

# Seeing sunflowers

CONRAD M. HARRIS



## The sunflower

THE clearest memory I have from my first year as a medical student is of a botany practical. We had the task, one sunny Thursday afternoon, of cutting a very thin slice of a sunflower stem, putting it under the microscope and drawing what we saw. In the textbook there was an illustration of what we should expect to see: as I remember, it showed regular, neatly-packed cells with several smaller structures inside them. After the blood-letting which always accompanied our efforts at razoring a helianthus, we produced passable copies of this illustration, labelled the parts, and sat back satisfied.

The lecturer wandered between the long wooden benches, inspecting our efforts in silence, until he reached the only member of our small group who was still at work — a mature student called Philip. 'Well', he said, 'thank goodness one of you draws what he sees'; and he made us all go and look at what Philip had done. The section on Philip's slide was as poor as those made by the rest of us: the cells were of all shapes and sizes, with their walls torn and their contents half-missing. What was different was that Philip's drawing faithfully reproduced this mess. After we had filed past he went back to making it even more accurate.

Having nothing better to do, the other students chatted about this for five minutes or so and reached complete agreement that Philip was wasting his time drawing something that looked so unlike a cross-section of a helianthus stem. In an examination he might even not be able to demonstrate that he knew what ought to be there; and in any case the exercise was pointless since when we qualified we were not going to have sunflowers as patients.

It was a small event and I did not expect to give it a second thought. To my surprise though, the memory kept coming back to me as though it were important. It was 30 years before I was able to put a meaning to it: the story was an allegory about the way in which we so often practise medicine, seeing only what we expect to see and defending ourselves in the name of common sense. Rarely do we see the sunflower as it is.

## Pickles and research

I imagine that it is usual, on being invited to give an eponymous lecture, to study the works of the person in whose memory one

C.M. Harris, MEd, FRCGP, professor of general practice, University of Leeds. This is an abridged version of the 1989 William Pickles lecture, which was delivered at the Spring meeting of the Royal College of General Practitioners held at Brighton University on 9 April 1989. The full text is available from the author.

© *Journal of the Royal College of General Practitioners*, 1989, 39, 313-319.

is to speak and to read such accounts of the life as have been written. This may be done out of piety or curiosity, but often, I suspect, it is to find a theme or to discover some justification for the theme one already intends to pursue. Certainly when I was honoured by being asked to deliver the twenty first William Pickles lecture I took down my copies of Pickles' *Epidemiology in country practice*<sup>1</sup> and John Pemberton's *Will Pickles of Wensleydale*;<sup>2</sup> and they did lead me, in time, to my theme.

Pickles was long settled in his Aysgarth practice and into his forties when, in 1926, he came across James Mackenzie's book *The principles of diagnosis and treatment in heart affections*,<sup>3</sup> the third edition of which was published that year, soon after Mackenzie's death. Pickles decided to make himself an expert on epidemiology, which at that time was the study of epidemic infections, rather than the statistically-based investigation of the determinants and distribution of disease that it became after the second world war. He set about reading the classical works on the subject: John Snow on cholera; William Budd on typhoid; and Creighton's *History of epidemics in Britain*. From Budd especially Pickles learned how he might take advantage of the well-defined and uncluttered lines of communication found in the countryside to unravel the mysteries of how epidemics spread.

## Catarrhal jaundice

In 1928 a major epidemic of catarrhal jaundice broke out in the dale, and in 1929 Pickles decided to investigate it. As he went about his daily work he doggedly established the 'short and only possible contact' wherever he could, and came to two principal conclusions: that the incubation period was 26–35 days; and that spread was respiratory, by droplet infection during close personal contact.<sup>4</sup> Attention is usually focussed admirably upon the way in which he reached his conclusions, but, since this was the most important research he ever published, we have to look at the conclusions themselves, to see how original and how valid they were.

First, the question of the incubation period. It is true that textbooks of the day gave it as short — under 10 days — but equally clear that opinion was divided. An exhaustive and influential review by Cockayne<sup>5</sup> in 1912 had stated that longer periods had been observed; in another review, in 1923, Blumer<sup>6</sup> had noted that it might be as long as 28 days; and a study by Booth and O'Kell in Surrey in 1928<sup>7</sup> set the limits as 20–40 days. Pickles actually cites the first and third of these references in his own paper, though he had not seen the Surrey findings when he started his own work.

Opinion was also divided about the mode of transmission. The 1929 edition of Price's *Textbook of the practice of medicine*<sup>8</sup> states that epidemics of catarrhal jaundice most often arise from a common cause — such as contaminated food or water — and that passage by person-to-person contact is uncommon. The standard paediatric text, by Hutchinson, was still asserting in 1936:<sup>9</sup> 'There is no reason to believe it is infectious'. On the other hand Cockayne had been emphatic that spread was by air-borne contagion; Blumer noted that there was almost unanimous agreement that personal contact was the normal mode of transmission; and the evidence in the Surrey outbreak was that direct contagion took place from person-to-person.

## An assessment

Clearly Pickles' conclusions were far from radical and I cannot help wondering how carefully he had read the literature on his subject. I think it best therefore to regard him as someone who

made a useful contribution to a debate which was not resolved for many years. The virus was finally isolated in the early 1970s; the incubation period is now believed to be two to six weeks, and transmission to be by the faecal-oral, rather than the respiratory route.

As a researcher then, Pickles is simply not in the same league as James Mackenzie and William Budd and it makes no sense to pretend otherwise. His importance to us is another matter, and one to which I will return.

### Research in general practice

The theme that thinking about Pickles gave me was research in general practice. If I say that I think this an important subject you may feel that, in my academic position, I could hardly do otherwise; but I have come to believe that research is bound up with the very survival of our discipline and is therefore something that concerns all of us. This lecture was drafted before the sweeping changes in the National Health Service intended by the government were published, and I recognize that in the present situation of confusion and anxiety it may seem naive to expect general practitioners suddenly to give a higher priority to research. In spite of this I have not altered what I intended to say. Events which obscure a truth do not invalidate it; and, while our roles in medicine and society remain, I think my arguments still stand.

General practice was defined 20 years ago<sup>10</sup> as a form of primary care in which people have a personal, continuing relationship with a doctor; and I believe that every aspect of this definition is under threat.

The personal nature of the relationship has been undergoing erosion for years. Many of the reasons for this have been laudable, and we are slowly learning to compensate for some of what has been lost by improving our records and by communicating regularly with other members of the primary care team.

We are one of the few countries that offers a continuing relationship in its primary care system, but not everyone here is convinced that its advantages are worth the sacrifice of direct access to a doctor with specialized knowledge, nor have we ever produced any strong evidence for our belief that a continuing relationship is of clinical benefit to individual patients. We can point out the professional and economic benefits of our referral system and the way in which having a registered list makes prevention, screening and audit easier and more effective, but our cherished continuity remains vulnerable to any reforming government.

The final threat is that the continuing relationship in primary care does not have to be with a doctor. For carrying out prevention and screening a medical degree is not needed — others probably do these tasks better than we do; and the growth of protocols for diagnosis and management is bound to diminish our first contact role. We may evolve into doctors of first referral — general physicians of a sort, but not general practitioners.

How do we defend keeping an extensively trained doctor in the situation of first contact, rather than someone trained only in how to deal with specific common situations? All we have to offer is our long university education and the characteristics that this is supposed to foster: an insistence on thinking for ourselves and a relentless but disciplined curiosity. Is there any way of demonstrating that we have these qualities other than by showing how we have used them to add to the sum of medical knowledge?

I am suggesting that the need for research in general practice has a political as well as an academic dimension — that unless we make ourselves credible as members of a learned profession we may be reducing our chances of survival, and I doubt if we have more than 10 years in which to do it.

### The problems

Of the 20 William Pickles lecturers who came before me, only one chose to talk about research. David Morrell, in 1979,<sup>11</sup> discussed developments since the days of Pickles, and, having given examples of fields in which general practice research would be valuable, he looked for reasons why the challenge was not being met. He noted a lack of training in research methods and of close contact with epidemiologists and statisticians; he suspected that the doctors who chose to go into general practice were unlikely also to be interested in research; he considered problems such as those posed by mobile populations and unhelpful partners; and finally he commented that the areas currently most in need of research were much more complicated than those faced by Pickles.

Ten years on, the situation is little different. There may be more research going on now, but a fairly recent calculation (Bain DJG, personal communication) suggests that each of the handful of academic general practitioners is, on average, publishing more papers than 250 service practitioners. I think we need to make some additions to David Morrell's list.

The first is the demands made by practice organization. It takes much time and effort to plan; to undertake screening, preventive care, education and audit properly; and to communicate with every member of the practice team. The keenest doctors, those possibly most likely to be interested in research, have been putting their energies into these activities in order to create practices of which they could be proud. In the future it looks as though practice organization will be even more demanding, with financial planning especially occupying a large part of our time.

The second is the way that clinical work can become routine, leading to the loss of both curiosity and intellectual rigour, and I suspect that this is true in all clinical settings, not just in general practice. It is easy to forget that every patient presents us, in a sense, with a research project in which we must gather data and reach conclusions. A good scientist goes to endless trouble to check for bias in his data and to validate his conclusions, but clinicians do not work in this way.

Sometimes they recognize patterns instantaneously; if not, they look for evidence that will support their diagnoses, rather than evidence that refutes them.<sup>12,13</sup> The approach is defensible of course — in terms of clinical experience, a lack of time and resources and the patient's needs. We may also suspect that it is in the doctor's interests to stop asking questions once he finds a pattern he knows how to deal with, for going on may lead him into uncharted waters. Whatever the reason, we constantly throw away opportunities to learn something new.

Similar issues are raised in clinical management too, since every time we intervene in people's lives we are conducting experiments, though pretty poorly controlled ones. We develop routines — drugs, advice about lifestyle, counselling or whatever else accords with our personal predilections — and come to believe in them, yet much of what we say is based on guesswork and ignorance. There is always a gap between what we know and what we need to know, and if we are not constantly exploring a little of it we are in danger of forgetting that it exists.

### The real failure

The additions I have made so far to David Morrell's list have been concerned with the boundaries between professionalism and practice; but there is one far more fundamental, and of a very different kind. It is, quite simply, that we do not yet know what research in general practice means: we have not agreed what ends we want to achieve, what methods we should use to achieve them, or how we are to create a structure within which the work can develop. When we do, all the other problems will become far easier to resolve.

There are now several journals which regularly carry compe-

tent research papers written from general practice by general practitioners. For the authors this is a useful and important service; for most general practitioners, on the other hand, the papers are deeply uninteresting. Why should this be so? The answer, I believe, is that they are almost always written in the language of epidemiology, which uses a vocabulary that few of us are familiar with and which expresses an approach that has no bearing upon a large part of our work. It is not a language in which all general practitioners can talk to each other, nor one in which we can write our own story.

How can we create our own language? What approach, what methods, what vocabulary for research in general practice will spark a more enthusiastic response in general practitioner readers? Must we start from first principles to find them? Can others help us?

When I first articulated these questions I felt ill-prepared even to begin to answer them. My first step was to read and think about the subject of research itself; my second was to look systematically at the research methods of disciplines related to our own. Slowly the steps grew into a journey, and it is on such a journey that I now want to take you. We shall be flying too high to see much detail; and I must warn you that many of the landmarks over which we shall pass have strange names. On venturing abroad you would not expect it to be otherwise.

### The aims of research

The aim of all research is understanding, but there are two very different schools of thought about how this aim may be attained. One is known as positivism; the other I shall call ethnography, which literally means 'describing people'.

Positivism holds that the only kind of reality amenable to a scientific approach is the one of observable and measurable phenomena, and it attempts to link these phenomena in a causal way in order to discover natural laws. Its characteristic mode of research is the experiment, in which total control is exercised over every variable except the one that is being studied. Where experiment is impossible, the researcher must in no way influence the processes he is observing. All concepts must be defined in operational terms and the operations must be capable of being accurately measured. Positivist research is concerned with objectivity; its theories must always be subject both to empirical confirmation and to attempts to refute them.

The ethnographic tradition does not deny that the criteria of positivism are valid in the fields to which they apply, but it insists that to impose them on cultural, social and psychological phenomena is to distort the nature of human existence. Men and women cannot be studied as though they are inanimate objects: to understand their behaviour it is necessary to accept that their actions may arise from the values they hold and the desires that motivate them.

Two further terms, used rather more often, are 'quantitative' and 'qualitative'. In quantitative research the techniques depend on some kind of measurement and conclusions are reported in terms of whatever measures have been employed; in qualitative research, understanding is seen in terms of meaning and conclusions are expressed as concepts. Quantitative is not synonymous with positivist, nor qualitative with ethnographic. In elementary chemistry we have the thoroughly positivist experiment, for example, of adding hydrochloric acid to sodium hydroxide and observing that it produces sodium chloride and water. In chemical terms this is a qualitative experiment to do with properties of matter. Quantitative methods, on the other hand, are nowadays used in ethnographic research to supplement and enrich its qualitative findings.

The most obvious sources of research methods for general practice are epidemiology, which is entirely quantitative, and sociology, which has both quantitative and qualitative techniques.

### Epidemiology

At the risk of gross over-simplification, I shall present epidemiology as employing three kinds of method: at the most positivist end there is the randomized controlled trial; at the other end, the survey; and, in the middle, the case-comparison study.<sup>14</sup>

The randomized controlled trial is modelled very closely on the experiment of the physical sciences. Because it cannot control all the variables directly, it divides the experimental population into subjects and controls by random allocation; the uncontrolled variables should then be distributed equally between the two groups and may therefore be ignored. Randomized controlled trials are typically used in testing new kinds of treatment or in any study where a planned intervention has to be evaluated.

The survey is a method imported from sociology. The concepts or factors that are to be studied, expressed in operational terms, are put to the chosen population; the results are counted; and the findings are presented statistically. The survey, at varying levels of sophistication, is probably the research method most often used nowadays by general practitioners.

In between these two extremes lies the case-comparison study, which builds upon the survey by adding a group with which the subjects may be compared, and it may repeat its measurements after some event or intervention has taken place. It falls short of being experimental because the composition of its two groups is not decided randomly: its subjects are its starting point and the members of the comparison group are matched to them. The need for this design is inevitable, since one cannot give half one's population a disease or an unnecessary treatment just to create a pure experiment. The best known case-comparison study in general practice is probably the Manchester oral contraceptive study.

In describing these three methods I have not mentioned any of the difficulties they present — to do with measuring instruments, scales, questionnaire design, intervening variables, what to match for and what statistical techniques to employ, for example. Learning how to cope with these problems requires training and experience: general practitioners who have neither need help if they want to do any kind of epidemiological research.

### Sociology

Sociology has a variety of research techniques both quantitative and qualitative.<sup>15</sup> The most quantitative is the survey, which was developed by the structuralists. They believed that people's actions were determined by the structure of society rather than by the beliefs, attitudes and motives of its individual members. A 'strong' structuralist like Talcott Parsons wrote about the doctor-patient relationship entirely in terms of the social roles of doctor and patient, paying no attention to the differing personal qualities of those who played them.<sup>16</sup> 'Weaker' structuralists were a little less rigid in that they took note of what people said before deciding which of their behaviours to study. This gave their surveys a more ethnographic basis and is worth noting because it demonstrates something which we will meet later on: a 'bottom-up' rather than a 'top-down' approach. By this I mean that the point of departure is observation at the grass-roots rather than some abstract idea thought to be crucial by the researcher.

Further down the road from positivism come the symbolic interactionists. Their view was that social reality was neither totally determined by the structure of society nor entirely produced by the actions of individuals expressing themselves freely; it was, rather, the result of a continuous process of negotiation between these two extremes. The perspective is of social life as a stage play, set in any kind of institution, building or workplace, in which a person presents himself and his activities to others, guides and controls the impressions they form of him.

and learns what he may or may not do if he is to sustain his performance in that setting.<sup>17</sup> The method that best allows a researcher to understand the players' actions is participant-observation, and the intent is ethnographic.

Participant-observation may take many forms, depending on the researcher's view of how far the emphasis is to be on participation and how far on observation. He must decide which members of the society he will stay closest to and how he is to achieve the objectivity required for understanding the implications of what he observes. One technique for doing this is formally divided into three stages.<sup>14</sup> In the first the researcher simply observes, with a mind as blank of preconceived ideas as possible; in the second, tentative theories are developed on the basis of the initial observations; and in the third the two strategies of 'theoretical sampling' and 'analytic induction' are employed. The former directs the researcher to where he should look next for his data, and the latter offers a systematic way of searching for evidence that may disconfirm whatever theories are emerging.

It has been argued that the participant-observer will not truly understand what he observes unless he is totally involved, and that, when he is, what he observes is his own participation. This is precisely the situation in the only truly ethnographic research method that general practice has evolved — the Balint group.

At the far end of the ethnographic road is the ethnologist, who not only believes that social reality is created entirely by individuals, but who goes further and says that no one can ever explain or even describe an interaction between two other people. Since any possibility of objectivity is denied, research methods are superfluous.

I have gone to these levels of detail solely in order to emphasize how structured and rigorous a discipline qualitative research in sociology can be. Its language and methods are at least as foreign to general practice as those of epidemiology and statistics.

### Anthropology

Anthropology has always been concerned with understanding cultures and it was the first contributor to the ethnographic tradition. Mainly qualitative in its approach, its most distinctive feature has been the places in which the fieldwork was performed. A very amusing account of such fieldwork can be found in Nigel Barley's book *The innocent anthropologist*,<sup>18</sup> which is the story of the author's adventures in a year spent among the Dowayo in the North Cameroons. He learned the fundamental problem of ethnography: that it is easy to discover facts, but difficult to think of anything intelligent to do with them. Success comes when they can be integrated; otherwise the outcome is referred to as 'butterfly-collecting', and the anthropologist simply records the curious customs he has observed in alphabetical, geographical or evolutionary order, according to the fashion of the day. The absence of formal research methods makes training very brief: the eminent anthropologist Evans-Pritchard used just to say to his proteges 'Get yourself a decent hamper from Fortnum and Mason's, and keep away from the native women'.

Barley did finally hit upon the way in which ceremonies to do with rainfall, circumcision and the first fruits of the field were related, and how they fitted into a set of beliefs about all aspects of fertility. It is easy for us to identify with his plight in an environment that was bewildering and often hostile, but I think that all we can learn from him is the importance of curiosity, imagination, persistence and a sense of humour.

William Foote Whyte, the father of fieldwork methods in industrial and urban settings, says that when he decided to introduce quantitative methods like surveys and sociometric instruments into his work to give it an extra dimension, he brought down upon himself the wrath of both anthropologists and sociologists.<sup>19</sup> It is now quite acceptable to do so, and Helman,

a general practitioner anthropologist, has used survey methods in exploring the beliefs of his patients about fevers, chills, tranquilizers and other sources of myths in modern life.<sup>20</sup>

### Psychology

I have found little distinctive research design in psychology. The psychodynamic end has been developed qualitatively by observation and theorizing; while most other areas have adopted an experimental model, measuring physiological, behavioural or cognitive outcomes and often bringing in a great deal of statistics. Experimental research of a similar kind has been undertaken in general practice into the uses of meditation, hypnosis and biofeedback for treating a variety of conditions without drugs.

One particular psychological method deserves special mention because it was used to produce the Manchester rating scales for vocational trainees and is currently being employed in a study of uncomfortable prescribing in general practice. The 'critical incident'<sup>21</sup> technique was developed for practical rather than academic purposes, as a way of defining what a person must be able to do in order to perform a particular job effectively. The researcher asks a number of people who have first-hand knowledge of the job to tell him anecdotes that illustrate some aspect of good performance; with enough anecdotes, and following certain procedures, he is then able to define, from the bottom up, how best to select and train candidates for the occupation he is looking at. The same technique has been used outside industrial psychology, as in trying to specify the elements of the concept of emotional immaturity.<sup>22</sup>

At this stage of the journey, my own feeling is that though we may have added some useful words to our vocabulary, we certainly have not found a language for ourselves. As for the methods I have mentioned, it is possible to learn them, use them and reach valuable conclusions through them, but none fits naturally into our daily life or covers enough of our concerns. They will not cause general practitioners to flock in their thousands to the banner of research. Bear with me then while we fly over one more territory — that of the science of which medicine is a part: biology.

### Lessons from biology

I was drawn to thinking about biology when I learned how it had to struggle for many years to gain respectability for its own research methods, and in Ernst Mayr's book *The growth of biological thought*<sup>23</sup> I have found many ideas that seem relevant to the situation of research in general practice. They map out the last leg of our journey.

Biology's lack of respectability was for centuries due mainly to the attitudes of the physicists, who believed that no form of research not modelled on theirs could be regarded as scientific; Mayr quotes numerous examples of their intellectual arrogance. Despite the achievements of Darwin, which had no experimental basis, it was only just over a 100 years ago that any vigorous revolt began to be articulated by biologists, and many years later Rutherford was still dismissing biology as 'postage stamp collecting'. Only very recently has it been generally accepted that the laws of physics are inevitably insufficient for understanding the workings of life forms which have developed over billions of years and which are incomparably more complex than almost anything in the inanimate world. Experiments modelled on those of the physical sciences answer questions about how things work; to understand why things are as they are we have to study historical processes.

Mayr therefore draws a crucial distinction between functional biology and evolutionary biology. Functional biology, whether at the molecular, the anatomical or the behavioural level, can proceed by way of observation, quantification and experiment

to discover biological laws. Evolutionary biology, whose central themes are adaptation and transformation, has to seek its concepts and principles in other ways. The phenomena which dominate living nature — species, ecosystems, communication and regulation, for example — are concerned with relationships and can be expressed in most cases only qualitatively. Methods must be comparative rather than experimental, with inferences based on meticulous and intelligently-directed observation.

In functional biology, as in the analogous physical sciences, experiments may be qualitative rather than quantitative. I cannot resist telling you of the work of Spallanzani who, some 200 years ago, set out to discover whether it was the spermatazoa or the seminal fluid as a whole that produced fertilization. He dressed some male frogs in little panties that were permeable to the fluid but not to the sperm, and found that they were then unable to fertilize the eggs of the females in which they were mated. There may be many examples of qualitative experiments that are more apposite or more recent, but I doubt if any is as memorable.

### Essences

Underlying the slowness of biology to develop as a science was something far more fundamental than a lack of respectability: the dead hand of a top-down philosophy that started with Pythagoras and held that causation in nature is regulated by laws that can be expressed in mathematical terms. This led to a search for unchanging realities in nature that were called essences; the philosophy was most clearly enunciated by Plato and later came to be known as essentialism. A triangle, no matter what combination of angles it has, is discontinuously different from every other polygon; for Plato the whole variable world of phenomena had to be constructed from a limited number of such essences, with any variability being attributable to some kind of imperfection. Triangle, tiger or tree as abstract ideas had an existence independent of, and more important than, the tangible forms they might take in real life. Much of the hostility that Darwin met arose because his views about adaptation and evolution were incompatible with the concept of unchanging essences accepted by most of his contemporaries.

We use today the body-mass index invented by Quetelet which relates height and weight in a formula for describing build: Quetelet was an essentialist thinker obsessed with discovering the characteristics of the average man — the essence of man — and, for him, variation was nothing more than errors around the mean. It was another Victorian, Francis Galton, who pointed out that values of this sort were just statistical constructs: what was truly interesting about any species was the nature and extent of the differences between its individual members.

The term used to denote this revolutionary, bottom-up philosophy was 'population-thinking'. Though to modern ears the term suggests a connection with epidemiology, this is misleading because it is concerned with the individuals in the population, not the population as a whole.

In biology, classification is very important, since the relationships that any classification system sets out reflect a view about the way the world is ordered. Essentialist classifications start with an abstract idea and proceed downwards by logical division; those that start from observation are composed upwards, with very different results. It was population-thinking, bottom-up classification and a recognition of the primacy of observation as the source of concepts and theories that liberated biology from 2000 years of sterile dogma. In a way it was the triumph of Aristotle, a great natural historian, over the more metaphysical Plato; and Mayr is quick to acknowledge that many of the successes of biology, including Darwin's work, were possible only because of the painstaking observations of earlier generations

of natural historians.

I suggest that there is a great deal in this that can encourage and guide us, starting with the story of how a now enormously successful science had to struggle to have the research methods it needed accepted as respectable. The cry of the evolutionary biologists that what was right for the study of inanimate objects did not suffice for the study of living organisms is not so very different from the complaint of the ethnographers about positivism. In one case the dimension of change over time was being ignored and, in the other, the dimension of individual beliefs and motives. The purpose of both rebellions was to add 'why' questions to the 'how' questions that experiment addresses; the conclusion in both was that inferences based on observation and comparative methods could be valid science.

### The natural historian in medicine

The observation and description of what is before one's eyes, unconditioned by preconceived ideas — as Philip saw and drew the sunflower — are the starting point of all scientific research, positivist and ethnographic, quantitative and qualitative. Observers and describers of living nature are naturally population-thinkers with a bottom-up approach; when they are not valued it is a sign that their discipline is in the grip of dogma and that, in James Mackenzie's words, 'whole fields will remain unexplored'.

The natural historian has an honourable tradition in medicine, starting with Hippocrates and ending only when intellectual leadership in the profession was taken over by those whose work was based on a quantitative, experimental approach. The tradition can be revived, and we should expect medicine to benefit if it is. As John Ryle put it so elegantly in his book *The natural history of disease*,<sup>24</sup> the science of the clinician is the study not of disease in man, but of man in disease.

There are no clinicians better placed than the doctors of first and continuing contact to observe and describe the common ills, and what is associated with their onset and their changes over time. The role of natural historian is ours for the taking. It is but an extension of our daily work, fitting into its rhythms because it may be picked up and set down at will; it does not ask us to learn a new language or any special techniques; it needs only a pen and paper, a cross-referencing system and a small but significant shift in the way we see our work. I believe that, if this role is seen to be properly valued, it will attract very many general practitioners who now avoid research because they feel intimidated by the paraphernalia that seem to surround it.

### Pathography

There is an old medical word, sadly fallen into disuse, for the descriptive task I have in mind — pathography; and, as pathographers, there is much for general practitioners to do. Let me give you a couple of examples.

We see patients with painful, maybe misshapen, knees whose X-rays show no osteoarthritic change. We know that the radiological evidence of osteoarthritis takes time to appear, but we do not know which painful knees will go on to show it. The natural history of osteoarthritis of the knee has not been written.

We meet patients who wake up at night with an epigastric pain that is relieved by food, milk or antacids. The symptom is common but not universal in people with duodenal ulcers, and it occurs in people who do not have ulcers. The cause is unknown. We do not know if the time of waking up is similar for all sufferers; if it is constant for the individual; if it is related to the normal time of going to bed, to the number of hours slept or to the time or nature of the last meal. We do not know its outcome, or whether it is particularly associated with any habit, state of mind or later complications.

There is no shortage of work for pathographers, for in no condition has a final version of the natural history been written. When Ian Richardson gave the William Pickles lecture in 1981 he took his title<sup>25</sup> from some lines of Sir Thomas Browne that urge a student to join imagination, sense, reason, experiment and speculation in his studies ‘... and so give life unto embryon truths and verities yet in their chaos’. In fact we never do behold chaos for more than a moment because we cannot tolerate it. The mind always starts to impose some kind of order upon it, and the longer this order remains unquestioned, the harder it is to go back to the vision of chaos and seek a new verity. Perhaps this, taken with something we learned from the history of biology, explains why we have not entered into our rich heritage as pathographers.

What we expect to find is powerfully conditioned by what we have learned from our medical teachers and therefore, at one remove, by what they were interested in. This sets the limits of what we ask our patients about and the extent to which we are prepared to ignore anything they tell us that is not required by, or does not fit, a pattern with which we are familiar. This pattern plays the part of an essence, and when we accept it we cease to be population-thinkers and ignore the evidence that is before our eyes.

### The hazards

How do we become natural historians, pathographers with open minds? We must start, I think, by recognizing the hazards of the discipline.

We cannot have completely open minds. When we see a man with pneumonia we will not think of recording the colour of his eyes because we have no place for such an observation in the framework of reference we bring to his bedside; yet who can swear that it is unimportant? There is a dilemma: if we stay within our existing frameworks, we learn nothing new; if we go outside them, we run the risk of becoming butterfly collectors, and not many of us will be content with that even though some Darwin of general practice may come along one day and put our collection to good use.

It is even less possible to have a completely open mind all the time. Every patient we see will present us with opportunities for pathography, but if we try to start 150 new pathographic projects every week we will soon be paralysed. Another dilemma then: we fail if we have open minds and fail if we keep them closed.

Merely posing these dilemmas suggests how we may avoid them. We can each take for ourselves one little patch of ignorance, ours to explore when we have the time and the inclination. Within this patch we will not behave as we usually do, for the normal rules will be suspended. We may not be able to leave all our frames of reference outside it, but we will not carry in those which impose on us a need to have answers and make decisions. Inside we will be free just to learn, to ask questions beyond those we customarily ask and beyond the dictates of our textbooks: the only limits will be those set by our imagination and our curiosity. No doubt a number of our patients will be astonished to see us in the role of learners, but many will be delighted and some will roll up their sleeves and join in the exploration.

### The rewards

Perhaps it will seem like entering chaos when we first throw off the shackles of immediate responsibility, but when we recover from the shock we will always find some lead to follow, and when that runs out there will be another. In time, we will have ideas about what goes with what and what follows what; but, because they will always be growing and changing, there will be no great pain when we have to discard them, and each time

that we do we will know a little more.

This will not be the systematic kind of knowledge on which lectures are based, but the sort that is embodied in anecdotes and speculation which we can exchange with colleagues, especially those whose areas are close to our own. Nearly 400 years ago, Francis Bacon expressed its value when he wrote in his *Advancement of learning*:

‘But as young men, when they knit and shape perfectly, do seldom grow to further stature; so knowledge, while it is in aphorisms and observations, it is in growth; but when it is once comprehended in exact methods, it may perchance be polished and illustrate and accommodated for use and practice; but it increaseth no more in bulk or substance’.

The early days of our pathography will not be a time of butterfly collecting, but a period in which the mind is preparing itself to be favoured by fortune.

How and when ideas and insights are born has been richly explored by Arthur Koestler, and in his book *The act of creation*<sup>26</sup> he defines the process as one in which a person suddenly makes an association between things already known, perhaps many years after each component item of knowledge was acquired. It is the seeing of analogies that no one saw before, the unlikely marriage of cabbages and kings whose union answers a previously insoluble problem; and, Koestler says, the ultimate matchmaker is the unconscious mind. His examples are of major discoveries by great scientists — Kepler, Gauss, Ampere, Pasteur and Einstein among others — but there is no reason to believe that there is any difference at a lower level in the preparation of the mind by long immersion. Certainly the process of creation as described by these scientists sounds remarkably like some words of Dryden in his introduction to *The rival ladies*, where he speaks of a time when the play was only ‘a confused mass of thoughts, tumbling over one another in the dark: when the Fancy was yet in its first work, moving the sleeping images of things towards the light, there to be distinguished, and then either chosen or rejected by the Judgement’.

Pathography will be the source of our ideas and insights, but over the horizon there will be developments we cannot yet foresee. Every discipline evolves its methods in response to the needs of the time and through the genius of its individual practitioners; and we will learn how best to combine descriptive research with quantitative methods, with experiment and with techniques not yet invented. We will discover how to collaborate most fruitfully with other clinicians, with medical scientists and with non-medical scientists too. Slowly we will create our own language, as unique as the clinical setting from which it springs. I cannot tell you what the language will sound like, but it is through pathography that we will move the sleeping images of things towards the light where we can name them.

### Conclusions and implications

The time has come to draw the threads of my argument together, to present my conclusions and consider their implications.

I believe that research in general practice has a great deal to offer medicine and that, if it becomes accepted as a normal activity, this could be important in assisting the survival of the first- and continuing-contact doctor in primary health care. There is scope for many kinds of research: positivist, ethnographic, quantitative, qualitative and descriptive.

A very small minority of general practitioners will have an interest in experimental laboratory science, and we must not be as ready as we have been in the past to see this as the province of hospital clinicians only. Anyone with a worthwhile idea and enough suitable patients is likely to find laboratory collaborators, as did Maurice Stone for his work on blood lipids and

fibrinogen, and Albert Jacob does in searching for a test for the early diagnosis of cancer.

A larger minority will want to master and apply the special skills of other disciplines: most commonly the quantitative techniques of epidemiology, but also perhaps the qualitative techniques of the behavioural sciences.

The doctors with these minority interests will select themselves and we must encourage them. There is much they can do to increase our understanding, improve the treatments we use and better the ways in which we organize our services. Important as they are, they are not our first concern — though we shall all have to become numerate enough to audit our work in our practices.

For most of us, the straightforward way of expressing and maintaining professional curiosity through the whole of our working lives should be pathography, and this implies that we must make some changes.

These can start, in a limited way, at the undergraduate level. In my own department we are developing exercises for students which make them go into far greater detail about the history and symptoms of some common conditions than they normally do when talking to patients, and compare what they learn with what their textbooks say. We are hoping that this will leave them with a permanent mistrust of essences.

Much greater change is needed at the postgraduate level. It is now quite common for trainees to undertake small research projects, usually of an epidemiological kind. Few do so with any great enthusiasm, and all too many, in my experience, suffer seriously from a lack of competent guidance. This is inevitable unless we add a training in epidemiological methods to the growing list of criteria for being a trainer.

If epidemiological projects became the exception, and pathographic projects the rule, the situation would be very different. I believe that most trainees would find themselves genuinely interested, that their trainers could really help them, and that the exchange of ideas at release courses could bring a fresh excitement to sessions often in need of it.

### The College

The College has in the past put its weight, and its money,<sup>27</sup> behind education and practice organization rather than research. There were good historical reasons for this, but it is now time to change the emphasis. Our membership examination should have a section, carrying a high proportion of the total marks, in which we find out from each would-be member what has aroused his or her curiosity and what he or she is doing about it. The effect could be enormous. Our journal may need a section in which members can write about their pathographical speculations and theories in a completely informal way, without the need to dress them in the white tie and tails of a standard research publication. It did, after all, have its origins in modest periodicals called 'Between ourselves' and *Research newsletter*.

I am all in favour of supporting our research units and university departments of general practice, of creating more research fellows and extending the bounty of our Scientific Foundation, but these are not enough; they will make no deep impression on general practice as a whole. For any real change we need a bottom-up approach and some population-thinking about the latent talents of our members. As a College we must really believe that general practitioners can make a contribution to clinical medicine by exercising their eyes, ears and imaginations in their daily work; and we must support them with a structure in which these faculties can be developed.

### Pickles as pathographer

It was Pickles' work that led me to my theme; my theme now leads me back to him, to complete the assessment I was trying

to make. It may be true that he produced little formal research, that his principal findings were not as original as he may have supposed them to be, and that he cuts a small figure beside giants like Budd and Mackenzie. Read him though as a natural historian, as a pathographer of infectious illnesses, and he sets a standard that few of us would not be happy to achieve. With a notebook, the help of his wife and daughter, no special technique other than a recording system learned from a school doctor and unflagging curiosity, he set out to show that his textbooks were wrong — as they were — and ended by making a useful contribution to a dark area of the medicine of his day. We could have no better model.

Perhaps the role of the general practitioner as the natural historian of medicine was never in doubt, but for me the journey on which I found out why was a considerable experience. T.S. Eliot tells us that

'... the end of all our exploring  
Will be to arrive where we started  
And know the place for the first time'.

I think he means that we will then be able to see sunflowers.

### References

1. Pickles WN. *Epidemiology in country practice*. Bristol: Wright, 1939.
2. Pemberton J. *Will Pickles of Wensleydale*. (2nd edition). London: Geoffrey Bles, 1972.
3. Mackenzie J. *The principles of diagnosis and treatment in heart affections*. London: Henry Frowde, Hodder and Stoughton, 1916.
4. Pickles WN. Epidemic catarrhal jaundice: an outbreak in Yorkshire. *Br Med J* 1930; **1**: 944-946.
5. Cockayne EA. Catarrhal jaundice, sporadic and epidemic, and its relation to acute yellow atrophy of the liver. *Q J Med* 1912; **6**: 1-29.
6. Blumer G. Infectious jaundice. *JAMA* 1923; **81**: 353-356.
7. Booth WE, O'Kell CC. Epidemic catarrhal jaundice. *Public Health* 1928; **41**: 237-243.
8. Price FW. *A textbook of the practice of medicine*. (3rd edition). Oxford University Press, 1929.
9. Hutchinson R. *Diseases of children*. (7th edition). London: Edward Arnold, 1936.
10. Royal College of General Practitioners. The educational needs of the future general practitioner. *J R Coll Gen Pract* 1969; **18**: 358-360.
11. Morrell D. Now and then. *J R Coll Gen Pract* 1979; **29**: 257-465.
12. Sackett DL, Haynes RB, Tugwell P. *Clinical diagnostic strategies in clinical epidemiology*. Boston: Little, Brown, 1985.
13. Ajeel NA, McCormick J. The use of routine blood counts. *Ir Med J* 1984; **77**: 100-101.
14. Long AF. *Research into health and illness*. Aldershot: Gower, 1984.
15. Bilton T. *Introductory sociology*. Basingstoke: MacMillan, 1981.
16. Parsons T. *The social system*. Glencoe, Illinois: Free Press, 1951.
17. Goffman E. *The presentation of self in everyday life*. Harmondsworth: Penguin, 1971.
18. Barley N. *The innocent anthropologist*. Harmondsworth: Penguin, 1986.
19. Whyte WF. *Learning from the field*. Beverley Hills: Sage, 1984.
20. Helman C. *Culture, health and illness*. Bristol: Wright, 1984.
21. Flanagan JC. The critical incident technique. *Psychol Bull* 1954; **51**: 327-358.
22. Eilbert LR. A study of emotional immaturity utilising the critical incident technique. *University of Pittsburgh Bulletin* 1953; **49**: 199-204.
23. Mayr E. *The growth of biological thought*. Cambridge: Harvard University Press, 1982.
24. Ryle J. *The natural history of disease*. (2nd edition). London: Oxford University Press, 1948.
25. Richardson IM. Verities yet in their chaos. *J R Coll Gen Pract* 1981; **31**: 328-333.
26. Koestler A. *The act of creation*. London: Pan (Picador), 1975.
27. Bain DJG. The Government, medical education and research. *J R Coll Gen Pract* 1988; **38**: 293-294.

### Address for correspondence

Professor C.M. Harris, The University of Leeds, Clinical Sciences Building, St James's Hospital, Leeds LS9 7TF.