AUTUMN BOOKS SUPPLEMENT

The pitfalls of bookmaking

The making of a multi-author book can be a long and frustrating business for all those involved — contributors, academic editor and publisher. Here Anthony Watkinson charts the sequence of events and describes what can go awry.

ALL RESEARCH scientists will have written papers in journals. Quite a few who have blamelessly reached middle age will be asked to write a chapter on their specialism in a multi-author volume, that is a book of newly commissioned contributions edited by a fellow scientist. All publishers meet people who will never contribute a chapter again because the experience has been too horrific. The edited book is where the publisher–author interface is raw and bleeding. What happens, and what can go wrong?

It is likely that our neophyte contributor (let us call him Campbell) will first be approached tentatively. A letter arrives from a well-known figure in his field, someone he met at a congress: Campbell is flattered, is likely to agree and then hears nothing more for months. Meanwhile the aspiring editor (Edwards, an energetic and ambitious man) has decided upon the field to be covered, has written a synopsis and picked a team of potential contributors, and is looking for a publisher. Contrary to general belief most books, like most journals, come to the publisher as ideas: few publishers are actually creative. Edwards's pitch (the form rarely varies) is as follows: the field is rapidly advancing/has reached a stage when a synthesis is desirable/has not been reviewed for 20 years (a chemist's argument this one); and although he would love to write a monograph, his commitments are too heavy/the literature is now too vast to be digested by an individual/the fractured nature of the field makes collaboration preferable.

. . . few publishers are actually creative.

There was a time when the handshake of the head of a publishing house, in the Athenaeum or over a cocktail on a roof in L.A., was a commitment enough. These days to the world outside "the publisher" is the relatively humble commissioning editor (here, Parsons). On receiving Edwards's proposal she has two tasks (the pronoun is carefully chosen on the basis of probability). First she takes advice, probably from both regular advisers and appropriate specialists. Advisers never *like* multi-authored books; they lack cohesion etc. Nevertheless, *on balance,* the synopsis appears to cover the ground, Edwards has a good reputation and, given a few judicious additions and subtractions, the proposal is worthwhile and should be accepted.

Parsons's second task is the filling-in of forms which vary from publisher to publisher, but which all aim to establish for the ruling accountants that the book will be profitable and will be profitable quickly enough. Parsons prepares a sales forecast (by imaginative extrapolation from sales of similar books) and on the same basis proposes a price. Some publishers arrive at the price first — the market price or what libraries are thought to find tolerable — and then subtract all the costs to check that the price is acceptable. Others begin by totting up the costs and then calculate the price that will produce the profit margin required by multiplying the "unit cost" (total cost divided by print number) by a certain factor, usually about six. Subtracting from the price the percentage average discount to booksellers and agents gives net income, out of which must come production costs (obtained from typesetters', printers' and binders' scales) and the royalty payment. What remains is the gross profit, which pays all the publisher's outgoings and overheads, leaving a net profit. A second calculation based on the forecasted rate of sales establishes that there will be an acceptable return on the investment within the first year or so.

In the case of Edwards's project the way ahead is now open. The book seems to be viable both academically and commercially. Parsons's final proposal is typical: the book is to be 100,000 words (300 printed pages) in length, comprised of ten chapters, and will sell at £25 with a print run of 1,500 copies. It is time to sign contracts, in which payment for Edwards and for Campbell and his fellow contributors may be a contentious point.

Almost all academic editors are these days offered royalties but contributors not necessarily. Sometimes there is a package: the editor gets a proportion and the contributors divide the rest. Other editors get the lot and make their own arrangements with their (probably medical) contributors. More common now is a royalty to the editor and a lump payment to the contributors, sometimes so much per page and sometimes a fixed sum per contribution, the publisher's argument being that the maintenance of tens of little royalty accounts is disproportionately expensive (overheads killing convenience like the problem of buying single nails). Whatever royalty is offered it is very likely to be a percentage of net income (list price less discount) not of list price: the computer is said to demand this. It used to be said — go for a royalty. However decreasing sales and lower real royalty rates nowadays mean that cash in hand is often preferable for the contributor.

Whatever the arrangement proposed, Campbell and his fellow contributors — maybe a little worried by now — should receive a contract within four months of first approach. If not, it could be that Edwards is hawking the book around less choosy publishers; alternatively, he has been told to shed a chapter or two and has not faced up to breaking the news to the contributors concerned.

Campbell's contract will be subsidiary to that between Edwards and the publisher. It may be a rather scrappy piece of paper but it must *clearly* state title and length of contribution, date by which the typescript should be delivered, who holds copyright and what reward is offered. The document is likely to bristle with misspellings, not surprisingly since in many firms the job of preparation is left to the filing clerk or an inadequately briefed contracts centre. Campbell should challenge any omission or ambiguity but it will need a mass revolt of all contributors to change the financial terms.

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It is usually best for all concerned if copyright for a whole book is held by one body, whether publisher or learned society.

The date of delivery must be realistic: if it is less than nine months ahead no one will treat it seriously and if it is over a year the tempo is going to be very slow. Length of contribution is often vaguely expressed: does the publisher mean printed pages or A4 double-spaced typescript, and, if printed, which format ("print area" is the relevant term here, not page size)? If given in terms of number of words, does this include space needed for figures and references? In addition offprint provision is important to some — are none to be provided, or 10, 25 or even 50? These are gratis but what about the chance of buying more? Campbell should also check on free copies: he can expect one, with the possibility of buying more at a discount.

At this same time Campbell should expect to receive more than a contract. Certainly there should be notes to authors. Because publishers continue to reprint documents drawn up by long-dead production directors in the days of letterpress, demanding imperial units, World List citations and India ink illustrations on Bristol board, such anachronisms should be challenged. Academics whose drawing offices have gone in the cuts might delicately ask about in-house help or perhaps a little advance. Wizards of the SEM should check on technical points and on how many micrographs are expected.

Even more important is guidance about the aims of the book. When Campbell was first approached he is unlikely to have received a blueprint: now Edwards must set out in particular the content, the aims and the level of writing. Are references to be comprehensive (the computer search) or selective? Campbell may be asked for a synopsis to be approved and circulated: if he is, Edwards is doing his job.

In ten months' time the postman will in theory stagger up the path of Edwards's house with ten impeccably presented typescripts. In theory. Even a good editor can do little except badger the laggards with circulars and phone calls; and even a good contributor such as Campbell is forced to realize that writing chapters comes after writing papers, grant applications and lectures, and that there will be some delay: he asks for an extension of a couple of months, is given it and gets his chapter in within the year. In any one book most chapters trickle in over a period of a few months, giving the good editor time to send back for rewriting, rewrite himself, cut, argue about nomenclature, argue about facts, query the references and demand replacement illustrations — edit, in other words.

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If Parsons receives the complete typescript earlier than 18 months from contracts going out she will be lucky. There are always some dropouts and the chapter that is twice the length asked for, typed single-spaced, without margins and with illegible handwritten corrections, figures that no drawing office can handle and already screened half-tones. Campbell should be prepared for delays and demand the chance to update his chapter; he should also know that it is those projects which go on through several updatings over seven years or more that can kill friendships.

When Campbell knows that the typescript has been delivered in its entirety to the publisher (editors are quick to write to say this), what sort of schedule can he then expect? Book publishing like house buying is a succession of delays: before Parsons passes the typescript to the copyeditor, until the copyeditor gets to work, while queries are sent out all over the world and answers come back, after the marked-up manuscript goes to the typesetter, and so on through the proof stage (with revised proofs if needed) and on to printing and binding. Assuming reasonable efficiency, no holding back because of cash-flow problems, a prompt postal service and no industrial action in the printing industry, reckon a year from delivery of the complete typescript to publication.

Book publishing like house buying is a succession of delays.

During this period decisions have at various stages to be made within the publishing house. Before money is spent on outside suppliers (typesetters, printers etc.) Parsons has to go through the same sort of procedures as she did at contract stage but this time with a real typescript. Inevitably the extent (the length of the typescript) will be greater than bargained for however tough Edwards may have been. Typesetting and printing costs may not be much higher in real terms (production controllers in publishing houses have learnt many lessons in the past few years) but the projected print run will undoubtedly be shorter because of the poor sales record of similar titles over the same period. There will usually be two calculations to be made: first on estimates, the stage which corresponds to what the contracts call "acceptance"; and second on real costs (typesetter's and printer's bills) when the final price is set. Campbell and even Edwards will know little of the in-house agonizing. Quite frequently publishers fail to remember to tell the editor the selling price of the book.

At handover of the typescript, an unworldly academic editor will hope to be given a schedule for proofs. There won't be one. If all goes well the best hope is for proofs within six months but prior warning, let alone a schedule, is most unlikely. Production staff are surprised every year by the fact that proofs going out in the summer holiday and congress season don't get returned for months. Proofs always arrive at the most inconvenient time with an unrealistic three weeks turn-round demand, possibly reduced to a week by transatlantic postal delays. The usual system for an edited book is for contributors to return proofs to the editor, who collates them and returns a master copy to the publisher.

Suppose that the proofs are a mess. On the advice of the production department Parsons has opted for a cheap typesetter and for the money has got omissions aplenty, poor alignment of tables and an unhealthy crop of typographical errors. Campbell knows that he will be charged if the cost of making corrections is over 10 per cent of the original cost of the composition of his chapter. What can he do? He can certainly correct all mistakes that have been made by the typesetter — without fear of cost or complaint. Sometimes, however, such "mistakes" are the result of over-creative editing yet it is best to give in gracefully unless actual errors of substance have been introduced. When galley proofs were the rule authors used to rewrite in proof on the Proustian model (his galleys were famous) but the contributor of a chapter to a work of scholarship nowadays has no such chance. Each alteration means a series of actions by a highly paid operative; it can cost as much as £1 to add a comma.

Meanwhile Edwards has been contacted by the marketing department of the publisher. There is a publicity form to be filled in. The form will have asked optimistic questions about potential media coverage and the nature of the expected readership. The fact will be a badly typed regurgitation of his own summary of what the book is about, a list of places where review copies are to be sent (one hopes) and possibly requests for photocopies of the membership lists of relevant learned societies. He will not be asked about the jacket (there is unlikely to be one and the artistic design prepared by his wife may well have been lost) and there will certainly be no launching party. The best Edwards can do, poor soul, is cut down the claims (if he is a realist) and make sure that the names of all the contributors are spelled correctly.

For Campbell and his fellow contributors, the actual publication will come as an anti-climax. Edwards, being a keen editor, may have written to say that the book is "out" but his advance copy does not mean that the bookshops know about it, and it certainly does not mean that the contributors will have received their statutory free copies. Campbell, looking for reassurance, is unlikely to find it on the shelves either in the local bookshop, which received one copy on a standing order plan, or the library which bought the copy but has cataloguing delays. Publishers rarely advertise, except for a regular entry in a journal or two in which an individual title may receive much the same amount of ▶

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Keeping it clean

R.C. Lewontin

Evolution and the Theory of Games. By John Maynard Smith. Pp.218. Hbk ISBN 0-521-24673-3; pbk ISBN 0-521-28884-3. (Cambridge University Press: 1982.) Hbk £18.50, $34.50; pbk £6.50, $11.95.

POPULATION genetics is the automobile mechanics of evolutionary theory. Population genetic theory is a deductive structure that asks the question: Given the mechanical details of inheritance, given mutation rates, recombination rates, migration rates, patterns of mating, the Darwinian fitnesses of various genotypes, and the relation between genotype and phenotype, how will the composition of a population or species change in time? The job of the theoretical population geneticist is to set up a mathematical machine into which the various parameters can be dumped, to turn the crank, and to produce the kinetics of evolution.

In practice this process has a number of serious difficulties. For even mildly complicated models involving several genes or non-random mating or non-constant fitness, the mathematical problems can be virtually intractable. Moreover, we do not usually know the genetic basis for traits of interest, nor are we able to measure the parameters in nature to any close accuracy.

As a consequence, evolutionists have continually searched for ways to avoid the dirty details of the kinetic equations of population genetics and to make predictions about evolution by some easier route. One form of approach has been to try to find laws of maximization or optimization, much as classical physics used Fermat's Principle of Least Action or the minimization of potential energy, in place of the kinetic equations themselves. To this class belong what R.A. Fisher called, with characteristic modesty, the Fundamental Theorem of Natural Selection, and Wright's principle of the maximization of mean fitness. Unfortunately, these maximization principles turn out not to apply generally, and the only way to know when they do is to solve the kinetic equations that we were trying to avoid in the first place.

Another attempt to make evolutionary predictions on the cheap was my introduction in 1961 of the apparatus of game theory into evolutionary biology. I had hoped that by drawing analogies — a population to the player, and the population's genetic structure and parameter values to a strategy — the entire apparatus of game theory could be used to make evolutionary predictions. But this attempt foundered on precisely the same rock as optimization theory: there is no guarantee that the actual kinetic process of gene frequency change will carry an evolving population to one of the so-called "solutions" in the game theoretic problem. That is, evolutionary equilibria are not necessarily the same as game-theoretic equilibria.

As luck would have it, I was visiting professor at Sussex University in 1971 when John Maynard Smith first began to reintroduce the metaphor of the game into evolutionary biology. At first, I was sceptical. The very words "Evolutionarily Stable Strategy" seemed to reek of the discredited attempts to avoid the messy realities of genetics. Only slowly did I come to understand that Maynard Smith was engaged in a totally different project, a project that is now lucidly explained in his new book *Evolution and the Theory of Games.* The name of the book unfortunately perpetuates a misunderstanding, for, with the exception of the metaphorical use of a few terms like "strategy" and "pay-off", Maynard Smith's theory makes no use whatever of game theoretical apparatus, but rather lies totally within the standard kinetic structure of population genetics. That is the secret of its success.

A central set of parameters for making predictions in population genetics is the set of reproductive fitnesses of the various genotypes in the population. In much of population genetic theory these fitnesses are assumed to be fixed constants characteristic of each genotype. Some progress has been made in building models that include simple dependence of fitness on population density, on the frequency of the genotype in the population, or on a fluctuating environment with known statistical properties. None of these models, however, deals with two important properties of organisms: first, any genotype may have a variety of phenotypes each with a different fitness; second, which phenotype is displayed and what its fitness is may depend upon the phenotype manifested by other individuals in the population.

This kind of doubly contingent fitness determination may make the average fitness manifested by each genotype rather difficult to compute, and so evolutionary prediction becomes very problematic. Animal behaviour in particular is full of such problems because an animal may sometimes act in one way and sometimes in another, while the fitness value of these actions will depend upon what other animals are doing. There is, then, a continuum of possible phenotypes, say from always being aggressive, through sometimes being aggressive, to always being passive, and every phenotype has a frequency-dependent fitness. And we have not even mentioned genotypes yet — not a very encouraging prospect for a theoretical population geneticist.

Maynard Smith's contribution has been to cut through this thicket by asking a somewhat more restricted question, the answer to which can be very powerful. He has developed a set of algorithms to find which of the patterns, when it is very common in the population, has a higher

fitness than any other pattern that enters the population as a rare variant. The implication is that once this phenotype is established in the population, no other can displace it. Such a "King of the Mountain" phenotype is called an Evolutionarily Stable Strategy, or ESS. It turns out that such patterns exist for a wide variety of situations in animal conflict, in learned behaviour, in resource allocation and in a variety of other very complex frequency-dependent fitness problems. The real issue is whether these patterns live up to their name. Are they really evolutionarily stable?

It is at this point that the messy details of genetics reassert themselves. If the ESS pattern can be achieved by a homozygote or if reproduction is asexual, then the answer is yes. On the other hand, if the ESS pattern can only be produced by a heterozygote, then clearly it is not achievable in pure form in a sexually reproducing population. Moreover, some populations are genetically polymorphic. Can the ESS be stably created by a mixture of genotypes obeying the rules of segregation and recombination? The answer is "Sometimes yes and sometimes no". Thus, the phenotype that Maynard Smith calls ESS might better be called the "Frequently Roughly Evolutionarily Stable Strategy". A fair amount of argument in *Evolution and the Theory of Games* is devoted to convincing us that the genetic basis of behavioural phenotypes is likely to make a FRESS into an ESS. So, for example, single locus overdominance is said, probably correctly, to be rare, and it is asserted that if many genes influence a character, it is probable that any intermediate phenotype can be produced by an appropriate mixed homozygote. It must be pointed out, however, that epistatic gene interaction may be very important and that linked genes will produce a pseudo-overdominance that decays only slowly, if at all.

Other criticisms have been made of ESS theory, but Maynard Smith discounts them as being beside the point. So, predictions of ESS depend upon the set of alternative "strategies" that are evaluated. But precisely the same is true of all kinetic predictions in population genetics, which are based entirely on the alternative genotypes actually present in the population. Indeed, ESS theory has the advantage over vague optimization theories that it makes very explicit what the set of alternatives are that are being considered. ESS theory is not easy to check against natural observations because both the parameters and the predictions are usually in the form of relative frequencies that have immense standard errors, unless sample sizes are very large. But, again, that is a problem for all of population genetics, so ESS predictions are no more unfalsifiable than anything else in evolutionary genetics. The criticisms are to the point, however, when ESS theory is used uncritically, and it must be admitted that the very words "Evolutionarily Stable Strategy" have a kind of puffery about them that invites the same sort of overgeneralization as the not-so-Fundamental Theorem of Natural Selection. After all, evolution has been going on for three billion years, and if some strategy is evolutionarily stable, why surely that must mean that it is here to stay. The letters ESS now rival DNA as the most fashionable acronym in biology, and we may expect that many vulgarizers both outside and within evolutionary biology will simply not appreciate the contingency of the method.

There is a deeper sense in which ESS theory shares a flaw with all attempts to make general explanations for evolutionary events. At any moment we will see characters that are common to all members of a species, and we will be tempted to ask "Why is the species like that?". To give the answer "Because that is its optimal phenotype" is to give a totally uncheckable answer because we cannot ever know what set of alternatives was available to the species during the course of its evolution to its present state. To say that the codfish lays millions of eggs in order to maximize the number of survivors is to forget that it *might* have become ovoviviparous and so have provided maternal care. The same problem exists for ESS theory. We simply cannot say that a species is at an ESS because we do not know what set of alternatives was available. That is, such theories have predictive power but not explanatory power, *post hoc*.

On balance, ESS theory, because it is so firmly rooted in the kinetics of population genetics, must be judged as one of the more fruitful directions in theoretical evolution. Like other attempts at making generalized rules, such as the Fundamental Theorem of Natural Selection, or the principle of maximization of mean fitness, it works when the details of the genetic system are appropriate, but not always. Thus, we are still not free to ignore the material particularities that underlie phenomena. When all is said and done, "God is in the details". □

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The man who was crunched by numbers

Richard L. Gregory

Charles Babbage: Pioneer of the Modern Computer. By Anthony Hyman. Pp.350. ISBN 0-19-858170-X. (Oxford University Press: 1982.) £12.50.

ANTHONY HYMAN'S biography of the inventor of programmable computers is no dry account of a premature technological innovation; and neither is it, as a previous biography, a shallow journalistic treatment of this truly remarkable man. Hyman has succeeded in bringing Babbage alive in the social context in which he played, as we may now appreciate, such a prominent part, and gives a clear account in just sufficient detail of Babbage's outstanding achievement, his Analytical Engine.

Charles Babbage (1791–1871) was born of a West Country banking family and became Lucasian Professor of Mathematics at the University of Cambridge (1823–1839). Elected to the Royal Society in 1816, he played a prominent if abrasive part in the scientific and technological life of his time; his influence extended to continental Europe, especially to Italy. As is well known, with advancing years he became a bitter and somewhat disappointed man; but this book shows the extraordinary range of his enthusiasms and his love of life. In retrospect he is far more to be envied than pitied, in spite of his famous defeats at the hands of Government, especially Peel's in the 1840s. Perhaps surprisingly, 20 years earlier Babbage was imaginatively supported by the Duke of Wellington when he was Prime Minister. As Hyman says (p.191):

> The underlying cause for this sad change was the developing Victorian academism. A wide gap was opening between the superior pure science of the Universities and inferior applied science, the bread and butter of the engineers. Babbage's engines fell through this gap into a century's oblivion.

Those engines were only understood by a few people, among them Byron's daughter Ada, Lady Lovelace. Most difficult to grasp was the notion that in its progress towards solutions such a machine could be affected by its own findings. Wheeled calculators were known from Pascal's machine of 1642, so concepts of representing numbers and carrying out arithmetical and logical operations were fairly familiar — and indeed have been from the prehistoric abacus — though these vitally important concepts have received remarkably little comment or appreciation from philosophers; or perhaps even more surprisingly, from mathematicians or astronomers, or financial people who might have benefited greatly.

Like Helmholtz's Unconscious Inference and Freud's Unconscious Wishes, perhaps Babbage's computing engines were an unwelcome threat to human dignity; and to social and moral responsibility, for consciousness then as it still does, seemed necessary for blame and praise. Babbage's own attitude to blame

and praise is convoluted. He rejected a knighthood, while also confessing to Ada that accolade and honours were particularly dear to him. He was justifiably upset that his Analytical Engine was excluded from the Great Exhibition of 1851; though he was no doubt delighted that his friend Charles Dickens based the HOW NOT TO DO IT OFFICE of *Little Dorrit* on Babbage's experience of Government's attitudes to those who have the wit to do new things.

Mary Evans

Charles Babbage, a man with a vision

Such contrasts run right through Babbage's life. He was so active in the society of the time that through Hyman's account one learns a great deal about this formative period of science and its relations with technology, as well as about Governments and how they dealt with novelty. Babbage was a man with a vision of the future industrial world as well as of computing; he was influential in the founding of several learned societies, as well as in the application of pure science in industry and commerce. He had an extremely active social life, too, being friendly with the great men of his time, and also appreciated the arts; on a splendid occasion when he was bored by a performance of Don Giovanni, he went behind the scenes and virtually invented modern theatre lighting. He invented the ophthalmoscope, and indeed everything he turned his hand to was affected by his touch. Babbage not only saw how the punch card system of Jacquard's loom could be used to control logical processes in a machine — and so how thought itself can be carried out by machinery — but, by almost superhuman effort, he developed precision engineering and mechanical drawing to a point where the general-purpose computer became very nearly if not quite a practical possibility. The full realization required the technology of electronics, but the basic concepts are there in beautifully fashioned cog wheels. (One is almost tempted to say cognitive wheels!). Babbage did as much as any man to create the machinery and the techniques — including engineering drawing — for precision mass production which has produced historically unimaginable wealth and knowledge.

All of this was achieved at the cost to himself not only of a large proportion of his personal fortune, but also in having to live with a painful gap between what he could see was needed and possible, and — for him — the stupidity and inertia of the society in which he lived. It is just this tension between his dream and our present reality that places Babbage as a man uniquely poised at the threshold of our age — leading surely to fully intelligent machines.

Anthony Hyman has succeeded in bringing the many themes of Babbage's experience and contributions together. His book is eclectic and thoughtful, as well as being both an inspiration and a warning to those of us who have faith in invention rather than received wisdom. □

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Science Museum, London

Babbage's Analytical Engine, which stands in the Science Museum in London. The machine was never completed. Babbage had thought out all of the principles on which modern computers function, but not until a century later, and the rise of electronics, could his ideas be fully realized.

Entombed in print

P.B. Medawar

A Biographical Dictionary of Scientists, 3rd Edn. Edited by Trevor I. Williams. Pp.674. ISBN 0-7136-2228-8. (Adam & Charles Black: 1982.) £15, $30.

THIS admirable book first appeared about 12 years ago and the call for a second and now for a third edition is evidence enough of its fulfilling a real need, for although it is good value it is not cheap. The innovations of the present edition are the addition of supplementary biographies of some 40 scientists who have qualified for admission — as one day we all shall — by dying, for all is in the past tense. Another valuable innovation is the inclusion of an index which under a variety of subject headings lists the names of the scientists specially connected with each one. Thus under the subject heading "Evolution" we are referred to von Baer, Darwin C, Darwin E, Dobzhansky, Haeckel, Hooker, Huxley, Lamarck, Malthus, Maupertuis, Nägeli, Naudin, Pringsheim N, Spencer, Vallisneri and Wallace — which is fair enough.

It was a claim of the first edition that it addressed general readers as well as the student of science but it really does not go very far to appease the former: the entries are baldly factual.

In this edition the grounds for inclusion of a name in the main body of the text remain as mysterious as ever (the task of deciding must have been extremely difficult). The omission of such a prominent figure as Darcy Wentworth Thompson can I suppose be excused on the grounds that he did not found a school and is not now thought of as a man who was scientifically very influential, though he was greatly admired. Clearly priority has always gone to practising scientists rather than to men who are not primarily known as or indeed known to be scientists, however great their influence on the general public, on fellow scientists or on literature or the arts.

Thus there is no place for Goethe though he did some work which he probably thought of as scientific (the "vertebral theory of the skull", for example) and his commitment to *Naturphilosophie* which exercised a wide influence — undoubtedly a harmful one. But I was sorry not to see H.G. Wells who, although he held only a fairly minor teaching post in zoology, was the author of a widely consulted zoological text and had a profound influence on the young through his brilliantly imaginative short stories which were greatly superior in all respects to those of Jules Verne. Francis Bacon rightly rates inclusion, however, because of his profound influence in throwing off the hegemony of Aristotelian thinking and propounding a philosophy of science which is recognizably congruent with modern thought even if it was in some ways pretty wrong headed. He was not

much of a scientist though, and one can understand why one of his biographers called him "a medieval philosopher haunted by a modern dream".

I did not spot any errors of fact but then, I was not looking for them, taking it quite for granted that a work of such great compass and of so many hands must surely contain a few mistakes, and being unwillingly to incur the deserved odium of picking them out and calling attention to them. In the outcome I rate this dictionary as a great success and a considerable feat of editorship and of publishing which will earn the continuing gratitude of all who have occasion to refer to it. □

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Along the lines of Einstein's thought

Arthur I. Miller

'Subtle is the Lord . . .': The Science and the Life of Albert Einstein. By Abraham Pais. Pp.552. ISBN 0-19-853907-X. (Oxford University Press: 1982.) £15, $25.

"SUBTLE is the Lord, but he is not malicious", remarked Einstein to a colleague one day in 1921. When queried further on this statement he replied that "Nature hides her secret because of her essential loftiness, but not by means of ruse". Einstein's life-long passion was to pluck from the cosmos the laws of nature ("her secret") that exist independently of the scientist as knower. And what a passion it was.

During the period from 1905 to 1916 Einstein set the foundations of twentieth century physics with two lines of research. One line dealt with invariance principles and culminated in the special (1905) and general (1915) theories of relativity. Then there were the statistical researches that resulted in a theory of Brownian motion (1905) and a route to the verification of atomic structure, the light quantum hypothesis (1905), the first quantum theory of the solid state (1907), the wave-particle duality for light (1909), and the A and B coefficients (1916) that would lead to the laser. Finally, in 1925 came one more major piece of creative work, namely his argument, independent of Louis de Broglie, for the wave-particle duality of matter.

Thematically Abraham Pais's "scientific biography" of Einstein is set out along those two lines of Einstein's research because "better than anyone before or after him he knew how to invent invariance principles, and make use of statistical fluctuations". After 1916 these two strands merged into Einstein's quest for the unified field theory, an episode in the history of science that Pais explores systematically for the first time.

Among Pais's informative biographical sections are those describing how Einstein's feats during the intensely creative period of 1905–1916 were accomplished in the face of events that would have distracted all but the most determined: he worked dutifully eight hours per day at the Patent Office in Berne (1902–1909); he moved between academic positions at the University of Zurich (1909–1911), the German University in Prague (1911–1912), back to his alma mater that had virtually disowned him in 1900, the ETH, Zurich (1912–1914), and then to the summit of the scientific world, the University of Berlin and the Kaiser Wilhelm Institute (1914–1933); he married Miliva Maric in 1903, by whom he had two sons before they separated in 1914; and he had to deal with health problems and the deprivations of war time. Little wonder that in December 1915 after completing the theory of general relativity Einstein wrote to the venerable H.A. Lorentz that he was "satisfied but rather worn out [*kaputt*]".

The episodes written with the most verve are those describing Einstein's tortuous path to the general theory of relativity and then his work towards the unified field theory. The combination of new material and infectious prose in these chapters conveys to the reader some of the excitement of doing science.

As an example consider Pais's description of Einstein's work towards the general theory of relativity. In 1913, owing to Einstein's hitherto unsuccessful research towards generalizing special relativity, Pais describes Einstein as one "who stands alone . . . possessing only a vision" of which he is "supremely confident". Let us fill in a bit. Since 1907 Einstein had been struggling to generalize special relativity to include accelerating reference systems and gravity. A key realization, Pais writes, was that he had to "start all over again". Pais argues cogently that while in Prague Einstein had done just that by using a curved space-time that required a mathematics with which he was not conversant. "Grossmann, you must help me or else I'll go crazy", he said to Marcel Grossmann his former classmate at the ETH upon rejoining him there as a colleague in 1912. There followed a joint paper with Grossmann who wrote the mathematical portion, polemics with the "fearsome" Max Abraham, and exchanges with Gustav Mie and Gunnar Nordström. These men offered alternative gravitational theories

couched in variations of a four-dimensional flat space-time the "belief in which amounts, I feel, to something like a superstition", wrote Einstein to Erwin Freundlich in early 1914. Pais concludes that "Only Lorentz had given him some encouragement". In 1913 the doyen of German physics, Max Planck, listened to Einstein describe his work towards a general relativity theory and then told him that even if he succeeded "no one will believe you".

The onset of war does not affect Einstein's energy; the war years "rank among the most productive of his career". Amidst the tumult about him and his unsuccessful research on gravitation, in early 1915 Einstein turned to experiment in work on magnetism with Wander Johannes de Haas.

Then, in November 1915, the breakthrough occurred for the gravitation problem. He returned to the requirement of the general covariance of the field equations that he had "abandoned only with a heavy heart" in 1912. This time Einstein knew he was on the right track, because, Pais writes, "one week before the general theory of relativity was completed [25 November 1915] Einstein obtained" the correct result for the advance of Mercury's perihelion. Pais continues:

> This discovery was, I believe, by far the strongest emotional experience in Einstein's scientific life, perhaps in all his life. Nature had spoken to him. He had to be right.

There followed Einstein's prediction for the bending of light near the Sun and its verification by Arthur Stanley Eddington's Solar eclipse expedition in 1919; then was "Einstein canonized".

After 1920 Einstein left the mainstream of scientific research to pursue his vision of a generalization of relativity that unified electromagnetism and gravitation. The generalization would be based on the themes of continuity and classical causality, themes that were antithetical to the emerging atomic physics of Niels Bohr, Max Born and Werner Heisenberg. Pais asserts that Einstein had always considered a quantum theory as "provisional". He traces this strand in Einstein's thinking in great detail starting from the 1905 light quantum paper, complete with an illuminating analysis of the Bohr–Einstein dialogue of 1927–1930. Unfortunately, Pais rejects abruptly the important philosophical–physical analysis in the Einstein–Rosen–Podolsky paper of 1935 by quoting one of Bohr's *et cathedra* pronouncements of complementarity. Pais's reason, as he states straightaway in his book, is that "I would say that at his best [Einstein] was not" a philosopher.

Pais realizes that Einstein's decision to pursue a unified field theory poses a profound question that is ripe for historical analysis:

> Why does this man, who contributed so incomparably much to the creation of modern physics, remain so attached to the nineteenth century view of causality?

In outline his reply is: Einstein's infatuation with unification is evident from the beginning, as seen in his reaction to momentarily promising results from a 1901 paper where he compared intermolecular and gravitational forces — "a wonderful feeling to recognize the unifying features of a complex of phenomena", wrote Einstein to Grossmann in 1901. Although Einstein quickly abandoned the 1901 results, Pais considers that "this wonderful feeling sustained him through life". Then there was success with unification in special relativity (electricity and magnetism) and general relativity (geometry and gravity). These results, continues Pais, led him on a "quest for harmony" (geometrization of gravity and electromagnetism) based on the continuity and classical causality inherent in the differential equations of a classical field theory.

Here, and at other key points, it would have been appropriate for Pais to have developed Einstein's philosophical analysis of physical theory. Analyses of Einstein's scientific research that take seriously contemporaneous philosophical currents reveal that he was indeed a philosopher–scientist. While some lines of his eclectic philosophical view remained constant, others were transformed by his scientific research. Some investigation into this dimension of Einstein, and into other issues that are not readily apparent from the primary scientific literature, would have lent more depth to Pais's reply to a key question in the history of ideas. Pais's choice to exclude philosophical issues and his sparse use of recent historical scholarship is most costly to his analysis of special relativity theory — for example, on Einstein's invention of the theory and the theory's interpretation by Lorentz, Planck and Henri Poincaré.

Pais's forte is in the domain that he set out for himself; namely, to blend expository accounts of Einstein's complex physics with his scientific correspondence. This is no mean feat. A particularly valuable part of the book is where Pais goes on to bring research on gravitation and unification, as currently interpreted, up to date.

Abraham Pais is a distinguished physicist in his own right; his scientific biography of Einstein has clearly been a labour of love. The level of the scientific presentation is high, but so are the rewards, and the book fills in one part of the mosaic that is Einstein, his life, his times and his science. Together with such general biographical works as P. Frank's *Einstein: His Life and Times* and B. Hoffmann's *Albert Einstein: Creator and Rebel*, Pais's examination of Einstein's scientific oeuvre will be an important acquisition for both physicists and historians and philosophers of science. □

From 'Subtle is the Lord . . .'

Einstein in March 1955, a month before his death.

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Myths of religion, myths of science

John Habgood

Abusing Science: The Case Against Creationism. By Philip Kitcher. Pp.213. ISBN 0-262-11085-7. (MIT Press: 1982.) $15, £10.50.

CREATIONISM, or "creation science", claims to provide a scientific alternative to orthodox evolutionary theory, in terms which bear a suspicious resemblance to a literal interpretation of the first chapters of Genesis. Its proponents are capable of stating their case with a display of scholarship which looks convincing to those who are not experts in biology, and their plea for tolerance, for a "balanced treatment" of creationism and evolution in school curricula, has made some disturbing headway in the United States. In alliance with the Moral Majority, creationists are now seen as a formidable threat by educationalists, scientists and non-fundamentalist theologians. As Philip Kitcher puts it ". . . although the Creationist campaign is advertised as an assault on evolutionary theory, it really constitutes an attack on the whole of science".

Kitcher's book is an impressive and absorbing counter-attack, which subjects all the main creationist arguments to detailed criticism. Those arguments are many and various, ranging from the accusation that evolutionary theory is unfalsifiable, to determined efforts to falsify it by appealing to palaeontology, thermodynamics, statistics, geology, uncertainties about transitional forms in the evolution of species — in fact any argument, however ancient or discredited, which seems to throw doubt on the evolutionist case. Kitcher relentlessly exposes the confusions, misunderstandings and misrepresentations on which the creationist platform is based, and provides, as he says, "a manual of intellectual self-defense" for those who find themselves under pressure. It is a splendid piece of demolition, but the effect is not merely negative. A valuable spin-off is his clear demonstration of the difference between science and pseudo-science, and this is why the book should be of interest even to those who stand outside the immediate controversies.

Why do intelligent people indulge in such silliness as creationism? The motive, despite tactical disclaimers to the contrary, is plainly a religious one, and it is a sad fact that for many Christians, particularly in the United States, Biblical literalism is still the rock on which faith is built. Kitcher attempts to meet this problem in a final chapter by a sympathetic treatment of religious faith, but the account is sketchy and this is the weakest part of the book.

The fact that creationists link their attacks on evolution with general denunciation of atheism, communism, nazism, behaviourism, racism and every other bugbear of liberal and illiberal thinking, is an extreme expression of a much more widely felt malaise. The fear that science can be dehumanizing is not confined to cranks. Sensitive scientists have always known that they are engaged in a human and fallible activity which entails adherence to some basic human values. But popularly, and in the mouths of some of its exponents, it does not come across like that. A hard, debunking kind of science which cuts human beings and their aspirations down to size too brutally, provokes its own reaction. The cure for creation science and its like, therefore, lies deeper than the arguments mustered here, important though they are. Scientists may claim that it is not their business to take care of human self-esteem, and that in any case the theory of evolution, by showing us where we have come from, does not reduce our worth. This is fair comment. But it is also true that the refusal to face questions about human worth in the name of scientific objectivity can easily give the impression that such questions do not matter.

It is not enough, therefore, to expose religious obfuscation. The appeal of movements such as creationism will not be undermined until ordinary people feel in their bones that religious and scientific experience can be properly integrated. *Abusing Science* is a useful first step in clearing away some of the rubbish.

I have, however, one small niggle about the book. Kitcher begins by referring to the famous encounter between Bishop Wilberforce and T.H. Huxley at the British Association in 1860. This has become part of the mythology of science, but it is a gross libel on Wilberforce to classify him as a nineteenth-century equivalent of today's creationists. In his day there were good scientific arguments to be levelled against Darwinism, and the fact that he used ridicule unwisely does not affect the genuineness of the issues at stake. The point about creationism is that the arguments are now known to be worthless, so that twentieth-century religious polemic in pseudo-scientific dress belongs to quite a different moral dimension. Scientific myths, like religious ones, sometimes need to be allowed to die. □

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Curing nonsense

Stuart Sutherland

Pluto's Republic. By Peter Medawar. Pp.351. ISBN 0-19-217726-5. (Oxford University Press: 1982.) £12.50, $25.

THE intelligensia's attitude to science is becoming increasingly ambivalent. While resenting their dependence on some of its products and fearing the dangers of others, they rejoice at the least hint that there may be something wrong with a well-established scientific theory. This propensity is encouraged by the wilful ignorance of newspaper correspondents, as witness the recent nonsensical attempt to treat cladistics and punctuation as disproofs of Darwinian theory.

Yet despite this aversion to science, nothing is more calculated to lend credence to shoddy thinking than the claim, however far fetched, that it is scientific. Psychoanalysts repeatedly make this assertion about their subject and many universities to their shame have attempted to conceal the vacuity of their social studies courses by calling them "social sciences". In a recent tome on English literature, Mr Leon Edel even claims to be practising "scientific" literary criticism. It is a pity that those in arts subjects have so little faith in what they are doing that they feel obliged to dignify it with the name of science. The confusion of the intelligensia is further illustrated by the fact that they increasingly take refuge in the gibberish of such authors as Lacan, Laing and Levi-Strauss and in such creeds as ethnomethodology, where nothing is true but thinking makes it so.

Unfortunately, few scientists have the time or the patience to expose the nonsense produced by what Sir Peter Medawar calls "the intellectual underworld", and we should be grateful to him for appointing himself a defender of reason and for untoppling some of its assailants. It is true that most of the denizens of *Pluto's Republic* are more fools than knaves, but they are none the less harmful for that.

The book incorporates two of Sir Peter's previous collections of essays, and also includes a dozen or so essays not previously published in book form. It is pleasant to reread such classic exercises in debunking as his reviews of Teilhard de Chardin's *The Phenomenon of Man* and Arthur Koestler's *The Act of Creation*, even though both books are now largely forgotten. Nothing is more difficult than exposing nonsense since one cannot refute an argument where none exists. For the most part Sir Peter exposes the authors he is attacking by direct quotation, though he cannot resist adding his own ironical asides. For example, having quoted from Teilhard "Consciousness was now leaping up and boiling in a space of super-sensory relationships and representations", he adds "The analogy . . . is with the vaporisation of water when it is brought to boiling point, and the image of hot vapour remains when all else is forgotten". He is also good at showing how a thesis leads to untenable conclusions. He points out, for instance, that since according to Koestler laughter is produced by the "bisociation" of two incompatible "matrices" of thought, the disproof of a scientific theory should produce gales of laughter. As Sir Peter remarks, "We are not amused". Irony fails him in the face of the fatuity of a passage from one of R.D. Laing's associates, David Cooper: "Curing usually implies the chemical treatment of raw materials so that they may taste better, be more useful, or last longer. Curing is essentially a medical perversion". Sir Peter's recurrent assaults on psychoanalysis are, however, more amusing than original.

His onslaughts are interspersed with the Johnsonian thump of common sense. He remarks, for example, of dreams that they may merely be noise conveying no information whatsoever, and of Levi-Strauss's insistence on the scientific validity of the Siberian myth that the touch of a woodpecker's beak will cure tooth ache: "Would not someone actually suffering from tooth ache incline towards a more pragmatic style of belief?". His common sense is also illustrated in a recent essay demonstrating that economists can never make accurate predictions: among other obstacles is the fact that the predictions may themselves influence the state of the economy. And he presents a sensible defence of genetic counselling: "Scientific evidence bears on decisions which are not of course themselves scientific".

He also resembles Dr Johnson in his capacity to write with an air of authority even — as occasionally happens — on topics he seems to know little about. For example, he asserts that intelligence cannot be measured by a single number and that there cannot be such a thing as a "culture free" test of intelligence, but he makes no reference to the extensive research on both topics.

About a third of his new book is devoted to the philosophy of science. Although these essays are a little repetitious, Sir Peter provides a clearly stated summary of Karl Popper's position, to which he has added with considerable historical erudition an account of the work of some of those, such as Whewell, who anticipated Popper many years ago. One cannot help wishing that Sir Peter had taken up his sword to destroy some of Popper's apostates such as Feyerabend, whose confusions are as much in need of exposing as those of Teilhard or Koestler.

Although not everything Sir Peter has to say is original, it is always well said. It is a truism that among academics scientists write better than those in arts subjects, but he writes exceptionally well even for a scientist. He is never short of an apt quotation, a nicely chosen simile ("They accepted the contemporary and far from adequate form of Darwinism in much the same way that nicely brought up people accept their religion, that is, in a manner that contrives to be both tenacious and perfunctory"), or a witticism ("Haldane was totally lacking in worldly sense, a sulky innocent, a whole-hearted believer in *Them* — the agents of that hidden conspiracy against ordinary decent people, the authorities who withheld the grants he had never asked for and who broke the promises they had never made").

Ben Schwartz

Sir Peter Medawar — the Johnsonian thump of common sense.

Sir Peter has invented a literary genre of his own, which might be called *belles-sciences*, though he is perhaps at his best when he is exposing nonsense, of which there is still an abundance. If he thought *The Act of Creation* boring, let him try reading Lacan. □

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Nuts about PK

Martin Gardner

Psychokinesis: A Study of Paranormal Forces Through the Ages. By John L. Randall. Pp.256. ISBN 0-285-62540-3. (Souvenir Press: 1982.) £8.95.

MANY worthless books by writers who call themselves parapsychologists have been published in recent years, but *Psychokinesis* by John Randall, a biology teacher at Coventry School, tops them all. It is not just that he rakes over stale ground but that his book is hopelessly out of date.

Consider, for example, his enthusiastic endorsement of the psychokinetic (PK) powers of Uri Geller, the Israeli magician turned flim-flam artist. Randall makes much of John Taylor's high praise of Geller, and of the spoon-bending children featured in Taylor's book *Superminds* (Macmillan, 1973). He never informs his readers that in 1978 Taylor executed a 180-degree turn and denounced all psychic metal bending as fraud. Taylor even wrote a book about his disenchantment: *Science and the Supernatural,* published by Dutton in 1980.

Consider the pages in which Randall rhapsodizes over Geller's alleged alteration of the chemical structure of a piece of nitinol wire, giving it a new "memory" which experts could not remove. That is totally false. Eldon Byrd was in error when he made this sensational claim, reporting on his nitinol tests with Geller in Charles Panati's now discredited anthology, *The Geller Papers* (Houghton Mifflin, 1976). Experts at the Lawrence Livermore Laboratory, in California, removed the wire's memory easily. This was carefully detailed in my paper, "Geller, Gulls and Nitinol" (1977) (reprinted in *Science: Good, Bad and Bogus*), but either Randall never read it or, what is worse, read it but did not want to mention it.

Consider too the section in which Randall extolls the "thoughtography" of Ted Serios, a Chicago bellhop, who for a short time was apparently able to project onto Polaroid film his memory of photographs he had seen earlier in magazines. No one, says Randall, ever found evidence of trickery. Nonsense. In 1967 Charles Reynolds and David Eisendrath published in *Popular Photography* a complete exposé of how Serios performed his whimsical trick. Ted has been unable to replicate it since, although magician James Randi demonstrates it regularly and with more skill. Did Randall know of this exposé? If not, his research was amateurish. If he did, his failure to discuss it is reprehensible.

The sad fact is that Randall buys almost everything on the psi scene no matter how flimsy the evidence. He believes the PK is behind the spirit rappings and the table liftings produced by great mediums of the

past. Many pages describe the levitations of human bodies from early Christian saints to meditating yogis; Randall suspects that PK helped Nijinsky make his high leaps on the ballet stage! He is persuaded that PK can hurl heavy objects across a room, move the hands of a clock and cause light objects, such as matches and pill boxes, to "walk" across a table.

A postscript summarizes the 1981 "minilab" phenomena recently disclosed by W.E. Cox, a former associate of J.B. Rhine and a confirmed Gellerite. Leather rings link and unlink. A pencil rises in the air and writes on a notepad. Randall assures us that Cox captured all these wonders on film, but he doesn't reveal that the films are peppered with time breaks, and that Cox's efforts to prevent fraud were so minimal that when he presented his stop-action films at a parapsychology conclave last year in Syracuse, New York, they were greeted with derision.

Randall even thinks PK is behind an old parlour trick in which four people use extended little fingers to hoist a heavy person from a chair. Samuel Pepys describes it in his *Diary* as a pastime of French schoolgirls. Geller does this stunt often in his stage act, using a fat man from the audience and three volunteers to assist him in the lifting. (By including himself among the lifters Geller can make sure the lift fails on the first attempt when PK forces are not invoked.) Hilarious photographs of Colin Wilson being "levitated" in this manner are in his preposterous book, *The Geller Phenomenon* (Aldus Books, 1976). "As far as I know", writes Randall, "no physicist has ever given a 'normal' explanation of this phenomenon".

What is there to explain? The first lift is uncoordinated. When it is repeated, after a preliminary ritual of some sort, the four lifts are synchronized, the weight is thereby evenly distributed four ways, everybody tries harder and up goes the fat man.

Randall believes everything. Well, not quite. He is surprisingly sceptical about what parapsychologists call "animal psi". When Helmut Schmidt's cockroaches seemed to use their PK to bias an electronic randomizer, Randall suspects it was not cockroach PK that did it, but PK from Schmidt or one of his assistants. Randall himself has pubished papers on how an experimenter's PK can influence animal life; in one case it modified the paths taken by crawling wood lice, in another test, the direction in which gerbils jumped. He was unable to replicate the positive results of a 1952 experiment by Nigel Richmond in which Richmond's PK influenced the paths of swimming paramecia.

To explain PK Randall dredges up an old theory, popular with Spiritualists a hundred years ago, that psychics somehow have the power to shift matter in and out of hyperspace. This was the opinion of a German physicist Johann Zöllner, to whom Randall devotes an entire chapter. Zöllner used the theory to account for the sensational results of his experiments with the famous American slate-writing medium, Henry Slade. His book about Slade, *Transcendental Physics* (English translation, 1880), is almost as funny as Jule Eisenbud's *The World of Ted Serios* (Morrow, 1967). Randall thinks I was unfair to Zöllner when I once called him "a remarkably stupid fellow". On the contrary, Randall is convinced that Zöllner was an astute investigator of Slade's genuine powers.

Randall's bibliography of 193 references is notable only for its omissions. Where, for example, is Edward Girden's classic paper, "A Review of Psychokinesis" (*Psychological Bulletin* **59**, 353–388; 1962)? In view of Randall's claim that magicians have no idea how Geller does his tiresome tricks, why does he neglect to list two books on sale in magic shops, *Confessions of a Psychic* (Karl Fulves, 1975) and *Further Confessions of a Psychic* (Karl Fulves, 1980), both by Uriah Fuller, which give Geller's techniques in full detail?

Uninformed readers may find Randall's book persuasive, but most professional parapsychologists will be as embarrassed by it as they are by the scribblings of such irresponsible journalists of the occult as Colin Wilson, Lyall Watson and D. Scott Rogo. *Psychokinesis* is only the latest, but surely not the last, of a seemingly endless line of lurid books about the paranormal, hacked out by gullible believers who are incapable of distinguishing competent investigations from shabby research and anecdotal poppycock. □

Martin Gardner is a science writer, formerly with Scientific American *and now a regular contributor to* Discover. *His most recent books are* Science: Good, Bad and Bogus *(Prometheus, 1981) and* Aha, Gotcha! *(W.H. Freeman, 1982).*

Neotony: is the child father to the man?

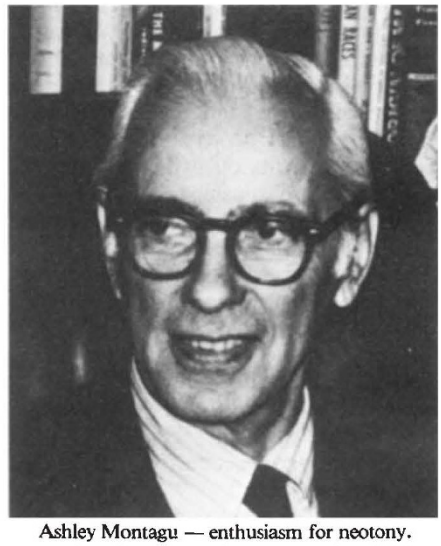
P.E. Bryant

Growing Young. By Ashley Montagu. Pp.306. ISBN 0-07-042841-7. (McGraw-Hill: 1981.) $12.95.

IT IS easy to detect a certain general ambivalence about the significance of childhood. On the one hand children seem to have some admirable and quite distinctive talents: they learn, for example, some very complicated things such as language (and sometimes two or more languages) with astonishing ease and they impress us with their quickness and their inventiveness. But on the other hand child psychologists keep on telling us that children lack many essential and therefore desirable qualities, and indeed these professionals devote much energy and ingenuity to the goal of hurrying on children's "development" and making them more like adults as quickly as possible.

Several people seem to manage to take both points of view at the same time even though the two obviously contradict one another. Ashley Montagu is not one of these people. His book is devoted unambiguously and enthusiastically to the proposition that childish qualities are not only desirable: they are, he thinks, precisely what distinguishes us from other species.

There are two parts to his thesis, the first anatomical and the second behavioural. The first part is the more familiar of the two and it also has more empirical support than the second. Neotony, a theory originally put forward by Bolk and lately extended in a well-known book by Stephen Gould (*Ontogeny and Phylogeny*; Harvard University Press, 1977), is Ashley Montagu's starting point. He takes up the argument that adult human beings preserve more of the physical features which characterize their childhood than do the adults of other species. Moreover, he argues, childhood takes up a larger proportion of the life span of humans than it does of other species. This, as far as the author is concerned, is not just a quantitative difference. He dwells on the evidence that human babies are born at an early



Ashley Montagu — enthusiasm for neotony.

gestational stage, and suggests that this is a significant factor in the quality of human social relationships. His argument is that much of family life is influenced by the need to protect and nurture helpless human infants.

Having made this case persuasively and clearly the author then tries as well to make a direct connection with behaviour, and

here he is notably less successful. His theory is that neotony is not just a matter of anatomy; human behaviour too, he claims, is strikingly "neotonous". The author attributes man's evolutionary success to the retention of traits which are characteristic of human childhood. These so-called "neotonous traits" include curiosity, creativity and sensitivity. Ashley Montagu's idea is that these are childish things which adults, and particularly those who are successful or at least admirable, retain through their lives.

There are several things wrong with this second part of the book. One is simply that the author rarely produces anything like convincing evidence for his thesis. There never has been any evidence at all that children are more creative than adults, for the very good reason that we have never had any good measures of creativity. The author's discussion of what pathetically unconvincing measures there are is strikingly uncritical and not even up to date. But his case is even less compelling when he talks about sensitivity, or flexibility or optimism (all of them put forward as neotonous traits), for here he can produce no evidence at all. Too often he is reduced either to stating his own unsupported impressions or to quoting the equally unsupported opinions of other theorists.

Another problem is that Ashley Montagu's (always attractive) enthusiasm for his neotonous case is so strong that he seems unable to notice that his thesis is, as far as several other people's theories are concerned, an antithesis. He gaily quotes Piaget, and yet apparently fails to notice that Piaget spent his whole working life trying to prove the startling intellectual inadequacies of childhood. The contrast between Montagu's optimism and Piaget's pessimism about childish behaviour is stark indeed: I cannot see how they could both be right. But there is no reason why Montagu should be worried by this, since there is now plenty of evidence that Piaget's gloomy stance was, to say the least, exaggerated.

This leads us to another difficulty about the book. Not only does the author skirt round possible arguments against his thesis; he also ignores a great deal of evidence which could be used to support it. The last ten years or so have seen a series of extraordinarily striking experiments which apparently demonstrate that even very young children have intellectual capacities which had been previously thought to come much later on in life. Since this research seems to show how sophisticated and effective even very young children are, it really is right up Ashley Montagu's street. Yet he all but ignores it. So we are left with a book which may indeed be right, but right often for reasons which the author himself leaves out. □

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In between ethology and neurophysiology

R.M. Seyfarth

The Human Primate. By Richard Passingham. Pp.390. Hbk ISBN 0-7167-1356-X; pbk ISBN 0-7167-1357-8. (W.H. Freeman: 1982.) Hbk $19.95, £14.95; pbk $10.95, £7.50.

A GOOD science book, one is told, does not just organize and summarize results: it presents a novel synthesis of previous work, offers new hypotheses and points the way to new areas of investigation. This is a difficult task in any field, but it promises to be particularly frustrating when the chosen topics are evolution, the brain, intelligence, and language in human and non-human primates. Not only is this research scientifically complex, but it also has uncomfortable Cartesian overtones, since in the long run it aims to establish a boundary between human beings and other animals. Moreover, there's Catch-22. The more we find human features in the brain or in the communication of non-human primates, the more these animals become ideal models for research on human pathologies. At the same time, increasing evidence of similarities between human beings, apes and monkeys raises serious ethical and scientific questions about the conduct of such laboratory research. The ethical questions are being debated — for example by Marian Dawkins in *Animal Suffering: The Science of Animal Welfare* (Chapman & Hall, 1980). Now Richard Passingham raises some of the scientific issues that are equally worthy of consideration.

How should we characterize a brain that is so much like our own and yet clearly different? Where do the intellectual talents of monkeys and apes end and our own unique abilities begin? Is it wise, given the extraordinary anatomical similarities between ourselves and other primates, to assume a quantal difference between our language and their communication? In addressing these issues Passingham sets himself a considerable task, and it is a testament to his efforts that he partially succeeds.

In my view, the least interesting chapters are those on non-human primate relatives, technology, and culture, in which the author simply reviews areas that are traditionally covered in most biological anthropology textbooks. There are other chapters, however, where the material itself is not new but where the author juxtaposes studies that have not previously been treated together. In Chapter 5 Passingham nicely connects laboratory studies of animal intelligence (usually found in psychology textbooks) with field research on the same species (usually found elsewhere). Similarly, in Chapter 6 he tries (with less success) to link the laboratory "ape language" projects with research on the communication of primates in their natural habitats. Although such field and laboratory studies seem obviously related intellectually, *The Human Primate* is, surprisingly, one of the first books to treat them together.

This said, novel syntheses can be frustrating because they often reveal more about biases and failures than about insights and successes. Take intelligence: here Passingham reviews the correlation between performance on a variety of psychological tests and at least one index of brain development. To no one's surprise, monkeys and apes score better than other mammals. Insufficient emphasis, however, is placed on the fact that as yet we have no test that reliably distinguishes between the skills of apes and of Old World monkeys. Somehow we *know* that chimps are smarter than macaques, but thus far comparative psychologists have been unable to describe what this actually means. Even when species differences in test performance are found, no successful effort has been made to determine the relation between such scores and the animals' natural social behaviour. For instance while Clutton-Brock and Harvey have shown that primate species with larger brains have larger home ranges, to those interested in how intelligence works and has evolved this remains nothing more than a tantalizing correlation.

Similarly, in other sections Passingham synthesizes two studies on one page, only to adopt a page later the same narrow perspective that has kept them apart for so long. In comparing the "ape language" studies with research on the natural signals of chimps, he correctly notes that evolution is unlikely to have supplied an animal with communicative skills for which it has no need. Thus we should expect, if only as a working hypothesis, to find that free-ranging chimps or other primates are capable of using sounds or other signs to represent objects in the world around them. Passingham concludes too readily that there is no such evidence. The fact is: no one has really looked. For while the ape language projects have begun on the assumption that their subjects are capable of at least rudimentary semantics, observers of the same or related species in the wild have generally not even considered this as a possibility. Given such biases, it is not surprising that few parallels have been found between primates' abilities in the laboratory and their natural communication in the wild. But it would be premature to conclude that no such continuities exist when, until recently, they haven't even been considered.

The Human Primate makes its most important contribution, and has the most to say about the future conduct of research on non-human primates, where it addresses studies of brain mechanisms,

language and communication. Consider, for example, the search for neural lateralization in non-human primates, or for non-human primate analogues of Broca's and Wernicke's areas in the human brain. Here Passingham's lucid review makes one aware that most research thus far has been a curious combination of highly sophisticated neurophysiology and behavioural assays that are crude and primitive by comparison. When the subjects are human beings, every effort is made to see that they live as normal lives as possible, and that they are tested with meaningful stimuli like words, pictures and other conspecifics. In contrast monkey subjects are often kept in isolation cages and tested with tones, hisses and human speech. Under these conditions is it any wonder that parallels in brain function are often hard to find? Passingham's review offers a challenge for future research, because it shows clearly that good neurophysiology requires equally good ethology. In order to understand the monkey's brain we must also understand — and respect — the social environment in which it has evolved. □

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Race is everything

W.F. Bynum

The Idea of Race in Science: Great Britain 1800–1960. By Nancy Stepan. Pp.230. UK ISBN 0-333-28856-4; US ISBN 0-208-01972-3. (Macmillan Press, London/ Archon (Shoe String Press), Hamden, Connecticut: 1982.) £20, $27.50.

IN LIBERAL circles nowadays, "race", if not exactly a dirty word, is at least one which can be vaguely embarrassing. Modern anthropology stresses social forms rather than biological natures as central to the differences between "us" and "them". This was not always the case. As the Scottish anatomist Robert Knox wrote in 1850, "Race is everything: literature, science, art — in a word, civilization, depends on it". To be sure, Knox was a little bit suspect even in his own time, since his religious views were heterodox and he had been the prime recipient of the merchandise purveyed by Burke and Hare. But Knox's racial theories were part of the nineteenth-century fascination with the subject, and not simply as it related to the variations between Hottentots and Englishmen. Anthropology could be studied closer to home: in the differences between the Saxon, who built his detached home in splendid, self-reliant isolation, and the Celt, who swarmed in the slums of the teeming Victorian cities. Norman yokes and Aryan races were part of the substance of nineteenth-century science.

The title of Nancy Stepan's new book is significant: *Race in Science.* Concentrating primarily on Britain, she traces the rise of the scientific idea of race in the eighteenth century, through its burgeoning in the next century, to its decline in our own. The peripheries are rather fuzzy, for there was a preoccupation with the nature of human differences long before the Enlightenment, and the modern legacy of old-fashioned racial studies (what Stepan calls the "new science of human diversity") is not without its historical reverberations. Not ten years ago Britain's oldest university press published a massive book by a Fellow of its oldest scientific society entitled simply *Race.* Nevertheless, Stepan insists, the

> tradition of scientific racism which had lasted from the early nineteenth century until the end of the Second World War if not later, has at long last been laid almost to rest.

The phrase "scientific racism" raises issues which, as we shall see, Stepan is conscious of but does not resolve. But what she does, soberly and cogently, is to describe the idea of race in Britain during the past two centuries. Her book is based on an intelligent reading of the relevant primary and secondary literature, and the result is a thoughtful, well-written synthesis. Three principal themes successively dominated debates on race during the period Stepan covers: the original unity or diversity of mankind; man's evolutionary origins; and the relative eugenic worth of the various human races.

Traditional theological and scientific orthodoxy asserted that all human beings are descended from a single pair, created by God and placed in the Garden of Eden. In the nineteenth century, this monogenist view was most authoritatively developed by the Bristol physician and ethnologist James Cowles Prichard (1786–1848). In a series of erudite books, culminating in the five-volume, third edition of his *Researches into the Physical History of Mankind* (1836–1847), Prichard marshalled the anatomical, physiological and linguistic evidence favouring the original unity of man. His Christian vision inspired many of the founders of the Ethnological Society of London (1843), but by the time of Prichard's death a more secular, continental-inspired polygenism (as represented by Knox) was rapidly gaining ascendency in British circles. This mid-century polygenism was rooted in the notion of the biological *type* and stressed the permanency of inheritance at the expense of the more fluid environmentalism of earlier naturalists.

After 1859, of course, Darwinian evolution and the contemporary discovery of man's antiquity gave a broader framework for theories of the origin of human beings and human races. Curiously

enough, as Stepan correctly insists (echoing a point persuasively argued by the historian of anthropology George W. Stocking, Jr), late nineteenth-century debates were often couched in roughly the same terms as pre-Darwinian ones. Polygenism persisted long after evolutionary biology might have been expected to have made its traditional formulation irrelevant. Part of the intellectual reason is that typological thinking can be neater and more satisfying than the population or statistical thinking inherent in Darwinian definitions of species and in the Darwinian vision of nature.

Statistics, however, was also central to eugenics, named and inspired by Darwin's cousin Francis Galton. In Britain, eugenicists were generally more concerned with social class than with race, although Stepan deftly picks her way through the eugenics movement, focusing on the all-too-easy transition from class to race. Both eugenics and racial biology in Britain were profoundly affected by the revulsion to Nazi ideas and practices, and the establishment of the neo-Darwinian synthesis by Ronald Fisher, J.B.S. Haldane and Theodosius Dobzhansky gave new sophistication to population genetics at the expense of the typological approach. Since the Second World War, the main arena for these debates has been the psychological laboratory rather than the anthropological field station, as the nature–nurture polarity has revolved primarily around intelligence. In the United States, which at least until the recent past has had a more turbulent racial history than Britain, Arthur Jensen's researches have racial overtones; in Britain, Cyril Burt's data were aimed at showing class differences in intelligence.

Stepan discusses all these issues and more in a book which has many virtues. What she does not do, however, is explore analytically the implications of one of her explicit working assumptions:

> that though the connection between racism and science is inescapable, the story of scientific racism is not merely a story of "pseudoscience". Bad science, perhaps, but not pseudoscience [p.xvi].

Her work hints at, but never comes to grips with, the relation between "science" and "prejudice", and between "good" science and "bad" science; with the nature of scientific knowledge; or with those wider issues of class, sex and empire, fact and value, which are central to that telling phrase, *scientific racism*. To write such a book would be vastly ambitious, but it should be possible even within Stepan's own liberal historiographical framework. In the meantime, her present book deserves to be read and pondered by anyone interested in the human sciences or their history. □

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Theoretical archaeology in Melos

Peter Warren

An Island Polity: The Archaeology of Exploitation in Melos. Edited by Colin Renfrew and Malcolm Wagstaff. Pp.375. ISBN 0-521-23785-8. (Cambridge University Press: 1982.) £35, $65.

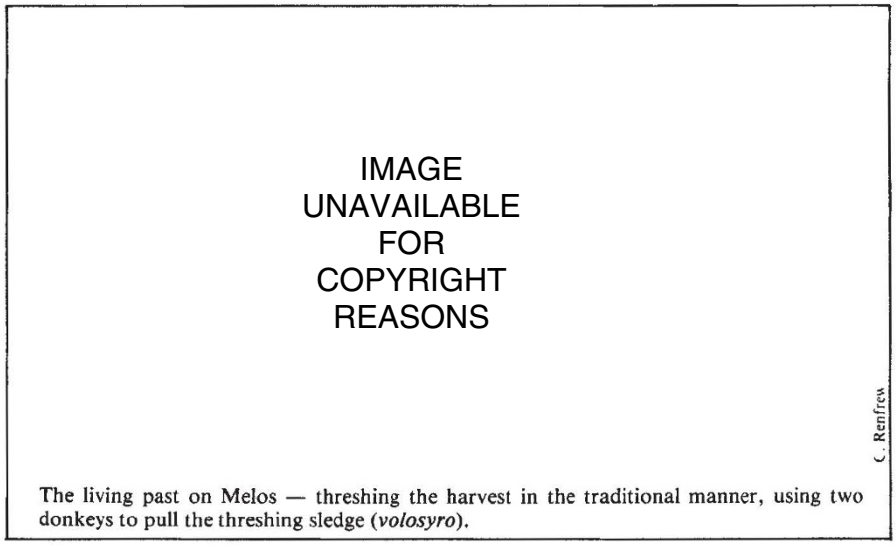
FROM 1974 to 1977 the Greek island of Melos was the subject of a multi-disciplinary environmental survey, the fieldwork being closely integrated with excavations at the great Bronze Age site of Phylakopi. An account of these excavations, directed for the British School at Athens by Professor Colin Renfrew, will be published separately. The present volume has as its basis the publication of the survey. There are 15 contributors, J. Cherry, C. Renfrew, B. Sparkes and M. Wagstaff writing major parts. Most of the team involved are or were at the University of Southampton and the volume must certainly rank as a major research achievement of that institution.

The project was concerned with the economic, social and political history of Melos, from the Neolithic period to the present, in relation to the island's whole environment as a resource for exploitation. Geology, geomorphological evolution, agriculture, animal husbandry and mineral exploitation are discussed in detail. The mineralogical studies include reference to millstone extraction, Hellenistic and Roman mining of sulphur and other minerals, and a detailed account by R. Torrence of the celebrated obsidian sources. Her statistical analysis of waste and worked pieces indicates unsystematic exploitation of the sources, with no evidence that Phylakopi controlled an Aegean market economy in obsidian.

Other contributions examine settlement distributions, forms and history (together with an excellent site gazetteer by Cherry), and the changing impact of autonomy and external control on the Melian populations. There is some fine topographical work by Cherry and Sparkes on the Classical town of Ancient Melos. Wagstaff has a most informative chapter on the relations of mediaeval Melos with the outside world, with invasion maps of Arab and other raiders worthy of a central European *Völkerwanderungatlas*. The same author and S. Augustson examine traditional land use in the island; their interesting data for plot distances and journey times from farms and homes suggest controlling factors specific to Melos, or at least in contrast to those for Crete and Messenia. It would have been helpful if the land-use questionnaires had been included in the book, though the authors do provide much statistical information on the calculation of labour intensity coefficients for agriculture. The text throughout is supported by an extensive range of maps and tables.

Today all this is expected of such a project in early Aegean cultural studies. The account of the University of Minnesota's work in Messenia, *The University of Minnesota Messenia Expedition: Reconstructing a Bronze Age Regional Environment* (1972), is a magnificent predecessor. Current work in the southern Argolid and in Boeotia by American and British teams respectively is on similar, large-scale, multidisciplinary lines.

But here all comparisons stop. The present volume is *sui generis* in Aegean studies. It has a character and purpose far beyond the achievements of the studies so far referred to. The guiding inspiration is Professor Renfrew's. The purpose is not simply environmental reconstruction and explanation of state formation and disaggregation through time; rather it is to use Melos as an instance of the general processes affecting modes of state formation. So the character of the book is largely theoretical, deploying a wide range of models to investigate the central aim.



The living past on Melos — threshing the harvest in the traditional manner, using two donkeys to pull the threshing sledge (*volosyro*).

At times the text is not easy to follow: detailed modelling of the "intensification of subsistence production" in terms of the "production function" and "the incentive elasticity of labour input" are crucial concepts. A model of "socio-spatio-information hierarchy" for Melian state formation is discussed but rejected. So too are explanations based on irrigation, redistribution, secondary state status, importation of prestige goods and a temple-based system. The explanatory model that is chosen is one of "peer polity interaction". This answers well to *growth* in the economies and societies of the Aegean city states, growth through continuous economic exchanges between the states, from their differing resource bases. But I am not sure that it explains *initial* state formation. In Renfrew's final chapter the foundation of Classical Melos by Dorians emigrating from Lakonia, discussed earlier in the book, is strangely neglected.

My own view, which I am confident by no means all Aegean archaeologists will share, is that this book will have an important (and perhaps divisive) influence on future Aegean studies which attempt to go beyond the descriptive and try to tackle economic and social questions. It therefore merits long and serious study, even if one feels that what are proposed in the introduction as general propositions and hypotheses which "have dictated the final form of the book" seem either so general as to be unhelpfully obvious (resource potentials will support a range of population densities) or look rather like inferences after the fieldwork was done. But any big field project will develop its aims as it goes along; it responds to observed data. There is certainly no harm in that. □

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Politics, personalia and the past

Jonathan Tubb

Digging for God and Country: Exploration, Archeology, and the Secret Struggle for the Holy Land 1799–1917. By Neil Asher Silberman. Pp.228. ISBN 0-394-51139-5. (Knopf: 1982.) $16.95.

THERE are many people whose radios never change from a station broadcasting popular music yet will avidly read books on the life of Vivaldi or the extra-marital affairs of Alban Berg and will derive great enjoyment from them. The point of making this statement is simply to emphasize that it is not necessary to be a Palestinian archaeologist, or for that matter an archaeologist of any sort, to appreciate Neil Asher Silberman's remarkable book.

Unlike other authors who have charted the progress of exploration in the Holy Land, Silberman has gone much further, not only chronologically by extending the story back to the exploration by Napolean Bonaparte's "Scientific and Artistic Commission", but also in manner of presentation. He has avoided a tedious procession of facts about who dug what, where, when, and what they found, and instead presents a series of fascinating insights into the various researches seen in the context of the prevailing socio-political situation.

For example, not only does the author describe in great detail the complicated events which followed the discovery of the Moabite stone, events which finally led to the tragic destruction of the monument, but adds a new dimension by setting the whole sad saga against the backdrop of the equally intricate international relations among Great Britain, France and Germany just prior to the Franco-Prussian war. The near frenzy which accompanied the desires of the various scholars to acquire this monument is explicable in terms of its outstanding importance. For the black basalt stela, discovered at the site of biblical Dibon, commemorates the successful attempt of Mesha, King of Moab in the ninth century BC, to liberate his country from Israelite domination. It was, therefore, one of the first and most spectacular examples of an ancient inscription corroborating a biblical narrative, for the same events, albeit with a changed nationalistic bias, are recorded in the Second Book of Kings (II 3: 4-27).

So too, Silberman helps us to understand that the series of really massive excavations conducted by the *Deutscher Palästina Verein* during the early years of this century, e.g. Sellin at Ta'anach, Schumacher at Megiddo, and Sellin and Watzinger at Jericho (Carl Watzinger was the co-director at Jericho, not Schumacher as stated by Silberman), were conceived as part of a deliberate German policy to support and strengthen the failing Ottoman Empire as a buffer against its fragmentation at the hands of the British, French and Russians.

Ultimately, however, this is a book of personalia and Silberman paints vivid, if not always flattering portraits of many of the giants of the subject. Charles Clermont-Ganneau, the brilliant French archaeologist and scholar, comes off worst, depicted as the sort of man to whom one would not tell the time of day for fear of it being denied and ridiculed in the *Times*. In contrast the author gives a sympathetic account of the rather pathetic antiquities dealer, Shapira, whose almost certainly genuine "Dead Sea Scroll" fell prey to the same Clermont-Ganneau, an experience which was to lead eventually to the poor man's suicide. Even some of the best forgotten exploits are enthusiastically remembered by Silberman, such as Seetzen's ill-prepared and disastrously executed survey of Eastern Palestine or Montague Parker's outrageous search for the treasure of Solomon beneath the Dome of the Rock in Jerusalem.

Much attention is rightly given to the outstanding and pioneering work of the Palestine Exploration Fund. It was this

Frontispiece of C. Conder's *Tent Work in Palestine* (1878).

organization more than any other which changed the course of exploration in the Holy Land, removing it once and for all from the hands of the wealthy dilettante and firmly establishing it as an academic and scientific discipline. Today, Palestinian archaeology is the most sophisticated of all the branches of Near Eastern archaeology, with refined stratigraphic excavation techniques and pottery chronologies capable of differentiating between periods as little as 50 years apart. Silberman successfully shows how the groundwork for these achievements was laid: ironically perhaps, two of the great innovators, Sir Flinders Petrie and George Reisner, were "borrowed" from Egyptology.

Relevant and interesting illustrations have been chosen to accompany the text, and the map showing the location of the various late nineteenth and early twentieth century excavations is a most useful adjunct. Altogether, this is an excellent book which can be recommended to both specialist and non-specialist. For students of Near Eastern archaeology, it must be placed in the category of essential reading.

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Variations on the theme of cartography

Sarah Tyacke

Early Thematic Mapping in the History of Cartography. By Arthur H. Robinson. Pp.266. ISBN 0-226-72285-6. (University of Chicago Press: 1982.) $35, £24.50.

OPEN any atlas today and the chances are that many of the maps you see will be thematic, that is they show the distribution of some particular phenomenon stretched across a base map. The themes may range from the physical structure of the world to any aspect of human life, including abstract concepts; they are mapped by a number of graphic methods, immediately understandable by almost everyone, such as dots, flow lines, isolines and various types of tonal shading. These cartographical commonplaces were rarely, if at all, in evidence before 1700 and only took off in the first half of the nineteenth century. Neither the idea of thematic mapping nor the techniques for doing it were widely disseminated in intellectual and other educated circles of earlier times.

As the result of many years of research, A.H. Robinson has recovered the history of thematic mapping and vividly recounted its tender early years and its remarkable flowering under the impact of official and other statistical gathering from the end of the eighteenth century onwards. Unlike the general history of cartography, Robinson has few predecessors in this field and has provided *ab initio* the framework into which the maps gleaned from the works of statisticians, medical doctors, geologists, administrators and others may be slotted.

Although such maps are now very much the province of geographers and allied professionals, the earliest maps, so termed, were more usually drawn to illustrate or to analyse particular distributions or correlations between phenomena and the countries concerned by specialists in other fields; as such the maps are not to be found in atlases but are attached to official reports and professional papers, and illustrate a variety of subjects. For example, medical statistics on cholera were transformed graphically by Dr John Snow in 1855 to demonstrate the relationship between contaminated water supplies and the disease in London: it is these types of maps that Robinson describes and illustrates in the book.

Robinson has felt obliged to narrow his view of thematic mapping, however, to what he regards as the thematic map *sensu stricto*:

> ideally the thematic map has as its purpose the display of the overall structure or meaningful character of a distribution rather than simply showing where some things are.

In so doing — and additionally because he does not see some classes of thematic map as contributing to the development of graphic means of representation of data on maps — a number of types of thematic maps are not considered: for example, land use maps and other categories which may perhaps be called "special purpose" maps are not included. In the latter grouping might be mentioned such items as military plans, road maps and ecclesiastical maps. These types may or may not differ in graphic representation from that found on topographic maps but their function is primarily for a specific purpose and often illustrates a theme. In the case of ecclesiastical mapping a typical thematic example would be an atlas describing the numbers and distribution of properties owned by the Capuchin monks dated 1712. A key to the numbers of monasteries, priests, monks and lay brothers accompanies each map. The idea of representing the numbers by some manner of proportional symbolism is not present but the thematic element is quite obvious in this type of map.

Although it would indeed seem to be the case that the introduction of various thematic maps and symbols did not, as Robinson shows, occur before the end of the seventeenth century, it is perhaps unwise to assume that no one had previously conceived of the idea of depicting the structure of one phenomenon on a map. Certainly such an idea was not at all common but it is evident, even from some sixteenth century texts and maps which survive, that at least some people came close to the idea; for instance Queen Elizabeth I's minister William Cecil drew a map of the south-west of England and Wales showing the distribution of copper and lead mines in the 1570s. If one can see in this and similar attempts appreciation of some of the practical uses of such mapping, it must also be admitted that these maps show the qualitative location of phenomena rather than their quantitative distribution. It is quantitative mapping and its methods of representation which, as Robinson rightly points out, only emerge in the first half of the nineteenth century as a response to statistical studies.

The book is completed by 110 illustrations, many coloured, which are for the most part models of reproduction. In particular the selective use of legible detail rather than the reproduction of entire maps, which are so often indecipherable, is most welcome. Beyond this the great value of Robinson's work is that he has not only found a great many examples of thematic mapping, however defined, across a wide spectrum of professional endeavours, but has then produced a convincing and illuminating history which may be truly regarded as seminal. □

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Changing visions of an ancient society

Arthur A. Demarest

Ancient Maya Civilization. By Norman Hammond. Pp.337. Hbk ISBN 0-521-24017-4; pbk ISBN 0-521-28399-X. (Cambridge University Press/Rutgers University Press: 1982.) Hbk £22.50, $27.50; pbk £7.95, $12.95.

A MILLENNIUM ago the dense Peten jungle of Central America swallowed up many of the sprawling cities of the ancient Mayas, entombing their lofty temples, elegant palaces and graceful sculptures beneath a blanket of vegetation. During the past century and a half archaeologists, scholars and adventurers have spent much of their lives trying to unravel the mysteries of this lost civilization. *Ancient Maya Civilization* by Norman Hammond of Rutgers University is the most recent presentation of their findings and of our understanding of ancient Maya history and culture.

The Maya-speaking Indians of eastern Mexico and northern Central America have a heritage that can now be traced back for over 3,000 years. It was, however, the Classic Period (AD250 to 900) that witnessed the florescence of Maya culture — leaving a legacy of hundreds of archaeological sites in the jungles of northern Guatemala, Belize and the Yucatan peninsula. During this golden age, great centres such as Tikal, Palenque and Uaxactun flourished in an environment which appears utterly inhospitable to European and North American eyes. The hallmarks of this period are its fine masonry architecture, monuments sculptured in delicate bas-relief, raised stone roads and polychrome-painted ceramics whose beauty rivals the vase painting of Classical Greece. The sophistication of the Classic Period of Maya culture can be seen in its fully developed writing system, only partially deciphered, which records calendrical calculations and astronomical knowledge of astonishing accuracy — all based solely upon relentless non-telescopic observation and recording. Despite these cultural achievements, the great jungle cities of the southern zone of Maya culture were abandoned some time about AD900. Archaeologists have suggested various causes for the fall of the southern lowland centres including invasion, disease, ecological mismanagement, peasant revolt or complex combinations of such factors. Yet no consensus has been reached and this collapse remains a great mystery.

Hammond's *Ancient Maya Civilization* could not be more timely, since it comes fast upon the heels of a revolution in Maya archaeology. For half a century, scholars have understood the general nature of the Maya intellectual achievements. These have been beautifully described in the classic works of great Mayanists such as J. Eric S. Thompson and Sylvanus Morley. However, we can now see that these scholars misunderstood many fundamental aspects of Maya culture. The Maya were characterized as isolated, peaceful, theocratic societies supported by small populations of peasants using simple slash-and-burn agriculture and governed by priests concerned only with the worship of time and with astrological lore. Hammond's book incorporates the most recent research on settlement, agriculture and hieroglyphics, which shows Classic Maya society to have been far more dynamic than previously believed. Breakthroughs in hieroglyphic interpretation have revealed that the Classic Maya political landscape consisted of petty lords battling for control of regional city-states. Meanwhile, large-scale archaeological surveys have uncovered complex agricultural systems — raised fields, canals, terracing and other means of water control — which supported dense (proto-urban) populations.

Hammond artfully combines the knowledge gained by the early scholars with the most recent interpretations. First, he draws in the reader unfamiliar with the subject with dramatic, at times romantic, chapters on nineteenth-century explorations of the Maya ruins and twentieth-century archaeological projects. The following, more substantive, chapters on Maya ecology and cultural history are rendered accessible by numerous illustrations, maps and charts. The tone then shifts in thematic chapters on subsistence, politics and trade, in which Hammond presents the evidence for the new, more realistic, characterization of ancient Maya society. Yet in the subsequent accounts of Maya architecture, art and religion, he draws heavily upon the works of the early Mayanists with an undisguised admiration. Hammond also maintains a balance in his discussion of still controversial topics, such as specific interpretations of Maya dynastic history, foreign influences on the Maya and the causes of the Classic Maya collapse. In short, the author is surprisingly successful in integrating old and new conceptions of the ancient Maya.

The book, however, is less successful in dealing with the regional diversity of the civilization. Hammond himself is an active field archaeologist, excavating in Belize, and his text reflects a regional bias towards the southern, lowland manifestation of Maya culture. In the chapter reviewing the rise and fall of Maya civilization, he gives only the most superficial treatment of the development of the great centres in Yucatan, the Guatemalan highlands in the south, and the south-eastern frontier in El Salvador and Honduras. In the thematic chapters, Hammond similarly draws most of his examples from the same handful of sites in Belize and the southern lowlands. Students and laymen reading Hammond's outline of Maya history also should be made aware that his very early datings for the beginnings and development of complex village societies in the lowlands (which I personally accept) are still being hotly debated.

These criticisms notwithstanding, the book is an excellent introduction to the ancient Maya. Indeed, even the author's research biases, when combined with his crisp journalistic style, help to convey the fluid nature of Maya archaeology in which interpretations are undergoing constant revision as each excavation unearths new contradictions and fresh controversies. Hammond's book achieves a unique fusion of a reverence for the early archaeologists and explorers with an insistence on the most current interpretations. It is a perspective which gives the reader a new vision of both this lost civilization and the volatile discipline that is rediscovering it.

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A ring of irony

J.W. Cornforth

The Enchanted Ring: The Untold Story of Penicillin. By John C. Sheehan. Pp.224. ISBN 0-262-19204-7. (MIT Press: 1982.) $15, £10.50.

IN 1941, when Howard Florey's team at Oxford had demonstrated the value of penicillin for treating bacterial infections, it became imperative to find better ways of producing the drug in quantity. A huge cooperative effort by 39 industrial, university and government laboratories in the United Kingdom and United States explored not only the existing method of fermentation, but also the chemistry of penicillin in the hope of producing it by synthesis if fermentation techniques could not be improved. Research on the fermentation process was brilliantly successful, and the problem of producing penicillin was solved. In contrast, although the chemical effort succeeded in finding out the structure of penicillin it failed to find an efficient method of synthesis.

As the urgency of the problem faded, chemists engaged on the synthesis of penicillin turned to other work. The most notable exception was John Sheehan, of Max Tishler's team at the Merck laboratories. Leaving Merck for the Massachusetts Institute of Technology after the war, Sheehan sought a rational synthesis of penicillin and in the end he found one. His book is partly an account of his personal involvement with the penicillin problem and partly an anecdotal story of the drug's discovery and development. It is difficult to picture the readership at which Professor Sheehan is aiming; but the book

will certainly interest, and probably irritate, all those who have worked on penicillin.

Sheehan's account of the early days of penicillin reads largely as an attempt to magnify Fleming's contribution at the expense of Florey's and Chain's. He throws no new light on this old controversy. More novel is his account of the organization behind the research and production effort in the United States in 1941–1944. He draws here on minutes of the US Committee on Medical Research, and other official papers. He has recorded on tape the reminiscences of some principal participants, notably Karl Folkers, and he reproduces extracts from these, along with an account from Hamao Umezawa of a belated Japanese effort to produce penicillin in 1944–1945. The absence of chronological order makes difficulties for the reader, especially in a book so obsessed by priority and credit. The difficulty persists throughout.

The part of the book dealing with chemistry is in many ways the most disappointing. Chemists are likely to be put off by the unexpectedly poor appearance and layout of the diagrams, and by oversimplification that sometimes sacrifices truth, whilst the non-chemical reader may well be confused and misled. This is particularly true of the chapter where one would expect it least — the description of Sheehan's own work. A brief account, based less on the book than on the scientific literature, seems indicated here to make clear Sheehan's contribution.

The molecule of penicillin is relatively small and simple, and the difficulty of synthesis arises mainly from the presence of several reactive groupings that cannot easily be manipulated without mutual interference. It was already recognized by the end of 1943 that pencillin is an anhydride of penicilloic acid, a substance easily made from penicillin and easily synthesized. A decision between two possible structures for this anhydride was eventually made in favour of the β-lactam structure, but it was found to be impossible to produce this from penicilloic acid or its simple derivatives. That was the problem that Sheehan solved. His earlier successes were obtained by making inert analogues of penicilloic acid that enabled him to use a quite vigorous reagent — thionyl chloride — to close the strained β-lactam ring. The breakthrough came when he discovered that carbodiimides could be used to form amides in mild conditions from carboxylic acids and amines; a most valuable peptide synthesis arose from this finding. The cyclization of penicilloic acid to penicillin is a special type of internal peptide synthesis, and when the carbodiimide method was applied to a suitable penicilloic acid a natural penicillin was formed in low yield. This work was submitted for publication in February 1957.

Sheehan's method for synthesizing penicilloic acids proceeded by way of an analogue lacking the acyl side-chain characteristic of penicillins. He tried the long shot of cyclizing this compound and obtained a crude mixture containing a very small proportion of racemic 6-aminopenicillanic acid (penicillin without the acyl side-chain). When this mixture was treated with an appropriate acyl chloride, formation of a penicillin could be shown to have taken place. Sheehan did not publish this experiment but he applied for a patent on it, in March 1957. In 1959, he modified the process by using a trityl group to protect the reactive amino group before cyclization; he then obtained much better yields in closure of the β-lactam ring, though the yields from subsequent removal of protecting groups were poor. This procedure gave him fully synthetic 6-aminopenicillanic acid.

In 1957, workers at the Beecham Laboratories isolated 6-aminopenicillanic acid in pure form by fermentation. This completely independent discovery was the basis for an enormous development of "semi-synthetic" penicillins carrying "unnatural" acyl side-chains. That whole development rests on aminopenicillanic acid derived from fermentations and no penicillin has been produced in commercial quantities by total synthesis. Nevertheless, Sheehan's commercially valueless procedure enabled him to gain, after a protracted legal battle, priority of invention. The last chapter in his book describes that battle.

Sheehan's achievement in making a total synthesis of penicillin was a splendid piece of work. His book, in contrast, lends its own sub-title — *The Untold Story of Penicillin* — a ring of irony. □

Sir John Cornforth is Royal Society Research Professor at the University of Sussex. He is a part author of The Chemistry of Penicillin *(Princeton University Press, 1949).*

Elks, Whelks, and their Ilk

THE monarchs of the Irish bogs
Succumbed to neither men nor dogs
But (most ecologists agree)
To calcium deficiency.
They scoured the base-deficient peat
For antlers and old shells to eat
And, finding all too few, they died
Around the Celtic countryside.
. . .
Then mourn the passing of the elks,
But note the wisdom of the whelks
That roam the shore — their
native heath
With silver-indurated teeth
And bore to death their mollusc friends,
Who come to sad, unsuccoured ends.
Without the need for gins and limes,
The whelks survive to modern times.
. . .
Thus ungulate and gastropod,
And all that live by sea or sod,
Are doomed to be — or not to be
By biogeochemistry.

Ralph Lewin

Children of clay?

N.W. Pirie

Genetic Takeover and the Mineral Origins of Life. By A.G. Cairns-Smith. Pp.477. ISBN 0-521-23312-7. (Cambridge University Press: 1982.) £15, $29.50.

EMINENT physicists have written so much cocksure balderdash about the nature and origins of life, that it is reasonable to be apprehensive about a contribution to the subject from a chemist. We have become accustomed to the naive assumption that, because life is now dominated by the behaviour of proteins and nucleic acids, a study of the conditions necessary for their probiotic synthesis and interaction is relevant. (If that assumption were valid, the right approach to the history of the use of fire would be to study the mechanism of a cigarette lighter, and a study of the composition of paper and ink would illuminate the history of writing.) Cairns-Smith argues instead that proteins and nucleic acids were successful late arrivals on the biological scene. This is an unpopular, but not wholly novel, point of view — a few of us have been adumbrating it for many years. What is novel is that he suggests a concrete alternative hypothesis, almost literally concrete because he gives silicates a fundamental role.

The argument is that clay minerals are not simple rock fragments, but are recrystallized structures with a final form which depends on the conditions of crystallization, the extent to which supersaturated solutions are seeded and the transmission of accidental defects. The last point will be the most controversial. An evolving quasi-biological system needs a genotype and phenotype: this is the old distinction between form and substance. In biology, contrary to Aristotle's dictum, it is the form, the genotype, that lasts and not the substance.

About a quarter of the book is occupied by a description of the immense variety of clay minerals, the distortions and defects in them, and their behaviour in the presence of other substances. The photomicrograms and electronmicrograms show complicated folded laths and other structures, colourfully described as "micro-origami", "card houses" and "a self-assembling maze". Even without the detailed and plausible argument which is presented here, the structures shown in these illustrations support the idea that, if prevital evolution happens anywhere, this is a reasonable group of substances within which to search for it. The old analogy between life and crystallization, which was discussed by Kant, Reil, Troland and others, may now legitimately be elevated from the status of analogy to that of hypothesis. Nevertheless, it may be argued that the mechanism suggested — for example a particular type of clay crystal perpetuating its ability to thrive in crevices

in sandstone by shedding "seeds" into neighbouring crevices — is rather remotely connected with what we normally regard as life. That objection applies with equal force to the organic polymerization picture which has recently become conventional. Neither picture fills the gap between the never living and the nearly living in a wholly satisfactory manner. But it is pleasant to have a change of outlook.

Cairns-Smith chooses clay minerals as the vehicle for biopoesis because of their versatility and indubitable abundance on the probiotic Earth. However, evidence from meteorites, and from *in vitro* experiments going back to the time of Berzelius and Berthelot, make it equally certain that tars would also be present. Tars are the main product of the tediously repetitious experiments in which gas mixtures, made up according to constantly changing opinions about the composition of the probiotic atmosphere, are now exposed to various types of energetic insult. Tars are mentioned here several times. They are invariably disparaged as inert end-products or as "tarry chaos". I have been arguing for many years that sunlit tar is as likely a site as any other for an early stage of biopoesis, but after reading this book I would include clay minerals in the notional mixture and, following Goldschmidt, some minerals containing the transition elements. Everyone to his taste. Cairns-Smith is a clay enthusiast: he enjoys giving clay a role more dynamic than mere adsorption and sequestration.

There is no suggestion here that clay filled this dynamic role in the hypothetical eobiont any better than protein would have done. On the contrary, there is the explicit statement: "Anything clay could do organic molecules would eventually come to do better". To quote again, the discussion is said to be about: "What was biochemistry like when protein was an optional extra?". Several passages emphasize the point that eobionts need not have been small. The idea that life started small presumably stems from a fallacious conflation of early life with viruses. Being obligate parasites, viruses have obviously nothing to do with the case: it is not even useful to classify them as organisms. It is only when competition with other organisms puts a premium on rapid reproduction that smallness becomes advantageous. In the beginning there was no competition. As Cairns-Smith puts it:

> The microscopic size of the modern bacterium should be seen as one of its most notable achievements rather than any sign of evolutionary infancy. The bacterium is a highly concentrated package of ingenuity.

If minerals have the potentialities attributed to them here, it should be possible to demonstrate processes comparable to evolution taking place among them now. This point is discussed briefly with references to some as yet unconfirmed claims that such processes occur in various precipitates. It is not enough to show that there are progressive changes in these precipitates; to be convincing it should also be demonstrated that the change is hastened when a fresh precipitate is inoculated with a "seed". The absence of evidence for this does not invalidate the hypothesis. About a gillion (10^9) years seem to have elapsed between the time when physical conditions on Earth became comparable to those with which we are familiar, and the formation of the first organisms. Relevant *in vitro* processes need not therefore be rapid.

Evidence that the first organisms were inorganic could also come from an appraisal of the present-day biological role of minerals. Some organisms show great enterprise in exploiting specialized properties such as the magnetism and hardness of Fe_3O_4 and the density of $BaSO_4$. It has been suggested that the five-fold symmetry of most echinoderms is imposed by the 75° angle between the faces of calcite crystals, and it may be legitimate to wonder whether the greater intracellular importance of K rather than Na is a relic of the behaviour of these ions in clays. On the whole however, such typical mineral elements as Al and Si seem to have been steadily eliminated. It is primitive plants that make most use of Al, and primitive animals of Si — though there are still substantial amounts, with obscure functions, in the human body. Clearly, evidence for a mineral background to biology is tenuous: the postulated takeover is nearly complete. We are, on the whole, reasonably satisfied with the result. Sir Walter Scott's words are apposite and partly justify the title of this review: "Well hast thou done, frail child of clay!".

Recent articles and letters to the Editor of *Nature* have shown that there is disagreement about the principles that operate and have operated in evolution. The theme often crops up in this book. At a time when we are still arguing about the survival value of many queer features in living organisms, some of the statements made have a rather over-confident tone. It will be interesting to see what comments on that aspect of Cairns-Smith's thesis are made by reviewers with a primary interest in evolution rather than chemistry. Cairns-Smith is commendably tentative, he recognizes that a crystalline gene might not work and says that: ". . . when we are near to the answer we will find many similar answers, and so we will not be able to say exactly how life must have started on Earth". He writes clearly with many neat phrases and some humour. The result is an excellent book — the most plausible and sensible that I have seen on the subject. □

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How to get the wind up in the future

F.J.P. Clarke

Wind Energy for the Eighties: A Review by Members of the British Wind Association. Pp.372. ISBN 0-906048-73-7. (Peter Peregrinus: 1982.) £19.50, $49.50.

Is wind energy a credible source of electricity for the world, and in particular for the UK? *Wind Energy in the Eighties*, a status review written by experts from the British Wind Energy Association, gives a clear answer: wind energy could readily contribute 20 per cent of the UK's electricity supplies and it could be well on the way to that by the year 2000. How valid is that claim?

Although detailed resource surveys have still to be carried out both on land and offshore, the book explains why resource size is unlikely to be a limitation. Because the wind is an unreliable source of power, our electricity system is limited in what it can accept to perhaps 20 per cent of total demand. But that much at least the Electricity Boards agree can be accepted; and since the feasible resource base is likely to be much greater than that, resource size is unlikely to be limiting — provided, that is, the economics are right.

There are two aspects to this latter question, the value of wind power and its cost. Because of the unreliability of wind power, its value must be judged primarily in terms of the conventional fuels it displaces when the wind is blowing, which depends on the plant mix — how much coal or oil will be saved? And that depends on how many wind machines are in the system; the more wind machines there are, the smaller is the value of the next machine to come on stream because the highest running-cost conventional plant will always be displaced first. Modelling such system economics is difficult; present models may be inadequate, and the fact that one is concerned with a plant mix well into the future adds to the difficulties. Nevertheless estimates can be made and are given in the book, assumptions being clearly stated.

The second aspect of the economics is the cost of the wind machines themselves and their associated plant. Here, overseas estimates tend to come out lower than those made in the UK. While this may in part be the problem of relating US dollars or Swedish kroner to British pounds, the book does point up differences in technical assumptions that also contribute.

From all these facts, speculations and assumptions the balanced conclusions emerge that wind machines can be integrated into the electricity grid in substantial numbers, and that in some cases it should be economically worthwhile to do just that.

If these conclusions seem modest against the claims sometimes made for wind power, it should be remembered that they are formulated by professional scientists and engineers who are as aware of the uncertainties as of the very real promise of the technology. More knowledge is needed to lower costs and encourage adoption of the technology, especially offshore.

One of the important concepts behind the possibility of lowering costs is the compliant structure. The design approach in the UK has so far been based upon structural stiffness to avoid resonance and vibration which are notoriously difficult to predict, and which can be so damaging. Such thinking was also behind the earlier US designs. However, subsequent US, Swedish and German designs have gone for a more compliant structure. This involves a much better understanding of the dynamics of the whole structure so that lighter, smaller and less-expensive components can be used. The dynamic forces can then be absorbed with compliance rather than with stiffness. As is admitted in the book, this approach is rapidly gaining favour as an important way to reduce costs, though the extent to which the concept can be applied in the highest wind regimes in the UK has still to be determined. However, as reflected by the cursory treatment, the UK is lagging in this area.

Two basic types of machine are being developed industrially in the UK and both receive good coverage together with their overseas counterparts. The horizontal axis machine had the earlier start, and this has been followed by the vertical axis concept. The rotor blades of horizontal axis machines are subject to reversing gravitational loadings as they turn, thus making them the components most at risk from fatigue failure. The vertical axis machines avoid this and have the additional advantage of being omnidirectional; however they would be more prone to other problems which need investigation.

At megawatts size either machine would look large alongside the traditional windmill and could reach higher than the largest electricity pylons, thus raising the problem of visual amenity. Also, there have been reports from the United States of television interference, windows rattling and other environmental impacts; but so far they seem to be rather isolated events. On this type of issue, as on techno-economic issues, real answers will come only when large machines are up and working in the UK.

Such possible environmental problems make it attractive to place machines in shallow waters offshore. This would cost more, but, it is claimed, expense may be outweighed by the higher wind speeds and the environmental freedom to build bigger structures. Whatever these pros and cons, different practical and institutional advantages are also covered in the book. Wind machines of 1–10MW provide a modular power source that can be added to the system quickly as needed, without all the well-known problems of massive construction projects. In addition, it is pointed out that such machines could provide the beginnings of decentralized power supplies — a possibility that could attract support across political boundaries.

Wind Energy for the Eighties brings together professional views from the academic world, from industry both public and private, from research institutions and from countries overseas. Apart from its value as a status report, the book sets out simply and clearly the background theory to many of the topics. Students and those new to wind energy will find it as good a source book as those already in the field. It is a pleasure to welcome it. □

F.J.P. Clarke was Chairman of the National Wind Energy Steering Committee until 1981. He is now Director of Strategic Studies at the United Kingdom Atomic Energy Authority.

IMAGE UNAVAILABLE FOR COPYRIGHT REASONS

Wind Energy Group

The shape of wind machines to come — an artist's impression of the wind turbine to be built on Burgar Hill, Orkney, by the Wind Energy Group. Overall the structure is 75m in height, taller than Nelson's Column in London.

Cornucopian faith

Eric Ashby

Catastrophe or Cornucopia: The Environment, Politics and The Future. By Stephen Cotgrove. Pp.154. Hbk ISBN 0-471-10079-X; pbk ISBN 0-471-10166-4. (Wiley: 1982.) Hbk £16, $35; pbk £6.95, $15.30.

OVERHEARD in a pub, two women talking: "She was as stubborn as a mule. But I was just as firm". This is the theme of Professor Cotgrove's book: the controversy between those he calls catastrophists and those he calls cornucopians, those who predict a collapse of industrial society and those who believe that threats to our survival will continue to be kept at bay by the very products of that society. Each side so sure it is right and so ready to dismiss the other side as irrational. Each side having access to the same set of facts but interpreting them so differently.

Professor Cotgrove is a sociologist who believes in using quantified data prepared from questionnaires for an analysis of what a layman would call "public opinion" on environmental issues. His aim is to discover what sort of people are catastrophists, and what sort cornucopians. He wisely discards these picturesque (and in my view misleading) labels for more specific categories: environmentalists (a sample of 576 persons from the Conservation Society and the Friends of the Earth), industrialists (a sample of 400 from *Business Who's Who* and *Who's Who of British Engineers*), trade union officials (399 names from the *Trade Union Handbook*), conservationists (500 names from the World Wildlife Fund, UK Branch), and, to represent the public, 1018 names drawn from the electoral registers of Bath, Oldham and Swindon. These people were asked to respond to a battery of questions about their attitudes to the environment, to various other social issues, to the effectiveness of the present political system, to the morality or otherwise of the imperative of economic growth. With this background of information Professor Cotgrove pursues his aim, concentrating largely on environmentalists on the one hand and industrialists on the other.

The initial results cannot be described as surprising: 92.4 per cent of environmentalists consider environmental problems to be "extremely" or "very" serious; only 45.9 per cent of industrialists share this view. Each category of the public behaves predictably. The environmentalists were selected from a sub-group which has deliberately carried its campaign into politics; belief is often hardened into ideology. Conservationists are milder people, less politicized, inclined to give a higher priority to law and order and to the economy than they do to environmental issues (except the conservation of the species to which they are attached: birds, or whales, or redwoods). Industrialists

naturally put their faith in the creation of wealth and tend to underplay anything that threatens that faith. Trade union officials find themselves, as one would expect, sitting on the environmental fence. Of course they want improved social conditions, less pollution, public finance for environmental tasks, but when it comes to jobs versus the environment one doesn't need Professor Cotgrove's statistics to provide an answer.

After this somewhat pedestrian start the book gathers pace. Environmentalists and industrialists emerge as the personifications of two opposing life-styles which Professor Cotgrove (unwisely, I think, for it begs the $64 question in the book) calls "paradigms". The typical environmentalist is in early middle age, well educated, engaged in what industrialists would call a non-productive occupation (as teacher, social worker, academic, doctor), left wing in politics (unlike conservationists, who are commonly right wing), hostile to the current enthusiasm for economic individualism and commitment to economic growth. The typical industrialist in the sample is over 40, also well educated, engaged in production or marketing of commodities, right wing in politics and living — except in his home life: the book makes that distinction — by the strenuous survival standards of the market place.

In this crude classification some truth, of course, is lost. If environmentalists and industrialists were victims of their respective paradigms there would be no cross-fertilization of ideas between them. It's not as bad as that. It was the industrialist who successfully flooded Latin America with mass-produced automobiles, Aurelio Peccei, who founded the Club of Rome and has become the high priest of environmentalists. So not every reader will agree with Professor Cotgrove when he says:

> Industrialists and environmentalists . . . inhabit different worlds. From where they each stand, the world looks different. What is rational and reasonable from one perspective is irrational from another.

It is a caricature, but the merit of caricatures is that they do carry some truth. It is not necessary to swallow the whole of Professor Cotgrove's thesis in order to get from him some useful, if uncomfortable, ideas. The idea that strikes me as most important is the suggestion that the issues environmentalists get so worked up about — acid rain, nuclear waste, lead in petrol, draining of wetlands — are symbols of a much deeper rejection of the social order that finds it fitting to exploit the environment in these ways. Accordingly, though the industrialist and the environmentalist might agree on the desirability of taking lead out of petrol, they would act for totally different reasons: the one, out of expediency, the other as a step toward ridding the air of a moral, as well as a material, impurity. The book quotes the well known work by Mary Douglas on the anthropologist's concept of pollution; it might, even more appropriately, have quoted the traditional Middle English meaning of the word: "to render ceremonially or morally impure, desecrate, destroy the sanctity of. . .".

For this is what Professor Cotgrove is driving at: the idea that environmentalists are not out just to tidy up land, air and water (to do no more than that would be rejected as "cosmetic" treatment). They are out to overthrow the lifestyle that has, in their view, desecrated the countryside and de-humanized cities, and to put in its place what Illich calls "conviviality". A few years ago this sort of protest would have been academic in the pejorative meaning of the word. But a change has occurred that makes this protest potentially significant: it is the undeniable loss of confidence in the capacity of our political system to cope with our social and economic problems, a "decline in political legitimacy". To come back to Professor Cotgrove's statistics: it is not surprising that 74 per cent of environmentalists have little trust in political parties; it is surprising (at any rate, it ought to be) that 68 per cent of industrialists, too, have "little trust". And even more surprising that, when asked whether they support direct (extra-parliamentary) action to influence government decisions on environmental issues, there is still support from 23 per cent of the industrialists who responded to the questionnaire. The framework of consensus within which protest groups could easily be tolerated is showing ominous signs of weakness. Professor Cotgrove suggests (as have, of course, many others before him) that the change from industrial to post-industrial society may not be a smooth one. Indeed, if he is right to use the word "paradigm" for the ruling lifestyle and the challenge being made to it, then the two paradigms cannot co-exist (that is why I don't think "paradigm" is the best word to use). And the question becomes not "whether?" but "how?" the transition will take place.

It is at this point, at the end of the book, that the reader may wish Professor Cotgrove had carried him a bit farther. We have to be content with I'd call the Heilbroner formula: get your house in order by constitutional means, or you'll find the constitution swept away and replaced by Leviathan. The urgent need is for reflection on the kind of political institutions which might evolve, from what we have to what we need. In his book *Ecology and the Politics of Scarcity* (which Professor Cotgrove does not refer to) William Ophuls has made some suggestions which could be the prolegomenon to an environmental ethic. Sociologists have an important part to play in finding out how such an ethic could be disseminated among a people still living (as Professor Cotgrove puts it) under a "Cornucopian faith". □

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Keeping nuclear weapons inside the club

Stephen M. Meyer

Controlling the Bomb: Nuclear Proliferation in the 1980s. By Lewis A. Dunn. Pp.209. Hbk ISBN 0-300-02820-2; pbk ISBN 0-300-02821-0. (Yale University Press: 1982.) Hbk $21, £16; pbk $6.95, £5.45.

NINETEEN eighty two has been a fairly active year for watchers of international conflict. Britain and Argentina went to war over the Falkland Islands. The Iran–Iraq War flared to new levels of ferocity, culminating in the largest tank battles since the Second World War. Meanwhile, Israel engaged PLO and Syrian forces in Lebanon.

It is worth pondering how each of these conflicts might have evolved had they taken place in 1992, when many of the protagonists might have added nuclear weapons to their arsenals. What might have been the result of the detonation of an Argentine nuclear warhead in the midst of the British naval task force? What would have happened if the Iraqis, Iranians or both had nuclear bombs for their tactical aircraft? And imagine the effects of sudden demonstration by the PLO that it had acquired a nuclear weapon (by theft, purchase or gift). In fact, none of these is implausible, and unless we wish to see such items in future news headlines we must begin to take a more serious and systematic approach to coping with the problem of nuclear weapons proliferation.

It is in this context that Lewis Dunn's *Controlling the Bomb* makes several timely contributions to our understanding of the spread of nuclear weapons, the threats posed by further proliferation and the options for slowing its pace and scope. In what is, perhaps, the most important aspect of this book, Dunn discards two pernicious notions currently much in vogue: that nuclear proliferation is essentially a technological phenomenon, and that further proliferation may have benign — if not beneficial — effects.

The first notion has dominated the literature since the beginning of the 1970s. Nuclear weapons proliferation was seen as a genuine technological imperative, a

deterministic outgrowth of the presence of nuclear power plants. Accordingly, the eradication of nuclear power technology (and associated plutonium stocks) was prescribed as the most effective approach to minimizing, if not eliminating, that threat. In reviewing events of the past decades, Dunn exposes the fundamental fallacy of this view. While technical difficulties have indeed been a factor in some cases of nuclear restraint, decisions to pursue — or abstain from — the acquisition of nuclear weapons have been driven by politico-military considerations. Efforts by Israel, South Africa, Libya, Iraq and Pakistan to acquire nuclear weapons cannot be explained by the corrupting influences of indigenous nuclear power generation because the former preceded the latter. Conversely, the failure of West Germany, Japan, Sweden, Switzerland, Canada, Belgium, South Korea and some two dozen other countries to produce nuclear weaponry cannot be accounted for by a lack of expertise. In either case, the total eradication of nuclear power technology would not affect these countries' long-term capability of putting together a nuclear weapons programme using indigenous resources.

As Dunn implies later, technical problems do play an important role in non-proliferation efforts simply by buying time. However, the effectiveness of technological constraints is undergoing continual erosion. Dunn points to the sources of trouble. In their rush to increase export revenues and markets, the primary supplier states have been backsliding on controls over sales of sensitive nuclear know-how and products. Grey and black marketeering have added to the flow of nuclear goods. Compounding the problem is the rise of third-tier nuclear suppliers — India and Argentina, for example. In the end the ability to produce nuclear weapons is inherent in the scientific, technological and industrial growth that is part of economic development.

While he does not see an inexorable drift towards the further spread of nuclear weapons, Dunn acknowledges that there are likely to be a number of instances where the confluence of technical expertise and political will produces new members of the nuclear club. Is there any reason to interfere, to try to alter the pace and scope of proliferation? Here Dunn confronts the second misconception. The presence of nuclear weapons, it is argued, can bring peace and stability to conflict-prone areas, such as the Middle East, by establishing regional deterrent systems similar to that which has prevented a US–USSR war for almost 40 years. Just as the porcupine can walk safely through the forest, so too can the small nuclear power dwell among a world of hostile countries. In explicitly rejecting such bizarre formulations, Dunn points to a host of factors that invalidate these analogies, while suggesting considerable threats ahead: the geographical proximity and political instability of those countries which may come to possess the bomb, superpower relations with such nations, and terrorist activities.

Recognizing the clear threat posed by the wider possession of nuclear weapons, the second half of *Controlling the Bomb* outlines a range of alternatives for a US anti-proliferation strategy. The first set of measures is aimed at checking the pace and scope of further proliferation. Dunn argues that — in order to buy time — technical barriers should be increased so as to make entry into the nuclear club as difficult as possible. Dunn notes that pilot-scale and research facilities can pose a greater threat of proliferation than full-scale commercial nuclear power facilities. Also, the United States should continue to limit the number of countries engaged independently in fuel reprocessing, while re-establishing itself as a reliable supplier of nuclear fuel. Multinational nuclear research and energy programmes (for instance to deal with international plutonium storage) should replace national projects, thereby giving developing countries access to advanced nuclear technology but without the risks associated with independent national programmes. Closing of technology export loopholes is necessary as well.

In order to affect relevant incentives and disincentives, Dunn also proposes a US strategy of political, economic, military and diplomatic sanctions. The objective is to impose such large drawbacks that they overwhelm any existing urge to acquire nuclear weapons. As Dunn emphasizes, this can only be accomplished through case-by-case analysis and direct intervention, not by some global or universal non-proliferation policy. In this respect, he notes — but, unfortunately, does not pursue in great detail — the fact that day-to-day US foreign policy does have an important effect on the balance of proliferation incentives and disincentives; for example the pursuit of other, frequently more important, foreign policy objectives may conflict with non-proliferation interests as often as they reinforce such interests.

A second set of measures is concerned with "limiting damage" in face of further proliferation. Dunn argues that there may be "natural" stopping points in the development of nuclear weapons — so-called proliferation firebreaks — behind which nations could be held by the threat of sanctions and other forms of diplomatic intervention. Where the drive towards the acquisition of a nuclear arsenal could not be prevented completely, emphasis could be placed on retarding the futher evolution of the budding, but still small, nuclear force. Some countries might be contained at the convert (untested weapons) stage (e.g. Israel); some at the PNE (peaceful nuclear explosive) stage (e.g. India); some at the small stockpile stage; and so on.

Lastly, the task of mitigating the consequences of nuclear proliferation is addressed. Dunn considers three thresholds to contend with: after nuclear weapons production is first detected; after a first nuclear test; and after nuclear use in warfare. An assortment of possible US responses is suggested. Technical assistance might be offered after the fact to reduce the dangers of unauthorized or accidental use. Security guarantees could be given to non-nuclear weapons states likely to be affected by the newly born nuclear power. Adjustments to alliance relationships may be required. And finally, consideration must be given to military intervention against a budding nuclear force that poses a clear threat to peace, or against one that comes under the control of non-governmental forces.

Dunn concedes that many of the policy options in each of these three categories of control conflict with one another. As a result, the possibilities for developing a coherent US non-proliferation strategy are quite small. Thus, the reader should not be surprised to find no single blueprint for ensuring a world in which nuclear weapons are no more widely available than they now are. Controlling the bomb will require careful attention to decision-making in individual countries, and to the development of an approach to the problem that has the flexibility and versatility to tailor responses to the case at hand. Dunn deserves much credit for getting our thinking back on the right track. □

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Bytes and pieces from an expert hand

Margaret A. Boden

Machine Intelligence and Related Topics: An Information Scientist's Weekend Book. By Donald Michie. Pp.316. ISBN 0-677-05560-9. (Gordon & Breach: 1982.) $55.

DONALD Michie has played a crucial role in the development of artificial intelligence (AI) in Great Britain. He was a moving force behind the first British AI department, set up nearly 20 years ago in Edinburgh and recognized as of world-class stature. His commitment and enthusiasm derives partly from his friendship as a young man with Alan Turing (a colleague at Bletchley during the Second World War), on whose work our understanding of computation is grounded. The experience at Bletchley was influential too in its emphasis on the applications and social context of science, and on the value of interdisciplinary cooperation in scientific research.

Michie's intellectual pedigree commands attention, and not surprisingly there are many nuggets of interest in his book. But this collection of papers is, overall, disappointing. It is an uneasy mixture, ranging from ephemeral two-page journalistic pieces, through accounts of AI aimed at the general reader, to technical discussions of chess programs and probability theory. It includes personal memoirs of Turing, advice to eminent scientists in danger of being seduced into administration and circuit-lecturing, and impassioned criticism of the Science Research Council's Lighthill Report — which seriously hindered the progress of AI in Britain a decade ago and from which the field has not yet recovered.

The examples of machine intelligence on which Michie concentrates are expert systems and chess programs. Expert systems are codifications of knowledge and inference about specific topics, such as geology or the diagnosis of infectious meningitis. The general interest of these is obvious, and his description of a variety of such knowledge-based systems (some of which are already in professional or commerical use) is lucid and informative. Chess programs might appear to be mere toys, but Michie argues that chess is especially well-suited to the experimental development of AI. It is a complex but relatively well-understood and clearly defined domain, and it demands research into techniques of heuristic search, pattern-recognition and inductive learning. AI work in other areas — such as language-understanding — is mentioned, but does not receive the detailed attention given to chess and expert systems.

Each paper in this collection was appropriate, even useful, in its original context. Michie is technically sophisticated, lucid in expression, historically sensitive and imaginative with respect to the future potential of AI. Few readers will find nothing of interest here. But it would be a strange person — even if he was that odd beast, an information-scientist — who would devote much of a weekend to reading this book. Despite Michie's advice that eminent scientists should spend their time doing innovative research rather than surveying their field, I hope he will one day give us what he is so well equipped to write: a sustained discussion of machine intelligence and its social context. □

Margaret A. Boden is Professor of Philosophy and Psychology at the University of Sussex. Her books include Artificial Intelligence and Natural Man *(Basic Books, 1977).*

Where are they now?

James S. Trefil

Contact with the Stars: The Search for Extraterrestrial Life. By Reinhard Breuer. Pp.292. ISBN 0-7167-1355-1. (W.H. Freeman: 1982.) $28.50, £11.95.

IT IS HARD to criticize a man who agrees with you. In *Contact with the Stars,* newly translated from the 1978 German publication, Reinhard Breuer presents a view of extraterrestrial life which is becoming something of a new orthodoxy in science: the view that the development of life and of technology is not nearly as straightforward as was thought during the salad days of the 1960s, and that humanity, far from being a junior member of a widespread "galactic club", may, in fact, be a unique feature of our galaxy.

Such a view has many sources, one of which is the pioneering work of Michael Hart on the evolution of planetary atmospheres. Hart showed that the various forces that shape atmospheric development — chemistry, vulcanism, stellar evolution and the effects of life itself — comprise a finely balanced system that would require very little in the way of perturbation to upset it. In the case of the Earth, for example, the atmosphere has been undergoing changes in composition over its four-billion-year history, a period when the brightness of the Sun was increasing by 25 per cent. Life is possible on the Earth only because these two changes took place in exactly the right sequence to cancel each other out. One misstep and the Earth would have experienced a runaway greenhouse effect like Venus or become a frozen ball like Mars or Titan. As I write this, watching my daughters playing in the surf at Cape Hatteras, it is hard to imagine that everything I see — the clouds, the people, the ocean itself — is the result of a happy set of chance occurrences that are unlikely to have been repeated elsewhere. Yet that is precisely what the past 20 years of research on the origins of life tells us.

Another strong argument against the existence of copious numbers of extraterrestrial civilizations is attributed (perhaps apocryphally) to Enrico Fermi. When first presented with the statement that the number of such civilizations in the galaxy could well surpass a million, he is supposed to have remarked "Where is everybody?". The point is that it does not take much imagination to envisage the emergence of humanity as a star-faring race. The idea that we have the technical ability to colonize the entire galaxy is not difficult to accept in the age of space exploration, and calculations tell us that a wave of colonization centred on the Earth would sweep through the galaxy in some 30 million years — a mere blink of the eye in the lifetime of the cosmos. If there really are a million other races out there, most of them will be older and more advanced than

we are, so one can argue that the fact that they're not *here* is strong prima facie evidence that they're not *there*, either.

These two types of evidence — one from the study of the origin of life, the other from consideration of the human future in space — have convinced many members of the scientific community that the optimistic estimates of the abundance of extraterrestrial life bandied about in the 1960s and 1970s were simply wrong. As so often happens when new evidence arrives on the scene, old orthodoxies must give way to new. The "galactic club", conducive as it may be to good science fiction, is no exception to the rule.

Contact With the Stars must be understood, then, in the context of the general scientific counter-revolution on the subject of extraterrestrial civilizations. The new ideas have yet to percolate through to the general public, of course, but they are already well known to the experts. The book touches on all of the main topics related to the development of life — stellar evolution, genetic coding, space colonies, UFOs and so on. Some of the topics are treated very well. For example, I got a great deal out of Breuer's discussion of the debate on whether or not we have reached the end of the line in biological evolution. Unlike some who claim that a vastly superior human race will result from genetic engineering, Breuer argues that the genetic code is already carrying just about all the information it can, given the unavoidable copying errors which must occur in replication. This general attitude of informed scepticism is the greatest strength of the book.

On the debit side, the book reads more like a series of unconnected lectures than a single, cohesive document. The author often ventures into asides that range far from the topic under consideration. For example, after having repeatedly asserted his belief that we are alone in the galaxy, he launches into a lengthy and detailed discussion of strategies for communicating with the very extraterrestrials who aren't supposed to be listening. One can only wonder as to why so much effort has been spent devising complex pictographic codes if attempts to communicate are necessarily doomed to failure. □

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Books for Christmas

THE next review supplement to be published in *Nature* is Christmas Books, which will appear in the issue of December 9th.

As well as articles on recent science books for children, and on bird books and space science/astronomy books, the supplement will include reviews of *Aha! Gotcha, Darwin for Beginners, More Random Walks in Science, The Cult of the Expert* and *A Geological Miscellany.*

Time as time out of sight and mind

P.W. Atkins

On Time: An Investigation into Scientific Knowledge and Human Experience. By Michael Shallis. Pp.208. ISBN 0-09-148950-4. (Burnett Books/Hutchinson: 1982.) £8.95.

THERE are three types of scientist. One type believes that the whole of human experience is open to scientific investigation and rationalization. Another type believes that there are aspects of the world that lie outside the scope of science. The third type doesn't care one way or the other, and simply gets on with the job. The author of *On Time* is a member of Class 2; I am a member of Class 1, and therein lies a profound disagreement.

Dr Shallis has addressed himself to one of the most elusive aspects of the world: the nature of time. He takes us through a sequence of attitudes to time, ranging from its measurement, through its role in physics (encompassing relativity, entropy and causality), and ends with a series of chapters dealing with what most hard-nosed scientists regard as outside their domain, and lying beyond credulity. All this he treats with an engaging earnestness that fully captures his intention of approaching his subject with "the scientific attitude . . . of a wide-eyed child".

I suspect that you will like this book if you believe that there are aspects of the world outside science; for all *aficionados* of the paranormal warm to support from within the ranks of practising scientists. I suspect, also, that if you do not believe that science is incapable of dealing with these purported phenomena, such as by dismissing them as hoaxes or by explaining them in terms of established physics, then you will find this book a distressing mish-mash of credulity and passion. What you will not be able to complain about is, as the author so disingenuously reminds us, the impossibility of not going the whole hog on the paranormal once you have embarked on its foothills.

I was particularly struck by one phrase that occurs early on, where Dr Shallis remarks that, by extending conventional "instructional" science to encompass the unreproducible, occasional events that he thinks sometimes obtrude into the world too spasmodically to be captured by scientific method, he provides a description of reality which "will inevitably be richer than the former one". It seems to me that exactly the same remarks are made in favour of the use of hallucinogenic drugs; and with as much force. The heightened richness Dr Shallis asks us to accept (while acknowledging that it may inspire scorn) includes just about everything that may be regarded as symptomatic of crackpots: astrology, precognition, angels and gods (with, I think, fairies and bent teaspoons thrown in for good measure). Some idea of the style of argument is captured by the manner in which the book speaks of α-emission. Apparently, this process is "absolutely causeless" (p.119) except for the possibility that it may be "influenced by mind" (p.125) — particularly, it seems, the minds of young chicks.

I hope that enough has been said to represent without distortion the thrust of Dr Shallis's argument: be totally credulous, swallow the whole of the paranormal, give weight to the hallucinations of the wishful thinking and the downright potty, and disregard the successful continuing progress of modern science. If you accept that argument then you will indeed accompany the author as far as his conclusion, that time is intrinsically paradoxical and must for ever remain elusive. The rejection of that argument, which in the face of the evidence presented here doesn't seem to present much difficulty, keeps open the path to comprehension. □

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Breaking the laws

Michael Berry

The Accidental Universe. By P.C.W. Davies. Pp.139. Hbk ISBN 0-521-24212-6; pbk ISBN 0-521-28692-1. (Cambridge University Press: 1982.) Hbk £10, $19.95; pbk £4.95, $9.95.

WHAT would the world be like if the force of gravity were a little stronger? If the proton were a little lighter? It is the purpose of Paul Davies's latest book to study questions such as these and, more generally, to explore the sensitivity of various features of the Universe to the values of the fundamental constants of nature. As groundwork the book begins with an account of the laws of physics and the scales on which they operate, from the shortest "Planck" lengths and times, through those characteristic of nuclear physics, up to cosmological ones.

The main body of the work is devoted to showing how these levels of structure are exquisitely interlocked, so that a change in one level can have astonishing repercussions elsewhere. For example, if the strength of the weak nuclear force (compared to gravity) were slightly different, this would greatly affect energy transport in stars, and probably prevent supernova explosions spewing forth the heavy elements which condense into planets and

ultimately ourselves. If the strong nuclear force were a few per cent stronger or weaker there would probably be no stars at all. And if the primordial energy density had been minutely different, the Universe would either have re-contracted implosively long ago (and long before stars and planets could have formed) or exploded so violently as to inhibit condensation into galaxies. Related to this sensitivity are the celebrated "coincidences" involving large numbers; for example, the ratios of the age of the Universe to the time for light to cross a nucleus, and of the strength of gravity to the strength of electromagnetism, are both about 10^{40}.

By examining an impressive variety of such cases (basing his arguments largely on a review article by B.J. Carr and M.J. Rees — *Nature* **278**, 605; 1979), Davies concludes that the Universe is so "fine-tuned" to its present condition by the values of fundamental constants that even a very small alteration would produce a world vastly different from ours and almost certainly unable to support life physically constituted as we know it. This leads him into a discussion of the anthropic principle, which in its weak form asserts: "What we can expect to observe must be restricted by the conditions necessary for our presence as observers", and in its strong form asserts: "The Universe must be such as to admit the creation of observers within it at some stage".

This sudden emergence into cosmology of the idea that the physical structure of the Universe is very special and related to its cognizability is rather strange. It seems to be connected in a curious way with the current preoccupation in applied mathematics with the concepts of genericity and structural stability as especially emphasized by René Thom. Much effort is being expended to determine the nature of the generic (that is, typical, or non-special) solutions of the equations representing physical laws, in the belief that these, being stable against perturbation, can represent persistent structures (condensed matter or life, for example). But the equations themselves are highly non-generic; Maxwell's laws of electromagnetism, for example, are a very special set of linear relations which strongly constrain two vector fields, and any modification would lead to wrong "laws". In this picture of generic solutions of non-generic equations it is difficult to see how the fundamental constants fit in: are they "random initial conditions" or are their ratios determined by laws we do not yet know?

The consequences of physical laws have been explored much more extensively employing the actual ratios of fundamental constants than with hypothetical modified values. It might be that evolution towards structural complexity is virtually irresistible, would survive such modification and still lead to intelligence (albeit with a physical basis quite different from ours — Olaf Stapledon's novel *Star Maker* comes to mind, with intelligence developing via organized fluid motion in the wisps of galaxies and, later, inside stars). If this is correct — that is, if in the space of fundamental constants it could be shown to be generic for universes to have intelligence as an "attractor" of physical laws — the strong anthropic principle would follow as a marvellous consequence, and this aspect of the "coincidences" would lose its mystery.

Paul Davies does not devote much space to such speculations but quite properly concentrates on explaining the physics underlying them. He does this with the lucidity and authority we have come to expect from him. The book should be accessible to anyone with an undergraduate acquaintance with physics, and I warmly recommend it as an excellent introduction to this new and important idea. □

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Common ground in a changing climate

Hubert Lamb

The Earth's Climate: Past and Future. By M.I. Budyko. Pp.307. ISBN 0-12-139460-3. (Academic: 1982.) $39.50, £26.20.

THIS important book by the Soviet Union's outstanding climatologist, Professor M.I. Budyko, is a landmark in the development of climatology as a branch of physical science. At the same time, because climate itself touches more or less every aspect of life on Earth, and because of the way the author draws on the varied works of his long and active career, the book ends by being a statement of Budyko's life philosophy and perhaps of the broadest view of the world as seen from the Soviet Union. As such, it is not only an important work (and one written with notable lucidity) but of wide interest.

Since about 1960 Budyko has been actively involved in concern over man's impact on the environment and with the possibility that the overall effect of the accelerating increase of carbon dioxide in the atmosphere might so disturb the balance of in- and out-going radiation as to lead to an unacceptable warming of the Earth. He was also among the first to discuss the feasibility of counter-measures, such as introducing an aerosol veil into the stratosphere to reduce the warming.

The Earth's climate is some 40°C warmer than would be expected at this distance from the Sun thanks to the extent to which out-radiation is intercepted by the carbon dioxide, water vapour and so on in the atmosphere. Among the most interesting features of Budyko's book is the way in which he traces the changes of climate through the geological past in terms of the carbon dioxide and water supplied by volcanic activity. Decline set in as the volcanism eased off and as vegetation appeared and increased, removing carbon dioxide and converting it to oxygen. This decline was accompanied by a lowering of temperature and the appearance of polar ice. Other shorter-term temperature drops have been associated with periods of specially active volcanism and frequent loading of the atmosphere with veils of dust and aerosol.

However, not all geologists agree that the record of carbon dioxide in the atmosphere fits so well with the known history of prevailing temperatures. Nor can the temperature history of the past century or more be explained by carbon dioxide alone. I am concerned that not only Budyko's book but prevailing opinion among leading meteorologists and climatologists may seriously over-emphasize the importance of carbon dioxide *vis-à-vis* other causes of climatic change. In this book it turns out that in 101 pages carbon dioxide is considered as a major cause of climatic changes, compared with 21 pages for volcanism, and 25 pages for the capacity of polar ice variations and other albedo variations to amplify the overall effect on world temperature. Possible variations of the solar constant get two pages, variations of cloudiness five pages, and continental drift/polar wandering and the Earth's orbital variations about four pages each. The last named variables are surely under-stressed.

The frequent forecasts nowadays, in this book and elsewhere, of a coming drastic change of climate due to man's production of carbon dioxide (and other effluents) are in general a good deal too confident. Despite its basis in the laws of physics, so well expounded in this book, climate is the product of a complex of influences that defies precise modelling. There is no clearly definable limit to the range or the rate of natural climatic variability, particularly the coolings associated with waves of volcanism and (perhaps) other causes. Once within the past 300 years, between 1690 and 1750, there was an oscillation which changed the ten-year average temperatures in England by nearly 2°C, probably the local expression of a global phenomenon. Carbon dioxide changes can be presumed to have played no part in this. The prevailing confidence in the carbon dioxide forecasts is based on their grounding in straightforward physics, but our experience of the difficulties of short-range meteorological forecasts is not entirely irrelevant to this problem.

That said, the possibility of a drastic

climatic warming due to the carbon dioxide has nonetheless to be taken seriously and Budyko has long been pressing upon scientists and politicians alike the urgency of the matter. On the one side, the increase of carbon dioxide could, if its effect were not counteracted by natural or anthropogenic influences, warm world climate by as much as 3°C within about 50 years — and possibly by 10–15°C in the highest latitudes, with disappearance of the polar ice. This and the corresponding changes in the rainfall pattern would be expected to shift the agricultural crop zones and the desert boundary northwards by a highly inconvenient number of degrees of latitude. This would also initiate the rapid melting of the land-based ice-sheets in Greenland and elsewhere, possibly including the great ice-sheet in Antarctica, with a consequent rise of ocean level that would overwhelm the Netherlands and drown large parts of the world's biggest cities. On the other side, forward calculation of the astronomical (Earth's orbital) variations points to a new ice age beginning within about 5,000 years (not the 10,000 to 15,000 years mentioned in the book on the basis of an outdated calculation).

Against this background Budyko sees the increase in carbon dioxide through burning fossil fuel as a benign effect if only it can be moderated to a slower rate of change to which we may be able to adapt. In his eyes man is restoring the atmosphere to its state before the growth of vegetation and is thereby prolonging the possibility of existence of human life and of a biological environment sufficient for our needs. Otherwise the continuing natural decline which set in before the Pliocene would, he suspects, doom the biosphere to extinction within one million years. There are interesting asides in his final chapter about the uniqueness of the Earth's biosphere and the improbability of life, let alone technologically proficient beings, elsewhere in the Universe. On another page, he develops a vision of the future made possible by our postponement of the run-down of carbon dioxide in the atmosphere: a future with a much denser human population and more varied life-forms and, presumably, accelerated through-put of the food chains. Not everyone, however, will see that as progress to a better world.

But in the end there is a noteworthy agreement between the conclusions reached on both sides of the cultural and philosophical divide about our situation in the world today, particularly as regards the threat that man's activities present to our planet. Budyko quotes a Soviet colleague, mentioning a well-known statement by Karl Marx, that

> culture leaves behind itself a desert if it develops spontaneously and is not directed deliberately . . . a developed society ruins nature and destroys the environment from which it derives all its resources.

In this we are no better than our primitive ancestors, whatever their thoughts about the world, who ran so many species of the animals they hunted to extinction. To avert the danger, mankind "must form [take charge of] nature on the global scale". This demand for a cultural dictatorship corresponds to the recognition in informed Western circles of our responsibility towards our successors on Earth and of the need for controls and restraints based on wise counsels and better knowledge. □

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Philosophy in retreat

Stephen Hawking

Cosmology, Physics, and Philosophy. By Benjamin Gal-Or. Pp.522. ISBN 0-387-90581-2. (Springer-Verlag: 1982.) DM 69.

THE ancients considered the ultimate question of "Life, the Universe, and Everything" to be part of philosophy even though they were not sure that the answer was 42. For instance, Aristotle wrote a book on the structure of the heavens in which he put forward good reasons for believing that the Earth was spherical and even gave a figure for the circumference that was correct to within a factor of two. Other philosophers up to and including Kant also considered the structure of the physical Universe to be within the arena of philosophy.

In more recent times, however, philosophers have largely abandoned cosmological questions to physicists and have shrunk the scope of their enquiries to such an extent that many of them now hold that "the sole remaining task for philosophy is the critique of language". Although physicists have brought about great changes in our ideas about the Universe and in our concepts of space and time, they tend to shy away from questions that they regard as philosophical or metaphysical like "does time have a beginning" or "why is the Universe the way it is".

Benjamin Gal-Or, the author of *Cosmology, Physics, and Philosophy*, bemoans the gulf that has grown up between philosophy and cosmology and attempts to bridge it by informing philosophers about the modern theories of physics and by trying to interest physicists in philosophy. In the first of these endeavours he may have some success. The book contains a reasonable account of the hot big-bang theory. It is however overlaid with a lot of other ideas from the modern literature on theoretical physics which the author has included rather uncritically, even though they may not be relevant to the main contents or they may be ephemeral and not taken very seriously by others in the field. An example of this is a flow diagram in the introduction which is meant to show the relations between the main ideas in the book but which is more like a maze with about 40 or 50 boxes connected by arrows. I feel that non-physicists who want to find out about modern theories of the Universe might do better to read the clear and reasonably short book by Steven Weinberg entitled *The First Three Minutes*.

The book's attempt to inform physicists about philosophy is not very successful, at least as far as this physicist is concerned. I found it rather confused and verging on the mystical. One is frequently enjoined to cast aside prejudices and to embrace "gravitism" and "havayism", which is supposed to be the science of the whole, though it was never clear to me what one was supposed to do with them. I also found it rather irritating to have about a quarter of each page in italics for emphasis. Nevertheless, the task that Gal-Or has attempted is a very important one and this book will have to serve until someone writes a better one. □

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In muddied waters

Michael Ruse

The Shaping of Man: Philosophical Aspects of Sociobiology. By Roger Trigg. Pp.186. Hbk ISBN 0-631-13023-3; pbk ISBN 0-631-13028-4. (Basil Blackwell: 1982.) Hbk £12.50; pbk £5.95. To be published in the USA next year by Schocken Books.

THE study of animal social behaviour from an evolutionary perspective dates from the publication of Darwin's *Origin*. But, for a number of reasons — not the least of which is its sheer difficulty — such study languished for a century; and this despite the valiant efforts of a number of workers, most notably and creditably the so-called "ethologists". However, in the past 10–15 years, this member of the family of evolutionary science has awoken, and now races ahead, both theoretically and empirically. Under the new rubric of "sociobiology", the biological basis of animal social behaviour is a rich hunting ground for the brightest and most ambitious of graduate students.

Yet, not everyone has welcomed sociobiology; at least, not everyone has welcomed sociobiology as applied to our own species, *Homo sapiens*. Expectedly, left-wing thinkers are wary of any attempt to relate human behaviour to genes, and they have been joined in their opposition

by others. To my regret, members of my own discipline, philosophy, have added their voices to the chorus of dissent, happily "proving" that not only is human sociobiology pernicious and dangerous, but that it is also conceptually impossible! I speak of "regret", because although it is undoubtedly the case that some pretty daft things have been said by advocates of human sociobiology, it surely should be of some moment to us as philosophers that human beings are modified monkeys, rather than the miraculous recent creations of a Being determined to produce offspring in His own image.

The pendulum of opinion now appears to be swinging back, however, and Roger Trigg's new book is happy confirmation of this fact. He is far from an uncritical devotee of human sociobiology, but equally he is convinced that as philosophers — as inquirers into the ultimate nature of man — "we reject the findings of human biology at our peril" (p. xviii).

What is man? Trigg runs through a number of answers and this large survey is one of the irritating aspects of the book; it takes an interminable time before the author turns to his sub-title and to biology. For some one hundred pages we get a Cook's tour, as Trigg takes us rapidly through a selection of the many non-biological answers that have been given to his question. In fairness, the tour is not without value. I am reminded of Woody Allen's comment about how philosophy frustrated him, because he could never tell the difference between heuristics and hermaneutics. He should read Trigg's book, for he will find the solution to that problem, as well as to many others.

Specifically, Trigg introduces us to those such as B.F. Skinner and David Hume who would treat man as object. Some familiar, but nonetheless pertinent objections are raised, for instance the inability of those who regard man as an object to regard themselves as objects! Then Trigg looks at those who take the opposite tack, regarding man as subject. Included here are such thinkers as J-P. Sartre, R.G. Collingwood and H. Gadamer. The difficulties in this approach are noted, including that of ever escaping from the relativism which stems from the overall presuppositions. Next, Trigg looks at sociological approaches to man (his answer is that they are limited), and discusses whether man is a genuine species (yes, but this doesn't tell us as much as we might hope).

And so, finally, we get to the question promised by the book's title: "Is biology the key to the nature of man?". Trigg's answer is guarded. On the one hand, he agrees that biology — specifically human sociobiology — seems to have at least the potential to throw light on human behaviour and its causes; on the other, he warns of the limitations and confusions in the biological approach.

I confess that there were times through this part of his discussion when I felt that Trigg was being a little less than fair to sociobiology (or, at least, to its proponents). Sociobiologists are certainly aware of obvious traps. For instance, if one explains behaviour through (say) kin selection, then although causally one is thinking in terms of reproductive benefits to the individual, it does not follow that one can draw sweeping claims about all humans being "selfish" or some such thing. Talk of "selfish genes", for instance, is metaphorical, and does not commit sociobiologists to the thesis that every human being is a conscious self-interested schemer.

Yet at this point, Trigg accuses sociobiologists of "muddying the waters" for they juxtapose "altruism with an ordinary notion of advantage and self-interest" (p.117). Rather it is Trigg who is muddying waters, or at least who is accusing sociobiologists of being more naive than they really are. Surely he would not suggest that any attempt to give a causal analysis of human behaviour is doomed to failure? Nor even would he suggest that all causes must be related at once to our immediate conscious awareness. Suppose one says that an adult behaves well because of strict childhood discipline. This may or may not be true, but seems to makes sense if the adult does not usually consciously remember the childhood training. If this is so, why is it wrong in principle for sociobiologists to formulate explanations using factors which operate below (or outside) the level of consciousness? And why should they not use metaphors such as "selfish"? Every other scientist uses metaphors (force, work, energy, power, attraction, repulsion and so on).

Finally, Trigg raises the relationship between biology and morality. Again, his conclusion is cautious:

> The insights of sociobiology may give us further understanding of man's nature and give us material on which to base our claims. They cannot take away from us the capability of rational moral choice, based on our conception of the nature of man. The paradox is that any attempt through rational argument to treat man as merely another animal, no different from other animals, is itself self-refuting [p.162].

I'm not quite sure of the strength of this supposed paradox. Is it empirical, in that man simply seems different from other animals? Or is it necessary, in that the very attempt to explain ourselves proves that biology is limited in power? Either way, my own inclination is to agree with Trigg that there is more to morality than biology, in the sense that one certainly cannot deduce morality from biology. Our biological urges are not always "good" urges. Nevertheless, I would go further than Trigg — if indeed we are the product of evolution, then our moral sense presumably has some adaptive value, and hence biology can perhaps throw light on the content of our biology. Biology may not justify in some absolute way our sense of the worth of human being, but it can show why we have such a sense. And perhaps this is all the proof we can ever hope for.

Clearly I don't agree with some of Trigg's conclusions, any more than he would with mine. But that is the nature of philosophy (nor is such disagreement a sign of any inherent weakness in the subject). What counts is that although he is overlong in getting to his main theme, Trigg stimulates one to think on important matters. And that is no small praise. □

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Good, bad & biology

Marian Dawkins

The Nature of the Beast: Are Animals Moral? By Stephen R.L. Clark. Pp.127. ISBN 0-19-219130-6. (Oxford University Press: 1982.) £7.95, $14.95.

STEPHEN Clark has made a tactical error. He is a philosopher writing about animal behaviour and attempting to explore the possible analogy between what animals do and human morality. But he unfortunately fails to conceal his contempt for biologists and many of their theories about animals. "The philosopher at a gathering of biologists", he remarks in the Introduction, "is the one in a state of permanent exasperation that these scientists do not understand their own concepts!". He dismisses various biological theories as "merely ridiculous" or "broken-backed and to point this out should be a job for any competent editor, let alone a philosopher".

Writing about a subject which is not your own is always hazardous; but to write in such a way that you raise the hackles of those working in the field before you have even got down to saying what you want to say is hardly the way to persuade biologists that a dose of philosophy would be a good thing. I say this because my own reaction to the book was strongly coloured by resentment at a number of implications, such as that all ethologists are right-wing ideologues who have to be taught a lesson on how to think straight. This resentment probably made me less tolerant of factual errors, more irritated by grammatical awkwardness and far less open to the contribution this book may make than I would have been had exactly the same ideas been put over in a different spirit. For the subject of the book — whether what human beings regard as "moral" has anything to do with what animals have been shaped by

natural selection to do — is an important one. There has indeed been a lot of muddled thinking and perhaps facile generalizations from other species to our own. So having expressed my irritation, I will now try to concentrate on content rather than style.

Dr Clark is concerned with what he calls the "morals of Nature". Parent animals care for their children, do not always kill their rivals and apparently respect sexual taboos and figures of authority. There are at least superficial similarities between these examples and conduct which we would call moral if it were done by a human being. But would we be right to describe such behaviour as moral in a non-human species? Are the inhibitions that a wolf feels when it stops short of killing a rival anything like moral ones? And can we learn anything about human morality from studying animals?

To answer these questions, Dr Clark examines various aspects of animal behaviour, including parental behaviour, territory and dominance, drawing on considerable factual detail in the process. He then comes to the conclusions that other animals are not moral in the usual human sense of the word because they do not have the ability to see that they could be wrong. He describes animals as ethical because they "respond to aspects of a situation and to features of their kindred, that a good man would also respect". But he says they are not moral,

> for they do not, as far as we can see, have any occasion to moralise about themselves or to construct intellectual systems to accommodate their immediate responses.

So far Dr Clark's conclusions are easy enough to follow (apart from reservations about some animals which behave in ways which a good man might not respect), but they are neither very surprising nor very different from common sense. It is when he tackles the question of whether human beings should, in any way, adopt the "morals of Nature" that it becomes much more difficult to see what he means. He believes that "the ways of the beasts can set us good examples". Yet he has already argued that the "beasts" are not moral, so it is not clear why he thinks they may set us moral examples. He also believes that some of our moral habits, such as caring for relatives, are "bred into" us. And yet he firmly states that morality does not and cannot come from biology.

One is, in short, left more confused at the end of the book than at the beginning. His scathing criticisms of biologists might have been a great deal easier to stomach if he himself had been able to show that clarity of thought that he feels biologists so obviously lack. □

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Separated learning and symbol making

Carolyn A. Ristau

The Unique Animal: The Origin, Nature and Consequences of Human Intelligence. By Don D. Davis. Pp.336. Hbk ISBN 0-907152-02-3; pbk ISBN 0-907152-01-5. (Prytaneum Press: 1981.) Hbk £12.95, $25; pbk £6.95, $12.95.

DON Davis has a provocatively simple hypothesis of the difference between the minds of man and beast. He proposes that man alone is capable of separated learning — of proposing a connection between two or more events separated in time. This entails more than remembering over a long period of time; connecting two seemingly distinct events is essential. As a consequence only human beings can create symbols or discover a relationship between two non-contiguous events, and can hypothesize or propose such connections. Man's hypotheses flower in his magic, religion and science. Man alone has placed a seed in the ground one day, seen a plant grow at that place days later and connected the two events.

But is this the domain only of human beings as Davis claims? Ethologists might note, for example, that certain termites grow fungi and that some ants capture, tend and "raise" aphids for milking, although no one has shown that an individual insect could "discover" the relationships involved. Again, whether man is the only symbol maker seems highly debatable. At least some apes seem to have learned symbols taught by man — as Davis agrees — and some apes' use of novel sign combinations may be indicative of an ability to create a symbol. To conclude that natural animal communication is not symbolic may merely reflect a limitation of our methods of study. For example, the honey bee's dance may be a symbolic achievement when it is used to indicate the location of food.

Equally relevant is the question of whether man is the only creature that in learning can tolerate gaps between environmental events of more than a few seconds. Davis dismisses the obvious counter-examples — taste aversion learning and events that are marked by an especially salient stimulus such as a loud noise — as extremely narrow exceptions. Yet whether animals, like human beings, can "mark" events or locations (an ability that shares characteristics with the process of naming) remains a matter for both philosophical discussion and experimentation. Scent-marking by animals and food-caching with later retrieval may represent rudimentary forms of marking.

To be sure Davis's concept of separated learning *is* powerful, particularly given the way that contiguity as a prerequisite for learning has dominated experimental psychology for decades. No clear examples of such learning by animals come to mind, in contrast with the ease with which Davis is able to apply the concept to pre-historical science, the discovery of fire, of death's inevitability, of smelting, pottery, writing and record-keeping, and the interrelations of magic, religion and science.

Davis's book is of intellectual interest to a broad audience both scientific and popular. There are gaps in his references, however, particularly of pertinent recent research, leading to some incorrect generalizations; and some of his interpretations of research are questionable. But his ideas are important and provocative. They clearly merit attention. □

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The pitfalls of bookmaking *(continued from p.112)*

▶ space as an entry in a telephone book. The main thrust (as publishers like to say) is by direct mail which ideally means a leaflet, brochure or booklet landing on the mat of potential customers quickly and reliably. However postal and handling expenses have escalated in recent years and even publishers who specialize in this type of marketing have drawn in their horns somewhat. There can be months of delay before the information about a particular book, especially if it appeals to small groups in different disciplines, reaches all possible readers. Campbell is bound to be on the list which is mailed last.

In the first year of publication perhaps as many as 800 copies will have been sold even though no one seems to know about the book. A further 200 sales will appear on the statement for the following year and then probably no more than another 100 over the next three years. Campbell, a reflective character, may wonder whether this is *the* way to reach the 1,100 purchasers who alone seem to be interested. Where is the electronic revolution? Even publishers stop and wonder, as they see the latest list of overstocks to be "wasted" because they are unsaleable, and ponder whether the print runs should in future be set below 1,000.

Meanwhile, over lunch with Parsons an adviser to the firm suggests there is room for a nice, tightly edited volume on the topic of Campbell's original chapter. The field has grown. The ideal editor would be Campbell himself, and he is approached. Does he accept? □

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