THE PHYSIOLOGY OF DIGESTION:

A REVIEW OF THE

Leçons de Physiologie experimentale appliquee a la Médicine faites au College de France, par M. Claude Bernard, Professeur de Médicine au Collége de France, Membre de l'Institut de France, &c., &c. Paris, 1855–6. Two vols. 8vo, pp. 512 and 510.

Lectures on Experimental Physiology as applied to Medicine. Delivered at the College of France by M. Claude Bernard, Professor of Medicine at the College of France, Member of the Institute of France, &c., &c.

The volumes we are to consider in this review are the first of a series, seven in number, containing the physiological views of Professor Claude Bernard, as expressed to the College of France in a series of lectures of that practical and demonstrative character which distinguishes the teaching of Biological science in the continental seats of learning. The author of these lectures, long an assistant of Magendie's, now his successor, has prosecuted the experimental method of his master and predecessor with so much tact, penetration, and perseverance, that perhaps no physiological inquiries have, in these times, been more productive of the fruits of discovery than those of Claude Bernard, and we cannot but regard him as one of the greatest physiologists France has produced since the days of Magendie. In the following pages, we shall endeavour to give our reader an analysis of the contents of these volumes, and in doing so, we shall make the views of Bernard a text, in the exposition of which it may be necessary to bring forward the observations of other authors, whose memoirs are comparatively little known in this country—at least they are not referred to in the most recent text-books of Physiology. We are of opinion that this is the most useful of the many ways of writing a review; for, by it not only is a condensed analysis of the author's particular opinions given, but the relation in which these stand to the observations and reflections of other thinkers on the same subject is, at the same time, brought out, and the ordinary reader is thus enabled to master an acquaintance with a subject, the literature of which might otherwise be too extensive either for his time or for his patience.

The first of these volumes is entirely devoted to a subject with which Bernard's name and fame are most intimately connected, *i. e.*, the glucogenic function of the liver. That sugar exists normally in the liver, is a fact long since made known by M. Magendie in France, by Garrod in England, by Schmidt, Lehmann, and others in Germany; more than that, Magendie, in 1846, read a memoir to the Academy of Sciences, in which he stated that sugar occurred normally in the blood, and that saccharine matter was most abundant during the period of digestion; but it remained for Bernard to indicate the connection between those isolated observations, and to complete the links of the chain, by showing that the liver is an organ, having for one of its chief functions, the preparation of saccharine matter, independently of the nature of the food. After several papers, Bernard, in 1853, published a work containing the results of an extensive series of investigations. Since that time, the glucogenic function of the liver has been a subject on which the most opposite and conflicting opinions have been expressed, and there is, perhaps, not a single fact of the whole question which has not been again and again asserted and denied. Even during the last year, the glucogenic theory has been denied an existence, and that upon grounds altogether new, but to this we shall again have occasion to allude.

After some chemical considerations upon the different varieties of sugar, and the means of recognising these qualitively and quantitively, our author demontrates the first fundamental fact of his doctrine, *i. e.*, that, as after death we find the kidney impregnated with urine, the testis with semen, and other glands with their secretions, so do we find the liver charged with sugar. Bernard has instituted a wide series of researches, from which it results that this is the case in man and other mammiferæ, birds, reptiles, fishes, molluscs, and some crustaceans. The average amount of sugar in the liver of mammifers was from half a scruple to two scruples for the four oz. of liver. By the use of the polariscope, Bernard found this sugar to be identical with that of diabetic urine—a fact since contested by Drs Owen Rees and Pavy.

Regarding the source of this hepatic sugar, the idea that the diet of the animal has aught to do with it, Bernard entirely refutes, by proving that the amount of sugar is the same, or nearly so, in the liver of the herbiverous, as it is in that of the carnivorous animal, that it is the same in dogs nourished on a purely azotised diet, as in dogs fed on a mixed diet. (P. 86.) But his chief ground for referring the source of this sugar to the liver itself, rests upon the *post mortem* examination of the blood of the portal and hepatic veins. An experiment of this kind, made upon an animal which has been nourished with a diet containing neither amylaceous nor saccharine principles, shows, that whilst the portal blood is quite free from sugar, that of the hepatic vein is charged with it. The correctness of this observation of Bernard's has been fully established by the careful comparative analysis of portal and hepatic venous blood, made by the celebrated chemist, Lehmann.[1] From these *post mortem* facts, but particularly from the last mentioned, Bernard comes to the conclusion, that the liver, besides secreting bile, is endowed with a far more important function—that of secreting and rendering to the blood, a considerable amount of sugar. From some observations on the Limax flava, he even attempts to localise anatomically these two functions, and to assign to each distinct anatomical elements, and emits the hypothesis, that as the bile has been supposed to be secreted by the cells

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[1] LEHMANN, *Lehrbuch der Physiologischen Chemie.* 1852.

occupying the centre of a hepatic lobule, (Kölliker), the sugar may be referred to the cells of the periphery. Neither of these opinions is supported by substantial facts. Having thus localised the source of sugar in the living animal, Bernard, in chap. v., traces its course in the circulation, and shows that it exists in greatest quantity at the junction of the vena hepatica with the vena cava, and that it gradually diminishes in quantity, until it reaches the lungs, where, under ordinary circumstances, it disappears. But, like other secretions, that of sugar is subject to certain physiological oscillations, during which its amount corresponds with the activity of the digestion, so that, when the latter function is at its maximum, sugar is found to go beyond the lungs, and to enter the general circulation, where it disappears, without being found in the urine or other excretions.

The general mechanism of the glucogenic function being thus roughly sketched, our author, in the next place, applies himself to an examination of the circumstances influencing the production of sugar. These he arranges under three classes:—

1. The modifications which the glandular elements may undergo.
2. The modifications presented by the circulation of the organ, viewed either in regard to the chemical composition of the blood, or in regard to the mechanical conditions of the circulation.
3. The influence of the nervous system on this secretion.

The result of the first of these inquiries appears to be rather imperfect, no careful analyses of the effects of disease having been made; but the effects of different kinds of diet have been carefully studied, and the general results are the following:—Abstinence causes a diminution of sugar in proportion as the blood itself diminishes; a resistance to this effect of abstinence is met with in cold-blooded animals, as frogs and fishes, in whose livers sugar continues to be secreted for a much longer time during abstinence than in the livers of birds and mammals. Connected with the effects of abstinence, Professor Valentin has made some interesting observations on hybernating animals, (marmots and hedgehogs), in which he found that both bile and liver sugar were present during the whole of the winter sleep of these animals, notwithstanding complete abstinence for five or six months.[1] In frogs, on the other hand, Schiff observed that, during the month of February, their livers contained no sugar.[2]

Under a purely fatty diet, Bernard found that the sugar diminished in the liver almost in the same degree as during abstinence. To try the effects of an azotised diet, some dogs were fed for several days on pure gelatine dissolved in water, and, on examining the livers of these animals, sugar was found in the normal proportion; hence Bernard concludes that it is the azotised elements of the food which are employed to form the sugar; and the analyses of Lehmann strongly support this conclusion, since they shew that the blood of the vena porta,

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[1] VALENTIN, *Untersuchungen zur Naturlehre*, iii p. 195.
[2] SCHIFF, *Archiv für physiologische Heilkunde*, i. p. 263.

in traversing the liver, loses a certain quantity of its azotised principles, and that fibrine becomes notably diminished. The results of the experiments on the effect of an amylaceous and saccharine diet, are somewhat curious; for although in both cases sugar was found in the portal blood on its way to the liver, the amount of sugar in the latter did not surpass that contained in the livers of dogs fed entirely upon gelatine. From this fact, one is led to suppose that the liver must have the power of destroying sugar, as well as of forming it—a rather singular conclusion. But although, in regard to sugar, no difference existed, Bernard remarked that the decoction of the hepatic tissue of an animal, nourished exclusively on amylaceous and saccharine principles, differs materially from that of an animal fed on gelatine or other azotised food, in presenting a turbid, milky, emulsive-like appearance; and, from some experiments on saccharine absorption, he comes to this conclusion:—

"There does not rest a doubt upon the fact, that saccharine matters, arriving by the portal vein, do not traverse the liver, but that they occasion in this organ the production of this new matter, which gives to the liquid this whitish appearance, and which appears to be a fatty matter united with a protein substance."—P. 154.

This is a very important point in the history of the subject; for in the production of this emulsive matter lies the theory of the intimate mechanism of the glucogenic function, and it is in this direction that the most important progress has been made during the two years that have elapsed since the publication of these volumes. The broad fact, that sugar is formed in the liver independently of any direct extraneous origin from the food, having been satisfactorily determined, the question naturally arises, from what material, where, and by what means is this formation of sugar accomplished? Although the answers to these questions are by no means so perfect as may be desired, still, since the publication of Bernard's classical volume, much light has been thrown upon the subject. Whilst Lehmann has shewn that after flesh diet, neither in the contents of the stomach or intestine, nor in the portal blood itself, is a substance to be found which through simple agents (as diastase, weak sulphuric acid, &c.), can be easily changed into sugar, Bernard,[1] and, perhaps, more especially Hensen,[2] have proved that, in the substance of the liver, there exists a material, which, when dried, is white, amorphous, tasteless, soluble in water with opalescence, non-nitrogenous, colours iodine blue, changed by sulphuric acid into dextrine, and, with a higher temperature, into dextrine and a little sugar, (Bernard). Under the influence of saliva, pancreatic tissue, diastase, and also blood, (V. Hensen), it becomes rapidly converted into sugar. The researches of Pelouze[3] have shewn, that in many of its chemical characters, it closely resembles vegetable

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[1] Bernard, *Comptes rendus*, i. No. 12 and 26. *Gazette medicale*, No. 13. *Gazette hebdomadaire*, No. 28.

[2] V. Hensen, *Virchow's Archiv für Pathologische Anatomie*, Bd. xi., p. 395.

[3] Pelouze, *Comptes rendus*, i. No. 26.

starch; and Schiff,[1] who is also of the same opinion, believes that he has found it microscopically in the cells of the liver in the form of minute granules, differing in chemical and physical properties from the fat granules found in those cells. As to the source of this glucogenic material, the experiments of Bernard on diet, and the analyses of Lehmann already referred to, would make it appear that it is derived from the azotised elements of the food, but that, with a non-nitrogenous diet, its amount is greatly increased. The recent researches of Pavy on the effects of diet[2] have precisely indicated the amount of this glucogenic matter, or, as he has named it, *hepatine*, found in the liver after an azotised and a non-azotised diet: in the former case, it was, on an average, 6.97 per cent.; in the latter, 14.5 per cent., or nearly twice as much.

Our information on the substance, which acts as a ferment in transforming this material into sugar, is as yet extremely imperfect. Bernard, in these lectures, does not refer to it; indeed, at that time, although he regarded the process as a fermentation, he had not so far penetrated its intimate mechanism. Hensen, in his inquiries into the subject, has found that the blood, not only of the portal system, as at first his facts led him to believe, but also that of the carotid or vena cava changes the glucogenic matter into sugar—nay, that in the liver itself a body may be found, after all the glucogenic matter is converted into sugar, which effects the saccharine transformation of newly added glucogenic material.

After making some remarks upon the peculiarities of the hepatic circulation and the influences of temperature, age, sex, &c., in the glucogenic function, Bernard details his experiments upon the temperature of the blood in different regions, and as the result states:—

> "The hottest point of the blood of the whole organism is found in the vena cava, at the entrance of the hepatic veins."—P. 201.

From this observation Bernard concludes that the liver is the chief seat for the production of animal heat, and that production of heat is a consequence of the change of material necessary for the formation of sugar in that organ. Now, to this mode of reasoning we must object, since it seems to be based upon the supposition that, where most free heat is present, there most heat is produced. The temperature of the blood in a vessel must depend not only on production and communication of heat, but upon the relation which these bear to the abstraction of heat. Thus, in the lungs, where the evaporation and the heating of the inspired air must necessarily produce such cooling effects as to overbalance the reception of heat, it by no means follows that the consequent reduction of temperature proves a diminished heat production. So in the liver, opposite conditions exist which make it probable that the blood flowing out of it owes its high temperature as

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[1] Schiff, *Archiv für physiologische Heilkunde*, i. p. 263.
[2] Pavy, *Guy's Hospital Reports*, vol. iv. 1858.

much to the diminished cooling influences as to the actual production of warmth.

In regard to the destruction of sugar, Bernard narrates some experiments which show that, within certain limits, sugar is destructible in the blood (p. 217), and that these limits are determined by the amount of blood in the animal. Some kinds of sugar—such as cane sugar, sugar of beet-root, &c.—are never destroyed, but are constantly eliminated by the kidneys. It has been already mentioned, that, if sugar is poured into the blood in large quantity, it is not destroyed in the lungs, but passes beyond these organs into the general circulation: this, indeed, occurs during digestion, but in a state of health the sugar thus generalized throughout the circulation never passes into the urine, the sensibility of the kidneys not being susceptible of such a stimulus as this small amount of sugar in the blood may produce. From some of Bernard's experiments, it would, however, appear that the presence of sugar in the urine is merely a question of the amount existing in the blood. Lehmann has determined, in dogs and rabbits, that if the amount of sugar arriving at the lung surpasses the 30th of a scruple for the 100 scruples of blood, the sugar appears in the urine. Experiments are detailed in chap. xi., which show that, if the proportion of sugar given up to the blood is increased by pressure on the liver, or by reducing the quantity of the blood itself, sugar passes into the general circulation. It is important to keep in view these experiments—which all appear to have been most carefully made, and in accuracy of detail are unchallengeable—in connection with a theory which supposes that the destruction of sugar is effected by oxidation in the lungs. M. Reynoso, having observed that under ether and chloroform, sugar appeared in the urine, supposed that it resulted from imperfect oxygenation of the sugar in the lungs. Bernard was at first an advocate of this theory; but, in consequence of new facts, he has been compelled to abandon it. The principal objections to the theory are that, although from about the 5th month of intra-uterine existence to the 6th or 7th, the urine is found impregnated with sugar, at the termination of this period, instead of going on increasing towards birth, as it would do, if its increase depended upon a want of pulmonary oxygenation, the urine becomes entirely freed from it. Add to this the fact that the urine of very young fœtuses contains much sugar, when the liver contains not a trace. Besides this indirect evidence against the oxygenation theory, direct proofs against it are furnished by the experiments of Bernard, showing that non-saccharine blood absorbs more oxygen, and gives up relatively less carbonic acid, than saccharine blood; and, by the experiments of M.M. Reynault and Reiset, which prove that the amount of oxygen exhaled in carbonic acid gas, relative to the amount absorbed, is greater during digestion than during abstinence. Lastly, experiments are detailed in Bernard's work, proving that oxygen has no more power in the speedy destruction of sugar than other gases. Another theory, which supposes that the destruction of sugar in the economy is due to combustion from contact with alkalies, is held by Bernard to be equally insufficient; and the experiments of Lehmann and Becker, with injections of sugar and alkalies, strengthen his opinion.

Without advocating any theory as to the destruction of sugar, farther than that the process occurs in the blood, M. Bernard expresses his conviction, that it is at the moment of its formation that the sugar of the economy plays the most important part. Besides being intimately concerned in the production of heat, (a hypothesis which we hold as being far from proved), he supposes that the production of sugar is closely connected with the formation and development of organic cells, and founds his opinion upon the observation, that a fœtal lung or muscle, during its development, contains a matter susceptible of being changed into sugar (p. 250). The analyses of Lehmann have taught us that the red cells of the blood of the vena hepatica are essentially different from those of the portal blood, but whether they are new cells formed in the liver, remains a question upon which fresh investigations are wanted. We cannot but regard the doctrines which Bernard advances on the subject as extremely problematical.

The influence of the nervous system upon the glucogenic functions, and the artificial production of diabetes by puncture of the floor of the 4th ventricle, form the subjects remaining for consideration; but we must forbear entering into the details of these phenomena, wonderful and interesting though they be; and we regret this the less, as they will again come under our notice in the next two volumes of Bernard's work.

The discovery of the glucogenic functions of the liver, and the philosophic manner in which the facts connected with it have been studied, form perhaps the brightest pages in the history of Bernard's scientific achievements. His doctrines have been made the subject of the keenest scientific criticism, as of the most evident puerile cavillings, but as yet the most formidable of those attacks, especially the objections of Figuier, Colin, and Longet, have been shown, by Bernard and Lehmann, to rest upon great errors in the mode of observation. Very recently, the glucogenic function has been denied an existence by Dr Pavy of London,[1] one who is by no means a new dabbler in this field. He maintains that the phenomena on which the theory rests, are all observed *after death*, and from his experiments of catheterism of the right side of the heart of a living animal, he comes to the conclusion that the formation of sugar in the liver is a mere *post mortem* change. Now, we feel inclined to admit that Bernard was not justified logically in concluding, that, because after death the livers of all animals contained sugar, and because, after death, sugar exists in the blood of the hepatic veins, it must also be formed there during life. However, if Dr Pavy would examine these lectures more

[1] *Guy's Hospital Reports*, vol. iv. Third Series.

carefully, he would find that Bernard did not rest his opinions *solely* upon this *post mortem* evidence, but that he examined blood taken during life from the right ventricle and other parts of the vascular system (p. 120); and then what becomes of the experiments of artificial diabetes, if the sugar is only a *post mortem* product? We cannot help thinking that Dr Pavy has not sufficiently performed the experiment of catheterism of the right ventricle, on which alone the question he has raised depends.

The second of the volumes before us contains the author's views upon the various functions subservient to digestion, viz., the nature and uses of the salivary, gastric, biliary, pancreatic, and intestinal secretions; but by far the most of his attention is devoted to the salivary and pancreatic functions. We think that the arrangement would have been better had he taken up each of these functions in their natural order of succession, instead of going from the salivary to the pancreatic organs, and then returning to the gastric, biliary, and intestinal functions.

Beginning with a sketch of the history of the anatomy and physiology of the salivary glands, Bernard claims for himself the distinction "of having been the first at this period (1847) to obtain the submaxillary and sublingual salivas in the dog, and to have shown that they differ from the parotid saliva in many of their physical and chemical properties," (p. 31.) Anatomy shows no such essential difference in the structure of these organs as to lead one to suppose that the secretions differ physically, chemically, and functionally; since, although the arrangement and disposition of the glandular structures may present differences in different animals, and perhaps in different glands, the ultimate element of all is the same, viz., a simple epithelial cell, somewhat granular, and possessing one or more nuclei. Physiological experiment and induction show that the parotid is subservient to mastication, being most developed in animals in whom trituration is difficult and slow; and, as appears from the interesting experiments of M. Colin,[1] the saliva secreted by the parotid in animals which chew with alternate sides of the mouth, corresponds to the sides employed in the masticating process. M. Bernard details many interesting experiments, proving that the parotid secretion plays a special part in mastication, and that its amount is regulated by the hardness and dryness of the food, the nervous channels being the lingual branch of the 5th pair, and the facial, irritation of either of which produces abundant salivation, (p. 69.)

The secretion of the submaxillary gland differs from the parotid in being less fluid, and, owing to the presence of organic matter, in becoming gelatinous on cooling. From experiments with a variety of sapid bodies placed upon the tongue, Bernard has observed that in this gland most activity occurs, and thence infers that its saliva is subservient to the sense of taste. This conclusion would appear to be

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[1] Colin, *Traité de physiologie comparée des animaux domestiques.* Paris, 1854.

strengthened by a fact furnished by comparative anatomy—that in the carnivoræ the gland is largely developed, whilst in graminivoræ it is almost awanting. Experiments with the nervous connections of the gland also support this view, for galvanization of the central end of the lingual branch of the 5th pair (divided below the submaxillary ganglion) produces abundant submaxillary secretion: the nerve conveying the impression to the gland is probably the corda tympani; at all events, galvanization of the branches of the submaxillary ganglion have this effect. The secretion of the sublingual gland is physically marked by a very great viscidity, from which, as well as from other characters, Bernard is inclined to regard it as a large muciparous gland, (p. 93). According to physical characters, he distinguishes the secretions of these glands into two classes; 1, the fluid or true saliva, secreted by the parotid and submaxillary; and 2, the viscid or mucus-like salivas, secreted by the sublingual and the muciparous glands. In regard to an accurate chemical knowledge of the different salivas, neither the analyses of Bernard, nor those of any other chemist, have furnished us with satisfactory results. These physical properties undoubtedly depend upon organic matters peculiar to each saliva.

After having examined, as far as possible, the separate salivary fluids, Bernard considers the chemical characters of the mixed saliva, under the heads of water and organic matters. On these different constituents, but more especially on the nature of the organic elements, no two chemists are exactly agreed—a result which is perhaps owing, on the one hand, to the different chemical methods employed in their study, and on the other, to the fact that the mixed saliva is subject to alterations, the mechanism of which it is impossible to understand until the different fluids of which the mixed saliva consists, have been individually studied.

Passing over, then, Bernard's opinions regarding the chemical constitution of the saliva, let us follow him through the last and most interesting part of his lectures on the saliva, on its mechanical and chemical uses. And first, in regard to the function of the parotid, which, as we before said, is connected with mastication, the following experiment well supports this conclusion:—An artificial opening is made in the æsophagus of a horse, so that any food which it might take would not enter the stomach, but would appear externally. Five hundred grammes of oats are given to the animal, and at the end of 9 minutes the whole is eaten, each bolus having passed out of the opening, bruised, softened, quite moist, clammy in the interior, and enveloped in a layer of gluey mucus-like saliva. The parotid ducts are now cut, so that no saliva flows into the mouth. Other 500 grammes of oats are given; mastication appears to be difficult, for the time taken for each bolus amounted to a minute and a-half, and when it had passed out, it was quite moulded, and surrounded by much mucosity, was smaller than those passed before section of the ducts; although the oats seemed well bruised, the interior the mass, instead of being clammy, appeared to be brittle, and very slightly moist. At the end of 25 minutes, the animal had not eaten the 500 grammes, 360 still remained. This experiment clearly shows the share which the secretion of the parotid has in the performance of mastication. Many other experiments, instituted by the Commission of Hygiene, are brought forward to prove the important mechanical part which the saliva plays in the acts of mastication, gustation, and deglutition. Bernard does not at all allude to the mechanical uses of the saliva, first noticed by Liebig—that, namely, of conveying a certain amount of oxygen to the stomach, to serve in some way in the functions of that organ. The hypothesis of Lehmann on this subject is that oxygen thus introduced prevents the resorption of the hydrogen compounds formed in the intestine, which would cause injurious consequences if introduced into the blood; but it seems very improbable that the small quantity of oxygen introduced into the stomach could affect the large amount of gas often formed in the liver and intestines. Observations on this subject are still awanting, and we are surprised at Bernard's not alluding to it.

In regard to the chemical uses of the saliva, the question as to the effect it has on the starchy elements of the food, has received much attention from physiologists since the discovery of Leuchs that saliva added to starch transforms it into sugar; and Bernard gives it a due share of attention. He observes, at p. 156: "This property of transforming hydrated starch into sugar is far from being special to the saliva, a series of other organic substances possess the same property; above all, when they are in the process of decomposition." He then goes on to state the facts which induce him to believe that this property is due to a decomposition of organic matter; but these do not appear to us to be at all satisfactory. All that we can deduce from them is, that in man the activity of the saliva in transforming starch into sugar, is much greater than in the lower animals. With human mixed saliva, with an infusion of the glands of man, or with the fluid derived from parotid fistulæ, starch is immediately transformed. Lehmann has asserted,[1] that "the sugar ferment exists only in a mixture of submaxillary and muciparous fluids." An observation of Bernard's, on human parotid and submaxillary saliva, (p. 165), would appear to contradict this; although with pure saliva from the parotid submaxillary or sublingual glands of the dog, no effect followed, either when they were used singly or mixed, the natural saliva of these animals (which contains muciparous secretions) does transform starch, though more slowly than that of man. The most important of Bernard's objections to the theory, that saliva acts in digestion by transforming starch into sugar, rests upon an experimental fact which he adduces, viz., that a dog having a fistula of the stomach, was fed on starch; an hour afterwards the contents of the stomach were tested for sugar, and scarcely any was found. To this experiment we may oppose the experience of Frerichs, Lehmann,[1] Jacubowitsch, and de Schroeder.

[1] Lehmann, *Handbuch der Physiologischen Chemie*, p. 152.

Jacubowitsch observed, that in a dog having a fistula of the stomach, fed on starch, sugar was found in its stomach, but after ligature of the salivary ducts, no sugar was formed.[1] Again, Schroeder,[2] under Schmidt's direction, convinced himself that sugar was formed in abundance in the stomach of a woman, who had a gastric fistula. In this case, after the use of bread, sugar was not found for some hours, but after paste had been introduced, a saccharine fluid was speedily formed. With these facts before us, and especially the experiments of Jacubowitsch, we feel inclined to believe, notwithstanding Bernard's objections, that the saliva has a chemical function to perform as well as a mechanical one, and that this function consists in the change of starchy matters in the stomach into sugar. That the effect of human saliva upon starch is due to its being in a state of decomposition is a mere assumption.

The subject to which Bernard next addresses himself, is the physiology of the pancreas, a gland long supposed to resemble in function the true salivary glands, an error resulting from the assumption of identity of function, from identity of structure. After giving a sketch of the history of the subject, Bernard claims for himself the discovery of one of its chief uses, *i.e.*, its chemical and physical action upon fat. In 1846, he was led to this discovery by a comparative study of digestion in carnivorous and herbivorous animals. He remarked, that when fat was introduced into the stomach of rabbits, it became modified only when it had reached a certain distance from the pylorus, low down in the intestine. In dogs this did not happen. The absorption of fat by the lacteals manifested the same difference, *i.e.*, in rabbits it occurred at a considerable distance from the pylorus, whilst in dogs it appeared at the commencement of the duodenum. Being naturally led to inquire into the cause of this difference, Bernard found that it corresponded with a difference in the point of ejection of the pancreatic fluid into the intestine; and that, whilst in dogs the pancreatic ducts opened very near the stomach, in rabbits the same ducts opened pretty far down the intestine; these points corresponded exactly to the parts at which fat became modified, (p. 179). The results of these, as well as of other extended inquiries, were published in 1848. Now, in regard to this action of the pancreatic secretion upon fat, Bernard certainly has had great merit in bringing forward many new facts illustrative of the subject, but his claim to the discovery of the function cannot be awarded him, since, in 1834, it was pointed out by Eberle, that the liquid of the pancreas has, for its function, the emulsionising of fatty substances.[3] In the historical sketch given by Bernard, we find no mention made of Eberle's work.

The results of Bernard's anatomical inquiries are important, since, from his directions, it appears that two ducts, a large and a small

[1] Jacubowitsch, *de Saliva. Dorpat*, 1848.
[2] De Schroeder, *Succi Gastr-human vis Digestiva*, 1813.
[3] Eberle, *Physiologie der Verdaung Würzburg*, 1834.

one, are constantly present. This observation has been confirmed by Verneuil, and its importance is great; for, in experimenting with the organ, ignorance of this fact would lead to grave physiological errors.

After giving some very precise directions for making pancreatic fistula, Bernard explains how it has happened that authors have given such different statements regarding the nature of the pancreatic fluid, by showing that much depends upon the nature of the animal selected for the operation of fistula, some animals being much more liable to peritoneal inflammation than others, and owing to the susceptibility of secretion in undergoing changes from the occurrence of pathological phenomena, one must be careful that these latter are absent. Regarding the nature of this secretion, as he has obtained it, the following description contains the sum of his observations:—

The pancreatic juice, like the rest of the intestinal secretions, is intermittent, commencing almost immediately after the ingestion of alimentary substances into the stomach, (p. 196), colourless, limpid, unctuous, tenacious, and strongly alkaline. It resembles albumen in being coagulable by heat and the mineral acids, but it also presents chemical peculiarities which differentiate it from that substance with which it has been confounded. Besides water and saline matters, pancreatic juice contains a peculiar organic matter, to which the coagulability of the juice, and its peculiar physiological properties, are to be attributed. This organic matter possesses properties common to albumen and to caseine, without being a mixture of either. It is to the presence of this substance, which plays the part of a true ferment, that the pancreatic fluid owes its property of being, without doubt, the most alterable in the economy. One of the most singular properties the organic matter possesses, is its reddening under the influence of chlorine. Experiments are detailed by Bernard, which show that no other liquid in the organism produces this red reaction with chlorine water. Nitric acid has a similar effect in producing a red reaction.

In its abnormal conditions, such as are produced by peritonitis, the fluid becomes more abundant, continuous in its flow, and not coagulable by heat. By Frerichs and others, these have been described as normal characters, and it is from such errors, as well as from similarity of anatomical structure, that the pancreatic and the salivary glands have been confounded. These pathological conditions are no doubt due to an altered proportion of the salts, water, and organic matter. In different animals Bernard has observed little difference in the nature of the pancreatic fluid. In the rabbit it appears to be less coagulable; and in the goose it alters less rapidly than in other animals. But from these physico-chemical considerations let us turn to the physiological aspects of the organ.

To the pancreatic juice three functions have been attributed by modern physiologists:—1*st*, Digestion of fat; 2*nd*, Digestion of azotised matters; 3*rd*, Conversion of starch into sugar. We shall briefly consider Bernard's opinions upon each of these functions.

1*st*, In regard to the digestion of fat. Bernard holds that this is the principal and most important function of the pancreas. Although he is not entitled to priority in making the observation, he has done much to establish the facts upon which the theory is based. The grounds he advances for his belief are—1*st*, Fatty matters injected into the stomach are emulsionised and absorbed only at the point of junction of the pancreatic ducts with the intestine; and, as comparative anatomy shows that this point varies in situation, the experiment is, as it were, made for us by nature, (p. 254). 2*nd*, When pancreatic fluid is mixed with fluid fat or oil, an emulsion is instantly produced, which is persistent. This experiment may be performed equally well with olive oil, fresh butter, mutton suet, pork fat, or any other fatty body. On examining the change which had occurred, Bernard determined, by chemical means, that the fat is decomposed into glycerine and a fatty acid. This result is most apparent with butter, the acid of which (butyric) is easily recognised by its characteristic odour. No other liquid produces such a reaction with fat; in proof of which assertion, Bernard has examined bile, saliva, gastric juice, serum of the blood, cerebro-spinal fluid, and semen; in none of these was the emulsion persistent. The power of forming an emulsion with fat, would appear to be proportionate to the amount of organic matter in the pancreatic juice (p. 262). 3*rd*, When the two pancreatic ducts of a dog are tied, Bernard remarked (p. 270), that the fat remained unaltered in the small intestine, and that the lacteals contained a chyle very opaline and poor in fatty matters. 4*th*, When the pancreas is destroyed by injecting into its ducts liquefied fat, mercury or other substances, the animal, four or five days after the operation, eats with extreme voracity; and if fatty matters are given to it, these are found in great abundance in its excrements. Emaciation gradually occurs, and the animal at last dies in a state of complete marasmus, presenting to the last an extreme voracity, and fatty excrements. It might be objected to these experiments, that fat was given so abundantly that it escaped digestion; but, to refute such an argument, Bernard fed two dogs with a similar amount of fat, and in one of them obliterated the pancreas by injection of its ducts. A comparative examination of the excrements, revealed, in the dog which was operated on, an abundance of fat, whilst in the other the fœces were quite natural (p. 290). We think these experiments would have been much more satisfactory had Bernard made careful calculations of the amount of fat given, and of the amount found in the excrements. Bernard follows up these experimental proofs by the narration of cases of disease of the pancreas, being accompanied symptomatically with the presence of fat in the stools.

One would imagine that the truth of a theory, apparently so strongly supported by experimental and other evidence, could scarcely be doubted; but like all other scientific doctrines, it has had an apparently overwhelming host of objections brought against it. In reply to the criticisms made by Frerichs[1] and Lenz,[2] Bernard has shown that their experiments were faulty, from their ignorance of the fact, that there are *two* pancreatic ducts; and that, when they supposed they had cut off the access of pancreatic fluid to the fat, by ligature of a duct, another existed by means of which the pancreatic secretion could easily find its way into the intestine. To those who have found that, after the administration of fat, white lacteals may be observed *above* the entrance of the pancreatic ducts, Bernard replies that this may be due either to the reflux of pancreatic fluid, or to the presence of certain small glands in the wall of the intestine, which have a function similar to the pancreas. We do not think that the proofs of the function of these glands are very satisfactory. The result of Bernard's experiments on the action of the pancreatic fluid upon fat, has been confirmed by Bidder and Schmidt,[3] but their experiments lead them to suppose that, *within the organism,* the acid gastric juice, through its free acids, prevents the emulsionising influence of the pancreatic fluid upon fat. The most recent observations on this subject are those of Colin[4] and Berard,[5] whose experimental investigations lead them to the conclusion that the pancreas is not necessary for the digestion of fat. Schiff has arrived at a similar conclusion from pathological evidence. When we bear in mind the result of recent researches on the action of the bile, and more particularly the experiments of Bidder and Schmidt, we feel some hesitation in admitting that the pancreatic is the *only* secretion destined to promote the absorption of fat; but we feel fully warranted in accepting so far the conclusion of Bernard, that, from its mechanical, if not from its chemical influence, it is to be regarded as having a decided influence in this respect.

In chap. xii., Bernard takes up the question of the absorption of fat by the intestinal villi, but he merely gives the views of Goodsir, Gruby, Delafond, and Brücke, not venturing himself to express an opinion upon this difficult and much-disputed question. Whether the fat enters by an opening in the epethelial cell, and then courses along a canal connecting the epithelial cell with corpuscles of connective tissue, which again are continuous with the lymph vessels (Heidenhain); or whether there are no such actual and constant channels for the passage of the fat except the central canal, the rows of fat globules seen at the points of the villi, being merely caused by its presence in the intercellular and non-walled passages (Funke), are questions which it would require the finest microscopical investigations to determine. We do not think that facts so often alluded to by Bernard, of the mechanical or emulsifying and the chemical decomposing influence of the pancreatic juice, at all explain the mechanism of the ab-

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[1] Frerichs, *Art. Digestion in Wagner's Handworterbuch der Physiologie,* 1849.

[2] Lenz, *de adipis concoct. et absorptione. Dorpat,* 1850.

[3] Bidder and Schmidt, *die Verdaungsäfte und der Stoffwechsel,* 1852.

[4] Colin, *L'Union Médicale,* t. xi. No. 50, 1857.

[5] Berard and Colin, *L'Union Médicale,* vol. xii., No. 8, 1858.

sorption of fat. At p. 320, when speaking of the elaborated chyle, he remarks :—

"But if one seeks chemically for the presence of glycerine, or of a fatty acid, it is impossible to find them in it. It would appear that the fat we find in the chyle has undergone a physical, but does not seem to have suffered any chemical alteration."

And a little farther on he says :—

"There is, then, in the intestine, a species of neutralisation of the fatty acids. *It is probable that this part is played by the bile.*"

From the latter remark, it would appear that, for the absorption of fat, Bernard considers *something more* than the influence of the pancreatic juice is requisite; and if we are right in this conclusion, we only feel surprised that he has not paid more attention to the fact, and that he has so exclusively regarded the agency of the pancreatic fluid in the absorption of fat. Upon the agency of the bile in the mechanism of fat absorption, our readers will perhaps excuse us giving an account of the very interesting researches of Wistinghausen, prosecuted under the direction of Bidder and Schmidt; and we do this the more readily, as Bernard's work is extremely meagre in the consideration of this subject.

Wistinghausen commenced his researches by a series of experiments on endosmosis, in which he sought to determine the amount of pressure requisite to propel a given fluid through an animal membrane. He found that oil required a much greater pressure than water, and a still greater pressure was requisite if the membrane be saturated with water; that solution of potash went through more easily than water; that a solution of bile passed as easily as water, but when mixed with albumen, with greater difficulty. The cause of this difference in the facility of penetrating membranes, Wistinghausen sought for, in the greater or less chemical affinity existing between the fluids employed and the animal membrane. He sought for the affinity of bile in an affinity of the same to albumen; when, however, albumen was added to the bile, the attraction became less. If upon one side of the membrane he placed a fluid which had an attraction for oil, such as solution of potash, the oil entered through very easily, from its tendency to form soap. Oil passed, however, easily through the membrane when on one side was placed an emulsion of oil and bile, and on the other side albumen. From these experiments Wistinghausen seeks to explain the process of fat-absorption with the aid of bile, in the following manner. Every animal membrane, which permits of endosmosis, necessarily contains an innumerable quantity of the finest invisible pores, through which fluids penetrate; we can represent them as a system of short, infinitely fine capillary tubules. The bile has a chemical affinity to the substance of the tube wall, penetrates the tube, since it is attracted by the walls, and forms in each a column of fluid. The fat has a mechanical affinity to the bile, (for it is emulsionized by it), and is drawn into the column of fluid which fills the endosmotic pores; and thus penetration of the membrane is effected.[1] Such an explanation, although, no doubt, theoretical, is based upon physical facts and reasonings, and is the best that has been offered to explain the phenomena of fat-absorption. That the bile has a share in the digestion of fat, is proved by the careful researches of Bidder and Schmidt, which show that when bile is excluded from the intestine by ligature of the ductus choledochus, the amount of fat absorbed is five or seven times less than in the normal condition. Bearing this in mind, we must be very careful in accepting Bernard's proposition, that the pancreatic fluid is the *only* one concerned in the digestion of fat, although to him is undoubtedly due the credit of having enunciated strongly the fact that the chyle is nothing more than a mixture of lymph and fat. In regard to their mode of absorption, he now distinguishes two groups of the products of digestion; 1st, the saccharine and albuminous absorbed exclusively by the portal vein, and necessarily traversing the liver before arriving at the lung; 2nd, fatty substances, absorbed by the chyliferous vessels, and arriving in the general system and the lung, without having previously passed through the liver," (p. 325).

The latter statement is not quite correct; for if one examines the blood of the portal vein, either chemically (Lehmann), or microscopically, a certain amount of fat may always be found, to be employed, no doubt, in the formation of bile.

2d, In regard to its action upon starch, Bernard attributes to the pancreatic juice the principal share in the conversion of this substance into sugar. He says, that when the pancreas is destroyed, and the animal fed upon starch, only a very small portion is digested, and it is passed with the excrements, just as it had been taken, (p. 329.)

Now, in the normal condition, the excrements of animals which have been fed upon starch, contain it, quite unaltered: this we have observed in rabbits; and, if such is the case, Bernard's experiments are almost valueless, as they do not contain exact quantitative analyses of the amount found in the excrement of an animal whose pancreas has been removed, compared with that found in the excrement of a perfectly healthy animal. No one will venture to doubt that the pancreatic secretion has great power in transforming amylaceous into saccharine principles; but that this function is exclusively possessed by it, we do not believe. In speaking of the saliva, we have already referred to experiments, proving that it possesses a similar property.

3d, The pancreatic juice possesses still another important function, that which it exercises upon azotised matters. " When one places raw flesh in contact with pancreatic juice, it softens it considerably, but putrefaction soon invades it. But if this action is tried upon the same matters, after they have been boiled or digested by the gastric juice, the result is quite different, and there is a real solution of them," (p. 334).

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[1] Wistinghausen, *Experimenta quædam endosmot. de bilis in absorptione adipum neutr partibus. Dorpat,* 1851. Funke's *Lehrbuch der Physiologie*, p. 315.

This action upon azotised matters was first noticed by Purkinje and Pappenheim,[1] and it has recently been made the subject of a memoir by Corvisart.[2] The latter observer affirms, that the digestion of azotised matters is common to the stomach and the pancreas—the latter being a sort of supplementary organ to the former—and the digestion of both being alike, in the sense that each transforms the azotised matters (gelatine, cellular tissue, albumen, fibrine, caseine, musculine,) into a digestible product exactly similar (albuminose or peptone). When an azotised matter has undergone completely the gastric digestion, the pancreatic juice does not exercise any action upon it, and does not transform it into another peptone; since it is only upon the albuminoid substances which have left the stomach, before they have been transformed into albuminose, that the pancreas acts. The opinion of Bernard, that the pancreas likewise digests azotised matters, would thus appear to be strongly supported by the elaborate chemical investigations contained in the work of Corvisart. We feel surprised that, whilst Bernard has devoted whole chapters to the action of this secretion upon fatty matters, he should discuss its other functions, apparently equally important, in the course of half-a-dozen pages. In concluding his lectures on the pancreas, he remarks:—" We conclude, then, that the pancreatic juice is not a special liquid, acting only upon one class of aliment—*it is destined, on the contrary, to act upon all kinds of food*," (p. 334).

M. Bernard devotes the remaining lectures to a consideration of the successive action of the digestive fluids, dwelling more particularly upon those which he has not hitherto taken up. Let us, in conclusion, follow him through this *resumé*.

After repeating his opinion, that the saliva does not exercise any chemical influence upon the food, but that it has merely a mechanical part to play, Bernard narrates the history of the gastric juice, the mode of making fistulæ, (Blondlot's), and the results of chemical inquiries into the nature of this fluid. The experiments made by him and Barreswil upon dogs, agree in shewing that the much-questioned free acid is, in these animals, at least, the lactic, (p. 393). As to the action of the gastric juice upon the food, Bernard is inclined to give it a very insignificant part to fill. He says, in speaking of the effects of the juice out of the body, (p. 400), " on fatty matters in the liquid state, it has no action; but on those which are contained in fatty vesicles, the juice dissolves the walls of the cell, and permits the fat to escape in the fluid state; thus the gastric juice fluidifies fat, but it has no further action on its physical and chemical qualities." In regard to starch, " when it is contained in vegetable cells, as in potatoes, the gastric juice dissolves the azotised walls of the cells, and the grains of starch are set free, and placed in direct contact with the gastric juice," (p. 401); here "the starch is physically altered, by becoming hydrated, but

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[1] See BURDACH'S *Traité de Physiologie traduit par Jourdan*, vol. ix., p. 317.
[2] CORVISART, *Sur une fonction peu connue du Pancréas.* 1857–8.

it is not chemically altered; sugar is only affected by prolonged contact, and then cane sugar may be reduced to sugar of starch, and the latter may be changed into lactic acid by a prolonged contact; azotised elements, such as flesh, are softened and reduced to a kind of grey paste; but if you examine the effect microscopically, you will find that it is only the cellular tissue uniting the muscular fibres which has been dissolved, but the fibres themselves are not, (p. 402): boiled albumen is re-dissolved, milk is coagulated, the azotised parts of bone are dissolved, and the earthy parts disassociated." Excluding, as he does, every action of the saliva, the result of gastric digestion is pretty much the same in the stomach as it is out of it, (p. 416): and its special use is considered by Bernard to be that of *preparing* the food for a true digestion in the intestine, by a process very similar to boiling. "The proof that the action of the gastric juice dissolves a gelatinous matter between the true tissues, is, that on taking the animal matters contained in the stomach of a dog during digestion, as raw or cooked meat, on moistening them with water, and throwing the whole upon a filter, you obtain a clear, transparent liquid, less acid than the gastric juice, and becoming gelatinous upon cooling. This action takes place, especially when you diminish the acidity of the gastric juice, which hinders, as you know, gelatine from becoming solid on cooling. Thus, in *résumé*, the gastric juice has for its object the solution, in azotised food, of those animal matters capable of affording glue or gelatine by their solution; and we see that ebullition produces exactly the same effect; so that, as regards the general action which the gastric juice appears to exert upon all alimentary substances, it is that of causing them to undergo the action which prolonged boiling produces," (p. 418).

We are somewhat astonished to find Bernard ascribing to the stomach so very insignificant a part in the digestive process, and have very great doubts as to the correctness of his opinion, that it is only on the gelatiginous tissues the gastric juice acts. Indeed, we have some difficulty in reconciling this opinion with a statement he himself makes at p. 408, where he says, "I have seen, in animals killed during digestion, the gastric juice destroy the stomach, the half of the liver, the spleen, and part of the intestines, provided the precaution has been taken to keep the animal in a stove, at a temperature corresponding to that of the animal." If the gastric juice only digests the cellular tissue, how was such an extensive destruction accomplished? The majority of physiologists (Lehmann, Bidder and Schmidt, Frerichs, Funke, and Corvisart) are of opinion that the gastric juice, by virtue of the combined action of its acid (lactic) and of its organic body (pepsine), digests a large part of the azotised food, *i.e.*, that it not only dissolves it, but so changes its constitution, that in the new products (Albuminose, Mialhe; Peptones, Lehmann), certain characters are presented which were not present in the matters before digestion. According to Lehmann, the principal changes are, that the new product or Peptone is not coagulable by heat or mineral acids, and forms insoluble compounds with the metallic salts. From the recent investigations of a Dutch observer (Cnoop Koopmans), it would appear that, as regards the effect of this secretion upon vegetable albuminous substances, the poorer it is in acid, the more powerfully does it act on gluten and other vegetable azotised matters; whereas Bidder and Schmidt have found that the digestive power is commensurate with the amount of acid present in the juice. These apparently contradictory opinions may be reconciled by the fact that the different proteine compounds show different degrees of resistance to the action of the juice, and that it is probable that in different animals the acidity of the juice is in close relation to the kind of food adapted for their nutrition. Any one may convince himself that Bernard is wrong in supposing that the gastric juice has no action save on the cellular tissue, by simply placing in artificially prepared gastric juice, some pieces of coagulated white of egg, blood fibrine, or muscular tissue. These gradually become transparent, gelatinous, and at last dissolve, and by precipitation with alcohol, their digested products or Peptones may be obtained.

The first fluid concerned in the intestinal digestion is the bile, and to it, accordingly, Bernard next refers:—

"In acting on fatty matters, which go out of the stomach, the bile does not produce in them any apparent modification. Amylaceous matters suffer no modification by the action of bile alone, only it becomes difficult to recognise the presence of starch with iodine, in the presence of bile. *It is particularly upon the azotised albuminous matters that the bile appears to act* in an evident manner; *there is a precipitate of the azotised matters* which have been dissolved, so that at the entrance of the duodenum, all the alimentary matters are insoluble."—P. 430.

"Thus we can say that there are two digestions: one, the gastric digestion, which is but preparatory: the other, the intestinal digestion, which is definitive. *The action of the bile is interposed between these two digestions, arrests the stomach digestion, in order to permit the intestinal digestion to commence.*"—P. 432.

Some pages further on, he speaks of the necessity of the operation of the bile before that of the pancreatic juice, and then almost immediately after, he avers, that the latter cannot operate on azotised matters unless mixed with bile, and that this mixture of bile and pancreatic juice constitutes a new liquid endowed with properties entirely special to it. To this bilio-pancreatic liquid Bernard attributes a digestive omnipotence: it digests azotised, fatty, and amylaceous matters, only it is necessary that these have been previously prepared, *i.e.*, that is, that the fatty matters be previously deprived of their envelope, that the amylaceous matters be hydrated, and that the albuminous matters be modified by an action similar to that which the gastric juice effects (p. 443). We should have observed that, in the duodenum, Bernard distinguishes two classes of glands: 1st, the glands of Brunner, situated at the commencement of the gut, just beyond the pylorus, furnish a viscid, gluey liquid, which appears to have much analogy with the sublingual saliva; this liquid is immediately mixed with the gastric juice and the bile, so that it is impossible to determine its action; 2nd, other glands situated around the orifice of the pancreatic duct, which act like the pancreas in emulsionising fat, and converting starch into glucose, (p. 427.)

In regard to the action of the bile, there appears to be some degree of confusion in Bernard's work, for whilst at one part he speaks of the necessity of its acting prior to the pancreas, in precipitating azotised matters, a few pages further on, he speaks of it as acting conjointly with the pancreatic fluid, and forming a mixture, having the special and peculiar property of digesting everything. Also, in relation to the principal chemical observation he has made regarding the action of the bile, that of precipitating albuminous matters already acted on by the stomach, he says, at page 422, "this precipitate is not formed in gastric juice, the secretion of which is excited in a fasting animal:" at page 420, he would seem to contradict this positive statement by saying, "However, it is necessary to know if some of the characters which one attributes to a digested matter, do not proceed directly from the gastric secretion;" and at page 423, "that would seem to indicate that the bile precipitates pepsine."

This important point (the precipitation of digested matters by the bile) has been most carefully investigated by Corvisart, and the result of the numerous experiments he has instituted is, that it is the bile itself which is precipitated by the gastric juice or the acid chyme;[1] the same opinion is entertained by Funke.[2] The function of the bile has ever been a much vexed question, and in Bernard's work we do not find any nice analytical investigations which tend to demonstrate the part which this fluid plays in digestion. No facts are given to show in what particular it modifies the action of the pancreatic fluid; we are merely told that when pancreatic juice and bile are mixed, a compound is formed which digests every variety of food. This is not saying more than has been already said in regard to the pancreatic fluid, to which, it will be remembered, Bernard attributes the power of digesting fatty, amylaceous, and azotised substances. The action of the bile in digestion still remains, to a certain extent, an unsolved problem. The most trustworthy experimental researches (those of Arnold, Bidder and Schmidt, Kölliker and Müller), prove that abstraction of bile through a fistula produces gradual emaciation, and, after a considerable period, death; that if, in such circumstances, the amount of nutriment be very much increased, emaciation does not occur, and the loss of bile is, as it were, compensated for, the animal gaining in weight, *not by fat, but by increase of muscle*, sometimes by feeding, the loss of bile may even be over-compensated (Kölliker). Now, when we remember that the greatest part of the bile re-enters the blood of the portal vein, this loss of weight and emaciation, and the effect of a compensating increase of diet is explained, but the share which the bile has in digestion, in its interim residence in the intestine is not rendered more intelligible. On this point the experiments of Bidder and Schmidt, and Wistinghausen, already referred to, in

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[1] Corvisart, p. 30. [2] Funke, Bd. I., p. 278.

speaking of fat-absorption, throw more light, and render it extremely probable, that the presence of bile assists absorption of fat, not by any chemical action, nor by forming an emulsion (which seems, to Bernard to be quite a satisfactory explanation of the mode in which the pancreatic fluid assists the absorption of fat), but by facilitating endosmotic action.

In concluding this somewhat lengthy exposition of "Bernard's Lectures upon Digestion," we willingly forbear presuming to express a general opinion upon the work of a physiologist of his high standing, but we may be allowed to express our high appreciation of the value of such experimental inquiries, as those we have just reviewed. It is to such investigations as these, that physicians must look for the attainment of a satisfactory and philosophical basis for the medical art; and we feel convinced that, until these physiological questions are settled, pathology and practice can never attain a secure footing. This end can be reached only by the cautious physical and chemical analysis of the normal and abnormal phènomena observed in living animals, and though the investigation be slow and laborious, we feel certain that it forms the true path to brilliant and enduring results.

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