in the course of a night without doing more than I wished, and with great benefit to my patient; and the sleep which has in due time resulted a *post hoc*, in one sense a *propter hoc*, has been in my opinion not the effect of the soporific power of the opium, which, having been expended in overcoming or antagonizing the excitement of the nervous system, the essence of the disease, allows of the consequent sleep,—that sweet sleep worthy indeed of the encomium of the poets from Shakspeare downwards; and if closely observed it has not the characteristic of the heavy sleep produced by opium.

Proceeding to the second part of my subject, I am induced to believe we can advantageously explain the whole of the phenomena as the mere secondary effects of one cause—that cause, gastric irritation. There is a red tongue, with aphthous appearances; there is tenderness of the epigastrium. I believe that the dysphagia is the pure result of excited reflex action; the dyspnoea, the palpitation, and the irregular action of the heart likewise. The nervous connection is direct, and daily observation strengthens the probability.

One of my patients who, ever since his recovery from an attack of this disease, has erred on the side of abstemiousness, has the same difficulty of swallowing whenever he suffers from indigestion, and sometimes is entirely unable to swallow. This would appear to be a miniature portrait of hydrophobia.

Another patient, whose case is improving under the use of alkalis, bitters, and alteratives, every time she sits down to dinner is seized with such a violent paroxysm of coughing as to be obliged to leave the table. The salivary secretion is increased, and probably depraved, and is the irritant in the stomach in this case, and excites the reflex action in the diaphragm and other respiratory muscles.

The vomiting, the colic, the convulsions, are all as readily explicable upon the same principles, and case after case might be cited illustrative of the power of one irritation being capable of producing several secondary effects.

It would be interesting to pause and examine the mechanism by which the complexity of reflex action was effected in these cases. Certain is it, that many nerves of great apparent dissimilarity in their properties and functions must have been employed for their production—namely, the glosso-pharyngeal, the accessory, the laryngeal, the phrenic, and other associated respiratory nerves, the cardiac plexus, those spinal nerves which are associated with the phrenic in the act of vomiting,—for the stomach, we know, is altogether passive,—the sympathetic itself, and the anterior branches of the lumbar spinal nerves. Is it probable in the next place, that such a number of nerves, conveying reflected disordered action, could be excited to this by one source of irritation—by one efficient nerve, the pneumogastric? I believe it is *vagus* by name—*vagus* by nature. Connected seemingly everywhere with other nerves, entering largely into many associated actions, it is this which renders the stomach so remarkable for its sympathies. But to return, the original symptoms of the disease were frightfully aggravated by all this waste of nervous power, but they yielded kindly to treatment.

Although time hastens I am tempted to borrow another form of disease illustrative of another principle of the reflex function, namely, that the same effect

may arise from two or more causes. This is spermatorrhœa, involuntary seminal discharge. A gentleman, the subject of it, who, notwithstanding his positive denial of it, has probably abused his generative functions by solitary indulgence; whenever he luxuriates in sexual desire, even looks upon a woman to lust after her, immediately suffers the involuntary emission; and on awaking in a morning, discovers that the same thing has happened. The excitor in one instance, is a mental cause; in the other a distended bladder. I desired him to pass his water during the night, and then emission never occurred. There is besides, in this disorder, chronic inflammation or irritation about the termination of the common ejaculatory ducts, but the emission takes place in the morning and awakes him. Children too, who wet their beds, may be broken of the habit by keeping them, if possible, upon their backs, with their pelvis raised, thus taking the irritant away from the neck of the bladder.

Let us recollect also that the effects of excited reflex action are transmitted through sensiferous as well as motor nerves. The pain in the side in leucorrhœa, the facial pain in early pregnancy, point to the same principle. It would be easy to state many cases as examples, but it is enough to recognize the principle, to which, when it speaks in the terrible language of tetanus, hydrophobia, epilepsy, and convulsions, we are obliged to listen,—in its seeming caprice or coquetry, as in chorea and hysteria, we give it heed.

Let us equally regard it in those minor complaints of every-day occurrence, upon the treatment of which, after all, depends the success of the practitioner.

Let us discard the language of sympathy as unworthy the present status of physiological knowledge, and substitute for it that founded upon induction and truth; and while we assume to ourselves the laurels of success, the result of well-earned labours, let us not have the ingratitude to forget that we owe to Dr. Marshall Hall some acknowledgment for having brought so clearly to light the explanations of facts which have so long slumbered under the cloak of obscurity, for having established certain principles of physiology, which, from their important bearing upon the treatment of disease, must entitle him to be considered a benefactor of mankind.

MEMOIR ON THE DIGESTION AND THE ASSIMILATION OF AMYLACEOUS AND SACCHARINE MATTERS.

By Dr. MIALHE, Professor Agrégé at the Faculty of Medicine, Paris.

(Continued from page 311.)

II. HYDRO-CARBONATED SUBSTANCES NOT IMMEDIATELY DECOMPOSABLE BY THE ALKALIES OF THE BLOOD, OR HYDRO-CARBONATED SUBSTANCES NOT IMMEDIATELY ASSIMILABLE.

Sugar of cane or of beet root.

Cane-sugar is not immediately capable of assimilation, the alkalies of the blood not having any decomposing action upon it; thus, when injected into the veins it passes into the urine without having experienced any alteration whatever, (Bernard and Barreswil;) but when it is dissolved in gastric juice, it acquires the

property of being assimilable, for if then injected into the veins, it no longer shews itself in the urine, (Bernard and Barreswil.) MM. Bouchardat and Sandras have also shown that cane-sugar introduced naturally into the blood passes in the urine. "We have," say they, "successfully repeated the experiment of Messrs. Bernard and Barreswil; we have injected half a gramme of cane-sugar into the veins of a dog, and we have met with this sugar in the urine; we have replaced the cane-sugar by the same quantity of glucose in one experiment, and by the same quantity of converted (*inverté*) sugar in another; and in each of these instances, we have found neither glucose nor converted sugar.

These facts evidently shew the necessity for the mollecular transformation of cane-sugar into grape-sugar or glucose, in order that it may undergo the decomposing action of the alkalies, and become assimilable; how then is it that MM. Bouchardat and Sandras can have asserted that all their experiments, with the exception of those relating to the conversion of sugar into formic acid, are opposed to my views MM. Bouchardat and Sandras might themselves have recognized in these facts one of the best arguments in favour of my theory; but these authors have preferred giving an erroneous explanation, in which they assign to the oxygen of the atmosphere a part which is contradicted by observation.

Fecula or starch.

Fecula as such, is not assimilable, for like cane-sugar, it is not capable of decomposition by weak alkaline solutions; it ought therefore to undergo some change to become assimilable. I have sought to ascertain what chemical phenomena might be the cause of this transformation of starch into dextrine and into glucose, and have arrived at the discovery of animal diastase. I proceed to shew that it is solely by means of diastase that the transformation of the starch is effected.

If, in their last work upon indigestion, MM. Bouchardat and Sandras have attributed the change of starch into dextrine and glucose, to the presence of a substance analogous to diastase in the digestive passages, they have no where indicated the source of this diastase, and have even altogether forgotten to study the action of the saliva, of which they make no mention. It is, nevertheless, precisely in the saliva that I have found the substance analogous to diastase.

I have said that Leuchs was the first to recognise that starch, brought to the state of starch-paste (*empois*), by baking, and heated with fresh saliva, becomes liquid in the space of some hours, and it is then found to be converted into glucose; that Sebastian had also shown that starch digested with saliva, loses its property of becoming blue with iodine. I have very frequently repeated these experiments, and have established that the product of the reaction of saliva upon fecula is always in the first instance dextrine, and not sugar of starch, as Leuchs has stated; I have besides assured myself that, in order that starch may be quickly changed into dextrine and into glucose by the salivary secretion at the temperature of the animal body, it must be disintegrated, an effect which is obtained by boiling it in water, or pounding it cold. This explains why carnivorous animals digest starch

with far greater difficulty than ruminant animals and granivorous birds. Who does not know that these two descriptions of animals are provided with a masticating or grinding apparatus far more perfect than that of the carnivora?

I have established it as a principle that animals digest feculent aliments with a facility in proportion as their salivary glands are more developed, and their masticating or grinding apparatus more perfect; and for proof in support of what I advance, I would refer to the facts collected by Burdach in his *Traité de Physiologie Générale*, to shew that the humidity of the mouth promotes digestion. It results from my researches that the action of the saliva upon starch is much more rapid than had been thought. Let a certain quantity of starch, in the state of starch-paste (*empois*), newly prepared, be introduced into the mouth, and immediately submitted to mastication; in less than a minute, the insipid taste of the starch-paste will be replaced by a taste manifestly saccharine, altogether analogous to that of syrup of dextrine. This short time is sufficient for the saliva partially to change the fecula into dextrine, and into grape-sugar or glucose, so that the starch thus modified, when filtered, is no longer influenced by iodine. With hydrated starch diffused in water and filtered, the action of the saliva is still more manifest: it is almost instantaneous, and the transformation of the starch is even complete. I have convinced myself of this fact by shaking for a few seconds a mixture of equal parts of filtered starch and of saliva, *carefully neutralized*, also filtered, and submitting the mixture immediately afterwards to the action of ioduretted water, no change of colour took place.

In the foregoing experiments, as well as in those which are to follow, I have clearly shewn the action of the saliva upon fecula by the tests both of iodine and of potass, and have thus been able to obtain results infinitely more precise than by having recourse to iodine only. Thus, when saliva is made to react on starch, a transformation more or less complete of this latter substance into dextrine and into sugar of starch occurs. If the change is complete the iodine solution causes no change of colour; but if the transformation is incomplete, the iodine can only give indications on the proportion of starch modified, the iodic colouring being in inverse ratio to the proportion of starch which is changed, for the iodine only acts on the fecula not decomposed. We must, therefore, have recourse to potass, which, contrary to iodine, exercises its effects only on the modified starch. With this view the amylo-salivary solution is filtered, a few drops of liquid caustic potass are added, and heat applied; the proportion of modified starch is estimated by the degree to which the solution becomes coloured, pure starch not being coloured by alkaline solutions.

Action of saliva on crude fecula.

Crude fecula is only very slowly or very imperfectly soluble in saliva. At the first moment of contact the action is indeed null, but when the starch is digested for two or three days in fresh saliva, taking care to assist the action by a temperature of 40° or 45° centigrade, the transformation of the starch is evident, and is proved by the want of colour from iodine, and the marked yellowish-brown colour produced by potass in the filtered amylo-salivary solution.

Action of saliva on crude starch when pounded.

In proportion to the slowness of the action of the saliva on crude fecula, in the state of aggregation which is proper to it, so on the other hand is the action rapidly manifested on starch disintegrated by trituration. A few hours contact are sufficient in this case to render the transformation of the starch complete. The preceding indications enable us to see why animals which have the most perfect masticatory or grinding apparatus—or again, which have the digestive apparatus most strongly developed, are precisely those by which crude fecula is most easily digested, as STEPHENS, and more recently MM. BOUCHARDAT and SANDRAS have shown.

Action of saliva on bread.

When bread is well baked it is quickly acted upon by the saliva, thus it is observed to acquire a marked sweet taste during mastication; but when the bread is badly baked, when all the starch which it contains is not disintegrated, the transformation of this body into dextrine and into glucose is imperfect, and all that part which has not been thus modified remains undigested.

Action of saliva upon unleavened or wafer bread.

Notwithstanding that unleavened bread has not undergone panary fermentation, it nevertheless constitutes an eminently digestible food, provided that all the grains of fecula which it contains have been entirely disintegrated by heat, and are ready to undergo the solvent action of the saliva.

(To be continued.)

ON LIVING ANIMALCULÆ IN HUMAN URINE.

TO THE EDITOR OF THE PROVINCIAL MEDICAL AND SURGICAL JOURNAL.

SIR,

In answer to Dr. Shearman's communication of July 1st, I beg to state that I have frequently observed the animalcules he has described in human urine; they are referred by Dr. Golding Bird, to the genus *Vibrio*, (*V. Lineola*?) and the remarks of this author are quite in accordance with my own observations.

Last November I have recorded the case of a youth who was greatly emaciated, and had long been suffering from violent urinary irritation. The urine was very abundant, seventy ounces in twenty-four hours, it was passed with great agony, such indeed was the irritation of the bladder that the presence of stone was suspected, and frequently sought for, but in vain. The urine was pale, but muddy, specific gravity 1.010; it contained traces of free sulphuretted hydrogen, and was highly albuminous. By the aid of the microscope, pus-globules, altered blood-discs, and thousands of animalcules were observed. The animalculæ were present in urine directly obtained from the bladder, and died when decomposition had fairly set in. The patient had great thirst; the tongue was foul, having long red papillæ; the appetite was capricious, and the food taken produced great flatulence; the bowels were purged, but the food passed in an undigested and fœtid state; the pulse was 110, feeble; the skin hot; and he had a slight cough. There was also some tenderness over the lumbar region. This case had been submitted to a great variety of treatment, but the warm bath, a moderate and easily-digested diet;

together with gentle laxatives and tonics, were the means of giving him considerable relief. The patient shortly afterwards left the town, so that I am ignorant of the final result.

I was led to enquire whether the animalcules had been generated in the bladder, by finding them in a specimen examined only two hours after the evacuation of the urine. In all former instances I had taken it for granted that they had been formed after the urine had stood some time, and had already begun to decompose. I still think that they are intimately associated with decomposition, since they are only observed in strumous and debilitated persons, where the vital energy is unable to correct the tendency to decomposition which the urine containing them is especially liable to. In the case I have alluded to, the urine contained free sulphuretted hydrogen and albumen; the latter I have almost uniformly observed in connection with vibriones. Dr. Shearman would oblige me by noting this point.

I remain, Sir,

Your obedient servant,

J. H. STALLARD.

New Street, Leicester, July 6, 1846.

DR. SHEARMAN'S CASE OF ANIMALCULÆ IN URINE.

TO THE EDITOR OF THE PROVINCIAL MEDICAL AND SURGICAL JOURNAL.

SIR,—

On looking over the number of your valuable Journal for July 1st., I notice a query by Dr. Shearman, of Rotherham, respecting a case in which he has found numberless animalcules in the urine of one of his patients. Dr. Shearman states that these animalcules are "perfectly unlike spermatozoa, and appear to be pretty nearly the same circumference from head to tail."

From the whole history of the case, however, as reported by Dr. Shearman, it appears to me that he has met with one of those insidious cases of spermatorrhœa so well described by M. Lallemand, and which are far more common than is often suspected. I would therefore suggest to Dr. Shearman that he cause his patient to pass urine into three different glasses. One of these should contain the first portion of the excretion; the second an ounce or two passed after the urethra has been well washed out by the first discharge, and the third the remainder of the fluid to be excreted. I venture to predicate that in the second glass no animalcules will be found, whilst they will be abundant in the third, containing the last drops of the urine, and probably a few will be found in the first glass, having adhered to the walls of the urethra from previous seminal discharge. Nothing is more common in advanced cases of spermatorrhœa than an entire change in the form of the spermatozoa, which seem to be habitually expelled before they are fully developed.

I remain, Sir,

Your obedient servant,

H. McDougall, M.R.C.S.E.,

Formerly House Surgeon to University College Hospital.

46, Berners Street, London,

July 7, 1846.