

The Maudsley Lecture, 1987 Changing Disciplines in Psychiatry

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It is incumbent on anyone who is honoured by an invitation to deliver the Maudsley Lecture to familiarise himself with the preceding lectures of the series. For me, the task has been unexpectedly instructive. Since its inception in 1919, the list of lecturers is impressive by virtue of not only their eminence, but also the heterogeneity and range of their selected topics. Taken as a group, the lectures emerge as a line of beacons which illuminate the direction, if not always the advances, of psychological medicine over the past three generations.

In the pre-war period, this variety reflected in some measure the original nature of the bequest, according to which two categories of lecture were to be delivered in alternate years, the one 'scientific' and devoted to original work, the other 'popular' and concerned with any subject connected with the hygiene of the mind (Crichton Browne, 1920). The scientific category included such topics as Sir Frederick Mott's "Further researches on dementia praecox" (Mott, 1921), Edwin Goodall's (1927) "Dealing with some of the work done to elucidate the pathology of disease failing to be considered under the rubric 'insanity'" and C. E. Spearman's (1929) "The psychiatric use of the methods and results of experimental psychology". To the popular category were allocated more general themes: Sir Farquhar Buzzard's (1933) "Education in medicine", Lord Macmillan's (1934) 'The Professional Mind' and Mr Justice McCarthy's (1931) 'Truth'.

In the post-war years, this system of alternation appears to have given way to a more uniform pattern. As Erwin Stengel pointed out, each lecture has tended to focus on a large issue, drawn either from the field of clinical psychiatry or from one of the many areas of basic or applied studies in which psychiatrists are interested (Stengel, 1965). In most instances, the lecturer has chosen to speak on some topic within his or her sphere of expertise, and the spectrum has proved broad enough to incorporate themes ranging from Sir Frederick Bartlett's (1946) 'Intelligence as a social problem' to Grey Walter's (1950) 'The function of electrical rhythms in the brain' and John Ryle's 'Nosophobia' (Ryle, 1948a), the first Maudsley lecture that I attended, and the one that is indirectly linked most closely with my own.

To maintain the tradition, I have deemed it appropriate to relate my contribution to some of the work with which I have been associated, but the content prompted less difficulty than the form in which it might be presented. The occasion is clearly no longer appropriate for popular lectures, now that the mass media have taken over the field to such an extent as to have given rise to Henry Miller's (1969) diatribe on the psychiatrist as magician, prepared to "misuse his jargon to confuse any and every topical issue in an incessant series of television appearances". But nor, I think, should it provide no more than a platform for the specialised presentation of clinical or scientific material, some of which might otherwise qualify for publication in the *Journal of Irreproducible Results*. I felt there was something to be said for a broader framework than has sometimes been evident, and I was glad to find a guiding precedent in Adolf Meyer's (1933) Maudsley lecture, which was constructed in what he called a 'semi-historical and semi-personal' format.

The historical element opens the door at once to mention of Henry Maudsley. His work and influence on British psychiatry have constituted the subject of two previous Maudsley lectures (Bond, unpubl; Lewis, 1951), and most lecturers have endorsed Sir David Henderson's vignette of "a sensitive, solitary man, a keen student, a brilliant and independent thinker, with something of the quality of genius" (Henderson, 1939). More recently, some medical historians have called for a re-evaluation (Clark, 1982; Showalter, 1987; Turner, 1988). My own concern, however, is more with the roots and soil of Maudsley's outlook than with its flowering, for the familiar portrait of the white-bearded patriarch who died in 1918 tends to obscure the fact that his formative years were passed in the high noon of the 19th century. His first paper, 'The correlation of mental and physical forces' (Maudsley, 1859), was published when he was aged 24, in 1859, the year when Landseer's lions appeared in Trafalgar Square, when Gladstone became Chancellor of the Exchequer and the Franco-Austrian war commenced; the year that also saw the appearance of Mill's *Essay on Liberty*, Samuel Smiles's *Self Help*, Dickens' *A Tale of Two Cities* and, most significantly, Darwin's *Origin of Species*. To that most perceptive of

historians, G. M. Young, 1859 ushered in the “years of division” between early and late Victorian England, separating what he called the “statistical” from the “historical” age:

“We are passing from the statistical to the historical age, where the ground and explanation of ideas, as of institutions, is looked for in their origins: their future calculated by observation of the historic curve. As early Victorian thought is regulated by the conception of progress, so the late Victorian mind is overshadowed by the doctrine of evolution. But the idea of progress – achieved by experiment, consolidated by laws or custom, registered by statutes – had, without much straining of logic or conscience, been made to engage with the dominant Protestant faith, and thus, equally in both its modes: in the individualism of the soul working out its own salvation, in the charity which sought above all things the welfare of others. . . . Religion, conceived as a central system of ideas, aspirations, and practices to be imposed on society, was losing its place in the English world” (Young, 1936).

This loss was taking place not only the English world, it might be added. The drift away from religion was part of a wider process, one which Owen Chadwick (1975) has aptly termed the “secularisation of the European mind”.

Maudsley’s period of maturation, then, coincided with a crucial period in the history of ideas. Unlike most physicians, as Sir John Macpherson (1928) pointed out in his Maudsley lecture, he was “a man in advance of his time – one of the galaxy of intellectuals that adorned the mid-Victorian period”, and as such he was well aware of the impact of evolutionary theory and scientific materialism on what Harold Nicolson (1968) described as “that intricate weaving and unweaving of taste and distaste, that kaleidoscope and continuous reshaping of intellect and indifference, of surprise and expectation, which we call, somewhat indolently, ‘the spirit of the age’”. Writing in the 1890s, Bernard Shaw could look back in pleasure:

“Let any man of middle age . . . consider what has happened within a single generation to the articles of faith his father regarded as eternal, nay, to the very scepticisms and blasphemies of his youth . . . and he will begin to realize how much of our barbarous Theology the man of the future will do without”. (Shaw, 1891).

But Shaw, secure in his socialist faith, was in a minority. For a majority of late Victorian thinkers, the strain was overwhelming, so much so that Gertrude Himmelfarb (1968) has concluded that:

“Victorian intellectuals dwelled, for the most part, upon the plains of madness – that deceptively peaceful countryside where philosophers paraded as journalists and writers showed off their Rugby Blues more proudly than their Oxford Firsts. Here lived those scientists and rationalists (Darwin, Huxley, Spencer) who suffered from lifelong illnesses which defied medical diagnosis and cure; novelists of domestic manners and morals (Bulmer Lytton, Thackeray, Meredith, Dickens) whose marriages were tragically unhappy; religious libertarians (Harrison, Stephen, Morley) who were zealous puritans; successful and wealthy writers (Macaulay, Dickens, Darwin) who were obsessed with the fear of bankruptcy; moral critics (Caryle, Eliot, Mill, Ruskin) who lived in the shadow of sexual aberrations and improprieties; and in general an intellectual community suffering a larger proportion of nervous breakdowns, it would seem, than almost any other.”

With his adherence to notions of ‘degeneration’ and his sceptical views on treatment, what Maudsley had to offer his troubled contemporaries must have been cold comfort. From the beginning, his writings are deeply imbued with an analytical, agnostic, ultimately pessimistic, approach to the major philosophical questions associated with religious belief, moral conduct, and the mind–body problem. That first paper contains a credo which underpins many of his subsequent expositions:

“Man, indeed only progresses in knowledge in so far as he progresses in physical science; it is in this mainly that the progress of civilisation has consisted . . . it is so far true, that no change has taken place of late in the principles of morality; but it seems impossible to deny that there has been an extension of the application of those principles – an increase in practical morality; and this, not as the result of any supposed exacerbation of moral principle, but as the simple and inevitable result of the progress of science. The abstract moral truth, that man ‘should do unto others as he would have others do unto him’, though for ages preached, and for ages recognised as true, would not avail to induce the rich man to improve his poor neighbour’s pig-sty habitation. And the poor man being left, morality notwithstanding, to live like a pig, acted also in some measure like a pig but modern science has taught us that a filthy habitation, and a foul atmosphere and unwholesome food, are directly destructive to human life; and cholera and fever have done what religion and morality had attempted and failed to do; and now, as a result, is appearing the dawn of a social science”.

When Maudsley here speaks of progress, he does so in the spirit of G. M. Young’s ‘historical age’.

There is nothing eudaemonical in his use of a word which to Herbert Spencer (1892) was “not an accident, but a necessity. Surely must evil and immorality disappear; surely must men become perfect”. And Maudsley’s final sentence suggests that, for him, Young’s ‘statistical age’ had been superseded rather than merely succeeded by the burgeoning of the natural sciences, for he ignores altogether the large claims to the scientific study of society on a statistical basis, which had been lodged 30 years earlier. Melioristic and overtly political in outlook, and drawing on Benthamite utilitarianism and on philosophical radicalism for their ideas, the advocates of the statistical movement participated in a widespread reaction to industrialisation in a society that was rapidly changing its values and looking for a way of rationalising political economy and formulating social ‘laws’ (Cullen, 1975; Metz, 1984). The term ‘social statistics’ was coined to cover moral as well as vital statistics. The former were concerned mainly with education, crime, and intemperance, the latter with the collection of data on birth and mortality. From these collections of facts and figures, there emerged the study of ‘sanitary statistics’, conceived as the numerical relationship between the hygienic conditions of living and the risks to health and life, especially in urban conditions, which constituted the basis of the concept of public health in Britain during this epoch in the work of men like Owen Chadwick, William Farr, Thomas Southwood Smith, and John Snow. On the European mainland, there developed a slightly different but parallel trend that was to lead to Neumann’s (1847) challenging assertion that while it draws heavily on the biological sciences, medicine is intrinsically and essentially a social science. Much of this early work was focused on epidemics of infectious disease, correlating death rates with the hygiene of the living conditions of the population at large. Paradoxically, its impetus was impaired by the emergence of bacteriology in the 1870s, when the microscope of the laboratory worker came into its own and the emphasis was placed on the biological causation of disease. The statisticians diversified their activities and loosened their political links, some to concentrate on what was to become empirical sociology, some to develop the field of biometrics, and others to apply medical statistics to the study of morbidity and mortality. By the beginning of this century Alfred Grotjahn (1911) was able to construct a framework of social hygiene, and in his autobiography he put the matter plainly:

“I was, from the beginning, convinced that medical statistics must be regarded as the basis of socio-

pathological and social-hygienic study and therefore needed the most careful cultivation. At that time the victorious march of bacteriology had relegated medical statistics to the background. . . . It was not necessary to rob bacteriology of a single leaf of her garland in order to insist that this view was erroneous and that a quantitative study of the theory of epidemics was as indispensable as before the bacteriological era” (Grotjahn, 1932)

Grotjahn’s words have cast a long shadow. The debate over these two approaches to medicine was to extend far beyond the infectious diseases and continues to the present day. Reviewing the characteristics of these two complementary forms of clinical science, Sir James Spence (1954) referred to one as clinical cartography, aiming chiefly to map the contours and terrain of disease, the other as clinical phenomenology, an experimental discipline that “studies the isolated phenomena of disease or of disordered function and its aim is to explain their mechanisms and their clinical significance”. For a while, the spectacular achievements of experimental medicine appeared to hold out boundless prospects, founded on Lewis Thomas’ (1984) claim that 50 years ago “medicine was changing into a technology based on genuine science”. By “genuine” he meant biological. Since then, a period of disillusionment has set in, and no less an authority than Macfarlane Burnet has gone so far as to assert that “the contribution of laboratory science to medicine has virtually come to an end” (Burnet, 1971). Colin Dollery (1978) has spelt out the reasons for a more sober verdict in his monograph explicitly entitled *The End of an Age of Optimism*.

Perhaps the most far-sighted overview of this dichotomy, however, was provided by John Ryle, the first professor of social medicine in this country. Unlike most earlier representatives of this viewpoint, Ryle was an eminent clinician, a former physician at Guy’s Hospital, and Regius Professor of Physic at Cambridge (Rosen, 1948; Webster, 1986). Towards the end of his career, in 1948, he summarised his standpoint in a volume of essays, which he entitled *Changing Disciplines*:

‘Looking back, it has seemed to me that while Medicine – through scientific and technical advances – has greatly gained in potentiality during the past quarter of a century, it has in the process become less surely attuned to some fundamental human needs – to the broader social needs of the group or community. . . . In the midst of great social changes we have not succeeded in registering and explaining the accompanying changes in the quantity and quality of many of our main diseases some of

which I shall later refer to as our 'modern endemics'. . . . We no longer believe that medical truths are only or chiefly to be discovered under the microscope, by means of the test-tube and the animal experiment, or by clinical examination and increasingly elaborate pathological studies at the bedside. Psychological and sociological studies have an important part to play. Even so, it is not yet appreciated how intimately disease and social circumstances are inter-related. The whole natural history of disease in human communities, as well as in individuals, is ripe for a fuller and more exhaustive study (Ryle, 1948b).

It was on this basis that Ryle constructed his own concept of social medicine, drawing on the 'changing disciplines' of his – and my – title. These included demography, biostatistics, history, sociology, some branches of psychology, and above all, epidemiology, especially the epidemiology of non-infectious disease, a notion that was then still controversial. Ryle himself spoke of 'endemics', and in many ways, as Erik Strömberg (1968) has argued, the word epidemiology would have been more appropriate. Nonetheless, the term epidemiology has stuck, because of its historical associations, and has come increasingly to be the term of choice for the mass aspects of disease. It is a word, incidentally, which – as far as I can determine – has received explicit mention, and that in passing, by only two previous Maudsley lecturers.

How did these 'changing disciplines' impinge on the study of psychological medicine? The 19th century witnessed numerous debates on such topics as moral treatment, the increase and the prevention of insanity, and the putative nexus between civilisation and mental disease. Most of them were conducted in very general terms, although a few individuals appreciated the potential significance of employing the numerical approach to mental disease under institutional care very early (Black, 1811). By and large, however, this was not the use to which the statistics of insanity were widely put. The professional world of most 19th century psychiatrists was limited by the asylum walls where, as Gerald Grob (1985) has pointed out, psychiatry developed as "a managerial and administrative speciality". In consequence, the statistics of mental disorder were collected and published primarily to canvas public support and to establish the legitimacy of public mental hospitals. For the purposes of scientific inquiry, the data were gravely handicapped by the inadequacy of psychiatric diagnosis, and most psychiatrists were largely unaware of the importance of classification which William Farr, the first Registrar General, had early recognised as "another

name for generalisation", adding that "successive generalisations constitute the laws of the natural sciences".

The isolation of mental-hospital practice further retarded the interest of psychiatrists in the mainstream of general medicine as well as public health. This separation was reinforced by the seeming irrelevance of the bacteriological revolution to the aetiology of most mental disorders. The tenacious beliefs held by a few clinicians in the causal importance of micro-organisms for many mental illnesses may have represented their tribute to the successful public-health campaigns against infectious diseases, but on the whole, as the published deliberations of the Medico-Psychological Association demonstrate, most psychiatrists seem to have taken little account of these developments.

By the early years of this century, a change of opinion could be detected, stimulated in part by the progressive policy in Germany. Like Henry Maudsley, R. G. Rows, Assistant Medical Officer and Pathologist to the Lancaster County Asylum, was much impressed by the organisation and work of the German university psychiatric clinics, but he drew quite different conclusions from his observations. Maudsley himself, as J. R. Rees pointed out in his Maudsley lecture, was unable to see much future for the public-health movement in the field of mental illness (Rees, 1957), but in 1912, in a paper entitled "The development of psychiatric science as a branch of public health", Rows (1912) commented adversely on the British asylum service, and concluded that:

. . . the causes of insanity resemble the causes of the diseases to combat which our service of Public Health has been instituted; and that in order to achieve good results in the treatment of mental disorder, the work must be undertaken by a service of men of high scientific training and keen enthusiasm."

The First World War temporarily interrupted this trend, but it reappeared shortly afterwards in the Report of the Royal Commission on Lunacy and Mental Disorder (the Macmillan Commission) (1926), which emphasised that mental disorder was "essentially a public health problem, to be dealt with on public health lines". In that same year, a Royal Charter was granted to the Medico-Psychological Association, and Lt-Colonel J. R. Lord delivered his presidential address on the topic of "The Clinical Study of Mental Disorders". Seventy-nine pages long, it was printed (Lord, 1926) as a special number of the *Journal of Mental Science* (presumably only

a portion was delivered verbally!), and contains a strongly worded critique of the isolationism of the mental hospital, and a plea for a broader view of the discipline. "Medical anthropology," he stated, "would not be an inapt synonym for modern psychiatry" and, he went on, "The Royal Medico-Psychological Association is concerned with the practice of psychiatry in the broadest sense. Psychiatry and mental hygiene are to be regarded as part of the large problem of public health". Emphasising the need for research, Lord drew particular attention to the inadequacies of diagnosis and classification for statistical analysis, commenting bluntly that "the terms used, with a few exceptions, are confusing and meaningless".

The same trend is discernible among the early Maudsley lectures. In 1924, John Carswell devoted his address to "Some sociological considerations bearing upon the occurrence, prevention and treatment of mental disorders", drawing on some dubious statistics from the Glasgow area (Carswell, 1924). Two years later, George M. Robertson (1926) chose as his theme "The prevention of insanity – A preliminary survey of the problem". On the basis of data derived from the 21st Annual Report of the Commission in Lunacy of the State of New York, he generalised, none too convincingly, on prevention, eugenics, mental hygiene, temperance, education, early treatment, and the putative increase of insanity, concluding with an exhortation to the psychiatrist of his day: "Far too long has he sulked in his tent: now he must come out into the market place. He must co-operate with the general practitioner, and get into touch with the outer world, the out-patient department and the social service worker".

Fine words, but how little force they carried was evident from the Maudsley Lecture delivered 20 years later by Sir Laurence Brock, a Chairman of the Board of Control, on "Psychiatry and the public health service" (Brock, 1946). For Brock, the cream of the profession apparently consisted in what he called the Olympians of Harley St. As for the rest, his views on training reflect his own outlook and that, presumably, of officialdom:

"The new entrant should be treated as a cadet – i.e. someone having the status of an officer but still needing instruction and supervision. . . it is difficult if not impossible to determine whether a young doctor is worth postgraduate training until he has lived in mental hospital long enough to decide whether he likes the life or not."

In the face of such an outlook it is less surprising that although the application of epidemiological

principles to the cause and treatment of pellagra had already provided a convincing demonstration of the public-health approach to mental illness, during the interwar years the concept of mental hygiene carried more meaning for public health workers than for psychiatrists. Since its inception in the USA, the mental-hygiene movement had been primarily concerned with psychiatric issues in the community at large – the school, the home, and the courts – and so became involved with a broad spectrum of social problems. In Britain much of the ground was broken by the voluntary organisations, as exemplified by the prewar activities of the Mental After-Care Association, the Child Guidance Council, the Central Association for Mental Welfare, and the National Council for Mental Hygiene.

The full impact of these currents of opinion on psychiatry in this country, however, had to await World War II, when the potential psychological ill-effects of bombing and of internal migration on the civilian population were recognised as potential public-health problems (Mackintosh, 1944). In the army the need to evaluate and treat psychiatric morbidity was soon appreciated, and selection procedures and rehabilitation programmes were introduced. In the RAF, a number of far-reaching medico-statistical studies were carried out on the classification, distribution, and causation of psychological disorders among flying personnel. Outstanding among these were the epidemiological inquiries on pilot stress conducted by Donald Reid (1947) and Austin Bradford Hill's little-known early work on the reliability of psychiatric diagnosis (Hill & Williams, 1947), which anticipated much research in this sphere.

During the war, furthermore, a remarkable series of planning activities was initiated, including the 1944 Education Act, the Beveridge Report, and the preparation of the foundations of the National Health Service. Medicine itself underwent a penetrating self-examination by the Report of the Inter-Departmental Committee on Medical Schools (Goodenough Committee) (1944), which emphasised the need for both recognition of the importance of psychiatry and its affiliation with social medicine. The Royal College of Physicians' broad view of psychiatry as the study of human behaviour in a social setting led naturally to its conclusion that psychiatry and social medicine be regarded as "the inside and outside of the same glove" (Interim Report of the Committee on Psychological Medicine, 1943).

The early 1940s, then, constituted an historical turning-point in the confluence of factors bringing psychiatry into the new field of force generated by

social medicine and the changing disciplines associated with it. And at this juncture, the historical becomes the personal, although at the time I was barely aware of the process, thereby exemplifying G. M. Trevelyan's observation that people who are present at a turning-point in history are usually unaware that history is turning. For me the pre-potent factor was the exposition of these ideas by several outstanding teachers, foremost among them John Ryle himself, during my undergraduate years. One of the consequences of their teaching was an awareness that for the systematic investigation of mental disorder, there were many advantages attached to a clearly defined catchment area. Some years later, chance, possibly favouring the prepared mind, brought me into contact with the Buckinghamshire County Mental Hospital, St John's Hospital at Stone, near Aylesbury. There I was given free access to the institutional records that enabled me to mount a study of the whole population of an area hospital. This investigation and its sequelae are summarised below. All the work was conducted in Buckinghamshire and with the support of several local colleagues, especially Dr David Watt.

Some years before I began my first study, Sir Aubrey Lewis (1945) observed that: "so far as psychiatry is conceived as a branch of social medicine and public health, it must rely for its advancement upon methods which require accurate statistics such as it is the business of official intelligence to supply, as well as upon the individual and perhaps more original methods which are in keeping with its main tradition". A major obstacle to investigations of this type, however, had been the disparity between the basic demographic and clinical data and the outdated and uninformative diagnostic categories recorded in the case-notes, a point made explicitly by Lord (1926) 60 years ago. Commenting on the need for "... the collection, registration and classification of clinical data for the purposes of psychiatry generally and research work in particular", he went on: "I have never written up a good case in the records of a mental hospital without finding that it was a pure waste of time. The sex, age, occupation, civil state, diagnosis, and result would appear in statistical form, but otherwise the rest of my labour would be hidden, perhaps for centuries, between heavy brass-mounted boards and that would be the end of it. Why should such labour in large measure be lost to the common good and the advancement of psychiatry?"

Why indeed? It seemed to me worth exploring the possibility of combining the carefully recorded accurate statistics of a county mental hospital with a clinical analysis of the case-notes based on the study of all recorded sources, including follow-up

information. The task proved unexpectedly rewarding, and with these data it became possible to undertake a trend analysis by comparing intramural events in two separate time periods (Shepherd, 1957). The initial triennium, 1931–1933, covered the final phase of the custodial era, symbolised by the Mental Treatment Act of 1930. This preceded a more active phase of administration that was to be exploited by the appointment in 1934 of a forward-looking physician–superintendent who was wedded to the concept "... of a Mental Health Service rather than one of mere Mental Hospital administration – a policy of co-operation rather than custody". His vigorous reforms resulted in a sharp increase in medical personnel, the creation of out-patient clinics, the introduction of domiciliary consultations and the provision of beds in a local general hospital, all aiming to render the mental hospital, in his words: "... more a centre of active medical treatment and less a place of care and custody for the chronic harmless case."

By the mid-1940s, the fruits of this policy were apparent in the form of a local system of community care that had been established long before the notion became fashionable. Over this period, the outcome of patients with functional psychoses admitted during the second triennium 1945–1947 had become significantly superior to that of the initial cohort in terms of discharge rates, length of stay in hospital, remissions, readmissions, and mortality. An investigation of the related circumstances identified three possible reasons for this amelioration, namely, the new administrative policy, the admission of less severe cases, and the arrival of new forms of somatic treatments such as continuous narcosis, ECT, sedatives, deep insulin coma therapy, and prefrontal leucotomy. A closer analysis made it clear that the physical measures were of least importance and that the changes were largely attributable to what Cheney & Drewry (1938) had referred to 10 years earlier as the "non-specific effects of treatment". By this term they indicated not only the impact of environmental and interpersonal factors but, by implication, the need to demonstrate the superiority of any new treatment over the intensive application of good hospital care and management. 'Non-specific' in this context was largely synonymous with 'non-biological', and constituted the standard by which any supposedly 'specific' treatments could be assessed.

The role of these non-specific, psychosocial factors seemed to me to carry wider significance. Behind the discussion of the seemingly remote question of the interpretation of mental hospital statistics, there lay, and still lies, a profound difference of opinion

concerning not only the place of somatic treatments, but also the causation of the illnesses themselves. In large measure, the specific–non-specific dichotomy reflects the divide between the biological and the psychosocial approaches to the aetiology and treatment of mental disorder (Shepherd & Sartorius, 1988).

The practical aspects of this issue soon became apparent when the psychotropic drugs entered the scene in the mid-1950s. The action of these drugs was supposedly ‘biological’ or ‘specific’, and their impact, it was claimed, overshadowed any ‘non-specific’ or ‘psychosocial’ effects, which were dismissed as placebos. The consequences of this contention were highlighted when the claims for the efficacy of these compounds were employed in support of the national policy of running down the mental hospitals. The so-called ‘pharmaceutical’ era was seen as spearheading a major shift in policy, and the terms ‘deinstitutionalisation’ and ‘community care’ began to attract widespread support.

In the subsequent debate a closer examination of the facts, or rather the figures, showed that the official view represented a gross oversimplification of a complex phenomenon. Essentially, the policy was based on national statistics that took no account of local variation. How relevant this could be became apparent from our extension of the original trend analysis at St John’s hospital to cover the 4-year period 1954–1957, i.e. from the year before the introduction of psychotropic drugs to the year in which they were prescribed in a large scale. The findings showed that very little change had occurred during this time (Shepherd *et al*, 1961). The major movement of the hospital population, defined in terms of a higher discharge rate and a shorter hospital stay, had in fact taken place 10 years earlier, largely as a result of the setting up of the active mental-health service in the area. A very similar pattern was reported from Norway by Ødegaard (1964), who drew the seemingly paradoxical conclusion that “in hospitals with a favourable situation, the psychotropic drugs brought little or no improvement, or even a decrease in the rate of discharges. In hospitals with a low pre-drug discharge rate, on the other hand, the improvement was considerable”.

This had, of course, long been recognised by clinical observers, particularly with regard to schizophrenic patients. As long ago as 1908, Eugen Bleuler commented on the difference in outcome and prognosis of two groups of schizophrenic patients under his care.

“... in Neu-Rheinau the patients with a ‘favourable’ outcome were more severely ill than those allocated to the same category in Burghölzli or in Alt-Rheinau. As I had known most of the patients personally for years, it seemed unlikely that this was due to an error. The observation was confirmed by the physicians in Alt-Rheinau. The explanation for this fact, I concluded, was that Neu-Rheinau was run in accordance with new ideas and, from the start, had been organised so as to be filled by patients from every quarter of the globe. Here it was possible to create a new spirit which allowed the transformation of a number of severe and chronic catatonics into good workers. This difference between different parts of the same psychiatric service is very striking.” (Bleuler, 1908).

The introduction of long-acting drugs administered by injection as ‘maintenance’ or ‘continuation’ treatment of schizophrenic patients in the late 1960s enabled us to apply similar methods to a more focused issue. The putative advantages attaching to continuation therapy were mainly social, in that it was claimed: (1) to facilitate the return of patients to the community; (2) to reduce the burden on the family; and (3) to render rehabilitation easier and more economical. Several reports conformed well to what had become the expectations of the day, although the hard evidence about the relative efficacy of short- and long-acting neuroleptic continuation treatment was negligible. Nonetheless, in Britain, as elsewhere, the climate of clinical opinion came to favour the view that a diagnosis of schizophrenia in hospital should lead to pharmacotherapy by the parenteral route in a majority of cases.

Accordingly, we carried out a retrospective study of the outcome of two cohorts of all schizophrenic patients discharged from St John’s Hospital during 1967–1968 and 1970–1971 respectively (Shepherd & Watt, 1975). The first period preceded the establishment of special clinics for the administration of long-acting drugs, the second followed their introduction. By comparing the numbers of patients readmitted and their length of time in hospital, we were able to obtain an index of the impact of the drugs on the movement of the schizophrenic population. The result of this analysis showed clearly that while fewer discharged schizophrenic patients were readmitted to hospital when on long-acting medication, this finding characterised only those patients who enjoyed a more favourable prognosis regardless of treatment.

Such findings underlined the importance of examining more closely the natural history of the disorder, on which a rational approach to treatment ultimately depends. Within the National Health Service, it is probable that most schizophrenic

patients make contact with hospital facilities, which can therefore be employed as a method of case identification. For the study of outcome, however, the limitations of any inquiry based on hospital statistics had clearly been reached, and prospective inquiries became indispensable.

Ideally, any such study should be based on the four criteria needed for the establishment of reliable data. These are: (1) a representative population; (2) a standardised method of diagnosis; (3) a prospective inquiry over at least 5 years; and (4) the recording of independent clinical and social criteria. No investigation has so far met all four specified criteria, but it proved possible to construct and follow up a reasonably representative cohort of patients diagnosed as schizophrenic by standardised criteria of known reliability, so as to ascertain the clinical and social outcome independently over the subsequent quinquennium (Watt *et al*, 1983). The results proved to be much better, most strikingly among women, than most earlier workers had maintained.

While some of this improvement may be attributable to drugs, it is apparent that even if a particular form of treatment can be shown to facilitate discharge from hospital and the maintaining of an extramural existence, its importance cannot be adequately assessed without reference to the subsequent quality of life in the community. Earlier studies had emphasised the point by suggesting that the mental hospital atmosphere had merely been transferred to the home. A direct examination of this issue demands an assessment of the impact of treatment on both the patients' clinical status and social capacity, within the framework of a controlled therapeutic trial.

My own involvement in such investigations dated back some years, one of the first having been carried out at St John's Hospital as early as 1956 (Shepherd & Watt, 1956). Subsequently, my work on the Medical Research Council (MRC) Clinical Trials Sub-Committee, which had been set up for the evaluation of treatment modalities in psychiatry, brought me into close collaboration with Sir Austin Bradford Hill who helped demonstrate for the first time that the basic principles of the multicentred clinical trial could be applied to mental disorder (Medical Research Council, 1965).

Under the auspices of the MRC Clinical Trials Committee, we were able to organise a comparative double-blind clinical trial of two drugs at St John's Hospital. Its aim was to assess the relative efficacy of the two compounds in the continuation therapy of patients returning to the community following hospital stays for an acute schizophrenic episode.

For the purpose, standardised clinical and social assessments were made independently over 12 months (Falloon *et al*, 1978*a,b*).

With regard to the results of this complex experiment, I emphasise here only the two most relevant. First, in terms of the clinical criteria employed, there was nothing to choose between the two drug regimes. Secondly, and quite unexpectedly, one of the drugs – originally introduced as no more than a control for the other – proved to be superior on the various measures of social functioning. Other workers have made it clear that an adequate explanation of these findings involves more than drug action, and the authors of a comparable American study concluded: "The development of successful treatment programmes may hinge on our learning more about the nature of this social dysfunction" (Schooler & Spohn, 1982). If this should prove to be the case, the psychosocial factors in treatment could, paradoxically, be regarded as being as 'specific' as the postulated biological factors.

Although hospital-based inquiries, as I have tried to demonstrate, carry some meaning for major mental illness, their scope is clearly limited, and must be complemented by examination of psychiatric morbidity in the community at large. The term 'community psychiatry' has been widely misapplied by restricting it to the fate of patients discharged from hospital. In a public-health context, it should refer to the general population. Only then is it possible to examine the distribution of illness in its early and less severe forms, and to approach the vexed question of what Ryle (1948*b*) called "The meaning of normal and the measurement of health", an objective particularly important for mental disorders characterised by 'dimensional' rather than 'categorical' phenomena.

Buckinghamshire provided an early example of such work in the records of the 17th century practitioner, Richard Napier, whose material has been carefully evaluated by Michael MacDonald (1981). The primary-care health services for adults in the county in the 20th century, however, proved too scattered for investigation and it was more feasible to concentrate on the population of children via the county schools and the child-guidance services, which provide a reliable statistical framework (Shepherd *et al*, 1971).

The basic information was derived from specially constructed questionnaires (6300 in all) dealing with health, behaviour, and family background, which were sent to the parents of a 1-in-10 random sample of all children aged between 5 and 15 years attending local authority schools in the county. The items of conduct in the questionnaire covered most of the

common behavioural problems characterising the schoolchild population attending child-guidance clinics. At the same time, separate questionnaires were prepared for despatch to the children's schools, where they were completed by the class teachers.

The availability of quantitative and qualitative data obtained independently from school and home made it possible to examine the areas of concordance and discordance between the reported behavioural disorders in the two settings. It emerged that while difficulties at home were significantly associated with poor academic performance and also with behaviour 'problems' at school, some children exhibited disorders of behaviour only at home or only at school. Furthermore, many of the recorded items of supposedly disturbed behaviour could not in themselves be taken as indices of abnormality: several items were reported as occurring very rarely, others so frequently as to characterise a majority of the population.

To take account of these factors, operational criteria of 'deviant' behaviour were constructed, to provide a quantitative index of behaviour typical of the child's age and sex. As many as four-fifths of the sample of children attending the county child-guidance clinics were characterised by a deviant total score, and there was a significant association between the recorded occurrence of deviant conduct at home and the reporting of behavioural problems in school. This concept of deviance, however, has nothing to say about duration, which is closely bound up with the notion of morbidity, since disturbed behaviour in childhood can represent no more than a developmental phase or a transient reaction to short-lived stresses. For this reason, a follow-up study was carried out on two matched groups of 400 children, one of which was originally characterised as deviant, and the other not. An analysis of associated factors showed the former group of children and their families to be less healthy, less successful, and more exposed to stress at home and at school than were the control group. Nonetheless, 2.5 years later, approximately three-quarters of the deviant group were reported as exhibiting a reduced number of deviant items, and about half were said to exhibit no evidence of such behaviour. The notion of morbidity attaching to an item of conduct therefore must incorporate frequency, intensity, behavioural associations, and duration before it transcends the boundaries of 'normality'.

With respect to the historical background of this work, extending over 30 years, I would stress, firstly, its dependence on close collaboration with colleagues whose professional skills lie in 'changing disciplines' outside the biomedical orbit – in particular, epide-

miology, statistics, sociology, and social psychology. Secondly, in retrospect, it is now apparent that such work exemplifies a movement in psychiatric research that has come into its own during the post-war period. This has been characterised by the plethora of studies on diagnosis and classification; on measurement, and especially on what Alvan Feinstein (1987) calls 'clinimetrics', the design and application of 'instruments' for the assessment of psychopathology; on the evaluation of treatments and of health services; and on environmental studies. In themselves, none of these topics is new. The novelty consists in the increased frequency and intensity with which they are being applied to the study of mental disorder. All of them incorporate the criteria implicit in Michael Oakeshott's (1985) definition of science as the study of quantitative generalisations, and in a medical context they share an adherence to the epidemiological method.

I am not, of course, suggesting that these methods and techniques constitute the only form of relevant scientific enquiry. The post-war period has also witnessed a fresh influx of experimental studies of mental disorder and dysfunction and a welcome quickening of interest among laboratory-based neuroscientists in the somatic substrate of mental illness; much of what is now designated 'biological psychiatry' has resulted from their efforts. Nor am I disregarding the traditionally idiographic approach of clinical psychiatry, with its emphasis on the description and interpretation of general psychopathology. I have chosen to highlight a third approach because it is still relatively less familiar and has been more widely misunderstood. Its significance may be summarised by a brief mention of the research objectives associated with the six principles enunciated by Alfred Grotjahn (1911) in his *Social Pathology*, namely, prevalence, clinical phenomena, outcome, treatment, aetiology, and prevention.

An appreciation of the extent of mental illness is probably the best-established result of the work so far, underlining Grotjahn's (1911) verdict: "The significance of illness from a social point of view is primarily determined by its frequency". One major finding has been the identification of a large volume of conspicuous psychiatric morbidity at the level of primary care which falls outside the purview of the mental health services, and about which the specialist psychiatrist knows all too little (Shepherd *et al*, 1966). In addition, the search for inconspicuous morbidity in the community at large has renewed interest in the vexed issue of case definition and the boundaries of mental ill health. Equally, there has been a quickening of interest in clinical phenomena in their own right: firstly, with the aim of quantification;

secondly, to complete the clinical picture of syndromes derived from hospital studies; and thirdly, to delineate more accurately the so-called minor forms of mental disorder. All this assumes increasing practical importance as epidemiological inquiries become recognised as the basis of rational health policy (Levine & Lilienfeld, 1987).

The study of these various syndromes further incorporates a temporal dimension, since all too often their natural history, especially of the non-psychotic conditions, remains to be firmly established. In this task it becomes necessary to examine clinical and social criteria independently, since disorders of function figure prominently in the presenting picture. The evaluation of therapeutic intervention also demands an acknowledgement of this distinction, especially since the adoption of the clinical trial as a standard method of assessment has exposed the role of non-specific factors and the need to examine the much underestimated placebo effect.

With regard to aetiology, it is relevant to recall that not until 1951 were the statistics of morbidity as distinct from mortality made the subject of a Registrar General's report, which stated squarely that if "... the search for specific (aetiological) agents halted the progress of social medicine, then the role of nutrition, psychological medicine, stress and other trends have reawakened interest in 'broader domains of aetiology'" (McKay, 1951). These 'broader domains' imply the concept of multiple causation, whose counterpart in mental illness is the notion of 'multidimensionality', which has come to underlie several systems of classification. The search for aetiological factors within this framework is, in Griesinger's terminology, that of causal association rather than causal mechanisms, leading to the realms of predisposition, and to precipitating, pathoplastic, and preformative factors, rather than to the exogenous, pathogenic link in the aetiological chain which, when identified, lends itself more readily to preventive action. Such noxae, which have so far been identified in relatively few mental disorders, are, by contrast, closely linked to genetic predisposition and to patterns of living or life-style which are less susceptible to modification (Report of the WHO Scientific Working Group, 1982). The scope for prophylactic intervention is correspondingly limited.

From all these considerations, the message is clear. "Medicine", as the sociologist Robert Merton once observed (1957) "is at heart a polygamist becoming wedded to as many of the sciences and practical arts as prove their worth". His comment applies *a fortiori* to psychological medicine, but it is now apparent that in these liberated times the new wives are already showing signs of independence not only from the

older females but also from the polygamous male, viewing themselves not so much as help-mates as partners, even senior partners, in a joint enterprise (Shepherd, 1982). The emergence of 'behavioural medicine' in the USA indicates which way the wind is blowing, and psychiatrists have now to familiarise themselves with unfamiliar viewpoints and techniques if their role is not to be diminished.

Finally, I would emphasise the significance of this trend for members of the Royal College of Psychiatrists. Much of the work, as I have tried to illustrate, can be carried out in area hospitals or centres as well as university clinics or specialised laboratories. Accordingly, it provides ample opportunity for psychiatric physicians, with or without academic affiliations, to conduct research on their own patients in their own setting, drawing on their knowledge of the ecological matrix in which mental illness is embedded. Here the College can, and should, play a crucial role. Many of us deplored the low priority given to research at its foundation and have applauded the recent moves to remedy that deficiency. They will surely help regain something of that 'breadth of vision' which, according to a recent Maudsley lecturer (Jones, 1978), the profession has lost in the past few years. Fortunately, we need no longer argue the case for changing the disciplines required for the task. They have already been changed.

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