

point, I will proceed to submit for your inspection experimental results which will give you an idea of the amount of real assistance that can be expected to be artificially given through digestion towards assimilation by specimens such as these, with but a single exception—specimens that I purchased from our leading establishments in London as pepsine, or the digestive principle.

ON THE
DOCTRINE OF IRRITATION AND ON THE
NATURE OF THE EARLY CHANGES IN
INFLAMMATION.*

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It is almost universally held that what is termed "irritation," or "excitation," exerts a most important influence upon the process of nutrition. The action of "stimuli" upon living matter is considered to be a necessary preliminary to the manifestation of any phenomena. So, different kinds of stimuli or excitants—mechanical, chemical, and electrical—are spoken of; and it is held that every vital action must be preceded by an "excitation."

Increased "irritation" is supposed to "excite" increased action, and to cause a living "cell" to take up an increased proportion of nutrient matter within a given period of time. Irritation is believed to be the starting-point of those complex phenomena which in the higher animals we speak of as "inflammation"; and, according to the views now held, it is impossible to conceive the process of inflammation taking place without the previous occurrence of irritation.

Although of late years the changes occurring in the anatomical elements of tissues (cells) in health and disease have been carefully studied by very many observers, there still exists the greatest difference of opinion with regard to the nature of the processes which occur, even in the very simplest structures, during their growth.

A normal epithelial cell, for example, takes up a certain quantity of nutrient matter within a certain time, and certain changes are produced in a definite order and in regular succession. This cell, it is said, may be excited or stimulated to *increased action*. The normal process of nutrition may be exaggerated, and the cell may take up a much larger quantity of nutrient matter within a corresponding period of time, and increase in size with greater rapidity than before. Other changes may be observed in the epithelial cell, but these need not now be considered.†

What is the nature of this process of "stimulation," "excitation," or "irritation," and how does it produce these most important results? How is it, for example, that a particle of sand falling on the conjunctiva, or a thread passed through a living tissue, causes an increased action of the cells in the immediate neighbourhood? It is certain that the change is not dependent upon nervous influence, for it occurs in tissues destitute of nerves, and may be observed in plants. To say that the exaggerated action is the result of the "irritation" is no explanation at all: it is no more than stating the fact, that the cells grow faster, and take up more nutrient matter, soon after a foreign body is forced into close contact with them, so as to displace or injure them; for not only are the cells that are touched influenced, but all those in the immediate neighbourhood.

In every case of the so-called *stimulation* or *excitation*, the access of nutrient matter to the living and active part of the cell is facilitated. By the "irritant" perhaps a hole is made in the outer inactive matter (cell-wall), or a portion of it may be removed. The pabulum reaches the living matter more readily than it does under normal circumstances. The rapidity of growth of living matter—that is, the rate at which living matter communicates to inanimate pabulum the same powers with which it is endowed, varies simply according to the rapidity with which the inanimate matter comes into contact with the living matter. The soft living matter in normal tissues is always separated

from the nutrient pabulum by a certain thickness of matter which possesses no inherent powers of growth (cell-wall). If this be rendered more permeable to nutrient fluids, or reduced in thickness, or a portion of the surface torn away so as to expose the soft matter within, it is obvious that there will be greater freedom of access for the nutrient pabulum, supposing this to be present. The soft living matter will take up the inanimate pabulum more rapidly than normally, and convert it more quickly into living matter. In fact, normally, the conversion of pabulum into living matter proceeds *under certain restrictions*, while under the conditions supposed the *restrictions are to some extent removed, and so the process goes on faster*.

So far, then, from "stimuli," "irritants," or "excitants" causing *increased action*, they operate simply by removing restrictions, and thus the living matter is permitted to grow much faster than was possible under the conditions which existed previously. Irritation is not the starting-point of inflammation, but the first change is simply a more rapid appropriation of the nutrient matter by the living matter of the cells, consequent upon the impediments which interfered with the free access of this nutrient matter being diminished. The process of nutrition—the conversion of inanimate pabulum into living matter, may be explained both in health and disease without assuming the occurrence of an *irritation* or *excitation*, and we may study and describe the changes without using the terms *irritants*, *excitants*, or *stimuli*.

ON SOME
CASES OF SMALL-POX TREATED BY THE
SARRACENIA PURPUREA.

By H. CHALMERS MILES, R.A., F.S.S., L.R.C.P.E.

TOWARDS the conclusion of my paper "On the Employment of the Sarracenia Purpurea as a Remedy for Small-pox" (which was recently published in THE LANCET, with the courteous sanction of the Director-General of the Army Medical Department), I ventured to express an earnest hope that any of my professional brethren in civil life who possessed wide opportunities for collecting authentic data, would test the efficacy of the root in as large a number of cases as their practice permitted. It must have been obvious to everyone who favoured the monograph with perusal, that my efforts were directed towards placing before the profession, succinctly and without reserve, each item of information which I was able to gather concerning the medicinal action of the sarracenia purpurea, and its effects in variola, whether modified or pure. Any unprejudiced and candid reader will have readily observed that every sentence which was penned, every expression which was uttered, carried with it the single intention of conveying to the medical world every appreciable fact which the writer had stored on this novel and highly-important subject. A long interval—nearly a twelvemonth—has elapsed since the original communication introducing the "pitcher-plant" as a medicinal agent in small-pox was read to, and discussed at, the Epidemiological Society; and two months have passed away since the transmission from Canada of my second manuscript on this subject, and its publication in the pages of THE LANCET. It will therefore, I think, be acknowledged on all sides that no eager haste or unseemly precipitancy has been exhibited in this matter; and we may now anticipate that intelligent and impartial practitioners will give an early and ample trial to a remedy which, it is believed, will deprive this loathsome and dangerous disease of nearly all the horrors with which popular imagination invests it.

In furtherance of my desire that the profession should have the earliest information concerning the remedial action of the sarracenia, I beg to place on record a few brief notes of four very interesting cases of variola treated by the root, which have been kindly forwarded to me by Mr. S. K. Burch, of Shaftesbury-crescent, Pimlico; and shall be very glad if such cases are permitted occasionally to appear in THE LANCET, until such time as in my opinion the effect of this medicine on small-pox is placed fairly and intelligibly before the profession and the world. I make no comment on the cases narrated, while the practice of publishing only "*sample cases*" is altogether ignored.

Mr. Burch mentions that he did not see my paper until ten

* Part of a Lecture delivered during the Summer Session, 1862.

† See Lectures "On the Structure and Growth of the Simple Tissues."

days after it was published; but that on the 31st of October he commenced the administration of the sarracenia to Mrs. A., who had several days previously been attacked with "distinct small-pox of very severe type." On the 31st, when the sarracenia was first given, "the pustules on the face were so full that, although distinct, a pin's point could scarcely be placed between them. The secondary fever was very high." On seeing her the next day (the decoction of the root having been given at the strength recommended, and at the prescribed periods), Mr. Burch found "the change very great: all the fever had left, the pustules on the face were half the size and of a dark-brown colour, and the skin was to be seen between them, while on the legs they were entirely depressed and flattened. Many of them curiously rose again for one day, then flattened and desiccated. On the 4th of November they all dried up and fell off. The patient remarked that she felt better after every dose. The sarracenia did not act as a diuretic after the first day; but the tongue rapidly cleaned. Mrs. A. was delirious the first night she took the decoction, and her husband and nurse had great trouble in keeping her in bed, as she was so anxious to get at the medicine." There were no marks of pitting in any of the cases now related.

CASE B.—A lady, who lives in the same house as the former patient, took the sarracenia in mild doses as a *preventive*; but on the 11th of November the eruption (varioid) began to appear, though in a very slight form, about thirty or more pustules showing themselves in different parts, except about the lips, where they were confluent, and rapidly becoming purulent; they desiccated, so that on the 15th she was quite well.

CASE C.—A nurse in the same house also took the decoction as a preventive, and had variola in a still lighter form, and was ill only a few days.

CASE D.—"Sent for me on Nov. 14th. The varioid eruption was beginning to appear about the face, &c. It soon became confluent in many places, but about the body the pustules were distinct. On the 21st, having given the patient no medicine besides the decoction, there were no signs of desiccation or pitting; the pustules were purulent, but not filled out as they usually are on the eighth day; those on the legs were of a purple colour and small. She has been slightly delirious on several nights, but the fever has not been high. I told her to take the medicine regularly, when she said there was no fear of her not doing so, as she quite longed for the time to take it. On the 22nd the pustules began to dry up, and on the 27th were falling off rapidly; where they have done so there are no signs of pitting."

Each of these patients had been vaccinated. About Case A there is an additional circumstance worthy of mention. "The lady was between three and four months advanced in pregnancy. During the two days prior to commencing the sarracenia she hardly felt the child, and thought it dead, and also had a slight bloody discharge. After taking the decoction two days, she felt the child as strong as ever. This lady has been prematurely confined (between six and seven months) on two previous occasions."

Mr. Burch concludes by remarking—especially with reference to the last case—"Although the sarracenia has not acted quite so rapidly as I expected, still it has succeeded wonderfully, and will be a great boon. I am recommending all my medical friends to try it, and shall be happy to give you any further information, as I think the employment of the sarracenia purpurea in small-pox cannot be too much known."

Naval and Military Club, Clifford-street, Dec. 1st, 1862.

ON DIPHTHERIA.

By MICHAEL W. FISHER, M.D. GLAS.

THE mortality from diphtheria is still so great that any speculations as to its cause or hints as to its treatment may not even now be out of place; and it is because I differ so widely from Dr. Wade in the treatment of this disease, as recorded in a most valuable paper on this subject which appeared in THE LANCET of the 23rd of August, that I am tempted to note down my own experience, feeling well assured that I should have a far greater dread of it were I dependent on such a system of treatment as that recommended by Dr. Wade.

If, as I think, I have really discovered the cause of diphtheria to be the marsh malaria, it will be to others, and has been to me, much more satisfactory to have to do with the cause rather than to have to endeavour to eliminate an unknown poison in

an unknown manner by such drugs as iodide of potassium, cream of tartar, &c.

For my own part, I am most fully convinced that diphtheria has for its cause the malaria, which conviction has gradually forced itself upon me, *volens volens*. I have made no theory on the subject, and afterwards endeavoured to bolster it up with facts; but it is simply the result of a not very long experience in a locality where the malaria is a very pregnant cause of disease.

I would not have it thought that I am mistaking the kind of sore-throat named diphtheria, as I studied it under Trousseau before it had been well recognised in England, and have seen the system of treatment adopted in France almost, if not entirely, useless, both in their hands and my own.

The plan of treatment I adopt is usually as follows:—An emetic to begin with, consisting of equal parts of sulphate of zinc and ipecacuanha, which cleans the stomach and bowels of a large quantity of vitiated bile. The patient after this assumes a less nervous, less depressed appearance; the pulse expands and increases in force. Indeed, I believe this vitiated bile has much to do with all the unpleasant concomitants of diphtheria; so much of it is often voided and vomited that it would seem that whilst these medicines were unable to produce their ordinary effect.

I consider it of the first importance to clear out the alimentary canal; and do not hesitate to order a strong purge if needed, always being careful to avoid calomel or mercurials.

As soon as the vomiting has ceased, I prescribe fifteen minims of tincture of iron in water, to be repeated every three hours, or more frequently if the symptoms are really alarming. At bed-time I order the following draught:—Sulphate of quinine, fifteen grains; dilute sulphuric acid, half a drachm; tincture of opium, seven minims; water to an ounce and a half: to be repeated the second, fourth, and seventh days, and, if necessary, for a longer period at longer intervals. I rarely find it necessary to administer more than four such doses, and the throat usually begins to assume a healthy healing appearance after the second day.

Since adopting this system of treatment, I have lost but one patient—a child who was so refractory that the medicines were not taken; nor had a troublesome, tedious case, or one followed by any of the sequelæ so often met with after this so much dreaded disease.

I enjoin a liberal diet, especially milk, wine if attainable, if not, beer or porter, taken in moderate quantity.

I have not found the same satisfactory results from iron and quinine, or quinine, administered in the same mixture, nor with the quinine draught without the iron at regular intervals.

I do not now purpose giving my reasons for believing and stating that diphtheria is but a form of ague or intermittent fever. I have only desired to record the result of a very satisfactory mode of treating a disease so much dreaded both by physician and patient, reserving for a future time the results of my experience and reasoning on the subject of the malaria.

Dr. Wade seems to have met with many symptoms connected with diphtheria that I have often observed. I more particularly allude to notes Nos. 9 and 13, wherein mention is made of such symptoms as somnolence, coma, indifference to surrounding objects, albuminuria, suppression of urine, rheumatic swellings of the joints, &c., which tend to show that the same disease is due to the same cause in different localities. I have never observed croup as a complication of diphtheria; but I have seen it due to the same cause, and yield to a somewhat similar treatment.

The post-mortem appearances, as observed and recorded by Dr. Wade, are such as we would expect to meet with in a case of death from malaria. I have not been able to observe such appearances; but doubt not that some alteration of both liver and spleen might be found in every case of death from diphtheria.

In note No. 57, Dr. Wade states that "the faucial exudation should be considered as the local manifestation of a general disease." I entirely coincide in this opinion, and never adopt any local treatment, as nitrate of silver, nor have I ever seen any benefit result from such applications.

I would remark, lastly, that such a dose of quinine may only be prescribed after the bowels have been well cleared out, and the patient in bed, as in either case, if these precautions be neglected, vomiting is pretty sure to be the unwished-for consequence; indeed, some uncomfortable symptoms are generally experienced from the first dose, after which the medicine is well tolerated if not administered more frequently than here recommended.

Terrington St. Clements, 1862.