the views of the Paris physiologists, the following case is quite inexplicable; as such, however, it is one of immense interest.

S. M., aged 33, was admitted on June 28th into St. Bartholomew's Hospital. She complained of pain in the neck, with some obscure fullness and tenderness of the bones in that region, and, what unfortunately but too well marked the real nature of the disease, very considerable want of motion of the lower limbs, amounting to paraplegia, but *no want of sensation*. This singular paraplegia of the lower limbs, with comparatively good use of the upper extremities, though the poor woman looked quite cheerful, and ate her food, and exerted herself in the occupations of hospital life, left something undefined about the case. The treatment did not present any peculiarity: she had had pains of the chest and abdomen, for which Mr. Lawrence ordered alkalis and bitters; blisters also had been applied, for what definite purpose is not ascertained, to the stomach and chest.

During the last week of September, Mr. Lawrence ordered her the oleum jecoris aselli, as he then believed the disease to be scrofulous disease of the cervical vertebrae. About this period also, some obscure swelling under the jaw was observed, not painful nor attended with any soreness. It then continued slowly to increase through the first week of October, and some deep seated inflammation under the carotid vessels was now suspected for the first time. The paraplegic symptoms remain unchanged, the woman otherwise did not complain very much. The tumour now began to pulsate very forcibly, and if seen for the first time might have been taken for aneurism; Mr. Lawrence, however, remembering the quantity of loose cellular tissue in the parts adjoining the larynx, pharynx, and parts adjoining the sterno-mastoid muscle, made the diagnosis of abscess in connexion with disease (carious or otherwise) of the processes or spines of the cervical vertebrae, implicating the theca, if not the very coverings of the medulla spinalis itself. The sensation of the lower and upper extremities was good, but paralysis of motion in the former existed. Suddenly, about a week since, on the 26th of October, intense difficulty of breathing and dysphagia set in; the nurse at night made an alarm that the woman was choking; she shrieked and cried with pain. Mr. Lawrence, seeing there was no time to spare, at once made an incision behind the edge of the sterno-mastoid muscle. About five ounces of well formed pus were discharged, the woman now feels quite relieved, and the paraplegic symptoms have disappeared. K.

Original Communications.

THE TREATMENT OF FEVERS:

WITH SPECIAL REFERENCE TO VENTILATION.

By T. HERBERT BARKER, M.D., Bedford.

[Read before the Annual Meeting of the British Medical Association at Birmingham, July 30th, 1856.]

MUCH of the discrepancy of medical writings arises rather from a vagueness in terms and expressions, than from any striking difference in the facts observed, or in the deductions drawn by the medical authors. This specially obtains in regard to the use which is made of the term "fever". In past nosology, every condition of the body implying an increased heat of surface was considered a fever; and to this day, the same generalising phraseology retains a deep-rooted and extended position. Yet I take it, that if we are to consider the subject of fever with any advantage or point, we must define the class of diseases which we would specify under the individual term; indeed, all modern pathology shows the necessity of this, for surely, to take a

single example, no two pathological conditions can be more widely distinct from each other than those which we call by the names "inflammatory fever" and "typhus fever". Physiologically, these two states stand in exact opposition to each other; health, or the natural physiology of the body, holding a place midway between them.

Thus, in the presence of the inflammatory condition, we have an exaggeration of all the vital processes; we find not only the hot skin and the quick heart-beat, but the increased combustion as evinced in the abnormally increased elimination of carbonic acid, and in the tonic state of the circulation and the superfibrinised condition of the blood. In the typhus case, on the other hand, we find the decreased combustion of the body, a wavering and prostrated circulation, and defibrinised fluid blood, ready to exude almost passively through the coats of the capillary system. These are broad points of difference, which so essentially separate and isolate the two forms of diseased action, that nothing but an absolutely superficial knowledge of physiology and pathology can lead to the conception that they are in any way identical: and I maintain that we have no right to confound two such opposing states of the system with each other. In making use of the term "fever", I shall confine myself to that specific condition of the body in which all the symptoms, general and special, of typhus, as I have marked them out, are maintained, and which Franz Simon has described under the head *hypnosis*, to mark the difference between it and the inflammatory state, or that of *hyperinosis*.*

DEFINITION OF FEVER.

Setting aside, therefore, all possibility of confounding two separate pathological conditions by the abstruse application of a term, it may be well to consider what diseases are strictly to be classed under the head "fever". A marked line of distinction bounding the true fever-class is, that these diseases, under all circumstances, have their origin in the absorption of a poison, which, in regard to typhus, scarlet fever, measles, and small-pox (all of which come under the head of hypnosis, according to Simon), differs in each disease; while, in regard to those secondary forms of disease which are confounded with typhus, and are known under the names of continued fever, typhoid, adynamic, atonic, nervous, putrid, gastric, gastroenteric, petechial, and dothenteric, the poison is the same, and the differences of symptoms are rather differences of degree and of local complication than anything else.

In treating of this fever-class, my object is to dwell mainly on a rational system of treatment, applicable, as I believe, to all the diseases of the class, but especially applicable, as experience has taught me, to that subdivision of them of which typhus may be taken as the specific type.

That this important subject requires, at the present time, a careful and unbiassed survey, conducted under the influence of progressive and plain spoken argument, will not, I think, be denied; since, at the present time, nothing can be more unhappy or unsuccessful than the thousand and one vague and speculative modes of treatment which have been and are adopted. Let us take, as an illustration on this point, the facts which the Registrar-General's returns teach us in regard to the mortality from diseases of the hypnotic class. In the years 1850, 51, 52, and 53, there occurred from only three of these diseases—viz., small-pox, scarlet-fever, and typhus—the following number of deaths:—

	1850.	1851.	1852.	1853.
Small-pox ..	4,666	6,097	7,320	3,151
Scarlet-fever..	13,370	13,634	18,887	15,699
Typhus	14,308	17,121	17,845	18,013
Total	32,334	37,752	44,052	36,863

Making a grand total of not fewer than 151,000, a majority

* Animal Chemistry, with reference to the Physiology and Pathology of Man. Sydenham Society's edition, vol. i, p. 305.

of which cases, possibly the whole, were subjected to one or other form of medical treatment. It may be argued that the number of cases of diseases, out of which these deaths are taken, is not and cannot be supplied, and that therefore we cannot estimate the value of any particular form of treatment; but, at all events, the immense numbers of deaths indicate clearly enough that there can be no form of treatment which admits of being put in opposition to such a mass of startling facts.

The statistical point may be pushed a little further, also, by referring to the deaths from typhus alone in England in general, and in my own locality in particular. Thus the deaths from typhus in England were in—

1847	30,320
1848	21,400
1849	17,896
1850	14,298
1851	17,121
1852	17,845
1853	18,013.

Giving an average of not fewer than 19,557 annually.

The deaths from typhus in 100,000 living, were in—

1850	805
1851	969
1852	997
1853	995

The proportional number of deaths from typhus to 100,000 of deaths from all causes, were in—

1850	30,542
1851	44,050
1852	44,564
1853	43,480

A reference to the Registrar-General's returns of births and deaths, will show a weekly average of nearly fifty deaths from typhus in London alone.

In regard to Bedford and its neighbourhood, the facts are as follows:—The number of deaths from fever in Bedford and thirty-nine surrounding rural parishes (comprising an average population of 31,768 persons) during sixteen years, was 918, giving an annual average of 57.

RATIONALISM AND EMPIRICISM.

The treatment of the disease in its widest application is divisible into two distinct forms, which may be designated as rational and empirical. I do not mean to convey in the word empirical any kind of slight; inasmuch as the use I shall make of it is defined by Celsus, and is widely different from what is generally understood. Empirical treatment then, as I define it, is a treatment based, as Dr. Henry has well expressed, on a process of accidental observation made without any preceding deduction or argument. The rational treatment, on the other hand, is a treatment based solely on deduction, and worked out by scientific formulæ, and implying an intimate knowledge not only of the laws of disease but of healthy life.*

Now, in each of these systems, there may be a sound physiological argument at the bottom; but they differ, in that the rational plan is laid in a pre-existing knowledge of physiology, and is the result of physiological research, while in the empirical plan, any success that may attend it, though due to some physiological cause, is not necessarily connected with the knowledge of that cause. Thus, the discovery of quinine as a cure for ague was accidental, and made in ignorance of physiological laws—nay, even to this time the same ignorance in regard to this special remedy obtains. Yet, from the day when the first fever-stricken Peruvian drank from the pool in which the bark-tree lay immersed, and received new life from the draught, from that day a new physiological phenomenon was laid open

for explanation; since to presume that quinine does not produce its curative effects on physiological principles, is equivalent to supposing that the application of food to the uses of the economy is removed from the domain of physiology.

Not the slightest objection, therefore, is to be offered to empirical treatment when this is proved by repeated observation and beyond dispute to be useful; and every accidental fact should be accepted as a subject for inquiry, unless such inquiry should imply any unnecessary risks, or should be *a priori* opposed to known laws. But I must at the same time express my full conviction, that useful accidental discoveries of an empirical kind must always be few and far between, and that they assist but little in the progress of inquiry. It is in the world of science, as in the world of commerce; he who goes through life, trusting solely for his success to some rare accident which may turn up in the way of good fortune, may, in the proportion of one to a thousand, once in life realise the fact; but this is no argument against the common-sense and certain principle of the man who goes on, step by step, ignoring all thoughts of fortune and her freaks, and beating her out of time by storing up day by day, and hour by hour, the slow fruits of his exertions.

By this same rule, the scientific treatment of disease must rest mainly on a laboured and constant system of inquiry into physiological laws, and the modification of these laws by disease. In the way of progress this is the only safe standing ground; and so far from its preventing an accidental discovery from being made, it becomes a test of the truth of any such presumed discovery, and offers an explanation of the reason of such discovery, should it be a fact.

I have dwelt a little in detail on these points in relation to the science of treatment, because the logic of treatment is, even in this advanced day, in a very crippled condition. In fact, there are now two distinct streams of medical learning: there is a current of physiology running through our domain, yet adding but little to the resources of practice; and there is another current, very old, very rough, very muddy, which may be called the empirical current, the sources of which are innumerable, and the navigation of which, owing to the influx of so many streams, is not always satisfactory.

NUMEROUS FORMS OF TREATMENT.

In the treatment of fever alone, the use of the term being confined to that type of which typhus is the representative, the literature of medicine informs us that at least twenty-five distinct forms of treatment have been suggested. I do not convey in this statement an idea of the special number of drugs or medicines which have been recommended, for these are past all description numerous, and are but instruments in their way; but I speak of absolute plans of treatment, systematically laid out, and intended to embrace all the features of the disease, but yet having no specific object, being based on no sound views of the laws of life, and being all proved wanting when placed in the balance of practice and weighed without a bias.

It would be impossible to follow out a history of all these systems; the bold blood-letting plan of Jackson, Armstrong, and Clutterbuck; the blood-letting *coup sur coup* of Broussais; the stimulant system of Brown; the contrastimulant of Rasori; the alterative of Schotte; the cinchonism plan of Huxham, Lind and Fordyce; the cold water plan of Currie; the saline plan of Stevens and Graves; the carbonic acid plan of Clanny; the derivative plan of Kerksig and Callisen; the purgative plan of Hamilton; and the like. Suffice it to say, that the treatment suggested in all these systems was based on one of two erroneous views: either it was addressed to some local affection, which in truth forms but a section of the whole disorder, and was subsidiary to the general cause; or, if addressed to the disease in general it was so addressed because the author of the plan had, in a few particular cases, observed that a system, perhaps in the first place

* "The term empiricism implies a system based on the mere observation of facts without deduction, and on experiments made at hazard without any precise or preconceived object; rationalism, in its strictest sense, implies that mode of inquiry in which truth is obtained by the influence of reason or observation, and where experiments are performed as the means to an end, and as a test of the truth of what has been conceived beforehand." (ASSOCIATION MEDICAL JOURNAL, April 14th, 1854.)

accidentally hit upon, was, by comparison with other modes of treatment, more successful.

The want of success which has followed the majority of systems thus brought forward has been accounted for by some writers, among whom Dr. Copland stands most prominent, on the supposition that every plan of treatment has its advantages, and that the mistake has consisted in making one plan prevail through the whole period of the attack, instead of being guided by symptoms and addressing a new line of practice to each stage or modification of the disorder. At first sight, this argument sounds plausible enough. On closer view, however, it falls to pieces in every way; for, in the first place, it ignores the existence of a general producing cause, which never can be denied when a multitude of people are stricken with the same disorder; secondly, it places the local manifestation above the general; thirdly, it presumes, that one line of treatment can be at once abandoned and another taken up; as though one should say that the effect of wine on a man whose blood has been drawn, is the same as on a man whose blood has not been drawn; or as though, if I were to mercurialise a man to-day, I could stop that mercurialisation to-morrow, and commence a new plan; while, lastly, this wavering system in the treatment of fever has, in practice, proved no more successful than the steady continuance of one systematic course.

In the unsatisfactory state of mind to which these conclusions lead us, what then are we to do? Are we to give up our endeavours in regard to the management of fever? Certainly not; but this it does behoove us to do, to give up once and for ever the idea that we can, in the vulgar sense of the word, cure fevers by merely administering medicines. On this point thus spoke Pitcairn: "I do not like fever-curers. We may guide a fever; we cannot cure it. What would you think of a pilot who attempted to quell a storm? Either position is absurd."

To understand the true management of fever cases, we must take clear views: first, as regards their most probable cause, and secondly, as regards those physiological modifications, or in other words, those pathological conditions to which the body is subjected in the progress of the affection.

CAUSE OF FEVER.

In relation to the cause, we must assume it to be in the first place external; that is to say, it does not originate in the body *per se*, any more than the effects of a known poison, such as prussic acid, can originate in the body *per se*. This point scarcely requires to be dilated on; it is obvious. It is proved in the facts that the disease takes an epidemic form; that it is favoured by the position of certain localities; that it passes from one individual to another under favouring circumstances; and that the symptoms in one case of fever of the same type are, in a general sense, analogous to or even identical with the symptoms observed in another case of the same type.

Secondly, from the differences of type which occur, we must assume that each fever has its special poison; since we can no more suppose that the fever of scarlatina is under any circumstances or condition of constitution the same as the poison of typhus, than we can suppose that the symptoms of opium poisoning are the same as those arising from mercurial ptyalism. These two facts, with regard to the external causes of fevers in general, and the specific cause of a fever in particular, have this important bearing on treatment, that they lead to the question, whether one fever poison may not be introduced into the body by one channel, as by the lungs, another by a different channel, as by the stomach? or whether they may not all be received by any absorbing surface if applied to it?

At the same time, in a knowledge of the facts that the cause is external, and that there are only two main channels through the medium of which such poisons are generally received into the system, namely, the respiratory surface and the alimentary surface—except in some rare cases where the skin may be the channel, or where direct inoculation

may be practised; in the knowledge of these simple facts we gain a deeper insight into success in treatment than we do in learning all the curative dogmata which have been set up ever since medicine commenced; for we learn that, without even being acquainted with the absolute nature of the poison, we may ascertain the conditions under which it has manifested itself, and that, from the moment our services are called into requisition, we may remove from the patient the possibility of being subjected to the influence of the cause.

I do not think that any one will dispute this position; for, to take a simple analogy, if a person were to be subjected to the atmosphere of a closed room, through which a minute proportion of some cyanogen compound were diffused, of what avail would treatment of that patient's special symptoms be, if the eyes were blinded to the agent which was doing its deadly work, interfering with the main-spring of the patient's life? If this patient were convulsed, would leeches to his head do him any good, while he still breathed the same air? If he vomited, would an opiate be the means of cure? I think not; and in this mere argument I sum up the reasons why, in the so-called curative systems of treatment directed to fever, so much want of success has uncrowned the efforts that have been made.

MODIFIED PHYSIOLOGICAL CONDITIONS INDUCED BY FEVER.

These modifications are simply those which arise from any poison that interferes with the chemistry of life. It is from no fault in the organs of the body, in the first instance, that febrile diseases are set up, but from the chemical changes in function which accrue from other influences being brought to bear upon these organs. It is from the fact that the lungs receive into themselves, either directly through the bronchial canals by the air, or through the pulmonary artery by the blood, in cases where the poisons are first absorbed by the alimentary surface, a morbid agent, which, by virtue of an unexplained chemical process, called, in the absence of a better name, catalysis, prevents the organs from performing their natural and true physiological functions; upon which prevention, as a necessity, all the leading symptoms usually characteristic of low fever are set up.

I do not here enter into the question of what is called the zymosis or ferment, which supposes that some fever poisons procreate, so to speak, when they arrive in the body; for this, probable as it is, does not modify the view that ought to be taken of treatment; but I speak simply of the reception of poisons in the first instance, and of their chemical and perhaps physical effects upon the system at large.

And what are these effects in detail? Just such as would be expected to follow from any cause that interferes with the organisation of the blood. First, and most prominently, a modification of the blood itself, a want of purification of that fluid, a mechanical difficulty in its transmission through the lungs; a darkened appearance of the external surface as a further result; a congestion of the kidneys, and, thereupon, suppression of urine; an interference with the natural secretion of the alimentary canal, a prostration of the muscular powers (for the muscle wastes without being regenerated); and an interference with the functions of the brain, for the self-same reason.

Further still, in this condition of the system the finer ramifications of the circulatory apparatus are interfered with. That the capillary system should retain its entirety, it, as well as the muscular and other issues, must be regenerated and built up; failing in this point, as in fever, the walls of the capillary vessels easily give way, and the fluid blood exudes out of its natural channels.

So, in a word, in fever cases the whole physiology of life is changed; and the term putrid, applied by the old writers, if not chemically correct according to the ideas we now hold of putrefaction, retains, as regards appearances, a palpable meaning.

Nor are the changes which I have thus traced out from

physiological reasoning in any sense imaginary; they are what are met with both in pathology and in practice. We see them all in the living patient; we see them in the dusky skin, in the petechial exudation, in the dry tongue, in the laboured breathing, the muscular prostration, in the feeble circulation, in the delirium, and even in the occasional profuse discharges from the bowels; for what difference, in the abstract, is there between these exudations from the glands on the alimentary surface, and the exudation of blood in the petechial spot?

In watching fever cases, moreover, we are frequently struck with the rapidity with which they advance, and seeing that even the strongest suffer as severely as the feeble.

This again admits of a physiological reading; for the respiratory muscles, the heart, and the digestive system, in people of vigorous life, must always be up to the mark; and when they are interfered with by any cause, the result is felt instantly, inasmuch as the demands of the other parts of the system on these parts are always markedly developed. Hence, when a strong man is struck down with fever—when he can neither breathe with power, circulate his blood with power, nor receive through his stomach the amount of nourishment to which he is accustomed—the changes in his body are rigidly brought out, and he dies soonest, because he can tolerate a diminution of the vital powers least.

And, as we observe these indications at the bed-side, so do we observe their results in the dead subject; we find the lungs congested, the muscles softened, the blood fluid and disintegrated, the corpuscles even broken up, the fibrin diminished, the serum surcharged with bile and colouring matter, which the congested liver will not eliminate, the surface of the alimentary canal softened, its mucous coat possibly disintegrated, and the matter which it contains undergoing that process of fermentation which invariably obtains when the excretions lie in contact with tissues themselves undergoing a kind of decomposition.

I must trespass a little longer, in speaking of the physiological pathology of fever, to refer to the elimination of poisons from the body; for here too we get a guide to practice. In regard to ordinary poisons, I mean such as a man can take into his own hands and administer, we see that they are eliminated by various channels, such elimination depending in some degree on the physical character of the poison itself. Thus, although the volatile poisons, such as chloroform and ether, may be thrown off by the stomach, as is proved by their detection in the vomited matters of patients brought under their influence by inhalation, as Dr. Snow has shown,* yet these poisons and others of the class pass off mainly by the breath; while other poisons not volatile, such as antimony, go off, as Dr. Richardson has proved,† by the alimentary surface, by the liver, the kidneys, and as some have presumed, by the skin, and not in any case by the lungs, because they find no favouring medium of conveyance in the expired gases.

The ruling fact, then, seems to be that all volatile poisons may pass off by every channel, because every excretion is a medium for them; the expired air from the lungs can carry them with it, and the excreted fluids can hold them in solution, while the more solid poisons can only pass off in a soluble state.

How far this rule obtains in regard to fever poisons we are not prepared to say. It is fair to presume that they are all volatile, or at all events, that they may all be transmitted from short distances, at least, through the medium of the atmosphere; and hence it is not too much to assume that they may all pass off by every excretory surface, and that the critical evacuations in fever cases are what they are presumed to be, *critical*, in two senses of the word; *i. e.*, favourably critical, because they may bear with them the eliminated poison; fatally critical, because the prostrated system may not be able to sustain the loss thus

implied. Indeed, we see in practice how these poisons are in some cases thrown off; for example, in small-pox we know that they pass off by the skin—but the main channel of their elimination is probably the breath.

This physiological line of argument need not be followed further. I have introduced these views to show that our treatment of fever cases must not be based on any special observance of the particular symptoms, but must be directed primarily to the main-spring of those symptoms—their cause; and, secondarily, only to the symptoms themselves.

In saying this, I do not mean to assert that some medicinal measure may not, under particular circumstances, be of signal benefit. I believe that in many cases when there is great congestion, tympany, and a loaded state of the alimentary canal, a purge may do good, by removing obnoxious matters from the bowels, and by assisting also in the elimination of the poison; for, in case of recovery, such recovery commonly takes place by the elimination of the poison, and this, as I have shown, may take place by the various eliminatory surfaces. In like manner, diuretics, diaphoretics, and even emetics, may, when judiciously used, be useful. I shall refer in the sequel to cinchona, and its applications.

On the other side, in cases where from the natural efforts to expel the poison, the elimination from the skin or the bowels is too copious (for nature does not always regulate her curative principles to a nicety), the opposite plan of treatment, namely, the use of astringents, may be called for and prove of service. In both classes of cases the utmost care must be used; in the one class, that the elimination artificially produced be not excessive; in the other, that the astringent effect be not brought about too abruptly. In some degree, on this point, I agree with Dr. Copland, although differing from him in regard to the general principle which he inculcates. No doubt, the celebrity of the different systems of practice which have been alluded to, has had its origin in the natural facts above recorded.

[To be continued.]

Reviews and Notices.

THE TREATMENT OF CANCEROUS DISEASE BY CAUSTICS. A Critical Enquiry into the Modern Therapeutics of Cancer. Being the ADDRESS IN SURGERY delivered at Birmingham on the occasion of the Annual Meeting of the Provincial Medical and Surgical Association. By LANGSTON PARKER, Surgeon to the Queen's Hospital, Birmingham, etc. pp. 40. London: Churchill. Birmingham and Manchester: Cornish. 1856.

AFTER expressing clearly and concisely his views regarding the merits of excision and compression, and stating his opinion that cancer is in some cases constitutional, and in others primarily local, Mr. PARKER proceeds to enumerate the principal caustics which have been used in the treatment of cancer, and to point out the cases in which he considers their application to be called for.

The greater part of the information we possess regarding the use of caustics in cancerous disease is derived from the French surgeons, with many of whom this mode of treatment is in great favour. The caustics enumerated by Mr. Parker are arsenical pastes, chloride of zinc pastes, concentrated mineral acids, solid potassa cum calce (caustic of Filhos), and chloride of bromine, used either alone or in combination with the chlorides of zinc, antimony, or gold. Of these, the author seems to have had most experience in the use of the chloride of zinc, arsenical paste, and the caustic of Filhos, all of which he regards as highly useful, the latter especially in epithelial growths. To allay the pain produced by the application of the chloride of zinc, he applies at the same time a bladder of ice and salt, on the plan recommended by Dr. James Arnott.

The classes, situations, and stages of cancerous diseases,

* Paper read at the Medical Society of London.
† See ASSOCIATION JOURNAL, May 10th, 1856.