

Great War." Photographs and a plan of the building are included in the report. "The present rate of supply of houses in the borough is totally inadequate to meet present needs." Of 25 samples of milk examined for tubercle bacilli, three were found to be infected. In his school report Dr. Gebbie says there is a marked improvement in the general standard of cleanliness, but that inspections at frequent intervals are absolutely necessary to maintain the improvement. In July a committee of teachers arranged a visit to Wembley, and 353 children had to be examined by the medical officer; 51 per cent. of these showed evidence of successful vaccination. A few certificates were withheld on account of "nits" in the hair. The prospect of missing the trip and the energy of the school nurses in using Sacker combs at the clinic produced the desired result, and the withheld certificates were eventually granted.

Yeovil.

Dr. Charles W. Gee reports that the housing shortage still continues, and that a large number of houses are occupied by two or more families. The school care committee continues to provide from voluntary contributions much appreciated assistance

to necessitous children who do not come within the scope of the Poor-law. Dr. Gee says an open-air school is badly needed to deal with pre-tuberculous anæmic and debilitated children, and special classes in such a school for dull and backward children. "In the meantime playground classes should be utilised as much as possible."

INFECTIOUS DISEASE IN ENGLAND AND WALES DURING THE WEEK ENDED JULY 25TH, 1925.

Notifications.—The following cases of infectious disease were notified during the week: Small-pox, 65; scarlet fever, 1580; diphtheria, 868; enteric fever, 60; pneumonia, 606; puerperal fever, 44; cerebro-spinal fever, 4; acute poliomyelitis, 6; acute polio-encephalitis, 2; encephalitis lethargica, 50; continued fever, 4; dysentery, 5; ophthalmia neonatorum, 107. There were no cases of cholera, plague, or typhus notified during the week.

Deaths.—In the aggregate of great towns, including London, there were 6 deaths from enteric fever, 44 from measles, 8 from scarlet fever, 39 from whooping-cough, 22 from influenza, and 29 from diphtheria. There were 64 deaths of children under 2 years from diarrhoea and enteritis, as compared with 50, 44, and 29 in the preceding weeks. In London itself there were 9 deaths from diphtheria, 6 from whooping-cough, and none from influenza.

Correspondence.

"Audi alteram partem."

SURGERY BEFORE LISTER.

To the Editor of THE LANCET.

SIR,—In your review of Sir William Watson Cheyne's "Lister and His Achievement," being the first Lister Memorial Lecture delivered at the Royal College of Surgeons of England on May 14th, you say: "There is no reader of our pages but knows what surgery was before Lister's time. . . ." This, I think, is questionable, and the difficulty is to enable the present generation to realise the state of affairs that existed at that period. Even those who experienced something of the fringe of its horrors are apt sometimes to forget the advantages we enjoy to-day and to neglect some necessary precaution, with the result that we are pulled up suddenly only to realise their importance by a calamity. Well do I remember my first visits to a surgical ward in one of the smaller hospitals nearly 35 years ago, where the newer methods of wound treatment had not yet taken root. The surgeon pulling on the silk ligatures which had to be left hanging from the end of an amputation stump was a familiar sight and the regular application of linseed meal poultices to a suppurating tuberculous hip with its sour, sickly-smelling discharge. The earlier stage of a similar case left a still more vivid impression, for I can still see the surgeon incising a tuberculous abscess without any precautions whatever—for he neither removed his coat nor bathed his hands—and inviting me out of the kindness of his heart, for he was a great teacher, to thrust my unwashed finger into the freshly opened abscess so that I might feel the eroded head of the femur!

I have been told that in the old Royal Infirmary at Newcastle there was a "condemned" ward into which post-operative cases were put after a first rigor which so frequently ushered in a fatal septicæmia, and that patients commonly died of septicæmia after such simple operations as amputation of the toe, and that severe sepsis after any operation meant many months of painful suffering often followed by permanently impaired health. As Osler said, only those who have lived in the pre-Listerian days can appreciate the revolution which has taken place in surgery. All students ought to read the story of "Rab and His Friends," by Dr. John Brown.¹ There they will find in beautiful language an accurate description of an old-time operation for removal of

the breast with the most wonderful picture of the after progress cruelly marred by the onset of a fatal blood poisoning—a name more embracing and expressive than those now commonly used. I can also strongly recommend "The Edinburgh School of Surgery Before Lister," by Alexander Miles, published by A. and C. Black. From it they will gather a vivid picture of operations in the earlier days and of the great men bold enough to perform them. But there is still room for some work describing the state of wound treatment just before Lister, and it should be done now while those who can most vividly write the story are still with us.

Of course, mention is often made of the conditions existing before Lister, but we require an exact description with some detail as much for educational as for historical purposes. I may be ignorant of what already exists, but the sort of work I have in mind should be easily accessible and suitable for student reading. It would supply a real want.

I am, Sir, yours faithfully,
Newcastle-upon-Tyne, July 31st. G. GREY TURNER.

THE ÆTIOLGY OF CANCER.

To the Editor of THE LANCET.

SIR,—I think it ought not to be forgotten that ten years ago¹ Dr. A. S. Leyton and Dr. Helen G. Leyton isolated a streptothrix from sarcomata in rats and reproduced the disease in normal rats. Their procedure was briefly as follows: The press-juice from the tumour was passed through a porcelain filter, cultures were made from the filtrate, and the streptothrix grew. Rats were inoculated with the streptothrix and sarcomata reproduced. Acting on this brilliant piece of work Mr. W. Stevenson and I were able last year to cure a large sarcoma in a woman aged 38. The tumour was situated on the left side of the neck, and was so large that the patient's neck was turned to the right and fixed. Mr. Stevenson had reduced the tumour by about one-third of its bulk by radium. I then removed an intact piece of the tumour and obtained a profuse growth of diphtheroid bacilli (streptothrix). An antigen of this produced marked focal reaction in the tumour and general reaction. The tumour steadily retrogressed with the course of inoculations and completely disappeared. Some months later there was a recurrence in the glands of the left armpit from which I isolated the streptothrix again.

I am, Sir, yours faithfully,
University College, Dublin, July 24th. W. M. CROFTON.

To the Editor of THE LANCET.

SIR,—In your leading article of July 25th on the above subject you state "imagination can run on

¹ Reprinted in "Horæ Subsecivæ," second series (A. and C. Black), and in "Collected Short Stories," first series, "World's Classics" (Oxford University Press. 2s.).

¹ THE LANCET, 1916, i., 513.

almost indefinitely and in this case legitimately." With that before me, I venture to add to the flood of hypotheses with the hope that my addition thereto may not be regarded as illegitimately indefinite. I use, or it may be unwittingly pervert, the facts summarised in the Ministry of Health Memorandum (July, 1924) by Dr. J. A. Murray, by Dr. Mervyn Crofton in a paper¹ on Cancer, and the published work of Dr. A. Leitch.

There is no specific germ of cancer. The government of metabolism at sites in the animal body presumably varies in perfection, in accord with the grade of strain to which these are subjected in fulfilment of the respective functions subserved. Local irritation of mechanical, chemical, and toxic origin may be followed at certain sites by interruption of the sequence of processes of maintenance. In the human being more especially this may occur when these factors are aggravated by senescence and faulty dietetics. Independent of phagocytosis in the bloodstream, cell growth may be aided by ultra-microscopic microbes acting direct on effete matters, their influence on which may be restricted by toxic substances possessed by the healthy cell. In the presence of submaximal injury of cells as contrasted with their actual death, a struggle would ensue; in this the scavenger microbe may be at a disadvantage, toxic substances may gain the upper hand. When the process of irritation is chronic, the cells thus guarded would depend largely on physico-chemical processes (as illustrated in vitro experiments with fresh culture medium), and would assume a practically independent and insular existence with a scavenger service reduced to a minimum, if at all existent. Cells rich in toxin would guard the periphery. The toxic substance having fulfilled the function of protection locally might become surplus to requirements (as shown in the experiment in vitro of lack of growth in absence of fresh culture medium) and on entering the circulation would interfere with the scavenger microbe at the particular sites liable to overstrain in functioning, or owing to local irritation as defined above. Hence, there is no specific metastasis. On such an hypothesis cure of cancer would depend upon the judicious and intermittent flooding of the independent cell area (strictly within its confines) with the ultra-microscopic scavengers, in the endeavour to restore the balance of normal growth.

It seems to me that Dr. Gye's conclusion that *two* factors are necessary is met by this hypothesis, and is strengthened by the statement in the memorandum of the Ministry of Health that "the active agent behaves thus far like an ultra-microscopic virus, *but it has not yet been possible to produce the disease with artificial cultures of it.*" (Italics not in the original.) You seem to approach the subject much from the same point of view I express when, in your leading article, you state, "It may turn out that the virus does not normally exist in the body at all, but is generated within it in something of the same relation to the cells as the bacteriophage has to the bacilli."

A further tendency in this direction is found in your mention of the doctrine of Drs. Gye and Cramer involving "defence rupture" (kataphylaxis). My hypothesis would, however, regard Dr. Gye's "particulate bodies" as scavengers whose presence under normal conditions stimulates the production of a defence toxin in proportion to the zeal permissible on their part. When Dr. Gye injects both the particulate body (his "virus") together with the toxin (his "specific factor") he imitates, presumably, in healthy tissue the circumstance of the struggle of the scavengers and the defence toxin in the metabolism incident to submaximal cell injury; and is successful in securing the results he desires, because from the start he handicaps the particulate bodies by upsetting the balance with toxin in excess to the normal of the site. In this view it would be premature to regard the particulate body as a virus. Having regard to the largely accepted local irritation

theory in causation² of cancer, more light would be thrown on the subject were Dr. Gye's "virus" to be injected without his "specific factor" in tissue which recently has been subjected to submaximal injury.

This hypothesis may be *pour rire* when examined by experts; but I trust it will be regarded as quite as good as that which was accepted some time back as to the baneful effects of tomatoes in producing cancer!—I am, Sir, yours faithfully,

W. G. KING,

Hendon, N.W., August 3rd, 1925. Colonel, I.M.S. (Ret.)

To the Editor of THE LANCET.

SIR,—Dr. Gye's remarkable researches have aroused intense interest. They have a special interest to me through my association with the work, on lines very similar in theory, of my father, the late Dr. W. Ford Robertson. His paper¹ on the Relation of Carcinoma to Infections describes the result of inoculation of mice with cultures of an anaerobic diphtheroid bacillus isolated from five different human breast carcinoma and one rodent ulcer. He did not filter these cultures as he looked upon this bacterium, universally present under these conditions, as being probably the essential factor. Nineteen out of the 22 experimental mice were inoculated with pure anaerobic diphtheroid cultures in saline which had been subcultured from the primary broth into special anaerobic hæmoglobin glucose agar for the purpose of enhancing purity of culture. On the two remaining mice primary cultures were used. In the light of Dr. Gye's work it would appear that Dr. Robertson deprived the inoculum of some of its virulence, for of the six positive experiments in the group, the resulting tumours did not appear until a lapse of from 9 to 23 months, while in the two cases where primary cultures were used (presumably containing besides the anaerobic diphtheroid bacillus a filterable virus) the period for tumour development was as short as two to three months.

Dr. Robertson's experiments are remarkable, in that out of a total of 22 mice which were inoculated with human cancer anaerobic diphtheroid cultures, without any specific substance, eight developed carcinomata and one sarcoma, and a further nine developed hyperplastic tumours strongly suspicious of malignancy (13 of these being examined and verified by the Laboratory of the College of Physicians, Edinburgh), making a total experimental production of 36 per cent. tumours of definite malignancy, and a further 41 per cent. of suspicious malignancy. Control mice to the number of over 30 were kept for longer periods during the experiments without developing any kind of tumour.

With regard to the specific substance this must have largely been removed by the technique adopted in the majority of Dr. Robertson's experiments, and further, only two mice were inoculated with cultures that might contain specific substance, the others being treated with pure surface subculture in saline, which presumably could not have contained this material. Tumours were produced, however, but in a relatively much longer period, suggesting that owing to the removal of specific substance or its destruction by secondary organisms, further time was required for its redevelopment in vivo, either by unknown toxic factors, the diphtheroid itself, or the virus incorporated with it.

I am, Sir, yours faithfully,

W. MARSDEN FORD ROBERTSON.

Southport, July 28th, 1925.

To the Editor of THE LANCET.

SIR,—There are probably few pathologists in this country who have not already read the most interesting papers on cancer and the ultra-microscopic viruses by Dr. Gye and Mr. Barnard. The work, moreover, has been widely discussed and the results and conclusions

¹ The Bordeaux Congress, 1924.

² Brit. Med. Jour., 1921, ii., 929.

appear to be accepted as correct by many pathologists. All, however, who wish well for the reputation of British medical science will be anxious lest an over-enthusiastic reception should blind workers to any possible sources of error which a more considered judgment might suggest ought to be thoroughly probed before a work of this nature is accepted as having been *proved* to be correct. Nothing could do greater harm to the reputation of British scientists that they should be shown to be in error, possibly in a year's time, by some worker in a foreign laboratory. I feel sure, therefore, that both Dr. Gye and Mr. Barnard will appreciate the desire of some workers to be given a little more information on several crucial points, a satisfactory answer to which would go a long way towards convincing those whose critical analysis of the results leaves them in some little doubt.

In view of the fact that ultra-microscopic viruses will tolerate pure glycerine and to a certain extent ether, is Dr. Gye convinced that his chloroform-treated filtrate is quite free from the virus or contagion? Might not a small amount of specific contagion be left and be made active by non-specific products, including acids present in what he believes to be his cultures, in the same way that toxins affect the virulence of bacteria? Can he give us some experiments conclusively disproving this possibility? How many times has the experiment given in Chart 5 been made, and has the result always been the same? Have large quantities of the same treated filtrate only been inoculated, as controls, with negative results? Have cultures of non-cancerous tissue been tested side by side with cultures of cancerous tissue when the same treated filtrate was added in each case? In making subcultures from the primary cultures have any comparative experiments been carried out with subcultures made after heating the *primary* culture to such a temperature that any virus would certainly be destroyed? Would a fifth subculture from this fail to be made active by a chloroform-treated filtrate which for certain made active the fifth subculture derived from the unheated primary culture? Such experiments giving clean-cut results would, I suggest, be considered weighty evidence by most pathologists who may not be quite convinced by the result given in Chart 11.

This investigation of Dr. Gye and Mr. Barnard must have a considerable effect on all investigations at present being carried out on the ultra-microscopic viruses, and it is of great importance to other workers that their own investigations shall be carried along the right lines. I am sure if Dr. Gye is in a position to give us the information I have suggested it will be of great assistance to, and will be appreciated by, those working in other laboratories.

I am, Sir, yours faithfully,

F. W. TWORT.

The Brown Institution, University of London,
July 20th, 1925.

To the Editor of THE LANCET.

SIR,—Dr. Gye's paper in your issue of July 18th, in which he reports the very important results of his experiments on the passage of the Rous chicken sarcoma No. 1, raises a point of interest which has been in my mind since I first knew of his preliminary work, and which acquires more importance in the light of his more recent results with mammalian tumours. The point concerns the second filterable fowl tumour referred to in his paper—chicken sarcoma No. 7, of the Rockefeller Institute—a tumour which I originally transplanted and passed by filtrates, and the details of which were first published—without my consent—under the names of Rous, Murphy, and Tytler.¹ This tumour I have described separately.²

The original of this tumour was an apparently inert, cartilaginous nodule on the keel of the sternum, and during the first few passages the growth remained

entirely benign in type—well encapsulated, cartilaginous during the first few weeks of growth, ceasing to grow after six to eight weeks, and then becoming more or less completely ossified, with the production of marrow spaces and blood-forming tissue. At about the fifth or sixth passage, however, it quite rapidly began to take on malignant qualities, and in the course of three or four passages became transformed into a soft, rapidly growing and invading, undifferentiated tumour, and at this stage I was able to pass it by means of a Berkefeld filtrate. The tumour passed out of my hands at this time, and for information as to its later course I was indebted to my former colleague, Dr. J. B. Murphy, from whom I understood that it acquired most of the characteristics of chicken sarcoma No. 1, to which it now bore a striking resemblance histologically, but always retained certain specific qualities of its own.

The point I wish to make, in the light of Dr. Gye's work, is the possibility that this originally benign tumour may have become infected by the active virus of chicken sarcoma No. 1. Numerous fowls bearing the latter tumour—many with large, extensively ulcerated growths—were being handled in the same laboratory, the danger of contagion was not at that time seriously considered, and we did not have the conception of a virus which could outlive the activity of the tissue preparations in use.

The suggestion is entirely speculative and impossible of confirmation, and would, indeed, have a very limited interest in connexion with the fowl tumour alone. But Dr. Gye's work leads us to hope, as I understand, with some justification, that he will be able to demonstrate the activity of his virus in the production of mammalian tumours, and to extend the results of his work to the whole field of malignant growths. In this case, the point I have raised may perhaps supplement in a small way the suggestion contained in his epoch-making work that contagion in cancer may be a factor of importance, a view which will no doubt appear revolutionary to the medical profession as a whole, as it does to me.

I am, Sir, yours faithfully,

W. H. TYTLER.

King Edward VII. Welsh National Memorial
Association, Central Tuberculosis Laboratory,
Cardiff, July 20th, 1925.

To the Editor of THE LANCET.

SIR,—It is a great pity that your leading article of July 25th did not appear in the same issue as Dr. Gye's communication. The communication was accompanied instead by a leading article which took a purely one-sided view of the question, accepting Dr. Gye's hypothesis as well as his facts, and even going beyond them in suggesting that the "specific factor" may be produced from irritated tissues, whereas Dr. Gye had produced it only from pre-existing tumours. Your article of July 25th took a more judicial attitude, and suggested that there might be another explanation of Dr. Gye's facts than his hypothesis of an extrinsic parasite, but, as far as I have seen, no notice of this second leading article has been taken by the daily press. The impression has thus been transmitted throughout the world that Dr. Gye has discovered the cancer parasite, and this impression is intensified by the unqualified support of the leading medical journal in the kingdom.

Pathologists are apt to ignore, or to regard with contempt, the views as to the constitution of living matter which are accepted by biologists. These views are that the cell is not the ultimate unit of life but is itself composed of different orders of smaller living units each of which is capable of growth and multiplication. Some of these units such as chromosomes, centrosomes, Golgi bodies, chondriosomes, &c., are visible under the microscope and their growth and multiplication can be studied directly. Others are invisible, but their existence is supported by very strong evidence.

According to Weismann the ultra-microscopic particles (determinants and biophores) are specific

¹ Jour. Amer. Med. Assoc., 1912, lix., 1793.

² Jour. Exp. Med., 1913, xvii., 466.

in character and determine the nature of the cells of which they form part. Since these views were first promulgated they have received remarkable independent confirmation in the facts of Mendelian heredity. There is little or no difference between Weismann's "determinant" and Mendel's "factor." Both are intercellular and ultra-microscopic, and capable of growth and multiplication. The "determinant" determines the nature of the cell and the "factor" determines the character with which it is associated. It appears to me that here we have an explanation which is in better accord with Dr. Gye's facts than his own hypothesis of an extrinsic cancer "virus."

Since cells can be cultivated under artificial conditions apart from the organisms of which they form part, there is no a priori reason why it should not be possible to cultivate these ultra-microscopic units apart from the cells. It appears to me that Dr. Gye's facts would be explained equally well on this supposition as they would be on his supposition that the "virus" is an extrinsic agent; and, for some of his facts, my suggestion offers the easier explanation. Cancer cells presumably contain an ultra-microscopic "factor" or "determinant" for cancer, and it is quite possible that Dr. Gye has succeeded in cultivating this factor, since he has not brought forward any evidence that his "virus" is an extrinsic germ.

The great majority of pathologists have always recognised that the known phenomena of cancer are incompatible with the theory of an extrinsic specific causal agent, and it does not appear that Dr. Gye's facts have removed the objections to this theory. To take one point only, it is now possible to induce cancer in animals without taking thought of any parasite. If an extrinsic parasite is to be considered necessary for the production of cancer, this organism must be present at all times either in all animals or else in the atmosphere. Otherwise it would be impossible to produce cancer without the specific introduction of the cancer parasite. There is, thus, very strong evidence in favour of the view that this so-called "virus" is not an extrinsic parasite, but is a constituent part of the cancer cell itself. This view is in accordance with the phenomena of cancer, whereas the hypothesis of an extrinsic agent is not. The question whether the virus is extrinsic in origin or is derived from the cancer cell itself is one of extreme importance, and it is much to be regretted that Dr. Gye's article was not submitted to more adequate criticism before being published, so that both sides of the question could have been presented to the public at the same time.

If my suggestion is correct, this work of Dr. Gye opens up vast possibilities in the way of cultivating Mendelian factors in vitro.

I am, Sir, yours faithfully,

CHARLES POWELL WHITE.

Manchester University, August 3rd, 1925.

To the Editor of THE LANCET.

SIR,—Dr. Gye's recent investigations have been of special interest to me, because I believe his virus will prove to be identical with the ultra-microscopic phase of the complex micro-organism which I described in 1921. This micro-organism I have obtained from a large number of cancers of all types. I have shown that the larger elements (coccal, bacillary, fungal) which soon appear in any ordinary culture medium containing a piece of cancerous tissue can be traced directly from minute elements, which emerge from the cells, or indirectly from an amorphous material, which escapes from the cells as globules and rods and which in this early stage is often curiously resistant to ordinary stains. This unstained "plasm" often first appears in and escaping from the dying cells as minute elements just on the verge of visibility. A study of these facts led me to write in 1922 that "the organism has, during its parasitic phase, acquired the faculty of

infecting individual cells and of living in a sort of symbiotic relationship with the cells which it inhabits. This conclusion . . . goes far to confirm the view which I advanced in a previous paper (1921) that the organism lives parasitically in a minute phase which is unrevealed by ordinary methods of staining." The globules and rods can sometimes be seen to emerge from the cells with a clearness that is diagrammatic. The organised forms (coccus, bacillus, yeast) often spring from refractile elements, which appear in and are detached from the globule and rod. Glover of New York and his colleagues have recently described a microbe which they have obtained from all types of cancer and whose general characters are similar to mine.

A remarkable feature of this micro-organism is that the alternative forms, although springing from one common stock, can pursue each an individual and stable life as coccus, bacillus, or yeast, and they may resist any efforts to change them, although I have frequently during five years' study convinced myself that under certain conditions any one form can pass over into any other form. These facts are foreign to ordinary bacteriological teaching and have made my views uncongenial to many bacteriologists, although there is a great deal of evidence in the literature to support them (Löhnis, Mellon, de Negri, Hort, Almquist, &c.). It is, I believe, apparent from the recent literature that fixity of form in bacteria is illusory as a criterion of specific characters, for as Löhnis, Mellon, I and others have shown the alternative phases of the same organism can often and probably usually pursue an independent true-to-type existence. It is sometimes urged that such a conception is opposed to the facts of biology. This is not so. It is opposed to much of the traditional teaching of bacteriology, but it is obviously in conformity with the great biological fact that a multitude of different cellular elements, each capable of independent propagation, are commonly derivable from one common germ-plasm. The further back we go in the world of life-forms the more we find the differentiated cell retains its multipotential characters of the original plasm. In the bacteria this retention would seem to be complete. The essential resemblance between the primitive bacterial matrix and the germ-plasm of higher life led me to name the former the bacterioplasm.

The first indications of these facts in regard to the cancer parasite came to me when I discovered that "plasm" rod or globule, minute granule or thread (in this stage strikingly similar to Rickettsia), bacillus or coccus could apparently, depending on the culture medium and other factors, be derived indifferently from the cells of the same piece of cancer. The confirmation of these facts came with the discovery that from the same "plasm" all the different forms were derivable. In this stage the refractile granule is a common index. During this phase the germinating "plasm" is easily mistaken for masses of debris to which the bacterial forms have adhered. As Löhnis has pointed out, in the past it has been commonly looked upon as dirt.

Sir, with the attention now being paid to the cancer parasite I look forward confidently to an early confirmation of these views first published in 1921. For long I have been urging that a similar reorientation of the bacteriological mind would probably quickly resolve the difficulties surrounding typhus, influenza, small-pox, &c. If it be true that Weigl, Breinl, and Fejgin have all succeeded in deriving the *Bacillus proteus* (X 19) commonly associated with typhus from the Rickettsia of typhus, we have the first augury of the unexpected facts which will transpire with the application of this broadened outlook. We can safely prophesy big developments along the same lines in the near future.

That familiar bacteria may possess a filterable phase is suggested by the work of Heymans (*B. anthracis*), Valtis, Vannucci, &c. (*B. tuberculosis*), Almquist (*B. typhosus*), Hort (*meringococcus*), and Löhnis. The very striking investigations of Löhnis

make it likely that all bacteria have a filterable mode of life; it may even be that this is the essential parasitic form of all bacteria.

I am, Sir, yours faithfully,
Edinburgh, August 1st, 1925. JAMES YOUNG.

THE PRINCIPLES OF DYNAMIC PSYCHOLOGY.

To the Editor of THE LANCET.

SIR,—An opportunity, all too brief, occurred for the discussion of psychotherapy at the meeting reported in your last issue of the Medico-Psychological Association in Birmingham. Some of the opinions then expressed may be taken as typical of the progress the medical profession as a whole has made in the acceptance of the principles. Ten years ago it was difficult to obtain a hearing in medical circles for a paper dealing with the value of suggestion. The successes obtained during the war by the exponents of the Seale Hayne school and others, in the treatment of the conversion neuroses altered this, and the profession was bound to admit that suggestion had a definite therapeutic value. Having thus swallowed suggestion whole the profession appears inclined to rest there. If one may judge by the majority of practitioners one meets, the general attitude may be summed up by some such statement as this. "There are certain somewhat eccentric persons who have the power of curing neurotic conditions by suggestion, and there are certain nasty-minded cranks who believe that they can do good by means of a mystic ritual called psycho-analysis. Anyway, neurotic patients are a nuisance and if a bottle of bromide cannot satisfy them, let them go where they will."

There are those of us, Sir, who hold a different opinion. We say that considering the vast amount of misery, social and economic loss, caused by functional nerve disorders, the apathy of the profession, including most of its teachers, is nothing less than tragic. We know that suggestion at the best can do no more than remove some obvious symptom, and does nothing to enable the patient to adapt himself to his environment. It has no effect on phobias or anxieties, it will not cure the psychasthenic, or remove a tendency to criminal outbreaks. In order to treat such conditions as these one must have a knowledge of the workings of each individual patient's mind. One must recognise the existence of a dynamic Unconscious, and realise that the symptoms represent for each patient the satisfaction of some inner need, the solution of some unconscious mental conflict. We realise that an unhealthy body is not without influence on the mind. We do not minimise the importance of tracking down and eliminating all septic foci. We are aware that a dose of calomel may effectively purge melancholy, but we see too many individuals who have been subjected without benefit to every possible minor and major surgical procedure to believe that the root cause of a neurosis can be somatic.

Surely the time has come when students should not leave the medical schools without having learned something of the principles of dynamic psychology. Men when they go into practice will find themselves daily confronted with conditions which neither medicine nor surgery can solve. They will be consulted by women and men whom no examination, however thorough, will convince that they are not the victims of hidden cancer or tubercle. They will be asked to prescribe for adolescent girls who lose both appetite and sleep unaccountably. Healthy youths will present themselves with tales of woe which do not fit in with anything which the student has learned. These, as well as the readily recognisable hysteric and neurasthenic, will try his patience and waste his time and theirs, before taking their troubles elsewhere, unless he has the necessary psychological knowledge to deal with them. More important still, he will have many opportunities of dealing with the beginnings of neurosis in childhood, but he will pass them by.

The teaching I would advocate need not be definitely committed to the theories of either Freud, Jung, or Adler. Once imbued with the idea of the Unconscious as a dynamic force and not a lumber room, the interested student will pursue the subject on his own lines. I would emphasise, however, that as regards the lecturer, the terms neurologist and psychologist are not interchangeable, and it is for his psychological rather than his neurological knowledge that the lecturer should be chosen. An important part of the course would deal with the training of the young child, as it is during the nursery years that the foundations of most neuroses are laid, but something too would need to be said as to the part played in the anxiety states by the sex life of the adult. I am aware that post-graduate courses on these lines are in existence in some centres, but such lectures touch only the fringe of the profession. Such training as I advocate would at any rate enable the student in after life to recognise these cases when he sees them and enable him to advise treatment, even if he did not care to carry it out himself.

It is a mistake to think that there is no choice between neglect and a formal psycho-analysis. An astonishing amount of psychology can be dispensed over the counter, as it were, with lasting benefit to the recipients. Patients are frequently astonished, but never resentful of questions asked about their sex life, and show themselves extraordinarily grateful if the physician is able to point out where the shoe pinches. A varying amount of mental analysis is required in other cases and sometimes may be combined with suggestion, whilst in some other types the analysis must be carried to rock bottom. These last should be referred to recognised experts, but unless we train our students where are these experts to come from? Already this branch of therapeutics is overrun by men and women who have had no medical training. Some of them are honest and competent workers; others are not so, and bring disrepute upon a science which by its very nature arouses instinctive opposition. The value of psycho-analysis is freely recognised by educationists, the clergy are fully alive to its social uses; how long will the medical profession continue to turn the blind eye?

I am, Sir, yours faithfully,
Birmingham, August 4th. R. MACDONALD LADELL.

PUBLIC HEALTH OF MARKET DRAYTON.

To the Editor of THE LANCET.

SIR,—I enclose report of a recent conference at Hales Hall (the rural dean in the chair) which throws some light on the causes of high infant mortality for some years past at Market Drayton, but does not solve the question, "Why does the Minister of Health steadily refuse to publish the report as to the high infant mortality at that little town, made by his own medical inspector last autumn?" We infer from a statement made by Dr. Wheatley, the M.O.H. for Shropshire, on May 9th, that "bad housing and ignorance" were the principal causes, and that "the health education" should begin in the elementary school (*Newport and Market Drayton Advertiser*, May 9th, 1925).

At the conference, reported in the issue of the same newspaper on July 25th, we gather that the medical inspector's report recommended more education, probably including the education of mothers at the infant welfare centre, and help for that centre. The report, we are informed, also stated that "the sanitary record is poor"; and it "suggested the appointment of a woman guardian." Surely this report should be published in full; the ratepayers should be told what improvements should be made in administration by the guardians which a woman guardian could assist; ignorance should be dispelled, the interest of the inhabitants enlisted, and the lives of the infants saved.

I am, Sir, yours faithfully,
August 4th, 1925. J. THEODORE DODD, M.A., J.P.