"A contrived document, this photograph is frankly described in the Torres Straits Expedition reports. Not only does the caption acknowledge that lines have been drawn on the photograph to highlight features of the body painting traditionally applied to a woman who has just passed through her puberty rites (which are so obvious that they could not pass unnoticed), it also reveals that the costume she is wearing was especially made to suit . . . photographic requirements." [From *The Savage Within*]

anthropology as a legitimate and independent science (namely by the formation of Section H for anthropology); and coinciding with this event was the ensconcement of E. B. Tylor (1832–1917) at Oxford, where he was successively appointed keeper of the University Museum, reader in anthropology (1884), and full professor (1896). Although in retrospect Tylor's promotions were an important factor in securing a position for anthropology in the British academic landscape, the future of the discipline at the time was far from certain, as Kuklick explains.

It is against this general backdrop that Kuklick examines in considerable detail the nascent social structure of British academic anthropology. Aspects of this period in British anthropology have been covered by George Stocking in his *Victorian Anthropology* (1987), but Kuklick manages to cast a fresh light on what may appear to many to be well-trodden territory, while at the same time orchestrating a wealth of biographical and institutional information she has gleaned from archival sources and primary literature. To the nonspecialist reader (namely those of us who are not cultural anthropologists), the opening chapters of her book are relatively straightforward, but as the focus shifts to the 20th century many of Kuklick's arguments, and more particularly the generalizations she derives from them, presume an intimacy with anthropological theory that many of her prospective readers may not have.

To put Kuklick's argument briefly and simplistically, the orientation of British anthropology prior to the First World War had been dominated by the evolutionist viewpoint (represented by Tylor for one), which was committed to the notions of progress and directionality in human history. Ultimately these and related ideas coalesced into a theoretical lens through which Western civilization was viewed as the standard by which all other cultures should be judged—a perspective that clearly was not at variance with either the task of managing an expanding colonial regime or that of monitoring the benefits of social reform at home. But after the mayhem of 1914–18, the enthusiasm for evolutionist schemes and their extolling of Western civilization was palpably diminished, and in some quarters of the British intelligentsia even extinguished. It was in this context that the political and theoretical orientation of British anthropology shifted dramatically away from evolutionism to functionalism under the initial influence of Bronislaw Malinowski (1884–1942) and later of A. R. Radcliffe-Brown (1881–1955). In direct contrast to the evolutionists, the functionalists had little or no interest in history. Rather, they were far more interested in understanding how individual societies operated. In a nutshell, they envisioned themselves and their science as being of greater practical service. But, contrary to expectations, the functionalists found themselves cast in the role of cultural critics, with their ethnographic findings being of little practical value to their colonial mentors, who by and large were still operating under the illusion that the world had not changed. The structural-functional approach, however, continued to dominate British anthropology until well after the Second World War and the transformation of the British Empire into a commonwealth of former colonies. In recounting these developments Kuklick gives a relatively detailed and balanced review of the earlier influence of A. C. Haddon's (1855–1940) multidisciplinary expedition to the Torres Straits in 1898–99, and in particular of the frequently overlooked pivotal role of W. H. R. Rivers (1864–1922).

Although Kuklick initially deals with anthropology as it was and is formally observed at Oxford (and later at Cambridge)—namely, as a tripartite discipline—she becomes in the course of her book increasingly preoccupied with the concerns of cultural anthropology and neglectful of developments in archeology and physical anthropology. The absence of any substantive discussion of the activity during the inter-war years of such workers as V. G. Childe, H. J. Fleure, A. Keith, G. M. Morant, and M. Tildesley is regrettable. And it should also be noted that this study examines the developments in British anthropology without recognition of any external influences in the form of intellectual exchange between British and Continental and American anthropologists. The book is nevertheless of value and will undoubtedly be of considerable service in stimulating discussion in graduate seminars as well as providing food for thought in cultural anthropological circles.

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Questions for Selectionists

Natural Selection. Domains, Levels, and Challenges. GEORGE C. WILLIAMS. Oxford University Press, New York, 1992. x, 208 pp., illus. $55; paper, $24.95. Oxford Series in Ecology and Evolution, 4.

When scientists agree on central concepts, a field comes of age—think of the laws of motion in Newtonian physics or the chemical bond and the nature of heat in chemistry. In evolutionary biology, according to Williams, the central concepts are natural selection, mechanism, and historicity. Evolutionary biologists are still hard at work on foundations; this field is still coming of age. Williams's book judges the progress achieved, states the issues not yet resolved, and takes a clear stand on controversial points.

It is not the first time. In 1966, Williams published a book with the title *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought* that became a classic. It is still widely read and widely recommended to students. This new book could appropriately carry the same title. Comparing the two measures a quarter-century of progress in evolutionary thought.

In 1966, Williams was concerned to make clear that natural selection acts on genes, not on species; to destroy fuzzy-headed thinking about group selection; and to call attention to the central roles of life histories and sex. He succeeded fully. No one entertains seriously any more the sort of group-selectionist thinking that was common before Williams, together with Ghiselin and Maynard Smith, made his critique;

much impressive progress has come from single-minded concentration on genes as units of selection and the conflicts that arise among them from different patterns of transmission; and many books and thousands of papers have been written on life history evolution and the evolution of sex. Williams did not do it alone, but his influence was great, and it was a hard act to follow.

Has he done it again? He has, and more.

Williams starts by distinguishing clearly between replicators and interactors: replicators consist of information, interactors of material stuff. The distinction is not new; the power and precision with which Williams applies it are. He then defines the things on which natural selection can work: any pieces of information whose history can be represented by a dendrogram, including but not limited to genes. Thus selection among clades is in principle possible and a plausible explanation for the taxonomic distribution of sexual reproduction, semelparity, and small clutches. Williams applies his dendrogram criterion ruthlessly to destroy the notion that natural selection could operate on units like species, on trait groups, or on symbiotic associations like lichens. He suggests how to weigh the relative power of selection within and among gene pools: compare rates of turnover of genes with rates of turnover of gene pools, not rates of death of individuals with rates of death of clades. Here Williams updates his 1966 critique of group selection to deal with recent developments.

Williams is not afraid to stick his neck out. The comparative method is currently popular. It combines phylogenetic systematics with new statistical methods to attack questions suggested by comparing traits in related species. Williams takes issue with several of the developing dogmas. Rather than judge adaptation by comparison, he prefers the criterion of conformity to design specifications as determined, for example, by an optimality model. Rather than count the number of independent phylogenetic events, he poses an implicit question: "I am unaware of any study of the logical differences among phylogenetic independence of gene pools, phylogenetic independence of gene lineages within a single gene pool, and developmental independence of different individuals in a population or clone" (p. 103). His challenge is useful; it stems from the dendrogram criterion, a sharp tool that can cut deep.

Most stimulating is the discussion of outstanding paradoxes. Williams is skeptical about current explanations of leks, of female pheromones, and of helpful stress; he has little use for species considered as individuals or as units of selection; he rejects the peripheral isolate theory of speciation. He wonders why body temperature does not vary more in birds and mammals, why vertebrates that live in the ocean (except the Agnatha) do not maintain higher electrolyte concentrations, why there are no viviparous birds or turtles, why we blink simultaneously (and thus are blind 5 percent of the time).

At a few points, he misses important issues. He does not discuss the context-dependency of genetic information. The meaning of a given DNA sequence varies wildly depending upon the organism, tissue, and cell in which it finds itself. "Hier," as a string of letters, means "here" in German and "yesterday" in French; there is information in context. Information resides not only in genes but in the materials out of which organisms are built, as Alberch and Oster have convincingly shown. Thus some of the information in the context that gives genes their meanings does not reside in other genes. Evolutionary biology has not yet digested the implications.

In discussing genetic conflict, Williams neglects to mention a method of resolving conflicts. Consider a cytoplasmic element, like a feminizing bacterium. Its interest is always to occur in a female, through whose eggs it can be vertically transmitted, and to that end it feminizes its host. However, as an evolutionarily stable strategy it is often in the host's interest to allocate sex equally to sons and daughters. Thus there is a conflict of interest between host and parasite. This conflict can be resolved if the cytoplasmic element is incorporated into the host's nuclear genome, as appears to have happened in a pillbug, where the element now functions as part of a sex chromosome—a brilliant solution. Such stable and durable conflict resolution may also account for the incorporation of genes for essential mitochondrial functions into the nuclear genome.

Though he does not discuss fitness directly, Williams does make comments on "success" that leave me puzzled. He judges success not just by counting the number of copies of information present but by measuring the amount of material stuff on which it imposes pattern. To me, evolutionary success is survival up to now; whether a piece of DNA imposes pattern on a nanogram of *Escherichia coli* or a ton of elephant is beside the point. I await clarification.

This book constructively critiques central evolutionary ideas. It should be published with the 1966 book in one volume. Together they make a devastating, and exciting, read.

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Genetics by Nation

The History and Development of Human Genetics. Progress in Different Countries. KRISHNA R. DRONAMRAJU, Ed. World Scientific, River Edge, NJ. 1992. xii. 303 pp., illus. $86. Based on a symposium, Washington, DC, Oct. 1991.

J. V. Neel in his introduction to this volume cites a number of reasons why human genetics has become so active in recent years: the advent of biochemical and molecular genetics; new methodologies in cytogenetics; development of somatic cell genetics; availability of high-speed and high-capacity computers; concern over environmental (radiation and chemical) mutagens; absence of infectious and nutritional diseases in developed countries; interconvertibility of genetic knowledge between various species; availability of funding; and the possibility that with all these developments interested geneticists who previously avoided the field ("closet" human geneticists) could now make scientific contributions.

The book consists of papers presented at a satellite meeting of the 1991 International Congress of Human Genetics. Though it includes a few substantial historical analyses, most of the contributions are limited to reporting names and trends in various countries, including Canada, France, Italy, Hungary, Japan, Israel, Egypt, Chile, and Brazil. No attempt was made to cover recent developments in the United States. The British contributions are limited to an analysis of Fisher's and Haldane's work with special reference to blood groups. Haldane's impact on human genetics in India is covered in a separate chapter by the editor.

A substantial article by E. A. Carlson on the contributions of the pioneering United States geneticist H. J. Muller to human heredity is particularly interesting. Mentioning the eugenic past of human genetics, Carlson refers to the field as a "lotus that emerged from a dung heap" and compares its history to the antecedents of chemistry in alchemy and of astronomy in astrology. No other papers in the volume deal with eugenics and its relation to human genetics, which were discussed in another symposium at the congress.

The history of the active field of human gene mapping is covered by N. E. Morton in characteristically iconoclastic manner. Considering extensive current attempts to identify human genes in complex non-Mendelian diseases by linkage, he points out that "no linkage test in man has detected a major locus not defined by segregation