Book Reviews

The Evaluation of Forensic DNA Evidence. By Committee on DNA Forensic Science: an Update, National Research Council. National Academy Press, 1996. 254 pages. Price £30.95, hard cover. ISBN 0 309 05395 1.

Although DNA evidence is now widely used in criminal trials, challenges are still being raised by the defence and there is inconsistency in the way that inferences are presented, both in the USA and elsewhere. An important issue has been the calculation of the probability of a DNA match when racial or other sub-population differences in marker frequencies have to be accounted for.

A Committee on DNA Technology in Forensic Science reviewed the technique for the National Research Council and reported in 1992. Although many of its recommendations on procedures have become accepted, others have not. Since then, much more data have accumulated, further research has been undertaken into the relevant methods of population genetics and statistics, and different methods of DNA profiling have come into use. The NRC therefore established a new committee, chaired by the distinguished population geneticist, J. F. Crow, to review the issues. This book comprises its report.

A particular problem is to take account of population substructure, which can lead to an incorrect calculation of the probability of a specific DNA profile match. If population-wide or other inappropriate allele frequencies are used, match probabilities can be computed which are too low, whereas justice may be better served by being conservative. To meet this concern, the 1992 Committee proposed and recommended the use of the 'ceiling principle', according to which allele frequencies should be estimated in each of 15–20 homogeneous populations spanning the racial and ethnic diversity in the USA, and the highest allele frequency among the groups or 5%, whichever is larger, should be used to compute the DNA profile frequency. The Committee also recommended that, until there were sufficient data to put the ceiling principle into effect, an 'interim ceiling' should be applied, by which the upper 95% confidence limit of the frequency of each allele should be computed for each racial group and the highest of these or 10% used. The ceiling and interim ceiling principles have come under considerable attack from population geneticists, both because they do not have a logical basis and because they do not take account of well established racial differences in allele frequencies. The new Committee recommends the ceiling principle should be abandoned, and suggests how the limited amount of substructure within racial groups should be accommodated by adopting conventional population genetic methods. It recommends that calculations should be based on observed allele frequencies for each major racial group and a coefficient describing population differentiation (Wright's F_{ST}) used to specify the local departure from Hardy–Weinberg frequencies within each race.

An important further assumption in calculating match probabilities is that multi-locus genotype frequencies can be computed simply from the product of those at individual loci, which requires that there should be linkage equilibrium among loci. The Committee gives examples to show how well this assumption is supported by the data, and recommends its use be continued.

Issues of statistical methodology and presentation of evidence are also considered. Notably the Committee recommends the use of likelihood ratios to express the conclusions of a match, i.e. the ratio of the probability of the evidence sample if it came from the suspect to that if it came from an unknown person. Large numbers result, but they seem easier to deal with than incomprehensibly small match probabilities. One problem which has led to a considerable amount of research and controversy is that of 'binning' for the highly polymorphic VNTR loci which have been used in DNA profiling. This is ceasing to be an issue as there is an increasing move towards PCR-based methods using somewhat less polymorphic markers, but for which precise fragment lengths can be specified.

Overall the Committee is strongly supportive of the use of DNA evidence in criminal proceedings, and of the use of the population genetic methods which have been developed to provide the evidence. It is to be hoped that the methods become unequivocally accepted, providing of course proper lab and numerical procedures are used and documented, so that lawyers will have to find other grounds for defending a client charged on the basis of DNA evidence.

Although this is not a textbook but the report of a

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committee, it is of value to the general reader in that it reviews the basic methods and difficult problems in a clear way, and provides a large number of examples in an easily accessible form.

WILLIAM G. HILL
Institute of Cell, Animal and Population Biology, University of Edinburgh

Natural Selection and Patrick Matthew – Evolutionary Concepts in the Nineteenth Century. By W. J. DEMPSTER. The Pentland Press, 1996. 365 + xv pages. Price £12.50. ISBN 1 85821 356 8.

The first reaction to the title of this book is likely to be Patrick Who? Why it should be so and why it perpetuates an injustice are the twin themes of the author. The essential facts can be briefly stated. Patrick Matthew (1790–1874) was a Scottish gentleman-farmer who managed one of the largest fruit orchards in the Carse of Gowrie. He was well read, widely travelled on the continent of Europe, especially Germany, for a time supported the Chartist movement and entertained radical and egalitarian political views. But his place in the history of biology rests on his early perception of the role of natural selection which he set out in a few succinct sentences nearly thirty years before the publication of Darwin's *The Origin of Species*. The key statement appeared where it might be least expected – in an Appendix to a book on *Naval Timber and Aboriculture* published in 1831. Matthew was interested in the stability or otherwise of species and set down his views in one of several notes dealing with quite diverse topics. He recognized adaptation: 'There is a law universal in nature, tending to render every reproductive being the best possible suited to its condition', and perceived natural selection as the means to that end: 'As Nature ... has a power of increase far beyond what is needed to supply the place of what falls by Time's decay, those individuals who possess not the strength, swiftness, hardihood, or cunning, fall prematurely without reproducing ... their place being occupied by the more perfect of their own kind, who are pressing on the means of subsistence.' In another section of the Appendix he recognized the unremitting action of this 'circumstance-adaptive law' which 'acting in concert with the tendency which the progeny have to take the more particular properties of the parents' and 'operating upon the slight but natural disposition to sport in the progeny (seedling variety), does not preclude the supposed influence of volition or sensation may have over the configuration of the body'. He recommended experimental investigation of the variation among progeny to discover the causes. This was really not bad going for 1831. The author argues, with justification, that Matthew's lifelong familiarity with breeding and selecting fruit trees made selection by natural conditions seem a self-evident truth and reminds us of the extent to which Darwin's advocacy of natural selection was nurtured by his innumerable discussions with breeders of domesticated animals.

Matthew's book was reviewed in several journals favourably, except for the dangerous political views expressed in parts of the Appendix. In *The Gardeners' Chronicle* J. C. Loudon, who dominated the horticultural press, noted Matthew's discussion of species. There the matter rested until *The Gardeners' Chronicle* published in 1860 a review of Darwin's *Origin* which prompted a long comment by Matthew in which he pointed out that he had already published in 1831 the conclusions which Darwin had reached after 20 years investigation and had applied them in his practical forestry. Darwin, in his reply, freely acknowledged that Matthew 'had anticipated by many years the explanation which I have offered of the origin of species, under the name of natural selection'. He excused his ignorance of Matthew's views by reason of their brevity and their inclusion in an appendix to a book on Naval Timber and Arboriculture.

Upon these foundations the author has built a considerable edifice in which he sets Matthew's contribution in the context of ideas about evolutionary change and species in the early nineteenth century. He considers the influence of Buffon, Cuvier, St Hilaire, Erasmus Darwin, Lyell and Lamarck, who did so much to establish the idea of gradual change, but who has been historically underrated because he was wrong about acquired characters. He notes that Darwin's pangenesis did not prove such an albatross. Chamber's *Vestiges of Creation* (1846) brought the general notion of change over time in both the organic and inorganic world to a wide audience, although how this was to be effected was left to God. A great deal of research has gone into the discussion of the origin of evolutionary concepts. This should interest the historian of science, not only because some of the material will be unfamiliar but also because of the approach. The author, who has no sympathy with what he terms the 'Darwin industry', loses no opportunity to present people like Darwin and Huxley, either as thinkers or persons, in such an unfavourable light that his obsessive denigration may alienate the reader.

Admittedly there is a great deal of interesting material in this book but it is not easy to read. It is too long and suffers from a repetitive and rambling style. The author has an axe to grind; he was encouraged to write the book by the Trustees of the Patrick Matthew Trust and he has done more than justice to his subject. Various philosophical statements by Matthew are seen as premonitions of much later ideas or discoveries. For example his 'protean principle of life capable of gradual circumstance – suited modifications' is equated with DNA. If the author had not gone to such lengths to inflate his hero's role in the development of evolutionary theory he would have won more respect for him as an original thinker. But when all is said and done, at least we shall remember Patrick Matthew as

the man who pipped Darwin and Wallace on natural selection by about thirty years.

FORBES W. ROBERTSON
Edinburgh

Correcting the Blueprint of Life – An Historical Account of the Discovery of DNA Repair Mechanisms. By ERROL C. FRIEDBERG. Cold Spring Harbor Laboratory Press, 1997. 210 pages. Price $49.00 (hardback). ISBN 0 87969 507 2.

History of science can be written in terms either of scientific ideas and experiments or of personalities and inter-personal relationships. Errol Friedberg, who has himself worked on DNA repair mechanisms over the last 30 years or so, gives us both approaches. Starting with Kelner's discovery of photoreactivation (in 1948, before Watson and Crick) and its subsequent explanation in terms of DNA, he proceeds in successive chapters through excision repair, other modes of damage removal, correction of base-pair mismatches, and finally the SOS response of *E. coli* to ultraviolet radiation. The history does not extend much beyond the 1970s, and is thus almost entirely about bacteria and bacteriophages, excluding the more recent explosion of work on yeast repair systems (reviewed by Prakash *et al*. in the *Ann. Rev. Genetics* of 1993).

Each chapter quotes extensively from correspondence and conversations, and we learn a good deal about the careers and motivations of the researchers involved. The main characters are Kelner, Delbruck, Luria, Dulbecco, Goodgal, Rupert, Setlow, Howard-Flanders, Cairns, Lindahl, Meselson, Witkin and Radman. Miroslav Radman, the main discoverer of the SOS system, gets special attention, and I hope he will be happy with the somewhat back-handed eulogy that Friedberg bestows upon him. I have one correction to make. The late Cy Levinthal appears only in a peripheral role, but it still seems important to get his name right; he was Cyrus, not Cyril as alleged here.

My main criticism of Friedberg's well-researched and informative book is that it tends to fall between its two stools. Its purely scientific side is sufficiently good to make one sorry that it was not done better. The anecdotal digressions and discursive style make for readability but not for a clear presentation of the development of the science. References to important papers are to be found scattered among the notes at the back of the book, but these are not really an adequate substitute for a proper bibliography. There are some good photographs of many of the people involved, but only one explanatory diagram. And there are no subheadings to act as signposts within the mostly rather long chapters. The scientific story is there, but it takes some effort to extract it.

Those who like their science history to be about struggles for supremacy may be a little disappointed in this book. Apart from two disagreements about priority (Kelner and Dulbecco, Setlow and Howard-Flanders), both settled more or less amicably, little personal conflict is recorded here. Nor will followers of Thomas Kuhn find much last-ditch defence of established paradigms. The author himself implies that an entrenched belief in DNA stability retarded research on DNA repair, but does not give us much evidence in support of this view. It seems, rather, that once the genetic role and structure of DNA had been established, the study of DNA repair proceeded, certainly with surprises and complications, but without profound disagreements. In Kuhn's terminology it was fairly normal science, but it was, and remains, exciting and important for all that.

J. R. S. FINCHAM
Division of Biological Sciences,
University of Edinburgh