SOME PROBLEMS IN THE STUDY OF PSYCHOTIC ILLNESS*

By

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I am deeply appreciative of the great honour you have done me in electing me as your President for the coming session. Although very sensible of this honour I am also much aware of my own shortcomings when I remember the distinguished men who have preceded me in this office. The following contribution, I am afraid, contains very little that is original. The purpose of this paper is rather to emphasize a point of view in the study of psychotic illness, which does not always seem to receive adequate attention.

It is only in recent years that some medical schools and universities have had the opportunity of carrying out investigations on psychotic patients by reason of the fact that in some instances they have acquired in-patient departments or have worked in conjunction with mental hospitals. The history of the development of research, especially scientific research, in the psychoses in this country is an interesting one. In the last three decades of the nineteenth century and up to the outbreak of the first world war there were, in addition to the Bethlem Royal Hospital, two outstanding psychiatric institutions where psychiatrists with a sincere interest in their specialty would invariably receive their training, apart from going abroad. They were, of course, the West Riding Asylum, Wakefield (now Stanley Royd Hospital), and the Royal Edinburgh Asylum. In the universities and medical schools generally, little or no interest was taken in the subject, the clinical practice of the specialty being largely carried out by neurologists.

You are all very familiar with the history of the Bethlem Royal, and with the limited time at my disposal I only propose to give the briefest details of other hospitals in so far as they have a bearing on this paper. Firstly, with regard to Wakefield, this hospital was opened in 1818, having been built on the lines suggested by Mr. Samuel Tuke of The Retreat. Its first Medical Director, Sir William Ellis, later became Superintendent of Hanwell Asylum in 1831. During the time of the next three Medical Directors, Drs. Corsellis, Alderson and Cleaton, there were no noteworthy developments, but it was during this period from 1857-58 that Dr. Henry Maudsley was on the staff of Wakefield Asylum as a temporary house surgeon, and apparently even during that short period made a great impression. He was awarded a testimonial by the Committee in which they expressed great admiration for his ability.

From the point of view of research the most important phase in the history of Wakefield Asylum started with the appointment of Dr. J. C. Browne (later Sir James Crichton Browne) as Superintendent in 1866. One of the unsuccessful applicants for the post at that time was Dr. T. S. (later the famous

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Sir Thomas) Clouston. It was during Crichton Browne's ten years of office that Wakefield began to acquire European fame. Indeed it may be said that he, Crichton Browne, was the first ever to pursue scientific research in mental diseases in this country. At that time it must be remembered that as far as the organic field was concerned there was no bacteriology or biochemistry, and physiology was only rudimentary. Crichton Browne naturally therefore concentrated on morbid anatomy and pathology, and on attempting to correlate clinical manifestations of mental disease with abnormalities in this field. He was responsible for the opening of the famous Pathological Laboratory and for the appointment of the first pathologist, Dr. T. W. McDowall. In his Annual Report to his Committee in 1872 Crichton Browne writes: "The appointment of a pathologist which you have thus sanctioned is, I believe, a somewhat momentous step in the march of scientific progress in the lunatic asylums of this country. As far as I am aware, no other asylum is yet provided with such an officer." (In fact he was 20 years ahead of other institutions.) Crichton Browne not only fostered research in this hospital but also kept his medical staff up to date by inviting prominent lecturers and doctors to hold discussions at the hospital. In this way he was undoubtedly able to create a milieu which fostered a spirit of enquiry, and helped to maintain the medical work at Wakefield on a high plane. A few words about this remarkable man might not be out of place. During his medical education he was a pupil of Lister and also studied at the University of Paris. His medical career started in 1861 and he was medical officer successively in Devon, Derby and Warwick Asylums. As stated, in 1866 he became Director of Wakefield where his clinical and pathological investigations paved the way for the later researches of some of our greatest neurologists. In 1876 he was appointed a Lord Chancellor's Visitor in Lunacy, a post he held until 1922. This may seem a very strange promotion but by then he had already made a name for himself, and in 1878 he was President of the Medico-Psychological Association. In 1883 at the proposal of Charles Darwin, he was elected a Fellow of the Royal Society. He was given numerous honorary degrees and was one of the founders of that famous journal *Brain* which he edited for a time with Hughlings Jackson and David Ferrier. He had a large number of interests and his literary output was prolific even in advancing years. He was a gifted orator and his writings bear the imprint of accurate thought and clear expression and of considerable literary ability. He gave the first annual Maudsley Lecture in 1920. I was privileged to see him once at an R.M.P.A. meeting in 1934 or 35. His Dundreary whiskers and Victorian garb, together with his erect bearing even when he was well over 90, was a sight never to be forgotten.

It is beyond the scope of this address to enumerate all the famous men who passed through Wakefield Asylum in the next 30 to 40 years, but it is perhaps not always remembered that the late Sir David Ferrier was one of the earlier workers in the Wakefield laboratory and here carried out in 1873 his epoch-making experimental researches on the localization of cerebral function. From 1872 to 1915 no less than 23 pathologists received their training at Wakefield, among whom figured Edwin Goodall in 1890–91. In 1876 Crichton Browne left and was succeeded by Herbert Major as Director. He had already been in the service of the hospital since 1871. He had apparently acquired a considerable reputation by his histological researches in conjunction with William Bevan Lewis, who was appointed the fourth pathologist in 1876. It is interesting to note that during his period of office reference is made to his lectures on mental diseases at the Leeds School of Medicine. Bevan Lewis

succeeded as Director in 1884; he acquired a greater scientific reputation than any of his predecessors or successors at Wakefield for his extensive, painstaking, accurate and detailed description of the neuropathology and neurohistology of the various forms of psychosis. In this field Bevan Lewis was a leading exponent, whether in this country or abroad. He devised a method of staining frozen sections of cortex with a British dye, aniline blue-black, which was superior to any the Germans had manufactured, and he demonstrated what he called the "scavenger" cells, now known as astrocytes. The lipoid inclusion bodies were regarded as the remains of phagocytosed degenerated nerve cells. There is no doubt that Bevan Lewis can be regarded as a pioneer in this field. Much preliminary work was done showing certain changes in acute psychoses, and others in chronic dementias. Whether all these changes were secondary, due to intercurrent disease, to post-mortem change or to some toxic action, is for the neurohistologists to decide. Much of his research was incorporated in his two textbooks, first in 1882, *The Human Brain: Histological and Coarse Methods of Research*, and secondly his well-known *Textbook of Mental Diseases*, first published in 1889, a second edition of which appeared in 1898. Bevan Lewis retired in 1910 and was succeeded by Joseph Shaw Bolton, who while at Claybury, had done some important work on the structure of the cerebral cortex in mental diseases. He also invented an important modification of the Weigert-Pal stain for myelinated fibres which could be applied to frozen sections. He continued in the tradition of Wakefield and in 1914 published his well-known book entitled *The Brain in Health and Disease*. He retired in 1934. It is sometimes forgotten that during his period of office he initiated, in conjunction with Leeds University, the first D.P.M. examination in this country.

Meanwhile, in 1893 Sir Thomas Clouston, who for some time had been Superintendent of the Royal Edinburgh Asylum, secured the services of Dr. William Ford Robertson, who had for some years worked at the Edinburgh Royal Infirmary in pathology. In 1894 Ford Robertson commenced active work in collaboration with Dr. James Middlemass on the pathology of the nervous system in relation to mental disease. He was chiefly concerned with the neuropathology and histology of the nerve cells and nerve fibres. Ford Robertson's research gained him an immediate reputation. Clouston was quick to realize the importance of this work, and with his colleagues initiated what was known as the Scottish Asylums Pathological Scheme, with a joint laboratory and pathologist, for the promotion of study and research into the pathology of the insane. William Ford Robertson was appointed as the first pathologist under this scheme in 1897. Grants for assistance soon came in from the Medical Research Council, the Carnegie Trust and other bodies. He retained this post for a quarter of a century and, as is well known, later diverted his attention to bacteriology, especially to the actions of the toxins of diphtheroid bacilli.

In 1893 a sub-committee was set up by the L.C.C. to advise on research in pathology in the L.C.C. asylums, and also to encourage research by young medical officers. This report was eventually approved by the Asylums Committee and the expenditure of £4,000 was authorized for the building of a pathological laboratory and museum at Claybury Asylum. When the laboratory was opened, Dr. Frederick (later Sir Frederick) Mott was appointed the first Pathologist to all L.C.C. asylums and Director of the Laboratory. All his early work was done at Claybury, for although Henry Maudsley made his generous donation in 1908, which eventually resulted in the foundation of the Maudsley Hospital, Mott was not able to transfer his laboratory

there until many years had elapsed, albeit before the Maudsley Hospital opened officially in 1923.

By 1910 scientific tools available for somatic research in mental disease had undergone a considerable change; moreover Freud, Jung and Adler were already looming on the horizon. In the years before the first world war neuropathology had seemed to have spent itself and bacteriology was on the wane, but biochemistry and physiology were beginning to assert themselves. It was at about this time that another epoch in the investigation of psychotic illness in mental hospitals began. A new mental hospital at Whitchurch, near Cardiff, was opened under the superintendency of the late Edwin Goodall who had received most of his earlier training at Wakefield. Goodall was alive to the possibilities of the new science of biochemistry and persuaded his committee to build some first-rate laboratories and to appoint Dr. R. V. Stanford as a full-time research biochemist. While maintaining their interest in general pathology and bacteriology Stanford and Goodall concentrated their efforts on the abnormalities in the body fluids of psychotic patients. Stanford and his colleagues carried out much useful work and had support from the Medical Research Council. Thus, in 1912 he demolished the hypothesis that indicanuria, so often found in the insane, was due to intestinal putrefaction, and showed that these indole compounds were really a product of abnormal tryptophane metabolism. I mention this because today biochemists are much pre-occupied with the metabolism of these compounds with an indole ring. However, little notice was taken of his paper at the time, There can be no doubt, however, on the whole that the laboratory came a little before its time. Ignorance in our basic knowledge of biochemistry (e.g. enzymology, carbo-hydrate metabolism, energy-producing mechanisms, etc.) was too much of a handicap for any real advances to be made. By the time his successor, J. H. Quastel, a pupil of Gowland Hopkins, took over in 1928 the position had improved and tremendous progress in this field was to ensue in the coming 30 years. This research department first initiated by Goodall has now grown into the well-known M.R.C. Neuropsychiatric Research Institute.

It might seem irrelevant to the subject under discussion that the development of these mental hospital departments should be stressed, but it is done firstly to demonstrate that up to the end of the first world war and a few years after, the medical schools and universities had done practically nothing to promote teaching, study or research in psychiatry, and secondly to show that a few mental hospitals properly equipped and staffed can adequately serve not only as a training ground but also as centres for research in psychotic as opposed to neurotic illness. From 1920 onwards until the 1930's but few new developments took place in the mental hospitals. Whereas in the psychiatric out-patient departments of the teaching hospitals and general hospitals interest grew in the new movements of dynamic psychology and sociology, in this aided by Freud, Jung and Adler, the mental hospitals were still very much isolated and, with one or two honourable exceptions, had no out-patient departments up to and until January, 1931, and they dealt solely with certified psychotic patients. Their influence in the scientific world was definitely on the wane, especially owing to the development of large post-graduate hospital and teaching hospital departments. That the centre of gravity had moved to the teaching hospitals cannot be doubted. I would like to quote from Professor Mapother's address to this Society in 1934 regarding scientific research: "The first point to emphasize is that opinion concerning psychiatry in England is far too largely formed by those in practice. Therefore not only is it unduly

optimistic, but of a kind which dispenses with the laborious observation and experiment that forms the basis of every progressive science. It must be recognized that under modern conditions really important research cannot be a hobby for the leisure of those in either institutional or private practice. The army of science cannot be a militia—techniques are too difficult. Likewise it cannot be entirely a short service army."

The development since that time to the present is familiar to all. Scientific work in mental hospitals was definitely not fashionable any longer. The advent of new physical treatments gave much more immediate returns. All scientific work seemed to be centralized in one gigantic post-graduate hospital. Chairs of psychiatry were founded in many provincial universities, a number of medical schools acquired psychiatric beds and rosy views about psychiatric research in universities were put forward up and down the country. A new era seemed to be dawning in the field of research into mental ill-health.

In most aspects of our discipline this optimism has undoubtedly been justified. The advances made, for example, in physical treatments, rehabilitation, psychosomatic medicine, epilepsy, child psychiatry, group therapy and the like, are well known to everyone. When we consider the problem of the functional or endogenous psychoses, however, the picture is entirely different. It is unreasonable, and one might say in present circumstances inconceivable, to expect the medical schools or university departments with their strictly limited number of beds to undertake long-term serial scientific research on psychotic patients, which involves daily biochemical, neurophysiological and clinical investigations, spread over many weeks or months, or even a year. Experience has shown that isolated observations on a large number of psychotic patients have only been of value in a limited number of instances, or even perhaps of greater value in providing exercises for our statisticians, but in so far as they give us a clue to the metabolic changes in these patients they have been of no value at all.

The credit for starting the long-term intensive investigation must go to Dr. Rolf Gjessing, whose death last March was a great loss to psychiatry. In this connection I would like to quote what the late K. F. Scheid, who worked at the Kaiser Wilhelm Institute in Munich, said in an article in 1937. Scheid was the author of a well-known monograph on "Febrile Episodes in Schizophrenia". Speaking of what he called the concept of the polyphasic reactions of a pathological process and the need for serial investigations, he says: "The recognition of this technique in the investigation of the somatopathology of the schizophrenic psychoses has only very lately begun to bear fruit. Only Gjessing has taken seriously the demand for serial investigations, and in this way he was able to demonstrate interesting results. On the other hand, by far the greatest number of German investigators have confined themselves to a statistical evaluation of diagnosis in which a certain chemical or physical method has been applied to show how these results were characteristic of schizophrenia itself. However, as we are, throughout, dealing with polyphasic reactions one is bound to reach contradictory results, quite apart from the fact that, during a period of time, the changing morbid processes could not be recorded." This is the independent view of someone who had for years adopted this particular technique. I may be forgiven, perhaps, for quoting what Gjessing himself wrote in 1950. Speaking of the schizophrenias he says: "It is clearly not enough to investigate a small part of the whole complex of functions for a few days or weeks only. We cannot assume that functional (i.e. metabolic) disturbances are the same at the beginning, at the climax or in the terminal stages of an illness, for instance in hypertension, diabetes or tuberculosis. So

long as we do not know which disturbances are of importance we have to record a series of representative functions from several ranges at the same time. These functions may be more or less related to each other but probably not always to the same extent. The correlated psychic condition must be recorded day by day along with the rest, even if the psychosis itself is a terminal symptom occurring when the fundamental metabolic disturbance has reached the limit of psychic adaptation." His classical work on periodic catatonia is well known to everyone.

In view of the state of confusion with regard to the diagnosis of schizophrenia Gjessing warned us that unless we are sure of the homogeneity of a group of patients it is better to start with a single one. He maintained that such a complete and well-planned investigation of the endogenous psychoses is the only one which is likely to yield any results. In the same paper he goes on to say about the problem of schizophrenia: "Team work by specially trained workers in physiology, biochemistry, enzymology and clinical research is essential, together with enough assistants of every sort—research workers keen on the task and willing to work like desperadoes even for years on this particular problem. It is an undertaking for which the State will have to defray the expenses. However, if we regard the problem even from the viewpoint of the national economy, we can assert that as the State is spending millions annually on the unproductive permanent maintenance of mentally disabled patients, it would not be unreasonable to devote, say, one per cent. of this expenditure to finding out if it is possible to reduce the main cause. Every accurate observation, seemingly unimportant, may be of value because it may lead to further knowledge. Investigation of this kind may give no rapid returns but the reliability of the results will compensate for that. Many short cuts have been tried during the past half century without success, and now an investigation on a really adequate scale is justified." This is the main thesis on which scientific research in a number of strategically placed mental hospitals should be based.

There are few who today would argue against the view that the functional psychoses are caused or precipitated by humoral changes in the organism, however or wherever these may originate. Indeed this was the view of Henry Maudsley as expressed in his book (1867). The opinion that they are entirely of psychogenic origin must now be regarded as unreasonable in the light of our present knowledge.

The last decade has seen a great surge forward and a renewed interest in neurophysiology, neurochemistry, neuropathology and above all in neuropharmacology. With the exception of neuropharmacology the advance in these disciplines has been chiefly on the experimental and fundamental side. The Institutes in the United States, notably those at Bethesda, St. Elizabeth and Illinois, together with some of the newly-founded professorial chairs at the Maudsley Hospital, exemplify this development. Mental hospitals, on the other hand, are doing less scientific research today than 30 or 40 years ago. It cannot be true that this is due entirely to a lack of enthusiasm: rather has there been a shift of emphasis because the scientific approach seems to have been much less rewarding in giving immediate results. Many progressive mental hospitals today are preoccupied with therapeutic trials and experiments in community care, group therapy and rehabilitation, and the phenomenology and ecology of mental disorder, whereas previously laboratory work was emphasized. Clinical psychologists and social therapists are now multiplying everywhere. It would be foolish to deny that these developments have been of immense and lasting benefit to our psychotic population in hospitals, but is it right

that scientific work in the disciplines mentioned above should be almost entirely discouraged? The view is taken in some quarters that serious scientific research must not be undertaken at the periphery, but must all be concentrated in the university centre. The argument is that keen young men with ideas and a flair for original work must be in constant touch with experts and receive the benefit of constant interchange of ideas and healthy criticism, whereas this is not possible for the research worker ploughing a lonely furrow in some distant mental hospital. This may be true in theory but in practice it is far from being the case. Further, research into psychotic illness has been entirely equated with research in other disciplines such as internal medicine, pathology, ophthalmology, etc. As long as this view predominates little or no progress can be made in the multi-disciplined long-term investigation of the endogenous psychoses after the Gjessing pattern. Progress has been made even more difficult by the administrative set-up since the National Health Act was introduced. Whereas the medical schools and universities have not suffered, the few mental hospitals engaged in this type of work have to endure a general lack of interest and understanding on the part of some Regional Hospital Boards. Except for the most elementary form of clinical research, schemes cannot be entertained or staff be appointed by the hospital authority unless the project has been vetted by one or more advisory committees, one of which is almost certainly to be a research advisory committee composed largely of non-psychiatric medical men, the majority of whom lean towards the university centre, and have, understandingly enough, a complete lack of appreciation of the problem. Even research grants from well-known voluntary bodies, who have put up money for a project sanctioned by their own professional advisers, may be the subject of this veto. It would be unfair to blame the Regional Hospital Boards for this state of affairs. Their primary duty is to administer the hospitals of their region efficiently. They are not usually bothered with this perplexing research item from other disciplines. Their responsibilities in providing the necessary facilities and allowing the use of certain hospital services are freely admitted, but whether it is reasonable to place upon them the onus of deciding whether a research project should be supported is indeed open to question. Furthermore, one gains the impression that Regional Boards and their officers would rather be without this invidious task. Where a mental hospital with scientific interests is not linked administratively to a university department a new approach, therefore, to this problem seems ripe for reconsideration. A more positive line by the Clinical Research Board could well be pursued without necessarily infringing on the democratic principles at the periphery.

It might well be asked what are these lines of research that should be undertaken in well-equipped and strategically placed mental hospitals. It is clearly beyond the capacity of one individual to give this answer. All one can say is that within recent years promising results have been obtained with new techniques and methods which are particularly well adapted for the serial and prolonged study of psychotic patients. This implies that a research worker appointed, in biochemistry for example, must have facilities for adequate technical assistance, not only for applied work in the clinical field, but also for pursuing basic and experimental research, and enjoy the same freedom which is enjoyed by his colleagues at university centres. There should be other special departments in a hospital of this kind, particularly electroencephalography, psychology and even neuropathology, to assist in the recording of data in a long-term serially devised undertaking. Lastly of course a fully trained clinician

must serve with this team. Mapother in 1934 suggested that each of those selected for knowledge of a particular science should have a training of a year or two in psychiatry so that he shall have a live feeling about its real problems.

There is no lack of projects, but it would be found of great advantage to investigate day by day and week by week over several months psychotic patients who show a periodic variation in their mental states, and to attempt to correlate any changes in the scientifically recorded observations with alteration in clinical status.

To refresh your memory I would like to refer to Gjessing's results in periodic catatonia. Details of his thorough and systematic approach can be read in the original. His experimental set-up, which it was my privilege to see at one time, was such that it attempted to eliminate all sources of error. A large number of observations were made daily, e.g. pulse rate, blood pressure, body temperature and bodily movements, basal metabolic rate, blood sugar, alkali reserve, and the excretion of nitrogen, sulphur and phosphorus. The main finding was that in this form of catatonia there was a marked nitrogen retention, a large amount of nitrogen being stored in the body. After a certain period, when the retention reached its peak, the store was completely emptied, being excreted in the urine in various forms. This coincided with a change of phase in the illness, usually the onset of stupor. His work has largely been confirmed by others, but apart from his findings the main interest lies in the new methodology he evolved.

This method has been applied in other fields, e.g. in the study of water metabolism in epileptics carried out by Greville and Tudor Jones, where three epileptic patients were examined daily for water retention and water excretion, two over a period of 5 months, and one for 3 months, control observations having been made on one non-epileptic patient. The object of the experiment was to determine whether there was any connection between the incidence of grand mal seizures and the exchange of water between the epileptic organism and its environment. The original paper can be consulted. The main findings were that changes in the fasting body weight under controlled conditions afforded a fair measure of the changes in body water. At most, 10 seizures out of 22 were preceded by a rise in the weight curve, and 10 out of 22 by a fall. This painstaking and time-consuming work rebutted the findings of Stubbe-Teglbjerg and demonstrated that the retention of water does not by any means precede a seizure. A negative water balance was observed after more than half of the isolated seizures. Unfortunately at this time electro-encephalography was in its infancy; continuous tracings could not therefore be done. In this instance then, the periodicity was supplied by the occurrence and recurrence of fits.

More rewarding subjects of study are manic-depressives, especially those cases showing recurrent attacks, circular and alternating insanity. H. Tomasson, as early as 1924, demonstrated disturbances of ionic equilibrium in blood electrolytes in manic-depressive psychosis, and claimed that the deviation from normal occurred before the onset of the pathological phase. This involved months of daily estimations and accurate clinical observation. He was specially interested in the calcium-sodium ratio of the serum, and he claimed to have shown that, as he put it, "the heightened emotional tone was accompanied by increased serum calcium and diminished serum sodium". He claimed to have demonstrated that there is a marked alteration in the calcium-sodium ratio which profoundly affects the metabolic and especially the vegetative functions. These changes may of course be secondary to other metabolic disturbances,

but at any rate they are facts which are of interest and can fit into any general metabolic projects on manic-depressive psychosis.

In 1954 John Dawson and his colleagues in Leeds, starting from the hypothesis that there might be a disturbance in carbohydrate metabolism in manic-depressive illness, investigated the blood acetoin (i.e. acetyl-methyl-carbinol) in both the manic and the depressive phases of the illness. Their investigations up to now have shown that there is an increase of the acetoin level in depressions and a decrease in the manias, as compared with the normal. Further studies under controlled conditions are now being conducted by Dawson at Runwell, in association with his colleagues on the clinical side and in the EEG and psychology departments. It is hoped that by such longitudinal studies further and more accurate data will be available showing the variations not only in the type of illness, and whether the changes precede the illness or not, but also the effect of treatments. At the same time, experimental and fundamental work is going on in the laboratory on the acetoin metabolism and the effect of potassium depletion. A great help in this work has been the provision of a small metabolic ward of two beds with the necessary nursing staff. Such a ward being only about 30 yards down the corridor from the main laboratory is an enormous advantage to the research worker.

During 1956 and 1957 Weil-Malherbe and the writer were able to show differences in the urinary excretion of catechol amines, that is adrenaline and noradrenaline, in the manic and depressive, and normal phases, in 3 patients with periodic attacks of manic-depressive psychosis. In two of these the manic and depressive phases alternated rhythmically. They were studied serially, and in one case for more than a period of six months. In addition, a number of grouped cases were investigated in which a number of samples were obtained during the pathological phase and after recovery. A significant increase of sodium and potassium excretion during the manic phase was also observed in one patient. Crammer, working in Birmingham, studied sodium and water metabolism in 2 chronic psychotic patients with recurring mental disturbances. Weight loss was accompanied by polyuria with increased secretion of sodium chloride, weight gain by the opposite. In one patient Crammer demonstrated loss of sodium at the beginning of an attack of depression, in the other just before the emergence from a depressive state and the start of a hypomania. He suggests a disturbance occurring centrally, probably in the hypothalamic endocrine system. It is also of interest to note that, in view of the great attention paid to serotonin metabolism recently, we found the excretion of its breakdown product, 5-hydroxyindoleacetic acid, increased in the manic phase in the two patients examined for this. Our determinations of this substance, however, were of a preliminary nature and the results need to be confirmed. The *main* constant result obtained in this investigation was a higher excretion of adrenaline and noradrenaline during the manic phase than during the depressed phase. There was also a tendency, although we have made no claims, for the excretion of adrenaline in the depressed phase to be lower than in the normal phase.

Bergsman, working in von Euler's department at the Karolinska Institute in Stockholm and largely with mental hospital material, has within the last two months published a monograph entitled "The Urinary Excretion of Adrenaline and Noradrenaline in some Mental Diseases". These patients were not examined serially, but samples were taken at least twice in the majority of cases during the pathological phase and four times in one case of mania. The results confirm the increased output of these two amines in the manic

phase, and the diminished output in the depressive phase as compared with normal controls. They also investigated schizophrenics, acute and chronic, senile dementias, phenylpyruvic amentias and neurasthenias, and the reaction of all groups to injection of insulin. As far as manic-depressives are concerned these two conditions are regarded as at opposite poles for respective excretion of adrenaline. He tends to the view that there is an endogenous factor which is responsible for the high and low production of adrenaline respectively. The results obtained by Bergsman agree closely with those obtained by us.

No doubt many more examples of this type of work could be given: for instance, the serial work in endocrinology such as was carried out by Hemphill and Reiss, repeated electroencephalographic recordings in conjunction with clinical and biochemical investigations before, during and after specific treatments. The spate of drugs now on the market affords ample opportunity for this type of study. and indeed such investigations are already under way. Surely it is only by such means that we can continue to collect facts which may eventually lead to a more rational form of treatment.

As one last word about the scientific approach to psychotic illness I would like to make a strong plea for the more extensive use of the science which has been grossly neglected in this country for the past 40 years, namely neuropathology. Professor Sir William Le Gros Clark stated in Oxford in 1952 that "the fundamental requirement is the study of human brain material, particularly when we know quite a lot about the patient during life". He stressed that a more intimate knowledge of the anatomical organizations and actions of certain regions of the human brain which remain very obscure may contribute quite important information regarding the physical correlates of mental disorders. Professor Meyer also described long-term studies of individual variability in the cortical cyto-architecture of normal brains. He suggested that this variability may not be haphazard, and that if we had the patience and facilities for such time-consuming investigations we might well arrive at interesting correlations between abnormal personality and brain morphology. With modern developments in staining technique, in cytochemistry and the advent of the electron microscope, there is undoubtedly a vast field of work waiting to be done. The advent, too, of neurosurgery in mental illness provided a stimulus for neuroanatomists and neurophysiologists. Degeneration in the dorso-medial nucleus of the thalamus has been demonstrated by the results not only of standard leucotomy but also following the more circumscribed operation of orbital cortex undercutting, at least in two cases. This links with experimental work on the connections of the thalamus, hypothalamus and the frontal cortex. The central connections of the temporal lobe are virtually unknown, and suitable material must eventually come to hand from patients who have undergone temporal lobectomies. The few neuropathologists working on these problems cannot possibly cope with the many projects waiting to be done, both in psychosis and mental deficiency. The advantages of a close link-up between a university department and a hospital in the field must be obvious to everyone.

Some of these projects may be rewarding in the near future; most of them, however, are only part of a process of building up facts and data for further elaboration which may take decades, if not generations. Each advance made now will give fresh clues for others to work on, and the modest but definite information that has been obtained in these fields convinces me that further effort is certainly worth while. One of the greatest dangers is expecting too much too soon, as this may divert both men and money into other fields.

In the foregoing an attempt has been made, however inadequately, to put

before you examples of the kind of project that seem to me to be eminently suitable to be undertaken in a mental hospital research department, and to be complementary to that being carried out in our universities and post-graduate schools. The question of recruitment of young men and women of the right type to this work inevitably prompts a brief word on undergraduate and post-graduate education. If one has had an opportunity of learning from the reactions of men and women who pass through the various grades of appointment in a hospital one gets quite a good general impression of their attitude towards both undergraduate and postgraduate education, and a great deal can also be learned from the current opinions held by our consultant colleagues in general hospitals.

Firstly, as regards undergraduate education, I have no doubt that Professor Sir David Henderson summed up the situation extremely well when he said in 1952, and I quote: "I think the first thing to do is to get the co-operation of our medical colleagues. Ours is still a branch of work that is not regarded in university circles as being of anywhere near the same standing and importance as general medicine, surgery and the rest of it. I think we should educate our own profession in regard to the place that psychiatry has to take. I believe that every medical student coming into the medical school for the first time should know that there is something in addition to a body, that there is a mind, and that later he may be required to treat the disorders of the mind that are likely to occur in his practice, whether he is in general practice or any other branch of medicine." A little later he says: "There should be an examination in psychiatry as part of the final clinical examination." No one in this hall I believe would argue against Sir David Henderson's thesis, when one remembers that more than one-third of the cases seen by a general practitioner are neurotic or psychosomatic in nature, and that over 40 per cent. of the hospital beds in this country are occupied by psychiatric patients.

The standard of our undergraduate education has now reached a high peak of excellence. Chairs have been founded in provincial universities and much has been done under difficult conditions, but the psychological effect on the student of having to learn a subject in which he is not required to take an examination is, I venture to say, damaging to the status of our specialty. It must be unique in any academic circles in the world for a professor to instruct undergraduate students in a subject in which they are not required to take an examination. In this way the position of our subject in the curriculum continues to suffer, particularly among our consultant colleagues in other branches of medicine. Furthermore, what is lost during undergraduate days can never quite be regained by postgraduate instruction.

Postgraduate facilities available in this country are some of the best in the world, but more emphasis could be laid on informal rather than formal education whose aim is a specific examination. One can only point out what seem to be anomalies. Whilst special examination is vital and the higher qualification very desirable it may be argued that an abnormal emphasis is placed on a higher qualification in general medicine. One finds that many young people take a large amount of study leave and spend unnecessarily a great deal of time in attempting to obtain such a qualification. If the undergraduate teaching in general medicine is sound and the postgraduate experience in a general hospital is adequate this should in all conscience be enough for anyone who wishes to specialize in psychiatry. If this rather outmoded insistence on a vast knowledge of general medicine is necessary it can only have a frustrating effect on many of our younger aspirants to psychiatry. There are

other disciplines which they could study with greater advantage, or, alternatively, better use could be made of study leave to visit clinics abroad, rather than to be preoccupied with the complications of smallpox or the mysteries of collagen diseases. Again, conditions must be made attractive and it does not help to denigrate the mental hospital and to encourage the view that mental illness of whatever kind can equally well be treated in a general hospital, which more often than not lacks the special facilities required for all types of cases. A sense of proportion is required if we are not to do a disservice to the public. The fact that a mental hospital has to keep its failures has a salutary effect on therapeutic enthusiasts *vis-à-vis* their colleagues in a psychiatric unit of a general hospital, whose failures can be transferred to the appropriate mental hospital and be forgotten.

However things may turn out after the new legislation is introduced there can be no doubt but that the mental hospitals will continue to treat the bulk of the functional psychoses who require a fairly prolonged stay. This has been confirmed in countries where psychiatric in-patient units in universities and general hospitals have existed for many decades. Indeed, an equilibrium has been reached abroad as it no doubt will do here. In Scandinavia, for instance, at least three of the professors of psychiatry are heads of mental hospitals, a fact which serves to demonstrate the importance attached to the long-term study of psychotic illness.

One may be forgiven for not emphasizing sufficiently the many other developments in psychiatry, but this has been done so ably by others. These remarks serve only as a plea that, with all the progress made in social forms of treatment, group therapy, community care and rehabilitation, and last but not least, in empirical treatments, we must not forget that as long as there is a small chance of success some of us have a duty to tread the unpopular and wearisome path of constantly applying newly-discovered scientific methods and techniques in the longitudinal study of psychotic illness, in the hope that our successors may one day, perhaps generations hence, find further clues to some at least, if not all, of these disabling conditions.

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