

"That a cup of hot coffee is the best preparation for the fatigues of a march is indisputable, and it should never be omitted. It is much better that the men should have it before leaving their ground, and not at the half-way halt as was common in my time in India: it invigorates them at starting, protects, particularly the young soldiers, against the griping abdominal pains to which they are subject, particularly in the dark and chilly hour preceding the dawn; and the vigour it imparts helps the system to resist the miasm which at this hour is most freely evolved from the soil. It is worthy of remark that coffee was first issued to European troops for this very purpose, on the advice of the great Larrey, during Napoleon's Egyptian campaign."

It is said by some that my condemnation of spirits in the above passage "is too sweeping"; that to many, or at least to some, "alcohol in moderation is a wholesome and useful refreshment"; and, in the pages of THE LANCET, that "common sense and a knowledge of men's habits are sometimes more useful guides than the results of science," and on this ground the issue of a moderate allowance of alcohol to soldiers at the end of a march is justified. I may be permitted to say in passing, that I am not a temperance fanatic; I am not a "total abstainer" from the temperate use of wine and beer, although, perhaps fortunately for myself, so constituted as to be unable without swift punishment to use spirits.

In discussing a question of this kind affecting the interests and well-being of an army, we must disregard individual peculiarities—what suits this man or that man,—and look solely to what is best for the mass. It is a legitimate part of the argument to point to the fact that alcohol is the curse of the army wherever it serves: the fruitful parent of crime and disease. The same is said every day, and with truth, of men in civil life, although the amount of mischief under both heads, for obvious reasons, is more easily measured in military life. If it can be shown that in coffee or tea we have an article of refreshment that is equal, or nearly equal, to alcohol, it is surely an object of immense importance to encourage the use of the one that, according to the well-worn line, "cheers but not inebriates." It is quite true, as the writer in THE LANCET, already quoted, puts it, that a 42nd Highlander "would as soon drink red ink as red wine," and that during his march the prospect of a glass of whisky would be very pleasing "to look forward to"; and I am prepared to go a little beyond this, and to add that if he had the hope of two or three more on the top of the first, he would think it something still better "to look forward to." The question is not what from the pernicious effect of habit the soldier likes, but what experience and, if you like, "common sense," shows is best for him and the State, whose costly and valuable servant he is. This is the principle that should govern all our dealings with questions affecting health and discipline. Now, disregarding exceptional cases, I maintain that the evidence in favour of hot tea or coffee as a refreshment after fatigue, and their superiority in this respect over alcohol, is overwhelming. I have referred in the quotation from my lecture, given above, to the experience gained in the trying Algerian campaigns. Looking back to my experience among sportsmen in India, I cannot recall a single example of a spirit-drinker who was able for any length of time to expose himself with impunity to the sun, while it is notorious that abstainers from alcohol are capable of doing so, as a general rule, to a great extent.

In my lecture (when on the subject of sunstroke) I gave an instructive example. The troops holding Canton were called on to turn out at midday. A battery of artillery had more cases of insolation than all the rest of the force put together, the reason being that the canteen was opened by the officer commanding with the "good intention" of "fortifying" his men with a glass of grog before starting. My able friend, Staff Surgeon-Major Becker, an accurate observer, and in this case an eye-witness, is my authority for this pregnant fact.

I cannot agree with the writer in THE LANCET already referred to in his reference to science in connexion with this subject. As I have elsewhere said, science in this only confirms observation. The experiments of Dr. Parkes and the late Count Wollowicz, prove to a demonstration that even in temperate climates alcohol is a hindrance and not an aid to work. As many of my friends know, I am in the habit of spending my autumn vacation on the mountains

of the north, and although not quite so young as I have been, I have again and again walked my whisky-drinking companions, friends, keeper, and gillie, to a standstill. In one word, alcohol in moderation may help a man to put on a "spurt," but it is no aid to a hard day's work; and I devoutly hope that the day is not distant when a War Minister will have the courage to decree that the issue of spirits to the soldier shall cease and determine.

Netley.

## ON THE VALUE OF THE STAINING PROCESS IN THE HISTOLOGY OF THE MORBID BRAIN.

By HERBERT C. MAJOR, M.B. EDIN.,

ASSISTANT MEDICAL OFFICER, WEST RIDING ASYLUM, WAKEFIELD.

THAT the various tissues of the body are, in health, susceptible to the action of staining fluids to a different degree, is a point which has long been recognised by histologists, and one which has led, in the hands of many, to great practical results. But true as this is with respect to different tissues, equally true is the point and equally valuable the fact as applied to the same tissue under various morbid conditions. And yet it may be doubted whether those who have set themselves to examine pathological changes have availed themselves fully of the advantage thus afforded them. I refer more especially to the histology of the brain and the morbid conditions which that organ presents in various forms of insanity. In this great field of inquiry, so full of new and all-absorbing interest, it is of the utmost importance that every fact should be brought to bear, and each so directed as to act to the greatest advantage.

In no other department of histological research has the process of staining been more widely used or attended with greater benefit; but, nevertheless, I believe, and it is my present object to show, that its advantages may be still further extended.

That the grey cortical layer of the cerebrum is, under certain circumstances, with difficulty stained by the colouring solution employed, is a fact which has not, and indeed which could not escape observation. But there are conditions altogether independent of the state of the cerebral substance which may bring about such a result, while, on the other hand, there are others which favour the staining process; and all these it is necessary well to weigh and consider, in order to guard against the fallacies they might induce. By means of a strong staining fluid, it need scarcely be said, any nervous tissue may be deeply stained, and the same result will be obtained by prolonged immersion in a weaker fluid. But further, the susceptibility of the brain-tissue to the colouring solution will greatly depend on the method of hardening employed. It is a well-known fact that when preserved in chromic acid, the subsequent staining of the cerebral substance is less rapid and intense than when spirit has been employed; and hence it would happen that sections of the same tissue, on which similar staining fluid had been allowed to act and for equal time, might still vary considerably in depth of colouration, provided the hardening method employed were different in each case. I need hardly point out how, under these circumstances, any inference drawn from the relative intensity of the staining must be entirely fallacious and misleading. But as the fallacy is great if due precautions are not taken, so is the information derived weighty and valuable if uniform conditions are observed. To bring the matter to a practical issue: I believe that the method of staining the brain-substance, as usually pursued in investigating pathological change, is faulty; that, due attention being paid to observe uniformity in the hardening process and in the thickness of the sections, the object should then be, not to leave the sections in the staining fluid till "sufficiently coloured," but, having first decided by experiment the time required to colour a section of healthy brain with a solution of given strength, to allow the latter such time and no longer for its action in all cases. I have at the present moment before me sections representing cases of acute mania, epilepsy, general paralysis of the insane, and senile

atrophy of the brain, with respect to all of which I have followed the course above recommended. The staining fluid employed was logwood, which I first used on the earnest recommendation of my friend, Mr. Clifford Gill, of the North Riding Asylum, and the superiority of which over carmine, in this department at least of histology, admits I think of no question. It requires but a very slight examination of these sections, even with the naked eye, to make it evident that the action of the logwood has not been the same in all cases. In that of acute mania, for example, the grey cortical layer is deeply stained and accurately defined from the internal white substance, which is hardly coloured; but very different is the result as seen in the other specimens. In senile atrophy, for instance, the cortical substance is comparatively but slightly coloured, less so in some instances than the medullary portion, and the line of demarcation between them is imperfectly defined.

Other variations are to be noticed, neither with which, nor with the histological changes on which they depend, is it my object at present to deal. But I point to the fact as one of deep interest, and, as I think, of real scientific value. I would urge that the utility of staining should not be limited to the definition of cells, and the tracing out of their connexions, but that it should constitute, in some sense, a test for the cerebral substance, and thus play no insignificant part in the detection of pathological change.

West Riding Asylum, Wakefield.

## Medical Societies.

### PATHOLOGICAL SOCIETY OF LONDON.

TUESDAY, MARCH 3RD, 1874.

SIR WM. JENNER, PRESIDENT, IN THE CHAIR.

#### DISCUSSION ON CANCER.

THE discussion was introduced by Mr. CAMPBELL DE MORGAN, whose remarks appear in another part of our impression.

THE PRESIDENT.—This is a very important and interesting paper, and full of suggestions. I trust that it will elicit some remarks from members of the Society who have given their special attention to the subject. If I might be allowed to ask one gentleman who is present to address us, I would call upon Mr. Simon.

MR. SIMON.—Sir, I rise in answer to your summons, but it is the last thing I should have thought of doing except in obedience to you. Not because I should not wish to argue some of the points which have been raised in this paper, but because I feel that so very thoughtful and so well-prepared a paper can hardly be dealt with in extemporaneous argument. In reading Mr. De Morgan's thesis, I have noted certain points put forward by him; but I feel that it is almost impossible to deal with them adequately otherwise than in an essay almost as complete, deliberate, and careful as his own. But at least I am very glad to have the opportunity, in rising, of saying how very deeply indebted I think the science of this subject is to the clinical study of cancer which has been made at the Middlesex Hospital, and eminently by Mr. De Morgan himself. Mr. De Morgan has referred to doctrines which I think the researches made at the Middlesex Hospital have entirely put out of question—the notion of any direct origination of cancer in the so-called blood-poison. That doctrine was, a quarter of a century ago, put forward by various writers on grounds that probably even then were insufficient for what they pretended to support, but which now certainly are disposed of. Probably, however, in those days the writers who argued for the blood source of cancer did not so much mean the blood alone as the influence of the total system, the total material constitution; and if they had been pressed on the subject, they might have stated that they meant more than the blood taken quite separately from the solids of the body. But I think that perhaps Mr. De Morgan,

in putting aside the blood quite effectually as an exciting cause of cancer, has not done justice to the influence that the total constitution of the body has on the development of cancer in the sense of predisposition. When one looks on the one hand at the hereditariness of the disease, fully established as I suppose it is in a large proportion of cases, and when one looks on the other hand at the insusceptibility of bodies generally to the inoculation of cancer, I think one is justified in assuming that a specific predisposition is necessary. And that specific predisposition I suppose it is which is hereditary—not like the germs which, as Pasteur has observed, make the pebrine of the silkworm hereditary, germs traceable through the ovum and so on; not hereditary perhaps in that sense, but the predisposition is hereditary as affecting the quality and general constitution of the body. I feel it extremely difficult to follow so well-considered a paper without asking the meaning of particular parts of it. But it seems to me that Mr. De Morgan rather combated the doctrine of the specificity of cancer, that he attributed a great deal to an almost accidental mobility of its elements. First of all, as regards cancer, I may say that I would use the word in the old-fashioned sense, not in the modern and, I think, inconvenient sense, the strictly anatomical. I think we have rather come to grief in using the word in that restricted sense. We want a word that shall cover what our forefathers meant; we want a word that shall cover scirrhus of the breast, medullary sarcoma of an extremity, glioma of the globe of the eye, epithelioma of the penis. We want a generic word to cover those cases, and cancer is a convenient word. I do not know any other that we can use. We want a word that we can turn into an adjective. Has cancer, in that sense of the word, a specificity, or does it differ from what I would call common tumours in the mobility of its elements? Speaking, of course, in Mr. De Morgan's presence, with great diffidence, I would say that I believe in the specificity of it. I believe that there is an absolute difference between tumours which will be classed as monstrosities, and tumours which are zymotic, which are contagious. I fully agree with Mr. De Morgan that the differences between those two great divisions of disease are not always expressed in a form or in differences of form that we are able to trace. Some rough approximation to distinctions may be made, but broadly we cannot in a general way distinguish them in form, and for a reason that I think is obvious. Perhaps to that reason I may give a word or two. The anatomical characters of cancer are, speaking in very general terms, signs of local irritation. Whatever stimulus is applied to the part, the part can only give certain vegetative results. We know what changes a part can go through in the way of its products. It cannot give bricks and mortar; it cannot be heterologous in that sense; it can give overgrowths that shall be more or less embryonic; it can give, in the more or less embryonic tissue that it produces, evidence that its process is more or less tumultuous; it can give forms that are more or less abortive. When one has gone through a few heads of that sort, one has exhausted the possibilities of variation in nutritive processes. The tumours which may be called mere monstrosities, which are as truly in that pathological class as the supernumerary finger or toe, are, it is true, more apt to have what one may call adult tissue, and to have that adult tissue without a quantity of abortive forms. That is a question of degree, but there is—I entirely admit it—an absence of complete anatomical distinction. This stimulus of cancer, whatever it may be, goes to the part, and the part, according to its nature, according to its own structure, undergoes changes. Then you get, according to the structural type of the part, that alveolated structure if the organ is a glandular or epithelial one; if, on the other hand, it is one of the connective tissue series, you get a stromatoid tumour, one of its many possibilities; but if the part is subject to mere hypertrophy you get the same sort of thing. Again, relapsiveness is not the test of cancer. The monstrosities may relapse, the mere textural hypertrophies. I remember a great many years ago to have removed a little bit of fibroid tumour on the top of a lady's chest. It had been removed before, but it had come back again. Her relative, who was an eminent member of our profession, not being very young, doubted if his eyes were sufficient to deal with the case, and he asked me to deal with it for him. I removed the tumour, going widely into the surrounding