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RECENT DISCOVERIES IN PHYSIOLOGY,

BY

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Art. III.—*Bernard's Recent Discoveries in Physiology*. Reported by F. Donaldson, M. D., Baltimore, Md.

Having a few months since witnessed some interesting experiments by M. Bernard, confirming the results of his late important researches in physiology, and not being aware of any notice of them having as yet appeared in our medical journals, we propose giving some account of them to the profession in this country. We are convinced alike of their accuracy and of the important light they are calculated to throw upon some heretofore obscure points in medical science. Indeed, M. B. has not as yet published some of the discoveries to which we shall call attention, and the only mention we have seen of them in print was some notes given in the "*Union Médicale*" of September last.

The first point of which we wish to speak is the discovery of the existence of a communication between the portal vein, the ascending cava and the kidneys, by means of which the urine is secreted from blood which has not as yet passed through the general circulation. It is called by its discoverer the *hepatico-renal circulation*.

Authors had frequently mentioned the rapidity with which substances taken into the stomach could be detected in the urinary secretion, and we have often read in the older writers of certain undefined secret passages. In fact, until cases of extrophy of the bladder proved that the urine came only from the kidneys, by means of the ureters, it was supposed by some that the bladder itself secreted it. Thus the effect of medicines on the economy in general and on the particular action of the kidneys was known to be different when given at different times, and physicians have acted upon the belief, but *why* no one knew. It had always remained undecided how it was that Tiedemann and Gmelin, and afterwards Magendie, in giving the nitrate of potash by the stomach should sometimes have been able to detect its presence in the blood and at others not, in the same animal. It was equally unexplained why poisons taken into the alimentary canal should sometimes have proved fatal, and at others not. Numerous theories had been thought of, but it was left to Bernard to find out the true explanation of these phenomena.

As not unfrequently happens with observers, it was in making some experiments with an entirely different object in view that M. B. was put upon the track of this discovery. He had previously proved that the mesenteric veins took up by far the greater portion of the digested alimentary substances, leaving to the lacteals, properly so called, the office principally of carrying off the oleaginous matters which had been digested by the action of the Pancreatic juice; and it was with the intention of testing what substances the kidneys could eliminate that he introduced into the stomach of a dog some prussiate of potash, and drawing off his urine in ten minutes he detected its presence by the addition of a few drops of a solution of a salt of iron, the beautiful blue of the prussiate of iron appearing immediately. He then killed the animal, and separately collected the blood of the renal veins and arteries, expecting to find the cyanide of potash in the artery and not in the vein, but to his astonishment the reverse was the case. The vein contained a large quantity and the artery scarcely a trace; the animal at the time, by accident, was digesting food. The experiment was repeated several times at different intervals, with invariably the same results, when the animals were in full digestion; but when they were not and the stomach and intestines were at rest, the contrary was the case—he could detect the potash in the blood of the artery, but not in that of the vein. Continuing his experiment, he traced the prussiate of potash from the stomach in its course to the kidneys, finding it in the vena porta, and in the vena cava below the point where the hepatic veins emptied, but nowhere else in the general venous or arterial system—thus showing that there was a different circulation during digestion from what existed at the time of abstinence. Having settled the physiological fact, it remained to solve the problem why it was that a substance taken into the stomach could be detected in the return circulation from an organ, when there were no traces of it in the artery which carried the blood to that viscus. If the fact above stated were not well ascertained and unquestionable, it would sound like an absurdity, it being so directly opposed to the universally received doctrine of the circulation of the blood—and such would it be, were there not a peculiar apparatus adapted by nature for the express purpose, as we shall proceed to show.

During the period of abstinence, the blood which has been collected by the mesenteric veins passes easily through the liver, but when the stomach and intestines are full and these veins absorb a large quantity of fluid and digested matter, the circulation becomes necessarily more active, and owing to the double circulation of the liver, the portal and the regular, the blood can pass but slowly, and consequently the organ becomes congested. To prevent this too great congestion, M. Bernard has discovered that there is provided a peculiar system of vessels conducting a portion of the blood directly from the vena porta to the vena cava ascendens, without its passing through the capillary hepatic circulation. These blood-vessels are found below the hepatic veins at the point where the substance of the liver adheres to the inferior vena cava. In the horse, this communication is very easily demonstrated, by blowing into the vena porta, when the air is heard escaping by the cava. M. B. exhibited a specimen of these vessels of a horse, injected with wax. They were as large as the veins of the stomach, and emptied themselves by open mouths. When the liver becomes engorged, and can receive no more blood, it passes off to the vena cava by these vessels, which serve as a kind of diverticulum, and in this way the free secretion of the liver is protected, as it would be interfered with by an excessive accumulation of the animal fluid. The right side of the heart in its turn is also secured from a too great rush of blood from the vena cava. It is known that the presence of the capsule of Glisson around the branches of the portal vein accelerates the ingress of blood into the liver, whereas the hepatic veins not being so protected, contract with the substance of the organ with which they are so intimately connected, and in the cut surface of the liver, the divisions of the portal vein are closed, but those of the hepatic remain open. The hepatic veins have, moreover, an evident muscular texture, which, when it contracts, draws the tissue of the liver with it. The object of this system is to render more active the circulation, in proportion as the liver becomes engorged. M. Bernard claims having discovered that there exists in the hepatic veins and in the vena cava, from the point where the hepatic empties to the origin of the renal veins, longitudinal contractile muscular fibres of organic life, while in other parts of the venous system circular fibres only are found.

This peculiar construction gives the vena cava the power of preventing too much blood from rushing to the right auricle by contracting and forcing the fluid downwards. This muscular development ceasing opposite the kidneys, the blood is thrown through the renal veins into that organ, and thus the stasis in the cava is relieved. In some animals, as in the rabbit, there are four valves in the cava immediately below the orifice of the renal veins, to prevent the reflux beyond that point. In this way it is proved that the blood coming from the portal vein is subjected to the action of the kidneys before passing through the heart into the general torrent of the circulation.

He has thus proved anatomically what he had previously established as a physiological fact.

The embarrassing question may be asked how the return circulation goes on from the inferior extremities during this stagnation and reflux in the cava. This our author thinks is the office of the collateral veins, the lumbar and vena azygos—they having a constant physiological part to perform, and not, as has been generally supposed, merely the duty of carrying on a supplementary circulation in certain pathological cases of obstruction of the principal venous trunks. The muscular structure in the cava is almost as large as that of the right auricle. In the horse and rabbit it is remarkable, and appears to constitute almost another heart, whereas in the stag, the dog and the cow it is but slight. It is owing to the great development of this singular provision to prevent the liver from becoming too much engorged that certain animals are enabled to run such great distances. In man, where this organization is but slightly marked, it has often been noticed that the liver becomes enlarged from excessive engorgement from much physical exertion. This mechanism of the circulation is prominently seen in animals who take large quantities of aliments but slightly nutritive, or who drink much of fluids.

In this manner we think it is satisfactorily demonstrated that the renal veins have a double duty to perform; during the time of abstinence they conduct the return circulation from the kidneys; during digestion they act as arteries just as the pulmonary veins do for the lungs, and the portal vein for the liver. In effect, during the reflux of blood mentioned above there is a distinct pulsation perceivable in the cava and in the renal veins. This, though difficult to show, yet may be seen by killing a rabbit during digestion and opening immediately the abdominal parietes.

As M. Bernard remarked, when one reflects upon the matter, it is not so much to be wondered at, if we recollect that in fish and in reptiles there exists a porto-renal vein by which a certain quantity of blood passes directly to the kidneys from the mesenteric veins, only a portion being sent to the lungs.

We give to the reader the three following experiments, of which we were witnesses; the results of them cannot be accounted for but by admitting the existence of this smaller circulation.

First Experiment.—By means of a syringe and a tube there was introduced into the stomach of a rabbit a large dose of the cyanide of potash mixed with some carbonate of soda; this last to correct the acidity of the gastric juice, some previous trials having failed from the formation of hydrocyanic acid and the consequent death of the animal.

Before giving the poison, some of the urine was drawn off and tested to prove that it contained no trace of it. Ten minutes after its administration, a few drops only of the solution of the acetate of iron gave the rich blue colour so characteristic of the Prussian blue. In half an hour the animal was killed, the blood of the jugulars and renal veins collected and their serum tested; in the latter, the presence of the medicine was very evident, whereas in the former there was scarcely a trace of it. Still more conclusive was the application of the test to the cut surfaces of the several viscera; that of the kidneys becoming of a deep blue colour, while the others revealed the least perceptible tinge. The inferior cava was much distended.

Second Experiment.—After cutting into the abdomen of a rabbit, and drawing out a small piece of intestine, M. Bernard introduced into one of the mesenteric veins a solution of the cyanide of potash of the strength of twenty parts to the hundred. On submitting the urine immediately afterwards to the test, it was found to contain a large quantity, and there were no unpleasant consequences resulting. Taking another rabbit, he injected into its jugular a solution of the same salt, of only two parts to a hundred, and in a few minutes, before the poison could be discovered in the urinary secretion, the animal was dead. The particular substance used is of no importance, the same result following the administration of any poisonous matter. It cannot be said that it was the liver which destroyed the effect of the medicine, for nearly the whole amount was passed off in the urine. The absence of any fatal consequences in the first rabbit can only be accounted for by the existence of the direct circulation, by means of which the potash was carried to the kidneys and there secreted; the small quantity carried to the heart, and thence through the arterial system, not being sufficient to act injuriously.

Third Experiment.—A solution of the lactate of iron was injected under the skin of the back of a rabbit, and a solution of the prussiate of potash under that of his thigh. The latter salt being very easily absorbed passed at once into the general circulation, and in a short time we were shown a blue coloration at the point where the salt of iron had been placed, a certain and unequivocal evidence of the presence of the prussiate of iron, which could only result from the contact of the two substances. In another animal the iron was inserted under the skin, but the potash given by the stomach, and after waiting much longer than in the first instance, there was no perceptible coloration, owing to the potash having passed off by the kidneys. On testing the blood from the jugular of the second rabbit, at first no coloration ensued; afterwards a slight bluish colour was discoverable, whereas in the urine a quantity of the salt was found, showing how small a proportion had entered the general circulation.

It may be well to remark, that owing to the slowness of the process of assimilation in the rabbit, it is almost always digesting.

Do not these experiments support and indeed prove the truth of M. Bernard's discovery? They explain facts which the imagination had vainly attempted formerly to solve, and show also the importance of choosing the proper mode of administering remedies; how, for example, one can sometimes produce much more energetic effects by the endermic method of introducing medicines. Their therapeutic importance in regard to the time to be selected for giving remedies to affect the general economy, or only the kidneys, will at once strike our readers. We can now account for the differences of the amount of nitrate of potash found in the blood of the animals experimented upon by others; in some cases it having been given during abstinence, and in others when the stomach was full.

We cannot overrate the important bearing these researches have upon the value of an analysis of the urine, the kind of alimentary substance consumed not only influencing, but changing completely, the composition of the renal secretion. The old distinction between herbivorous and carnivorous urine is caused entirely by the azotized or non-azotized food taken by the animals. The human urine in a healthy subject is always acid, but any one can in the course of thirty-six hours render it alkaline by confining himself exclusively to a full vegetable diet. The secretion of the rabbit, which is ordinarily so filled with carbonates, can be made acid by its being fed entirely on nitrogenized food. M. Bernard has proved that the urinary secretion of all animals, herbivorous or carnivorous, is acid when they are fasting, for the simple reason that in the absence of nutriment from without they consume their own tissues, a principal part of which are nitrogenized matters. How careful should we be to impute the presence of an excess of uric acid, or of any of its compounds, or of the carbonates, to any defect in assimilation, without inquiring into the nature of the diet, and of the time when the specimen was voided.

Secretion of Sugar by the Liver.—It was in the *Archives de Médecine*, of November, 1848, that M. Bernard astonished the scientific world by announcing his discovery that sugar, which was so generally found in the vegetable kingdom, also existed in the animal, not as derived from amylaceous and saccharine alimentary substances, but as a constant secretion. As vegetables do not find it already formed for them in the earth, but manufacture it themselves, so he has proved experimentally its production as one of the results of the habitual physiological action of the liver. Since that time he has instituted a number of experiments, confirming and completing his former researches. His doctrine as now taught, we proceed to give.

It is known that there are three kinds of sugar which are commonly taken as alimentary material. First, the cane sugar; that found in the sugar-cane, the beet, the carrot, &c. Secondly, the glucose, which exists in the grape and in different sweet fruits. Finally, milk sugar, a constituent of the mammary secretion of all animals. Of these the grape sugar is the only kind which is assimilable, the other being changed into it during the process of digestion. It had always been supposed that when sugar was found in the blood, or in any of the animal secretions, that it must have come from food which either contained it or from which it could be formed; and chemists had decided that such could not be the case from the protein or nitrogenized matter. Physiologists had inculcated the ingenious idea that animals could not in themselves create any principle found in their bodies, but only possessed the power of destroying what was furnished by the vegetable kingdom, and thus denied the possibility of sugar being manufactured by the animal organization, which only consumed, they said, what was obtained from without. Based upon this notion was the recognized treatment of diabetes,* that of withholding from the patient all amylaceous articles of diet. That this mode of treatment sometimes cured and always ameliorated the disease was unquestionable. M. Bernard's attention having been called to some obstinate cases of this affection, which had resisted all efforts to cure them, notwithstanding the exclusive use of azotized food, he determined to see if he could solve the question how sugar could continue to exist in such quantities in the urinary secretion when there was nothing digested which could furnish it to the system. He commenced his experiments by taking two dogs of the same size and age. One he put upon an amylaceous and saccharine diet, and the other upon meat exclusively. In a few days, by means of a syringe introduced into the jugular, he drew some blood from the right auricle of each of them, and after permitting the clot to form he tested the serum for sugar, and to his surprise, he found that in both was a large quantity. Astonished at this, he repeated a number of times the experiment, with always the same results, invariably finding sugar in the right auricle, whether the animals had been kept on nitrogenized or non-nitrogenized substances, and even when they had consumed no food for days. Pursuing his researches, he attempted to discover where the sugar came from, knowing that the right side of the heart could only be its receptacle. He accordingly examined the contents of all the principal venous trunks, the vena porta,† the inferior and superior cava, the jugular, &c., and, singular to say, he could nowhere detect its presence but in the hepatic veins and in the ascending cava, and thence to the right auricle. There being no trace of it in the blood flowing into the liver, nor yet in the pulmonary artery, was not our experimenter justified in coming to the conclusion that it was fabricated in the liver and destroyed in the lungs? That there were two sources from which the system obtained sugar; one from aliments, the other from the liver as one of its proper normal secretions?

Not content with this, he examined the parenchymatous tissue of the principal organs, and found a large quantity of sugar in the liver, some traces of it in the lungs, but he was unable to detect it in any other. Elated with what he now considered his brilliant discovery, he reported it to MM. Pelouze and Dumas, two of the most eminent chemists in Paris. They, naturally incredulous in regard to a point so calculated to upset the established doctrines as to the formation of sugar, insisted that there must be some mistake, and after

* We apply this term as confined to the disease in which there is sugar found in the urine.

† It may be well to add, for the benefit of any who may be disposed to repeat these experiments, that it is important to tie the vena porta near the liver immediately on opening the abdominal parietes, or otherwise there will be a reflux of the blood of that organ into it, from the removal of the pressure and the consequent detection of sugar in the vein.

witnessing the experiments, they resorted to the plausible theory that as the liver had the peculiar property of retaining and accumulating within its tissue certain metallic poisons, as arsenic, &c., it was probable that the animals which had been fed upon nitrogenized food or kept fasting had a few days previously eaten amylaceous substances, and thus the sugar formed from them had not all passed off from the liver. To show the correctness of his opinion, M. Bernard kept dogs for six weeks from all species of nutriment from which it was possible that sugar could be formed, and still as before he found it both in the blood coming from the liver and in the organ itself. With the energy for which he is so justly distinguished, he continued his investigations on different animals, and satisfied himself that sugar was to be found as the secretion of the liver of the horse, the ox, the dog, the cat and the rabbit; moreover, that it exists in birds, fish, reptiles, even in oysters and snails. And what is still more conclusive as to its being produced in the tissue of the liver, is that it can always be detected in that organ of a fœtus after the fifth month. Further still, the fœtus of oviparous animals which are separated from the mother have exactly the same kind of sugar in their liver and in no other organ!

Following up his experiments, M. B. has proved conclusively that the sugar he had found was a secretion by showing the influence of the nervous system over its production. As an irritation of the ophthalmic branch of the fifth pair leading to the lachrymal gland produced a free flow of tears, so a slight galvanic shock, or irritation with a knife applied to the medulla oblongata at the point of origin of the pneumogastric nerve, caused an increase in the secretion of sugar; so much so that a large quantity was carried off in the urine a few minutes after the operation. A violent shock to the corpora olivaria, or the cutting through of the nerve, would arrest the secretion, as was shown by autopsies made some hours afterwards. We give these not merely as statements made by the professor, but as confirmed by experiments repeated in our presence. We have seen several instances where cases of diabetes were produced in dogs and rabbits at pleasure—the urine drawn off previous to the operation giving us no evidence of the presence of sugar, whereas that voided twenty minutes after contained a large quantity, as did the blood and every secretion except the saliva, into which it never enters. So true is this that, in one instance, M. B. detected it in the urine of some kittens who had been fed by a cat, on whom he had a few minutes previous operated, showing that it had been transmitted through the milk. We have witnessed the arrest of the secretion by a violent shock to the nervous system, and by the communication of the nerve being destroyed, so that the urine, which a few minutes previous had contained a large quantity, was rendered perfectly free from it. Subsequent experiments have somewhat modified this last fact, M. B. having in some cases produced the secretion by irritating the olivary eminences notwithstanding the previous severance of the nerve. The probable explanation of this is that the grand sympathetic also serves as a conductor, as in a case of diabetes observed by Duncan; its volume below the diaphragm was found to be three or four times greater than what it normally should be. The portion of the medulla oblongata which appears to be most intimately connected with the production of sugar in the liver is not more than three lines in diameter, lying in the groove between the corpora restiformia, and the corpora olivaria, and over the adjoining part of the latter. M. B. can predict the amount which will be secreted from the depth of his incision; if the instrument employed is not thicker than a millimetre, or the twenty-fifth part of an inch, the proportion in the urine will be four parts in a hundred. Beyond a certain point, of course, there is great danger of killing the animal or of arresting the secretion.

The continuance of the presence of sugar in the urine after the operation is variable, according to the animal experimented upon, and also the manner employed. In general in the rabbit it lasts forty-eight hours, and in the dog four days, and in one instance as long as seven days.

There were several phenomena which presented themselves as accompanying these experiments which are well worth noticing. The animals were continually in motion; their excitability was such that one might have supposed that some preparation of strychnine had been used. This continued until the sugar could no longer be discovered in the urine, as did also the acceleration of the respiration; which can be explained by the extra duty the lungs had to perform in destroying so large a quantity of sugar. May not this excessive fatigue of the respiratory organs account for the liability of diabetic patients to pneumonia and phthisis, which so often are the cause of their death. A curious fact elicited was that the temperature of the body was diminished several degrees. This is singular, as M. Magendie, judging from the fact that the amount of sugar secreted was greater in birds and other animals where the temperature was higher, and indeed in proportion to the elevation of the temperature, had supposed that its destruction in the lungs was one of the causes of animal heat. In the rabbits rendered diabetic there was an increase of the urinary secretion, as there generally is in the human subject. The amount of salts appeared to be diminished, but this was owing to the quantity of liquid. As to the perspiration, which in man is to a great extent suppressed, it was difficult to decide on the animal.

The secretion of sugar may be arrested by different causes, as an acute pain caused by any operation on the nervous system, such as exposing the medulla oblongata, or pricking the sciatic nerve. Indeed, in renewing his experiments on the excitability of the eighth pair, M. B. has been surprised to find that often instead of augmenting the secretion, he has caused it to disappear, though the irritation appeared but slight; and now he acknowledges that the suppression takes place as a result of almost any lesion of the nervous system, except that of the olivary bodies and of the space about them, before mentioned. Diseases, such as intermittent fever, pneumonia, &c., or indeed anything which affects sensibly the nervous system, interrupt this secretion. There was a case in the service of M. Andral of a diabetic woman who ceased to discharge sugar in her urine at each attack of diarrhœa, to which she was subject. A slow lingering death from any cause has this effect. It is known that frequently in diabetic patients there is an absence during the last stage of the disease of the presence of the characteristic symptom in the urine caused by the complete exhaustion of the nervous energy. So it is not surprising if sometimes no sugar is found in the liver of patients who have died of diabetes. M. B. has invariably detected the presence of sugar in the livers of different animals, as he procured them from the butchers. Anxious to get a human liver of a subject that had not died of a disease which, by its long continuance, might have affected the saccharine secretion, he obtained that of a man who had been guillotined the day previous, and experimenting upon it before the class, he found it contained somewhat more than an ounce of diabetic sugar. A short time previous, he had had an opportunity of examining a patient of M. Rayer, who had for a long time been suffering with glucosuria, of which he had died suddenly; the amount of sugar in his liver was two ounces and a half, more than double that of the healthy one. In general, animals who eat amylaceous substances, secrete sugar in greater quantities than others, and the longer abstinence is prolonged the less the liver contains. Adults require and secrete more than the young.

As we mentioned before, the only kind of sugar which is assimilable is the grape, and all other varieties are converted into it by the combined action of the bile and the pancreatic juice before being taken into the general circulation. The sugar found in and secreted by the liver differs from the ordinary glucose only in certain physiological properties, in being more readily absorbed into the circulation, while it is more easily and in greater quantities destroyed in the lungs. It is proved to be of the second variety, moreover, by its difficult crystallization, by its reducing the salts of copper, and from the fact of its refracting rays of light to the left. It is distinguishable from the sugar of milk because the latter is indestructible in the blood, and never ferments, and it also corresponds in every particular to the saccharine matter of diabetic urine.

M. Bernard, in testing for the presence of sugar, used all the different modes heretofore proposed, the taste, evaporation of the liquid, fermentation on the addition of yeast, and the consequent production of alcohol and carbonic acid; the process of M. Péligot, which consists in making a saccharate of lime, and that of MM. Biot and Clerget, by means of polarized light. The most delicate and surest one employed was the double tartrate of copper and potash, which is known as "la liqueur de Bareswil."* It is Trommer's test

* We give the proportions of this reactive as one which is readily and easily applied.

℞.—Crystallized carbonate of soda,
Caustic potash, āā ℥i. ℈i.
Bi tartrate of potash ℥i. ℈ii.
Sulphate of copper ℥i.
Distilled water Oj.—M.
To be boiled and then filtered.

somewhat modified, the copper and potash being used together instead of separately. The addition of a few drops of this solution to the suspected urine or to the decoction of a piece of the liver, or to the serum of the blood, will, on the application of the heat of a spirit-lamp, in a few minutes reveal the presence even of a minute proportion of sugar by the liquid becoming at first of yellowish green, and by degrees more and more of a reddish-yellow colour in proportion to the amount contained. This coloration does not take place with any other variety of sugar but the glucose. Sometimes, however, the presence of organic matter may produce deception, and to get rid of it, it is recommended to precipitate it by the acetate of lead, and filtering it, to add some hydro-sulphuric acid, the liquid to be again filtered before using the test. In herbiferous urine it is well to add a few drops of sulphuric acid to free it of the amount of carbonates which may render obscure the coloration. The potash destroys the sugar, transforming it into glucic and paraglucic acid. The salt of copper yields part of its oxygen to substances which have a great affinity for it, and passes to the state of a hydrate and afterwards an anhydrous protoxide, and thus is caused the change of colour from blue to yellow and then to red. The amount of copper reduced being in proportion to the quantity of sugar, it is easy to calculate how much of the latter is present.

The interesting question arises as to what becomes of the sugar, whether secreted by the liver or formed from alimentary substances. We have seen that it is destroyed in the lungs, where with the blood it is exposed over a large surface to the contact of the air. M. Bernard proved by a simple experiment that the destruction of sugar was not, as was its production, under the influence of the nervous system; but altogether a chemical phenomenon. He cut both the pneumogastrics of an animal, and injecting some grape sugar into the blood, found that it was consumed, as in the case of the integrity of the nerves. Moreover, sugar in blood disappears when in contact with air out of the body, as well as in the lungs. It is necessary that the blood should be alkaline for this to take place, for if an acid be added, the destruction is prevented. The contrary is the case in regard to cane sugar, the presence of an alkali interfering with its being destroyed. Attempts have been made to render the blood of animals acid by the injection of vegetable acids, but death has always ensued too soon. M. Bernard found on adding an alkali to blood coming from the liver, that the destruction of the saccharine principle took place very gradually, and he is disposed to believe that the usual alkalinity of the blood favours, but is not the cause of, the consumption of the sugar, which is owing to a peculiar organic matter, some ferment which he has not yet been able to seize. He thinks this supposition probable from knowing the effect in other parts of the economy of animal substances which exist in very small proportions, and which apparently have but little power; the diastase, like the strong acids, converting amylaceous matter into sugar. This ferment acts not like yeast, by producing alcohol and carbonic acid, but by converting the sugar into lactic acid and carbonic acid; the latter of which is exhaled from the lungs. In the artificial diabetes produced on animals, the amount of carbonic acid given off by the lungs was much greater than it was before the experiments; and furthermore, their arterial blood became much darker, and gradually resumed its normal tint as the excess of sugar diminished. Thus it was proved that the amount of carbonic acid was in proportion to that of the sugar. It is calculated from experiments by injecting this diabetic sugar into the veins, that the lungs can destroy over and above what they ordinarily do, as much as five drachms (ʒv), whereas of the common grape sugar only one drachm (ʒi); all above this passing off in the secretions. Cane sugar thus introduced is found untouched in the urine.

If the secretion of sugar by a lesion of the olivary bodies surprises physiologists, its arrest by any trouble of the nervous system should not, for it has often been observed that the secretion of the mammary gland can be altered in quality or even entirely suspended by a strong moral impression, and still more by an acute physical pain. In the same way, a violent passion or fright has affected the formation of the bile, the elements of which remaining in the blood, cause a jaundiced appearance of the eyes, the skin, &c.

This discovery of this hepatic secretion shows us the nature of diabetes mellitus, for that disease has as its principal symptom an excess of this identical sugar.

At first Bernard was inclined to believe that the cause of the production of an abnormal quantity was some affection of the eighth pair of nerves, but his more recent researches have somewhat modified this opinion. Whether the primary lesion exists at their point of origin or in the liver itself it is difficult to decide. This organ is generally hypertrophied, but its anatomical examination has as yet shown nothing. The ancient theories, explanatory of this singular disease, are proved in a great measure to be groundless. The hypertrophy of the kidneys and the lungs as described by M. Rayer is accounted for by the extra duty they have to do; the one in eliminating the sugar from the blood and the other in its consumption.

Rollo regarded it as the effect of a disease of the stomach in which the gastric juice contains a principle not found in the healthy state, which acts upon starch, converting it into sugar. M. Bouchardat in urging this view, states the fact that large quantities of sugar have been found in matters vomited by diabetic patients, but this is no proof, for, as we before mentioned, the gastric juice itself, like the other secretions, contains more or less where from any cause there is an excess in the system. The ingenious reasoning of M. Bouchardat is rendered unnecessary, it being now established that it is not only from feculant substances taken into the stomach that sugar is formed. Moreover, it is not necessary, as he states, that sugar should first be transformed into lactic acid before being absorbed, it having been proved that grape sugar as such enters into the circulation and passes off by the lungs. M. Mialhe's idea of diabetes is founded upon two suppositions, both of which are gratuitous. In the first place, it is not the alkalinity of the blood which destroys the sugar, it being merely an accessory; then the suppressed cutaneous transpiration does not render the blood either acid or neutral, for it remains invariably, so Mr. Bernard stated, alkaline.

Experiments of suppressing the cutaneous exhalation of animals by varnishing them all over have neither rendered them diabetic nor yet altered the alkaline character of their blood. In admitting that the diabetic state is an exaggeration of a natural function of the liver and consequently a disease of that organ, it still remains to be determined what are the causes and how they act to produce it. It is not only the thirst which is increased in persons suffering with this disease, but the activity of all the nutritive functions is greater; the appetite is more craving; the respiration accelerated, &c.

The great frequency of this disease in England and Germany, where it is common among children and very old persons, though it more generally attacks the middle-aged, is accounted for by the habitual use of fermented liquors, which, it is said, favour its development. It is believed that debilitating causes, bad food, excesses, the passions, low and unhealthy habitations, render its production more easy.

The last point we propose noticing is the therapeutical application which, after all, ought to be the end and aim of all scientific medical researches. As it is certain that aliments which contain sugar or starch increase the amount of saccharine matter in the urine of diabetic patients, and thus aggravate the disease, they should, as far as possible, be avoided. In this way, we are able to destroy a part of the morbid element; but it is positive that, notwithstanding their suppression, the sugar continues to show itself in the secretions.

M. Bernard has found that the acids and the ammoniacal preparations recommended by some fatigue the stomach and effect but little; the efficacy of astringents has been much overstated. Quinine, in combination with iron, has produced good results. Any remedies which act decidedly upon the nervous system, such as alkalis (urged by Mialhe), iodine (tried by Lugol), opium, creosote, mercury, &c., are beneficial, but unfortunately their effect is but temporary, the constitution soon becoming accustomed to them. M. B. hopes that observers will in future direct their attention in their treatment to the liver and to the nervous system, and these experiments may yet give us some valuable results, now that the nature of the disease is settled.

We have already occupied so much space that we must postpone, for a future communication, giving the results of some investigations in regard to the *formation of fat*, the *action of the pancreatic juice upon oleaginous substances*, &c., which we hope will not prove uninteresting to our readers.

not appeared to me appreciably less, over the febrile excitement, and local morbid action, than when I have administered it by the mouth.

Although the results of my practice from this method of treatment have been highly favourable, one case stands on record in my case book, in which it was principally adopted, calculated to cast a doubt upon its invariable innocuousness. Determining in the commencement of the attack to treat the case principally with the antimonial enemata, I ventured—as I thought I might do with safety, having this intention in view, and as the attack was a very violent one—upon a more free administration of the remedy by the mouth, during the first eighteen hours, than is my common practice; and it is not improbable that it was to this, in conjunction with the action of mercurials administered occasionally by the mouth during the progress of the case, that the metastasis was in reality attributable. Still I could not entirely divest myself of doubt on the subject; but even admitting in this instance an unfavourable operation from the enema treatment, I still rest satisfied of its comparative safety, in contrast with the usual method of administering the remedy by the mouth.

In the case in question, up to the evening of the fifth day, the patient was apparently doing well; the febrile excitement regularly moderating, and the organic disease gradually subsiding. At this time, a *very slight* tendency to tympanitis was discoverable, and a more rapid removal of the thoracic dullness on percussion had taken place, than between any two of my previous visits. The patient expressed himself better, however, and the general symptoms were apparently favourable. It was consequently with no slight surprise on the part of the patient's friends, as well as of a professional friend who, on this occasion, visited the patient with me, that my intimations were received of a probable speedy fatal termination. The signs and symptoms of pneumonia rapidly disappeared; the tympanitis increased; diarrhœa, thirst, restlessness, jactitation, delirium followed by incomplete stupor of short duration, came on, and in ten hours from the time of the first symptom of the approaching change, he was dead.

Art. IV.—*Bernard's Recent Discoveries in Physiology.* Reported by F. Donaldson, M. D., Baltimore, Maryland.

In the previous number of this Journal, we called attention to, and we hope, by giving in detail his experiments, established, two of M. Claude Bernard's discoveries; the first, showing the existence of a communication between the portal vein and the vena cava ascendens, by means of which the kidneys secrete from blood which has not passed through the general circulation; the second, demonstrating that the liver has the prerogative, heretofore unsuspected, of fabricating, within its own proper tissue, sugar resembling very much the ordinary glucose, and in every particular the saccharine matter found in the urine and in other secretions of patients suffering with the disease known as diabetes mellitus. We now propose giving a brief account of the points in the process of *digestion*, which have been elucidated by the same physiologist; and then we will notice his investigations in regard to the *formation of fat* within the body, and independent of the alimentary materials.

There is, indeed, scarcely a department of physiology to which M. Bernard has not, by his experimental researches, made valuable additions; but that of digestion, from its great importance, has been his particular study, and to him, as we shall presently show, more than to any other living physiologist, do we owe the present advanced state of the science in regard to it; not even excepting Dr. Beaumont, the result of whose interesting experiments on the Canadian, Alexis St. Martin, have been so widely diffused.*

Bernard has had this advantage over Dr. Beaumont, that, whereas the latter could only experiment upon the action of the stomach and its secretions, he, by establishing fistulous openings into the stomach and into the ducts of all the secreting or excreting organs, has been able to carry his researches much farther. It was curious enough, to see walking about in the amphitheatre of the College of France, dogs and rabbits, unconscious contributors to science, with five or six orifices in their bodies, from which, at a moment's warning, there could be procured any secretion of the body, including that of the several salivary glands, the stomach, the liver, and the pancreas. The great value of M. Bernard's doctrines is, that they have been arrived at, not like those of many of his predecessors, from ingenious theories, but from actual experiments on the living subject, repeated over and over again, so that on many points they may be said to be demonstrated. Such, certainly, is the view entertained by all who have had the pleasure of witnessing them.

We cannot, of course, in the brief space of this article, enter as fully as we should wish into the phenomena of digestion, much less can we notice the various theories invented to explain them by the older physiologists, beyond what is necessary to render clear the points dwelt upon by our author. One cannot forbear remarking how much time, as well as genius, has been wasted in framing theories in explanation of conclusions drawn from imperfectly observed phenomena, and, indeed, in many cases perfectly independent of all ascertained facts. It has been too much the habit to form conjectures of what might take place in the economy, instead of endeavouring to find out from observation what actually does occur.—Nor was this error confined to men of the age of Hippocrates, who regarded digestion as the result of a *decoction*, nor to those who considered it severally as a *fermentation*, a *putrefaction*, or

* See Experiments and Observations on the Gastric Juice and Physiology of Digestion, by Wm. Beaumont, M. D., U. S. A.

a *trituration.* Even many of the conclusions in physiology of Liebig, profound and ingenious as they certainly are, are worthless to science, for the simple reason that they are so purely theoretical, supported by little or no observation of the living functions themselves.

Before coming to the more prominent points, we propose commencing, as did M. Bernard, with the *saliva* and its agency in digestion, and then proceeding in order with the other fluids connected with the preparing alimentary substances for assimilation.

We would naturally suppose that nothing could be easier than to ascertain exactly the office of a secretion which can be so readily procured from the mouth; and former observers had confined their investigations almost exclusively to the action of the mixed fluid there found. M. Bernard began his by inserting into Stenon's, Wharton's, and into one of the small ducts of the sublingual gland, small tubes, by means of which the several fluids making up the mixed buccal saliva could be examined separately. After which he produced a free flow from these ducts, by giving to the dog a piece of meat, the presence of which caused at once a free salivary secretion. What was most curious to observe was that, during mastication, the flow was almost entirely confined to the duct of the parotid and to the sublingual; whereas, during the motion of deglutition, when the tongue threw the bolus back into the pharynx, the secretion of the submaxillary was the greatest. Each separate secretion, as well as the saliva, as swallowed, was alkaline in its reaction, the acidity of the mouth during abstinence, which is owing to the secretion of the small buccal glands, being neutralized. M. Bernard remarked, he had always found that this, as other alkaline secretions, is best excited by the presence of acids, and the reverse is the case in regard to the acid fluids which flow most freely when alkalies are administered.

The physical characters of the several salivary secretions are very different; that of the parotid and sublingual glands is clear, and as limpid and thin as water, while that which comes from the submaxillary is thick and viscid, resembling in colour and in consistence ordinary simple syrup. From these facts M. Bernard has concluded that the mechanical use of these secretions is not the same—that of the parotid and the sublingual being principally to saturate the food, and thus facilitate mastication; the submaxillary, on the contrary, rendering easy the process of swallowing by its glutinous properties. In these, as in all his other investigations, he was not satisfied with the result of his first essay, but followed it up by others which confirmed it. He made an opening into the œsophagus of a horse, from which he drew the alimentary bolus as it descended, and on weighing it he found that by the imbibition of saliva it had increased elevenfold—showing what a large quantity of saliva was necessary. He next tied Wharton's duct, and found that it required forty-one minutes to masticate, so difficult was it, what previously had demanded only nine minutes; and the mass, when withdrawn from the œsophagus, was covered with mucus and a glutinous fluid, the interior was dry and friable, and the whole only increased in weight three and a half times. By giving freely of water to the animal to drink, it appeared to promote mastication almost as effectively as did the secretion of the parotid; the quantity of parotidian saliva being in proportion to the dryness and toughness of the ingested substance. If the ducts of the other two glands be tied, there is not the same difficulty in mastication. The water of the buccal mixed saliva is in the proportion of ninety-eight parts in a hundred, the other two being composed of the salts of potash and soda, and of the animal substance *ptyaline.* Thus it is shown that the mechanical use of the secretion of the salivary glands is very important.

In regard to its chemical action, it will be remembered, that Beaumont found that a piece of meat put directly into the stomach through the fistula, was digested fully as well as though it had been first subjected to the action of the saliva. From this fact, which has been repeatedly verified by others, it was concluded that this fluid had none other than its physical action on food. That this is a mistake, can easily be shown by placing cane sugar or any amylaceous substance in a test-tube filled with saliva, and applying heat of 98° Fahr.—when in a few minutes, and, according to Mialhe,* if it be powdered starch in solution, in less than one minute, there will be a transformation into dextrine, and then into grape sugar, and, if permitted to remain longer, into lactic and bituric acids. This may be easily tested by the addition, at the different stages, of the tincture of iodine to the solution—whereupon may be noticed the gradual disappearance of the blue of the iodide of starch, which changes at first to a rose, and then, owing to the complete alteration of the starch as such, the iodine is perceived to have no action at all. From these premises M. Mialhe was led to believe that, as the saliva, owing to a peculiar ferment, had that effect out of the body, therefore, such must be the case in the ordinary process of digestion, and concluded that this fluid completed the transformation of all that class of aliments; but Bernard has since proved the action of the saliva upon amylaceous matter to be very gradual, unless it is reduced to a powder, and is in solution; moreover, any acid placed in contact with it at once destroys its power, so that, as it cannot effect any change when it is mixed with the gastric juice, which is acid, the time is too short for it to have more than a very partial transforming power in the mouth and in the œsophagus. To establish this, M. Bernard killed a dog who had been fed upon potatoes, and, on opening the stomach, he found there the merest trace of sugar, but much unaltered starch, even in the mass passing through the pyloric orifice out of the stomach. This he thought conclusive of the point that, ordinarily, the saliva only acted mechanically in digestion, except in a slight degree upon amylaceous substances—nature having provided another fluid for the purpose of fitting this class of aliment, as well as fat and nitro-

* See "Mémoire sur la Digestion, et l'Assimilation des Matières Amylöides et Sucrées. Paris, 1846."

genized matter for absorption and nutrition. A singular fact, first noticed by M. Magendie,* and confirmed by Bernard, is, that neither the secretion of the parotid, nor that of either of the other glands separately, nor when mixed with each other, have any effect even upon amylaceous matter, but yet, that the mixed saliva of the mouth unquestionably has; from which it would appear that the active principle, whatever it is, that effects the transformation, must come from the small buccal glands. In diabetes, neither in the human subject nor as produced artificially in animals, can any trace of sugar be detected in the salivary secretion. We before noticed, that though the cyanide of potash administered by the mouth could be detected in the other secretions, yet it did not enter into the saliva.

The interesting question arises, as to what is the principle of the saliva, which, when permitted to remain sufficiently long in contact with starch, converts it into dextrine, sugar and lactic acid. Berzelius was the first to isolate, by the addition to filtered buccal saliva of six times its volume of absolute alcohol, an organic substance which he called *ptyaline.* Mialhe, in his memoir before the Academy of Sciences, maintained that the organic precipitate obtained by Berzelius and by himself, had properties analogous to the *vegetable diastase,* which in malt converts the starch into sugar, and that it should be named the *animal diastase.* There was no doubt that such was its action; but was it a substance of a peculiar nature existing only in the saliva? To test this, M. Bernard put other fluids of the body with starch, and, on raising them to the temperature of the body, found that many of them had exactly the same transforming power as the saliva. Such was the case with the water in which dried buccal mucous membrane had been soaked, showing, as he had supposed, that the ptyaline was derived from it. The serum of the blood, a liquid obtained from a cyst of the liver, the mucus from the nose in coryza, or indeed, from any irritated mucous membrane, had precisely the same effect. On giving a starch injection to a patient suffering with a diarrhœa, he found sugar in the stools, &c. From these results, M. Bernard concluded that the fermenting principle of the saliva was not different from some other nitrogenized matters in its action upon amylaceous substances.

The Gastric Juice.—At the commencement of his experiments upon this fluid, M. Bernard made before his class, an artifical opening into the stomach of a dog. He cut through the abdominal parietes in the right hypochondriac region, and, drawing out the stomach with a pair of forceps, he made an incision about an inch and a half in length, into which he introduced a short, wide canula, either end being terminated by a rim resembling very much a button. One of these ends being firmly clasped by the lips of the wound of the stomach, he was enabled to draw the organ near the surface, and fasten the other extremity of the tube to the integuments. A cork put into the external orifice, prevented any liquid from flowing out. In a few days, owing

* See Précis Elément. de Physiol., Paris, 1846.

to the slight sensibility of the peritoneum of the canine race, the animal was well, and the wound around the canula healed, so that the gastric juice could be extracted with perfect ease, when its secretion had been excited by the presence of some alimentary substance; the stomach, as shown by Beaumont, not producing any during the time of abstinence. The operation required but a few minutes, and to show how little uneasiness the presence of such an opening caused the poor animals, a dog was shown on whom a similar operation had been performed two years previous, and who had from that time furnished gastric juice whenever wanted. He appeared to be in perfect health.

Bernard has verified, as had previously M. Blondlot,* many of Beaumont's conclusions in regard to the action of the stomach and of its secretions. Their observations are important, as showing the identity of the digestive juices of the dog with that of the human subject. We forbear mentioning many of these results, which are doubtless familiar to our readers.

The gastric juice can be seen exuding from the mucous surface under the coat of mucus always found there, like the perspiration from the skin; but as to what it is exactly that secretes it, there appears to be considerable question. Bernard is inclined to believe that it is furnished by the corpuscles discovered by Gruby, which are very numerous, and exist only in the stomach; they are surrounded by a vascular network, and are found between the villosities. There are also two kinds of small glands perceptible in this mucous membrane—one a slight tube resembling the finger of a glove; the other with a bulb, which terminates in a narrow orifice at the gastric surface. Some have thought that the former of these furnish the mucus, and the latter the gastric juice. That the gastric juice comes more particularly, if not exclusively, from the portion of the mucous membrane around the pyloric orifice and from that near the liver, is proved by Bernard in a very pretty and simple experiment. On introducing into the jugular vein of one side some cyanide of potash, and into the other the proto-sulphate of iron, both in solution, they pass through the circulation side by side without combining, thus revealing the valuable fact that, though substances have a great affinity for each other, yet that no union of them can take place while they remain in the blood-vessels, probably because the heat evolved from a single or double chemical decomposition taking place in the circulation would be too great for the economy, as we see nature making use everywhere in the alterations and destructions in the animal system of mild ferments, and not of strong chemical agents. This fact of itself is one of great importance in demonstrating how unfounded are many of the ingenious theories of the chemical action of remedies after they have been absorbed. These substances, however, when brought into contact in the free secretions, unite at once where there is air. M. Bernard noticed, on examining the stomach of a dog whose jugulars he

* Traité Analytique de la Digestion.

had injected as above, that the mucous membrane of the stomach was of the normal colour, except near the pylorus and over the hepatic portion, where it was of the deep blue of the prussiate of iron, the result of the union of the two substances in the gastric juice, as it was poured out. The cyanide of potash can be detected in the secretion of the stomach in twenty-five or thirty minutes after its ingestion, earlier than in any of the other secretions. During the period of abstinence the constant presence of mucus renders the reaction of the stomach alkaline, and it is only when the presence of some substance in the cavity excites the flow of the gastric juice itself, that it becomes acid. M. Bernard finds, as did Beaumont, that any febrile movement has the effect of arresting this secretion, and thus interfering very materially with digestion—a practical fact which ought not to be overlooked.

The gastric juice as taken from the fistulous opening was invariably acid in its reaction, was clear as water, without taste, as without any odour except that of the particular animal. MM. Leuret and Lassaigne* have confirmed M. Bernard's observation, that the internal membrane of the stomach, when free from mucus, presents always an acid reaction.

In regard to the composition of this gastric fluid, it seems agreed, that at least ninety-eight parts in a hundred are pure water, the remaining two being made up of a free acid, the chlorides of lime and ammonia, phosphate of lime, an aromatic principle, and a particular animal matter, generally known as *pepsin*, and called by some *chymosine*, and by others again *gasterase*. M. Blondlot in his book already quoted, denies that there is a free acid in the gastric juice, the acidity of which he believes to be owing to the presence of the acid salt, the bi-phosphate of lime. His principal reason for this opinion was that he found, on the addition of carbonate of lime, that there was no effervescence produced. This, however, MM. Bernard and Barreswil† have proved to be caused by the extreme dilution of the acid, for, on concentrating the juice by evaporation, and then adding the lime, the carbonic acid was evolved. We have, moreover, the authority of M. Dumas in his Chimie Physiologique, that there is no biphosphate of lime in the stomach. The question has been much discussed as to what this free acid is, and what is its action in the process of digestion. Dr. Prout tells us that, on testing some gastric juice which he had distilled, with nitrate of silver, he found hydrochloric acid, and MM. Tiedeman and Gmelin say the same. Dr. Dunglison found this acid in the gastric juice of Dr. Beaumont's subject. Dr. Prout thinks that it is formed from the chloride of sodium of the blood being decomposed by a galvanic action, the liver and the gastric mucous membrane representing the two poles—the acid remaining in the secretion, the soda passes off by the liver. Berzelius said he could conceive of no other way than this by which it could be found in the stomach. However formed, there can be no doubting the

* Recherches Physiol. et Chimique.

† Analyze du Sucre Gastrique, 1844.

opinion, now the generally received one, of the distinguished persons named as having detected this acid in the juice when distilled—but this does not establish its presence as a free acid in the stomach itself, for it may be the result of a decomposition of some salt. MM. Bernard and Barreswil state that, on adding a small proportion of hydrochloric acid to the gastric juice, it does not pass in distillation until near the end of the process. They think that, in the rare cases where this acid has been found in the distilled secretion, it was owing, during the last moments of the operation, to a decomposition of the chlorides from the altered chemical affinity caused by the concentration, or from the action of some other principle there found, probably the lactic acid. Their ground for so thinking is that, on distilling water containing table salt, after having rendered the solution acid by the addition of lactic acid, the last drops show on using the nitrate of silver the precipitate of the chloride of silver, owing to the formation of muriatic acid. Another proof they urge that this acid cannot be present is that if a small proportion of oxalic acid be added, there is produced at once a white precipitate of the oxalate of lime, which formation the presence of a very minute quantity of hydrochloric acid would prevent. Some have thought that the acid principle might be *acetic acid*, but neither Blondlot nor MM. Bernard and Barreswil have been able to obtain it by distillation; which, owing to its being very volatile, is sufficient proof that it is not present. M. Chevreul, some years since, in analyzing some gastric juice for Magendie, concluded that the free acid was the *lactic acid;* this opinion is sustained by Bernard and Barreswil, in their recent memoir presented to the Academy of Sciences. They recognize it by all the characters insisted upon by distinguished chemists. It forms salts of lime, barium, copper, and zinc, soluble in water; it gives a salt of lime soluble in alcohol, and precipitable by ether from the alcoholic solution; and can produce a double salt of copper and lime, the colour of which is more intense than that of a simple salt.* It is, moreover, reasonable to suppose that nature would employ in the stomach, as she does elsewhere, mild agents, particularly as M. Bernard has proved that any acid principle would act just as efficiently as that found in the gastric juice; this he showed in artificial digestion with this secretion. On rendering it alkaline the process was completely arrested, but it commenced again on adding any acid, no matter what, provided the quantity was sufficient to change the reaction. Moreover, in injecting into the circulation food digested artificially with gastric juice, thus rendered acid again, if the acid employed was a strong mineral one, such as the hydrochloric, it was difficult to add so small a proportion that the injection would not prove fatal. Bernard found, in trying his experiments in injecting different substances into the veins, that the lactate of iron, which is the most absorbable of all ferruginous preparations, was the only one which

* See "Analyse du Sucre Gastrique." (Comptes Rendus de l'Acad. des Sciences, 1844.)

was not poisonous. From these facts M. Bernard concludes that *lactic acid* is the free acid found in the gastric juice, and that there is neither acetic nor hydrochloric present.

In regard to the action of the gastric juice, and to the whole process of digestion, we cannot but be struck with how valueless all the theories ever invented are in comparison with such facts as were afforded by the investigations of so accurate an observer as Dr. Beaumont, upon the opening into the stomach of St. Martin. The science now possesses two other cases of the kind in human subjects, a woman noticed by Circaud, and another spoken of by Helm,* and now of numerous ones produced artificially in animals. From these many points in digestion, before obscure, have been determined.

Dr. Beaumont was inclined to believe that the gastric juice was the only solvent, and that it acted upon and digested all kinds of food. This was a natural conclusion from seeing them all in solution, and as ready to pass out of the stomach, in, as he supposed, one homogeneous mass, called *chyme*. His observations unfortunately were confined to the operations in the stomach alone, and he mistook the dissolution of all alimentary substances, from the imbibition of the acidulated water, to be their digestion. M. Blondlot states, as the result of his experiments, that the action of the fluids of the stomach was limited to that class so rich in nutriment, the prominent constituent of which is nitrogen. He found that in artificial digestion such was certainly the case, and he concluded that the process was the same within the body. M. Bernard found, as we before mentioned, that amylaceous substances passed through the stomach, only having been but slightly altered by the saliva; in the same way, on opening the duodenum of a dog, he repeatedly found fatty matters coming from the pylorus perfectly unchanged. He then repeated Blondlot's experiments in regard to the action in the stomach upon meat, the principal constituents of which are fibrin, albumen, and creatin, all nitrogenized substances—he found that the gastric fluid had, when kept sufficiently long in contact with it, rendered it fit for absorption, for when he injected the fluid containing it into the circulation, that it was not discoverable in the urinary secretion, thus showing that it was assimilable; this was the case also with casein. He was astonished, however, to find, on examining the fluid mass passing through the pylorus, that he could still discover, though minutely divided, the muscular fibres in parts, showing that the whole amount had not been completely digested. So that, as the saliva had the properties of converting starch into dextrine, &c., when it remained long enough with it, yet that ordinarily in the process of digestion, the time of contact was too short for the complete action to take place. So it was in regard to the digestive powers of the gastric fluid on azotized food. M. B. agrees with Blondlot, that liquid albumen is not coagulated at first, before being digested in the stomach, but that it is merely rendered opalescent.

* Cours de Physiologie, par P. Bérard, 1850.

Casein, however, is coagulated before being altered by digestion; the mucous membrane itself, as is seen in that of the stomach of the calf, known as rennet, has the effect of coagulating milk. All other classes of aliment are merely saturated and softened by the saliva and the watery portion of the gastric juice, and thus more easily divided by the peristaltic motion preparatory to their digestion further down in the intestinal tube.

The action of the gastric juice is found by Bernard not to be the same in all animals, but to be modified by the kind of food used habitually; thus, in herbivorous animals, as proved from that collected from an ox, it acts much less perfectly than does that taken from a dog.

It is thus seen that not only did Beaumont exaggerate the action of the gastric juice, in supposing it the universal solvent, but Blondlot erred even in thinking that in the living subject all the nitrogenized articles were digested in the stomach. So that the term *chyme* has in fact no definite meaning—the bolus passing out of the stomach being composed partly of digested and partly of undigested food—there being only a portion of the amylaceous and of the azotized, which are ready for assimilation, and in ordinary digestion the whole mass is submitted to further action from the intestinal fluids. Instead of digestion being completed in the stomach, it is merely commenced.

The question has often presented itself as to what was the principle in the gastric fluid which acted upon food? Those who believed that there was present more or less of free hydrochloric acid, taught that it was it, or at least that because acids destroyed meat, therefore, it by so doing digested them. Of this hypothesis Blondlot disposed by trying in separate vials the comparative action of mere acidulated water and the gastric juice; in both there was a change, but in the former there was merely a disaggregation of the muscular fibres. Bernard's experiment was still more conclusive. After submitting pieces of meat to the action of the two liquids, as did Blondlot, he injected them into the circulation, and found that that which had been subjected to the action of the simple water and acid, had passed off entire in the urine, whereas the other, having been assimilated, could not be detected in the renal secretion. The action, moreover, stated M. B., of the two fluids upon bony matter was very different, the acid water attacked and destroyed the mineral portion, whereas the gastric juice digested the gelatin and left the phosphates and carbonates unaltered. This may be easily seen by examining the stools of dogs fed upon bones. The active principle of the gastric fluid is now generally admitted to be the organic principle *pepsin*, which can be precipitated by absolute alcohol. It differs from all other organic substances by its coagulating casein without the aid of an acid. It does not, however, digest any species of food when not mixed in an acid solution, as may be easily seen in artificial digestion by rendering the liquid alkaline, but the process may be again commenced by adding any other acid; whereas, if you raise the temperature above 98° Fahr., you destroy for ever the digestive properties of the pepsin, and the fluid ceases to act. The action of the acidulated water is necessary before the pepsin can effect anything; the latter acting in force in proportion as the food is softened and divided by the other.

The influence of the nervous system over the secretion of the gastric juice, and consequently upon digestion, has been much discussed, and contradictory experiments have been published, even by such men as Sir Benjamin Brodie, Dr. John Reid, Longet, and Magendie. Some asserting that the integrity of the pneumogastric was necessary, and some the contrary; others again that it was through the grand sympathetic that any nervous influence was conveyed. To show what M. Bernard has done to elucidate this point, we propose giving two or three of his experiments. The two substances, *emulsine*, the albuminous matter found in almonds, and so named by Liebig, and the *amygdaline*, the principle of bitter almonds, when administered separately, are perfectly innocuous, but when united, there is at once a formation of hydrocyanic acid, and of course if they come in contact in the stomach of an animal, they must prove poisonous. Aware of this, he selected two dogs, on one of whom only he performed the resection of the pneumogastric nerve; to both he administered at the same time a certain quantity of emulsine, and half an hour afterwards he gave to each of them the same amount of amygdaline. The dog whose nerves were intact escaped without injury, whereas the other died in a few minutes. From this it was concluded that the first animal had digested the emulsine before the amygdaline reached it, but, in the second dog, in consequence of the cutting off of the nervous communication the gastric juice was not secreted, and the first ingested substance was not acted upon, but remained in the organ, and the other coming in contact with it, there was a formation of prussic acid; which, being absorbed, proved fatal. Another experiment proved indirectly the same fact. He gave to a rabbit which had been fasting thirty-six hours, and whose urine had become clear and acid, a meal of carrots, and in two hours and a half the urinary secretion became opake and alkaline, and so remained for eighteen or twenty hours. He found by cutting the pneumogastric during that time, that the urine changed again to be acid in its reaction, and clear. On another animal he perceived that a resection of the nerve, immediately after the meal of carrots, prevented entirely the urine from becoming filled with carbonates, which cause the alkalinity and opacity, and which are derived from the digested vegetable. So the urine remained clear exactly as if there had been no carrots in the stomach. This effect is attributed to the non-secretion of the gastric fluid by which the substances could be digested; or, as in this case, prepared for digestion. There being no liquid in the stomach, of course they were not softened or divided, but remained there unaltered. We are at a loss to conceive of any other than M. B.'s explanation of these results, namely, that the gastric juice is under the influence of the eighth pair. But he has given us still more conclusive evidence of this in his researches on the stomach by means of fistulous openings. He cut the pneumogastric of an animal into whose stomach he had previously introduced a canula at the moment when there was a free flow of gastric juice excited by the presence of an alimentary bolus; at once he saw the mucous membrane, which had been tense and turgid the moment before, become withered and pale (the vascularity greatly diminished), and the gastric juice ceased to flow. On introducing his finger into the stomach itself, the walls were perceived to be perfectly flaccid, and there was no longer the gentle pressure which he had previously felt. Another proof that after the resection there was no further secretion, was that on putting sufficient salt into the stomach to destroy the power of the gastric juice secreted before the operation, the bolus became putrid in consequence of the arrest of digestion.

We have now traced the alimentary bolus to the pyloric orifice of the stomach about to pass into the duodenum. At this point, as we before stated, it has received the name of *chyme*, which, far from being a homogeneous mass, consists of the fatty matters unaltered, of the amylaceous but partially converted into sugar by the saliva, and finally of the azotized material digested ordinarily but in part by the gastric juice.* The whole has invariably an acid reaction.

It is difficult to ascertain from the text-books on physiology what are the prevailing views as to what takes place in the process of digestion below the stomach. The impression seems to be that the intestinal fluids separate the nutritious portion of the chyme, which is absorbed by the lacteals and known as *chyle*, from the excrementitious which passes through the intestine. Carpenter speaks of chyle as imperfectly elaborated blood. It is particularly to this heretofore obscure stage of digestion that we think M. Bernard has made some brilliant contributions, as we shall proceed to show.

The Bile.—The most important point in regard to this fluid is to decide in the first place the much discussed question whether or not it is purely excrementitious. That it is so, was supposed from the character of its proximate elements, the amount of hydro-carbon; to remove which, as an auxiliary to the lungs, whether from the aliment as such, or as effete matter taken from the blood in the course of the circulation, was thought to be its use. Moreover, it flows not as the ordinary secretions at the time of digestion, when needed, but, like the urine, more or less constantly. An objection, however, seems to present itself to its being exclusively excrementitious, from the point in the intestinal canal where it is emptied. Why, if merely to throw off what is injurious to the system, should it be poured out at the very commencement of the small intestines, where absorption is most active? Such is not the mode in which nature provides for the other excrements. It would, from this fact merely, be natural to suppose that it did act as a chylo-poietic fluid. But we

* Dr. Beaumont's tables, so frequently quoted and so much relied upon, of the mean time of digestion of the different articles of diet, are thus rendered of but little value—he having founded them upon the supposition that, when any substance disappears from the stomach, it has undergone complete digestion.

are not without experiments. M. Blondlot, whose authority we have so frequently quoted, declares,* after several times tying the ductus choledochus of dogs, that the biliary secretion is not necessary for digestion; because, though nearly all the animals died, yet that in two or three instances they survived months, and in one case years. The deaths he thought were caused by the retention of the bile within the blood, as was shown by the jaundiced tint of all the tissues and secretions of the body; indeed, in more than one case the bursting of the biliary duct into the peritoneum caused death. The inferences of Blondlot might appear conclusive had we not the experiments of the Belgian professor, M. Schwann, which have been since repeated and verified by M. Bernard. They too succeeded in tying the ductus choledochus without killing the animals, but on making autopsies of them some months after, they found that a new duct to convey the bile to the intestine had been formed! In other cases they inserted canulæ into the duct so as to convey the bile outward, and thus prevent a new one from being made, and the invariable result was fatal. This was conclusive, as it showed that death was produced not only by the poison of the bile, but by the mere absence of its flow into the digestive tube. Moreover, if the biliary fluid is only effete matter, why is it that it is not detected as such in the excrement of the bowels? Liebig tells us that no choleic acid is ever found in the stools, but that it must be re-absorbed; in fact, the colouring matter is all that is there discoverable of the bile. It being decided that this secretion is necessary for the process of digestion, and not merely excrementitious, the important question arises as to how and upon what substances it acts. To solve this, Sir Benjamin Brodie tied the ductus choledochus of cats, and on opening the abdomen he found that the lacteals were not filled, as usual, with a whitish mixture of the consistence of cream, but with a transparent fluid, which he regarded as composed of lymph and of the most liquid portion of the chyme. Supposing that the white chyle was fatty matter, he drew the conclusion which he published, that the bile digested fat. Magendie, on hearing of these interesting experiments, hastened to repeat them on dogs, his favourite victims. A great number were sacrificed before he could succeed in getting two of them to live even a few days; and in them, contrary to what Brodie had stated, he found that a white chyle had been absorbed by the lacteals, and that as before fæcal matters had been formed, notwithstanding the interruption of the flow of bile.

It is painful to think of the loss of life to the dog and cat race this difference of opinion, between such distinguished men, has given rise. M. Blondlot alone killed no less than twenty-five dogs in these experiments. Several others entered the field, among them Dr. Mayo, and MM. Tiedemann and Gmelin. It is curious, and at the same time discouraging, to see the different conclusions at which they arrived—the partisans of each equally positive in

* Essai sur les Fonctions du Foie, Paris, 1846.

their opinion. As we shall presently show, M. Bernard has cleared up the difficulty by explaining what is properly speaking the cause of the white fluid in the lacteals, from what it is formed and how.

Beaumont added bile, taken from a cow recently killed, to some chyme coming from St. Martin's stomach, and found that the effect was to separate it into three distinct parts, a reddish-brown sediment at the bottom, a whey-coloured fluid in the centre, and a creamy pellicle swimming on the top. The centre he concluded was chyle, the creamy pellicle digested fatty matter, and the sediment the excrementitious portion. Others had the same results. Müller* made an emulsion by adding bile to oleaginous substances. Tiedemann said that the white flakes seen in the liquid was mucus. This we believe was the uncertain state of our knowledge in regard to the action of the bile, when Bernard commenced his experiments. He had observed that in most animals, as in man, the duct from the pancreas opened either side by side in the duodenum, or else together with the biliary duct; in the dog that there were two canals from the pancreas, one emptying with the ductus choledochus and the other lower down in the intestine; in the rabbit, however, by a singular provision of nature, the pancreatic duct entered the duodenum eleven or twelve inches further down than that from the pancreas, the intestinal canal being unusually long for so small an animal.

Having procured the biliary secretion from the rabbit, perfectly pure and fresh, and certain that it contained no other secretion, he tried its effect upon the several varieties of food. He found that when thus used alone, it had no possible action except upon rancid oil, with which it formed a kind of soap, but upon pure oil there was nothing more than a mere mechanical mixture, the two substances separating when left standing, the oil floating on the surface. Believing that it could not be without its use, he determined to try it in combination with the other intestinal fluids. It was a thought worthy of him, and productive of important results.

Mixed with gastric juice, either artificially or in the upper part of the duodenum, as could be observed in the rabbit, it is shown to have several properties. It renders the mass alkaline; it precipitates and renders insoluble the azotised portion, leaving untouched the saccharine and the fatty. This precipitate Magendie designates "le chyle brut." It further renders the chyme indestructible and imputrefactive, by regulating the chemical reactions and the evolution of the gases arising from decomposition. This M. Bernard substantiated by showing two pieces of meat in vials, where they had been put three months previous, the one with gastric juice alone, and the other with gastric juice and bile mixed—from the former, there was a strong ammoniacal odour resulting from decomposition, whereas the latter was pure and free from any smell whatever.

By precipitation, bile, moreover, has the effect of arresting every kind of fer-

* Manuel de Physiologie.

mentation of all organic substances. To prove this to his auditors, Bernard introduced into the stomach of a dog a cake of yeast, and in two hours, when it was perfectly saturated with gastric juice, he withdrew it, and applying heat to a portion of it, there was an evolution of carbonic acid, showing the commencement of the fermenting process; to the remainder, before employing heat, he added some bile, and though kept over a hot water bath much longer than the other, there was no evidence of its fermenting. Another experiment was performed to demonstrate this property of preventing fermentation, by the administration of the emulsine and amygdaline. The emulsine, which is the ferment, was injected into the rectum, and the amygdaline into the stomach—in due time the latter descended, or part of it, and came in contact with the former in the rectum, and in a few minutes, from the formation of prussic acid, the animal died. In another subject the emulsine was placed in the stomach, and the amygdaline in the rectum, the reverse of what was done in the first instance. Here there was no fatal result, in consequence of the bile having destroyed the ferment before it reached the other substance in the rectum. Still another proof may be drawn from M. Schwann's experiments in introducing canulæ into the ductus choledochus. The animals licked the bile as it flowed externally, and it caused their death the sooner, by precipitating the pepsin in the stomach, and thus preventing all digestive action of the juice. In this way the bile prevents the formation of the gases in the intestinal canal from decomposition of the several articles of food. The medical application of this will at once strike your readers—when from any cause there is a deficient supply of bile to the intestine, or when more food is swallowed than the quantity of bile can act upon (for of course there is a limit to the amount of this secretion, as Beaumont found there was to the gastric juice), or if from any other pathological cause the character of the fluid is altered, or its action prevented, there is a putrid fermentation, from which there is an evolution of flatus and a consequent diarrhœa. Under these circumstances there is a predominance of alkalies, and we would suppose, that acids could be advantageously employed. By what principle the bile acts seems undetermined. Prof. Platner, of Heidelberg, supposes that the salt, the choleate of soda formed in the bile from the union of the peculiar fatty acid, choleic with the soda, acts upon the lactate of albumen, fibrin, or casein, as the case may be, and that there is a double decomposition resulting in the formation of the lactate of soda and the choleate of albumen, &c.

Before proceeding to the action of the bile in combination with the pancreatic juice, which constitute what is, properly speaking, the *intestinal fluid,* let us arrive at what to us is one of the most brilliant discoveries of this distinguished physiologist—*the use of the pancreas,* which is, that its secretion enjoys, to a very high degree, the prerogative some had attributed to the bile, that of emulsioning oleaginous bodies, and thus rendering them capable of absorption by the lacteals, and that in fact, it is the only fluid concerned in the digestion of fatty matter! In making some experiments, with a view of comparing the digestion of herbivorous animals with carnivorous, he, by keeping all other food out of its reach, forced a rabbit to eat nothing but meat, after which he opened the abdomen, and observed that the absorbent vessels of the small intestines contained a limpid fluid for the distance of about twelve inches below the pylorus, and that from that point they were white, and contained the same fluid which is found in the lacteals of the human subject, and in the dog throughout the whole extent of the duodenum. Remembering that, in man, the ducts of the liver and of the pancreas enter together near the pylorus, and that in the dog one of the pancreatic canals empties high up, with the duct coming from the liver, it at once occurred to his quick mind that it must be the secretion of the pancreas which made the milky fluid, by acting upon the fatty matters, and, of course, it was only below the insertion of the duct which conveyed it to the intestine that the absorbents contained the emulsion which gave them their peculiar white appearance.

He proceeded forthwith with direct experiments, and extracting pure pancreatic juice, he mixed it at a mild temperature with oil, and afterwards with butter, mutton tallow, and indeed, with all varieties of fat, and with them all he made an emulsion resembling in colour and in consistence, and apparently in every particular, the chyle he had extracted from the mesenteric lacteals. To test whether or not there was any other liquid or secretion of the body which had the same effect, he tried the bile, the saliva, the gastric juice, the serum of the blood, and even the cephalo-rachidian fluid; with all, the result was a mere mechanical mixture, which, on standing a few minutes, was destroyed just as though it had been attempted to mix oil and water; whereas, the emulsion formed by the fat and the pancreatic juice remained perfectly unchanged. These experiments, made out of the body, were not conclusive enough to a mind so accustomed to close and vigorous observation, but needed confirmation, which he was not slow in procuring for himself and for science. After keeping a rabbit fasting for some time, he gave it a full dose of twenty grammes of fat, and allowing sufficient time for it to be pushed down the intestine, he killed the animal in three hours, and found the absorbents nearly empty to the point of insertion of the pancreatic duct, containing only a small quantity of a limpid fluid; whereas, below that they were distended with white chyle. In the intestinal canal above the duct of the pancreas, there was some melted fat which was unaltered in colour, but below it was seen more or less of the same white emulsion corresponding to that contained in the lacteals. Here it was evident the biliary secretion had had no effect, for it was poured into the canal near the stomach, and the substance was unaltered for inches below. His next essay was in tying the pancreatic duct of another animal of the same species before giving the oil, and on opening the abdomen, after the same lapse of time, he found the lacteals free from chyle, and the oily matter undigested in the intestinal tube, passing down to be thrown off in the excrement. On putting a ligature around both the pancreatic ducts of a dog, he had the same result. He could now assuredly be satisfied of the truth of his discovery, that the digestion of fatty matters was the peculiar office of the pancreas. On repeating his experiments in the presence of M. Dumas, he remarked, that he knew of no alkali, not even the caustic potash, which could make with oil so perfect an emulsion, or in so short a time.

There can now be no difficulty in explaining the contradictory results of Brodie's and Magendie's experiments in regard to the action of the bile on fatty matter. The former had operated upon cats where, owing to the proximity of the ducts of the liver and the pancreas, it is difficult to tie the one without the other; and he, by putting a ligature around both, of course prevented the flow of the pancreatic juice as well as that of the bile, and thus interfered with the digestion of fat, and consequently of the appearance of the thick white fluid in the absorbents. Magendie selected the dog, and in tying the ductus choledochus he probably included also one of the pancreatic ducts; but he left free the other, which opens further down in the intestine, and which acted upon the particles of fat, and they were afterwards taken up by the lacteals. Müller's error can be accounted for in the same manner; in the bile he experimented with, there must have been a mixture of the pancreatic fluid. If further proof be desired, it may be found in the fact mentioned by M. Bernard, that the herbivorous animals, when confined to their ordinary food, have no white absorbents or lacteals. He has, moreover, found that in all mammiferous animals the pancreatic duct always empties with or below the biliary duct, never above.—For the purpose of showing those present how he obtained the pure secretion of the pancreas, and its action, he had a dog placed on the table before him, and, after securing him well by tying his feet and muzzling him, he cut into his right flank, and drawing out the pancreas, which was of a bright rose, the colour it always presents when secreting, he inserted a small silver tube into the duct, and secured it there by a ligature. At the end of the canula was an India-rubber bladder, to receive the secretion which flowed only a drop at a time. At the close of the hour, during which time the animal continued eating without being in the least disturbed, the sac was found to contain about two fluidrachms (f℥ij). The Professor remarked that, if the canula was withdrawn and the wound closed, it would be healed up in two or three days without any unpleasant consequences. The fluid, as thus drawn, was colourless and viscid, and in some respects resembled the saliva extracted from the submaxillary gland, but differed from it essentially, being coagulable by alcohol and by heat. Its alkalinity was evident, only a drop sufficing to restore the blue to reddened litmus paper. Its alkaline reaction had been denied by Tiedemann, which, said M. Bernard, must have been owing to his collecting it as it flowed into the intestine, where the acid mucus changed it, for, when collected pure from the duct itself, he had invariably found it alkaline. On adding some of the pancreatic fluid collected in the little bladder to olive oil, and stirring the mixture for a few minutes, there was formed a creamy fluid corresponding very closely to some chyle, which he had, a few minutes previous, collected from the lacteals and the thoracic duct of a rabbit fed upon the same oil. M.

Bernard exhibited the duodenum of a rabbit, and the absorbents in the mesentery. Those from opposite the insertion of the ductus choledochus down to the point where the pancreas empties its secretion, contained a transparent fluid in small quantity; whereas, below that, the white ramifications, showing the presence of the fatty emulsion, were very evident. This last experiment, which is perfectly conclusive, can be easily verified by any one. Keep a rabbit fasting for thirty-six hours, and then administer a dose of fat and kill it in a few hours. To push the oil down in the intestine, and to facilitate matters, a meal of some vegetable may be given.

Although oily and fatty matter are neutral in their reaction, the emulsion is at first alkaline; but in artificial digestion it soon becomes acid, owing, M. Bernard thinks, to the transformation of the fat into glycerin and the fatty acids, margaric, stearic, or oleic, as the case may be. This is evidenced by the odour of butyric acid when butter is the fatty substance used, and the mixture is permitted to stand some little time. We see it stated by Dr. Chambers* that Prof. Frericks, of Berlin, though he admits the truth of the digestion of fat by the secretion of the pancreas, yet denies that there is such a transformation, for that the oil globules do again unite. To this we reply, that we have ourselves seen the emulsion after it had remained for days without being disturbed, and, on a close examination, not the slightest change could be detected from what it was immediately after the mixture. Dr. Chambers says that any animal substance undergoing a chemical change, which pancreatic juice does with great rapidity out of the body, in contact with butter, will cause an interstitial fermentation of the butter itself, and thus give rise to the acid smelt. This may be the explanation; but both Drs. Frericks and Chambers have misunderstood M. Bernard, in supposing that he thought that there was a transformation of the fat into glycerin and the fatty acids in the intestine, from the action of the juice of the pancreas; on the contrary, he has proved such is not the case, by finding the emulsion collected in the lacteals to be still alkaline, and in it the presence of fat globules under the microscope, though very minutely divided. Moreover, we have the testimony of MM. Bouchardat and Sandras,† that they have found and recognized in the chyle, the oil of sweet almonds, mutton and pork fat. Certain it is that the globules found in ordinary fat cannot be assimilated without being altered, for, if injected into the circulation, they prove fatal by their arrest in the capillaries of the lungs, through which they are too large to pass, just as bubbles of air entering the veins cause death by the stoppage of the pulmonary capillaries. M. Bernard does think it probable that, in the ultimate changes which fat undergoes in the body, there is a transformation into glycerin and fatty acids. His ground for so thinking is the result of an experiment of M. Magendie,

* On Corpulence, or Excess of Fat in the Human Body, by T. K. Chambers. London, 1850.

† Recherches sur la Digestion, et l'Assimilation des Corps Gras, 1845.