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[*Frontispiece*

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the behaviour of, say, viruses or organ systems. The late Allen Gregg once said that psychiatry was a generality rather than a speciality. It is, in fact, both, as those of us who teach psychiatry have to bring home to our students as well as to our fellow teachers who are not psychiatrists.

You will have noted by now that I am gently preparing you for the topic of my address. Yes, I have decided to join those presidents who in some form or other have addressed themselves to the question, Whither or whence psychiatry? I can claim several excuses for this choice. I have been in psychiatry for more than forty years now, and my background differs from that of other members. By telling you about it, I shall come closer to you. I do not propose to present an autobiography, but I shall try to tell you how I have experienced the progress of psychiatry. I was able to watch part of it from an unusual vantage point, having worked the first twelve years of my psychiatric career in what was then one of the most important centres of psychiatric practice and research. My reflections will be concerned mainly with those areas of psychiatric progress in which I have been personally involved.

I received my medical education in the University of Vienna, which celebrated its 600th anniversary a few years ago. The teaching had more in common with the Scottish than with the English tradition. The big lecture and lecture-demonstration were the most important teaching methods, though students had limited access to patients. The lack of regular ward work during the undergraduate period was compensated for by several years of hospital service after qualification. To go into medical practice without having spent three or four years in hospital work was unheard of. Specialization was no longer deplored, but had been accepted as inevitable, and at least outwardly the various specialities were equally respected. It never occurred to me as a student that the two professors of, say, ophthalmology were less influential and important people than any one of the holders of the three chairs of internal medicine. There was only one chair of neurology and psychiatry when I was a student, but there had been two at the beginning of the century, one, a chair of neurology and psychiatry, held by Meynert, and the other, the chair of psychiatry and neurology, held by Krafft-Ebing. There were similar combinations in other specialties. Some senior students did research work under supervision. In my last year as an undergraduate I was Demonstrator in the Institute for Brain Research and completed two anatomical studies.

I do not remember when exactly I decided to specialize in neurology and psychiatry, but it must have been rather early in the undergraduate clinical course. I was probably influenced by the fame of Viennese psychiatry, both intramural and extramural. That was the time when the malarial treatment of general paralysis was recognized all over the world as a decisive breakthrough, when Freud's teachings had made their first impact on clinical psychiatry, and when post-encephalitic conditions, hitherto unknown, promised to throw new light on psychiatric symptoms. There was an air of excitement over Viennese psychiatry in those days, and it reached its climax when in 1926 Wagner-Jauregg was awarded the Nobel Prize in medicine. He has remained the only psychiatrist to have won this honour.

I should like to mention some of the men who were on the staff of the Klinik at the time and with whom I was privileged to work. Their names became well known internationally. There was von Economo, who had given the classical description of encephalitis lethargica, and was then immersed in the study of post-encephalitic conditions. He was also an outstanding brain anatomist, who mapped out the cellular architecture of the cerebral cortex. Under his guidance I studied the cortex of deaf and dumb persons.

Then there was Paul Schilder, the most brilliant among my teachers and the one who influenced me more than anybody else. As a young registrar he had described the symptoms and pathology of a hitherto unknown demyelinating disease and called it encephalitis periaxialis diffusa; it is now known as Schilder's disease. He also became deeply interested in psychoanalysis. He aimed at a synthesis of the physiological with the psychological approach. He formulated what he called "the principle of the double path", according to which abnormal

behaviour may result from either a physical or a psychological process. This is a more fruitful hypothesis than the traditional assumption that if an abnormal mental phenomenon can ever be found to be associated with a brain lesion, some such process must always be postulated whenever the phenomenon is observed. Schilder's best-known work was his book on *The Body Image*. In the latter part of his career he became interested in the borderland between psychopathology and sociology, thus preparing the ground for social psychiatry as we know it today. He pioneered group psychotherapy. The three years I worked with this remarkable man made a deep impact on me, and on the rare occasions when I had something like a new idea I could almost invariably trace it back to Schilder's influence.

Then there was Gerstmann, the neurologist who described finger agnosia and the syndrome named after him. I owed to him most of what I knew of neurology.

You may wonder how it was possible for a single university in a small and poor country with a population of only six millions to attract so much talent, and this in a specialty which in many parts of the world had less prestige than, say, general medicine and surgery. There were several reasons for this. The link with neurology, valuable though it was, cannot be the only explanation, because in Switzerland, where psychiatry and neurology are separated, as they have always been in this country, the standing of psychiatry as an academic discipline has always been high. The main reason why there was such a wealth of talent in the Klinik in the first three decades of this century was Vienna's position as the capital of central Europe up to the end of the First World War. Many of its professional people had migrated into the capital from the various parts of the Austro-Hungarian monarchy. There was a constant brain drain to Vienna, and quite a few of the famous members of the medical faculty at the time when I was a student were first or second generation immigrants. Neither Freud nor Economo nor Gerstmann were natives of what after the Great War became the Austrian Republic. Psychiatry and neurology would of course not have attracted so many brilliant people if they had not been well-established academic disciplines.

Competition for training posts was fierce. There was a surplus of graduates such as has never been known in this country. And there were no postgraduate qualifications to acquire. How then could a young doctor show his mettle? Only by hard work and research, as an unpaid clinical assistant, usually in the anatomy and pathology of the nervous systems. My first subject of research was the comparative anatomy of the nuclei around the posterior commissure, the second the fibre connections of the pituitary with the hypothalamus.

It is sometimes thought that the present day emphasis on research method is something new. It is true, of course, that research methods were less sophisticated in the past than they are today, but the fundamental importance of method was fully recognized. The most striking change between research then and today has been the emergence of the technical expert who has taken over much of the work which the research workers used to do themselves.

It was the interest in method, and not the intention to become a practising psychoanalyst, which caused me to apply for training to the Institute of Psychoanalysis. I had a training analysis lasting one year, rather short even by the standards of the twenties, but not exceptionally so. Sir Aubrey Lewis is credited with the remark "Stengel has only been singed". Be this as it may, for me this was an invaluable experience, and I wish it were possible for young psychiatrists today to have a shorter course of training than the full training analysis. I was a resident at the Klinik at the time. Unlike most other continental university departments, Wagner-Jauregg's Klinik was very liberal and encouraged the greatest variety of approaches. At the time when I started my psychiatric training, Freud no longer lectured, but there were regular seminars at his house, some of which I attended. I also had over a period to report to him about a patient of mine in whom he was interested. I remember the seminars vividly, especially the Professor's very lively participation. He was then well over seventy. I was also greatly impressed by the intense personal interest he took in my patient.

Wagner-Jauregg retired two years after I had entered the Klinik, and he was followed by Pötzl, who was well known for his studies on the aphasias and agnosias. He and Schilder were particularly interested in psychiatric symptoms caused by brain lesions, and especially in those features which they had in common with psychogenic symptoms. In 1929 Schilder went to New York, where he soon exerted a powerful influence in psychiatry. I met him for the last time in 1935 at the International Congress of Neurology in London.

Ever since the triumph of the malarial treatment of general paralysis, the hope for another breakthrough simmered among the medical staff of the Klinik. There was a feeling that with some imagination and boldness even schizophrenia might be conquered. Success seemed within grasp. After all, had not the malarial treatment been just such a lucky stroke? To emulate and even surpass Wagner-Jauregg, that archetype of a father-figure who had been the head of the Klinik for twenty-five years, was the great ambition of some of those who had worked under him for so long.

I remember a hush-hush surgical treatment of schizophrenia in 1929/30. It was only in 1932 that a short publication in a not very widely read journal (*Psychiat.-neurol. Wochensch.*, **34**, 110, 1932) revealed that Pötzl, between his painstaking studies of the visual agnosias, had, jointly with Hoff and a brain surgeon, devised and carried out a therapeutic operation on three chronic schizophrenics. The operation aimed at setting lesions in the medial nuclei of the thalamus with the express purpose of severing their connections with the frontal lobes. The operation was given up when none of the three cases showed a marked improvement. The result might have been different if the patients had not been hopelessly deteriorated cases. The authors concluded their report with the following observation: "Our attempt at changing by surgery the balance between subcortical and cortical mechanisms is only a beginning which may be modified in various ways before this approach should be finally abandoned." This was written three years before the idea of leutocomy was conceived in 1935. Pötzl and his associates did not pursue the matter, but they deserve to be regarded as the forerunners of Moniz. I mention this episode because it illustrates the preoccupation of the Viennese school with active treatment and with the hope of another therapeutic breakthrough. The failure of the first thalamotomy in schizophrenia must have come as a deep disappointment to Pötzl and his associates. This partly explains the almost rapturous reception of Sakel's insulin coma treatment at the Vienna Klinik.

Manfred Sakel was a fellow-student of mine and I knew him well. He was not an outstanding student, and nobody would have predicted that he was going to make a name for himself in the history of medicine. Soon after qualification, in 1926, he took up an appointment in a private nursing home near Berlin. In 1933 he returned to Vienna and succeeded in persuading the head of the Klinik to introduce insulin coma treatment. Sakel never held an official appointment at the Klinik or in any other hospital in Vienna. He remained an independent psychiatric practitioner. It was unprecedented for the chief of a university Klinik to put the resources of his departments at the disposal of somebody who had never had a formal postgraduate training and who had not worked his way up in the prescribed manner. So promising seemed the proposed treatment that the conventional taboos against outsiders were waived. Insulin coma treatment now belongs to the history of psychiatry, like the malarial treatment of general paralysis.

It was through insulin treatment of schizophrenia that I got to know personally a member of the R.M.P.A., and a very distinguished one too, our Past-President Dr. Isabel Wilson. She had been sent to Vienna by the Board of Control to study and report on the new treatment of schizophrenia. This was far from easy. The head of the Klinik and some of his staff had been swept off their feet by Sakel's enthusiasm. No attempt was made to compare the results of the new method with the progress of a suitably matched control group, as Wagner-Jauregg had done in the treatment of general paralysis. Dr. Wilson set about her difficult task with great energy, tact and charm. She not only attended the Klinik regularly but also carried out an opinion survey among psychiatrists in Vienna.

She had an excellent command of German and quickly picked up its Austrian variety. She had joined forces with a Swiss woman doctor who had also come to Vienna to study insulin therapy. I well remember the occasion when the two ladies invited me to have coffee with them, for the purpose of finding out what I really thought about insulin treatment. I told them that, being so close to the field of action, I was unable to form a detached opinion about the value of the treatment, and I expressed the hope that it would be possible in England to compare the outcome of schizophrenia in a treated with that of a comparable untreated group, possibly in different hospitals. Three years later I met Dr. Wilson again, this time at Dorset House, Bristol, which she visited as a Commissioner of the Board of Control. As it happened, the second member of this Association whom I met was another distinguished woman doctor, Elizabeth Casson, a truly remarkable person who offered us hospitality when we came to this country in 1938. It was she who introduced me into the R.M.P.A.

It is in fact, not quite correct that Dr. Isabel Wilson was the first member of the R.M.P.A. whom I got to know personally. Wagner-Jauregg was, of course, an Honorary Member of this Association. When I paid him a farewell visit before my hurried departure from Vienna in 1938, I told him that I was going to England. "To England?" he asked. "Wait a moment." He got up and searched among his papers. He came back with the 1938 edition of the R.M.P.A. Year Book. "In England", he said shaking his head, "they have plenty of psychiatrists." He wondered whether I would be able to make a living there. The R.M.P.A. membership was 969 at the time! He was pretty confident that the political upheaval would be over soon and that I could safely stay in Vienna. Politics was not his strongest line—at least not outside the university. Still, he was no worse than the majority of the professional politicians in those days.

Looking back at the insulin treatment of schizophrenia, and comparing it with present day therapies, one cannot fail to be impressed by its heroic quality. What a sweat and toil it was for all concerned, patients, doctors and nurses! There is no treatment in the history of psychiatry which made such demands on everybody involved in it. Perhaps its greatest merit and possibly its most important historical contribution was the way in which it created a therapeutic team around the patient. A well-run insulin department, such as, for instance, the one at the Crichton Royal directed by Mayer-Gross, was a closely integrated therapeutic community. It may be argued that we can witness similar teams at work in the operating theatres all over the country every day. But they do not operate on the same list five times a week over three months or longer.

I met Sakel for the last time in 1950 at the Congress in Paris. He was an unhappy and disappointed man. His belief in insulin coma treatment was unshaken, but he felt that he had not received the recognition due to him, that he had been robbed of the credit for convulsive shock treatment which he claimed had always been part of his method. Meduna, the originator of the Cardiazol shock therapy, hotly denied this. It is nevertheless true that epileptic convulsions were observed early in the insulin therapy and regarded as beneficial. Cardiazol treatment was soon replaced by electric convulsant therapy, which still holds the stage. E.C.T., too, was claimed to have been conceived independently of other treatments. However independent they may have appeared to their originators, all these treatments were applications of the principles applied systematically for the first time in the malarial treatment of general paralysis. Even that treatment had been tried before Wagner-Jauregg by Rosenblum in Odessa.* If one surveys the history of successful empirical treatments, one comes to the conclusion that hardly anybody can claim to have had the original idea. There have always been others who thought of it before, but almost invariably the credit goes to those who have the guts or the facilities to carry them through.† It is a comforting thought that any one of us may originate an idea which if taken

* Quoted from Wagner-Jauregg, *Lebenserinnerungen*, Vienna, 1950.

† Malcolm Flemyng in his *Neuropathia* (1740), quoted by Hunter and MacAlpine in *Three Hundred Years of Psychiatry* (1963), wrote as follows: "If we were clever enough to

up by somebody else may prove fruitful and even revolutionary.

The era of shock treatments was followed by the advent of leucotomy, another treatment which had been tried and given up previously. Again, there was a period when occasionally therapeutic enthusiasm got the better of critical judgment. Today the indications for this kind of operation are very limited. This is partly due to the new drug treatments, which can claim remarkable symptomatic successes. These treatments, too, are based on empirical observations. It is too early to assess their full significance for the progress of psychiatry.

The development of physical treatments is only one side of the story of the last forty years. Hand in hand with it went a profound change in the attitude towards mental illness and to the patients. This change has been both a corrective and a complement to the emphasis on physical treatments. It started before the war, partly as a reaction to the disappointing results of insulin coma treatment. Notwithstanding Freud's scepticism, a number of psychoanalysts undertook the treatment of psychotic patients, sometimes with apparent success. As in the field of the physical therapies, improvements were observed, provided the patients selected for treatment were not deteriorated chronic invalids. Although the number of patients treated with analytically-oriented psychotherapy was small, the knowledge that such treatment was possible in ambulant and even in some hospital cases, and that it was practised in such famous centres as the Zürich University Hospital, militated against psychotherapeutic nihilism and against the conviction that physical treatments alone held out any hope of improvement. Independently of those isolated efforts to reach the psychotic patient, Sakel had noted that the response to insulin coma was more satisfactory if a great deal of individual attention was given to the patient in the course of the treatment. It is now held by many that the beneficial effects of insulin treatment were due to the patient's involvement, in a state of extreme dependence, with helpful human figures, rather than to the repeated comas. Thus, paradoxically, the most drastic of all physical treatments contributed in the end to the recent emphasis on interpersonal relationships, especially in the treatment of schizophrenia. We see a similar paradox in the theories on aetiology where biochemical hypotheses thrive side by side with psychogenic ones. Whatever the outcome of these researches will be, one can safely venture the prediction that the aetiological factors, chemical or psychological or both, will be found to be of an altogether different order from those known to us today.

When I came to this country in 1938, I got to know a different kind of psychiatry compared with the one in which I had been brought up. First of all, it was a psychiatry independent of neurology. In most continental countries the link with neurology still exists. Recently it has been severed in two German universities, to the dismay of most psychiatrists, especially those working in University Kliniks. Separation is bound to come where it does not exist already. Even when I was a student it was difficult for one man to master both psychiatry and neurology. Today it is quite impossible. Most of you will be surprised to learn that Kretschmer, for instance, was Professor of Psychiatry and Neurology. He could not possibly do justice to both. Where psychiatry is independent, psychiatrists are more likely to be aware of the problems which are peculiar to psychiatry than they are where they have to practise neurology too.

I have had the good fortune to have moved about a great deal in this country. I have worked in various capacities in Bristol, Exeter, Edinburgh, Dumfries, Chichester, London and Sheffield, and thus have been able to get to know a good deal of British psychiatry and its progress during the last twenty-eight years.

induce a tertian or quartan fever by a safe and certain method, we might go far towards curing those diseases which depend on a weakness of the nerves or spirits", for "it has often been observed that . . . intercurrent fevers . . . strengthen the nerves." Pointing to the fact that this became the standard treatment for general paralysis in 1917 until superseded by penicillin, Hunter and MacAlpine add, "Practising on the east coast and near the fens where malaria was still endemic Flemyng was of course well placed to observe its beneficial effects in patients with nervous diseases, some of whom may well have suffered from general paralysis of the insane, although it was not then recognized as a specific disease entity."

The lack of university departments has had some positive consequences for psychiatry in this country, which it is hoped will not be lost with the emergence of academic and research units. Until recently psychiatry was practised almost exclusively in mental hospitals, where all new treatments, including occupational therapy, were introduced first. This was one of the reasons why British psychiatry has for so long led the world in patient care. The mental hospital has been changing under our eyes in the last two decades. It is losing its traditional role and is groping for a new identity. He would be a bold man who would try to predict what is going to become of it. It looks as if it will fall to British psychiatry to play a leading part in its reform.

The growing emphasis on social factors in the aetiology, the manifestations and the treatment of mental disorders is an important feature of present-day psychiatry. Social psychiatry can claim to be a new approach, probably the only new approach, and the emergence of the psychiatric social worker as a member of the therapeutic team has added a new dimension to clinical work.

I have so far dealt with the advent of physical treatments, the change in the function of the mental hospital and with the recent emphasis on social factors. There have, of course, been many other new developments, some of which I can only touch upon. The growth of child psychiatry is probably the most important one. We are only at the beginning of its contribution.

Much of the progress in psychiatry has been due to new discoveries made in other fields. Here the striking advances in the area of subnormality come to mind.

Epidemiology is another approach not indigenous to psychiatry. The application of its methods has already corrected many misconceptions and filled many gaps in the knowledge of mental disorders. We now have a much clearer appreciation of psychiatric morbidity than our predecessors had. Even if the hope that the epidemiology of mental disorders would lead to important aetiological discoveries should not be fulfilled, it has already yielded much useful information and is far from exhausted. There is no clinical psychiatrist who can ignore it in his work with patients and in his research. I can hardly call myself an epidemiologist by training and orientation, but my studies on attempted suicide, which originated from psychopathological problems, would have followed a different and probably less rewarding line without the influence of epidemiology.

Every scientific approach has its own peculiar pitfalls. I should like to illustrate this with some observations on suicide research. Epidemiology is concerned with disorders in populations. It deals with facts and figures. One easily succumbs to the illusion of certainty once one is faced with mathematical symbols. Every year national health offices and W.H.O. present lists of suicide rates, and many sweeping conclusions have been drawn from regional and national differences. The suicide statistics are regarded as the most reliable epidemiological data in psychiatry. But are they? Only recently the fundamental question of the comparability of suicide rates has been raised. There is evidence that many national and even regional statistical data are not comparable, because the methods of ascertainment differ. Some national suicide rates are derived from coroners' verdicts, others from certificates of family doctors, others again from post-mortem reports of police surgeons. To take it for granted that these figures are comparable means neglecting the most elementary requirements of epidemiology. Yet this is what has been done all along. The same criticism can be made of many other statistical investigations which purport to introduce scientific precision in the place of uncertainty. Needless to say, epidemiological methods are unsuitable for research into motivations underlying abnormal behaviour, although they may occasionally provide useful pointers.

The subject matter and the methods of psychoanalysis are in many respects the extreme opposites to those of epidemiology. Psychoanalysis has influenced psychiatry since the beginning of this century. It is in constant flux and has undergone considerable changes. Some psychoanalytic propositions have been subjected to tests of validation, experimental and otherwise, while others have defied such tests. Attempts have been made to translate psychoanalytic observations and theories into the language of learning theory, with which

psychoanalysis has a good deal in common. Many of them will no doubt be reformulated or discarded. While in this country the discussion about psychoanalysis among psychiatrists has not engendered much heat, it has dominated the psychiatric scene in the United States. Psychoanalysis has been integrated with psychiatry in many centres, a process which has not been generally welcomed among the more traditional psychoanalysts. However, psychoanalysis could not have it both ways—to be recognized and to be left alone. The great demand for psychoanalysis and kindred treatments among the middle and upper social classes in the United States is a remarkable social phenomenon which has no equivalent elsewhere. I have a pet theory of my own about it. I believe it to be related to certain aspects of modern life as well as to the unique material affluence of American society. All of us are being inundated by communications from outside which are pouring upon us through the mass media all the time. This may be partly responsible for the fact that people communicate with each other less and less. I often amuse myself by asking my colleagues when they last listened to their wives for a solid half-hour. None of them can remember. Our neurotic patients of all classes often complain that their spouses, especially the husbands, never have time to listen to them, although many of them work fewer and fewer hours. American society is the most affluent that has ever existed and many of its neurotic members can afford to pay a doctor for listening to them. In the United States the doctor is, within limits, a commodity which can be bought on the free market. It is not surprising that a considerable proportion of psychiatrists go into the private practice of psychotherapy of one kind or another. The same is true of clinical psychologists. Apart from its financial attractions in America—not in this country—psychoanalysis and psychotherapy are, of course, extremely interesting.

These observations of the enormous demand for psychotherapy among the middle and upper socio-economic classes in North America may sound superficial and may even cause offence to American colleagues. They are not advanced in a spirit of adverse criticism. They refer to a serious deficiency of human relationships in present day Western society, a deficiency which by a historical accident the psychotherapist is called upon to remedy by those who cannot tolerate it and who can afford to pay for its alleviation. We are still in the dark as to why people seek psychotherapy in spite of its limited effectiveness, and what they get out of it. Attempts at evaluating its effects in the same way as those of a treatment for measles or diabetes have proved singularly unenlightening.

I have no doubt that the psychoanalytic approach has a lot to offer to the psychiatrist. How to teach its elements to those who do not want to specialize in it is another matter. There are indications that group instruction will fill this gap. It would be a pity if psychiatrists should be unwilling and unable to look behind the appearances of human conduct. Too many of them seem to think that methods of enquiry no more subtle than those used by Dr. Gallup in his opinion polls can elucidate the motivations underlying abnormal behaviour.

I do not propose to discuss the advent of behaviour therapy on this occasion, except to point out that historically it presents a return to the purely symptomatic treatment of neurotic conditions. Nor can I dwell on the role of the clinical psychologist, another newcomer on the psychiatric scene.

In listening to me, unless you have already given up, you will have become aware of what an impossible task I have taken upon myself in trying to survey the progress of psychiatry in the last forty years. I hope you will understand why I have had to confine myself to those developments in which I have been personally involved. Let us look back, briefly, over those forty eventful years. It has been a period of hectic activity and also of new horizons. Psychiatry has developed a true therapeutic spirit which shows no sign of losing its momentum. The therapeutic explosion of the twenties and thirties was a reaction against the defeatism of the previous decades, when it was held that there was little or nothing one could do for the mentally ill as long as the aetiology of their illnesses was unknown. It is intriguing to speculate what would have happened if malarial therapy, that deviant and now obsolete treatment of general

paralysis of the insane, had never been thought of. What is left of G.P.I. would be treated with antibiotics today just the same. The general paralytics would have had to wait for an effective treatment a quarter of a century longer, a short enough period in the history of medicine. Yet without malarial treatment the recent history of psychiatry might have taken a different course, because the shock treatments inspired by malarial therapy might not have been thought of either.

I said that the last forty years have been a period of great activity in psychiatry especially in the therapeutic field. Has this also been a period of great ideas? I do not think it has and I was confirmed in this impression when I listened to Professor Pichot's historical account of French psychiatry this morning. What happened in the preceding four decades 1886 to 1926? That indeed was a period of great ideas. Hughlings Jackson, Francis Galton, Kraepelin, Freud, Janet, Pavlov, Bleuler, Jaspers, Adolf Meyer belonged to that period, although some of them lived into the next. Little has since been added to their observations and theories. In fact, as far as ideas are concerned we are living almost entirely on the investments made by those men before our time. I can hear some of you think: "Does it matter? In those days psychiatrists brooded over concepts and systems, while we are helping our patients." True, but there has been among our generation a serious lack of concern for ideas, which has often been associated with indifference to clinical observations. We have neglected our every-day conceptual and observational tools and we have run into difficulties as the result of this neglect. So many psychiatrists have preached for so long that diagnosis and classification do not matter that we often cannot compare the results of treatments or of epidemiological studies, because we do not know what the other fellow is talking about. A great deal of work has already been wasted through this breakdown of communications. We have an international classification of diseases, but few psychiatrists have bothered to use it, and those who have used it have often done so thoughtlessly. There has been a healthy resurgence of interest in concepts and classifications. Without such a revival the most sophisticated instruments will be of little avail, because we shall not be clear what it is we are measuring. This is why a mainly empirical and pragmatic approach is bound to prove inadequate.

I have offered you retrospective reflections on the progress of psychiatry, especially in those areas in which I have been a participant observer. My vision was limited by my own experiences. You may have felt at times that I was talking about my own progress in psychiatry rather than about the progress of psychiatry. I have made no effort to disentangle the two. It cannot be done if one surveys developments in which one has been personally involved. The progress of collective endeavour in any field of study has much in common with that of the individual. It has its ups and downs, its periods of advance, stagnation and even regression. But, unlike the progress of an individual, it is not subject to the limitations of age, ability and energy. It draws its powers and its inspirations from the insatiable needs of successive generations for more knowledge.

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A modicum of the insights and simple techniques of Dr. Greco would enliven and enlighten the life of many a psychiatric clinician who has got stale and can think only in terms of variations of medication and physical treatments. For the younger registrar who does not want to get involved in deep psychotherapy, yet feels that his work is lacking in shape and insight, this book could be of enormous help and might lead on to further reading.

M. H. B. JOYCE.

Psychopathology: Its Causes and Symptoms. By F. KRÄUPL TAYLOR. London: Butterworth & Co. 1966. Pp. 356. Price 70*s*.

The author's aim in this book is to review the various meanings of the word "psychopathology" and elucidate their implications. To this end symptoms and aetiology of disease are considered in detail in the first section. The bulk of the book is then devoted to "descriptive psychopathologies", and a third section deals with "dynamic psychopathologies". The result is acclaimed in a foreword by Professor Carstairs as "an exceptionally lucid account of the subject-matter of psychiatry and the present limits of knowledge in this specialty". But at times the book is rather heavy going, as for example when we read that "the alteration of consciousness in sleep is due to normal cerebro-physiological processes of a somniferous kind" (page 222). Inevitably, large areas of the writing have the features of a textbook, and despite the profusion of textbooks in psychiatry there is much of interest in the discussion of different syndromes since the author has at his command a fairly wide knowledge of the relevant literature. It is, however, surprising to be told that hypomania is not a psychotic illness and that the majority of suicides are due to depressive delusions. Further, some autonomic symptoms are accepted as hysterical with surprising alacrity.

More important than these errors is the uneven treatment awarded to different topics. Many parts of the book present a closely-reasoned discussion and evaluation of the matter in hand. Yet other parts, and particularly some of those like the section on "act phenomena" and "object-phenomena" on which much of the author's philosophy is based, are presented rather dogmatically and with cursory justification. The distinction offered between objective and subjective symptoms is of doubtful validity, to say the least. Much play is made with the disadvantages of "intuitive understanding", although it is acknowledged (page 33) that this may not be truly different from causal explanation. Dr. Taylor is ahead of many in recognizing this point—but discards

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