Ten books

Chosen by Robert Kendell

These are the ten books that I think influenced me most as a psychiatrist and a teacher. They are not necessarily the books I most enjoyed reading. Nor are they the books I would take with me to some hypothetical desert island. For that I would probably pick a complete Shakespeare, *War and Peace* and as much poetry as I was allowed.

Kraepelin and schizophrenia

I read Mary Barclay's translations of the eighth (1909–1913) edition of Kraepelin's 1896 *Psychiatrie (Dementia Praecox and Paraphrenia* and *Manic–Depressive Insanity and Paranoia*, Kraepelin, 1896*a*, *b*) while I was a trainee at the Maudsley and a young researcher at the Institute of Psychiatry. I was deeply impressed by the vast extent of Kraepelin's clinical experience and his intimate familiarity with the phenomena of psychosis. I learnt a great deal that I could not have learnt from any American or British writer and regretted my inability to read the original German text. His descriptions of dementia praecox and manic–depressive insanity were each based on his experience and his detailed case notes of nearly 1000 patients under his care in Heidelberg or Munich, and as the two together constituted only about a quarter of all admissions his total clinical experience must have covered some 8000 patients. As I read chapter after chapter I became increasingly convinced that Kraepelin had probably understood the varied manifestations and lifetime course of psychotic illness better than anyone else before or since. Indeed, it was clear that he had been well aware of many things that my contemporaries thought they had discovered themselves – like the evidence of intellectual deterioration in chronic schizophrenia and the fact that people with this disorder "do not usually follow a moving pendulum continuously, as normal persons do, but intermittently and hesitatingly".

I was also impressed by Kraepelin's reluctance to commit himself on key issues until the clinical evidence was decisive. Although he was by then in his mid 50s and had been grappling with these issues for nearly 20 years, he still had an open mind about the possibility of complete and lasting recovery from dementia praecox, about the validity of his concept of the paraphrenias as a group of disorders distinct from both dementia praecox and paranoia, and about his decision to classify involutional melancholia simply as a variant of manic–depressive psychosis. I was impressed, too, by his tributes to Bleuler, whom he might well have regarded as a rival. Indeed, in several places he used Bleuler's term 'schizophrenia' instead of his own rubric 'dementia praecox' and incorporated much of Bleuler's detailed analysis of schizophrenic speech into his text. Only "the Freudians" were treated derisively, for their "representation of arbitrary assumptions and conjectures as assured facts, which are used without hesitation for the building up of always new castles in the air . . . and the tendency to generalise beyond measure from single observations".

I read Zinkin's translation of Bleuler's *Dementia Praecox or the Group of Schizophrenias* (Bleuler, 1911) at much the same time as Kraepelin's two volumes and was almost equally impressed, although I noted that Bleuler was more dogmatic than Kraepelin and that whenever they disagreed – as they did over the possibility both of full recovery and of intellectual deterioration in schizophrenia, and about whether autism and ambivalence were sufficiently pervasive to justify being regarded as 'fundamental' symptoms – it was generally Kraepelin who proved to be right.

Mary Barclay's two translations from *Psychiatrie* had originally been published in 1919 and 1921 by the Edinburgh publisher E. & S. Livingstone, with a Foreword by Edinburgh University's first professor of psychiatry, George Robertson. When I moved to Edinburgh as Robertson's successor I contacted Churchill Livingstone and tried to persuade them to republish both works, only to be told by the managing director that there would be no market for them. He then sold the worldwide copyright in both to American publishers, who promptly published them. Unfortunately, he kept the UK copyright, so those new facsimile editions were available everywhere except in my own city and country!

Many years later I read Kraepelin's *Memoirs* (1987) and found them rather boring. The details of his career were interesting, of course, but apart from his zest for travel, his delight in an occasional holiday and his anglophobia, the impression he gave of himself was of a humourless, dedicated workaholic with a bee in his bonnet about alcohol.

Two other books about schizophrenia must be on my list – *Living with Schizophrenia: by the relatives* and *Schizophrenia from Within*. And I am going to cheat and count them as one, because they are very brief and complementary to one another, and were published together by the National Schizophrenia Fellowship in 1974. Both are compilations of short and mostly rather unemotional personal accounts, but together they paint a harrowing picture of what it is like to suffer from schizophrenia or to have a close relative – usually a son or daughter – with the illness. They also reveal how badly both psychiatric hospitals and an almost non-existent 'community care' usually fail to meet the needs of patients and their families, or at least did at that time. Psychiatrists, social workers, nurses, general practitioners and local authority officials are portrayed time and again as uncaring and uncomprehending, content, or perhaps compelled, to restrict their interest and their involvement to a few narrowly circumscribed situations and procedures. An occasional individual – sometimes a doctor, sometimes a nurse or social worker – is described by these anguished patients and mothers (most of the relatives are mothers) as someone who really cared, really seemed to understand and who tried hard and sometimes successfully to provide the help that was needed. But somehow these admirable individuals serve only to emphasise the inadequacies and indifference of most of their fellow professionals. Reading these bleak descriptions made me feel guilty and ashamed, both for my profession and for the health service of which I was a part. I hope that I have succeeded, at least intermittently, in being a better and less insensitive psychiatrist

https://doi.org/10.1192/bjp.177.2.185 Published online by Cambridge University Press

than I might otherwise have been, as a result of reading them.

Durkheim and Illich

I have been influenced, probably more than I realise, by the writings of several sociologists. Goffman, Scheff and Scull all forced me to think about important issues I had never previously considered and to question my previously unquestioned assumptions about mental illness, mental hospitals and the activities and motivations of psychiatrists. But they also irritated me. Although I was forced to concede that their arguments and criticisms had some validity, I was convinced they exaggerated, or even that they were deliberately distorting the facts, or referring only to those that supported their rather perverse conclusions. Durkheim was different. I read *Le Suicide* (1897) (in translation again, I'm afraid) without being irritated at all, despite his blatant assumption that suicide rates are determined entirely by social forces, that individual despair and hopelessness are irrelevant and that "all suicides of the insane are either devoid of any motive or determined by purely imaginary motives". What impressed me was his encyclopaedic knowledge of the suicide rates of every country and province in nineteenth century Europe and his ability to analyse, without any formal tests of statistical significance, the influence on these suicide rates of climate, religion, population density, marital status, fecundity, alcohol consumption, industrialisation, crime rates and political upheavals. Every conclusion seemed to be derived from his data, and his three great unifying concepts of egotistical, altruistic and anomic suicide seemed to explain the fluctuations we still see in national suicide rates and the differences between one country and another better than any alternative. In Durkheim's day, of course, there were no medical services aspiring to reduce the incidence of suicide, but it would not have surprised him to observe that twentieth-century data provide almost no evidence that their efforts are effective.

Although Ivan Illich was a Catholic priest with a doctorate in history rather than a sociologist, his best known book, *Medical Nemesis* (1975), belongs to the same genre as the work of Goffman and Scull. "The medical establishment", he proclaims in his first sentence, "has become a major threat to health". He then elaborates on this theme. The whole of human life is becoming increasingly medicalised. Birth, death, discomfort and unhappiness have all been taken over by doctors, and there is now a pill for every vicissitude of the human condition. The most imposing and costly building in a community is no longer a church or cathedral but a hospital; huge sums are spent on attempts to cure or prevent disease, with ever poorer returns; and the harm done by medical interventions increasingly outweighs any benefits they provide. Worse still, people have become so dependent on doctors that they have lost the ability to cope with any discomfort or handicap, however trivial, and the medical–industrial complex has become dangerously powerful.

It is easy to see the fallacies in Illich's arguments and his highly selective use of health statistics. He was also writing in the 1970s, when benzodiazepines and other tranquillisers were prescribed by the ton for demoralised women, when the American Medical Association was at the height of its influence and arrogance, and when Christiaan Barnard was greeted throughout the world by adulatory crowds for having prolonged a dying man's life for a few years by giving him someone else's heart. Even so, parts of Illich's polemic were echoed by serious medical commentators like Thomas McKeown and it seemed to me that he was drawing attention to some profoundly important and disturbing aspects of contemporary medicine. He certainly made me think.

Alcohol and drugs

During my training in the 1960s psychiatrists were just starting to take an interest in alcohol dependence. The conventional wisdom was that "alcoholism is a disease" and that the answer to the problem was to create special units for treating it, usually along the lines pioneered by Max Glatt, based on group psychotherapy. Unfortunately, alcohol consumption in the UK was increasing rapidly and so too was the number of alcoholics. There was no hope of providing enough treatment units, and anyway their efficacy was unproven. It was against this background that in 1975, while attending a World Health Organization meeting on classification in Geneva, I picked up a slim purple book to read on my way home. It was called *Alcohol Control Policies in Public Health Perspective* (Bruun *et al*, 1975) and had just been published by the Finnish Foundation for Alcohol Studies. Eleven people, of whom the senior author, Kettil Bruun, and five others were Finns, had contributed to its 106 pages, but it was no ordinary conference proceedings. It set out what was for me a completely new way of regarding alcohol problems, a new paradigm. It described the wide range of disorders resulting from excessive alcohol consumption and demonstrated with a series of compelling examples drawn from several different countries that the incidence of these disorders rose and fell as per capita alcohol consumption rose and fell in the general population, and that the distribution of consumption in a population was always unimodal and skewed, with a high proportion of all the alcohol consumed being drunk by a small proportion of the population. The book then described the various control policies adopted in Scandinavia, Canada and the UK to restrict alcohol consumption and, in particular, the evidence that per capita consumption fluctuated predictably with changes in the price, or cost to the consumer, of alcoholic beverages. It concluded that control of alcohol availability was an inescapable public health issue with important international as well as national implications.

By the time I was back in Edinburgh I had read the book from cover to cover and had become a convert to this new paradigm. I used it subsequently as the basis of a formal lecture to the Edinburgh College of Physicians, entitled 'Alcoholism: a medical or a political problem?', and for several years much of my research consisted of attempts to test and refine the population consumption paradigm.

Griffith Edwards and I had both worked for the then Dean of the Institute of Psychiatry, David Davies, as trainees at the Maudsley and had acquired from him our shared interest in alcohol dependence. Griffith had gone on to become director of the Institute of Psychiatry's Addiction Research Unit and in 1971 he was invited by the Royal Society of Medicine to give its Edwin Stevens Lectures for the Laity. His "two lectures on the drug problem", entitled "Unreason in an Age of Reason", were subsequently published by the Royal Society of Medicine (Edwards, 1971) and he sent me a copy. I read it and was enchanted. Under the guise of describing a consultation with a young man, recently sent down from university after a court conviction and fine for possessing a small quantity of cannabis, Griffith flitted to and fro across three continents and several

https://doi.org/10.1192/bjp.177.2.185 Published online by Cambridge University Press

hundred years of history, demonstrating to his listeners and readers the recurring dilemmas affecting mankind's attempts to come to terms with psychoactive drugs, the incompatible and sometimes rapidly changing views of European, Indian and Central American societies about the acceptability of individual drugs, and the arbitrary origins of our own and other cultures' current distinctions between harmless and dangerous, licit and illicit substances. Just as with Kettil Bruun's 'purple book', I was fascinated and immediately persuaded. Ever since, I have been convinced that there is little hope of any rational approach to our own or the international community's current drug problems – which are far worse now than they were in 1971 – without an understanding of history, and I have twice drawn heavily on Griffith Edwards' little book in College publications in an attempt to open other people's eyes as well.

Popper and Kuhn

As a student at Cambridge I read the History and Philosophy of Science as part of my Tripos, and have been glad ever since that I did so. At that time in the early 1950s philosophy textbooks still described scientific laws or hypotheses as deriving their authority from inductive reasoning, because although Karl Popper's *Logic der Forschung* had been published in Vienna in 1935 it had still not been translated into English. Luckily for me, my philosophy lecturer, Russ Hanson, provided his students with a samizdat translation of the crucial chapters and we discussed Popper's new criterion of falsifiability in his seminars. I was totally convinced by Popper's arguments. Indeed, in my youthful and probably rather naïve enthusiasm I could not understand why it had not been obvious to both philosophers and scientists themselves two hundred years earlier that scientific hypotheses could be corroborated or disproved, but never proved. When, in 1959, *Logic der Forschung* eventually appeared in English as *The Logic of Scientific Discovery* I read it from cover to cover and was even more firmly convinced than I had been before. From then on, falsifiability was my touchstone and although I retained a considerable respect for some psychoanalysts, I had no time for psychoanalytic theory. For me it was literature, not science or knowledge.

I read Thomas Kuhn's book, *The Structure of Scientific Revolutions* (1962), while I was in Vermont in 1969 and it made a similarly deep impression on me. As with Popper – and probably other outstandingly lucid original thinkers – I was so convinced by Kuhn's description of how and why a long-established scientific paradigm is replaced by a new one that I had difficulty understanding why what he was saying had not been obvious to everyone all along. I was particularly struck by the similarities he pointed out between political and scientific revolutions, and by his observation that new paradigms are almost invariably introduced by people who are either young or at least new to the field, and that established authorities usually fight a prolonged rearguard action to defend the old paradigm despite its increasingly obvious failings. During the 1970s, American psychiatry underwent its own paradigm change as the bastions of psychoanalysis on the East and West coasts crumbled before the intellectual onslaught of Eli Robins and his protégés from the mid-West, with their insistence on publicly verifiable data, operational definitions and matched controls. Familiarity with Kuhn's ideas made it much easier for me to understand, and at times to be amused by, that conflict, but I still failed to foresee how quickly and completely the new 'neo-Kraepelinian' paradigm would triumph.

The most obvious manifestation of this triumph was the publication in 1980 of the revolutionary third edition of the American Psychiatric Association's (APA's) *Diagnostic and Statistical Manual of Mental Disorders*, known to the world as DSM–III. Its multiple axes, operational definitions, novel grouping of syndromes and extensive field trials represented a radical break with the past, and this was rather brutally emphasised by the decision to discard time-honoured concepts like neurosis, hysteria and manic–depressive psychosis. Robert Spitzer, the chairman of the Task Force that developed DSM–III, and I had common interests and had been colleagues in the late 1960s, so I was aware of the vast amount of hard work, discussion, argument and persuasion that had gone into the preparation of this novel classification. (There had been 14 different advisory committees dealing with different parts of the glossary, and the field trials to assess the reliability of its 200 operational definitions had involved over 12 000 patients and 550 psychiatrists.) I was also aware how close the APA's Board of Trustees had come to rejecting it, and was pleased and relieved when they finally decided to endorse it. Even though DSM–III and its successors have sometimes been misused as 'cookbooks' to enable poorly trained psychiatrists to make diagnoses without thought or understanding, I am still convinced that it is one of the most important and influential books to be published in my professional life time. It led to a quantum change in the quality of most clinical and epidemiological research, and without it the World Health Organization would never have been able to introduce the radical changes embodied in ICD–10. I was strongly tempted, therefore, to include DSM–III in this list of ten books. In the end I did not for two reasons. As I was already convinced of the need for most of its innovative features before it appeared, I cannot really claim that it influenced me very greatly. Nor, strictly speaking, have I ever read it, although I have referred to it and consulted it so often that my personal copy is now disintegrating.

Illness and mental illness

The findings of the US/UK Diagnostic Project, for which I worked between 1966 and 1970, had made me keenly aware of the need, at least for research purposes, of operational definitions for individual mental illnesses like schizophrenia and mania. This in turn led me to realise that mental illness itself had never been satisfactorily defined, and when I moved to Edinburgh in 1974 I devoted my inaugural lecture to a not very successful attempt to provide a satisfactory definition of the term. As an indirect consequence of this, I subsequently received a complimentary copy of a 750 page volume entitled *Concepts of Health and Disease*, edited by three American academics – Caplan, Englehardt and McCartney. It was a compilation of 48 essays about health, illness and disease and the relationships between them. They spanned over 300 years and were written by a wide range of European and American philosophers, social scientists, physicians, psychiatrists and historians. I could never have found even half these essays myself by searching conventional databases like Medline, and they gave me a far better informed and wider understanding of the problem I was grappling with than I had had previously. After reading these essays I was convinced that labelling any given

https://doi.org/10.1192/bjp.177.2.185 Published online by Cambridge University Press

condition as a disease or disorder necessarily involves a value judgement, which means that different individuals and cultures can quite legitimately reach different conclusions. And after several attempts to lecture to medical audiences about the concepts of health and disease I was equally convinced that most doctors have little interest in the meaning of these fundamental terms, probably because, without realising it, they want to retain the freedom to use them inconsistently, while still imbuing their chosen usage with medical authority. So I am now sadder and wiser, and think I understand why a useful definition of mental illness is so elusive. I am still convinced, though, that although a lack of definitions may not matter much in most branches of medicine, the lack of an unambiguous definition of mental illness allows lawyers, judges and politicians to keep moving what ought to be our goal posts.

Plain Words

My mother was an English teacher and she gave me a profound respect for the English language, both its literature and its grammar. She also gave me a copy of Sir Ernest Gowers' *Plain Words* (1954), a slim volume I enjoyed browsing through so much that as a teenager I kept it for a long time by my bedside. And when my own children were teenagers I gave copies to them. Gowers taught me how to write, using simple Anglo-Saxon words and short sentences wherever possible. He also taught me to be wary of fashionable phrases, overworked metaphors, pedantic circumlocutions and the ponderous, pompous language beloved by officials, as well as helping me to avoid the misuse of words like mitigate, decimate and disinterested. The only thing he failed to do was to teach me how to spell. And it took Richard Asher, whose elegant writings I had greatly admired as a medical student and for whom I subsequently worked as a houseman, to teach me that good medical or scientific writing usually involves much revision and many drafts. I have enough insight to realise that my preoccupation with grammar and phraseology has made me a great trial to many committee secretaries, but I suspect it has also enabled me to hold readers' attention, express complicated ideas relatively clearly and succinctly, and get some pieces of rather mundane research published in better journals than they deserved.

American Psychiatric Association (1980) *Diagnostic and Statistical Manual of Mental Disorders* (3rd edn) (DSM–III). Washington, DC: APA.

Bleuler, E. (1911) *Dementia Praecox or the Group of Schizophrenias*. Trans. J. Zinkin (1950). New York: International Universities Press.

Bruun, K., *et al* (1975) *Alcohol Control Policies in Public Health Perspective*, Vol. 25. Finnish Foundation for Alcohol Studies.

Caplan, A. L., Engelhardt, H. T. & McCartney, J. L. (1981) *Concepts of Health and Disease: Interdisciplinary Perspectives*. Reading, MA: Addison-Wesley.

Durkheim, E. (1897) *Le Suicide*. Trans. J. A. Spaulding & G. Simpton (1951). New York: Free Press.

Edwards, G. (1971) *Unreason in an Age of Reason*. London: Royal Society of Medicine.

Gowers, E. (1954) *The Complete Plain Words*. London: HMSO.

Illich, I. (1975) *Medical Nemesis: The Expropriation of Health*. London: Calder & Boyars.

Kraepelin, E. (1896*a*) *Psychiatrie* (8th edn). Reprinted (1971) in part as *Dementia Praecox and Paraphrenia* (trans. R. M. Barclay). Huntington, NY: Robert E. Kreiger.

__ (1896*b*) *Psychiatrie* (8th edn). Reprinted (1976) in part as *Manic–Depressive Insanity and Paranoia* (trans. R. M. Barclay). Huntington, NY: Robert E. Kreiger.

__ (1987) *Emil Kraepelin Memoirs*. Berlin: Springer Verlag.

Kuhn, T. S. (1962) *The Structure of Scientific Revolutions*. Chicago, IL: University of Chicago Press.

Popper, K. R. (1959) *The Logic of Scientific Discovery*. London: Hutchinson & Co.

Robert Kendell Honorary Professor, Department of Psychiatry, Edinburgh University, 3 West Castle Road, Edinburgh EH10 5AT

https://doi.org/10.1192/bjp.177.2.185 Published online by Cambridge University Press