sults of his experiments, and alluding first to the effects of ether in promoting absorption of fatty matters, he says:—"Finally, in our experiments, the injection of the lacteals is due to the abundance of the pancreatic secretion, and to the extremely fine state of division of the fatty matters which occurs in the intestine. It is due, also, to the *absorbing power having been augmented by the ether*. This activity of absorption has been observed in the case of several poisons. We have already mentioned that the combination of ether with poisonous doses of strychnia and nicotine produced death more rapidly in the animals on which we experimented. The action of ether introduced into the digestive organs is, therefore:—1. To stimulate all the intestinal surface. 2. To stimulate the secretions which are poured into the intestine. 3. To modify and decidedly increase the absorbing power." In a short passage (page 428), at the end of the experiments referred to above, the following passage occurs:—"Ether increases the absorption of fatty matter, which it places at once in a state of fine division, and in contact with a more abundant secretion of pancreatic juice. In this way it produces a very marked white injection of the lacteals."

If necessary, I might multiply these extracts still more, but enough has, I think, already been quoted from the works of our first experimental physiologist to teach us the physiological action of ether. This action is twofold: (1) it stimulates the pancreas and glands of duodenum to pour out their secretions freely, and (2), at the same time, it facilitates the absorption of those very substances which these secretions are designed to digest. In other words, ether not only obtains for us the secretions required to digest fats, but promotes the absorption of these fats when digested. Can any physiological action be more clear, or any experimental proof be more satisfactory for the end we have in view?

[To be concluded.]

THE RELATION OF THE SECRETION OF PHOSPHORIC ACID TO TEMPERATURE IN CERTAIN CONDITIONS OF THE NERVOUS SYSTEM.*

By E. L. FOX, M.D., Clifton.

IN making careful observations on the temperature of the human body in health and disease, the question must constantly occur to every one: "Of what is this temperature the index?" We may assume that in the body the development of heat depends upon chemical agency; and many observations are being continually made by analysis of the excreta to connect the higher grades of temperature with increased secretion, or increased waste of tissue. It cannot be measured by the amount of carbonic acid excreted, as high temperature and increased number of respirations only exceptionally bear any direct relation to each other; nor can it be measured by the amount of urea excreted, since this varies according to the nature of the food; since, too, it is very doubtful whether the amount of urea is any measure of chemical change in nitrogenous tissues; and since temperature may fall at the time of large excretion of urea, as after the first few days of pneumonia, and may remain just the same with very variable amount of urea excreted, as in typhoid fever.

Another fact against the view that temperature of the body is an index of chemical change as measured by the excreta, is seen in the instances of chorea and diabetes mellitus. It is rare in either of these affections that the thermometer will much exceed 99 deg. Fahr., and yet in both the waste is considerable, and in diabetes may be excessively rapid and striking. In the face of these difficulties Claude Bernard long ago proposed the theory that animal heat was not due to combustion in the tissues, but to an acceleration of the passage of the blood through the tissues; and founded his opinion on the effects of galvanism of glands, and of division of the sympathetic nerve. Against this view, however, it may be stated that in many diseases temperature bears no relation to pulse, as in the first week of typhoid, in the last stage of typhoid in old people, in erysipelas, in remissions of temperature in acute phthisis, and in cerebro-spinal meningitis. Besides which, the fact that in some cases on record the temperature has become considerably elevated after death, proves that Bernard's theory fails to explain all phenomena on this subject.

Accepting, however, the theory that animal heat depends on chemical change within the body, and having no further expectation that excreted products will be found indices of its degree, we believe that the truth of the matter was expressed long ago by Grove in his essay on the Correlation of Forces:—"It may, however, be asked, what becomes of force

when motion is arrested or impeded by the counter-motion of another body? This is generally believed to produce rest, or entire destruction of motion, and consequent annihilation of force; so, indeed, it may, as regards the motion of the masses; but a new force, or new character of force, now ensues, the exponent of which, instead of visible motion, is heat. I venture to regard the heat which results from friction or percussion as a continuation of the force which was previously associated with the moving body, and which, when this impinges on another body, ceasing to exist as gross palpable motion continues to exist as heat." And Donders says, in a terse sentence: "It is not sufficient to deduce only the heat developed from metamorphosis of matter; but, in addition, some other force—say work—must have its cause therein; and only when neither electricity, nor light—developed in some animals—nor mechanical power, which proceeds from all, was lost externally; when, therefore, all were changed wholly into heat, could the quantity of heat developed correspond to the oxidation of the organic matters."

If all the results, therefore, of the oxidation of organic matters could be collected and measured, they would be indices, not of heat only, but of force—force that may partially take the shape, so to speak, of heat, but may be expressed in some other form.

Force in the human body is shown mainly in work and in heat; and this work may be mechanical or intellectual, according to the nature of the chemical metamorphosis. In a word, whilst wholly deprecating all materialistic views, intellectual work is as much a result of chemical agencies, as any secretion. In the endeavour to illustrate this point I have taken a series of cases in which there has been some abnormality of the nervous system, and have first calculated the amount of phosphoric acid passed in the twenty-four hours. Although the tabulated results refer only to the urine, it may be allowed that, speaking generally, they include about four-fifths of the phosphoric acid excreted. The cases are taken somewhat at random from those ordinarily admitted into hospital. They are divided into those in which noisy delirium was a prominent symptom; those in which there was low delirium; and, lastly, those in which there was some nervous lesion without delirium.

FIRST SERIES.—IN WHICH NOISY DELIRIUM OCCURRED.

Highest Temp.	Disease.	PO ⁵ in Urine in 24 hrs.
98½	Morbus Brightii	30 grains
98½	Epilepsy from Alcohol	61
98½	Epilepsy	43.5
104	Typhoid Perforation	45.9
102½	Puerperal Mania	90
100½	Puerperal Mania	96
98½	Chronic Meningitis	43.5

SECOND SERIES.—IN WHICH LOW DELIRIUM OCCURRED.

102	Acute Tubercle of Peritoneum and Intestines:	
	Diarrhoea	32.5
102	Typhoid	78
98½	Delirium Tremens	12
103	Delirium Tremens and Pneumonia [In Pneumonia, phosphoric acid is generally diminished]	96
102	Bright's Disease and Pneumonia	56.8
(No record)	Softening of Central Parts of Brain; Granular Kidneys; Tubercular Lungs	32.5

THIRD SERIES.—IN WHICH NO DELIRIUM OCCURRED.

98½	Slight Hemiplegia and Paralysis Agitans	34.5
98½	Fatuous Condition after prolonged Bathing	90
98½	Concussion after fall in Epileptic Fit	32.8
100½	Hemiplegia and Cerebral Softening	90
98½	Same case, later on	69.9
99	Chorea	74.5
98	Chorea, with Delusions	52
101	Traumatic Tetanus	35
102	Phthisis, with Delusions	59.9

Now, how does this temperature correspond to these excretions of phosphoric acid? In the first series, in which the PO⁵ varied from 30 grains to 96 *per diem*, the temperature was not higher than that of health, except in the case of typhoid fever, and in puerperal mania in which, on one day, the thermometer stood at 102½. In the second series the temperature was high in several cases, because the low delirium was simply an adjunct to acute diseases, pneumonia, acute tubercle, and typhoid fever. In the case of delirium tremens, in which only 12 grains of PO⁵ were passed in the 24 hours, the temperature was 98½. In the third series, in which no delirium existed, the temperature was normal, except in the acute stage of cerebral softening when it was 100½; in chorea, 99; in traumatic tetanus, 101; and in phthisis, with extreme emaciation, in which it was 102 deg., with but slight increase of PO⁵, of which it may be remarked that Beneke says that in all cases of tuber-

* Read in the Medical Section at the Annual Meeting of the British Medical Association in Oxford, August 1868.

culosis with emaciation, the earthy phosphates are increased. Competent observers have decided that mental excitement and intellectual exertion increase the amount of urinary water, increase also the urea, chlorine, sulphuric and phosphoric acids, notably the latter, and lessen the amount of uric acid. Yet, in the six cases of our first series, the PO^5 was only decreased in two of the cases of low delirium, in which we can hardly speak either of mental excitement or intellectual exertion; increase of PO^5 occurred in three cases, one of which was delirium tremens; whilst of those in which no delirium existed, there was great increase of PO^5 in the case of fatuity, great increase in the case of hemiplegia, with acute cerebral softening, with the mind quite clear and calm, and considerable increase in the chorea; whilst in the case of phthisis the increase was small, although delusions of a most horrible character haunted the patient night and day, and made life a burden to her.

It should be remarked that most of these cases were being fed on rather a low diet, including about twelve ounces of bread, one pint of fairly strong beef-tea, and about a pint of milk. The exceptions were the cases of phthisis and chorea, in which a meat diet was taken. Except the child suffering from traumatic tetanus, all the patients were adults. These observations tend to show that although a high temperature in cases in which delirium is a symptom, seems generally accompanied by an increased secretion of PO^5 , we yet may have a great increase in the excretion of this substance without any exaltation of temperature.

In some cases, as in puerperal mania, the excretion of PO^5 may be an index of the delirium, *plus* the heat; whilst in others the PO^5 may be largely increased without any exaltation of temperature, without any delirium, or, indeed, as in the case of fatuity, without evidence of the existence of any mental function. Whether, however, this state of fatuity is a condition in which all mental function is in abeyance, or whether it is not, sometimes, at least, a condition of mental activity, in which the connection between thoughts and their external expression is broken through shock or disease, is beyond our present purpose. It is, therefore, probable, that the amount of PO^5 excreted bears no certain relation to the character of the delirium, except this, that in delirium, accompanied by hyperæmia of brain, as in acute mania, or the acute delirium of French authors, there is an increase in the amount of PO^5 . In all other cases the relation seems wanting, although probably Dr. Bence Jones's observations on the diminution of PO^5 in delirium tremens, express a general, though not an universal truth; whilst the elevation of temperature depends upon such a vast number of complicated chemical actions—chemical actions that, under other circumstances, might produce work mechanical, or of some other kind, that any attempt to connect it with the excretion of one particular substance absolutely fails by experiment.

A PECULIAR FORM OF SCARLATINA.

By N. HECKFORD, Esq.,
Surgeon to the East London Children's Hospital.

I THINK it important to draw the attention of the members of our Association to my experience of a peculiar form of scarlatina lately prevalent in the eastern districts of London. In connection with the East London Children's Hospital, I have recently had under my care numerous cases which presented the following marked peculiarities. There was usually little or no febrile disturbance, often not sufficient to induce the mother to keep the child in bed. The rash was either altogether absent, or so slight, that no history of it could be obtained. The tongue did not exhibit its characteristic appearance. There was at first no accompanying sore throat; the submaxillary glands almost invariably inflamed, and, as a rule, the inflammation terminated in suppuration or sloughing. Indeed, it was for these "swellings in the throat" that the patients were often brought, the premonitory symptoms having been so slight as to escape notice. This condition was occasionally followed by a sort of diffident diphtheritic exudation, which exceptionally ended in ulceration of the fauces. At a later period, and especially in fatal cases, a rash appeared over the trunk and extremities resembling a purplish roseola. The mortality was excessively high. The record of the following chain of cases, being in-patients of the hospital, will aptly illustrate the insidious and marked character of this form of scarlatina.

CASE I.—Six weeks before admission, a boy, aged three years, living in a small street in which scarlatina was rife, was attacked with what was thought to be febricula; there was no sore throat or rash; the submaxillary glands inflamed and sloughed; the inflammation eventually greatly subsided, and matters promised well, but two deep sinuses, discharging a thin sanious pus remained. One night, at the end of this time, profuse venous hæmorrhage took place. Pressure by means of a compress saturated with a solution of perchloride of iron, arrested the bleeding

for two days, when it recurred, and again also on the fifth day. By this time the child had become quite blanched, and delirium traumaticum manifested itself. At this juncture I laid open the sinuses into one opening, and at the bottom of the wound, which was deep and sloughy, I found the blood issuing from a large vein, either the internal jugular or the internal maxillary. The jet was equal in size to a pigeon's quill. I determined to ligature the common carotid; but, being at the moment single-handed, I tightly plugged the wound with cotton-wool soaked in the perchloride, and directed manual pressure to be kept up. Soon afterwards, Mr. Buxton Shillitoe saw the case with me. The hæmorrhage having been completely checked, we resolved to give digital pressure a trial. This was continued without intermission for sixteen days. On the third a few drops of blood escaped, but on no other occasion. The plug was renewed every second or third day. Not long afterwards, the boy was discharged cured.

CASE II.—Three days after the admission of the above case, a strumous child, in the same ward, had an attack of typical scarlatina, sore throat, rash, "strawberry" tongue, etc. It died of dropsy and albuminuria.

CASE III.—Before the symptoms in the second case had unequivocally declared themselves, and the coming evil not being suspected, a boy, aged 8 years, was cut for stone by Mr. Shillitoe. Forty-eight hours after the operation it was evident that he, too, had scarlatina. He had high fever and a rash, but no sore throat. He made a most satisfactory recovery.

CASE IV.—A boy next to the above, also suffering from stone, but waiting for operation, went through a well-marked form of the disease.

CASE V.—A girl, aged five years, admitted for infantile paralysis of one leg, was apparently doing very well. For two or three days her appetite fell off, but she soon seemed to regain her usual status. However, as her face looked somewhat puffy, her urine was examined, and found to be highly albuminous. On the evening of the day this was discovered, having eaten her meals heartily, she vomited soon after going to bed. This was followed by urgent dyspnoea and sudden death. At the *post mortem* examination, both pleuræ and the pericardium were found filled with fluid.

CASE VI.—A stout healthy-looking girl (having hydatid of the liver), who, in common with the preceding case, had not been obliged to keep her bed for a single hour, was attacked with sudden and severe albuminuria. The only premonition was loss of appetite for a few days. She did not have dropsy, and is now well, excepting as regards her liver.

CASE VII.—This was a similar one to the two former, but the albumen was not copious.

OBSTETRIC MEMORANDA.

LACERATION OF VAGINA.

By A. B. STEELE, Esq., Lecturer on Midwifery, Liverpool Royal Infirmary School of Medicine.

I WAS summoned by a midwife to a woman, the mother of several children, who had been in labour for a few hours, all apparently going on well until just before I was sent for, when the pains ceased suddenly, and were replaced by a continuous sharp lancinating pain in the abdomen, the patient at the same time becoming very faint and sick, leading the midwife to suspect that rupture of the uterus had taken place. There was no unusual hæmorrhage. I found the woman in a state of incipient collapse; and, on examination *per vaginam*, the head low in the pelvis. I at once applied the forceps and delivered a living child in a few minutes and with very slight traction; the placenta followed immediately, with no more than the ordinary amount of discharge. On introducing my hand after delivery was completed, the uterus was found intact and moderately contracted; but in the situation of the posterior *cul-de-sac* of the vagina, was a rent large enough to admit easily the hand, which was readily passed along the posterior surface of the body of the uterus, and the convolutions of the intestines could be distinctly felt. The patient was carefully watched, opium was given freely, with stimulants, until reaction was established; and she gradually recovered without a bad symptom. The woman had been ill treated by her drunken husband, who, about a fortnight before labour came on, had kicked her violently in the back, which may possibly have caused the injury, although it is singular that so long an interval should elapse before the mischief manifested itself. The facility with which the forceps were used, and still more conclusively the pre-existence of symptoms of serious lesion before their application, show that the injury did not occur during delivery.