cannot satisfactorily treat severe surgical cases without the help of alcohol.

Numerous improvements in this age of active change are being made in the construction of our hospitals, and alterations in the system of nursing, which will lead hereafter to greater success in treatment, and a somewhat diminished death-rate, for that is the object we have in view; and I am rejoiced to think that it is so; but, so long as men get their bodies and limbs crushed and require amputations for compound fractures, or get their intestines pinched by a hernial stricture until the vitality of the part is hopelessly damaged, so long must there be deaths after operations; and, to repeat what I said at the beginning of my paper, I do not believe that large hospitals can be done away with, nor that, even when they are old as well as large, they deserve altogether the bad name that has been given to them.

THE INFLUENCE OF MODERN DOCTRINES OF INFLAMMATION UPON ITS TREATMENT.*

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ABSTRACT questions of pathology are perhaps more suited for quiet reflection at home, than for discussion at a meeting of this kind. At the same time we can scarcely fail to derive some advantage from the consideration of the doctrines of a process which, even now that it has been shorn of some of its vast proportions, still dominates over a large region of both medicine and surgery. The light which the vigorous nineteenth century questioning of nature has shed upon many regions of pathology, has illuminated none more highly than this. Morbid anatomy has shown that inflammation may exist, without giving rise to those symptoms by which its existence is, during life, best attested for the purposes of treatment.

The microscope has contributed in a high degree to more accurate knowledge of the minutiae of the inflammatory process. Indeed, we may go much further, and say that, without this instrument, these minutiae would be unknown and unknowable.

Observation of the natural course and progress of inflammations, in cases where they have been undisturbed by the administration of drugs, has furnished data of the utmost value to practitioners. Lastly, theories of inflammation, more especially those which involve a consideration of its relation to normal nutrition, have indisputably produced an effect upon the practice of us all, and in that of some of us have induced modifications so considerable, that it becomes of the last importance to inquire how far they are justified by clinical facts, and how far, also, they are logically justifiable, *i.e.*, how far they are necessary conclusions flowing from these theories.

The relation between fact, theory, and hypothesis, is well determined and simple; but it is so constantly overlooked in discussions of this kind, that I may be pardoned for briefly referring to it. Theory does not seek to make or invent facts, but only to arrange them, to determine their order, sequence, and etiological relation—how far any of them stand to others in the light of cause and effect. Facts without this arrangement, *i.e.*, without theory, are disorderly, chaotic, and of limited applicability.

Hypothesis, on the other hand, does invent (if we may use such an expression) facts, or rather it imagines that there may be facts, the existence of which is unproven. It does this to support a theory for which a sufficient number of ascertained facts do not exist.

Without staying to discuss the merits and demerits of hypotheses, and they have both, it is sufficient to remark that the principal danger of them is, that we too often fail to remember that they embody fictitious, and not real, facts. We may, with but little alteration, say of them, as has been said of words, they "are not things, but are very apt to become so."

The principal theorists whose views have, of late years, had a decided influence upon our practice, are Bennett and Virchow. One great object of both these able pathologists has been to determine the starting point of inflammation, and another to determine its essence.

Seeing that inflammation is a process—which word indicates that it may be divided into several steps, stages, or parts—and seeing that this process is one of great complexity, it may be doubted, and I for one do doubt, that any of these steps, stages, or elements (not even excepting the migration of the white corpuscles), will ever be found, which is of such pre-eminent importance as to entitle it to this latter distinction.

Still, in a less extended sense, there may be, and probably is, such

an essential element; in this way—all admit that, during the course of an inflammation, there are certain conditions of the local blood-vessels, which are also found in forms of disease to which the term inflammation has never been applied or considered applicable. In what way, *e.g.*, does inflammation differ from congestion or determination of blood? To this question, Dr. Bennett would answer—by the presence of exudation. And hence exudation might be termed the essential element of inflammation, as contrasted with congestion. This is a limited use of the term; whereas he considers exudation to be the essential element of inflammation, without any limitation. But here arises a difficulty: Bennett himself points out that in other cases there is an exudation from the vessels, as in cancer and tubercle, from which it follows that, admitting exudation to be the essential element of inflammation, we have between it and tubercle, or cancer, no line of demarcation, except the different subsequent developments of the exudation. We find also simple exudation, *i.e.*, exudation of the same characters, and with the same developmental tendencies, as the inflammatory, occurring in so orderly a manner, and with such an absence of disturbance of the circulation or of the nervous system, that no one would call it inflammation, and it could scarcely, if at all, be distinguished from ordinary nutrition. We come, then, to this: either everything is inflammation, or nothing. Dr. Bennett is disposed to choose the latter alternative, and to dispense with the term and idea of inflammation altogether.

I think these facts of great importance in a practical point of view, but would draw other lessons from them than those taught by Bennett. Here, I think, is a flagrant instance of words becoming things. It would be highly satisfactory to abolish inflammation by the act of a pathological parliament, if we could thereby abolish pneumonia, and peritonitis, and whitlow. As we cannot do so, we must see what more practical conclusions we can draw.

In the first place, we may see most plainly that there are all grades of the exudatory process, some of which so closely approximate to normal nutrition that they are undiscoverable by ordinary means, and demand no treatment. On the other hand, there are grades so tumultuous and disorderly in their development, that they are attended by phenomena, which render their recognition facile, and cry out for removal and relief. We must, then, as practitioners, and, I am bold to say, as pathologists, recognise two classes of inflammation, which we may, till some more precise phraseology be invented, rudely term: 1st. The pathological class; 2nd. The practical class. Of this latter, the prominent features are still, as in the days of Celsus, pain, heat, redness, and swelling. In the next place, we find that between these two classes there is no positive line of demarcation. Further, we must admit that, between the former of these classes and healthy orderly nutrition, there is no positive line of demarcation.

The absence of any abrupt line of separation may at first sight seem to the practitioner regrettable. It is not so; on the contrary, those who have practised longest, best know the limited range and powers of the weapons with which we fight. This continuity of actions encourages us to feel and remember the great restorative powers of nature, nowhere more visible than in the case of this pathologue of which we speak. Nature can often, unassisted, reduce to order the impetuous disorder of the inflammatory current; the more certainly so if we can, as we often may, remove the cause which first diverted nutrition from its normal course, or, if we can, as we often may, break one link of the chain of disordered action. We thus find that, although these theoretical views, pushed to undue and extravagant lengths, confound and paralyse us as practitioners, yet, on the contrary, when held soberly and moderately, they encourage and strengthen us.

It is not needful to enter at great length upon an exposition of the peculiar doctrine of Virchow. A few words will suffice. Exudation undergoes certain modifications and developments, such, for example, as its change into pus. Exudation is homogeneous and structureless; pus consists of cells floating in a liquid. Bennett believes that these cells arise from an aggregation of molecules spontaneously taking place in the exudation. When this latter, on the other hand, becomes converted into fibroid structure, he believes the fibres also are formed by molecular aggregation.

Virchow, on the contrary, holds that these structures—the pus-cell and the fibre—are offshoots from the original cells of the part, and that cell-growth may be the only microscopical phenomenon, as it is, according to him, the essential element of inflammation. This cell-growth is, however, tumultuous and irregular, the formative process being unduly active, the developmental one deficient. Hence, Virchow's theory I called the "cell-theory": his watchword being "*Omnis cellula cellula*"—every cell is born of a cell. It does not arise from any spontaneous aggregation of molecules, as Bennett would have us believe. I have no intention of raising the question, which of these views is the

* Read before the Birmingham and Midland Counties Branch.

more philosophical. I therefore do not say to which of these doctrines I rather incline. But I ask you to see that the cell-theory, quite as much as the exudation-theory, requires that we, as practitioners, should draw in some rude way that line of demarcation which nature has not definitely drawn between nutrition and pathological inflammation, between this and practical inflammation, and between either of these three and cancer, tubercle, and other pathological products.

It will be observed that neither theory tells us what force determines the first deviation from the course of orderly nutrition. It is probable that there may be divers forces acting in different cases.

Since inflammation is a process extending over a considerable space of time, we must recognise the fact that our treatment must vary according to the date at which we commence it. And, inasmuch as it is a complex process involving various tissues of an organ, we must be prepared to find that in one case one tissue, in another another may be chiefly affected; and that consequently it is obviously probable that we may, on this account, be obliged to adopt varieties of treatment.

But what I wish principally to enforce is, that, even if we grant that we have no drug or device which will act on the essential elements of inflammation, it does not follow that we may not be able to act beneficially upon other elements of so complex a process. For example, granting that we are unable to affect tissue-growth, we may yet be able, to the great benefit of the patient, to act by drugs upon the vascular or nervous elements. We have drugs, opium, antimony, mercury, aconite, salines, alcohol, purgatives, which do act, and very powerfully, upon these elements; and experience has taught me that this action is apparently beneficial. It is no answer to this to say: oh, but you are not treating the essential element of inflammation, but some non-essential element! My answer is, that it is true I am not treating an abstract idea or a fragment of tissue under the object-glass of a microscope, but a living man compounded of many structures and organs, which, even if remote from the primary disease, may be so compromised by it as to destroy his life. What harm would come of pneumonia of the apex of the lung if its effects did not radiate through the whole system? Why merely, at the worst, the destruction of an extent of lung-tissue, the loss of which would be no serious inconvenience. It may be that our drugs do no more than chain down the disease to its original ground, leaving it there to pursue its own unaltered course. This alone would be an enormous gain.

It is no argument to say that empirics who have preceded us have bled or salivated to death men who would have recovered untreated. We, I trust, enlightened by the advances of genuine science, shall avoid such excesses. If men in former days neglected or were ignorant of the use of alcohol in therapeutics, or if others, in later years, have used it in baneful excess, that is no reason why I should not employ it with a wise empiricism, not forgetting that, on the one hand, persons have been allowed to die from neglecting it, nor, on the other, that by its excess some have died who might otherwise have recovered.

Nor is it any argument against the use of these or any other drugs or methods to prove, if it could be proved, that they were first employed upon fanciful grounds, now shown to be untenable.

It is true that we have, of late years, been taught to appreciate the almost boundless resources of nature unassisted and uninterrupted. And the importance of this doctrine is almost inestimable. In no disease is this more manifest than in the one under consideration.

But, on the other hand, experience has shown me that many cases of inflammation have so little tendency to spontaneous cure, that the patient dies before the cure can be accomplished. Again, other cases, though they have this tendency to spontaneous recovery, do not recover without putting the patient to great and, as I contend, avoidable suffering. Take, for example, quinsy; and yet in no other disease have remedies greater alleviating power, and, indeed, if used early and boldly, the power of aborting the disease. Is it any answer to me, who believe this, to say that I have no direct power over the essential element of inflammation?

I had no intention in writing this paper, nor would the time at my disposal to-day permit me, to inquire into the respective value of different antiphlogistic drugs. What I did intend to do was to show that, as scientific practitioners, we are not precluded by the actual condition of science from employing these means, or others which may be suggested upon rational grounds as likely to be of benefit. But we must use them subject to the conditions imposed by the genuinely empirical method. What are these conditions? In a few words they may be stated.

We are in each individual case to watch the result; to abandon the experiment if the result seem to be unsatisfactory; not to continue it because authority has enjoined it, if to our common-sense judgment we find that, from some unknown cause, the result is different from what we had hoped and expected. In forming our judgment, we are to

make due allowance for its fallibility under the difficult conditions wherein we have to exercise it; giving, also, when we seem to have succeeded, due allowance to the cooperation of *natura medicatrix*.

The title of empiric was originally bestowed by their adversaries upon those who refused to be rigidly bound by the scientific dogmas of the day, when these dogmas appeared to be contradicted by the results of their own watchful observation.

We cannot now-a-days pretend to have so high an esteem for the then state of science, as to doubt that the empirics may often have been right. I trust that I esteem as highly as any one the vast labours of our own times and the wonderful scientific advances therefrom proceeding. But I feel that there is still scope for the empirical spirit. If, then, we, as practitioners, pursue the course above sketched out, we shall be justified in assuming the title of which some are ashamed, but in which I glory—the title of genuine empirics.

The true empiric is the intelligent minister of nature. The false empiric is the blind slave of art.

ULCERS OF THE LARYNX SIMULATING PHTHISIS.

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SOME cases of phthisis are accompanied by a deposit of tubercle in the trachea and larynx, and, as this deposit is usually followed by ulceration, such a condition accounts in some instances for the loss of voice in persons suffering from consumption; but, on the other hand, there are many cases of simple or of syphilitic ulceration of the larynx, in which symptoms are induced precisely similar to those due to a deposit of tubercle, and which, without the aid of the laryngoscope, cannot possibly be diagnosed from that disease. If the auscultatory symptoms of phthisis be marked, it is of course impossible to mistake the one disease for the other; but it is in those cases in which the chest-sounds are more or less masked, or in the earlier stages of phthisis, that a difficulty in diagnosis may appear. The importance of diagnosing between ulceration of the larynx and consumption cannot be overrated. The one—ulceration of the larynx—may, without proper remedies, remain persistent for an indefinite period, and may naturally become the cause of great uneasiness on the part of the patient and his relatives, whilst at the same time it is when detected readily amenable to treatment. The other—tubercular deposit in the lungs—is too well known to need further comment.

There is, as a rule, no pain accompanying ulceration of the larynx, unless the ulcer extend over the arytenoid cartilages, or into the œsophagus, when swallowing is productive of a sharp pricking sensation, which extends frequently into both ears. There is more or less constant cough, though usually of a dry character; and the cough, like that of phthisis, is worse on changing the atmosphere, as on entering or leaving the bedroom. Blood is frequently spat up with the cough, and is occasionally thrown off in larger quantities. The voice is often altered considerably. It may be soft and whispering, from the presence of œdema, or from thickening of one or both false vocal cords; or, on the contrary, it may assume the rough hacking character, from the inability of the patient to regulate the action of the true vocal cords. Such a condition may continue for months without abatement, and the system may now become affected. The patient becomes low spirited; he cannot converse, and he cannot properly attend to his duties; his appetite fails, and he becomes emaciated; he resembles one in consumption. On examining the chest, expansion is found to fall short of the expansion of health, but percussion and auscultation are unaltered. The following case, which occurred in the throat department at St. Mary's Hospital, under Dr. Sieveking and Mr. Norton, is a good example of ulceration of the larynx simulating phthisis.

H. R., a youth, nineteen years of age, pale and much emaciated, had been losing flesh for several months past, and been suffering from a cough for about a year and a half. He frequently spat up blood, but had never thrown up any large quantity at a time. His voice was of a hoarse, barking character. Swallowing produced no pain, but coughing was accompanied by a soreness in the throat. He complained of great exhaustion, and was too weak to attend to his vocation, which was that of a carpenter. An examination with the laryngoscope showed the right true vocal cord to be reddened, and superficially ulcerated in nearly the whole of its extent. The right false vocal cord or ventricular band was also inflamed, and upon it was situated a deep excavated ulcer of elongated form. In reply to a question, the patient denied that he had had venereal disease in any form, though the character of the ulcer led one to the belief that it was