Brown-Séguard, with the effect of demonstrating that a section of the anterior columns does not produce paralysis of voluntary motion, nor section of the posterior columns prevent conduction between the brain and posterior roots. To produce either of these results, the section must be continued into the grey matter. If two sections be made, however, midway between two neighbouring spinal nerve roots, then conduction between the parts above and below the sections is cut off. The explanation of this is to be found in the course taken by the nerve tubes as shown by Lockhart Clarke, which so diverge from one another, on passing into the cord, that no one transverse section of the column can divide them, although two at a certain distance from one another may. Thus histological research and experimental investigation support one another, and the two have now demonstrated that the conducting nerve tubes of the spinal roots of the nerves communicate through the grey matter of the cord, not only with the brain and the two sides of the body, but with each other.

These facts have served also to explain more fully the nature of those actions variously denominated automatic, reflex, and diastaltic, for the true knowledge of which we are indebted to the labours of Marshall Hall. It is now clear that the influences excited by irritation of nerves run continuously through the cord in certain directions, now communicating with muscles to produce spasms, and now with the glands and vessels to produce secretion and vaso-motor action, and this without any necessary connexion with the brain, and therefore without sensation.

The ganglionic system of nerves consists of numerous ganglia having connecting filaments, keeping up a communication with each other and with the cerebro-spinal centres. These communications are not direct, the various nerve tubes separating in a ganglion, and, whether they do or do not anastomose with nerve cells, on leaving it form a different combination of nerves. Hence every ganglion serves to break the conducting power of the nerves, or to modify it—probably both. In health we are not conscious of the actions of internal viscera principally supplied with these nerves, nor can volition act on muscular parts to which they are distributed. But let them be diseased, and they often excite excruciating yet peculiar pain, as in that caused by angina, by colic, or by a gall-stone. Again, mental emotions have a powerful influence on the contractions of the organic contractile tissue, as in palpitations of the heart, or as visible in bloodvessels on the production of pallor or of a blush. In the same manner mental emotions and desires act on the various glands, exciting or diminishing their action. Such results can only be explained by the connexion known to exist between these nerves and the spinal cord. The same phenomena may be produced by direct stimulation or by reflex action, each ganglion being a centre through which afferent and efferent nerves communicate,—the whole constituting an excito-nutrient and excito-secretory system, as has been well described by Dr. Campbell of the United States.*

The observations as to the effects of injuring the trunk of the ganglionic system, more especially in the neck, have excited the attention of numerous physiologists since the days of Petit in 1727, and more especially of Dupuy (1816), Breschet (1837), John Reil (1838), Biffi (1846), Budje and Waller (1851), and finally of Bernard and Brown-Séguard (1852). Bernard discovered the remarkable increase of heat which followed section of the nerve, and Brown-Séguard showed that cold was produced on applying galvanism to it. It is now recognised that if we cut the trunk of the sympathetic, heat commences in the neighbouring parts almost immediately, and will continue for weeks without producing inflammation, cedema, or other effect, so long as the animal remains in good condition. But if it falls sick, either spontaneously or in consequence of other operations, the nasal and ocular mucous membranes of the affected side become red and swollen, and secrete pus in great abundance. The inflammation of the conjunctiva described by Dupuy, J. Reid, and others, is therefore an accidental phenomenon produced by the debility of the animal, and may be avoided by giving it food and supporting its strength.

Thus in the same manner that when we irritate a sensitive nerve we excite motion through a motor nerve, or secretion and nutrition through the nerves distributed to glands or to the tissues, so we excite cold by irritating the ganglionic system of nerves, and heat by destroying their action or exhausting it. These phenomena are those of fever.

Doctrine of fever.—Fever is characterized first by diminution of temperature, and a feeling of cold succeeded by augmenta-

tion of heat. In intermittent fever this occurs rapidly, the whole phenomena at the longest occupying perhaps about six hours. In continued fevers it may last weeks. The effects of irritants on nerves are, first to excite, and then to suspend function. The first effect of the poison producing irritation in fever is to cause contraction of the vessels, palor and cold; subsequently it induces relaxation or paralysis of the vessels and heat. That is, a less amount of blood in the capillaries gives rise to a diminished exchange of chemical elements between it and the tissues, thereby occasioning loss of temperature, and a greater amount of blood increases this exchange, and elevates the temperature. We may therefore conclude that palor and a feeling of chill or rigor is indicative of excited or increased action of the organic nerves distributed to the vessels, whereas redness and a feeling of heat is a proof of diminished action in the same nerves. The poison therefore which enters the blood to produce fever, does so first by irritating, and subsequently impeding the functions of the ganglionic system of nerves. In the same manner that various poisons act on different portions of the nervous system, some on the brain, some on the spinal cord, and others on particular nerves, so the fever poison acts on the excito-nutrient and excito-secretory system. It resembles the operation of a narcotic in first stimulating or causing cold, and subsequently depressing or producing heat. Its influence also, when directed with greater force towards the cerebral, thoracic, or abdominal viscera, may serve to explain the peculiar forms and varieties of fever.

That certain eruptive fevers are communicable admits of no doubt; and it has been proved that the smallest particle of the virus may propagate in others its peculiar specific effects. Thus one drop of cow-pock matter rubbed down with an ounce of finely levigated sugar has given to the entire mass the property of communicating the poison in very small quantities. The same result has taken place by using the scab instead of a drop of purulent matter. Whether every minute granule of sugar under such circumstances is equally potent, or whether those granules only retain the power of communication as have attached to them a minute molecule of the morbid poison, has not been ascertained. Such propagating particles must be very minute, and capable of attaching themselves to rough substances, of floating in the air, or mixing with water. This is certain, that it can only be by a *molecular theory* of germs that the well-known facts connected with the increase and extension of epidemics can be accounted for. The action is a molecular action analogous to that of a ferment.

Action of quinine.—That quinine is a tonic I have long had reason to doubt. Quantities of this valuable drug appear to me to be annually wasted in administering it in all sorts of affections as a simple means of increasing appetite or communicating strength. I consider it a vegetable alkaloid, which, like others of its class, operates on special parts of the nervous system through the bloodvessels. Why it does so we are ignorant. That vegetable poisons do possess this influence constitutes an ultimate fact in the science of therapeutics. But in a similar manner to that in which opium operates on the brain, strychnine and hemlock on the spinal cord, and other drugs operate on other parts of the nervous system, so, I believe, quinine acts on the ganglionic system of nerves, controlling and diminishing those phenomena of fever which physiology has proved to be produced by their irritation or injury.

ON ASSIMILATION AND THE INFLUENCE OF ITS DEFECTS ON THE URINE.*

*Being the Gulstonian Lectures of 1862, delivered at the Royal
College of Physicians, London,*

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SEEING, whilst conducting some experiments upon artificial digestion, that I got no effect from a specimen of commercial pepsine that I employed, I was induced by curiosity to ascertain the general character of the pepsine sold in our metropolis. I therefore personally procured pepsine from establishments in different parts of London—establishments that I considered specially entitled to our confidence from their old-standing and high

* Essays on the Secretary and Excito-Secretory System of Nerves, &c. Philadelphia: 1857.

* Continued from vol. ii. 1862, page 615.

repute for respectability. According to the test I have employed, which I believe to constitute the most delicate test that can be used, only one out of the several specimens I obtained was possessed of any perceptible digestive property. I am bound, in fact, to say, from the evidence before me, that I have only yet met with one specimen of commercial pepsine that has been of any more digestive value than so much starch powder, which, be it observed, may be procured at a much lower cost when purposely purchased as such. Starch powder is, indeed, frequently, I might state generally, used as a diluent; but of this there is nothing to complain, so long as the price is regulated accordingly, and the preparation is endowed with efficient digestive properties.

Here are the results I have obtained on experimenting with the specimens of pepsine to which I have been alluding. I will remove the frogs' legs, which have labels attached to them to indicate the source of the pepsine employed. These shall be handed round, so that each may have an opportunity of examining closely the condition they are in. It may be desirable at once to state, that in all these experiments the external circumstances were identically the same. The bottles as you see them, with their liquid and suspended frogs' legs, were all introduced together into a water oven, the temperature of the interior of which was maintained at 110° Fahr., as shown by the use of a thermometer. At the end of four hours all the bottles were removed, and their contents were found in precisely the same condition as that in which they now appear.

Pepsine of Boudault procured from Mr. Squire's establishment. Dose stated to be fifteen grains. Sixty grains taken, and treated with two ounces of water and forty minims of strong hydrochloric acid. The frogs' legs present no evidence of digestive action.

Pepsine from Messrs. Savory and Moore. Dose stated to be ten to fifteen or twenty grains. Sixty grains taken, and treated with two ounces of water and forty minims of hydrochloric acid. The frogs' legs present no evidence of digestive action.

Pepsine from Mr. Morson. Here I was supplied with two specimens of pepsine. One was furnished to me as pure pepsine, or pepsine in the state obtained direct from the stomach of the calf; the other, the same diluted with starch. Twenty grains of the pure pepsine and forty grains of the diluted were taken, and treated each with two ounces of water and forty minims of hydrochloric acid. The frogs' legs in both instances offer no evidence of digestive action.

Pepsine from Messrs. Allen and Hanbury. English calf pepsine. Dose fifteen to twenty grains. Sixty grains were taken, and treated with two ounces of water and forty minims of hydrochloric acid. The frogs' legs present no evidence of digestive action.

Pepsine obtained from Messrs. Bullock and Reynolds. This I was informed was procured from the stomach of the pig, and the dose was stated to be from two to four grains. Twenty grains were taken, and treated with two ounces of water and forty minims of hydrochloric acid. The frogs' legs afford evidence of having been most energetically attacked, the portions immersed in the fluid being almost completely dissolved away.

In the experiment with the artificial gastric juice prepared in my own laboratory from an infusion of the dried mucous membrane of the pig, which I used in an early part of the lecture when speaking upon the nature of gastric digestion, the effects that you witnessed were produced by exposure for the same length of time in the oven alongside these bottles, containing the different specimens of pepsine to which I have just been referring.

At Apothecaries' Hall I was informed that only the pepsine obtained from Mr. Squire was kept. As I had already procured some of this direct from the person named, I did not think it necessary to experiment with that derived from a secondary source.

I may mention that with all these specimens of pepsine I have experimented before. The same quantities were taken, but only treated with an ounce instead of two ounces of water, and twenty minims instead of forty minims of hydrochloric acid in each case. The temperature ranged between 112° and 115°, and an exposure as long as eight hours was given. With Mr. Bullock's pig-pepsine the frog's leg was so much attacked at the end of two hours and a half as to be eaten through at the surface of the liquid; the muscles and bones also were thoroughly dissected out. With all the other specimens, at the end of the eight hours, there was not the slightest destruction of any portion of the surface, but the leg looked soddened; and when pinched with the forceps, easily gave way. The bones were also soft and flexible. An acidulated infusion

of the small intestine of a dog, however, had been experimented with at the same time, and the effect on the frog's leg was identically the same, the limb being swollen and sodden, and easily crushed with the forceps; the bones also were soft and flexible—an effect doubtless produced by the presence of the acid.

Now pepsine is an agent which, from what I can hear, has a most extensive—an increasing demand; and it is at the same time only to be purchased at considerable cost. Having witnessed the results that I have disclosed, I did not think it right to keep the information to myself, and allow the members of the medical profession to continue blindly prescribing as a remedy a material, according to my test, so completely devoid of really effective properties, as, with only the single exception, all the specimens of pepsine that I have examined have proved. As physicians, in our combat against disease we do not personally handle or systematically test the efficiency of the weapons we employ. We know what ought to be the properties of our various instruments; but if these instruments should prove false, how is it to be expected that they can stand when real service is required of them. Looking to the dependence we have to place upon the proper efficiency of the drugs we prescribe, really the position we occupy cannot be regarded, upon reflection, as otherwise than an anomalous one. I cannot learn that there is any system at the present day in operation by which, as a body, we take interest in securing the proper efficacy of that which, with our reputations at stake, we order to be dispensed to our patients. In our relation as a professional body with the public, we ought not, I hold, to consider our duty at an end when the prescription is given. We are looked upon as responsible for the result in the conduct of our case. We investigate and find out the nature of the complaint, and afterwards order the appropriate treatment. But what is the use of all this if we take no concern towards securing the proper efficiency of that which is supplied. A want of proper efficacy in the materials dispensed may bring upon our shoulders as much discredit amongst the public as an error in diagnosis, or a mistake in treatment. As we labour amongst ourselves to improve the science of our art, so ought we not to forget that for the successful practice of our art something besides attention to science is needed to give our profession the status it ought to enjoy. How, for instance, it might be justifiably asked, could anything but faith, implicit faith, on the part of our patient, have effected any good, where specimens of pepsine such as these, whose want of digestive power we have seen, were prescribed?

In these remarks that I have made I wish it to be distinctly understood that I do not for a moment impute an improper motive as having led to the state in which, with the one exception, all the samples of pepsine I have procured were found. The well-known respectability of the establishments whence these samples came is a sufficient guarantee that their ineffective state was as unknown there as amongst the members of our profession who may have been prescribing them. Such slight causes are sufficient to lead to the destruction of the digestive principle, that the production unwittingly of a useless material is easily to be accounted for.

I have lately been conducting some experiments on this point which show how easily the digestive power of pepsine may be lost. Two specimens of an infusion of the dried pig's stomach, possessing properly active properties, were taken, and the temperature of the one raised until it reached 130° and the other 140°. They were afterwards acidulated with the usual quantity of hydrochloric acid and exposed for four hours alongside each other in the water oven, at a temperature of 110°, a frog's leg having been introduced into each. In the specimen the heat of which had only been raised to 130° the frog's leg was digested away. In the other, the skin and muscles were only very slightly acted on at the surface.

A rich infusion of the mucous membrane of the stomach was, in another case, taken with the view of drying in the vacuum of an air-pump. A considerable time elapsed before the drying was complete, and there was evidence, during the process, of putrefaction taking place. Eight and a half grains of the dried extract left were treated with one ounce of water and twenty minims of hydrochloric acid. A frog's leg was introduced, and exposure to a temperature of between 112° and 115° given. At the end of five hours there was no effect externally perceptible; at the end of eight hours the muscles were found exposed and dissected out, but the form of the leg remained. In another instance, however, where an infusion of the stomach had been under the receiver of an air pump for a fortnight, and had been reduced to a viscid state, although smelling pretty strongly from decomposition, it still manifested signs of possessing energetic digestive properties.

According to my experience, the easiest way of procuring an artificial digestive solution is to simply dry the stomach of an animal, and make an infusion of it whenever required, treating it with the requisite amount of acid. Where the stomach is thin, it may be cleansed with water and taken entire; but where, as in the case of the pig, the stomach is exceedingly thick, I strip away the mucous membrane from the muscular layer, and, spreading it out, allow it to dry spontaneously, or at a suitable distance from a fire. Cut into shreds, it may be preserved in a convenient form for use. Upon one occasion I experimented with different quantities of the dried mucous membrane thus obtained from the stomach of a pig. Twenty, ten, and five grains were taken, and treated respectively with one ounce of water and twenty minims of hydrochloric acid. Frogs' legs being introduced, they were all exposed together to a temperature of 110°. At the end of an hour and three-quarters, the twenty- and ten-grain specimens had produced strong marks of action upon the skin. In three hours, all three specimens had led to the muscles being dissected out and the bones exposed. In eight hours all the legs were amputated at the surface of the liquid, and, with the exception of a few small fragments, had entirely disappeared. The quantity of pure pepsine, then, required for digestion is very minute.

Before I finish with the stomach as connected with primary assimilation, there is one other point I cannot refrain from touching upon—the immunity from digestion which the organ itself enjoys during life: a subject, I believe, that could not elicit from us a more profound consideration than it really deserves. The position is this:—The stomach receives in its cavity materials constituted like its own parietes. The materials are dissolved or liquefied, whilst the stomach itself remains intact. What, it may be asked, is the explanation of such an elective result? A vessel is used holding a material identical with itself whilst a process of solution is being accomplished: the one is dissolved, and the other is not. For instance, say a vase of marble has fragments of marble placed in it to be dissolved by an acid, and the fragments disappear, whilst the vessel remains intact. What can be brought forward satisfactorily to account for the immunity which, under such circumstances, the stomach enjoys? It was our countryman, John Hunter, who first directed attention to this point. In his communication published in the "Philosophical Transactions" for 1772, entitled "On the Digestion of the Stomach after Death," he remarks that in occasional instances, especially in persons who have died of sudden and violent deaths, on making an inspection of the body, the great extremity of the stomach is found so dissolved as to have allowed of the escape of its contents into the abdominal cavity. He also notices that, although a perforation of the stomach may not be observed, yet there are very few dead bodies in which the stomach is not at its greater end in some degree digested; this partial digestion being indicated by a thinness and transparency of the membranes, and by the blood which the vessels contain passing out at their digested ends when pressure is made from the larger towards the smaller branches, and appearing like drops on the inner surface.

The greatest extent of solution being observed in cases where the individual had been in the enjoyment of perfect health up to the moment of death, Hunter inferred that the phenomenon could not be the result of disease during life, but must be owing to an action exerted by the digestive fluid after the occurrence of death. If, then, the stomach were thus susceptible of being digested by its own secretion after death, the question immediately arose—What afforded it immunity from undergoing the same process of digestion during life? To Hunter it was the "living principle" that formed the required source of protection. To use his words, "The stomach, which at one instant—that is, while possessed of the living principle—was capable of resisting the digestive powers which it contained, the next moment—namely, when deprived of the living principle—is itself capable of being digested, either by the digestive powers of other stomachs, or by the remains of that power which it had of digesting other things." In illustration he says, "If it were possible for a man's hand to be introduced into the stomach of a living animal and kept there for some considerable time, it would be found that the dissolvent powers of the stomach could have no effect upon it; but if the same hand were separated from the body and introduced into the same stomach, we should then find that the stomach would immediately act upon it."

I have before me preparations of human stomachs belonging to the museum of Guy's Hospital, where considerable-sized deficiencies exist, the result of gastric solution. The same con-

dition is easily produced artificially with the lower animals. Under ordinary circumstances, although it is common to find the inner surface at the cardiac extremity bearing more or less evidence of attack by digestion, yet I think I have never witnessed that an actual perforation has taken place. If, however, when an animal has been killed during digestion, the temperature be afterwards maintained artificially, the result is far otherwise. With this stomach, which has been derived from the pig, the animal was killed whilst the organ was full, food having been purposely administered a few hours previous to death. The stomach, being simply removed with its contents, was suspended, by means of a string tied around its oesophageal and pyloric openings, in a large preparation glass, and exposed to a temperature of about 100°. Its contents were found some hours afterwards at the bottom of the glass, and upon looking at the organ itself there is an enormous rent which was produced by gastric solution.

I have here also a rabbit, in which the most extensive ravages have been produced by the digestive action of the gastric juice. The animal was killed about four hours after the ingestion of food. Its body was then placed in a warm situation, so that its temperature was maintained for several hours (about ten or twelve) quite equal to, or beyond that belonging to life. On opening the animal, the view displayed was truly remarkable. The stomach had completely disappeared, and its contents were dispersed over the peritoneal cavity; a portion of the liver was attacked and dissolved away; the greater part of the diaphragm was gone, and the ribs on the left side (the animal had been laid on this side) bared or completely dissected out; the gastric juice escaping from the chest, the flesh about the neck and upper extremity on the left side had been acted upon, and was in a state of disintegration, so that the slightest force was sufficient to pull the parts asunder. It is truly astonishing the effect produced under these circumstances by the solvent influence of the gastric juice.

ON THE ACTION OF THE HYPHOSPHITES OF SODA AND LIME, THE SO-CALLED "SPECIFIC" FOR TUBERCULAR DISEASES.

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In a paper read by Dr. Churchill before the Académie Impériale de Médecine in the year 1857, it is confidently stated that "the immediate cause, or at least an essential condition, of the tubercular diathesis consists in the diminution in the system of phosphorus in an oxydizable state," and that "the *specific* remedy for this disease is a preparation of phosphorus at the lowest possible degree of oxydation." Now, although such a statement was not based upon either chemical, physiological, or pathological investigation, but was purely hypothetical, yet I thought it might be desirable to test the efficacy of Dr. Churchill's treatment, and to publish the results, which, alas! were very uncomplimentary to the new "*specific*."* Dr. Churchill, however, having complained that this trial of his new remedy was of insufficient duration, I have thought it but fair to repeat the experiments, and at the same time to explain that the sole cause of the brevity on the former occasion was due to Dr. Churchill's own statements. We were informed in his paper that "even from the first day of taking the hypophosphites they produce a manifest increase of nervous power, together with an unusual feeling of comfort and strength: the appetite increases, the perspirations cease, sleep becomes calm and profound," &c. Now, as I failed to observe any such consequences in any one of my patients within two, or at most three, weeks of their taking the hypophosphites, I considered that it would be unjustifiably trifling with the disease to give a longer trial to the treatment.

Shortly after this, my colleague, Dr. Quain, published in THE LANCET the results of a far more elaborate series of experiments with the hypophosphites,† and came to the conclusion that

* Medical Times, Feb. 13th, 1858.

† THE LANCET, March 17th, 1860.