Education and the compleat scientist

J. Z. Young

Teaching and Learning about Science and Society. By John Ziman. Pp.190. ISBN 0-5212-3221-X. (Cambridge University Press: 1980.) £9.50, $22.50.

MOST scientists are interested in scientific education, if only for the memory of their own debt to it. I imagine that a great many became scientists because they were lucky enough to have an inspiring teacher. My own at Marlborough College was A. G. Lowndes, a remarkable man who trained several Fellows of the Royal Society and a Nobel Prize Winner (Peter Medawar). He inspired us by his own somewhat amateur researches, he showed us endless living things and, by making us work even in our spare time, taught us that all science involves much hard work.

These are perhaps the main items that must be included in any scientific education: it must catch enthusiasm, it must cover a huge mass of facts and it must find out those who can enjoy learning a lot of detail. This is how scientists are made, but in the process they seldom learn much about the position of science in society, still less about its history or philosophy.

It is these defects that are the particular concern of Professor Ziman as indicated by the title of his book. I cannot remember ever hearing them discussed by my teachers either at school or university. Probably many scientists still take up their subject because they are interested and like it, though there is now more concern about its social implications. They are all too liable to fall into what Ziman calls "scientism and technocracy", the uninformed assumption that everything that is done in the name of science is good. How can this restriction of the development of young scientists be avoided? Ziman has tried to encapsulate the problem under the acronym STS — Science, Technology and Society. These themes permeate the political, economic and cultural issues of our times yet "there is no *rationale* of STS as an educational subject", although various courses of study of it have been tried. In trying to provide this rationale Ziman manages to stimulate thoughts about a great many fundamental questions, even beyond those dealing with the educational problem itself.

For instance, one feels that the discussion obviously needs a definition of science, and many times Ziman seems on the point of providing one, but wisely never comes down to calling it a definition. Sometimes it is a question of method: "Science derives its practical power and authority from the rigours of its arguments and the hardness of its facts". Moreover, he repeatedly stresses that the production of scientific knowledge is a social process. Then only a page later we are in a different world of discourse: "There is no single 'scientific' map of reality — or if there were, it would be much too complicated and unwieldy to be grasped or used by anyone". Is this true? I suspect that Ziman doubts it himself. He pursues the question into a discussion of the hierarchy of scientific enquiry and the questions of emergence and reductionism,

> the notion that the properties of complex systems such as organisms or molecules can be 'reduced' to the laws satisfied by simpler systems such as cells or atoms — is not only a very dubious philosophy, it is a dangerous folly in science education, where the map appropriate to each level must be taught wholeheartedly according to its own lights

Yet probably most scientists have a hankering for that "folly". And is it indeed so foolish? It is true that chemistry and biology have their own laws but no one can go all the way in either of those sciences unless they know something about physics. However much "autonomy" there may be for each part of science there is still, at least for many of us, a scientific map or model of the world to which we try to refer all events and all knowledge, including the knowledge of ourselves. It seems that the human brain is so constructed that it tries to build all its information and schemes of action around a unified model. Of course this grows gradually in each one of us, centred at first around a parental scheme. The scientific model, if properly learned, can provide the adult substitute, which many people feel they lack in the absence of religion. That is of course not to say that science should be dogmatic or be treated as religion, but that it can provide what Ziman calls "the possibility of discovering order in nature" and to do this is a requirement for human life. We all need some system of order. Incidentally this is a characteristic specific to human beings and not present in our nearest relatives. Ziman actually recognizes this later in his book when he says that "scientific world pictures" allow "deductions that help us along the way through life".

This is only one of the ends that he believes could be achieved by the proper attention to STS education. He holds that they can only be reached by specifically designed courses for all levels of scientific education. In the later part of his book he discusses how these should be planned for different stages. He is against "General Science that is too sloppy and technical science that is too arbitrary", and he advocates a "valid science". This is easy enough to say until you come to the job of deciding what to put in and what to leave out. Ziman discusses the many ways in which it could be done. For instance, it can be through the social relevance and applications of science, through its possibilities as a vocation for the individual, or its history, or its philosophy or its value in solving world problems. But he does not try to specify the content of particular courses. His aim is to show that the teaching of science at present is grossly defective because it does not properly develop the understanding of the individual either of himself or society, or indeed of the nature of the science he is learning. His plea is that teachers of science should take definite steps to fill this gap by designing what he calls STS courses. He knows that there will be much opposition and anyone who has tried to introduce such courses will have experienced this. But there are many people ready to agree with his plea that a better understanding of the relations of science and society is needed for the proper training of research scientists as well as doctors and other technologists, not to mention civil servants, politicians and the general public. A major value of this interesting book is that it makes you realize how difficult it is to reach agreement either about the nature of science or its proper place in society.

J.Z. Young is Emeritus Professor of Anatomy at the University of London.

IQ gladiators in separate combat

Stuart Sutherland

Intelligence: The Battle for the Mind. H.J. Eysenck versus Leon Kamin. Pp.192. ISBN hbk 0-333-31279-1/0-471-08884-6; ISBN pbk 0-330-36399-4. (Macmillan Press, London/Wiley, New York/Pan: 1981.) Hbk £12, $12.95; pbk £2.95.

NATIVISTS have battled with empiricists for many years, but never with such fury as over the inheritability of intelligence. It might be thought that the issue could be resolved by bringing together representatives of the opposing sides and letting them argue out, point by point, the evidence for the respective roles of inheritance and of environment. *Intelligence: The Battle for the Mind* could have provided just such an opportunity, but unfortunately it is constructed in such a way that the combatants only skirmish and never meet in a decisive battle.

The book contains four sections. In the first, Professor Eysenck sets out his case for supposing that in Western society inheritance accounts for about 80% of the variation in intelligence. Although he does not enumerate them, he deploys 20 different lines of evidence from which he claims this inference can be drawn. In the second

section, Professor Kamin produces his criticisms of some of the evidence for the importance of heredity, but this section is not a reply to Eysenck's initial foray since it was evidently written without Kamin having read it. In consequence, many of Kamin's arguments are beside the point: for example, although Eysenck excludes all reference to Burt's data, now universally acknowledged — thanks to Kamin's brilliant detective work — to have been faked, Kamin devotes a chapter to attacking Burt. In the final two sections, which are much too brief, Eysenck replies to Kamin's set-piece and Kamin to Eysenck's, and there the matter rests.

No agreement is reached on which aspects of the evidence are reliable and which are not, and both authors are guilty of failing to meet the arguments adduced by the other. Thus, in his opening section Eysenck ignores many of the criticisms previously levelled by Kamin. For example, one of the strongest arguments in favour of the role of inheritance is that when monozygotic twins are brought in by foster parents, there is a higher correlation between the IQ of the natural parents and the child than between that of the foster parents and the child. Kamin has put forward two ingenious suggestions to explain this correlation in environmental terms. First, many of the foster children in question spent a year or more of their lives with their natural parents before moving to foster parents. Second, the IQ and the socio-economic status of foster parents is in general high and has much less variance than the IQ of the population at large: this reduction in environmental variance would reduce the effects of environment. One would have liked to have seen Professor Eysenck's reply to these arguments, but it is not to be found in the book. On the other hand, Eysenck points out that there is a higher correlation between the IQs of monozygotic twins reared together, but one looks in vain for Kamin's response to this argument.

Both authors give the impression of being determined to make a case and of selecting data that forward that case. It is a pity that Eysenck did not concentrate on the most solid evidence for inheritability and ignore dubious or unrepeatable findings. For example, he argues that there is a difference in the brain waves (EEGs) of the intelligent and unintelligent and that this difference suggests intelligence is inherited. Not only is the argument fallacious, since there is no reason why brain waves should not be in part determined by environmental factors, but recent attempts to repeat the finding Eysenck uses have failed.

Eysenck points out that Kamin does not have a theory of intelligence. With great ingenuity, Kamin has taken a number of pieces of evidence thought to support the hereditary case and has shown that there are other *possible* explanations: he has not proved that these other explanations are correct. Moreover, since Kamin treats each piece of evidence in isolation, he is able to use individual arguments that are inconsistent with one another. Thus, as Eysenck notes, Kamin explains the large differences in IQ between dizygotic twins reared *together* by assuming a large difference in the way they are treated within the family. Elsewhere, however, he explains the similarity in the IQs of monozygotic twins brought up *apart* by arguing that each pair is placed in families of a similar socio-economic background. Taken together, Kamin's two arguments imply the absurd conclusion that there is less environmental difference *between* families than *within* a family.

There are a few things on which both authors agree. Moving children from deprived homes to particularly good homes can bring about a shift of up to 20 points in IQ. Although Kamin regards this as compelling evidence for the overriding importance of environment, Eysenck is able to show that it is consistent with his own theory of intelligence. He does not maintain that a large shift in environment cannot change IQ, merely that in conditions as they exist today IQ is more determined by heredity than environment.

Kamin does not help his case by his personal virulence towards Eysenck. The fact that Eysenck has twice made a mistake about the sex of an author whom he cites has surely nothing to do with the inheritance of IQ; and it is merely distracting to the reader for Kamin to stress past errors made by Eysenck, at least some of which (for example, his defence of Burt) Eysenck now acknowledges. Moreover, Kamin's allegation that the aim of the science of genetics is to make "the world comfortably safe for white males" is both vituperative and false — white males were a good deal safer before IQ tests were ever thought of. Eysenck rightly insists that differences in the average IQs of groups have no bearing on how the individual should be treated. It is impossible to predict the individual's IQ from skin colour, sex or social class and in at least some instances the use of objective tests for selection has favoured underprivileged groups. Burt may have been a scientific scoundrel, but his introduction of intelligence tests for secondary education in Britain between the wars doubled the proportion of poor children in secondary schools.

Like all scientific discoveries, the results of work on intelligence testing can be used for good or ill. As Eysenck notes, recent decisions by anti-racists in the United States are likely to be extremely damaging to minority groups. For example, it is folly to insist that equal proportions of blacks and whites should enter training programmes for the educationally subnormal, if, for whatever reason, a higher proportion of blacks than of whites need the help of such training. Eysenck quotes a remark of Dr Johnson's which sums up the position: when asked whether men or women were more intelligent, he replied: "Which man? Which woman?".

Intelligence: The Battle for the Mind is a wasted opportunity. One feels that had Eysenck concentrated on the most compelling arguments for the role of inheritance, and had he attempted from the outset to meet Kamin's ingenious but *ad hoc* arguments where they could be met and conceded cases where they could not be met, the debate might have been settled. Eysenck might not have proven that IQ is 80% inherited, but he might have established once and for all that there is a strong inherited component.

Neither combatant comments on the scientific importance of intelligence testing. A much more destructive criticism can be made of the whole enterprise than any advanced by Kamin, namely, that it has told us nothing about how the human mind works nor has it given any answer to the really important applied problem — how can we make it work better? □

Stuart Sutherland is Director of the Centre for Research on Perception and Cognition at the University of Sussex.

Reality of science

Peter Newmark

An Imagined World. By June Goodfield. Pp.288. ISBN 0-06-011641-2. (Harper & Row: 1981.) $12.95 US only.

IF A scientist wishes to discover what makes a certain type of cell tick, she may well spend five years studying its behaviour and composition. If a writer wishes to find out what makes that scientist not only tick but chime, by way of making scientific discoveries, she may decide to track the scientist's every thought throughout the five years. That neither venture is guaranteed to succeed is illustrated by this book. The story, however, is not without interest.

In 1975 June Goodfield met Anna Brito (a pseudonym) who was then on sabbatical in New York and, within hours, decided that she fitted the bill of an individual research worker through whom to follow the process of science. Goodfield found Anna Brito "articulate, amusing and somewhat different from the majority of scientists I had met before", and sensed that Brito was at a stage where doors were beginning to open. In one sense, at least, she was right. Within a year of returning from New York to Glasgow University, the doors of New York's Memorial Sloan-Kettering Cancer Center had opened to Anna Brito. From then on Goodfield had ready access to the scientist. So except for the first year and for a period in 1977, when progress is recounted through letters and tape recordings, the tale is told in narrative form.