that the properties of compound bodies depend upon their principles of combination, and not upon the properties of their elements. For instance, barley and sugar are compounds of different properties, but by distillation they yield similar elements. In like manner a piece of muscle passing through the organs of digestion produces chyle: now the muscle and chyle are found by chymical analysis to yield the same products. It appears then, that the muscle and chyle differ from each other only in their mode of combination. The conversion, therefore, of a muscle into chyle, is a separation of its elements, and a recombination of them in a different manner; so that the new compound shall have new properties.

If the changes above mentioned depend upon elective attraction; a question may arise, why cannot chyle be produced at any time out of the body, by giving an opportunity for this attraction to act, or why animal and vegetable substances do not always exist in the form of chyle?

I answer, that when two substances have a disposition or power to unite with or decompose one another, it is not sufficient that they should merely

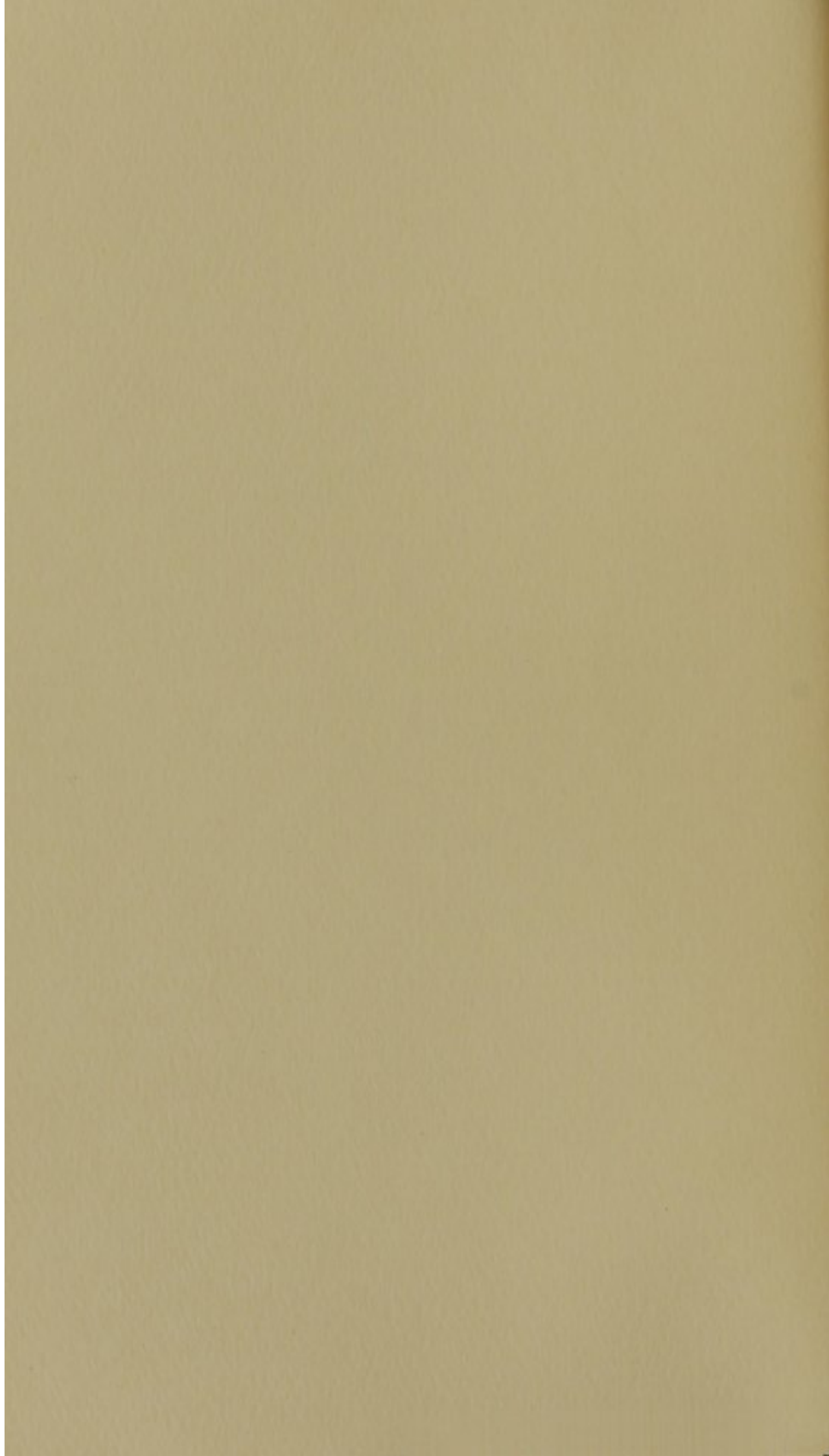
possess the propensity or even ability of producing the effect, but likewise be in suitable circumstances for that power to act. Thus, for example, a solution of sugar and water kept in a temperature not exceeding 40° of Fahrenheit's thermometer, will not have its elements to separate and recombine into wine, but if otherwise, this effect will be produced.

In this manner the action of the living stomach, and other organs of digestion, upon the food, are indispensably necessary, in order that those powers may act, by which a new state of aggregation may be effected.

Food placed in all the chymical circumstances similar to those in which it is placed in a living stomach, would never be converted into chyle, but would undergo other changes totally different. Animal food would putrefy, and vegetable food would become acid. If the living power of the stomach be strong and healthy, food will be perfectly digested; but if weak, it will go imperfectly through the organs of digestion, and some part of it will necessarily undergo those changes which it would have undergone in similar chymical circumstances; but will not be then acted on by the living organs of digestion.

For want of this circumstance, chyle never can be produced in any other place than in the organs of digestion of a living animal. The peculiar state of the food produced by the action of the living stomach, without which chyle cannot be formed, is illustrated by the following chymical fact, viz. Farinaceous matter, mucilage and native vegetable acid, may be combined into wine; but as an indispensably necessary pre-requisite, they must be formed into sugar. Whatever may be the reason or necessity for this, it is not easy to demonstrate; but most certainly, it is a *causa sine qua non*. Just so the food is brought into a state indispensably necessary in the living organs of digestion, previous to the formation of chyle.

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