PART ONE

RESEARCH AS AN ART

1

Puzzles, Problems and Enigmas*

This is what research means for the individual scientist.

When dons discuss the research they are doing, they refer to it as 'their own work'. But the tone of voice they use is that of children talking about playtime.

Real scientific research is very like play. It is unguided, personal activity, perfectly serious for those taking part, drawing unsuspected imaginative forces from the inner being, and deeply satisfying. The sociological theory that it is performed to produce contributions to learned journals in exchange for professional recognition may not be quite nonsense – but it is totally irrelevant to the psychology of the researcher. The alternative theory that the scientist is merely the agent of economic forces and social classes in an inevitable historical process may also have some merit, taking a broad view, but again it does not explain the passion with which this strange activity is pursued. Science is not a job; it is an obsession. One knows of old scholars, full of fame and honours, who never can retire; their addiction is unconquerable, like that of an elderly duke who can never leave off fox-hunting no matter how dangerous the falls for his brittle bones.

What is so fascinating about it? To sit for hours over a microscope counting cells, or particle tracks, or spines on the eggs of flies! To worry oneself sleepless over an algebraic equation or the wiring of an electric circuit! To read and reread, to write, and rewrite, a half page of mysterious jargon that only 17 other people in the world will really understand! That is what scientists seem to do all day, and half the night; why don't they get themselves healthy, manly jobs, like piloting aeroplanes, or managing insurance companies, which would be more fun, more leisurely, and better paid?

Scientific research is solving puzzles. The pleasure to be got from it is

* BBC Radio broadcast, January 1972, as part of the science curriculum of the Open University.

the pleasure of the crossword or jig-saw addict. First the blank diagram, or the scatter of meaningless pieces; then an occasional tentative clue or the few pieces of the same colour that seem to fit together; next a period of frustration, going over and over the list of clues, or trying piece after piece in the most unlikely conjunctions; then – ah, the sweet joy of the word that completes a doubtful acrostic, or the section that springs to life as a tree, or a house or a pot of flowers; finally, the completion of the pattern, with clue after clue solved in rapid succession, or the last few pieces tumbling into place. By accepting the challenge, the tension, the concentration, the frustration, we heighten the pleasure of the moments of revelation. The more difficult the puzzle, the greater the tension – and so much greater the delights of solution.

Scientific puzzles are truly difficult. The solutions may take months, or years; sometimes we must leave them unsolved to tantalize our successors. Think of Fermat's last theorem, an apparently simple property of numbers which *he* said he could demonstrate. It was published exactly three centuries ago, and is still neither proven nor found to be false. Or recall the long mystery of the magnetism of the Earth, first studied by Gilbert who was court physician to Queen Elizabeth, and only recently resolved by the convective dynamo model. Such puzzles, indeed, are never quite solved; unlike a crossword or a jig-saw, they have no boundaries; chop off one head, and, hydra-like, a hundred new ones grin maliciously at us.

One of the charms of a scientific puzzle is that we are seldom trying to tackle it alone. All over the world we discover unknown colleagues, working away on the same part of the picture, themselves putting in a piece here and there, and showing their appreciation when we too are successful. Much of the pleasure to be got from science is this comradeship – the friendship of enthusiasts with the same peculiar hobby. But if we take our game too seriously, we can also be badly hurt if we are forestalled in a discovery or shown to be wrong. I must admit, as a jig-saw addict, that I can't bear another member of the family leaning over my shoulder trying to put in some pieces – especially if one or the other of us has fitted a section together incorrectly!

It is sometimes asserted that modern science is a rat-race – everyone trying to do the other fellow down. This does not seem to be an essential feature of research, except in very fashionable fields, where a competitive spirit is deliberately fostered, and prizes are awarded to those who seem to have won a race to complete a promising section of the puzzle. There are really quite adequate rewards for any competent research worker in the tussle with Nature itself. Intense scientific rivalry causes as much damage by emotional stress as it achieves a spur to effort.

The fact that science is a communal activity provides the only strict rule of the game: the answer that we offer to any scientific question must be good enough to convince our scientific colleagues. It is useless to pretend that we have a solution if we cannot demonstrate it clearly to other people and persuade them that it is true. In a very broad sense, this is the difference between a science and an art, such as painting or poetry, where our statements about the world may be as private and personal as we care to make them. The purpose of science is to create a body of *public* knowledge, not an impalpable, shifting scenario that could be a camel for one man and a weasel for another.

This rule imposes rigid constraints on the content and methodology of research; I would myself say that it defines the whole activity. But then a good tough rule like this, which prohibits the soft, easy, self-deceiving solutions, is just what we need to make a puzzle really interesting. At least it gives us a reasonable test, beyond mere fashionable acclaim, of whether we are anywhere near right in our answer.

This is one of the basic difficulties; the correct solution of today's scientific puzzle is not waiting for us on the back page of tomorrow's newspaper. We cannot look at the picture on the box, or turn to the list of answers at the back of the book. Genuine scientific puzzles are not at all like the conventional 'problems' and 'exercises' at the end of each chapter of a school textbook. By definition, no-one knows the answers in advance, and only time will tell whether an apparently convincing solution is the real truth. This is a feature of research that often distresses the student, who has become accustomed to the closed, convergent intellectual world of elementary basic science, where it seems inconceivable that every simple question should not have a simple definite answer.

Many grown-up research workers fail to escape from this psychological scaffolding. The puzzles that they attack are clearly defined, within the current scientific conventions of the day, and the methods for their solution are familiar and well-tried. Such puzzles are to be found repeatedly in the laborious and expensive process of technical development – the design and manufacture of machines for everyday use – but they are also a major part of academic science under normal circumstances. Not only do they provide pleasurable employment for a great many talented people, but their solution is necessary for scientific progress. It is a great error to suppose, for example, that classical physics is a closed system, already complete at the death of Lord Kelvin in 1907, and thereafter superseded by quantum theory. There are innumerable questions that can still be posed in the language of classical physics, such as the nature of the convective magnetohydrodynamic motion in the liquid core of the Earth, whose exact solution would be both difficult and immensely instructive. The fact that all the formal mathematical machinery for such a solution may be ready to hand does not tell us the answer by merely turning a handle. Imagination, intuition and intellectual invention are as valuable here as they would be in any attempt at a total theory of fundamental particles or of the history of the cosmos. The dry, formal language that is conventionally used to report scientific work of this kind is as inadequate as a marriage certificate is to describe a love affair. Nobody except a computer theorist pretends to solve interesting puzzles by a succession of purely mechanical deductions; the much more subtle problems of genuine science cannot conceivably yield to such procedures. Even within the most formal, academic and sterile branches of science, the logical apparatus of symbols, deductive geometry and digital computation are mere aids to imaginative thought of an intuitive kind.

The best evidence that research is very far from a mechanical process is the vast amount of error to be found in the archives of science. Another psychological shock for the well-brought-up, conformist science student is the discovery that three quarters of the scientific papers on any subject are wrong. The sources of error are manifold – experimental inaccuracy, mistakes of arithmetic, neglect of significant disturbing effects, theoretical misconceptions, etc. The work is done in full earnest – and yet it fails to be quite true or convincing. This does not mean that most scientists are fools; it means, quite simply, that research is always much more difficult than it seems, and that truth is a very stern master. The man who can always get things right is immensely respected – but we are forgiving of honest error, for we know the pitfalls on the way.

For this reason, puzzle-solving within the boundaries of an established scientific discipline is by no means uncreative. Although the main lines of general theory may be correct, the archives must be purged of innumerable minor errors and misunderstandings and augmented with a multitude of further observations. As with a great deal of practical, applied science and technology, immense satisfaction may be obtained from the mere exercise of specialized skills and experience – the delight of the master craftsman in his own handiwork. It is the wise scientist who knows himself well enough to be content with this level of achievement, rather than battering himself to pieces in the search for a 'breakthrough'. Indeed, this is the limit of intellectual independence permitted to most of those who take part in the vast experiments of Big Science, where a whole team must cooperate in the use of very expensive apparatus.

Yet science does not stand still. As puzzle after puzzle is solved, within some orthodox framework of theory and method, we begin to perceive new *problems* – questions that can be easily formulated but have no obvious answers. In my student days, a typical problem of physics might have been 'What is the nature of nuclear forces?' or 'Why are some metals superconducting?' A corresponding biological problem would have been 'What is the chemical mechanism of heredity?' These problems have now been solved – but they have been replaced by others such as 'What is the connexion between quantum theory and gravitation?' or 'What is the cause of biological aging?'

For most pure scientists, the attempt to solve problems is the real goal of research. It is the problems that make the subject exciting and challenging, carrying one away from the settled textbook regions to the unexplored frontiers of knowledge. Problem-solving is difficult indeed, for it demands great leaps of insight and imagination, but it also wins the big prizes and makes the big names. It is a mistake to suppose, however, that only speculative imagination contributes to the overall progress of science. Much academic research that seems, superficially, to be mere puzzle-solving is directed more subtly, towards some larger problem, perhaps by clearing away minor errors and uncertainties that seem to confuse the issue. The clever-clever guys, with their fertile inventive minds, must be balanced by the learned scholars, who use their intellectual powers to criticize and reject the wilder fantasies, and thus keep science from running away into nonsense.

The fascination of research for the mature scientist is this balance, this tension, between beautiful new conjectures and well-established fact – a tension in which one must keep one's mind open to every possibility, and yet never be blind to unanswerable objections. Medawar has called it 'The art of the soluble' – the skill to choose questions that are not trivial, and yet can be attacked and answered within a lifetime of effort. For each of us, this strategy must be a personal decision, based upon a shrewd estimate of our own capabilities, our own strength of will and of the current state of the art. The framework of choice is always changing, as new discoveries are made and new problems come to the surface. It is a sobering exercise to look back, perhaps no more than five or ten years, and to see how the whole climate of opinion has altered, so that old mountains of difficulty have now shrunk into molehills, and old fogs of mystery have evaporated into clear air.

A certain spirit of conservatism is inherent in all branches of scholarship, for the hard-won truths of the past must at all costs be preserved. The social system of the scientific community, by giving great weight to the authority of age and experience, often puts obstacles in the way of the revolutionary thinker – as, for example, in the case of the theory of continental drift, which was rejected for half a century by 'official' geology. It is easy enough, with the wisdom of hindsight, to see the follies of our ancestors. We know very well that the active scientist must be an anarchist at heart, ready to overthrow the old gang' as soon as he doubts their credentials. But this is a counsel of perfection; the habits of thought, the mature experience, of a lifetime are not thus recklessly to be discarded.

For this reason, the highest honours of science are accorded to the revolutionary geniuses who do more than solve problems – they perceive that problems exist. The contribution of a Galileo, a Darwin or a Wegener may be described as the formulation and resolution of an *enigma* – a question concerning a deep and mysterious feature of existence, such as physical motion, biological speciation or the shape of the continents, that had scarcely been asked before. To dare to propose such problems for scientific investigation, they had to step out of their own mental skins; they had to look again through the eyes of childhood; they had to exercise uninhibited curiosity.

It is often said that real scientists are full of natural curiosity, but in our own highly organized scientific profession this talent is only cultivated in an institutional, collective manner. Without individual curiosity, there could have been no notion of an enigma; without an unhealthy interest in the enigmatic, there would be no scientific problems; only by the solution of problems have we acquired the technical power to solve the puzzles of our complex civilization.

2

Einstein*

He was the greatest; what does he mean to us now?

The traveller of independent mind eventually discovers, alas, that the popular sights – the Grand Canyon, the Sistine Chapel – deserve their reputation. It is well to be reminded that the most famous scientist of our times merited equally unstinted admiration. It was a pleasure and an inspiration to revisit this great intellectual monument with such civilized guides, to catch glimpses of this gentle genius at his creative task.

What does Einstein mean to us now? To the general public, he is a magician who uttered the formula by which matter could be turned into energy and who conjured away the reality of space and time. To the physics graduate, he is the most brilliant of all the professional star performers, contributing to every branch of modern physics – mechanics, electromagnetism, quantum theory, cosmology, statistical mechanics and the theory of solids – brief, lucid, modestly phrased papers that pierced to the heart of the mystery and made all things clear.

Only a few thousand experts are familiar with his greatest work, the general theory of relativity, which showed that gravitation could be represented as space–time geometry. It is a work of such physical, philosophical and mathematical originality and depth that it is doubtful whether anything like it could ever have been produced by any other man who has ever lived. Let us admit it: he *was* the greatest.

We admire also the lifestyle, so simple and modest, so kind and friendly, so courteous and good humoured, so liberal, wise and humane. Newton, as we are now informed, was arrogant, vain, suspicious and quarrelsome. It is difficult to imagine the character defects that will be discerned in our Einstein, 300 years from now. The failure of his first marriage seems no more than a mild accident of fortune. It was nothing to be ashamed of that he could not accept the probabilistic interpretation

* From a review of *Einstein* by Jeremy Bernstein (Fontana, 1973), and *Einstein: The Man and his Achievement*, edited by G. J. Whitrow (Dover, 1973), in the *New York Times Review*, September 23, 1973.

of quantum mechanics. Nor that he lavished his efforts for the last 30 years of life, without obvious success, on attempts to unify electromagnetism with gravitation. Like many other great artists, he had moved far beyond the tastes and style he had created for his contemporaries, and worked to satisfy only his own standards.

It is sad, nevertheless, that the 'age of Einstein' ended long before his death in 1955. Nazi persecution and exile in America broke into his life when he was 50, but were not so personally tragic for him as for several million other European Jews. He continued to the end as he had begun, dedicated to research, clear and concentrated in mind, undistracted by the world. But science itself moved away from Einstein.

Is he now, in Herbert Dingle's words, 'the popular idea of the typical scientist'? Can one honestly advise anyone to follow his example? How foolish not to take school seriously, not to strive to get a good degree! What scientific job will there be for the man who despises the lecture programmes and thesis advisers of graduate school, who fails to get a Ph.D.? How will he obtain access to the reprint exchanges, computer programs and scientific conferences as an assistant in a patent office? Will his papers be acceptable – without citations to all the current literature and unaccompanied by a bank draft to cover the page charges? How long will his promotion be deferred, as he fails for ten years to solve an apparently crazy problem? Where will he get the research grants to maintain his scientific reputation – if he cannot tell the research council what he will discover? What will they think of him in the nation's capital if he refuses, as a pacifist, to advise the Pentagon secretly on new weapon systems? Or, if he happens to be a Russian, will he be permitted to go to Israel even if he has done no secret work? One can even say that physics no longer has need of such intellects, that all the big problems have been solved, that all we are doing now is 'normal science,' filling in the details, exercising our minds with little puzzles, and devising more and more frivolous or destructive gadgets.

And yet, one day, who knows, there may arrive on my desk a scruffy handwritten manuscript, from a forgotten student, who got a Third and went off to become a lighthouse keeper (Einstein's ideal occupation!), explaining the mass spectrum of the elementary particles, or the nature of quasars, or the connexion between electromagnetism and gravitation. I shall be wise (and the world will be lucky) if I take a second look at it, and consider whether, beneath the fantastic notions, there may not lie a universe of truth.

3

Thematata*

What were Einstein's ideas, and where did they come from?

Professor Holton illustrates one of his essays with a *colère,* by the French artist Arman, consisting of the shattered instruments of a string quartet; to symbolize the whole collection of articles reprinted as *Thematic Origins of Scientific Thought* he might have used one of Gian Piero Pirovano's imaginative representations of the chaos of things *before* they are made. The 'science' of the published paper, which the philosophers so earnestly analyse, is S_2; the chrysalis of an idea in the mind of the scholar is S_1, essentially private and yet the source and origin of public knowledge. What goes into the creation of a scientific theory? Can we make a plausible reconstruction of the process of intellectual discovery?

The task is more difficult than it looks. The public statement of a new theory is not intended as a confession or an autobiographical playback of the creative act. As Holton puts it, S_2 is projected onto the '*x–y* plane' – the dimensions of empirical fact and logical theory. But S_1 extends deep into the '*z*' dimension, where tacit principles and intuitive comprehension are equally valid. His purpose, expressed in characteristically lucid, refined and scholarly prose, is to discover the *themata* that have been called into action in that private world. In a series of detailed studies, mainly concentrated on the 'nascent moment' of 1905 when Albert Einstein conceived the theory of relativity, he shows how much can be achieved by very careful attention to biographical details, autobiographical comments and the substance of the published work itself. It is a delicate intellectual exercise, of particular fascination for anyone who is familiar with modern physics, illuminating the inner meaning of scientific knowledge.

Every critic knows that the reconstruction of the creative act of any great artist can never be better than provocative speculation. But theoretical physics is an art whose elements are not beyond reckoning. For all his

* From a review of *Thematic Origins of Scientific Thought: Kepler to Einstein* by Gerald Holton (Harvard University Press, 1973); paid for, but apparently not published, by the *New York Times Book Review.*