markings are clearly ambiguous stratigraphically (see Figure 6.4, page 61). Exactly when during the Kachemak period (ca. 1500 B.C.–A.D. 1100) people first occupied the Uyak site is unknown. We do know that the site was unoccupied when Russians first contacted (1763) and settled (1784) Kodiak Island. Other settlements on Kodiak indicate occupation for 6,000–8,000 years and raise the possibility of a southern center for Neoeskimo development.

The main deficiency in the book results from inadequate editorial cross-checking of dates. An inordinate number of dates for events associated with the Larsen Bay case are given inconsistently in various places in the book, most conspicuously the actual date of reburial of the skeletal remains at Larsen Bay, which is consistently given as October 5th, 1991 (Saturday) with the exception of the volume editors who claim it was October 6th!

In the context of repatriation, P.L. 101-185 and P.L. 101-601 make it perfectly clear that the interests of scientists and the public at large no longer outweigh the concerns of Native Americans concerning their cultural heritage. The Larsen Bay case provided more than its share of painful lessons, lessons that the anthropological community can learn from and from which it can derive new cooperative and mutually beneficial policies toward valid repatriation requests. As I was leaving the AAA Symposium in San Francisco I scribbled a note in the margin of the revised schedule of papers given to the attendees. It said: "He [Pullar] convinced me to support repatriation" of the Uyak collection. Nothing presented in this book alters my conclusion.

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HUMAN BIODIVERSITY: GENES, RACE, AND HISTORY. By Jonathan Marks. New York: Aldine de Gruyter. 1995 321 pp. ISBN 0-202-02032-0. $23.95 (paper).

This book contains some genetics, a lot about race, but too much history. The book has two general themes: first, the history of ideas about genetic or biological variation and especially about the concept of race; and second, current knowledge and interpretations of genetic variation. Although the author attempts to show the relevance of the two themes to one another, I don't think that he succeeds. The ideas of Linneaus and Bouffon are of little use in understanding and developing current models of genetic variation. Marks disagrees, of course, and according to the blurb on the back cover, the tracing of ideas about race through the centuries is a major contribution of the book.

Marks begins by stating that most ideas in science are wrong, and it is incumbent upon scientists to learn from them. This is comparable to Santayana's overworked dictum that "those who cannot remember the past are condemned to repeat it." Thus, much of the book is a recital of an exhaustive list of the wrong ideas that have been advanced through the years about human genetic variation. This makes for a lot of negative reading and leads frequently to the response, Who cares? As the study of human genetic variation has developed as a science, it is irrelevant whether or not Sewall Wright supported the eugenics movement; his enormous contributions to the genetic models of evolution are basic and will continue to be so regardless of his politics.

The book begins with a brief history of the development of our knowledge of man's place in nature. Next, there is a short introduction to cladistics as applied to the primates. Gene pools, microevolution, and macroevolution are briefly described and an analysis of some aspects of human evolution follows. With the exception of a later brief discussion of the Eve hypothesis and genetic phylogenies, there is no further consideration of the fossil record, cladistics, or human evolution.

The next five chapters are the core subject of the book, the history of biological anthropology and ideas about human variation. Hooton gets off lightly and is even credited

with introducing the concept of polymorphism, but it certainly has little relevance to the use of polymorphism in genetics. On the other hand, Coon is castigated as "naïve" and "absurdly archaic" (p. 105). Coon's *Origin of Races*, or more specifically the considerable criticism of it, is considered to have resulted in a paradigm shift from typology to adaptation. I think he has Coon on the wrong side of his paradigm. An earlier book, Coon, Garn, and Birdsell's *Races: A Study of the Problems of Race Formation in Man*, together with the discovery of the association of the sickle cell gene and malaria, were more important in the increasing recognition of natural selection and adaptation as important determinants of racial variation in contrast to the non-adaptive concept of race employed by Hooton. Coon's *Origin of Races* is dedicated to Weidenreich, and both can be considered as ancestors of the multiregional hypothesis, which emphasizes evolution and local adaptation. Coon in his later career was keenly interested in natural selection in humans and was part of the new paradigm. The uproar over Coon's book was mainly based on his assertion that Africans evolved into *Homo sapiens* later than other groups. This was based on his acceptance of a very recent dating of the Broken Hill skull, which was later disproved.

Marks has also included an increasing recognition of the "social import of the scientific endeavor as it involves humans" in explanation of the paradigm shift. As he says, "The science of humans is simply political and value-laden in ways that the science of say, fruit flies is not" (p. 56). Coon's reply to Dobzhansky's charge that the duty of a scientist is to prevent misuse of his work is labelled naive and archaic because Coon didn't think he had to defend his work or show how it was politically correct. This exchange occurred over the use of Coon's *Origin of Races* to support racial segregation, but Coon's contrary position stemmed from the AAPA's earlier condemnation of Carleton Putnam's book, *Race and Reason*. Coon thought the Association had no business passing resolutions on what he considered to be unknown facts. In his autobiography he says he had read Putnam's book and had seen nothing "actionable" in it. Marks misquotes this statement as "seeing little worthy of objection" in it (p. 57).

Dobzhansky levelled the same criticism at me for my statement, "There are no races, there are only clines," which he said "plays into hands of race bigots." Marks also criticizes my statement as overstating the case in a significant way because races exist as social categories. Races are also mill streams and running contests, all of which are irrelevant to the development of models to analyze and understand genetic variation among human populations. Furthermore, as a member of the group labelled by Marks the "earlier generations," who "by focusing on the hereditary differences between populations, had defined for themselves a relatively trivial biological problem" (p. 133), I am somewhat chagrined to learn that my research is trivial.

Finally, beginning with chapter 8, or more than halfway through the book, basic genetics are introduced. The first such chapter is a short account of what genes are and how they change (one of these processes he labels *correction* when I think he means *conversion*). He uses hemoglobin as an example, which is reasonable since we know more about these loci that any others. The sickle gene is said to attain population frequencies of over 25% in West Africa. It doesn't. It is also stated that the haplotype variants of the beta-S gene are due to separate mutations, but they could as likely be due to gene conversion. The Senegal and Cameroon haplotypes have been found in India and the Middle East, so increasing data require many more mutations than now seem plausible by that process alone. He also states in a later chapter that the hemoglobin variation is recent (it is) and is due to the development of irrigation (it isn't). There is little irrigation agriculture in the tropical forest regions of Africa where the sickle cell gene frequencies are high.

After a short discussion of the Eve hypothesis and other phylogenies, the analysis of genetic variation is primarily done with a series of maps of eye color, skin color, two traits of the first principal component from Cavalli-Sforza and his associates' work, and of the sickle cell gene in Africa. Only the first has any explanation of the scale and it

has a mistake in the legend. The sickle cell gene map is also rather inaccurate.

The last four chapters contain a pastiche of topics that seem to be an attempt to mention most current hot topics. One contains a brief discussion of human demography, including fertility rates, while another briefly discusses some genetic disorders restricted to a few populations, Tay-Sachs' disease, cystic fibrosis, and, of course, AIDS. There is now more significant evidence from mice and men for cholera and other diarrheal diseases being less severe in cystic fibrosis than he states, but this is one of those trivial biological problems we old fogies are concerned with.

Finally, many of the current problems of human behavior and its genetic component are discussed. Sex, rape, homosexuality, aggression, IQ, and even playing basketball are considered, if only to deny a genetic component.

As I stated at the beginning, this is really two different books. However, its presentation of current knowledge about human genetic variation receives many fewer pages than the history of ideas about race. Human genetic variation is obviously not the author's primary interest, its treatment somewhat superficial, loosely organized, and thrown in as necessary. Restriction fragment length polymorphisms (RFLPs) are discussed in the section on hemoglobin and then defined and described later. There is a very short appendix on DNA which could easily be incorporated in the text, particularly since it uses as an example the alpha hemoglobin gene which is discussed in the text.

The major focus of the book can be seen in the conclusions of the last chapter. Beginning with a critique of an editorial in *Science* suggesting a genetic component to criminal behavior, all of the mistakes, racism, hereditarianism, confusion of biology and culture, and even neutrality in science are summarized. The final sentence is, "But it is tempting to commit those mistakes again and again" (p. 277).

Similar moralistic pronouncements are made in just about every chapter in the book, and morality is more important than scientific theory and data. This is shown by the absence of any equations and rather few numbers. These are the language of science, and there is nothing value-laden about $q = S1/(S1 + S2)$ any more than there is in $F = ma$. But if you like being preached to, read this book.

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RACE, EVOLUTION, AND BEHAVIOR: A LIFE HISTORY PERSPECTIVE. By J. Philippe Rushton. New Brunswick, NJ: Transaction. 1995. 334 pp. ISBN 1-56000-146-1. $34.95 (cloth).

Race is in the public eye again, and once more biological anthropologists must address problems with racial taxonomy and related misapplications of evolutionary theory. Rushton's book focuses on "racial" variation from an evolutionary perspective. His basic thesis is that race differences in behavior are explainable from the viewpoint of life history analysis, particularly the difference between *r*- and *K*-selected evolutionary strategies. According to Rushton, modern humans appeared first in Africa roughly 200,000 years ago and, being the "oldest," are the most *r*-selected. "Caucasoids" come next, followed by "Mongoloids," who are the most *K*-selected. These "later" groups are said to be more *K*-selected because they encountered "more challenging environments" (p. 7), particularly in Asia. Most of the book is devoted to describing data supporting this view: Asians, the most *K*-selected, have the largest brains, the highest IQ scores, the fastest reaction times, the latest ages of maturation, the smallest genitalia, and the lowest frequency of sexual intercourse (among other characteristics).

Rushton's model is faulty at many points. I focus here on those areas most directly related to biological anthropology, notably the definition and evolutionary meaning of race,