

destroyed our wild animals in Europe and North America, and took their land; we even make newspaper headlines of the escape of a lion from the zoo and public peace comes back only when it is known that the animal is shot. Why should we expect poor farmers of Africa to act differently?

Dubos stresses that they will not, that people do not like wilderness, that we may save some areas as places for adventuring or because it seems sense to keep the gene pools of wilderness species as intact as possible, but that we ought to replace most wilderness with something even better.

Dubos is asserting that there is a silent majority of people who want a friendly, humanized Earth to live on — not danger, not wilderness, not even what is productive or the result of good ecological management, but what is familiar or beautiful. Doubtless he is right, and doubtless the silent majority will have its way. I worry, though, at the tyranny of majorities to those few who, people-like, do not always run with the herd. A real wilderness is marvellous in ways that Thoreau could never feel. A desolate mountain top in the Brooks Range is a better place to be than a man-made space above the English Lakes. To stand, thinking yourself into invisibility in the Amazonian forest, alone and as the hours pass, cannot be equalled in a botanic garden or in the landscapes of a potentate. A proper wooing of Earth will see that she keeps bits of her wild temper intact.

Dubos tries to show that what people like is what they were programmed to like in ancient days when our species was fashioned in some forgotten African savanna. These parts of the book make uncomfortable reading. We are the learning species, the animal who learned to live in almost every habitat of the Earth before we discovered, through agriculture, how to transform our habitats. Yet Dubos sees the Peking Summer Palace as an attempt to recreate the supposed savanna of our species' youth. This has a feel of genetic determinism about it which is not quite nice. The essential thesis of wooing the Earth can stand without this.

Nor does the Dubos thesis need bolstering by ecological theory, yet his is a conservation ethic of sorts and all conservation nowadays argues before the "Court of Ecology". Dubos would have done better not to, for his is still the ecology of the environmental movement with the standard errors.

It is not a fact that ecosystem complexity yields stability. This was a hypothesis of the 1960s, built out of inspired speculating by Robert MacArthur (*Ecology* 36, 533–536; 1955) who drew mathematical analogies between junctions in information networks and species in ecosystems. By the mid-1970s, R.M. May (*Stability and Complexity in Model Ecosystems*; Princeton University Press, 1974), D. Goodman (*Q. Rev. Biol.* 50, 237–266; 1975) and others had shown how unrealistic this

was and many of us ecologists felt grave disquiet years before then. Modern conservationists appealing to ecological Courts really must read up the latest Court Decisions before applying their ecological law.

It is dangerous practice to talk about how ecosystems "evolve"; change they do, but to talk of them "evolving" suggests a mechanism of selection, of the stable replacing the unstable, the productive the unproductive. There are indeed ecologists who seek supra-organismal patterns of evolution like these — but they are not all of the profession and they may not be the wave of the future. Far better to remember that most of the energy flux used to drive an ecosystem is degraded in the non-living portions. In temperate forest some 0.7 per cent of incident solar energy enters the biota through photosynthesis and the other 99.3 per cent is degraded by raising temperatures or evaporating water (H. Lieth and R. H. Whittaker. *Primary Productivity of the Biosphere*; Springer-Verlag, 1975).

The biota adapt to this energetic reality much more than they control it. Progress towards a nebulous "climax" is a shuffling in line before the energy soup-kitchen, until everyone has found a place of sorts and there is a quasi peace. Incidentally, this is why our human systems have, in general, worked so well; we have altered the arrangement in the biological energy queue without tampering with the main patterns of energy flux. It is only when we do

something terrible to the landscape, such as promoting soil erosion or building a dam, that major diversions in primary energy flux reflect true system instability.

Dubos has a wise and sane vision — an Earth kept diverse and pleasant for human use, made richer by human invention than wild nature had left it. Leave some wilderness for some of us to spend a few hours in and change the rest, ever so gently, until it is good. Yet he protests how well he understands that all will be vain if our numbers keep on growing and that much will be imperilled if we go to nuclear war. But on the ecological subject of human numbers he has nothing to say. He believes that the remorseless rise of population has something to do with curbs in the death rate made by the medical profession and he savours the hope that our numbers may be levelling off. These are views not tenable in ecological logic. Numbers rise because the human breeding strategy remains that of achieving optimum clutch and the destructions Dubos fears will come about as we seek to find adequate niche-space for the surplus individuals that result (P.A. Colinvaux *Nature* 26, 256–357; 1976. *The Fates of Nations*; Simon and Schuster).

Yet it is good to read honest statements that all things done by mankind to the environment are not bad; indeed that most things done have had satisfactory results.

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Survival of the fittest author

Wilma George

A Delicate Arrangement: The Strange Case of Charles Darwin and Alfred Russel Wallace. By Arnold C. Brackman. Pp. 370. (Times Books: 1980.) \$12.50.

THIS is a disappointing book. It promises to uncover a plot but fails to convince and fills up pages with incomplete biography. Many people would agree with the author that Alfred Russel Wallace had less than his fair share of publicity but few would agree with his interpretation that Darwin cheated Wallace and that Lyell and Hooker connived at the plot.

Brackman rests his case on dates: the supposed date that Wallace sent his natural selection paper to Darwin and the supposed date that Darwin received it. Wallace thought he sent it in March 1858 and Darwin thought he received it on 18 June but Wallace's envelope with its postmark has not survived. Brackman claims that because Wallace posted a letter to F. Bates on 9 March which arrived on 3 June he must have posted his article to Darwin on the same date and, therefore, the article must have arrived on 3 June.

This is not evidence. This same "evidence" has been used by H. Lewis McKinney in *Wallace and Natural Selection* (Yale University Press, 1972) to claim that Wallace cheated (to say he was where he was not).

Brackman claims that Darwin cheated by saying he had received Wallace's article on 18 June (and not 3 June) in order to quarry it for data and ideas before writing to Lyell. The "evidence" is supported by the fact that Darwin did not keep letters during the 1850s — and that Wallace was a member of the lower classes.

Brackman implies that Charles Darwin or his son Francis destroyed letters because Wallace was writing to Darwin about his ideas on species. But Darwin did not keep letters systematically until he became famous. Brackman also implies that Darwin was *surreptitiously* using Wallace's 1855 paper on species when the 1855 paper was in print!

Brackman's narrative style is "surreptitious", to say the least: "A troubled Darwin opened Wallace's letter". In fact, it is downright dishonest: "For Darwin his success in securing Wallace a

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Wallace, aged 30 . . .

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. . . and at 46.

measure of security for the remainder of his natural life was an act of expiation". It is a dramatic language: "Darwin understandably panicked". And the reader might understandably panic, too: "Both letters exhibit guile, glibness, and guilt mixed with integrity and honor".

Darwin was distressed when Wallace's 1858 article reached him but distress does not prove guilt. There is no evidence that Darwin plundered Wallace's paper for ideas and wrote them into his own manuscript. Darwin had been brooding on his theory for years and had not written his book and I find his outcry about loss of priority entirely natural. Darwin did not seize on Wallace's idea of divergence and make it his own in the supposed two-week interval between receipt and admission of receipt of Wallace's 1858 article. In 1855, Wallace had written in a paper appearing in the *Annals and Magazine of Natural History* (16, 184-196): "But if two or more species have been independently formed

. . . then the series of affinities will be compound and can only be represented by a forked or many branched line". As for the mechanism, Darwin had written to Asa Gray in 1857 (and the letter is not a fake) of: "an unerring power at work in *natural selection* (the title of my book)" and of a principle of divergence "which shows that organic beings seem to branch out into all the places available".

I find no case against Darwin. I think that the explanation for Wallace's partial eclipse from the evolution-by-natural-selection debate is less dramatic. Darwin's fame came not from the appreciation of a new biological theory by a few members of the Linnean Society but from the intelligent reading public. Evolution by natural selection was a revolutionary theory but the public of Victorian England did not read the proceedings of the Linnean Society. It read books. And the readers, shattered by the convincing mechanism for a process which was already a subject of conversation, were troubled by the implications of the theory for the status of man.

Wallace did not write an *Origin* (it was Darwin who provided a detailed account of the process of organic evolution which was accessible to the reading public) and Wallace developed unacceptable ideas about the evolution of man. Finally, Wallace, the younger man by 14 years, insisted that their theory should be called Darwinism, summing up the situation himself when he wrote to Darwin in 1864:

Animal arms races

John R. Krebs

Tooth and Claw: Defensive Strategies in the Animal World. By J. L. Cloudsley-Thompson. Pp.252. (Dent/Biblio: 1980.) £9.95, \$22.50.

THIS is a popular book about the ways in which animals defend themselves against predators and the counter-adaptions of predators to overcome their prey. It covers similar ground to *Defence in Animals* by M. Edmunds (Longmans, 1974) and *The Ethology of Predation* by E. Curio (Springer-Verlag, 1976), but is aimed at a more general audience.

Most of the text consists of examples, many based on the author's own observations of different kinds of anti-predator adaptation and tricks used by predators to catch their prey. Examples of camouflage, mimicry, venoms, defensive spines, armour and so on are described in a charming and entertaining way. Most readers are certain to come away with at least one or two good coffee-time stories. My favourite is about the African water mongoose, which is alleged to catch birds by sticking its rear end in the air and distending its anus to make it look like a

You had worked it out in details I had never thought of, years before I had a ray of light on the subject, and my paper would never have convinced anybody or been noticed as more than an ingenious speculation, whereas your book has revolutionised the study of natural history.

But I am glad to see a book that publicizes Wallace's contribution to the theory of evolution by natural selection, a book that reprints both the 1855 and 1858 species papers — though it is a pity that Darwin's 1858 contributions were not included for the reader to make his own judgement — and a book that has some hilariously scabrous comments in the "Author's Notes". Wallace's name is heard today much more often than it was in the nineteenth century, partly because of the lessened obsession with the evolution of man and the increased interest in other implications of the theory, partly owing to the work of the historians of science and partly because of the return to fashion of evolutionary zoogeography founded by Wallace and stamped indelibly with his "line". But the more Wallace is linked with Darwin as a co-founder of the theory of evolution by natural selection, the more will historical accuracy be served. Thus, even a negative book like this contributes to the subject. □

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ripe fruit. Along comes an unsuspecting bird to peck at the fruit and with a smart about turn the wily mongoose snaps it up! Second place goes to the story about frogs' legs and the genitalia of French soldiers in nineteenth century Algeria (without giving too much away, the British are right to stick to pie and chips), and a close third is the account of how large tropical scolopendromorph and scutigermorph centipedes escape from attackers. When attacked or frightened, they shed one of their back legs; the automatized leg leaps around making loud creaking or stridulating sounds to attract the attention of the predator while the nonagintanovipede slips silently away.

Cloudsley-Thompson also tries to extract some general principles about coevolution of predators and prey. He draws a parallel between military arms races and the coevolutionary race between predators and prey. In both military and coevolutionary races, adaptations or innovations by one party call forth counter-adaptations by the other. These counter-adaptations lead in turn to counter-counter-adaptations and so on. One general principle applicable to both kinds of race is the concept of a "trade off" between competing demands. Heavy armour may render a tank less vulnerable