

THE ROCK CARLING FELLOWSHIP

1984

AN ANTHOLOGY
OF FALSE
ANTITHESES

Sir Douglas Black

THE NUFFIELD PROVINCIAL
HOSPITALS TRUST

1984

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THE AUTHOR

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INTRODUCTION

It is, of course, possible to write an essay so free from prejudice that it is also totally devoid of interest. The thing can be done, and often has been. But I shall try to avoid this particular temptation. We all have biases, some of them conscious, and for such biases we have at least the opportunity, if not to avoid them, at least to make them explicit, and that perhaps represents our minimum obligation. But we must also have unconscious biases, of which by definition we are unaware—it would therefore be unreasonable to expect us to disclose them. The only possible way in which we can help the reader to detect such biases is to say something of the general nature of our own experience. Of course, it is not appropriate to convert a Rock Carling Monograph into an autobiography; but I think it is fair to state objectively the types of posts which I have held, leaving the reader to ascribe to them—no doubt in the light of his own prejudices—any possible bias which might arise simply from doing the prescribed job. To conclude this introduction, I shall then confess those biases of which I am myself aware.

After conventional Scotch schooling, and the medical course in St Andrew's University, I spent five years of clinical research in Oxford and Cambridge before joining the RAMC, which for me meant further research on sprue, and a notable lack of military glory, stationed as I was in Poona. After demobilization in 1946, I climbed the academic ladder in Manchester, thereby learning the practical difficulties of combining practice, teaching, and research at a reasonable level, while at the same time devoting sufficient attention to administration to keep the ship afloat at some distance from the rocks. (In the sequence of titles from Lecturer through Reader to Professor, the middle one is the most beguiling, with its lack of implied

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commitment to do anything which is not pure pleasure—but in actual practice the differences are not great).

In the mid-sixties, I was appointed a member of the Medical Research Council, which Max Rosenheim characterized as one of those rare organizations whose sole purpose and justification is to do good. At the end of my four-year stint, I could not foresee that I would be recalled for a second term as Chairman of the Clinical Research Board, still less that I would later become an *ex officio* member as Chief Scientist to the DHSS. From my decade of involvement with research administration at the national level, I was translated by election to the Presidency of the Royal College of Physicians, which brought me closer once again to the mainstream of professional practice. I was fortunate indeed to hold this post for six years; and doubly fortunate after demitting office to be appointed the Rock Carling Fellow for 1984.

I will not insult my readers by prescribing what biases they should infer from the individual facets of a career which might be described as varied. The chances which have enabled me to function in a variety of environments have I believe increased my tolerance—though this of course may be a simple effect of ageing. But I should end this introduction by declaring the more general biases of which I am conscious. I have a bias in favour of religion, which brands me with a careless failure to react against my upbringing as a son of the manse; but in actual belief I steer a course between the Symplegades of scepticism and enthusiasm. I have a bias in favour of science, and of its application to medical practice; to imply an antagonism between science and humanity is to be guilty of one of the false antitheses which I shall discuss later. I am a professional elitist, within the framework that professionalism stands or falls to the extent to which it places the interests of the client above other considerations. I think that individuals are more important than society; but that there still has to be a balance between rights and duties, with perhaps in the public interest some favouritism towards duties. In politics, I regret what seems to be the

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demise of a consensus in support of the welfare state, as well as the general debasement of parliament in response to the antics of the media. What still reconciles me to living in this country is the frequent demonstration that to live in any other country would be worse.

The monograph which follows might perhaps be better described as a polygraph, for it draws its illustrations from a variety of areas. But there is a common theme, implied in summary form in the title, that in medicine, as in many other fields of human activity, there arise misunderstandings and even heated controversies which could be resolved if the participants would join in a search for the common ground, rather than immerse themselves in entrenched positions. There is, of course, a value in reforming zeal and divine discontent; but not all zeal actually accomplishes anything, and much if not most discontent is an expression of human weakness. As Julian Huxley and others have argued, man as a species is now largely responsible for his own future, and that of the planet in which we live. We are not doing too well, but I see more hope in building on the seeds of mutual aid which Kropotkin thought to discern in nature, rather than in making every issue an excuse for aggression. My own zeal is for compromise, which opens the accusation of sitting on fences; but when the fields on each side are deadly, a good broad fence is not a bad place to be.

I owe a major debt to the Nuffield Provincial Hospitals Trust for the award of the Rock Carling Fellowship; to the Wellcome Trust for providing me with office accommodation and secretarial help after one of my retirements; and to Mrs Jean Shephard for preparing the MS in all respects bar writing it.

The nature of error

In the first book of *Pseudodoxia Epidemica*, his treatise on vulgar and common errors, published in 1646 when he was 41 years of age, Sir Thomas Browne considers why it should be that gross errors prevail (1). He opens with the general statement that 'the first and father cause of common error, is the common infirmity of humane nature'. Conformably to the beliefs which he had earlier expounded in *Religio Medici*, he finds the origin of error in the successful temptation of Adam and Eve, and its perpetuation in the continuing activity of Satan. But as a further *general* cause of error he taxes the common people with credulity—the mass of people are 'more deceivable' than individuals; and also with relying more on the direct evidence of the senses than on reason, a point which he illustrates thus,

The greater part of mankind having but one eye of sence and reason, conceive the earth far bigger than the Sun, the fixed Stars lesser than the Moone, their figures plaine, and their spaces from earth equidistant.

In addition to these general and perhaps controversial causes of error, Sir Thomas distinguishes some *particular* causes—'misapprehension': 'credulity and supinity': and 'obstinate adherence unto Antiquity, or Authority'.

Under misapprehension, he distinguishes between *verbal* and *real* fallacies. Verbal fallacies arise from taking literally what is meant metaphorically. As an example—perhaps not the happiest possible, in view of what we now know of fivism—Sir Thomas gives the proscription of beans by Pythagoras, though he himself was very fond of them; but in this 'he had no other intention, then to disswade men from Magistracie, or undertaking the publicke offices of state; for by beanes were the magistrates elected in some parts of

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Greece'. Real fallacies can arise in various ways. For example, by arguing from the particular to the general, 'as when we conclude the vices or qualities of a few upon a whole Nation'. Or by holding false causes as true, for example,

Hereupon also are grounded the grosse mistakes, in the cure of many diseases, not only from the last medicine, and sympatheticall receipts, but amulets, charms, and all incantatory applications, deriving effects not only from inconcurring causes, but things devoid of all efficiencie whatsoever.

Credulity is not limited to the simple. Geber, Avicenna, and Almanzor were apparently satisfied with the explanation of earthquakes 'From the motion of a great Bull, upon whose hornes all the earth is poised'. Supinity is a lazy neglect of enquiry, 'rather believing then going to see, or doubting with ease and gratis, then beleiving with difficulty or purchase'—so that, 'by a content and acquiescence in every species of truth, we embrace the shadow thereof'.

Sir Thomas speaks strongly of the dangers of relying on authority, and especially on ancient authority—

But the mortallest enemy unto knowledge, and that which hath done the greatest execution upon truth, hath been a peremptory adhesion unto Authority, and more especially the establishing of our beliefe upon the dictates of Antiquity. . . . And the further removed from present times, are conceived to approach the nearer unto truth itselfe. Now hereby me thinks wee manifestly delude ourselves, and widely walke out of the track of truth.

He gives many examples of plagiarism among ancient authors, so that what appears to be multiple testimony is copied from a single doubtful source. As an example of something which is 'neither consonant unto reason, nor correspondant with experiment', he asks:

'What foole almost would believe, at least, what wise man would rely upon that Antidote delivered by Pierius in his Hieroglyphicks against the sting of a Scorpion? that is to sit upon an Asse with our face towards his taile; for so the paine leaveth the man, and passeth into the beast.

These various forms of error, set out with elegance by Sir Thomas Browne, are compatible with intellectual honesty, so long as the promulgator of error has the gentlemanly

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decency of being himself deceived before setting out to deceive others. But we also have to recognize the possibility of fraud in science, of which 'Pildown man' is perhaps the most famous fraud to have been discovered—as was, I believe, first suspected by Professor David Waterston, who taught me anatomy in St Andrew's University. Plagiarism, piracy and the concoction of false data, with appropriate examples of each of these frauds, have been described by Altman and Melcher (2).

As Medawar (3), following Popper (4), has pointed out, the problem of error is not satisfactorily dealt with by inductionist theories of the scientific process, which see error arising from the simple misapprehension of the facts given to us by nature. It is rather the case that error is a reflection of human fallibility in speculating about the facts; and such error is then capable of being revealed by critical search for facts or even for new observations which will constitute a valid test of the current hypothesis, and which, when at variance with it, must lead to its rejection, or better, to its constructive modification. Our picture of the natural universe is a compound of the brute facts of nature; of the prevailing scientific paradigms (which have of course been to some extent, but never completely, purged of error by earlier scientists); and of course our own apprehension, by no means error-proof, both of what we perceive directly of nature and of what we have learned from natural science. The extent to which our own cast of mind, moulded as it is by prevailing scientific notions, affects our perception of 'reality', was described in the lapidary phrase of William Whewell 'There is a mask of theory over the whole face of Nature' (5).

Given the possibility of error even in our concepts of inanimate nature, and given the likelihood that this possibility will not be lessened as we ascend the scale of biological organization from the organic molecule through organelle, cell, and tissue to the whole organism, and thence to the ecological complex and even to human society—given these possibilities, it must be with some trepidation that I venture to analyze some of the phenomena involved

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in health care, and not even in isolation, but in terms of the relationship between them. My general theme, expressed in my title, is the possibility that an excess of taxonomic zeal, or even of professional separatism, may have created dichotomies where none need exist, and that to the detriment of the service. If by any chance the upshot of such an analysis should be to encourage mutual confidence and sympathy between different branches of the 'greater medical profession', I shall be content (I am of course using T. F. Fox's phrase, which embraces all those engaged in providing health care, including that given by volunteers and by relatives).

Having thus committed myself to using as a model for discussion 'the false antithesis', I am faced with two problems. The first is to avoid the possible implication that the universe can be explained by a dialectical approach, Hegelian, materialist, or whatever. The second, given that some entities are truly antithetical, and that others are in no way antithetical, is then to discover entities, or rather pairs of entities, which satisfy the three criteria of being (1) apparently and even formally antithetical, but falsely or at least misleadingly so; (2) relevant to the provision of health services; and (3) worth discussing at all.

I believe that Utopianism, historicism, and indeed all systems of explanation which purport to account for the totality of events are both false, in neglecting the infinite variety of the universe; and dangerous, as having led to the major tyrannies of our own lifetime. On the theoretical aspect, I find entirely convincing the devastating criticism of the systems of Hegel and Marx in the second volume of Karl Popper's *The Open Society and its Enemies* (6). And as a pragmatist, I detest the practical consequences of the political philosophies which have derived from Hegel, extrapolated by Hitler; and from Marx, extrapolated by Stalin. To be positive, I am an individualist, not a collectivist; a meliorist rather than an optimist, as these terms are defined by Medawar (3); and my terrestrial hope lies in piecemeal social engineering, within a liberal and open society (6).

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Where then am I to discover appropriate antitheses, as defined above. I think in two main areas—the misunderstandings which engender depreciation of the scientific approach to problems, exemplified by Ian Kennedy's dispraise of 'scientific problem-solving'; and the dichotomy which has been postulated between the 'medical' and the 'social' models of health care.

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Naturally, not all such misunderstandings lend themselves readily to being cast in the antithetic mould. Elsewhere, for example (7), I have drawn attention to two common misconceptions of science, both fostered by science fiction—that it is ‘some kind of mystique, whose nature can only be understood by scientists themselves, and which involves a mode of thinking which is quite different from other intellectual activities’; or, at the other extreme, that it is no more than a great repository of facts, a sort of macropaedia whose pages can only be turned by scientists. Scientists themselves are sometimes guilty of thinking that their occupation involves little more than precise mensuration or the conscientious application of methodology; whereas it is much more a cast of mind, which combines imaginative creativity with rigorous criticism, especially of one’s own findings and hypotheses. It is because of such misunderstandings, and others like them, that we have the paradox that people who are dependent on applied science for food, clothing, communication, and indeed their entire material life-style are willing to bring to their consideration of science a combination of ignorance and hostility which they would be ashamed to deploy in considering any other large area of human activity. There is of course a distinction to be made between what Medawar (3) calls ‘scientism’, which he defines as ‘the belief that science knows or will soon know all the answers’ (presumably all the ‘worth-while’ answers); and ‘poetism’, which he defines as

the undisciplined exercise of the imaginative faculty to produce hypotheses which are held to be true, and defended against all criticism,

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merely because of their supposedly high inspirational origin or because they are specially well put or make an unusually strong appeal to some dark visceral mystic predilection of their authors.

But these are extremes, united only in their narrowness and stupidity; language which denotes objectively, and language which stirs the emotions, are both of value, provided that the purpose of each is both known by the author, and apparent to the recipient.

Confusion between the scientific and the literary use of language can be avoided with care in the appropriateness of the use of both; and of course both styles have a common root in language itself. The renaissance ideal of a mind open to both cultures may have become more difficult of attainment because of the proliferation of knowledge and of specialization; but in its elements it remains possible, and it is certainly desirable. Although I have touched on it elsewhere (8), it would take me too far out of my way to develop further the relation between scientific and literary activity. I wish instead to consider two antitheses, related to science, which may be false. The first of these relates to the methodology of science—induction or hypothecation; the second to its taxonomy—pure or applied.

'THE SCIENTIFIC METHOD': BACON OR POPPER

As a believer in titles which are pithy rather than compendious, I have used 'Bacon' as shorthand for the view that scientific beliefs are derived from brute facts by logical induction; and 'Popper' as shorthand for the 'hypothetico-deductive' view, which suggests that scientific hypotheses are not so much the result of random observations, but rather the cause of planned observations made to test the truth or falsehood of propositions which flow deductively from the hypothesis. It will not have escaped your notice that Bacon antedates Popper by some three hundred years; from which it flows that Bacon has had successors of the calibre of John Stuart Mill, whereas Popper was anticipated

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to a great extent by Claude Bernard and William Whewell. In the Jayne Lectures for 1968, reprinted in *Pluto's Republic* (3), Sir Peter Medawar gives a clear account of the issues, from the point of view of a biological scientist who broadly accepts Popper's views, and has indeed done much to bring them to a wider audience. His first lecture considers the paradox that almost without exception scientists lack interest or at any rate expressed interest in scientific methodology; the second lecture, 'mainly about induction', outlines the Baconian view; and the third, 'mainly about intuition', traces the development of Popper's 'hypothetico-deductive scheme'. Medawar is able to point to a number of areas in which Popper's scheme is strong, where the inductive scheme is weak—it distinguishes between discovery and testing; it encourages systematic rather than random observation; it allows for the remodelling of hypotheses by feedback from relevant observation; and it accounts for error as a facet of human fallibility, not in making observations, but in accounting for them. He also acknowledges potential weaknesses in the scheme—the possible replacement of a 'chaos of facts' by a 'chaos of hypotheses'; the possibility that an apparent falsification may itself be false, leading to unjustified rejection of an hypothesis; and its failure to account for the way in which hypotheses are generated. In this last respect, it resembles in its failure the alternative inductive scheme; just as we cannot see how the cumulation of random observations can ever 'prove a natural law', no more can we provide a calculus for the leap of the imagination which generates an hypothesis which has sufficient of reality in it to be worth putting to a critical test.

In his Herbert Spencer Lecture of 1979, Henry Harris (9) fastened on the possible error in supposed falsifications of an hypothesis. Thus the 'lack of verifiability' of scientific hypotheses, emphasized by Popper, may be ultimately matched by a 'lack of falsifiability'. To quote what seem to me to be two key sentences:

Popper's falsificationist doctrine thus leads eventually to the conclusion that no scientific proposition, at least none that rests on human

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observation, can ever be proved right or proved wrong. Experimental scientists find this difficult to accept.

The central role accorded to the hypothesis by Popper is weakened, but not of course destroyed, by instances where a false hypothesis has led to a major discovery—Harris mentions Columbus, and one might also mention that a belief in phlogiston did not prevent Joseph Priestley from discovering oxygen. More positively and importantly, there are beliefs in the observational and biological sciences which are unlikely to be falsified. Harris gives as examples the circulation of the blood, demonstrated once for all, even if incompletely as regards the capillaries, by William Harvey; and more recently, the circulation of lymphocytes between blood and lymph. And to quote further:

I do not believe that it will ever be shown that the blood of animals does not circulate; that anthrax is not caused by a bacterium; that proteins are not chains of amino-acids. Human beings may indeed make mistakes, but I see no merit in the idea that they can make nothing but mistakes.

Harris is not keen either on 'flair', or on logic. Of the first, he says 'Scientists with a "flair" for picking the right hypothesis simply have a more profound grasp of their subject'. And of logic,

The conclusions that scientists draw from their observations are imposed not by the rules of logical entailment but by the operational rules laid down by man's evolutionary history.

One of these 'rules' must be the nature of the real world.

I certainly find Popper's system, particularly as expounded by Medawar, to be more attractive than its inductivist rival. But I think that the two systems can be not so much reconciled as annulled by on the one hand the radical criticism of 'falsifiability' as an operational tool for dealing with hypotheses; and on the other hand the existence of vast stores of knowledge whose logical provenance is quite conjectural but which are pragmatically validated (10). Popper himself was well aware of the existence and value of such stores of knowledge, which he characterized as 'World 3', additional to the objective world of material things (World 1) and the subjective world of

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minds (World 2). The way in which all of us, including scientists, think about things is very much a product of the present state of World 3, which is of course constantly being added to by creative human activity in all spheres. Such activity may be expressed as tangible artefacts—a painting, a new invention, a piece of music, or a textbook—or, more subtly, by a way of looking at and further exploring a set of problems. From time to time, new sciences develop a consensus of theory and of methodology, to which Kuhn's term 'paradigm' (11) could be applied. This paradigm largely governs the behaviour of practitioners of that science, but it is also open to change, at which point what has been 'normal science' gives way to a 'scientific revolution', which begets another paradigm, within which a new form of 'normal science' is practised.

THE DANGERS OF TAXONOMY: 'PURE' AND 'APPLIED' SCIENCE

In this section, I shall try to indicate that over-emphasis on the distinction between pure and applied science can lead not merely to undervaluing the importance of practical application in the development of science itself, but also to serious distortion of the framework of support for scientific activity.

In taking the great name of Francis Bacon as a symbol of the inductive approach in the preceding section, I was of course reflecting the position of his followers, rather than that of Bacon himself, who envisaged a continuous interaction between observation and reasoning, 'a true and perfect marriage between the empirical and the rational faculty'. In the same way, it is his followers rather than Bacon himself who have overemphasized the distinction which he made between *experimenta lucifera* (those which shed light), and *experimenta fructifera* (those which yield fruits). He himself was not so neat in his separation of theory and practice, for he regarded his philosophy as carrying the promise of infinite utility (12). In warning the young scientist against 'the *snobismus* of pure and applied science' (13), Peter

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Medawar gives a quotation from Thomas Sprat, the earliest historian of the Royal Society, which likewise exempts Bacon from such a confusion. Answering those who labour the distinction between Bacon's two types of experiment, Sprat says

But they are to know, that in so large and so various an Art as this of Experiments, there are many degrees of usefulness: some may serve for real and plain benefit, without much delight: some for teaching without apparent profit: some for light now, and for use hereafter: some only for ornament, and curiosity. If they will persist in contemning all Experiments, except those which bring with them immediate gain, and a present harvest; they may as well cavil at the Providence of God, that he has not made all the seasons of the year, to be times of mowing, reaping and vintage.

Intellectual snobbery towards practical applications is not merely an undesirable trait of character; it has cost us dear, as a nation, in our failure to develop, and even to exploit commercially, the fruits of such discoveries as fibre-optics. Like other snobberies, it can be inverted by the practical man who despises any scientific activity which does not visibly lead to practical application in short order—like the man who asked (some time ago) what was the use of Faraday's experiments on electro-magnetism, and was rewarded with the epigrammatic but effective answer, 'What use is a baby?'

The essential interdependence between 'pure' and 'applied' science has been well described by Sir Frederick Dainton and his colleagues (14):

These distinctions are often not very useful and lead to popular misconceptions which can be damaging. There is still a tendency to classify engineers and technologists as 'applied' scientists. The adjectives 'pure' and 'applied' imply a division where none should exist and their use can be harmful. In the course of his work the engineer or technologist makes use of experiment and theory in just the same way as the 'pure' scientist, and at least as great demands are likely to be put upon his intelligence, judgment and imagination. Moreover, advances in knowledge or improvements in the use of any one of 'pure science' or 'engineering' or 'technology' depend enormously on progress in the other two.

It is ironical that this clear criticism of the separatist approach to scientific research appeared in the same

discussion document as the 'Rothschild Report' on the organization and management of government R and D; and that it was the latter which was accepted, in principle and to some extent in practice, by the Heath administration. (These were heady days—soon after, the government responded with equal precipitancy to the Nixon initiative which proposed to conquer cancer by putting billions of dollars into mission-oriented research. Admittedly, with perhaps some foresight of our future penury, our echo of Nixon did not carry with it any new money—instead, the Medical Research Council was forced to redeploy funds which would have otherwise supported other areas of research. I would personally find it rather difficult to draw much distinction between the results of the heavily funded US and the lightly funded UK programmes; the lesson being that lavish finance is impotent in the face of unripe time).

The Rothschild Report (15) clearly carried conviction with politicians and civil servants, but was not well received by the majority of scientists, and particularly those working in the areas covered by the Medical Research Council. As one of its critics, my attempt at a summary of what is already a rather terse document is open to question; so I would recommend serious students, in all fairness, to read the Report for themselves. The important elements in the Report seem to me to be a separation between basic and applied R & D, based on a definition of the fruits of applied R & D; and the 'principle' that applied R & D 'must be done on a customer—contractor basis'. The practical consequences flowing from these beliefs were that service departments of government, such as the health departments, should develop a capability to act as customers for research; that applied research done by Research Councils, such as the MRC, should be carried out under contract to the service departments; and that the service departments should be given financial leverage by the transfer of part of the budget of the research council (in the case of the MRC, approximately a quarter) to the service departments, the expectation being that it would be used to commission the research which the service department needed.

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The end-product of applied R & D could be an actual product, a method of production, or a method of operation (p. 3). The customer/contractor basis was outlined thus: 'The customer says what he wants; the contractor does it (if he can); and the customer pays'. For an arrangement based on these principles to work in a particular field of research, it would be necessary that the end-product should be definable, preferably in rather concrete terms; and that there should be a fruitful dialogue between customer and contractor.

The first of these prerequisites seems to me to fail in the areas relevant to health and disease, apart from a few rather limited problems of comparatively narrow importance, such as ventilation in theatres, for which a contract was in fact negotiated. But who, other than the hero of Watergate, would think of contracting for the cure of cancer, or the prevention of cerebrovascular accidents?

The importance of dialogue between customer and contractor was clearly recognized by Rothschild, in a key paragraph (p. 9).

No system for the administration and prosecution of applied R & D will work efficiently and successfully without a continuing dialogue between the customer, the Chief Scientist, the Controller R & D and those concerned with the actual prosecution of the R & D. In an efficient and successful organisation, all those concerned act and behave as a team in spite of formal accountabilities. Without the accountabilities, however, both efficiency and the probability of success are reduced.

I have personally always seen formality and accountability as hindrances to teamwork, which makes me, rightly or wrongly, disagree profoundly with the last sentence. But there is a further point, relating not to the general principle perhaps, but to the particular case of dialogue between the DHSS as 'customer', and the MRC as 'contractor'. The MRC had behind it half-a-century of successful management of research, and its Secretary was equipped to step into the role of 'Controller R & D', thereby completing the 'contractor' side of the dialogue. The DHSS had played a

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creditable, and indeed a pioneering role, in health services research, and was indeed the main source of funding for that important purpose; but it lacked the expertise to specify the problems which might be open to solution by the available techniques of physiological and clinical research. At the time of the Rothschild Report, there was no Chief Scientist in the DHSS, to try to present to the MRC a coherent programme of contractable and relevant research. Dr R. R. H. Cohen, who had played a notable part in the development of the health service research programme, took on the task of being the first Chief Scientist, even though he was approaching retirement. He set up a viable organization of committees, which would bring external expertise to the aid of the DHSS in forming a research programme; and of 'research liaison groups', which would help the 'customers' within the DHSS to try to express their requirements in researchable terms.

Soon after, and somewhat to my surprise as one who had been a critic of the customer/contractor principle, and of the transfer of MRC funds in part to the DHSS, I became Chief Scientist, so what follows becomes something of a personal narrative for the next few years.

At first, things went reasonably well, particularly as regards recruitment of external experts from a wide range of disciplines to join the Chief Scientist's Committee, and the various Research Liaison Groups (RLGs). However, it fairly soon became apparent that the commissioning process from the MRC was producing a bureaucratic nightmare of accounting; and, more serious still, that the customer organization within the DHSS was too rudimentary to match the well-developed potential contractor. The network of RLGs had only spread to about half the divisions of the DHSS when the cuts began—the cuts in civil service manpower nipped the servicing of RLG's in the bud; while cuts in the transferred funds were not particularly helpful in maintaining good relations with the MRC, in spite of notable forbearance by individuals. For a detailed and independent account of the various attempts made to overcome these difficulties, there is a valuable 'case study'

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by Maurice Kogan and his colleagues (16). The arrangements have also been subject to official review (17), following which the 'transferred funds' have been restored to the MRC.

Since I was Chief Scientist during the four years in which some part of the Rothschild hope faded, I cannot escape some responsibility. I cannot obviously exclude incompetence on my part, but what I can exclude is deliberate incompetence—I really did try to work the system. If you accept that plea, the question then arises—did the system fail because of the cuts in manpower and resources; or was it to some extent doomed to failure? Being careful to underline '*to some extent*', I believe the latter; and I can even try to define 'some extent'. I think the arrangements with the MRC were ill-considered, and that the business of 'transferred funds' impaired collaboration between the health departments and the Council; and it is my fervent hope that without this incubus, true partnership can be restored. While the most obvious damage, in the shape of financial uncertainty, was done to the MRC, I think that more serious, even if less obvious, damage was done to the DHSS, and through it to health services research. The administrative arrangements for research management were never lavish—after all, research accounts for less than 1 per cent of the DHSS budget; and the attempt to commission biomedical research from the MRC distracted them severely from what had previously been, and should in the future again become, their main task, for which they are well-fitted, the encouragement of research related to the provision of health and personal social services, and even of social security. The DHSS have direct responsibility for these services, and also direct access to the sources of statistical information needed for this type of research; but they need the help of the research councils, both MRC and SSRC, for two main reasons. The first is to bring in from the University and research world advisers from a wide range of disciplines—medicine and nursing, yes, but also economics, statistics, psychology, and sociology. The second, which may seem mundane but is nevertheless important, is that the

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DHSS at present cannot provide either training posts, or posts with tenure for research; the Councils can.

Although I was happy—who would not be?—to be rescued by the College in 1977, I am not doleful about my time in the DHSS. Health service research is as important now as ever, perhaps more so; and the DHSS can now once again give it undivided attention. Also, links of an informal and therefore more productive kind have now been established with both the MRC and the SSRC.

3

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Some fifteen years ago, in an account of medicine for the lay public (18), I wrote as follows:

The essential difference between the medicine of today and the medicine of a hundred years ago is that our intellectual comprehension of disease has entered another dimension. In so far as medicine ever can be a 'success story', the success comes ultimately from science.

But I went on at once to acknowledge that the application of this scientific knowledge depends on the individual doctor's art in understanding the patient's character—'Otherwise, the patient will reject the doctor, science and all, and suffer from it'. I now feel, more strongly than ever, that a false antithesis between the science and the art of medicine lies at the root of serious misconceptions of what doctors can do: and, just as important, what they cannot be expected to do. Particular examples of false antitheses are the subject of succeeding sections, but I would like to summarize my general theme in a single sentence—'The true antithesis of "caring medicine" is not "scientific medicine", or "high technology medicine", or "hospital medicine", or "academic medicine", or "orthodox medicine"; it is, quite simply, "bad medicine"'.

Some of the misunderstanding of the importance of science in medicine, and of the dangers of 'non-science', stems from misunderstanding of the pragmatic and essentially revisionist character of scientific thinking. Dogmatism may be and is displayed by individual scientists; but it then represents a flaw in their own character, not an attribute of science itself. There are, of course, external pressures on the doctor in his clinic, even more than on the scientist in

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his laboratory, to make clear-cut statements; but while he need not share the full measure of his uncertainty with patient or relative, he must himself remain fully aware of how much uncertainty persists in medical knowledge, in spite of all the advances which have been made. A lively consciousness of uncertainty not only fosters humility; it is also a spur to enquiry, which if we are very fortunate, may bring us some scintilla of new light. Before proceeding to my specific instances of false antitheses related to the practice of medicine, I would just remind you of the two points made in the preceding section: that there is no prescription of a mode of thinking which will infallibly lead to scientific discovery; and that rigid distinction between pure and applied science is difficult, empty, and even dangerous.

I have already made clear my conviction that medical science is a most important part, though not of course the whole, of medicine as currently practised; and I have also just recalled the misconceptions which can exist both of the nature of scientific thinking, and of the way in which science can be applied. Such misconceptions may well contribute to the first of my 'false antitheses' relating to medicine, the distinction made between 'orthodox' and 'alternative' medicine. This is a matter of such importance, and so pregnant with danger to progress in medical knowledge and practice, and thus to the welfare of patients, that I am sure it deserves the priority which I am according it.

MEDICINE AND ALTERNATIVE MEDICINE

At first glance, I must seem to be flying in the face of language itself when I venture to use the adjective 'false' in relation to the antithesis implied in the above title. But surely for an antithesis to be true, it must imply a contrast between two terms each of which is capable of rather clear-cut definition. But even 'medicine' is very difficult to define, and 'alternative medicine' quite impossible. The difficulty

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in defining 'medicine' stems from its changing nature, rightly responsive as it is to advances in relevant knowledge. Of course, the objects of medicine remain the same, to preserve health, and to palliate or cure disease. But the models or paradigms of health and disease are ever changing, as we learn more about nutrition, about immunology, about drugs and their actions; and as new techniques of biochemistry and of imaging extend the scope of our enquiry into bodily function and structure in health and disease.

The impossibility of defining 'alternative medicine', as I see it, stems from different causes. It is not a matter of steady growth in knowledge leading to evolution in practice. The claims are much more protean. At one moment, we are asked to respect the age-old wisdom of the East, expressed in Chinese or Indian systems of indigenous medicine; at the next, our wonderment is transferred to new forms and properties of electricity, to which physicists are strangers. Osteopathy, chiropraxis, homeopathy, acupuncture, herbalism, hypnosis, meditation,—the list is endless, and extremely varied. What can they possibly have in common? I suggest, a failure to appreciate on the one hand, the limitations of the scientific approach to medicine, and on the other hand its potential.

Of course, those who practise orthodox medicine may be inclined to over-value the scientific contribution to practical medicine. They have some excuse for doing so, stemming from their own experience over a lengthy period of practice. When I was a young doctor, the mortality of lobar pneumonia in young people was around 20 per cent, in spite of excellent nursing care; and thousands of patients were being treated, or rather segregated, in sanatoria—that experience gives me pause when I read, in the columns of *The Times* no less, that 'One of modern medicine's most notorious failures has been with respiratory disorders' (19). These same authors, at the end of three articles criticizing doctors for their neglect of alternative medicine, note the setting up by the Board of Science of the British Medical Association of a group to investigate alternative therapies

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—but, in contrast to most comment, they are somewhat damping towards this expression of interest: 'Any investigation of medical alternatives from such a source is unlikely to inspire confidence or trust'.

The Times, in a leading article (10 August), criticized 'the medical establishment' for disregard of the personal factor in medicine; overemphasis on science and on statistics; and an ungenerous attitude to alternative systems of treatment. This was followed by a considerable correspondence, in which a letter (16 August) from Professor Hoffenberg, President of the Royal College of Physicians, was notable for its restrained cogency. Some of the arguments in the leading article are difficult to follow; and some of the qualities imputed to doctors sound like a recipe for incompetent and unsuccessful practice. As an example of strained argument, the alleged unfitness of doctors to decide on access to health care is attributed to their lack of training in resource management (which is only one of several relevant factors, and not the most important); and to 'a growing loss of faith by the public in a purely scientific approach to medicine' (which, if true, might conceivably affect the demand for care by the public, but scarcely the decisions on access to care taken by doctors and administrators). With regard to the qualities ascribed to doctors, disregard for the personal factor by any doctor would quickly deprive him of patients; and if doctors are 'dazzled by the objective, computerized approach to healing', they are quite masterly in concealing it. The article also criticizes the dominance of statistics: 'This process leads to human beings becoming quantified as groups of units, and away from the reality of the patient as a unique individual'. This constellation of ascribed attributes seems to me to confuse the clinical and the epidemiological role of the doctor, both of which are important, but do require to be distinguished from one another.

There is of course little point in comparing, as *The Times* by implication does, a warm cultured healer with time at his disposal with an overworked harassed practitioner who may have no time for proper assessment and explanation, and who must in default accede to his patient's request for

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'tablets'. It would be equally pointless to compare a caring physician or surgeon, well-versed in the psychosomatic aspects of medicine—and there are many such—with the worst type of charlatan, trading for large gain on the credulity of those who resort to him. When we are comparing systems, we must not feed our prejudices by comparing the best in the system we favour, with the worst in that we discount. Rather, we must seek (*pace The Times*) for a measure of objectivity.

Of course, the orthodox system can boast a measure of coherence, and also a willingness to submit treatments, both old and new, to the discipline of a controlled trial. But the advocates of alternative medicine are not impressed by scientific coherence; and the controlled trial, even in my mind, lacks the universality of application claimed by its more perfervid advocates (20). Where an effect is small, and particularly where there are many relevant variables, trials can give conflicting results. On the other hand, a really important effect, as with insulin or cyanocobalamin, would make a formal trial unnecessary and even unethical. But it is this latter status that would almost certainly be claimed by advocates of at least some alternative systems of treatment. Also, in the more universal systems such as chiropraxis, the gamut of variables among the patients treated would make it hard to obtain comparable groups. Within orthodox medicine, treatments such as gastric freezing for peptic ulcer have in fact been abandoned, when trials have demonstrated their inefficacy with a high degree of probability. I hope I am not being excessively sceptical if I express doubts whether a similarly negative trial would lead to the cessation of osteopathy. To the anti-scientist, scientific evidence is anathema.

But if, as I believe it is, the theoretical basis, or rather bases, of various forms of alternative treatment is somewhat shaky; and if their advocates, not too unreasonably, were to find orthodox methods of assessment unconvincing, particularly if negative: yet a troublesome question remains, of some interest to patients. It is this, 'Do these treatments help?'. And in all honesty, the answer has to be, 'Yes'. Does

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the disturbing admission that many patients find relief from alternative therapies compel our assent to the theoretical basis of such treatments? Unquestionably, No. And why not? I think there are two main factors—the effect of suggestion both on the perception, and even on the course, of a disease; and the natural history of episodes of illness.

The placebo effect of a drug, even in organic illness, is well-recognized, and is indeed the basis for controls in any trial. That the doctor, or any other healer, also affords a placebo effect is recognized in the principle of the 'double-blind', which requires that the doctor should not be aware whether he is giving an active or an inert tablet. The combination, in some forms of alternative medicine, of a sympathetic healer, with ample time and impressive technique or apparatus, must be therapeutically powerful; frequent relief and occasional cure can be expected.

To understand why relief occurs with sufficient frequency to allow a flourishing industry in alternative medicine, we can look at the natural history of disease. The majority of episodes of illness are self-limiting, and these account for 'cures', both with orthodox and with alternative medicine; a small minority of illnesses are slowly or rapidly fatal, and a larger minority go on to states of chronic pain or ill-health. It is in this group that alternative medicine finds most of its successes, partly because of their frequency and duration, and partly because of the variability of symptoms. But in addition to these categories, there are episodes of illness, perhaps amounting to ten per cent or more of the total, in which the outcome can be critically and demonstrably influenced by treatment on orthodox lines. Orthodox, of course, is not the same as 'infallible'—there can be bad, or at least inappropriate, treatment as well as good. But it is in this area that diagnosis, followed by correct treatment, is all-important—and this is the danger area for alternative medicine.

I find it most paradoxical that at a time when soundly based scientific knowledge of disease and of therapeutics is steadily expanding, there should also be an expansion, apparent or real, of systems of treatment whose basis is

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unscientific, sometimes brazenly so. But recognition of this paradox, possibly even tinged with regret, does not absolve us from studying what may be good in one or other of the alternative therapies, and applying it, when validated, to our own patients. Even if the lesson is no more than time and patience in speaking with patients, as I suspect it may well be, it is still worth learning.

Suspicion of that part of medical practice which is science-based is not of course limited to advocates of alternative medicine. In his second Reith Lecture (21), Ian Kennedy says that 'we, all of us, have hitched our wagon to the wrong star, scientific medicine'. He sees an excess of the scientific approach both in medical education and in medical practice; and regards the curative bias of medicine as a distraction from what he sees as the more important area of promoting and maintaining health—a theme to which I shall return, when considering the false antithesis of prevention and cure. Again, John Bradshaw (22) sees science, and particularly its reflection in 'high technology', as something which erodes compassion in the practice of medicine—another false antithesis to which I shall return.

SCIENCE OR COMPASSION

I have just cited two examples of a view which criticizes modern medicine for excessive reliance on science and its associated technology, to the detriment of those aspects of medicine which are variously described as 'holistic', 'human', 'empathic', or simply 'caring'. The indictment is based on a number of strands—that the pursuit of science produces cold detached people; that preoccupation with technology destroys compassion and distracts attention from the needs of the patient; that an awareness of biological organization into systems and cells precludes awareness of the whole person; that preoccupation with restoring particular functions, relieving symptoms, and concentrating on the details of specific diseases may bring about neglect of basic underlying problems of personality, environment, or life-style; that the concentration of investigations in hospi-

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tals overemphasizes their importance in relation to the total burden of illness; and that medicine based on 'scientific problem-solving' runs away with resources which could be better spent.

To indicate the depth of feeling which informs this view, let me quote the first paragraph of the second of Ian Kennedy's Reith Lectures (21, p. 26), entitled *The New Magicians*:

My view can be stated briefly. Modern medicine has taken the wrong path. An inappropriate form of medicine has been created, in large part by doctors and medical scientists, and eagerly accepted by a willing populace. I will go further. The nature of modern medicine makes it positively deleterious to the health and well-being of the population. We have all been willing participants in allowing the creation of a myth, because it seems to serve our interests to believe that illness can be vanquished and death postponed until further notice, while it serves the interests of doctors to see themselves and be seen as, if not miracle workers (and of course they would be the first to deny this), then at least possible miracle workers.

Although I have felt it right to bring together these various strands, some of them may be better considered later, and I would like in this section to consider only the first two, relating to attitudes—that science and its associated technology lead to a detached and unsympathetic practice of medicine. Perhaps I should state the proposition, not in my own possibly biased paraphrase, but in the verdict which John Bradshaw enunciates through his hypothetical Judge (Bradshaw, p. 311):

It must be added also that the present very scientific medical approach to our diseases militates against the traditional compassion doctors have shown to their patients; and that it results in much damage in the shape of adverse effects of treatment, and also in the destruction of the individual's right to manage his own life and of a viable social group to manage its life and the lives of the individuals within it. Lastly and relatedly, their attitude to sickness, pain, suffering, and death fostered by doctors and the medicine they practise is an unhealthy one.

Those who believe, as I do, that there is no genuine and necessary antithesis between that part of medicine which is dependent on science and that which is informed by compassion are obliged to give grounds for denying the two

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propositions which I ascribed to those holding the contrary view. These are, that the pursuit of science produces cold detached people; and that preoccupation with technology destroys compassion.

SCIENTIFIC DETACHMENT

The belief that scientists are cold detached people has the nature of a value judgment, and as such cannot be rigidly proved or disproved—there being, for example, no objective measure of 'coldness' or 'detachment' in terms of which we could categorize a group of scientists. The problem is in no way solved by discovering individual scientists who give an impression of aloofness in personal contacts, any more than it would be by discovering scientists of notably warm personality. My own view is that there are 'cold' and 'warm' people in every walk of life. I cannot rule out the nature of a man's occupation as a possible determinant of his character; but I believe that for most people their character, whether innately or environmentally determined, is well-formed by the time at which they choose their occupation. My view does not, of course, rule out two possibilities—that naturally 'cold' people are attracted to science; and that in some way scientific pursuits may induce a veneer or appearance of 'coldness' even in the naturally 'warm'.

Neither as an undergraduate nor as a University teacher was I aware that students in the scientific Faculties were notably less extroverted or idealistic than those in other Faculties. But it would be wrong of me to rely on my own, possibly biased, testimony; so, may I turn to Ernst Kretschmer (23), who recognizes the frequency of the existing belief by saying, 'We are fond of characterizing science in contradistinction to art as "dry", and imagining to ourselves that scientific systems are produced as by a thousand assiduous bees setting cell upon cell'. But he soon goes on to provide a contrary view:

There is clear psychological evidence that the most significant scientists, who have produced great conceptions, have frequently shown a very lively originality and vibrated with sensibility and inward tension. Behind the cool stream of thought there lies a glowing passionate core

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and sometimes, carefully guarded, an autistic world of desires. There are classical cases in which some great deprivation, some heavy blow from life, has turned a man into scientific work and been the starting point of greater scientific achievements. In these cases, calm detachment has not been the primary possession of that personality, but has been attained by a fanatical struggle, and can only be regarded as the steely end-product of an immense inner struggle.

The notion of there being a specifically scientific temperament—whether it be ‘cold’ or ‘hot’—derives little support from Galton’s classical study of hereditary genius (24). Although the sons of scientific men quite commonly go in for science, Galton does not attribute this to heredity or temperament, but to early exposure to free enquiry, and to a certain tendency to perseveration.

The mass of mankind plods on, with eyes fixed on the footsteps of the generation that went before, too indifferent or too fearful to raise their glances to judge whether the path on which they are travelling is the best, or to learn the conditions by which they are surrounded and affected.

On the other hand, ‘the fathers of the ablest men of science have frequently been unscientific’, and their mothers were often of high intelligence, and open to enquiry, the subject of one of Galton’s blunter sentences:

of two men with equal abilities, the one who had a truth-loving mother would be the more likely to follow a career in science; while the other, if bred up under extremely narrowing circumstances, would become as the gifted children in China, nothing better than a student and professor of some dead literature.

(Perhaps I should explain that these remarks were published in 1869).

If I may turn now to the second of the disturbing possibilities which I raised, that the pursuit of science is inherently productive of ‘coldness’, even in the naturally warm, my own experience certainly again runs counter to it. But I can draw on a more extended and better informed discussion of the matter, by Michael Polanyi (25). I had the privilege of hearing his lecture, delivered to the Manchester Medical Society on 31 January 1956; and it made me question in some depth the stereotype of the unemotional

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scientist, of which I was already in a general way suspicious. I can still recapture from the printed word the conviction with which he spoke; but here I can only summarize the steps in his argument. He begins by giving examples of scientists yielding to emotion in the heat of discovery—Archimedes shouting ‘Eureka’; Kepler confessing to ‘sacred fury’; Louis Pasteur embracing his laboratory assistant in the corridor, after separating the racemic tartrates. Polanyi sees this excitement as based on ‘intellectual passion’, the realization that a discovery has got a particular kind of value, whose appreciation depends on a sense of intellectual beauty. Like other aesthetic appreciations, the assessment may be true or false. Kepler’s delight in the validity of his laws of planetary motion was well-founded; the scientific relevance, but not of course the painstaking accuracy, of the determination of atomic weights by T.W. Richards, Nobel prizewinner in 1914, was devalued by the discovery of isotopes. The non-identity of accuracy and relevance is an important point, to be made against those who equate science with mensuration, pleading that nothing is scientific which cannot be measured. On the contrary, much that is measurable, like the length of a piece of string, is scientifically devoid of relevance. Of course, scientists try hard to guess correctly; but, being human, they often fail. It is still worth distinguishing between *scientific guesses* which have turned out to be *mistaken*; and *unscientific guesses* which are not only false, but *incompetent*. It is from such errors, of both kinds, that controversies arise between scientists. Polanyi gives examples, and points out that they were often ‘conducted in passionate accents’. In his conclusion, he ascribes to intellectual passion a *selective* function, distinguishing what is scientifically relevant from what is not; a *heuristic* function, guiding us to construct new models of enquiry; and lastly, a *persuasive* function (not devoid of risk) which incites us to persuade others of the validity of our concepts.

I find enough of strength in these various arguments to make me believe that ‘scientific detachment’ is a myth of popular belief, not recognizable by scientists themselves nor

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to those who know them well. What of the similar, but not identical belief, that 'preoccupation with technology destroys compassion'.

TECHNOLOGY AND COMPASSION

I am fortunate in that the general aspects of medical technology have formed the subject of Brian Jennett's *Rock Carling* monograph (26); and that the problems of the more expensive medical techniques have been discussed by a Working Party of the Council for Science and Society (27). So I can address myself to the particular allegation just set out. There are, however, two directly relevant points to be made about medical technology. First, although closely related to medical science, whose advances indeed it has greatly helped, medical technology is available to those whose scientific understanding may be limited to that required to operate the technical apparatus, whether that be physical, chemical, or biological. Secondly, medical technology is pervasive of the modern practice of medicine, which benefits greatly in precision from the availability of the appropriate apparatus. It is sometimes argued that we could get rid either of expensive items of technology, or of technology in general, while retaining those advantages of modern medicine, such as the effective treatment of most infections, which are acknowledged even by its opponents. Such a view is a profound fallacy. The hallmark of scientifically-based treatment is its specificity, from which it follows that it must only be given, except possibly on a trial basis, after an accurate diagnosis has been made. Of course, an accurate diagnosis can at times be made on clinical grounds alone; but it may just as well depend on investigation in the laboratory or X-ray department. In the particular case of infections, bacteriological study, including sensitivity tests, is quite crucial to proper treatment; so that technology, albeit of a fairly simple kind, is inescapable.

Unless a simple wish to know what one is doing is regarded as in itself corrupting, and destructive of compassion, it is only the more extreme anti-scientists who would object to the pervasive technology whose application I have

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tried to illustrate. They would more probably focus their criticism on those situations in which a patient is enmeshed in monitoring apparatus, and dependent for a longer or shorter period on life-support systems. They will be unimpressed by the argument (true though it is) that only a small minority of patients are in such a situation, and that normally for only a limited period, until the hope of recovery is either realized or lost. They will argue that the very existence of such a situation is symptomatic of a sick society, corrupted by science, and draining its members, and particularly members of the health professions, of their natural human sympathies.

It cannot, of course, be denied that misplaced enthusiasm can lead to excessive investigation, some of which may cause both physical and psychological suffering to patients; nor that a similar lack of judgment can lead to the prolongation by artificial means of the shadow of life when its substance has already fled. (The most deplorable effect of the ignorant excursion of the 'Panorama' outfit into the difficult area of brain death was not the decline in the availability of kidneys for transplantation—bad though that was—but the prolongation of misery for the relatives of patients, already dead, who were kept in the appearance of life through media-induced fear of turning off life-support systems. Where does true compassion lie—in prolonging the shadow of life at all costs, or in relieving relatives of part of their misery?). That the opportunities afforded by the application of technology can be misused is sadly true; but it represents bad judgment, not a fault inherent in the technology, nor indeed necessarily a failure of compassion—the kindest of people can make errors of judgment.

Again, to speak from my own experience, the availability of artificial kidneys and of the means of renal transplantation has enlarged the compassion of doctors and nurses concerned with renal disease, not contracted it. I would wholeheartedly endorse the view of John McMichael (28):

Kindness, humanity and consideration should be an ingrained part of the behaviour of every clinician. Those who have to be taught how to be kind have surely lacked something in their very upbringing. The

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greatest unkindness to a patient is indeed medical incompetence. Failure to diagnose, understand and manage properly is costly not only in cash but in life and health of the sick man. The applications of modern safe and acceptable instrumental techniques in diagnosis and understanding have been painted by anti-scientists in emotional terms of lurid and sadistic cruelty. Such distorted exaggerations could equally be used to describe as terrifying and brutal almost any operative procedure which has developed since the days of Lister and Simpson. Even when Laennec introduced the stethoscope, there were some who complained that its use was fatal to the dignity of the physician and brought only discomfort to the patient!

Misunderstandings related to the 'medical model'

The relationships involved in the practice of medicine have proved to be a topic of considerable interest to sociologists for at least the past thirty years. Talcott Parsons (29) developed the concept of 'the sick role' as far back as 1950, looking on illness as a form of 'social deviance', and on the physician as an 'agent of social control'. The elements of conflict latent in this formulation have been extracted and elaborated with diligence and zeal by later generations of sociologists. This activity has led to perceptions of the purpose of medicine, and of the behaviour of its practitioners which call for a degree of patience and tolerance in any actual practitioners who may suddenly encounter them. It has proved convenient to speak of 'the medical model', implying what is perceived by doctors; and 'the social model', implying what is perceived by sociologists; but we must keep in mind that 'the medical model' is not something invented by doctors, but something which is imputed to them, often as a prelude to pretty radical criticism of how they supposedly think and act.

For a statement of these contrasting models, let me turn to the report of 'an advisory panel to the Research Initiatives Board of the Social Science Research Council', published by the Council in May 1977 (30). They give the following list of 'different emphases or polarities':

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individual	population
treatment	prevention
cure	care
medical	social
hospital	community
acute	chronic

They emphasize that they are not seeking to 'establish an artificial dichotomy between the medical and other models'. But others have been more forthright in seeing a dichotomy; and also more open in their criticism of the medical model, and their advocacy of its replacement by the social model.

My own belief is that this formulation is a tissue of false antitheses. Let us extract 'medical' and 'social' from the list as being headings; and amalgamate 'acute' with 'cure', and 'chronic' with 'care'. That leave us with apparent contrasts between individual and population medicine; between treatment and prevention; between that done in hospital and in the community; and between the 'acute' and 'chronic' sectors. I hope to show that the members of each of these pairs are not alternatives between which we can make a radical choice; but rather complementary aspects of what should be a unified system.

The making of lists, while it may be a useful device for the didactic teacher, carries with it the risk of obscuring the interdependence of the modalities which figure therein. I have already conglomerated acute with cure, and chronic with care. Acute medicine has also a certain bias towards individuals, and chronic medicine towards client groups; and 'acute medicine' usually implies treatment, rather than prevention. I hope, however, it may make for clarity of discussion if I accept for the moment a measure of artificial dichotomy; and consider first the proper clientele for medical activity; then the appropriate tempo of medical activity; then the ambience in which it is carried out; and finally, in this section, what should be its object, to treat or to prevent.

INDIVIDUAL MEDICINE AND
POPULATION MEDICINE

The traditional image of the doctor is of someone who responds to appeals for help from those who are ill, or from their relatives. This outlook, in which the majority of doctors acquiesce, produces a demand-led service; and a demand-led service must of necessity be biased towards individual medicine, since only an individual can articulate a demand. Nevertheless, and to the great benefit of the public health, individual doctors and sanitarians have pointed out that a demand-led service is incomplete, possibly for two main reasons. First of all, there is much need which never gains expression in demand, or at least not in demand on the health services; and, to a much less extent, there is a moiety of demand which is hard to equate with objective need. Secondly, the population has needs for such things as clean water and wholesome food, which bear critically upon health status, but which do not automatically engender individual recourse to the health services; it is here that demand has to come from the enlightened advocate, rather than from the individual sufferer.

Criticism of the conventional medical services, which respond primarily to perceived individual health needs, comes from two main sources—those who consider the present degree of emphasis on physical and social environmental factors to be inadequate, for the reasons just outlined; and those who believe that preoccupation with individual medicine leads to the neglect of the particular needs of disadvantaged groups, such as the elderly and the physically and mentally handicapped. This second concern has led to the evolution of the concept of 'client-oriented medicine'—an expression in which 'client' seems to me, with my particular bias, to be a term of art which subtly depersonalizes the individual member of the group, on which major attention is now focused. It is at least arguable that the segregation of groups which have in common only a particular category of disability, or at any rate segregation

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beyond what is clearly necessary for their own safety or that of others, may not be in the best interests of individuals, and possibly not even of the group as a whole. It is not unknown for patients who have been institutionalized for years to respond eagerly when they have regained their freedom, with its attendant challenges.

It seems to me that to make a radical contrast between the needs of individuals and those of populations, for either of these reasons, may be both theoretically and practically unwise. On the first apparent antithesis, between 'individual medicine', and 'public health and hygiene', it is surely apparent that good hygiene and nutrition benefit both individuals and the whole population, and must be regarded as a necessary substrate of civilized life, not something which should be competing for resources with the care of the sick. I have considered more fully elsewhere (31) the possible risks to medical practice and to medical education of over-emphasizing the client group approach to medical work. In terms of practice, these include the risks, already mentioned, of depersonalization and institutionalization for the patient; and for the doctor, the risk of overspecialization by what may be an artificial categorization. In medical education, there is already a great deal of unavoidable over-simplification, for didactic purposes; and I see little virtue in an approach which encourages students to see people in groups, rather than as individuals. I am not, of course, arguing against students being made aware of the particular needs of particular groups; what I am arguing against is that the whole emphasis of medical education should be on client-groups, and not on the individual patient.

I believe it to be the norm that the interests of the individual and of the population of which he forms a part are congruent, and to that extent an antithesis between individual and population medicine is false; but in the minority of instances where the reasonable interest of the individual may conflict with that of the population, I would myself give priority to the individual.

ACUTE OR CHRONIC SECTORS

There is a quite natural tendency for the media, and also those whom they purport to serve, to concentrate their attention on the quite remarkable achievements of modern medicine, whether these be achieved by pharmaceutical innovation, or by the ever more venturesome surgery which is made possible, among other things, by advances in our understanding of physiological and metabolic support systems, and the steady progress in the scope and safety of anaesthesia. Not only is it possible for operations, such as cardiac transplantation, to be performed which would have been impossible a few decades ago; it is also possible for patients to survive them.

It is, however, also in the nature of things that an appetite for spectacular breakthroughs is easier to arouse than it is to satisfy on a long-term basis; and the appetite so thwarted engenders a seed-bed for disenchantment. Add to this the increasing tendency, forced on us by economic scarcities to look critically at expenditures whose very mass makes them easily identifiable, in contrast to the many trickles of expenditure in the more hidden areas of the health service which may collectively constitute an equal or greater flood. And on the more positive side of things, ever since Crossman paid his fateful visits to mental hospitals, thinking about the health service has been informed by justified compassion for the deprived who have no effective voice in their own interest; and this is expressed in lay and political pressure to channel more resources into what are called the Cinderella services. Another consideration, perhaps less commonly expressed, is that the acute services are largely dominated by doctors and highly-trained nurses; whereas the chronic sector is much more a matter for less specialized nursing care, and the provision of hotel services by ancillary staff.

I first became pointedly aware of the strength of feeling on these matters, in the course of our discussions in the Working Party on Inequalities in Health, related to social class. As a step towards partial correction of such inequali-

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ties, we advocated an increased expenditure of £200m on preventive and community services. The provision of such services was common ground between the sociologists (Townsend and Smith) and the doctors (Black and Morris). But the ground divided somewhat sharply when—straying somewhat beyond our remit—we fell to considering where the money would come from. The sociologists saw no particular problem—it should be quite easy to find this sum from the acute services. The doctors thought that the acute services were already sufficiently under strain, and could not be further depleted without harming patients. Naturally, all four of us would have preferred 'new money' to be made available; but in the end we had to compromise on a table which set out the two modes of financing (transfer from acute services, or new money). It was quite a hard argument, and I recall at one stage having to make the point that depletion of the acute services would not be actually beneficial.

It is, of course, true that growth in the acute hospital services has been more rapid, since the inception of the health service, than growth in the provisions for chronic disability; but I do not see this as bad planning, but rather as a response, and an effective response, to perceived need. To particularize, effective acute treatment can prevent much chronic disability—and it was a major achievement of the health service to make such treatment more widely available throughout the country than ever it had been before. Again, some chronic disability, such as that arising from arthritic hip-joints or inguinal hernia, can be relieved by acute surgical intervention, but only if the resources for such intervention are available, and that within a finite time. Then, there is the particular case of patients with terminal renal failure, whose very survival depends on the resources of acute hospital medicine, in transplantation, in hospital dialysis, or in the back-up to home dialysis.

As in so many other health service contexts, the artificial gap between 'acute' and 'chronic' sectors is bridged in real life by the patient, who is likely to experience both at different times. But the providers also have a responsibility

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to make his or her transition easier—by good communication between different agencies, and by those who work in the acute sector undertaking greater responsibility for the rehabilitation and follow-up of their patients, instead of—as may happen—casting them adrift into special units and chronic institutions. I regard it as a hopeful sign that those with a particular interest in rehabilitation are on the one hand loosening their traditional links with rheumatology, and on the other hand establishing closer contacts with specialists such as neurologists and cardiologists, whose patients are liable to chronic disabling conditions.

HOSPITAL OR COMMUNITY

One of the first monographs published by the Nuffield Provincial Hospitals Trust dealt with the provision of medical care within hospital and out in the community (32). Since then, it seems to have become part of the conventional wisdom that there should be a shift of resources, both in the health service and in the personal social services, 'towards community care'—which, in a period of financial constraint, is likely to mean away from the hospital. Nor is this a mere theoretical obeisance towards an abstract principle; health authorities are actually planning shifts of resources of this kind. For example, in *The Times* newspaper of 10 November 1983, the Oxford Regional Health Authority is reported as planning, over a ten-year period, a shift of £16m from its 'acute hospital services', in order 'to provide extra funds for elderly, mentally ill, and mentally handicapped patients in community rather than hospital care'. I have already, in the section immediately preceding this, on the acute and chronic sectors, revealed myself as a dissenter from the notion that transfer of funds from hospital to community is a good recipe for a health service which has fallen on hard times. Let me now try to explain why.

The reasons given for this shift in resources include compassion for the most disadvantaged members of the community; a belief that it will somehow be cheaper to look

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after people in the community than it is in hospital; and, as already hinted, a predilection for the interests of less trained staff as compared with specialized professionals. (The third of these points, although I personally believe it to have some substance, is perhaps tendentious, and certainly hard to prove, as it concerns attitudes which not everyone might wish to reveal—I shall not discuss it further). I would, however, wish to express some reservations about the first two reasons. It might be thought that a desire to do good, and a desire to save money, though perhaps not entirely consistent with one another, are certainly not things which anyone with a sane mind and a warm heart would wish to criticize. However, in this hard world we clearly cannot do everything, and we therefore have to face the tough question—'What, within the limits of our resources, is most worth doing?'

There is, of course, no doubt that on any scale of need the elderly and the mentally deranged or inadequate would come very high; but need cannot be the sole criterion for determining resource allocation. At least as important, to my mind, is the likelihood of any actual benefit accruing from the use of resources. Given a decent standard of accommodation and tending—and surely nothing less is tolerable—what is actually likely to be achieved by deploying additional resources for the mentally handicapped, once they have attained maturity? And would not resources channelled differentially towards the elderly not perhaps give a higher dividend if directed towards remediable physical handicap in childhood? I realize that it may seem and even be, unworthy to temper compassion with pragmatism; but it may be inescapable. On the specific matter of transfer of patients from hospital to community, there may well be good social arguments for doing this in chronic, and especially in mental illness, in order to avoid the dangers of institutionalization; but it is not always apparent that resources apparently released in this way are actually deployed in strengthening community care.

This brings me to a consideration of the economic argument, that care in the community may be cheaper than

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care in hospital. The costs of hospital care of course are high, particularly in the acute sector, but also even in long-stay hospitals; but they have to be considered in relation to the benefits. The equation is perhaps different for the acute and for the chronic hospitals. On the *acute hospitals*, I doubt whether sufficient credit has been given for the increased efficiency which has arisen from earlier discharge; greater use of outpatient surgery; and the use of five-day wards for investigation. On the credit side, these things lead to a lower cost per outcome gained; and of course a proportion of successful interventions pay off handsomely in the prevention of chronic disability. But even if the single episode of illness is treated at lower cost, there is also the consequence that more episodes of illness are treated in the course of a year. And of course, increased efficiency in the treatment of illness does nothing to lower the high maintenance and hotel costs of a hospital. So we may well have a situation in which the visible cost of a hospital is steadily rising, even though it is actually functioning more efficiently, and so producing benefits in the relief of suffering and the prevention of chronic disability which are not easy to translate into cash terms. The economic situation is somewhat different with the *chronic hospital*, to use a loose term. Although the staff must ever be on the outlook for possible curative or palliative interventions, the over-riding problem is one of care. If a patient can be discharged into the community, there is an overt and quantifiable saving, real if the bed can be 'lost', apparent if he is replaced from a waiting-list. But it is far from easy to quantitate the cost to the community, borne as it is likely to be very largely by individual families, at an unknown social cost. The cost of accommodation in private or provided residential homes can of course be quantitated, and is naturally less than that of hospital accommodation; but, like care in the family, it is not applicable when there is either very high nursing dependency, or grossly disturbed behaviour (the Elephant and Castle, provided one goes underground, is quite a good vantage-point for observing the social cost of a high discharge rate from mental hospitals).

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At the risk of paradox, I believe the antithesis between hospital and community to be false, because it is both too simple and too complex.

On the hospital side, it is too simple because it confounds the vastly different functions of the acute hospital, the convalescent hospital, the community hospital, and the various types of long-stay hospital. On the community side, it also confounds widely differing modalities of health care. The inadequacy of the two-compartment model of hospital and community has been well brought out by Muir Gray (33) in his paper on 'four box health care'. He starts by making the point, with which I wholeheartedly agree, that since the hospital is in fact part of and serves the community, the hospital and community services are not mutually exclusive; they are interdependent. But he goes on to maintain that between them they provide only the lesser moiety of health care—the remainder being supplied by 'self care' (box 1), and 'the informal care supplied by family, friends and volunteers' (box 2). It is only those conditions which cannot be thus dealt with which come to formal community care (box 3), and to hospital (box 4). This analysis makes even clearer the folly of making the cost of a service the sole ground for favouring it in resource terms; for it would lead to the *reductio ad absurdum* of transferring all health care to the cheapest box, which would mean that we would not only have no health service at all, but would stop helping one another. No, in the real world priority has to be given to the *appropriateness* of care, the *cost* being considered in context, but not elevated to an absolute criterion of decision. (It is not irrelevant at this point to say that medical education is commonly criticized for its emphasis on diagnosis, rather than on treatment; but surely if we do not know what is the matter, we cannot judge what is going to constitute appropriate care).

Even though the two-compartment model, regarded in this way, is too simple; it is also, looked at another way, too complex. For it ignores the interdependence of hospital and community, mentioned in the previous paragraph; and it also ignores the unification which arises from a common

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purpose, that of helping the patient. The overriding test of the value of a health service lies in what it does for patients; in a good service the actual patient may spend time in hospital, or be cared for in the community, according to his needs, and not according to value judgments on the importance of the two sectors. And his transfer from one sector to another should be as easy and informal as possible, which is the hallmark of an integrated, patient-oriented service.

TREATMENT OR PREVENTION

Practically from its inception, the National Health Service has been stigmatized as an 'ill-health service'; and telling doctors what they should be doing is a game which can be played by any number of players. Among those who have overcome their natural repugnance to telling members of another profession what to do is the barrister Ian Kennedy. I have already commented (p. 24) on his criticism of doctors for their eager espousal of science, an addiction which in some strange way qualifies them for his title 'the new magicians'; but they are also culpable for their neglect of prevention. Once again to put the case in his own words (21, p. 42) he says

If we were to start all over again to design a model for modern medicine, most of us, I am sure, would opt for a design which concerned itself far, far more with the pursuit and preservation of health, of wellbeing. What we have instead is the very opposite: a system of medicine which reacts, which responds, which waits to pick up the broken pieces—a form of medicine, in short, concerned with illness, not health. A moment's thought demonstrates the folly of this.

Reasonably enough from this standpoint, he advocates (p. 27) less emphasis on science in medical education, and (p. 58) substantial reallocation of resources—

We must concentrate much more on primary preventive medicine. If this means, as it inevitably must, that some aspects of modern curative care must be neglected or abandoned, so be it. The benefits to be gained outweigh any loss.

It is this last confident statement which makes me uneasy. It is not, nor I believe could it be, supported by any actual

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costing. So let us consider it qualitatively. The benefits to be sought are presumably partly specific prevention of illnesses, and partly a general pursuit of healthy living, avoiding bad habits such as smoking, and cherishing good habits such as wise feeding and exercise. The merit of such proposals is incontestable, a point to which I shall return; but their acceptability and effectiveness are another matter. Two other glosses on the benefit side may be worth making. Effective prevention would notably improve the health of a population which adopted it, but the gain for each individual member is not striking—this is Geoffrey Rose's 'preventive paradox', which points out that 'a measure which brings large benefits to the community offers little to the participating individual' (34). Secondly, those preventive measures whose effect depends on participative effort by the individual are likely to benefit the more educated members of society rather than people of less education who may stand in greater need; to give a concrete example, in 1980 57 per cent of unskilled male workers smoked cigarettes, but only 21 per cent of professional men.

Turning from the benefits to be expected from increased emphasis on prevention to the losses which might be incurred by cutting back on 'modern curative care', I would like to match Geoffrey Rose's preventive paradox, with a 'therapeutic paradox' which I have developed elsewhere (35), and will not reconsider in detail here. To summarize the paradox itself,

Whereas the potential of medical knowledge for preserving and restoring health has never been greater and is still increasing, the systems for applying it have never been so sharply criticised.

Professor Kennedy could scarcely dissent from the second limb of that paradox; and I doubt whether, in general terms, he would really dissent from the first. But I surmise that this willingness to tolerate some loss of 'modern curative care' is based in part on very expensive *types* of treatment, such as heart transplants (p. 36), and in part on the adverse *outcomes* which mar the general success of modern therapeutics, particularly of course when these are misapplied, but

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also in the ordinary course of considered treatment. These are two different problems, of which the first is certainly the more difficult to defend—but I would still do so, on the basis that procedures which to begin with are expensive and hazardous can still contribute to the growth of knowledge, which in turn increases the likelihood of future individual benefit, particularly on account of parallel advances in other fields such as anaesthesia and after-care. On the second point, I fully concede that only a very foolish person would unconditionally guarantee in each individual case a good outcome from a situation in which an agent of disease, the constitution of a patient, and a therapeutic agent or agents are all interacting. If there were some means of knowing in advance that a form of therapy would be harmful, it would of course not be given; but the degree of prevision which even the best doctors have at their command is limited. In relation to Kennedy's 'benefit-loss' comparison, the point which I am making on the 'loss' side is that he may be underestimating it in two ways—cut-back in therapeutic medicine would prejudice the advance of reasonably established knowledge; and, because of the unpredictability of clinical situations, it would not be possible to discriminate in favour of the baby (good outcomes), and against the bath-water (bad outcomes).

My basic criticism of Kennedy's approach, however, is not that he may be overestimating the benefits of preventive medicine, and underestimating what would be lost by a cut-back on medical care; but that he regards prevention and treatment as alternatives, whereas in real life they are complementary. My support, possibly even bias, for clinical medicine does not blind me to the great value of preventive measures. If I knew what it meant, I am sure that I would support the Alma-Ata aim of 'health for all by the year 2000'.* I certainly support the more concrete initiatives

* After listening with patience to a 2-day debate on this lofty theme, in which speaker after speaker dwelt on the lack of 'relevance' of what I call practical medicine, I was stung into saying, 'If you pursue your objective merely by neglecting clinical medicine, you will end up with valetudinarianism in those who don't need medicine, and bad medicine for those who do.'

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towards prevention taken in recent years by the Health Departments, and by the Royal College of General Practitioners. I might even claim that our own Royal College of Physicians, while predominantly representing clinical medicine, has not been entirely inert in this area, with four reports on smoking, and reports on other forms of air pollution, on fluoridation, on dietary fibre, and on obesity. Like many other forms of beneficent human activity, effective prevention tends to rest on minute particulars, rather than on broad and general aspirations. In his Harveian Oration for 1982 (36), on 'Prospects for Prevention', Sir Richard Doll superbly demonstrated how much remains to be done by way of specific preventive measures, by government in discouraging injurious habits by legislation and taxation; by the health professions in disseminating sound advice on habits, diet, and exercise, and on specific measures of screening and inoculation; and perhaps most of all by individual action to promote our own health. In the same context, I would like to reiterate a point made by John Swales (37), in his comment on the Kennedy lectures that scientific knowledge is just as much the basis of effective prevention as it is of curative medicine—a point which becomes even more true if—controversially perhaps—we enlarge 'science' to include the behavioural sciences, which must surely have much to contribute in the future to making knowledge acceptable and influential to as wide a range of society as possible.

In summary, I see no fundamental antithesis between treatment and prevention. Both must be based on goodwill and on sound scientific knowledge; each can complement the other; and both are necessary, since the likelihood in our human condition of some form of catastrophe is no argument against either trying to prevent or postpone it, or against mitigating its consequences.

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FLEXIBILITY OR RIGIDITY IN TRAINING

Having spent some time and thought in considering apparent antitheses devised by others, perhaps I can introduce this topic by essaying one of my own. Here it is. 'The object of medical training is not to provide careers for doctors, but to provide good medical care for patients'. I believe that many people, if faced with this proposition, would accept it at face value; and indeed, as a statement of priority, I believe it to be well justified. But like other false, or at any rate over-emphasized, antitheses, what it misses out is the element of commonalty between the two contrasted objectives. It is surely in the interest of patients that their doctors should not be so concerned with their own affairs that they cannot approach the problems of their patients with an undivided mind. But more is needed in the doctor undertaking medical care than simply proper attitudes, important though these are—he also needs a level of knowledge and of practical skills appropriate to the tasks which he is undertaking. The dilemma inherent in training is that a specific career requires an assurance that skills have been acquired and appropriate experience gained, which calls for specification of the training programme; but on the other hand, the advance in medical knowledge, and the emergence of new environmental hazards, imply an ability to face the new and unexpected, an ability which may not survive a training which is too stereotyped. The argument for flexibility based on the need to accommodate to future changes perhaps smacks of a theoretical construction; but at

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least in the early stages of training it is reinforced by the pragmatic desirability of keeping more than a single option open.

To look now at the successive stages of medical training, I believe that we are fortunate in this country in that the *undergraduate* period of instruction, by definition, takes place within the university system, and not, as in Eastern Europe, under the aegis of the Ministry of Health. Of course, even in this country the Health Departments have a substantial interest in the quality of medical education, and indeed contribute substantially towards it by providing facilities for gaining medical experience, both in hospital and in the community. But the university setting, predominant in the early years, and substantial even in the clinical years, provides the opportunity for retaining some breadth of interest, which is an important ingredient of flexibility; the extent to which this opportunity is made available, and indeed grasped, varies from school to school, and indeed from student to student, but at least it is there.

In the *pre-registration* period, responsibility remains with the university system, but in the past at least the supervision by the universities has not been rigid, rather it has been flexible even to a fault, if such a thing were possible. The only major change which has taken place in recent years is the introduction of an option to spend part of the year in the community, whereas previously the whole year had to be spent in hospital residence. I am not personally too worried about any theoretical imperfections in the organization of the pre-registration year; for in their despite, it seems to me to be the year in which a doctor really comes of age.

Many years ago now, the Todd commission recommended that the first few years, usually three, after registration should constitute a period of *general professional training*. This recommendation has consistently been supported by the Royal Colleges of Physicians, and more recently by the Select Committee on Social Services, chaired by Mrs Renée Short, and by the Education Committee of the General Medical Council. (Mention of the GMC gives me

the opportunity to commend the extremely important role which they have played over the past three decades in liberating the undergraduate course from the shackles of detailed specification, but continuing to discharge their responsibility for it by promulgating guidelines on matters of principle. Their record augurs well for their discharge of the additional responsibility which has been laid on the Council for the supervision of postgraduate medical training). Either at the beginning of the period of general professional training, or soon after, the young doctor should be deciding whether he wishes to go into general practice, or into a career in hospital, or community medicine; if the former, he will enter vocational training, which is now required before becoming a principal in general practice. A career in community medicine or in occupational medicine can be undertaken under the general guidelines of the Faculties of Community and Occupational Medicine. My own career has been in hospital medicine, and my subsequent remarks in this section will be confined to the hospital specialties. Within the period of general professional training which I am now considering, I would like to express a view on two matters—the responsibility for supervision, and the extent of control which is desirable.

Although the universities, through their postgraduate Deans and Tutors, continue to have an important role in the later stages of medical training, the responsibility, under the General Medical Council, for maintaining standards of training lies with individual Colleges and Faculties during the period of general professional training, and with the higher training committees during the period of specialist training. These authorities are professional rather than academic, and this seems to me to be reasonable in the public interest, which requires in a future consultant a degree of professional competence, even more than academic ability. In specialist training, both the examinations and the inspection of training posts are carried out within the College system—the details naturally varying between specialties. For example, in England at least, the critical examinations are held for medicine and surgery during the

period of general professional training, and constitute 'entry' qualifications, signifying fitness to proceed to higher training; whereas in pathology and radiology, the critical examinations come near the end of training, and are an 'exit' qualification, signifying satisfactory completion of training.

The Short Committee suggested that one of the three years of general professional training should be spent in a discipline of medicine other than that chosen as a future career. This proposal has been studied, and to an extent commended, by the Education Committee of the General Medical Council. It has obvious advantages—broadening the experience of someone who may be about to enter a restricted specialty, and opening up a further option to someone who may have primarily selected a competitive specialty, with only a marginal chance of entering it. There is also the particular advantage for the future hospital specialist of spending some time in family practice, which could make his future recommendations to general practitioners more realistic. These are important advantages, and they offer increased flexibility, sufficient to justify commending such a course of action to a trainee. However, although in general an advocate of flexibility, I would stop well short of compulsory flexibility, recalling what was said of Mussolini's second Roman Empire—'Everything which is not prohibited is compulsory'. In other words, a sort of *wanderjahre* during general professional training is something to be encouraged as a recommendation, not something to be made mandatory by regulation.

After satisfactory completion of general professional training, the hospital trainee moves on to *specialist training*, during which he is likely to hold a senior registrar post in the health service, or its equivalent in a university clinical department. On the medical side, the supervisory body is the Joint Committee on Higher Medical Training, usually for obvious reasons just called the JCHMT. This is mainly a College body, but there are representatives or observers from the Universities, The Medical Research Council, and the Health Departments. There are now over twenty

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recognized specialties within internal medicine, each of which has a Standing Advisory Committee (SAC) to consider training requirements in detail. No doubt by coincidence, this structure to some extent models the tension between flexibility and rigidity. In the interests of flexibility, the parent body adorns its recommendations with modifying terms, such as 'normally'; and it openly declares the need for flexibility in an introductory statement:

It is not intended, nor is it possible, to lay down rigid prescriptions for the training of specialists. Any suggested framework in the following schedules should be interpreted flexibly. The needs of each individual trainee will vary according to his previous experience, personal aptitudes, interests and expected future career and his programme of training requires individual consideration.

In discussion within the JCHMT, I have repeatedly heard emphasis laid on the value of periods spent in research, in service overseas, or in general practice; and the regulations allow credit up to two years spent in such activities.

However, the structure as seen from the top looks somewhat different when looked at from the side, or from the bottom. Individual SACs, with a quite proper concern for their own responsibility to the parent body and through that to the public, are not always innocent of over-elaboration in their requirements, and over-insistence on their literal fulfilment. But to the university professor, this emphasis on fulfilment of service experience and acquisition of skills looks like discouragement of the research option. More important still, the trainee, surveying the various programmes, may dread any variation from them in however good a cause, and thus become the victim of self-inflicted rigidity, or if not self-inflicted, rigidly imposed by a timorous adviser.

More radical critics of the system question the need for accreditation, and would indeed like to see the supervisory structure of higher training demolished. I confess to having had yearnings myself in the past in that direction; but two practical considerations incline me to see some virtue in this apparent necessity. First of all, like Everest, it is there, and I

do not, in the present public disposition to question medical autonomy, see the regulatory authorities wishing it away—more likely, they would replace it with something still more restrictive. Secondly, the same pressures seem to me likely to lead to some form of specialist registration—something which I do not myself advocate, and in which I see many difficulties. But if it does come, we shall certainly be in a better position to face it, on the basis of an established system of accreditation, than if the whole exercise were being started from scratch under governmental scrutiny and even direction.

Finally, concentration on what are seen as the negative and restrictive aspects of the JCHMT should not be allowed to prevent recognition of the very substantial improvements which have been made in the training programmes of many hospitals on the recommendation of visiting teams—including the provision of facilities which the authorities might not have been persuaded to finance in any other way.

ACCESS OR PRIVACY

There is a moderate amount of public concern, and a much greater concern among a variety of pressure groups, about the security of information held in a mechanical data-processing system. Some of this is no doubt related to a Luddite suspicion of computers, which are not in fact inclined to make mistakes, but rather to process all too faithfully what is fed into them, which is again all too easily falsified by human error—as expressed in the transatlantic acronym GIGO—garbage in, garbage out. A further point is the much greater accessibility, and consequent insecurity, of the manual systems in which the greater part of health information about individuals is still stored. Access to information held in a computer can readily be restricted, whereas embarrassing pieces of paper turn up in the most unlikely places, such as rubbish-dumps. On the other hand, the sheer mass of information which can be both stored in and retrieved from a mechanical information system, given the appropriate access, does constitute solid ground for

minimizing unauthorized access to these stores. I say 'minimize', rather than 'prevent', not from defeatism, but because there is always some risk, however remote, of either human error or even mechanical failure.

Paradoxically, though only so at a superficial level, the same groups who show concern about preventing access by third parties are also the advocates of very open access to computerized data by the actual subject of such data. It is, of course, entirely reasonable that people should wish to know what information is being held about them in a mechanical system, particularly as by accident or even by design the system may have been fed with false information about them, or—much less probably—has processed it inaccurately. However, the granting of access to the subject of the information must normally mean converting it to a legible form, and this implies transfer from the greater security of mechanical storage to the lesser security of a manual record. This can of course be destroyed after use, but this precaution is likewise open to human error.

Somewhere between the custodian of the information, whose duty it is to keep it as secure as possible, and the subject of the information, there comes the person who actually wants to make some use of the stored information. In the case of personal information relating to health and personal social services, this will most usually be a health professional directly concerned with the individual, who may also have been at an earlier stage a compiler of the data; but he may also be an epidemiologist, an administrator, a relative, or even a policeman investigating serious crime. Some of these uses, which clearly vary in their legitimacy, may lead to publication, which introduces another dimension. Some general principles may be worth stating. Access by third parties must presuppose a real 'need to know'; and except in most unusual circumstances, to which I shall return, the consent of any individual must be freely obtained, particularly if he is to be identified.

I have been more directly interested in these problems through being the convener of an interprofessional working group, representing a number of professions engaged in the

health and personal social services. Information on the health and personal particulars of individuals is peculiarly sensitive, and justifies special treatment. There is a problem for legislators in taking account of the special sensitivity of health information within general provisions for the security of computerized information, as set out in the data protection bill. The general principle of the legislation is that those who hold information in a computer system must register both the type of information which they hold, and the circumstances under which they are prepared to disclose it. The person so registered is described as a 'data user'—in my view misleadingly, as he is really the custodian of a system, and not generally an actual user. I have no particular views on how computerized information should be protected in general; but from my colleagues on the working group, and particularly from Mr Paul Sieghart, I have learned a great deal about the particular problems of health information, using this as a general term to cover the personal information which is common to the health and personal social services. However, what I am now going to express are personal views, relating to access by the subject of the information; by health professionals; by research-workers in epidemiology; and by the police.

SUBJECT ACCESS

The general principle is that people should have reasonable access to information held about them. In many areas, 'reasonable' can be equated with 'total'; but I do not personally accept this in the case of a clinical record. To give total right of access would have two undesirable consequences—patients could discover that there was objective evidence of discreditable or fatal conditions, and be hurt thereby; and doctors would be tempted to diminished candour, if they knew that patients would have free access to their records. A further point is that an opinion on a patient's intelligence and veracity may be helpful on a later occasion to another doctor; but would not necessarily be gratifying to the patient studying his record. (I realize that my approach to this matter is old-fashioned; and my

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colleagues in the group seemed happier that patients should be made free of their records, if necessary being able to obtain access legally to them, subject to certain safeguards).

OTHER PROFESSIONALS

The management of even a single episode of illness commonly involves more than one member of a profession, and also members of other professions. It is in the general interest of the patient that communication between these colleagues should be easy. However, not all professions are bound by strict codes of confidentiality, and the patient certainly does not want his health information to be widely known. It follows that the exchange of information between professionals should be on a rather strict 'need to know' basis. It could be argued that, even so, the patient's consent should be specifically obtained for each transfer of information. In ordinary clinical contexts, I would not take this view, deeming it sufficient that the professionals were acting—as they ought to be—in the patient's interests rather than in their own. To put it more succinctly, I would in the clinical situation assume 'implied consent' rather than insist on 'explicit consent'.

An important point is that in these clinical contexts the true users of the records are the professionals, and not the official 'data users'; the professionals compile the records, and are the natural users of them. Rarely, there could be occasions when a member of one profession does not wish his confidential notes on a patient to be made available to a member of another profession—such dilemmas should be solved by discussion between the professionals, if necessary with arbitration by a colleague, and probably always with the consent of the patient to disclosure.

RESEARCH-WORKERS

Apart from their particular significance to the victim, episodes of illness are also of general interest in the important discipline of epidemiology. It is entirely in the patient's interest that bona fide research-workers should have free access to the records which are necessary for their

work. In many important areas, the lessons so obtained can be made public without in any way identifying the individuals from whose episodes of illness the lesson has been drawn; in such cases, I cannot see the necessity of obtaining express consent from each of what may be hundreds or even thousands of patients; the professional holders of the records should suffice. However, in clinical reporting which could lead to the identification of a patient, formal consent on their part is at the very least a matter of courtesy; better still, identifying particulars should be masked so far as possible.

THE POLICE

It is a reasonable supposition that in each instance so far considered the proposed disclosure has been in the patient's interest, and certainly in no way to his detriment. However, disclosure of identifying particulars to the police is a very different matter. At present, the public have general trust that what they tell a doctor will be treated as a confidence; and some doctors take the view that under no circumstances would they make any disclosure to the police of information which they had obtained from a patient. I do not take this view. Terrorism and drug-peddling seem to me to be offences so heinous that I would be anxious to give every assistance to those charged by society with the duty of preventing them. But clearly there must be safeguards. Disclosures might have to be made urgently, to be of any value—only strong evidence that a very serious crime had been, or was to be, committed would justify this. In the non-urgent situation, an order by a judge of adequate seniority, e.g. a high court judge, would be required. It is, of course, still open to a doctor, in conscience, to refuse to obey such an injunction; but he must then take the consequences, and few have in fact done so.

These instances certainly do not exhaust the areas in which a decision either to give or to withhold information can create problems. For example, there are the cases of relatives, of members of health authorities, of employers, and so on. But I think they give some notion of the general

shape of the dilemmas. Any information system must provide both access and privacy; and the degree of emphasis depends on the particular circumstances of the case.

‘ABSOLUTE SAFETY’

This section-heading differs from the others in that the antithesis to be considered is not brought out into the open, but remains internalized. To me, absolute safety is a contradiction in terms, and its pursuit, while at first glance it may seem reasonable, is at best unrealistic, and at worst disadvantageous. When I first planned this section, I thought to take as my example the ambivalence of modern therapeutics, exaggerated though the negative aspects of that often are. I shall still do so, but will also make some comment on the hazards associated with radio-activity, something which has recently engaged my attention, not entirely of my own volition.

The general maxim, *primum non nocere*, applies to the whole management of an episode of illness—investigation, surgical procedures, and medical treatment; but in each of them, the quest for safety, like vaulting ambition, may o’erleap itself, and fall on the other. Failure to proceed to an investigation such as hepatic or renal biopsy may delay or distort a diagnosis which is necessary in the patient’s interests; of course, the hazards of such procedures must be both weighed and explained to the patient, but in the last resort to shrink from them, carried out in competent hands and with competent interpretation, may be bad medicine. With the diffidence becoming a physician speaking of the craft of surgery, I suggest that the good surgeon does not shrink from procedures which he knows to be hazardous, for the sake of what are sometimes grandiloquently called his ‘operative statistics’. The best recipe for a high recovery rate is to operate on patients who are fit and well, and perhaps do not even need the operation; but this is not necessarily the best surgery. But let us leave these dangerous areas, more properly perhaps the province of the clinical

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pathologist and the surgeon, and turn to the dilemmas posed by medical treatment.

MEDICINES

With entire reason, the public and those who represent them require all the benefits to be obtained from drugs, and none of the adverse side-effects. Unfortunately this is simply 'not on'—show me a drug with no side-effects, and I'll show you a drug which is useless except as a placebo. But then the equally reasonable request is made—'Yes, there may be side-effects, but at least let us be told in advance what these are, so that we may make an informed decision whether to accept the treatment—or, if the worst comes to the worst, we may know whether we have a just claim to compensation.' Again, this would be perfectly all right if it were possible to make a comprehensive list of possible side-effects, and the factors which might cause them to become apparent. And if it were further possible to ensure that anyone taking the medicine should first of all read, then remember, and finally observe the precautions which would follow from such a list. And even then, there could arise side-effects of such infrequency that they would only happen after considerable numbers of patients had been exposed to the medicine; and this recognition would await a sufficient number of such infrequent happenings.

Let me illustrate the nature of the problem by a hypothetical case-history, which perhaps illustrates the difficulties in attempting to control an interaction which involves patients of differing constitution, disease agents of varying potency, drugs which may be differently handled by different people, and environmental agents of many different kinds. For patients with severe depression, which carries a risk of suicide, it may at times be legitimate to prescribe drugs which inhibit mono-amine oxidase, even though these are known to make patients liable to side-effects if they take foods containing large amounts of amines. Some of the side-effects can be serious and even fatal, such as hypertensive crises following the consumption of foods rich in tyramine. It is therefore common practice to warn

patients against taking such foods, and even to give them lists of 'foods to avoid'. But no such list can be comprehensive. A patient who is taking monoamine oxidase inhibitors may have been given a list of cheeses, red wine, etc. which he should not take; but he goes along to a delicatessen, and buys roll-mop herring, which is not in the list, but which gives him a nasty attack. Who then is to blame—the prescribing doctor, the dispensing pharmacist, the compiler of the list—or the delicatessen owner, who supplied the food which did the actual damage? It is considerations of this kind which led the Royal College of Physicians, following the Pearson Report, to advocate a 'no fault' system of compensation, such as seems to work in New Zealand, rather than 'strict liability' compensation, which could in all honesty be impossible to apportion. But the more relevant point for my present purpose is to bring out the essential unattainability of absolute safety in the use of drugs, combined with the risks of encouraging a degree of excessive caution (or defensive medicine) which leads to failure to prescribe appropriate medication for fear of hypothetical consequences. A further point, well brought out by David Kerr, is that over-conscientious emphasis on perhaps remote side-effects may lead simply to the patient refusing to take the medicine—as he says,

I hope I shall never be expected to explain to my hypertensives, already reluctant to take their drugs regularly, that there is a remote risk of heart failure, asthma, jaundice, diabetes, impotence, nightmares, motor accidents, gout, depression and writer's block' (38).

The illogicality of the attempt to ensure absolute safety, or conversely of assuming that no risk is 'acceptable', can also be inferred from the argument developed by Card and Mooney (39), and referred to by Kerr (38). Asking the very pertinent question, 'What is the monetary value of a human life?', they point out that, if not the true value, at least the estimated value, could be inferred from what people are willing to pay to save a life. Unfortunately it turns out that such estimates are very variable, from £50 to avert a stillbirth; through £1000 to save a child from

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poisoning by introducing childproof containers; £100,000 to put a rigid top on tractors, thereby preventing the death of the driver when the tractor overturns; to £20m incurred by changing the building regulations following the partial collapse of the high-rise flats at Ronan Point. At the time when they wrote (1977), the more costly recommendations had been implemented, the less costly not. This may not be so much an indictment of the folly of government as an indication of the high regard paid to an element of drama in any catastrophe. Train disasters and air crashes are big news, but not the greater loss of life and limb on the roads.

Perhaps to the element of drama, we should add fear of the unknown. As Richard Doll (36) has put it, 'Harm of a new sort or of an unfamiliar origin is considered worse than harm which is familiar.' And he adds,

Risks that are endured by choice are of less concern than those that are suffered involuntarily, and risks produced by an enterprise that affects people other than those who benefit from it are less acceptable than similar risks that affect only the potential beneficiaries.

ENVIRONMENTAL RISKS

These various considerations perhaps bring me to my second illustrative subject, the concern over discharge of radioactive wastes into the Irish Sea from the nuclear installation at Sellafield, formerly Windscale. In addition to contributing energy to our national stock from the process of nuclear fission, this installation has been charged with the disposal of radio-active materials derived from other nuclear installations, and even from other countries. An organization known as Greenpeace, in its concern for the environment, is committed to the belief that this country should not be deriving energy from nuclear fission, and that the discharge of radio-active waste into the Irish Sea is hazardous. Their views were featured in a film shown on Yorkshire Television, which was followed by a discussion in which representatives of the nuclear fuel industry were given a chance to reply. I should make it plain that in this personal statement I am not denying the sincerity of Greenpeace, or the responsible attitude of Yorkshire Television towards a

possible risk to health; and I am not either anticipating or criticizing the findings of current enquiries. My interest is in the nature of the public anxiety which followed the programme, in contrast to public complacency about what on the face of it seem to be much greater hazards, such as those of road traffic, of getting oil from beneath the North Sea, or even of smoking. I see concern as being partly rational, since it is known that some, possibly even all, levels of radiation may play a part in the induction of various forms of cancer; and partly irrational, since this is not only an invisible hazard, but also a new one, unlike electricity, equally invisible, but with which people have had time to come to terms. The 'case against Windscale', if I may so dramatize it, rests on three propositions.

(1). People in West Cumbria are exposed to high levels of radiation

(2). People in West Cumbria are more liable to cancer than people elsewhere.

(3). The second is a consequence of the first.

These three propositions have at least one thing in common, that they are extremely difficult either to prove or to disprove—this may be just another way of saying that they are essentially probabilistic statements, and not absolute statements, even if they are sometimes stated as such.

On 'high levels of radiation', we are all exposed to radiation, from the sun, from radio-active minerals, and even from the ^{40}K in our own bodies; people working in a nuclear installation, and to a less extent those living near it, are likely to receive more radiation, but we lack present knowledge of how much more would have to be equated with harm.

On the incidence of cancer in West Cumbria, the population is not static, and the mere finding of several cases of say leukaemia does not automatically prove an increased incidence.

Even if it were shown that there were both a raised exposure to radiation and an excess of cancer in West Cumbria, there would still be neither proof nor disproof of causality, even though radiation is known at certain levels to

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predispose to cancer. This is because cancer has many possible environmental causes other than radiation, including exposure to chemical agents.

Left to myself, I would be strongly inclined to bring in a verdict of 'not proven' on all three charges; but this is not very helpful, and we should be moving in due time to actual estimates of probability, even though the nature of the problem seems to me to exclude certainty, but unfortunately not the emotionalism which would normally depend on certainty.

ANIMAL EXPERIMENTS AND ANIMAL WELFARE

The extreme positions in this matter are held by those who say on the one hand that medical research is so absolute in its priority that no consideration of animal suffering should be allowed to stand in its way; and by those who say on the other hand that our moral obligation to animals is such that in the long-term all animal experimentation must cease. As a matter of record, I have never heard the first of these propositions verbally expressed; but I have heard the second of them enunciated in open forum. Enunciated or not, it is obviously a matter on which feelings can run so high that members of our own species have been physically assaulted, and premises broken into, in the supposed interests of other species. Such behaviour suggests either strong emotion, or a deficit of logical argument.

I suspect my bias in this matter is already fairly clear, but let me openly declare it. It is the bias of a practising physician over many years, at once pleased by the increasing opportunities of benefit for his patients, and distressed by occasional meetings with those whose beliefs and actions seem to threaten medical progress. To an extent, the activities of self-styled animal welfare organizations are a manifestation of a more general mistrust of the scientific approach, which I have considered in a previous chapter; but the focusing of this concern on anti-vivisection raises more specific issues. I would now wish to consider two

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questions—the importance of animal experimentation; and the practicality of replacing it by other methods of gaining knowledge.

If I may first set the scene with a true clinical story. I worked in Oxford at the time when the early scanty and rather crude preparations of penicillin were being given to patients for the first time; and I recall the case of a young man with cavernous sinus thrombosis, whose infection was cured, but who died from the blockage by blood clot of this vital channel. Some years later, observation of sweet-clover disease in cattle in Canada led Link to the discovery of more effective anti-coagulants, which had they been available might have saved that life. The story illustrates the possible interaction of information obtained in widely different ways—by observation of a bacterial culture; by the application of refined chemical methods; and by taking advantage of a naturally occurring animal disease, but following up the original observation by experimental animal work. Perhaps an obvious point, but one which is ignored by those who seek to isolate animal experiment, as a special and unique case, from all other means of acquiring knowledge. On the contrary, animal experimentation is only one part of a complex fabric; yet in itself it contains much variety, and it is variety which bears on the moral issue, for there is surely much difference between studies which occasion some pain, and those which consist almost entirely of observation. Without trying to be comprehensive, it may illustrate the variety of medical (and veterinary) work involving animals if we distinguish five types of studies, with their respective objectives.

(1). Physiological experiments, designed to increase our knowledge of how the body works. While there are important differences, there are also great similarities between the working of the human body and that of other related species. Studies of this type are sometimes dismissed as 'academic', whatever that may mean; but in truth they contribute to a body of knowledge which is basic to the whole wide range of medicines and procedures which alter function in such a way as to mitigate the effects of disease.

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(2). The use of animal models of disease, both those naturally occurring (as already illustrated), and those which can be induced. Such models can both enlarge our understanding of disease processes, and also provide a test-bed for trials of possible treatment.

(3). The development of new drugs. When physiological knowledge and more direct study of what is abnormal have sufficiently illuminated a disease process, the time has come to consider what family of drugs may be appropriate to influence the disease process favourably. But there is still much to be done to check the soundness of the theoretical approach, and to discover both the most suitable drug, and the best way to apply it. The early steps of this type of investigation are quite properly made in animals, even though at some later stage the problem has to be transferred to man, using the discipline of clinical pharmacology to determine the pharmacokinetics and pharmacodynamics of the new agent.

(4). Certain therapeutic agents, such as vaccines and hormones, are themselves of biological origin, and their preparation necessarily involves the use of animals. In the future, bacteria with modified genetic constitution may be used to produce biological therapeutic materials, and human-type insulin has already been produced in this way; but there is of course no certainty that all necessary biological materials can be so produced.

(5). The legitimate concern of licensing authorities with the possible toxicity of medicines to be given to man or animals has placed a formidable burden of toxicity testing on the shoulders of pharmaceutical industry. There is a tendency to add on additional tests, as new risks become apparent; but the expense both in resources and in delay is very high. Some simplification of procedures may well be possible; and there is theoretical as well as practical justification for shifting some of the burden of 'proving safety'—as if that could ever be done—on to improved methods of post-marketing surveillance. It may be worth making the point that there is an area of common interest between pharmaceutical industry and the animal welfare

lobby in their common desire to decrease the number of animals required for toxicity-testing.

This variety of modes and uses is important in any consideration of the regulation of animal experiments—something which is generally agreed must be done. It also provides good reason for considering seriously in what areas there may exist a possibility of replacing or lessening our dependence on animal work by developing other ways of obtaining the same information on products. This objective is not, incidentally, the peculiar prerogative of the animal welfare organizations—it is also pursued, in some depth, under the auspices of organizations dedicated to the support of medical research, and not to its abandonment (40).

It should be common ground that the purpose of any research involving animals (and for that matter of any research) should be clearly defined; and that this definition of purpose should then be followed by the most rigorous design of experiment to give a clear answer, but with the minimum of time and resources, including animals, required to do so. Ways of obtaining information without involving higher animal species must be explored. For example, the demonstration that a drug under test is mutagenic may make a company hesitate to proceed to the entire array of tests for carcinogenicity. But of course, while the presence of mutagenicity raises a suspicion of a cancer risk, the absence of mutagenicity in no way rules out such a hazard. So with this approach, we have to balance the saving of testing for cancer of a number of compounds against the risk of discarding a drug of real value which happened to be mutagenic, but not carcinogenic. That is only one example—other techniques such as cell or tissue culture, and the use of modified bacteria, are actively being explored, and this is right. But if the direct question is raised, can we ever do without animal experiments entirely, I would have to answer 'No', unless of course we agree seriously to slow down the pace of medical advance. I have a specific reason for this reluctant pessimism—it lies in the concept of levels of biological organization. This progresses from the organic molecule through subcellular organelles,

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cells, tissues, physiological systems, to intact organisms and even to societies. The ground rules, as it were, are different at each stage; and however cunningly we use the simpler levels, or even computer modelling, we can never be sure that the lessons will apply at the higher levels of organization, in which we are more directly interested.

I do not, therefore, see the extremes stated, perhaps caricatured, at the opening of this section, ever coming fully together. Nevertheless, I do see a measure of common interest, with progress being made towards some replacement of the large batches of animals used in toxicity-testing, by developing existing techniques, or discovering new ones in substitution. But I see 'alternative' as something of a danger word, given that one of the definitions given in the *Oxford English Dictionary* is 'Of two things; such that one or the other may be chosen, the choice of either involving the rejection of the other'.

6

To conclude

In this little book, I have brought together a number of examples which, as I see it, illustrate the concept that things which at first seem antithetical may on further examination be seen to be complementary. Many of my examples, though probably not all of them, stem from the tension between an individualistic and a collectivistic view of man's place in the world. For example, those who stress individualist values will have a leaning towards personal practice of one of the many professions which make up a health service, and beyond that the personal social services; while the collectivist view tends towards a greater preoccupation with such matters as prevention, utilitarianism, and Popper's 'piecemeal social engineering'. I find merit in both of these approaches, taken separately; but greater merit in the attempt to bring them together, in the understanding that we are each of us both individuals and also members of one another. Of course, the recognition that we have a duty to our neighbours simply brings us to the age-old question, 'Who is my neighbour?'. The gospels give one answer in the parable of the good Samaritan, and it is a good answer; but it does not solve all the problems. Is our duty, beyond what we owe to ourselves, to our family, to our friends, to our profession, to our country, or to mankind? I suppose in some degree to all of these—but each of us might weight the priorities differently. The preceding pages have no doubt revealed my own preference for small concrete individual attempts to help those who come to us for help; but I also respect those who interpret their duty more widely, at least so long as their benevolence is not so diffusely spread as to be innocent of any practical consequence. And I believe most firmly that those who profess great ends can be both

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inwardly and visibly corrupted, should they resort to evil means. Few of our actions are so simple and so devoid of consequence that we can be certain that the immediate wrong or even inconsiderate action will be amply redeemed by our noble purpose. My affection goes out to those who travel humbly along the road of immediate and direct good actions, rather than to those who are prepared to hurt their fellow-men for the sake of some greater good. It was said, no doubt wrongly, of the Puritans that they prohibited bear-baiting not because it was cruel to the bear, but because it gave pleasure to the spectators. Some of the antics of the extremist members of the anti-vivisection movement make me wonder whether their benevolence to all species quite justifies their maleficent actions to members of one.

The discussions which took place during the three-year gestation of our report on 'Inequalities in Health' convinced me of the essential correctness of the thesis put forward by many, but perhaps most convincingly by McKeown (41), that the major determinants of health and disease lie outside the realm of direct medical competence, so long as 'medicine' is narrowly conceived. But my whole thesis is that medicine must not be narrowly conceived, and that we must on the one hand gladly accept the gifts brought to us by the progress of medical science, and apply them for the benefit of patients; and on the other, as Virchow recognized, we must be the natural advocates of the disadvantaged. In other words, we need a synthesis of care for the individual and concern for the people.

Such a synthesis must come, if the future of our calling is to be assured and if it is to reach its full potential. But in approaching this synthesis, I am a gradualist. I do not advocate in medical training the complete recasting of the curriculum towards the community and social aspects of medicine. The simplistic solution of just giving more time to the behavioural sciences and social medicine seems to me less important than changing the outlook of clinical teachers towards greater emphasis on social factors. Students can be surprisingly shrewd in assessing the value of the wares set

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before them; and they may pay more heed to a preventive message delivered by someone whom they see as a 'practical doctor', than to the same message delivered by someone with an overt professional commitment to social medicine. The essence of what is needed in medical education for what we have just discussed is to discover, as Donald Acheson (42) puts it,

how the perspective of medicine which takes into account the whole population's health needs and which examines critically the shortfall in present practice may become part of every doctor's training and practice.

There are also implications for the practice of medicine and the professions related to it. An important minority of doctors and other health professionals devote themselves entirely to public health or to occupational health. But these aspects of health are also important to those in individual clinical practice, whether as specialists or as generalists. And clinical practice provides what are in some ways ideal opportunities for carrying out preventive measures, both tangible ones such as inoculations and screening for hypertension and other treatable conditions; and also for giving advice on health matters—less tangible, but possibly just as important. As I have emphasized earlier, prevention has to be seen as important in its own right, and not as an alternative to clinical medicine. The health departments rightly place great emphasis on prevention; but in the long-term an even more important stimulus to encouraging the preventive outlook may have come from the Royal College of General Practitioners. Firmly grounded as they are in clinical practice, their advocacy of the preventive approach seems to me particularly promising for the future.

Fears are sometimes expressed about the future of medicine, and particularly of science-based medicine, which is sometimes contrasted with what is called holistic medicine, but could better be described just as good medicine. I do not share these fears; and I find some grounds for this confidence in a broad consideration of the natural history of disease, which will of course remain a part of the terrestrial human condition, whatever preventive and

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therapeutic successes the future may bring. If one were able to take a hundred, or perhaps a thousand, episodes of acute disease, well over half of these would recover spontaneously, and irrespective of treatment; this happy provision of nature has been of great benefit both to orthodox and to alternative medicine, contributing greatly to the esteem in which both have been held. At the other extreme, there are of course a substantial number of episodes of illness which inevitably, and again irrespective of treatment, lead on to chronic disability, or even to death. But in between, there are episodes of illness, which at a rough estimate might make up 10 per cent of the whole, where the outcome is critically influenced, in either direction, by what is done, how well it is done, and whether it is done. Such episodes must be first discriminated from those which are self-limiting, or at the other extreme have a predetermined poor outcome—hence the cardinal importance of diagnosis. And when it has been established that something can, indeed must, be done, the very precision and power of modern therapy itself calls for the most accurate definition of the clinical situation in that particular patient. This is the absorbing task of clinical medicine, and it is growing in complexity. More important for the patient, it is also growing in scope. Rather boldly perhaps, I have roughly estimated the frequency of episodes in which medicine is critical at 10 per cent; but had that estimate been made when I qualified, close on fifty years ago, it might have been 1 per cent. It is in the steady, at times even dramatic, growth of the area where outcome is determined by what is done, or left undone, that I find my ground for optimism about the future scope of medicine.

Of course, the rather crude analysis which I have just given does not imply that the value of good medicine is limited to those areas where it demonstrably affects the concrete outcome. Self-limiting conditions, however benign in their outcome, can cause great anxiety; and it is an important medical task to identify those situations where reassurance can be given safely, and also confidently on the basis of knowledge and experience. And at the other

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extreme, progressive and fatal diseases call for continued medical and nursing support, a task which brings its own challenges, no less important than those met in other areas of medical and nursing activity. *Guérir quelquefois, soulager souvent, conforter toujours* remains a better definition of the role of the doctor than the adversarial models conjured up by some sociologists.

I hope that the artificial constraint which I have imposed on myself and on the reader by taking the model of the false antithesis has not proved too artificial, cumbersome, and obtrusive. But my underlying aim has been to commend the synthesis of apparent opposites; and in that I can claim to be in the excellent company of William Harvey, who said *Concordia res parvae crescunt, discordia magnae dilabuntur*. Which might, with quite unpardonable freedom, be paraphrased thus—'If we get together, we can do great things; if we quarrel, even our noblest concerns will founder'.

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