been formed is another; as to epithelial ingrowth, it is notably influenced by the relationships of the granulation tissue. As regards the formation of the pellicle in wounds coming under flavine treatment when already in the suppurating stage, some observers appear to be more familiar with this phenomenon than others and the differences in method of use which determine varying results require further elucidation.

So far as our observations are concerned and in the light of Drummond and McNee's and Carslaw's work, the importance of which rests both on the valuable therapeutic results obtained and also on the evidence of scientific attitude in seeking an appropriate clinical application for laboratory results, we feel that what we had anticipated for flavine antiseptics, and perhaps even more, is likely to be realised. It has not been suggested that from start to finish of any infected wound flavine ought to be applied continuously, and Drummond and McNee's work on wounds in the presuppurative stage, as well as Carslaw's observations on those already suppurating, afford a clear proof of the suggestion which we⁵ made, that the most valuable effects of flavine antiseptics are to be obtained in the first few days of application, and that later on, after serious infective processes have been prevented or overcome, it is advisable to

intermit their use or to substitute some other compound—thus, to prevent the appearance of sepsis and to control it when established are results which flavine antiseptics undoubtedly have shown themselves capable of accomplishing, with a very great measure of success, by comparatively simple procedures. Now that these important general points have been demonstrated by several independent series of observers, and that the broad lines of suitable application have been established, the way seems clear for further detailed work, which will also lead to the elaboration of methods for advantageously utilising the properties of these substances in other departments of surgery.

In conclusion, it may be added that in view of the preference shown by many surgeons for the use of antiseptics in the form of oily pastes, a procedure the rationale of which—apart perhaps from its "depôt" effect and its use in long narrow sinuses—is probably not yet fully understood, we hope that proflavine will soon be available in this form, e.g., as a fatty acid compound (oleate, &c.) which will be dissolved in oils and paraffins, or homogeneously mixed with these substances.

We are, Sir, yours faithfully,

C. H. BROWNING.

DAVID LIGAT

The Middlesex Hospital, Nov. 6th, 1917.

SPIRAL FIELDS OF VISION IN HYSTERIA, MALINGERING, AND NEURASTHENIA.

To the Editor of THE LANCET.

SIR,—In your issue of Oct. 20th Mr. W. M. Beaumont upholds the opinion expressed in Jones and Llewellyn's "Malingering, or the Simulation of Disease," that spiral fields of vision are characteristic of neurasthenia, in opposition to the view usually taught and quoted in your review of their book (THE LANCET, August 4th) that spiral fields are characteristic of hysteria. I am convinced that neither view is strictly accurate, as both narrow and spiral fields of vision are invariably the result of unconscious suggestion on the part of the observer. They are often produced in patients with hysterical symptoms, because, as Mr. Beaumont points out, such patients are generally abnormally suggestible, and they may be produced in neurasthenic patients, because they are abnormally easily fatigued and often abnormally suggestible in consequence, thus explaining the frequent association of hysterical symptoms with neurasthenia. Conclusive evidence of this view is brought forward by Morax, a distinguished French ophthalmologist, and formerly an assistant of Charcot, who was the first to describe these so-called ocular stigmata of hysteria. A true disciple of his great master, he continued for several years to find a narrow or spiral field of vision in all the hysterical cases he saw. But over 20 years ago he became a convert to Babinski's view that all the so-called hysterical stigmata are due to suggestion by the observer, and from that day he has not seen a single case of a narrowed or spiral field of vision. Babinski, who, as chef de clinique, had watched the development of Charcot's views on hysteria, soon felt himself forced to reject them, as he never found the so-called stigmata in any of a series of 100 consecutive cases of hysteria, which he examined in great detail by methods which excluded the possibility of suggestion. Morax adopted new methods to replace the perimeter, which it is practically impossible to use without at the same time suggesting a narrow field of vision, although, of course, the suggestion only takes effect in abnormally suggestible individuals.

In a small way my own experience has been identical with that of Babinski and Morax. I was in charge of the Neurological Department at Guy's Hospital for a year before I visited Babinski's clinique at La Pitié, and a week rarely passed in which I did not demonstrate one or more hysterical cases in which I had found hemi-anæsthesia and a narrow or spiral field of vision. In the nine years which have passed since then I have seen hysterical hemi-anæsthesia only once and I have never seen a narrow or spiral field of vision in a patient in whom these symptoms had not been looked for before he came to me, and my experience has not altered during the past 15 months, in which I have had several hundred soldiers with hysterical symptoms under my care.

I recently examined 17 healthy medical students who had not yet been in the wards or read any medicine, after asking them to pretend to the best of their ability that they were paralysed in the right arm and leg. On being pressed whether they complained of anything

FIG. 1.

Spiral contraction of field of vision in a case of admitted malingering.

FIG. 2.

Spiral contraction of field of vision in a case of hysterical fits without visual symptoms, in which the finger method, used before the perimeter, showed a normal field.

besides paralysis not one mentioned any disturbance of vision, but in answer to a leading question four said they saw less well in the right eye than in the left. Each of the four stated that it was a general deficiency of vision, and said nothing to indicate that he meant a narrow field of vision. Their fields were then examined by the apparently rough finger method, which has, however, the great advantage of suggesting nothing, and no abnormality was found. They were then examined with the perimeter in the ordinary way: in each case a typical narrow field of vision was found in the right eye and in one case in the left, but to a much less degree. In the only two cases in which it occurred to

me to continue the investigation after the first field had been marked out a spiral was obtained (Fig. 1) which was identical with that supposed to be characteristic of hysteria (Fig. 2). I expect that the reason why a spiral field of vision, which is the natural result of continued suggestion, has not been found in a larger proportion of hysterical cases showing a narrow field of vision is simply because it has not been looked for, the examiner being content when he has marked the limit of vision a single time in each direction.

Captain J. L. M. Symns and I have found in several cases of hysteria that an outward spiral was always obtained instead of an inward one if the white disc of the perimeter was moved outwards instead of inwards, as is commonly done. This proves conclusively that the ordinary inward spiral is the result of suggestion and not of fatigue.

In malingering some symptoms are produced voluntarily by the individual on his own initiative and others are produced voluntarily after being prompted by suggestions made unconsciously by the observer. The same two processes occur in hysteria, with the difference that they are involuntary, the symptoms being produced subconsciously, some by auto-suggestion and others by suggestions made by the observer. It is thus quite impossible to diagnose hysteria from malingering by a study of the symptoms alone.

I am, Sir, yours faithfully,

Netley, Oct. 21st, 1917.

ARTHUR F. HURST.

THE FUTURE OF THE POOR-LAW.

To the Editor of THE LANCET.

SIR,—It is time for those who wish to continue as Poor-law medical officers to wake up. There is a proposition which many are wanting to see acted on—viz., that the Poor-law service should become part of, and merged into, the present and future general insurance scheme. The majority (about 2 to 1) of the members of the Poor-law Medical Officers' Association do disapprove of any alteration. It would help if all Poor-law medical officers would send me each their opinion and wish in as few decisive words as possible. I could then tabulate them and find the wish of the majority and make the conclusion known in THE LANCET and *British Medical Journal*.

There is a promise of compensation for vested interests if lost by change of Act of Parliament, but the compensation is never equal to the office and money lost. It may be a compensation, but not an absolute *quid pro quo*. It is not right that those who have no personal interests should decide for those whose actual living and other details of work centre round a Poor-law appointment. It is easy to look down from a higher perch and wish to brush special departmental appointments into the general hubbub. This is just what a State service commandant would like to do. I say again that the future of the Poor-law will be influenced by the amount of resistance offered by its medical and civil officers.

I am, Sir, yours faithfully,

A. WITHERS GREEN,

Honorary Secretary, Poor-law Medical Officers' Association.

Wardrobe-place, E.C. 4, Nov. 2nd, 1917.

INSANE SAILORS AND SOLDIERS.

To the Editor of THE LANCET.

SIR,—Dr. J. F. Gemmel writes with feeling on this subject in your issue of Oct. 20th. I myself have an intense admiration and deep sympathy for our insane sailors and soldiers. So much so that when the Board of Control circularised asylum committees I advised our committee to suggest just such a project as Dr. Gemmel's. The Board of Control replied that such a project was not feasible for two reasons (if I remember correctly). The first was that the mental invalids would be out of reach of their friends, and the second that the numbers affected would be too great. I have always found the Board deeply interested in the welfare of the insane.

The points for consideration seem to me to be as follows:—1. To erect an asylum for these so-called "service patients" is to erect a monument to them. The public would call it the "soldiers' asylum." The stigma would remain and be magnified. The same result would follow if we kept an already-erected asylum for them. 2. Removal from War Office influence, from all reminders of war, would be bene-

ficial. This can be better done as the Board of Control suggests. 3. Documentary evidence, or the want of it, will not make these patients either more or less insane. And certification is protection. Certified patients are protected by the Board of Control. 4. From the point of view of the patients' friends the patients are best in the nearest asylum to them (and this can always be arranged under existing circumstances), and in the nearest cemetery after death. 5. Amongst my cases those patients in whom sufficient intelligence remains have preferred to go near their friends. 6. The term pauper is a misnomer. Many, I should say the majority of patients, in so-called "pauper asylums" are not paupers. Furthermore, most of them come from the same class as the majority of our sailors and soldiers. These will not feel their position as patients but merely as *insane* patients. 7. The allowance of 3s. 9d. above the maintenance rate is not all: 2s. 6d. is allowed for extra comforts and should be made use of. Perhaps even this is not enough, but no doubt time will tell and more may be granted.

I consider that *every* patient who has been in the Army or Navy honestly should be classed as a "Service patient." They did their best and broke down. All honour to them! Much can be done for them. Where there are many special wards can be allocated to them. Special privileges can be granted to them. Special kindness can be shown to them. If the letter of my old colleague Dr. Gemmel encourages other superintendents to do their very best for insane sailors and soldiers it will not have been written in vain.

I am, Sir, yours faithfully,

W. J. A. ERSKINE.

County Asylum, Newport, I.W., Nov. 2nd, 1917.

THE VESTIBULAR TESTS AND FUNCTIONAL DEAFNESS.

To the Editor of THE LANCET.

SIR,—I think that if Dr. P. McBride were to carry out the vestibular tests regularly in cases of perceptive deafness their scepticism as to their general usefulness as aids in the diagnosis of functional deafness might become modified. It is now about five years since I first drew attention to the interesting and still unexplained fact that the vestibular sense-organ seems to become impaired in harmony with the degree of severity of the cochlear disease in cases showing perceptive deafness due to organic disease. This phenomenon appears, as Dr. McBride says, even in cases of noise-deafness. On the other hand, I have shown, and I believe that the observation has been generally confirmed, that in functional nerve-deafness, whether due to hysteria or neurasthenia, the vestibular reactions are as lively as, or even more lively than, the normal.

To these broad general statements there are, no doubt, exceptions to be made, and if any of your correspondents are interested in the subject I refer them to the Proceedings of the Otological Section of the Royal Society of Medicine, and more particularly to the discussion on nystagmus (jointly with the Ophthalmological and Neurological Sections), where I had an opportunity of expressing views on the subject which subsequent experience has not materially modified.

I am, Sir, yours faithfully,

Nov. 6th, 1917.

DAN MCKENZIE.

References.—Proceeds. Roy. Soc. Med., Otol. Sect., vol. iii., p. 74, vol. v., p. 18; and Joint Discussion on Nystagmus, Feb. 26th, 1914.

THE SOURCE OF SPIROCHÆTES IN URINE.

To the Editor of THE LANCET.

SIR,—As one among others who have recently been engaged with the investigation of trench fever, I wish to make one or two criticisms of the paper by Captain A. T. Nankivell and Captain C. E. Sundell in your columns, containing their observations of spirochætes in the urine in cases of this disease. In the first place I should like to ask them whether in any of their cases they have ever found spirochætes of the type which they describe in a specimen of urine collected by catheter. Again I notice that in only one of their positive cases was the smegma examined for spirochætes, none being found. I have not seen, nor have I met anyone who has seen, this type of spirochæte in a catheter specimen of urine. On the other hand, in a case of my own—a recruit who