Soc. Sci. Med. Vol. 31, No. 8, pp. 891–912, 1990
Printed in Great Britain.

0277-9536/90 $3.00 + 0.00

SCIENTIFIC RACISM: REFLECTIONS ON PEER REVIEW, SCIENCE AND IDEOLOGY*

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Abstract—The scholars who use *Social Science & Medicine* in their research and teaching, who publish their work in it, participate in its peer review of manuscripts, and attend its conferences belong to various nationalities, disciplines, and cultural traditions. Our common enterprise originated in and depends upon liberal democratic social institutions, and assumes their values. With all our differences and disagreements, we are committed to scientific research in a common effort to improve human health and welfare. Our professional careers are a large part of our personal lives, so that our science, our lives, and our values are a single fabric. The present lecture is a meditation on this situation based upon my own heritage, personal experience, and career in anthropology, and on the recent publication in *Social Science & Medicine* of an essay that attributed the epidemiology of AIDS to racial variation.

Key words—racism, AIDS, peer review

My topic is the community of social scientists and the civilization that maintains it. I will be anecdotal throughout this lecture, so let us begin with a recent event: *Social Science & Medicine* has just published an article that seems to me to be transparent racist pseudo-science. The article is 'Population differences in susceptibility to AIDS: an evolutionary analysis,' by two psychologists at the University of Western Ontario, J. Philippe Rushton and Anthony F. Bogaert [1]. The problem that I want us to consider is why Peter McEwan and the scholars he asked to evaluate the manuscript did not consider it to be transparent racist pseudo-science? Peter told a Canadian newspaper reporter who phoned him about the matter that the manuscript was read by a sociologist, a psychologist and a physician, and that "The reviewers and I share the view that the case was sufficiently respectable scientifically to merit publication. We are open to all shades of opinion. The only thing we require is that the material be of sufficient high quality" [2].

Before I read the essay, or had even heard of it, I got a letter from a Canadian anthropologist returning a manuscript on Africa that he had agreed to evaluate, and saying that he would not act as a reviewer for a journal that published Rushton's work. He enclosed a clipping from a Toronto newspaper that reported the interview with Peter I have just quoted. This seemed to me to be a bit hot-headed, but I answered immediately saying that I would send copies of his letter and my reply to Peter. I told him that if the article was, as reported, about racial differences, then Peter should have sent it for evaluation to an anthropologist or biologist who specialized in research on human evolution and the genetics of racial variation. In any case, I reminded him that peer review is often imperfect, and that he did his Africanist colleague a disservice by refusing to review his entirely unrelated manuscript.

In the following days I received long distance phone calls from two Advisory Editors for the journal who were outraged that it was publishing an article by Rushton, though neither one of them had read it, and were responding to discussions with colleagues in Canada. I was surprised that someone I had never heard of was so infamous, but argued that no matter how bad the article might turn out to be, my callers were wrong to assert that it would corrupt the people who read *Social Science & Medicine*. Nevertheless, I agreed to fax the letter I had mailed to Peter, with a note asking whether publication could be reconsidered. I did not realize that the essay had already been published.

Fax is a wonderful technology. Peter's answer arrived within twenty-four hours.

> With regard to the Rushton paper, it is too late to prevent publication even if we wished. The paper was accepted because following two extensive revisions it presented a case that, however contentious, justified consideration. I am guided by the principle that there must be no sacred cows—all reasoned argument has the right to enter the general arena of discourse even if at times this provokes outrage in one quarter or another.... It was obvious from the moment of its arrival that Rushton's paper dealt with a highly sensitive issue and his writings have been the subject of heated controversy in several other places, but this was no reason for evasion. Critics should address the substance of the paper rather than its publication; the weaker they believe it to be the easier should be the task of demolition. Shrill denunciation is no more convincing than bald assertion. I believe we have a duty to defend the bastions of freedom of legal expression, however provocative this may occasionally seem to those within whom prejudice masquerades as given truth.

Peter's eloquence struck home, for I subscribe wholeheartedly to the principles of free speech and scientific discourse, but seemed in this case to have advocated censorship and closure, particularly since I still had not read the Rushton essay. Reading it was a shock, for it was worse than I had expected. It

*This is an edited version of the invited Opening Address at the *XIth International Conference on the Social Sciences & Medicine* held at Leeuwenhorst Congres Center, The Netherlands, 24–28 July, 1989.

convinced me that in evoking noble sentiments about scientific publication, Peter had missed the point. He and his reviewers had simply failed to recognize the character of the work. Its disingenuous underpinnings and inherently racist premises were transparent to me, but not to them. Why? What appealed to these social scientists so that garbled biology and sociology appeared to be "sufficiently respectable scientifically to merit publication", and racism appeared to be "reasoned argument"?

To answer this question we must summarize the Rushton essay and relate it to the discourse on race in biocultural studies of human adaptation and evolution, and in our society at large. But as I do this you should remember that our topic is the community of social scientists: How does it work? and What is the nature of the civilization that nurtures it? Whether or not you have read the Rushton essay, I expect that my story so far resembles some pattern of events that you have observed or gossiped about on other occasions, and thus has elements familiar to you. We are looking, as Aristotle said, for the general in the particular. To give Rushton's work context I will recount my own reasons for becoming an anthropologist, and speak personally from a career of teaching, research and editorial work. I invite you to compare your experiences and conception of the social sciences to mine, for we have lived through a good portion of the twentieth century and share memories of it, and we participate together in the enterprise that Peter McEwan heads, helping to edit *Social Science & Medicine*, evaluating manuscripts sent to us in its peer review process, publishing in it ourselves, and attending its conferences. This work is continuous with our work in universities, professional associations, governmental agencies and so on, forming a community of scholars divided on regional, national, disciplinary and linguistic lines. The civilization that encompasses and sustains this heterogeneity is never far from our minds.

Rushton is part of our community. He is a member of the psychology department at a good university. He wrote the article in *Social Science & Medicine* while a Fellow of the Guggenheim Foundation, and he cites other articles he has published in respected scientific series, including the *Proceedings of the National Academy of Sciences, U.S.A.* Thus, his work has successfully undergone peer review by numerous institutions. When we look for the general in the particulars of his work we are not looking at another culture alien to our own, but at the culture of our own community.

Rushton and Bogaert's argument is straightforward (From now on I will only refer to the senior author since the flap that has developed in Canada about this work centers on Rushton. Also, I will use his categories and generalizations and comment on them after the summary). The AIDS virus infects Negroid populations more than Caucasoids, and Caucasoids more than Mongoloids. In Africa, where it originated and is transmitted by heterosexual intercourse as well as by other practices, it is more widespread than in other parts of the world. In North America and other dominantly Caucasian populations, where the primary modes of transmission are homosexual intercourse and intravenous drug use, the sector of the population with African ancestry is much more infected than the Caucasians. The lowest rates of infection are in the Mongolian populations of Asia, and among minority peoples of Mongolian ancestry in other regions. This epidemiological pattern corresponds to racial differences in temperament and behavior that increase the risk of AIDS infection. Rushton does not argue that the three races differ in biological resistance to the virus, with the Mongoloids most resistant and the Negroids most susceptible. The difference in susceptibility that he tries to prove resides in genetically grounded social behavior—in qualities of intellectual and moral life. This difference arose in the course of human evolution through the processes that differentiated Mongoloid, Caucasoid and Negroid racial types.

Rushton's ideas are largely summarized in a drawing, a table and a chart that he borrowed from other scholars to explain the distinction between *r* and *K* selection, and finally, a table that he created to apply this distinction to human races. The distinction between *r* and *K* selection was new to me, but the general concept in these borrowed items was familiar.

The drawing suggested a scale between 4 animal groups represented by an oyster, a fish, a frog, and 3 orders of mammals, a rabbit (Lagomorpha), tiger (Carnivora) and baboon (Primate) (Fig. 1). Actually, Rushton modified the drawing from the original, where the last two animals were a lion and a gorilla. At the *r* end of the scale the reproductive strategy is prolix, producing tiny eggs of which relatively few mature to reproduce, and at the *K* end the strategy involves prolonged uterine development, single birth and slow maturation. Table 1 spells out the distinction between *r* and *K* life styles [1, p. 1215].

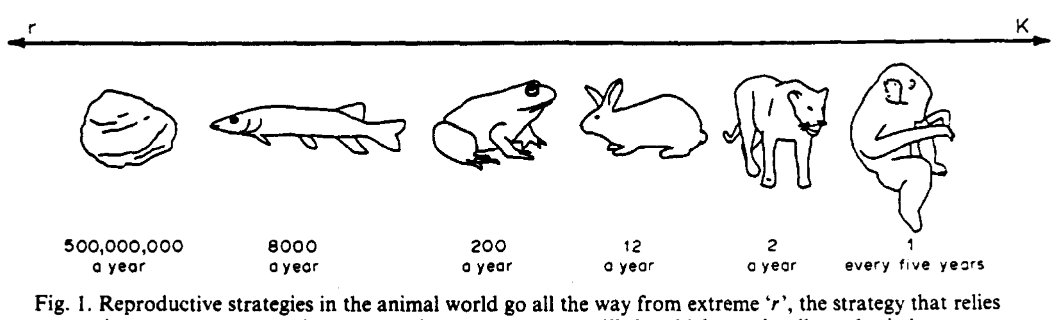

Fig. 1. Reproductive strategies in the animal world go all the way from extreme '*r*', the strategy that relies on maximum egg output and no parental care, to extreme '*K*', in which nearly all emphasis is on care and the birthrate is reduced to a minimum [1, p. 1214].

Table 1. Some life history, social behavior, and physiological differences between r- and K-strategies [1, p. 1215]

r-Strategist	K-Strategist
Family characteristics	
Large litter size	Small litter size
Short spacing between births	Long spacing between births
Many offspring	Few offspring
High rate of infant mortality	Low rate of infant mortality
Low degree of parental care	High degree of parental care
Individual characteristics	
Rapid rate of maturation	Slow rate of maturation
Early sexual reproduction	Delayed sexual reproduction
Short life	Long life
High reproductive effort	Low reproductive effort
High energy utilization	Efficient energy utilization
Low intelligence	High intelligence
Population characteristics	
Opportunistic exploiters of environment	Consistent exploiters of environment
Dispersing colonizers	Stable occupiers of habitat
Variable population size	Stable population size
Competition variable, often lax	Competition keen
Social system characteristics	
Low degree of social organization	High degree of social organization
Low amounts of altruism	High amounts of altruism

The chart, taken from Lovejoy [4, p. 342], shows K selection within the Primate Order, revealed by a pattern of prolonged development which is most advanced among hominids (Fig. 2).

This was familiar ground, the stuff I used to teach undergraduates and that one encounters in articles on human evolution in popular science magazines or on educational television. It was the story of arboreal adaptations followed by a shift to ground dwelling bipedalism, with the opposable thumb, stereoscopic vision, hand–eye coordination, omnivorous diet and single births described as primate adaptations to an aboreal habitat, and with specialized hind limbs adapted to bipedal locomotion that freed the arms to carry food, babies and weapons, and with these adaptations in turn accompanied by displacement of the estrus cycle by continuous sexual receptivity, pair bonding and parental cooperation in raising fragile infants. The story continues with increasing brain size and selection for complex behavior, including tool production, linguistic symboling, and the control of fire. Throughout this evolutionary story I would emphasize the reciprocal relationship between biological and cultural mechanisms of adaptation. The omnivorous diet including increasing consumption of animal tissue, but mediated by cultural adaptations in tool production and cooking so that carnivore diet occurred without carnivore biological specializations in jaws and teeth. The whole sequence characterized by neoteny, a change in growth pattern that lengthened the period of childhood dependency and learning, giving rise to the ultimate species specific behavior of grammatical speech.

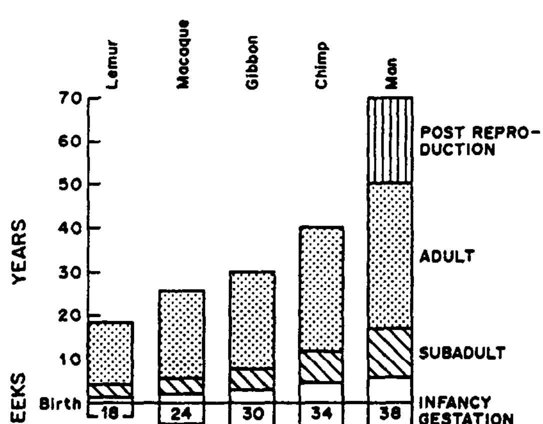

Fig. 2. Progressive prolongation of life phases and gestation in primates [4].

Rushton's chart showing that hominids are K-selected was familiar scientific discourse, but his use of the r-K distinction to describe racial variation was something else, also quite familiar, for here were the racist premises that I grew up with in the American south, the assertion that Negroes have small brains, low intelligence, big sex organs, mature rapidly, have exaggerated sexual impulses, and permissive sexual attitudes, are lawless, aggressive, and have unstable marital relationships (Table 2).

When I phoned a former student of mine who is now a biological anthropologist at a distinguished medical school to ask his advice in preparing this paper, he compared my problem to the difficulty of confronting religious fundamentalists who want something that they call 'creation science' taught in biology courses, rather than Darwinian evolution. They have studied the scientific literature and use language borrowed from it along with scattered facts to prove a Biblical view of the world. If you can't just say flat-out that this is a spurious enterprise, as I

Table 2. Ranking of populations on r/K associated attributes [1, p. 1216]

	Mongoloids	Caucasoids	Negroids
Brain weight and intelligence			
Cranial capacity	1448 cm^3	1408 cm^3	1334 cm^3
Brain weight at autopsy	1351 g	1336 g	1286 g
Millions of 'excess neurons'	8900	8650	8550
IQ test scores	107	100	85
Maturation rate			
Gestation time	?	Medium	Fast
Skeletal development	?	Medium	Fast
Age of walking	Slow	Medium	Fast
Age of first intercourse	Slow	Medium	Fast
Age of first pregnancy	Slow	Medium	Fast
Brain weight decline begins	Age 35	Age 25	?
Life-span	Long	Medium	Short
Personality and temperament			
Activity level	Low	Medium	High
Aggressiveness	Low	Medium	High
Cautiousness	High	Medium	Low
Dominance	Low	Medium	High
Impulsivity	Low	Medium	High
Sociability	Low	Medium	High
Reproductive effort			
Multiple birthing rate	Low	Medium	High
Size of genitalia	Small	Medium	Large
Secondary sex characteristics	Small	Medium	Large
Intercourse frequencies	Low	Medium	High
Permissive attitudes	Low	Medium	High
Sexually transmitted diseases	Low	Medium	High
Androgen levels	Low	Medium	High
Social organization			
Law abidingness	High	Medium	Low
Marital stability	High	Medium	Low
Mental health	High	Medium	Low

would like to say of Rushton's article, then refuting their work step by step, my friend said, is "like trying to shovel manure from the barn with a teaspoon."

So many things are wrong with Rushton's article that I despair of ever persuading the author that this science is spurious. Like the fundamentalists who cloth their religious cosmology in a scientific vocabulary, Rushton's agenda lies outside the work itself. But I am constrained to shovel away for awhile just because we are in the barn on our own farm, and I must try to persuade Peter McEwan and his reviewers that we are not in the parlour having tea with a scholar whose work merits publication in our favorite scientific journal. I do not despair of the enterprise because I have worked harmoniously on the journal with Peter for more than a decade, and know him to be fair minded.

The distinction between r-selected species and K-selected species was first made by Robert H. MacArthur and Edward O. Wilson in an innovative book that has stimulated mathematical formulations of the demographic characteristics of island biotas. K is defined as "The 'carrying capacity of the environment.' i.e., the number of individuals in a population of a given species at the population equilibrium" [3]. And r is "The 'intrinsic rate of increase,' the per capita rate of net increase in a given environment" [3, p. x]. A high rate of increase will help a species colonize an island, but when it reaches the carrying capacity of the habitat, crowding will increase competition and K-selection will favor genotypes that most efficiently use resources. r-selection favors the ability to increase the size of the population and thus to take advantage of increased resources, and to recover from temporary enviromental insults. MacArthur and Wilson wrote:

> In defining the peculiarities of post-colonization evolution, a fundamental distinction was made between r selection and K selection. The intrinsic rate of population increase, r, is likely to be increased in the earliest stages of colonization, when population growth is unrestricted. Moreover, r will be held at a high value by those species whose histories include frequently repeated colonizing episodes. But most species occupying stable habitats, once they have attained their maximum population size, K, will tend once again to reduce r. There will be a simultaneous tendency to increase K through finer adaptation to the local environment. Thereafter, the relative amounts of r selection and K selection will be determined by the stability of the local environment [3, p. 178].

Rushton's characterization of human races as r-selected or K-selected does not come from MacArthur and Wilson, but appears to be derived from evolutionary theorizing by the anthropologist, C. Owen Lovejoy [4], whose ideas were popularized in a best selling book about the discovery of an extraordinary Australopithecus fossil [5]. Table 3 contrasts Hominid and Pongid adaptations. Although the K strategy of adaptation is a general mammalian trait, and well developed among the primates, Lovejoy argued that Hominids diverged from the Pongids through an adaptive strategy that involved r-selection to reduce the period between births (K-selection would have increased the length of time between births).

Fig. 3. Mechanical model of demographic variables in hominoids. The R is the intrinsic rate of population increase (1 = static population size). An increase in the lengths of the four periods on the bar to the right (birth space, gestation, infant dependency, and sexual maturity) is accompanied by a comparable shift of longevity to the left, but without realization of that longevity, prolonged maturation reduces R and leads to extinction or replacement by populations in which life phases are chronologically shorter. Of the four variables on the right, only birth space can be significantly shortened (shifted to the left) without alteration of primate aging physiology [4, p. 343].

Birth spacing is one of five demographic variables in equilibrium when a population reaches the carrying capacity of the environment (Fig. 3). Actual longevity—the probability of surviving—depends on genetic life potential and on interaction with the environment such as avoiding predators and securing food. The higher primates enhance survivorship, e.g. reduce environmentally induced mortality, by "strong social bonds, high levels of intelligence, intense parenting, and long periods of learning" [4, p. 343]. The success of Hominids in changing the ecological niche they inhabit through cultural adaptations, and in colonizing new environments could only have occurred by altering two of the five demographic variables, survivorship and birth interval. The other demographic variables are "direct linear functions of mammalian developmental physiology" [4, p. 344].

Lovejoy observed that Hominid evolution involved the novelty among the large primates of r-selection for decreased birth intervals which allowed more rapid population increases than would otherwise be the case, and thus facilitated the colonization of new environments. He described this novel pattern as a complex of feedback relationships between common mammalian behavioral elements and selection for bipedalism, pair bonding and intensified sexuality.

Bipedalism occurs early in the fossil record, and it freed the arms to carry food. Provisioning is a primary parental strategy of canids (dogs, wolves,

Table 3. Hominid and Pongid adaptations [5, p. 338]

Hominid	Pongid (Ape)
Exclusively ground-dwelling	Some predominantly in trees. Some predominantly on the ground. None exclusively terrestrial.
Bipedal	Not bipedal
Pair-bonded, leading to establishment of nuclear families	Not pair-bonded. No nuclear families except in gibbons
Increasing immobility of females and young. Possibility of a home base	Females move to secure food and take infants with them. No home base
Food sharing	No food sharing
Beginnings of tool use and tool making	Tool use absent or inconsequential
Brain continues to enlarge	Brain does not enlarge
Continuous sexuality	Sexuality only during estrus
Multiple infant care	Single infant care

foxes) and of birds, but among large primates it is peculiar to ourselves, where adults provision each other as well as infants. The arms were also freed to carry altricial infants, thus inhancing the survivorship of fragile genotypes. A sexual division of labor, with male and female provisioning and continuous sexuality, rather than arousal limited to estrus, favored pair bonding and formation of nuclear families with shorter birth intervals and multiple dependent offspring. This evolving social matrix nurtured the prolongation of growth and learning and more efficient exploitation of the environment.

Here is the ground for Rushton's perversion of the distinction between K-selection and r-selection in Hominid evolution. Lovejoy observed that we display "a greater elaboration of epigamic characters than any other primate," and he goes on to say that among Hominids

> marked epigamic dimorphism is achieved by elaboration of parasexual characters in both males and females, rather than in males alone. Their display value is clearly cross-sexual and not intrasexual as in other primates. It should be stressed that these epigamic characters are highly variable and can thus be viewed as a mechanism for establishing and displaying individual sexual uniqueness, and that such uniqueness would play a major role in the maintenance of pair bonds [4, pp. 346–347].

Lovejoy is describing the peculiar character of human sexual attraction and love. Without clothing male genitals are large and prominently displayed, compared to other primates, and they are decorated on our naked bodies with pubic hair of various patterns and thickness. Female breasts are relatively prominent and variously shaped, and male beards vary in thickness and pattern along with other parasexual traits in the somatic profiles of both sexes. This pronounced sexual dimorphism and polymorphism facilitates the individualized and intense sexual preferences and attachments involved in human pair bonding, short birth intervals, and effective parenting of several highly dependent children at the same time. In other words, a dash of r-selection was an essential element in the distinctive evolutionary pattern of our species. Rushton garbled the concept of Hominid r-selection for birth spacing by identifying it with an alleged racial pattern of psychological and social traits that he associated with a small brain and large genital phenotype.

My God, must I go on? I must go on.

To the contrasting pole of K-selection, expressed somatically in big brains and small genitals, Rushton attributed the racial character of law abidingness, low aggression and low dominance. He identified the Mongoloids with this pattern, despite the long history of Asian violence, including Japanese military conquests in this century, beginning with their defeat early in the century of Czarist Russia and bloody participation in the first and second World Wars. Are we to consider the Communist revolution in China and its sequalae up to the current executions of workers and students, or the activities of Mr Pol Pot and his followers, to illustrate racial law abidingness, low aggression and low dominance? Rushton gives no reasons at all for attributing these character traits to Mongoloids in his *Social Science & Medicine* article, but in an earlier essay in another journal from which he recycled the tables and charts he used in *Social Science & Medicine*, he cited psychology observations of babies, and personality tests administered to students [6]. How this justified an aggregate description of hundreds of millions of people with highly differentiated cultural traditions he does not say. Instead, he gives us K-selected Asian sexual inhibition which he says is expressed in Chinese and other Asian worry about premature ejaculation and the culture-bound syndrome of *Koro*, anxiety about the disappearance altogether of their little penises.

Of course, the real crunch of Rushton's argument is his r-selected pole of uninhibited black sexuality. He tells us that a review of sex therapy in Britain revealed that Asian immigrants suffered from premature ejaculation, and that this was not a concern of blacks. But his clinchers on black sexuality are United States rape statistics, a study of child abuse in Philadelphia, and statistics on sexually transmitted diseases. What can I say after all the years of social science research on race issues? It is like trying to correct Rushton's view of racial differences on I.Q. tests, which he associates with the sizes of brains and genitals. Shall we enquire how the female organs were measured? and whether penises were measured from the tip of the foreskin, or the tip of the glans? Shall we discount the racial brain comparisons by observing that brain size is related to body size and that the average differences between male and female brains is greater than the differences he asserts between big and small brain races? What is the use?

I can't go on. I must go on.

One last teaspoon full! Rushton says that he predicted racial differences in r/K sexual strategies

> because human populations are known to differ in egg production ... the rate per 1000 of diazygotic twins (the r-strategy, caused by production of two eggs at once ...) among Mongoloids is 4, among Caucasoids, 8, and among Negroids, 16, with some African populations having rates as high as 57/1000 [1, p. 1215].

When I consulted Rushton's source [7], it turned out that the reliability of the African statistics was low, for they were all based on hospital reports in the 1950s and 60s in societies where an unrepresentative and a limited segment of the black population used hospitals for birthing. From my own observations in Mexico and India, I am skeptical about the accuracy of provincial hospital records in Third World countries. Even so, Rushton ignored the author's reasoning about dizygotic twinning. Bulmer, the study's author, wrote that since birthing itself is hazardous, the mother of twins is at a selective disadvantage to the mother of a single baby, except under modern conditions of delivery and prenatal care. He asks, Why wasn't twinning eliminated through natural selection? He reasons that genetic drift in small populations of prehistoric times might have been more important than natural selection, and that this might explain different rates of twinning in modern populations. This is certainly an alternative to Rushton's hypothesis, but Bulmer himself sets it aside because it contradicts the "general feeling among human geneticists that genetic drift has not played an important part in human evolution" [7, p. 180].

It occurred to neither Bulmer or Rushton, so far as I can tell, that twinning may be evidence of more effective parenting. The norm for primates is single births, but for Marmosets the modal birth is dizygotic twins. This r-selected trait is facilitated among these small South and Central American monkeys by pair bonding and K-selection for males who provide extensive paternal care for the young [4, p. 345]. Thus, r-selection and K-selection are complementary processes, rather than polar opposites, in the evolution of this reproductive strategy. The same possibility is suggested by Lovejoy's theoretical reconstruction of the role of sexuality and birth spacing in human evolution. This turns Rushton's argument on its head, for the higher rates of twinning among blacks would be evidence that more effective nuclear families reduced the mortality rates of the mothers of twins, and nurtured offspring who carried this trait to reproductive age.

Bulmer's second speculation about why natural selection did not eliminate dizygotic twinning was that it indicated "a balanced polymorphism due to heterozygote advantage" on the model of sickle-cell genes and resistance to malaria [8, p. 181]. Selection for balanced polymorphism is quite a different interpretation than Rushton's pattern of r-selected racial traits, but that Rushton ignored Bulmer's hopothesis is consistent with the manner, or rhetoric, of his essay. It is the manner of a debater making as forceful appearing a case as he can. It is a rhetoric that picks and chooses evidence, that ignores alternative hypotheses, that elides issues. In short, the essay is not constructed in the spirit and manner of a scientific argument, but instead is an ideological tract. We social scientists are not good at telling the difference.

Indeed, most of the most influential work in the social sciences is ideological, and most of our criticisms of each other are ideologically grounded. Non social scientists generally recognize the fact that the social sciences are mostly ideological, and that they have produced in this century a very small amount of scientific knowledge compared to the great bulk of their publications. Our claim to being scientific is one of the main intellectual scandals of the academic world, though most of us live comfortably with our shame.

We do like the appearance of being scientific. It is our way of being respectable, and in applied work our bread depends upon it because the people who hire us expect us to produce data and language that will help them implement and justify programmatic goals. George Bush and Margaret Thatcher look for the economist who will tell them what they want to hear, or Deans and Department Chairmen for the psychologist or sociologist who has the right theoretical orientation to meet their requirements. Still, totalitarian countries have less use for the social sciences than liberal democracies, The kind of work published in *Social Science & Medicine* does not thrive in Communist or Fascist dictatorships, or in countries ruled by religious ideologues. By and large, we believe in, and our social science is meant to promote, pluralism and democracy.

These pronouncements may seem terribly excathedra to you. Let me give particulars from personal experience so that you can see how I arrived at them.

When the Japanese bombing of Pearl Harbor brought the United States into the Second World War, I was an 18-year-old freshman at a small Southern college. I liked to party, and I liked girls, but I was, and I was generally thought to be, a sissy. I thought that I was hopelessly homosexual. I wrote poetry, liked art, and was filled with vague ambitions to be a great man, to make something beautiful, to do something noble. I read about the University of Iowa in *Life* magazine, where artists were awarded Ph.D. degrees for writing novels and painting pictures. So I persuaded my father to let me go to Iowa by working the next summer at an incendiary bomb plant to defray the expense. At Iowa I met two Jewish girls and dated one of them. The other one dated a boy from Texas. My attitudes confirmed their opinion of Southerners, and they were astonished at themselves for having anything to do with me.

Despite unhappiness with my own family and community, and desire to escape them, I had no other culture than the one that I had grown up with. Over half the population in my home town was black, before I started to school I had a black nanny, and we always had a black cook. The African Americans were called Niggers, and lots of jokes described their sexual powers and simple-mindedness. Our schools were segregated, as were all other public facilities, and where there was only one, like the public library, black people were not allowed to use it. At the movies they had a separate entrance and a blocked off section of the balcony.

Jews also lived in my town, and owned the most prominent clothing and jewelry stores on Main Street, but they were not allowed to join the Country Club, and jokes about Jews were as frequent and as demeaning as those about African Americans.

My Jewish girlfriend considered me an exotic creature, for she and her roommate were from Brooklyn in New York City, and they were Communists. One day Gert, who was truly in love with the boy from Texas, was crying because he told her that other men in his rooming house had teased him about having a kike girlfriend. For me, this was an epiphany. It revealed the world of suffering we inflict on each other with our thoughtless conventions. I realized that we are both tormented and tormentors.

I had to do something with this new knowledge, so I absconded to Chicago to become a Communist. I arrived in November, shortly after my 19th birthday, found a full-time job, a rooming house, and a Communist night school that opened just after Christmas. I subscribed to *The Daily Worker*, and registered for a course on race relations. Meanwhile, my father agreed to pay my tuition at the nearby night school of the University of Chicago, where I registered for an introductory anthropology course. I did not know what it was, but the lady at the desk recommended it.

The race relations course at the Abraham Lincoln School had readings from Stalin, Lenin, Marx and Engles, and discussed the way that leaders in the Soviet Union had solved the 'nationalities question'. I failed to connect their example with the life I had lived on the Mississippi Delta.

The anthropology course was quite different. The teacher had been an expert witness for the defense in the Scopes trial, where a teacher in Tennessee broke the law by teaching evolutionary biology. It was against the law in my home state as well, so I had read *The Origin of Species* in high school on my own, as a banned book. Now in Chicago I learned the names and characteristics of human fossils from an expert. Sinanthropus, *Pithecanthropus erectus*, Neanderthal, Cromagnon—the scientific words rolled on my tongue like delicious fruit. Here was evolutionary data to shock the folks back home. Here was another culture to learn, one dedicated to a critical search for truth, and romantic to boot, for it promised adventure in far off places searching for the reasons we are the kind of animal we are.

But more at hand, and in contrast to the Marxist course, here was a scientific way to study racial variation, and to reject the pervasive racism of American society.

I realize now that the anthropology I learned in the spring of 1943 was inadequately grounded in evolutionary theory, and that even it was tainted with racist misconceptions, but it is useful to describe its argument on race as the background for Rushton's essay.

This is from memory after nearly fifty years, but because the science I learned was ideologically important to me, I believe that my recall is essentially correct. My teacher was Fay-Cooper Cole, who had been trained by Franz Boas, the founder of academic anthropology in the United States. Like Rushton, Cole accepted the division of our species into three racial groups. He might have used the terms 'basic races', or 'primary races'. Let me illustrate the kind of thinking that was involved with two phylogenetic charts from a famous textbook. Notice in the 'Family tree of the primates' (Fig. 4) that the core line for all primate evolution is represented by the white race from which all other members of the order diverge. The anthropocentricism and racism are astonishing to me now, but in 1943 this sort of thing seemed reasonable, as did the 'Family tree of man' (Fig. 5) [8]. I now cringe at the sexism of the title, though I was taken with 'the science of man' in my youth. Here again the 'Basic White' race is the main line of evolution, with the two other 'primary races' branching from it:

1. The 'Ancestral Negroids' who led directly to the modern African population, and a branch that accounts for the pygmies (Congo Negrillo), Southeast Asian Negritos, and the Melanesians.
2. The Mongoloids, whose miscegenation with whites and Melanesians is represented by tendrils.

Then, two minor racial lines are represented by small peripheral populations, the Australian aborigines and the Bushmen of the Kalahari desert.

Unlike Rushton's use of the three race scheme, Fay-Cooper Cole emphasized the extremely heterogeneous nature of these categories, and their conventional or arbitrary character. The last point was made by telling us that well-qualified anthropologists disagreed with each other about the number of races that they counted. For example, they disputed about whether the Australian aborigines were a separate race, or represented a 'primitive Caucasoid' population, along with the Veddas, a tribal people in Sri Lanka, and 'the hairy Ainu' of Northern Japan. This was my first lesson in scientific nominalism, that scientific categories are our own constructions and not 'God's truth'.

Stanley Garn's map (Fig. 6) [9] showing nine races will remind anyone familiar with human variation of the regional differences in physical appearance between populations in different parts of Europe, North Africa and the Near East, which are here lumped together as one area, as are regions that extend from Siberia through China to Indonesia, here shown to be the area of a single Asiatic race.

Fay-Cooper Cole emphasized the extreme heterogeneity encompassed by each category when humanity was reduced to three races. One was obliged in this case to acknowledge local populations of Caucasoids who were as dark skinned, or darker, than local groups of Negroids in Ethiopia. This provided a debator's point against white racists I grew up with. Cole provided similar debator's points with reference to variations in head form, height, and other somatic features, and reasoned from this that races should be defined by trait complexes for which single traits varied in ways that prevented drawing racial boundaries. Rushton's typological use of race categories, which follows the norm of popular culture and social science usage, sorts people into a simple set of contrasting boxes, yet the geographical reality is a continuous distribution of variable traits that blend with each other as one moves from one region to another.

The central lesson on typological thinking was directed by Cole at the concept of a pure race, and at the supposedly negative effects of race mixture. Rather than dealing with the continuous distribution of physical variations in Europe, three races, Nordic, Alpine, and Mediterranean, were said to compose the Caucasian stock. With photographs of these types, and anthropometric charts, scientists were supposed to be able to sort people out who were racially pure, or one of a great variety of mixtures of these types. This same idea is expressed by the tendrils in Hooton's phylogenetic chart. Like Rushton's work, the European racial types were said to possess well defined intellectual and moral qualities. Culture, language and religion were garbled with biology by labeling Jews a mongrel race, and calling Nordics an Aryan race. The whole enterprise was transparently wicked to all of us in Fay-Cooper Cole's class for what I still believe to be good scientific and moral reasons. My conviction, then and now, is that the moral and scientific reasons were complementary, that good judgment in the social sciences depends on moral knowledge and sensibility as well as on the critical use of scientific reasoning.

A final point. The variability of physical traits such as skin color, height and head form was said to show that they were adaptive to environmental differences in nutrition, exposure to sunlight, and so on. Dolicocephalic, brachycephalic, the words that anthropologists used to describe these variations were scientific music to my ears, in comparison to Stalin

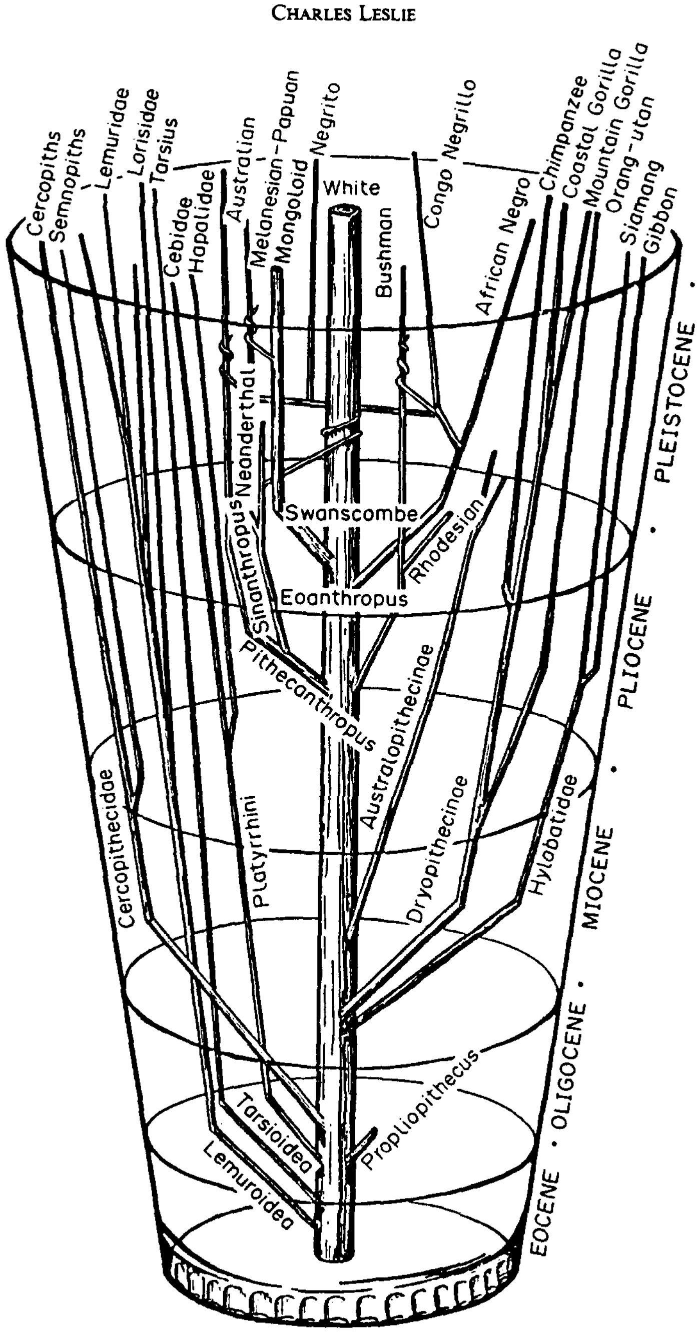

Fig. 4. Family tree of the primates [8, p. 411].

and Lenin on the nationalities problem. Cole described Boas's research on generational changes in the head form of European immigrants to America as evidence that basic physical measurements used by German medical scientists (anthropology was a branch of medicine in Germany) were subject to environmental variations unrelated to genetic change. Thus, with the war on in Europe and the Pacific, part of the refutation of Nazi pseudo-science that I learned argued that racial classifications should give greater weight to non-adaptive than to adaptive traits. We were shown in class how to sort out skulls according to these traits. The Mongoloids, for example, had shovel-shaped incisor teeth, wide zygomatic arches, smooth frontal bones with no supra-orbital ridge, and a characteristic difference from other races in the sutures joining the bones of the cranium. This emphasis on non-adaptive traits discounted the Darwinian notion that natural selection gave rise to racial variation through adaptations to different environments.

With this small start on a career in anthropology, I joined the army air corps and became a bomber pilot. It was in the last group of cadets at most of the installations where we were trained. By the time we graduated the army had more pilots than it could use, and the war was ending. One evening at the movies the newsreel showed concentration camps being liberated—the mounds of shoes, the bleached cadavers, the barracks and barbed wire. It was too awful for us

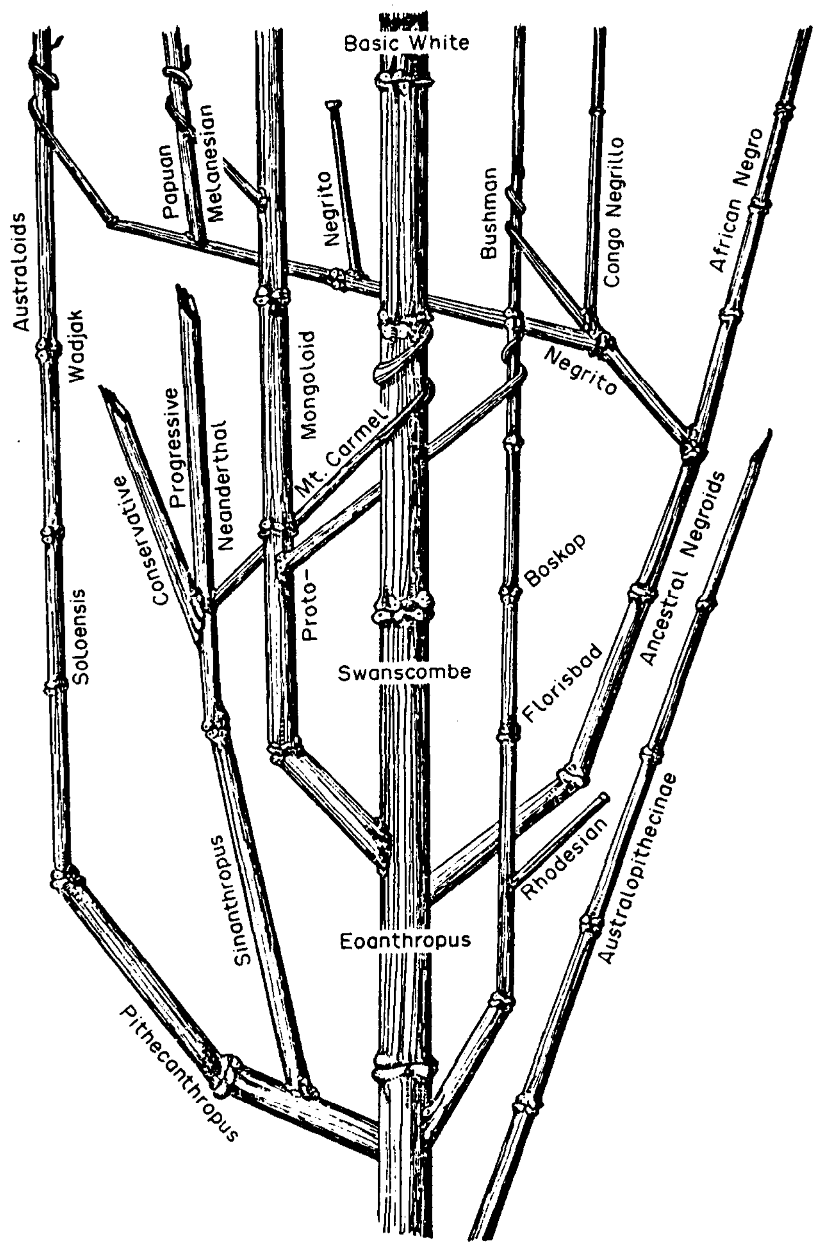

Fig. 5. Family tree of man [8, p. 413].

to have imagined, but seeing that it had happened, I knew that I and my family and the people in my home town and the soldiers I had trained with could have done it instead of the Germans. Our pride, like theirs was our racism and antisemiticism. The army air corps was entirely segregated, and the only black soldiers on our base were assigned exclusively to unskilled service jobs. The medical corps kept separate blood supplies for black and white troops. A pamphlet by the anthropologist, Ruth Benedict, on human races was banned from distribution on military posts. The Germans had outdone us, but they were our kind.

German medical scientists and anthropologists took an important role in the Holocaust [10]. Eugen Fischer, Director of the Kaiser Wilhelm Institute for Anthropology, was an enthusiastic advocate of the Nazi racial ideology, and was elected Rector of the University of Berlin soon after Hitler gained power. With other medical anthropologists, he helped to design and implement the ostensibly therapeutic program of eugenics of the German government [11]. But scientific racism was not a German aberration, it had a long and respectable career within the international communities of medicine, biology and the social sciences. From its origins in the nineteenth century until the second World War, it was as at home in France, England, Japan and the United States as it was in Germany.

The politicalization of scientific racism by the Nazis, and the shock of the full revelation at the end of the war of its conseqeunces, were critical events in

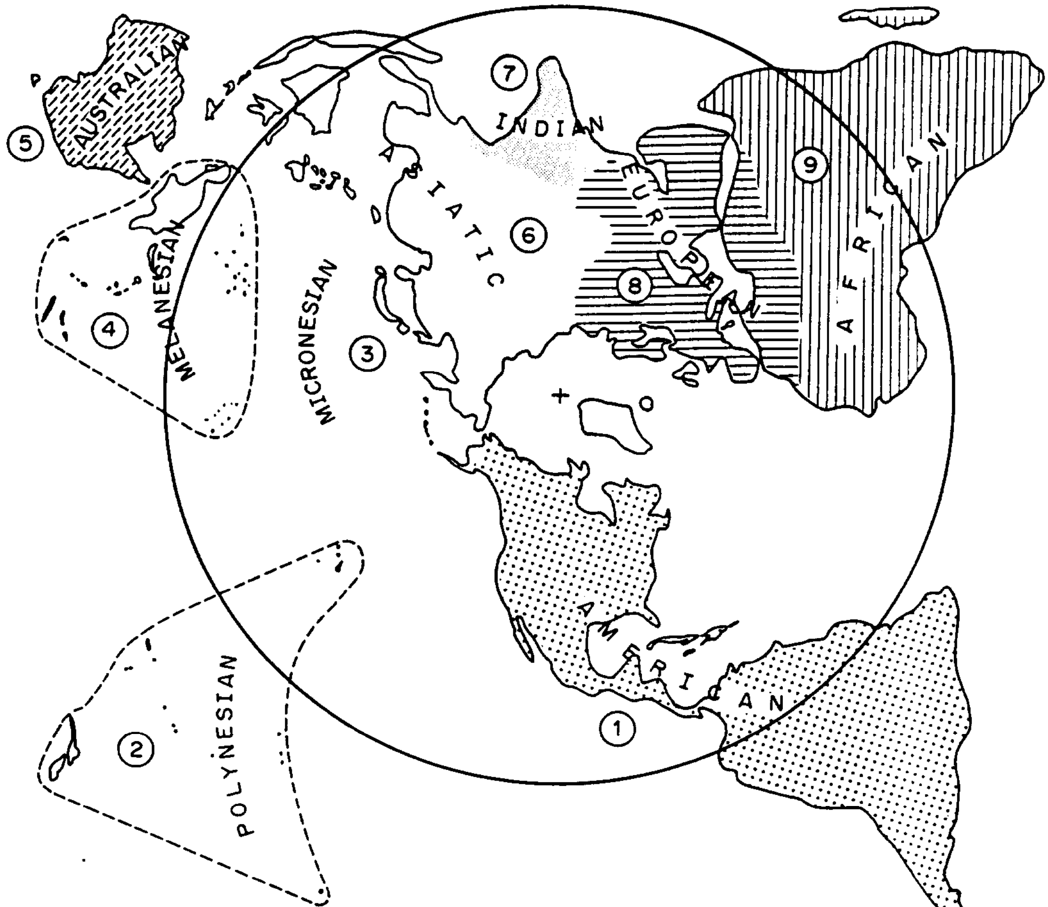

Fig. 6. Polar projection map of the world showing the approximate limits of the geographical races described in the text. This spread-out view shows that large areas of ocean have contributed to reproductive isolation while contiguous land masses allow for easier gene flow.

the history of twentieth century science, and led to an international effort to ground biocultural research on racial variation in more adequate scientific theory and a more humane ethic. UNESCO formed a committee of social scientists for this purpose in 1949. Ashley Montagu, an American anthropologist, wrote the first position paper made public in 1950. Then, a second committee was formed of biologists and anthropologists who worked with Ashley Montagu to revise the statement on race, and circulate it among other scientists for peer review so that a final version could be released in 1951 [12]. This effort to reach a scientific concensus that would turn away from the social ideology, garbled biology and typological thinking that had proven to be so disastrous, emphasized that the real units of human evolution are not racial types but local interbreeding populations. Variation within these populations is the key fact to be studied, not typological uniformity, since variation provides the stuff that allows processes of natural selection to operate.

The theory that guided the UNESCO statement on race is often referred to as 'the neo-Darwinian synthesis'. The rediscovery of Mendalian genetics at the beginning of this century seemed to many scientists to displace the theory of natural selection. The neo-Darwinian synthesis was mainly the work of geneticists and other biologists in Britain and America in the second quarter of this century who reformulated Darwinian thinking in the light of developing genetic knowledge and methods of research.

Soon after I returned to the university to start my education over again, first in its liberal arts college, and then in its graduate department of anthropology, Chicago hired Sherwood Washburn from the Columbia University medical school. He initiated and led a program that revolutionized biocultural anthropology in the United States. He called it "the new physical anthropology", and like the UNESCO statement on race, it was grounded in the neo-Darwinian synthesis. The old preoccupation with classification and anthropometry was shown to be a dead science that generated no useful hypothesis, a waste of time based on an inadequate grasp of functional anatomy, or downright foolish typological thinking that garbled evolutionary concepts.

Now here is the point, from Washburn's perspective, a good deal of what I had learned about human evolution and race was wrong. I had to unlearn some things that had been liberating at the time I chose between becoming a communist or an anthropologist. For example, Fay-Cooper Cole emphasized the importance of non-adaptive traits in racial variation, but Washburn taught on the contrary, that the adaptiveness of physical traits was the main issue in evolution, and to say that a trait was non-adaptive was either to say that it was unimportant, or that you were ignorant about its adaptive significance.

In challenging anthropologists to assimilate the advancing skills and problem solving theories of research that had created the neo-Darwinian synthesis, Washburn brought a new professionalism and rigor to the discipline. A major problem was to bring taxonomic order to the fossil record. For example, *Pithcanthropus erectus* and *Sinanthropus pekinensis* were assigned different generic and specific names by their discoverers, but in fact they belonged to a single species which is now called *Homo erectus*. Another problem was to clarify the theoretical grounds for different interpretations of the fossil record. Figure 7 [13] is from a symposium that Washburn organized for this purpose. The upper left chart represents the fossil data, and the one below it shows a typological interpretation that avoids inferences about genetic relationships. The phylogenetic charts on the right side of Fig. 7 should be compared to Figs 4 and 5 from Hooton's textbook, which made racial variations in modern populations the central fact of hominid evolution. The interpretation on the lower right of Fig. 7 makes speciation processes a prominent feature of Hominid evolution, as did Hooton, but modern racial variations disappear entirely in it and in the chart on the upper right that emphasizes genetic continuity. The reason for this is that modern races are taxonomic subdivisions of *Homo sapiens sapiens*, and thus they evolved *after* the emergence of this species. With proper taxonomy and evolutionary reasoning they are not relevant to the evolution of earlier hominids.

The new physical anthropology shifted research after World War II from concern for racial variation to questions of functional anatomy and hominid adaptation. This required new research on primate ecology and behavior so that the distinctive features of hominid adaptations could be analyzed in the larger context of primate and other mammalian adaptive patterns. A burst of innovative work in primate ethology was particularly exciting in the 1950s and 60s, but *ad hoc* speculation about the adaptive significance of racial variation also fluorished in this period, and Washburn criticized it in his Presidential address to the American Anthropological Association.

> When I was a student, there were naive racial interpretations based on the metrical data. When these became unacceptable politically the same people used naive constitutional correlations to reach the same conclusions of social importance. Today we have naive concepts of adaptation, taking the place of the earlier interpretations, and a recrudescence of the racial thinking [14].

Washburn addressed the topic of race because the Executive Board of the association requested that he do so. The civil rights movement was gathering force, and Carlton Coon, a renowned anthropologist at a leading American university, had just published a book that scandalized his colleagues when portions of the text were leaked before publication to opponents of civil rights legislation. They also considered Coon's theory of separate evolutionary development for different racial groups, which resembles Rushton's thinking, to be genetically improbable. Washburn asserted,

> There are no three primary races, no three major groups. The idea of three primary races stems from nineteenth-century typology . . . If we look to real history we will always find more than three races, because there are more than three major areas in which the raciation of our species was taking place [14, p. 523].

He illustrated the nonsense of much speculation about racial adaptations by analyzing the claim that Mongoloids were an arctic-adapted race. Their short limbs, flat noses and stocky build were supposed to resemble the cold adaptations of some other arctic mammals, and their dark complexion was said to be an adaptation to intense arctic sunlight. Yet in Europe blond complexion and narrow noses were correlated with cold climate, and in Asia a great many Mongoloids have lived for millenia in hot, moist climates. With respect to noses, Washburn wrote

> Let us look at it differently. The nose is the center of a face. Most of a face is concerned with teeth, and bones, and muscles that have to do with chewing. The Mongoloid face is primarily the result of large masseter muscles and the bones from which these muscle arise (malar and gonial angles). This is a complex structural pattern related to the teeth, and a superficially very similar pattern may be seen in the Bushman, whose facial form can hardly be attributed to adaptation to cold [14, p, 525].

Washburn's point was that anthropologists had to learn functional anatomy if they wanted to reason about anatomical adaptations, and that they should not ignore data that did not support their reasoning.

The overall point of Washburn's Presidential Address was that race was of minor importance for understanding the great span of human evolution. It only concerned adaptations during the late Middle and Upper Paleolithic periods, after the emergence of *Homo sapiens sapiens*. Besides that

> The conditions under which the races evolved are mainly gone, and there are new causes for mutation, new kinds of selection, and vast migration. Today the numbers and distribution of the peoples of the world are due primarily to culture. Some people think the new conditions are so different that it is better no longer to use the word race... but I personally think this confuses more than it clarifies [14, p. 527].

Despite Washburn's misgivings about biologists who wanted to abandon the concept of race, this is the direction that physical anthropology took in the following decades. The units of evolution were populations, not racial types. Thus, variations between populations were the objects of study, and the shift in thinking as the term 'population' displaced the term 'race' was fully in line with neo-Darwinian theory. A more radical shift focused research on the distribution of frequencies of separate genetic traits to reveal biological gradients, or clines. In this work race simply disappeared because the gradients for different traits did not correspond. Finally, advances in biochemistry led to the analysis of geographically

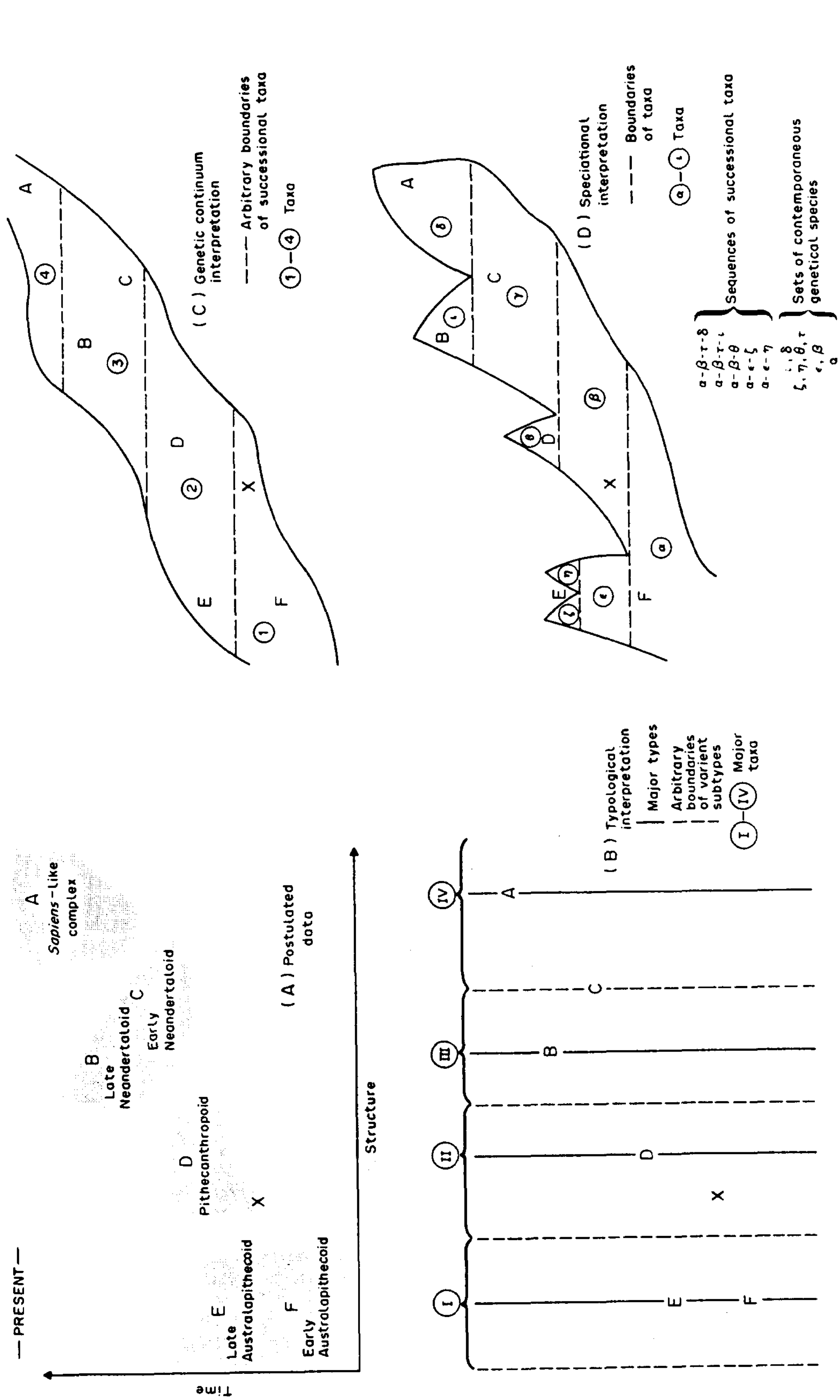

Fig. 7. Postulated data (simplified and generalized) and three possible taxonomic interpretations of known hominide [13].

localized genetic clusters of largely non-adaptive molecular traits, and this is the cutting edge of current work in what used to be called (and is still sometimes called) racial variation. The geneticist, Cavalli-Sforza, at Stanford University is a leader in this research, yet he and his colleagues reprimanded critics of one of their publications a few weeks ago by asserting, "We never spoke of 'races', a concept which, for humans, is devoid of a useful scientific definition" [15].

Littlefield *et al.* [16] analyzed American textbooks in physical anthropology published between 1932 and 1979 to document the potential demise of the race concept in this discipline. The whole system of higher education expanded rapidly after World War II. The eleven graduate programs in anthropology in 1950 had grown to eighty by 1975, and well over a thousand colleges and universities hired their first anthropologists in this period. The number of physical anthropologists was small, compared to those who specialized in social anthropology or archaeology, but as we joined the faculties of small liberal arts colleges (I was the first anthropologist in 1956 to join the Pomona College faculty in Claremont, California) we taught some biological anthropology along with our own specialties.

Nine textbooks were published in physical anthropology between 1932 and 1960, and three authors wrote six of them [15, p. 642]. One of these authors was Ashley Montagu, who wrote the UNESCO Statement on Race. He took the lead in arguing that biologists should abandon the race concept in dealing with human variation because the assumptions embedded in common social usage made it unsuitable for scientific discourse. He was joined by other leading physical anthropologists as the debate continued through the 1960s. Joseph Birdsell, for example, used a neo-Darwinian definition in the first edition of his textbook, saying that "A race is an interbreeding population whose gene pool is different from all other populations," but in a second edition he declared that "The use of the term race has been discontinued because it is scientifically undefinable and carries social implications that are harmful and disruptive" [16, p. 643].

The expansion of instruction led to the publication of 49 textbooks in physical anthropology between 1960 and 1979. Littlefield *et al.* document a change in the treatment of race as this occurred, and as the debate came to a head in the 1960s.

> Of the books included in our study 20 were published between 1932 and 1969, and 13 of these expressed the prevailing outlook that races exist. Only 3 rejected the race concept; the race concept was not mentioned in 2, both published in the late 1960s, and in two cases the panel was unable to reach agreement on the text's classification.... The picture changes dramatically after 1970. Of the 38 texts published in the 1970s, only 12 supported the race concept while 14 opposed it.... This shift became especially pronounced in 1975–79, when the view that races do not exist was expressed in 10 textbooks and became the modal position, with only 5 texts arguing that races are "real" [16, p. 642].

With this background, you can see how shocking Rushton's essay would be to me personally, and as a professional anthropologist. It is doubly offensive because he is a knowledgeable man. He uses neo-Darwinian terminology in referring to r/K selection, and in using 'population' rather than 'race' in the title, 'Population differences in susceptibility to AIDS'. Rushton's sophistication is the reason that I call his work disingenuous and pseudo-scientific, rather than simply erroneous. Let me explain.

Medical researchers in the United States have been astonished over the past two decades by the exposure of one case after another of fraud, plagerism and deceit at Harvard, Yale, Stanford, the Sloan Kettering Institute and other centers for scientific work [17]. Representative John Dingell is currently holding a series of Congressional hearings on scientific misconduct. These hearings have involved David Baltimore, a Nobel laureate at MIT, who published a paper with colleagues, one of whom is accused of contributing fraudulent work.

The scientific community is concerned about the potential threat of political interference if attempts are made to legislate processes of peer review and other aspects of scientific work. The Harvard biologist, Steven Jay Gould, has just published an editorial in the *New York Times* to refute the notion in these hearings that error in scientific work forms a continuum from relatively innocent sloppiness and fudging to more serious misconduct. On the contrary, Gould asserts, "Fraud and error are as different as arsenic and apple pie. The first is a pathology and a poison, the second an unavoidable consequence of any complex human activity" [18]. Congressman Dingell's legislative concern to regulate scientific work to reduce or eliminate error is wrongheaded, according to Gould, because such work involves theoretical controversies, disagreements about the facts, inevitable slips of reasoning and observation, and these errors stimulate other scientists to correct them. In the spirit of Peter McEwan's reaction to objections to the publication of Rushton's article, Gould quotes Darwin's famous observation, "False views, if supported by some evidence, do little harm, for every one takes a salutary pleasure in proving their falseness" [18].

But criticizing errors in Rushton's essay afford no pleasure. Analyzing them is a chore that does not advance the scientific understanding of AIDS, or of racial variation and evolutionary theory. Refutation is a duty because in my opinion his essay is poison. The way that he compounds errors of fact and theory is not a sign of intellectual daring, bold new insights, original observations and new lines of thought. It is familiar racist thinking, a part of our popular culture, and as scientific as the astrological advice in our morning newspapers.

Why did Rushton's essay seem "respectable enough as science" and "reasonably argued" to Peter and the outside reviewers? Of course, I cannot answer for them, but I can guess. To be brief, I will number my speculations and not elaborate on them.

1. Almost all of the contributors to *Social Science & Medicine* are positivists. We like what we call hard data, and Rushton's article, with its maps, tables and charts, looked like

the work of a positivist. Perhaps, also, its scientific vocabulary sounded persuasive. Someone who writes about r/K selection, and holds a Guggenheim fellowship, sounds like he knows what he is talking about. We social scientists like the sound and appearance of the natural sciences.

2. Rushton's paper may have appealed to the reviewers because it affirmed a commonsense way of thinking about race. Our popular culture convinces us that people come in types, and that the types correspond to larger groupings that we recognize by skin color, hair form and so forth. Newspapers and television, sociology and police reports sort people by these folkloric typologies. We understand them, and we experience the world this way. Many of us may not be able to tell one Chinaman from another, but we can readily tell a Chinaman from a Nigerian. Thus, Rushton's aggregation of hundreds of millions of people with very diffrent cultures and physical characteristics into three races, and his allegation that these three types differ in characteristic ways in intelligence, growth pattern, sexuality and temperament exactly corresponds to our everyday interaction with each other. Our epidemiologists and social scientists regularly use these racial categories, so it may have been hard for those who reviewed Rushton's manuscript to see that this whole edifice is built on sand.
3. With Peter McEwan, we subscribe to liberal principles of discourse with multiple, conflicting voices. Also, on the whole we trust each other. Our conflicted community is built on trust that peer review will be even-handed, that we will not violate confidentiality, and that we will study and write in an honest manner. When someone is a member of our community, it is very hard for us to think of his work as disingenuous. Perhaps elements in Rushton's essay seemed doubtful to Peter and the reviewers, but they thought that this was the apple pie of error that could be corrected by others in a fruitful scientific exchange. Thus, their good faith may have prevented them from seeing the reality that I have tried to expose. This is cruel work. We should not be too polite in getting it done. The issue is obvious to members of the scientific community when astrology is involved. If Rushton had submitted a paper on 'Astrological susceptability to AIDS', Peter and the outside reviewers would have agreed that it was inappropriate for *Social Science & Medicine*. I doubt that Peter would have bothered to send it out for peer review. We no longer have the burden of refuting astrologers because we agree that their pretense to science is fraudulent. Clearly, the scientific tradition in which Rushton's article is written does not yet evoke this degree of consensus. We should all be disturbed and puzzled about why this is so.

Peer review is the most neglected topic in the sociology of science [19, 20], perhaps we feel a taboo against studying this aspect of our community because it is so important to our careers. Its anonymity allows scholars with little power to judge the work of those superior to them without fear of reprisal, but just this confidentiality invites abuse, and we often hear complaints of this nature. Public Citizen, Inc., a public interest law firm organized by Ralph Nader, is presently trying to persuade the National Science Foundation "to run its peer reviews a bit more like a judicial proceeding, with open files, an opportunity for applicants to rebut their critics, and a clear system of appeals" [21]. This legalistic transformation of a communal process that is supposed to run on good faith is likely to be resisted by the scientific community. My own experience editing the anthropology for *Social Science & Medicine*, and twenty books in the University of California Press series, *Comparative Studies of Health Systems and Medical Care*, has been that the vast majority of reviewers are conscientious and fair. The occasional biased review sticks out from other reviews of the same manuscript. The most common problem I have encountered is the reviewer who is too lenient on poorly written or inadequately argued work.

I have worked hard on occasion to see a book through review that had been rejected by another publisher. On occasion, a manuscript I asked the author to revise was published elsewhere without this additional work. Similarly, a journal editor once observed that *Social Science & Medicine* had published articles that he had rejected, and in the very next issue the lead article of his journal was one that I had rejected after it had been through peer review twice and twice rewritten. We are fortunate to have a number of journals and book publishers, a number of places where scholars can apply for research funding or fellowships or jobs, and that the peer review process works as well as it does to uphold standards of performance. But we really don't know very well how it works, and its failures are not well documented.

Finally, I cannot close this essay without thanking Peter McEwan for inviting me to work for his journal. I have always felt that its excellence was due to his intelligence and generosity. And I want to thank him and other members of the planning committee for inviting me to give this talk. I disappointed Peter, who did not think that the address was suitable for the occasion, but I tried to speak from an informed heart, and in the end that is the only worthwhile thing we can do.

Acknowledgements—Karen Rosenberg, Mary B. Williams, Matt Cartmill, Allan Young, Margaret Lock and Lee Mullett helped me with references and critical advice and moral support in writing this paper. I am grateful to them.

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COMMENTS

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In an autobiographical tone Leslie [1] describes the development of his opposition to the widespread judgement of the community in which he was raised that Negroids were, on averge, more criminal, less intelligent and more sexual than Caucasoids. His resistance was strengthened when he took courses first in Marxism and then in anthropology, and he eventually rejected the utility of the concept of race for science. Leslie is pessimistic about the possibility of objectivity in a field marred by politial ideologies; to him, the "fakery and racism" of my paper is "transparent" and he cannot comprehend why the intuitive obviousness of this is not more widely shared. For Leslie, the study of human behaviour is as much a moral political enterprise as it is a scientific one; it "is meant to promote, pluralism and democracy".

AIDS and race: more information. Our 1989 paper [2] discussed the worldwide racial distribution of the 100,410 cases of AIDS that had been reported as of 1 July 1988 to the World Health Organization (WHO). By 1 April 1990 that figure had grown to 237,110 showing an 18 month doubling time and a crystallization of the racial pattern of the pandemic. New calculations show that black Caribbean countries have as big an AIDS problem as do African countries. When the figures are worked out on a per capita basis, the three most affected countries in the world are in the Caribbean—Bermuda, the Bahamas and French Guiana. In this region AIDS is transmitted primarily through heterosexual intercourse and there is little intravenous drug use.

The data in Table 1 were collated by me from official statistics published by the World Health Organization as of 1 April 1990 (World Health Organization Update, Global Programme on AIDS). The number of AIDS cases per million population was computed to give an indication of the relative seriousness of the epidemic between countries with different sizes of populations after excluding countries reporting fewer than 100 cases. The population size of the country was taken from estimates standardized for mid-1987 by the United Nations using data available as of 1 April 1989 [3]. On this measure Canada has an AIDS rate of 139 per million making it the 25th most affected country in the world. Of the other top countries, 12 are in Africa, 9 are in the Caribbean, 2 are in Europe, and the other is the United States. It must be kept in mind that for every person officially diagnosed with the AIDS disease there are at least 25 others with the contagious HIV virus. Although the figures in Table 1 must be read with caution, especially those from countries with less than a million population, where a few cases can disproportionately affect the per captia total, nonetheless the racial pattern of the pandemic seems established. Indeed, given the underreporting from African and Caribbean countries, the figures in Table 1 must be considered conservative estimates.

Additional evidence for the racial pattern comes from finer grain analysis of the data from within the United States to 1 March 1990 (Centers for Disease Control, HIV/AIDS Surveillance Report, March 1990) where Negroids are overrepresented in every

Table 1. The 25 countries most affected by AIDS based on per capita cumulative cases reported to the World Health Organization as of 1 July 1990

Country	Date of report	Cumulative number of cases	Population in millions (as of mid-1987)	Cases per million
1. Bermuda	31.12.89	135	0.056	2411
2. French Guiana	31.12.89	191	0.086	2221
3. Bahamas	31.12.89	437	0.240	1821
4. Congo	31.12.89	1940	1.837	1056
5. Malawi	08.01.90	7160	7.499	955
6. Uganda	31.12.89	12,444	16.599	750
7. Burundi	31.12.89	2784	5.001	557
8. U.S.A.	31.06.90	133,889	243.934	549
9. Guadeloupe	31.12.89	182	0.337	540
10. Barbados	31.03.90	122	0.254	480
11. Trinidad	31.12.89	567	1.241	449
12. Haiti	30.09.89	2331	5.438	429
13. Zambia	07.05.90	3000	7.563	397
14. Martinique	31.03.90	125	0.334	374
15. Zaire	31.01.90	11,732	32.461	361
16. Rwanda	31.12.89	2285	6.529	350
17. Cote D'Ivorie	01.02.90	3647	11.142	327
18. Zimbabwe	31.03.90	2357	8.640	273
19. Tanzania	01.03.90	6251	23.217	269
20. Kenya	30.06.89	6004	22.936	262
21. Central African Republic	31.12.88	662	2.703	245
22. Switzerland	30.04.90	1280	6.545	196
23. Dominican Republic	31.03.90	1262	6.716	188
24. France	31.03.90	9718	55.632	175
25. Canada	03.05.90	3818	25.652	119

exposure category. If the U.S.A. were racially divided into separate countries, the approx. 30 million Afro-Americans (12% of total) with 34,431 cases of AIDS would have a rate of 1148 per million, equivalent to other Negroid populations in Africa and the Caribbean.

One point often made is that blacks in the United States have AIDS primarily because of intravenous drug use. Although 39–46% of adult American blacks who acquired AIDS did so through drug use, between 48 and 55% acquired it through sexual transmission, 11% heterosexually (compared to 2% of whites). Of all 6027 adult AIDS cases transmitted heterosexually (5% of total), 3747 or 62% involved blacks, with another 17% being Hispanic. Hispanics, of course, are a linguistic group; racially, a proportion is black or partly black, especially in New York. Blacks are also overrepresented in the 'male homosexual/bisexual contact' exposure category (17% versus a population expectation of 12%). Overall, in the last two years, blacks in the United States increased their total share of the AIDS figures from 26 to 27.6%, Hispanics increased from 14 to 15.6%, Mongoloids stayed in at less than 1%, and whites decreased from 59 to 56%. It would have been instructive to compare these figures with those in Canada but the Federal Centre in Ottawa does not break down the figures by race. The prediction is that if they did, as in other multi-racial societies, the pattern would be Orientals < whites < blacks.

In Canada, some reports do not indicate that blacks are disproportionately more infected with HIV, but this is so controversial that authorities are apparently afraid to record the race of AIDS victims. Stories in *The Globe and Mail* (1 July, 1989; 23 December, 1989) showed that by the beginning of May 1989, 116 of the 40,000 people in Quebec who had been born in Haiti had come down with the disease, an incidence of 2900 per million, higher than any country's official report to WHO (Table 1). Of the heterosexuals in Quebec who had contracted the disease, 25 (58%) did so by having sex with a person from Haiti. Similar figures are emerging in the Province of Ontario: A story in *The Toronto Star* (26 July, 1989) indicated that the number of black people in Toronto with AIDS had grown in the previous three months from 39 to 54—an increase of 38%. Of these 54 black people, 12 (22%) were women. Because only 49 of 1102 white HIV carriers were women (4%) the figures suggested that in Canada, as elsewhere, AIDS among blacks is being spread heterosexually.

Racial and political sensitivities, in part, also fuel reluctance to openly discuss matters elsewhere in the world. African countries report only a small proprotion of their cases, partly out of concern that the West will perceive Africans as promiscous [4]. In Durban, South Africa, following the recognition of black STD clinic attendees as being at risk for HIV infection, testing was suspended by the city's health department; the racial disparities also being clear from blood donor data [5]. Cuba claims a much lower rate of AIDS than elsewhere in the intensely affected Caribbean despite (a) the perceived necessity for universal testing and quarantine, (b) the hundreds of thousands of military personnel who have rotated to duty in Central Africa with concomitant increments of syphilis and gonorrhea, and (c) the studies of refugees in the U.S. showing high infection rates as early as 1980 [6].

Following my work with Bogaert on race differences in sexual behaviour [7, 8], some critics have argued [9] that little evidence exists that the races differ in sexual activities because of biases in the data and because the data are not based on random samples. It may come as a surprise to learn that we don't need random samples and that, in fact, very few hypotheses are tested this way;

we often need only to hold the setting constant and select from groups not too extreme on the distributions. Following this procedure, I have conducted several as yet unpublished interview studies with young Mongoloids, Caucasoids and Negroids in cities from Canada and the United States, asking questions about their age of first sexual intercourse and the total number of their sexual partners. I have consistently found that the *average* black person reports having an earlier age of first intercourse and more sexual partners than does the *average* white person who reports having an earlier age of first intercourse and more sexual partners than does the *average* Oriental person. I stress the word average since there is much variation in each group. These results thus join those already published [7, 8, 10–12] showing that the racial differences in sexuality are widespread and relatively easy to determine.

The reality of race. Although the topic of race differences abounds with ideological minefields, it is possible to rise above them. Imagine that a team of extra-terrestrial biologists arrived on earth to study humans. Would they not quickly observe that, like many other species, humans showed considerable geographical variation in morphology? Surely three major geographical populations or 'races' would be identified immediately and investigation mounted into how many others existed. Questions about the origin of the body types would be asked and also whether they covaried with life history variables including reproductive tactics in particular. If these scientists had a solid understanding of evolutionary biology, they would also investigate if these populations differed behaviourally, for example with respect to parental investment and social organization and, if they did, how these differences might have evolved. Such an approach has proved very fruitful for population biologists studying other animals, particularly since E. O. Wilson's [13] synthesis of sociobiology. If we are as interested in gaining knowledge as would be these 'extra-terrestrials', then we should apply similar procedures to our study of *Homo sapiens.*

The existence of genetic variation both within and between populations is, in fact, the first postulate of Darwinian theory. Without variation, natural selection would have nothing to work on. (The second postulate of evolutionary theory is that some parts of the variation are more successful at replication than are others.) Thus, from a sociobiological point of view, it is predictable that separate breeding populations will come to differ, genetically, in the mechanisms underlying their behaviour. This is because populations adapt to their environments behaviourally, as well as morphologically [13].

Behavioural, physiological and anatomical differences among the races follow a remarkable pattern [14, 15], a summary of which was presented as Table 3 by Rushton and Bogaert [2] and repeated as Table 2 by Leslie [1]. The observation that on over 60 different variables including brain size and intelligence, rate of maturation, sexuality, personality and social organization, Caucasoids average consistently *between* Mongoloids and Negroids, offers an array of theoretical and empirical problems for analysis.

The predictive nature of race undermines Leslie's argument that racial terminology is poorly justified. Similarly devalued must also be the judgement of influential anthropologist Ashley Montagu [16] who, as Leslie documents, successfully advocated the substitution of the phrase "ethnic group" for "race" in order to shift the emphasis away from a "question begging . . . biologistic bias" (p. 697).

The scientific devaluation of the Leslie–Montagu position must occur because it obfuscates higher level conceptual order. For example, the rate of dizygotic twinning per 1000 births among Mongoloids is <4, among Caucasoids, 8, and among Negroids, >16, regardless of which country the samples are taken from, with some African populations having twinning rates as high as 57 per 1000 [17, 18]. The incidence of non-monozygotic triplets and quadruplets shows comparable rank orders [17, 18]. The tendency to multiple ovulation is inherited largely through the race of the mother, independently of the race of the father, as observed in Mongoloid–Cauasoid crosses in Hawaii and Caucasoid–Negroid crosses in Brazil [17]. It is misleading of Leslie to suggest that Bulmer's [17] data on racial group differences in twinning are unreliable; many additional surveys on multiple birthing support this epidemiological pattern [18–20] as well as the relation of multiple birthing to r/K reproductive strategies [21]. Perhaps as a result of matching evolutionary processes to ovarian production, parallel differences in testes size have been found among the races. The difference is twofold lower in Mongoloids than in Caucasoids (9 g vs 21 g), too large a dissimilarity to be accounted for in terms of body size [22, 23]. Although the data are much less conclusive, larger testes have sometimes been found in Negroids than Caucasoids [22, 24].

The efficient unit of analysis, therefore, is the higher order concept of race, within which cluster the different ethnic groups and, ultimately, individuals. Leslie's claim that the concept of race is not useful for human populations not only obscures higher level conceptual order and ignores the approach of population biologists studying other species but also neglects recent developments in the field of medicine and social science. Biomedical-anthropology is a new discipline to study race × diet interactions. For example, the ability of adults to easily digest milk is largely limited to Caucasoids and a lack of knowledge here may have increased mortality among the needy in Third World countries who were inadvertently provided with milk products to alleviate hunger. Other researchers are considering whether there are racial differences in susceptibility to drug addiction; for example, some 80% of Mongoloids become flushed when given alcohol.

The origin of human races. The behavioural and morphological data—in which Caucasoids consistently average *between* Negroids and Mongoloids—can be used to help decide between the various reconstructions of human evolution which Leslie finds so problematic. Current thinking, especially among those physical anthropologists who use molecular biology (blood group, serum protein, mtDNA and nuclear DNA) to buttress the more usual data from paleontology, involves a single origin model for the emergence of modern humans instead of the alternative multiregional models. An African origin is envisaged, perhaps even as recently as 140,000 to 290,000 years ago, with an African–non African split $110{,}000 \pm 34{,}000$ years ago, and a European–Asian split $41{,}000 \pm 15{,}000$ years ago [25–27]. Thus the sequence in which the races emerged in earth history matches the phased linearity of the suite of r/K characters. This parallel is not readily predictable from multiregional origin models based on long periods of separation, in which no consistent pattern of character appearance is expected.

The genetic evidence in favour of the single origin model is that (a) rates of change in mtDNA place the modern human origin at 140,000 to 290,000 years ago; (b) genetic variation is greatest within African populations, which is predictable if they appeared earliest; (c) protein analyses date a Negroid–non Negroid split at $110{,}000 \pm 34{,}000$ years ago and a Caucasoid–Mongoloid split at $41{,}000 \pm 15{,}000$ years ago; (d) blood group data indicate that Caucasoids are intermediate to Negroids and Mongoloids in genetic distance; (e) genetic variation between human populations is low in comparison with variation within populations or with that found in other hominoids thus suggesting a short time period of geographical differentiation [25]. Paleontological data are consistent with the foregoing because the oldest human fossils (92,000 years) have been found in Africa and/or the Middle East, which is the likely pathway from Africa into Eurasia [26].

But why would Mongoloids have ended up the most K-selected? As populations moved north they encountered more predictable and yet more challenging environments, including the ice ages which ended only about 10,000 years ago. Predictable environments are one ecological precondition for K-selection. Tropical savannahs, due to sudden droughts and devastating viral, bacterial and parasitic epidemics, are generally less predictable for long lived species than are temperate and Arctic conditions. Although the Arctic climate varies greatly over one year, it is highly stable among years. The harsher yet more predictable the environment the more stringent would have been the selection pressures for intelligence, forward planning and sexual and personal restraint.

Conclusion. Many of the differences between the races summarized in Leslie's Table 2 appear to confirm the averaged perceptions of the community of Leslie's youth. Although many have striven to dismiss all these as 'stereotypes', the psychometric evidence shows that when human judgements are aggregated and calibrated against other criteria, they are typically found to be valid. This appears to be as true of judging intelligence and social behaviour, where converging evidence can be marshalled to assess 'construct validity', as it is of judging temperature and weights where objective standards can be applied [28]. As a starting point for scholarly discourse, the statistically significant average differences between the races in AIDS, as well as other traits of longer standing, must be acknowledged to exist, even when their interpretation is problematic.

If observed racial differences in crime, educational achievement and sexual behaviour are hypothesized to be due entirely to environmental differences such as "the consequences of living in a racist society", objections are seldom made. If evolutionary and genetic hypotheses are suggested, then *ad hominem* attacks follow almost inevitably. Thus it is not the *data* that are controversial but rather their explanation. The only way to minimize ideological bias is to scrutinize the goodness of fit between data and theory. Which of the alternative theories, then, is more powerful and best fits the total array of assembled data?

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In order to 'enter the scientific arena', a manuscript should clearly demonstrate that its subject is important and relevant to the journal's audience, the data accurate, the methods appropriate, the exposition clear, and the logic and reasoning sound. In an ideal world, reviewers acting on behalf of a scientific journal would measure each submission solely on the basis of these criteria, and our research publications would consistently be filled with good science. Unfortunately, reviewers can sometimes be careless, politically motivated, and cavalier. As a consequence, submissions which lack at least one of the above criteria are often accepted for publication.

As Professor Leslie notes, one area in which the review process is often deficient is its critical response to style and form. He points out that substandard ideas often receive unwarranted attention simply because they have been cloaked in the language of science; creationists, among others, succeed in impressing the naive by adopting 'a scientific vocabulary to translate their cosmology'. We are all familiar with this special language, because we are all forced to use it; not to do so would place us in jeopardy with reviewers. This special language can take the ordinary and make it appear profound. Most of us are familiar with 'translation' sheets periodically devised by students who have 'discovered' the language and, even though they are about to adopt it (their dissertations would be in peril if they did not), nevertheless take great satisfaction in pointing out its excessive pomposity: for 'the solute was hydrated and vigorously agitated' read 'I put the stuff in water and shook it'. There are some legitimate reasons for using a more formal scientific language. It is less ambiguous than ordinary language. It is free of idiomatic reference and therefore more universally understood (scientific French is easier to read than common French). But it is also a two-edged sword. How often has mediocre science been allowed to parade as cogent analysis simply because it has been cloaked in scientific parlance?

The manuscript by Rushton and Bogaert was written in the dialect of evolutionary biology. *When translated into common parlance, however, it is virtual nonsense.* Its reviewers were either unfamiliar with the language in which this nonsense was versed, or were overly impressed by it. The *science* being proffered in the Rushton and Bogaert paper, however, as Professor Leslie has elegantly established, is utterly transparent. Let us suppose, for a moment, that they were to submit a similar paper to a journal of mammalian behavior not on human behavior, but that of yellow marmots. Would they be allowed merely to state that "aggressiveness" could be classified in categories of "low, medium, and high" without definition of the specific behavioral criteria on which it was judged? Would they be allowed to lump all secondary sex characters and report them as "small, medium, or large", without accompanying explanation? Would they be allowed to use age-at-death, or age at first intercourse, as measures of *maturation rate* when a host of more direct and vastly more appropriate indices are available? In Table 3 of the Rushton and Bogaert paper "life span" is listed as "long" for "Mongoloids", "medium" for "Caucasoids", and "short" for "Negroids". What reviewer could be so unaware of even the most basic rudiments of environmental determination of human demographic response not to be in awe of such biological naivete? Did no one read this table? It is a hodgepodge of species characters and behaviors which are obviously learned, with levels of generalization too gross to even be considered ordinal in most cases! Did no social scientist read this paper? We are told that Chinese and Japanese inexperience with "premarital sex" and their lack of permissiveness and concern with sexual display are heritable; that 52% of British female university students "think about sex everyday", but only "1% of Japanese female students did so". These examples are from a subsection of the paper which attempts to establish that they (and a host of other equally ludicrous examples) are manifestations of *genotypic variation* in reproductive strategy! I am particularly interested in Rushton and Bogaert's (presumably) polygenic models for the inheritance of "social organizational complexity", and their projections as to the prospect of identifying which chromosome bears the loci which lead to "decentralized organizations with weak power structures". Perhaps these are pleiotropic characters of a single dominant gene? Those of us interested in social insects look forward with anticipation to their further clarification of these models.

It would seem that in recent years we have become progressively more accepting of mediocre analyses and substandard ideas simply because they are written in the increasingly complex dialects of science. It seems to me that publication of the Rushton and Bogaert manuscript is as much a reflection of this general reduction in standards as it is a failure of the review process to prevent publication of racist views. When the editorial process fails to systematically reject poor quality science, racist tracts are more likely to pass through such porous filters. As McEwan points out, this is but one of a series of Rushton manuscripts.

With the proliferation of scientific jargon comes an ever increasing need for reviewers who are familiar with that jargon. Otherwise it will become increasingly easy to use it as a means to successfully publish in journals whose primary audience is unfamiliar with it. I could suggest the analogy that we do not publish papers by Russian authors merely because they are written in Russian, and we therefore cannot expect effective reviews of Russian manuscripts by those who do not read the language. This analogy, however, is inappropriate: Russian language journals are intended for Russian scholars—non-Russian speakers are not expected to profit from them. If the use of dialect and jargon is sufficient to mask the fundamental science of a manuscript, then the manuscript must be rejected *on those grounds alone*. Good quality science can be made understandable to a wide audience, just as poor quality science can be hidden from that same audience.

McEwan tells us that "critics should address the substance of the [Rushton and Bogaert] paper rather than its publication". Yet it contains no objective measures of behavior, nor evidence of inheritance pattern, nor reliable indices of supposed evolutionary correlates. The paper is simply a compendium of 'bald assertions' which should have been rejected by its reviewers. Otherwise such papers become the focal point of spiraling assertions and counter-assertions. Not even the 'softest' science, i.e. one whose data are derived principally from the reports of trained observers, can survive such editorial laxity. In short, if reviewers consistently employed the same standards that allowed publication of the Rushton and Bogaert paper, the effects on virtually all branches of biosocial science, whether sociology or mammalian behavior, would be devastating.

Poor science is becoming increasingly prevalent precisely because the review process is increasingly ineffective. If we do not reject papers whose underlying deductive and/or inductive processes are transparently artless and naive, then we must expect to see more Rushton-styled analyses in our journals. It is not just scientific racism that must be weeded out by the reviewers, but shoddy science. Let me close with an example. In the *most recent issue* of an anthropology journal one can find the following equation for human cranial capacity:

$$\text{cm}^3 = 1{,}274 - 10.9\ (\text{gathering}) + 2.4\ (\text{latitude})$$

along with a description of its potential use:

> ...This equation indicates that each 10% of subsistence coming from gathering reduces cranial capacity by 10.9 cm^3. Each degree of latitude increases the cranial capacity by 2.4 cm^3.

If we admit this quality of analysis to our major journals, can we not expect transparent racism to be far behind?

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There may be many criticisms to be made of Rushton's paper but it surely merits a more reasoned and less personal attack than that of Leslie. His emotional tirade, although motivated by humanitarian considerations with which I have much sympathy, does disservice to the advancement of social science.

Leslie's essay is so rambling that it is difficult to know what he is trying to say or what he believes to be the truth. There are also striking logical lapses: for example, he questions the brain size data by pointing out that larger brains usually go with larger bodies, apparently failing to appreciate that this reinforces Rushton's argument, since Mongoloids (who have the biggest brains) have smaller bodies than Negroids (with the smallest brains).

The idea that humans have large, prominent genitalia in order to promote pair bonds he attributes to a 1981 paper by Lovejoy. This makes me wonder where he was in 1967 when Desmond Morris wrote *The Naked Ape*. Anyway, the trouble with this idea is that humans are not characteristically pair-bonding animals. Gibbons and marmosets may be, but real pair-bonding is seen mostly in birds. Generally speaking, humans mix the promiscuity of chimpanzees with the harem-building of gorillas and the rapist tactics of orangutans (not surprisingly, our nearest relatives). The idea that humans are at the apex of an evolutionary development towards monogamy is a fantasy deriving more from moral hopes than behavioural observation. Scientists should be concerned with what *is*, not what they *would like* to be.

But herein lies the real problem with Leslie's article. He seems to argue that social science can never be value free and therefore we should abandon attempts to make it so. Since this is his own position, he then projects the same attitude onto Rushton and presumes that he is engaged in deliberate mischief-making. I suspect the accusations of 'fraud' are probably actionable but that is Rushton's business.

Another thing that Leslie seems to be saying (insofar as anything he says is clear) is that race does not exist. In reply to this I will adopt his anecdotal approach and tell a story of my own. When I first taught at a California University I had to subject myself to finger-printing and answer many questions that I considered impertinent, including 'what is your religion?' and 'what is your race?'. Finding this slightly offensive, I answered 'none' to both questions, whereupon the Mexican girl behind the desk scratched out my second answer and wrote 'Caucasian'.

My question to Leslie is this: how did she know I was Caucasian if race does not exist, and how are government agencies able to use it as a basis for affirmative action?

The fact that any categorization is arbitrary at its borders does not negate its descriptive usefulness. People vary continuously on many traits, but psychologists still find it useful to compare groups of, say, typical extraverts and introverts. (My resistance to the Californian exercise was based on what I perceived as a political rather than scientific motive for making racial categorizations.) And if there are group differences, shouldn't we be able to study them scientifically even if the results concur with unflattering stereotypes. Evil-intentioned people might quote these results for their own purposes but ignorance of the truth will not rid the world of evil people.

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Any hypothesis which seeks to relate racial and sexual variables is certain to arouse the most profound reactions. Its examination is likely to test to the limit, if not beyond, the individual human capacity to exercise objective judgement. For this reason it is imperative that debate be addressed dispassionately in a manner appropriate to a genuine search toward truth rather than demagogically as in some political debate.

The Rushton paper demanded a cool, reasoned rebuttal. Unhappily, when the rejoinder came it was in the form of an Address to an international meeting which had a quite different agenda, and it was couched in language and presented in the kind of inflammatory manner that the subject, being so susceptible to excite, needed so much to avoid.

Leslie's response raises a number of important, separate but related issues which need to be unravelled. The first is the substantive question: is the Rushton hypothesis viable? There is no doubt that the dialect of evolutionary biology used in the original paper has been widely discredited and that some of the behavioural variables quoted by Rushton are highly suspect. It is understandable that these flaws should be scornfully rejected by his critics. But questions remain. To reinforce criticism alternative explanations for apparently legitimate variations should be provided. Why is it, to take an example from the physical domain, that there appears to be variation in gamete production, and, to take a behavioural example, variation in patterns of premarital coitus? How should it be that variations in cranial capacity and brain weight have been recorded and that there is evidence for gross variation in sexual activity? It is not enough to cry wolf. Can variations such as these be explained away as artefacts of faulty methodology? If so, the faults should be explicated and thus laid to rest. Variations that appear valid should be recognised and where appropriate replicated or confirmed.

The whole concept of race needs more careful and close analysis. Is it so objectively dubious a construct in human affairs as never to be used? If not, in what areas of for example epidemiology or social science is it a relevant or useful explanatory variable? How should it be defined? Such questions are at the heart of the Rushton–Leslie divide yet one noted scholar invited to comment told me he found "each deserving of severe criticism ... Rushton is without rigour ... Leslie's criticism is clearly 'over the top' and seems also to be totally misdirected". That such observations are possible demonstrates the need for more dispassionate enquiry and debate which can only illuminate, but censorship distinguish.

It may be that whatever physical or behavioural variations can be confirmed are as irrelevant to epidemiological or psycho-social understanding as skin colour. But the argument needs to be stated and defended, not assumed as a holy tenet which it is scientific heresy to question. Alternative explanations will then be advanced to account for whatever variations may remain. As matters stand, there is a danger that certain branches of science, by fearing to enter the arena, fail to meet their obligation.

I have a more general concern. The social sciences are often by necessity inexact. Complex relationships can be considered in a *confirmatory* mode of enquiry or in an *inquisitive* mode. The former selects data and arbitrary interpretations in order to confirm an *a priori* view of the world. The latter postpones interpretation and is reluctant to ascribe causal relationships without the most undeniable evidence. One guides the data to confirm the conclusion, the other permits the conclusion to be guided by the data. One presents itself more definitely, more stridently, the other can be hesitant, conditional, tentative. But this is not all. The confirmatory mode is generally permeated by a moral, political or religious stance, and is thereby deeply emotive, the other is motivated by a spirit of genuine enquiry. One is manipulative, using enquiry to bolster its own presuppositions and employing every gambit to gain acceptance, the other may at times appear ideologically barren, preferring only the gentler voice of reason. Most of us have had experience of both modes of approach and the depth of passion and power play that can erupt when the confirmatory line is even questioned.

The second issue implicit in Leslie's paper raises the question of censorship in general and whether, in this particular case, publication was justified. There is the closely related matter of the use of the emotive term 'racism'.

In a recent balanced and telling critique of Rushton's general position, James Flynn has written "As the Canadian media over the last few months amply demonstrate, Rushton has been the target of much abuse and labelled a racist. There is a real danger that his future work will not get a hearing, thanks to outlets being intimidated by the fear of unwelcome press scrutiny. This is wrong in itself and it is worth remarking that the truth can never be racist, nor can telling the truth as you see it, assuming there is no evidence of wilful neglect of evidence, an accusation Rushton need not fear. Suppressing Rushton's views also means that those who believe they can make a reasoned case against him are silenced, for lack of dialogue, which is to say everyone is the loser" [1].

I believe a journal has a duty never to allow preconception or prejudice to influence publication. In the long term, truth is unassailable and what is false will be condemned. Condemned not by derision, not by censorship, but by reason.

If courage and intellectual integrity are needed to examine certain aspects of the world that we desperately hope are not as they might at first appear then the requisite courage and integrity must be forthcoming.

Racism is by definition prejudiced; the only way to decide whether an hypothesis is or is not racist is to give it the opportunity to present itself so that it can be openly examined. Only then, if it is universally rejected, should it and its proponents be labelled racist. It was, after all, social science that first drew attention to the blind captivating power of labelling.

The question of peer review is raised. After twenty-five years of observing the process in operation I have come to the conclusion that, like democracy, it may not always be effective in achieving its ends but it is the best method available. It would help to balance its most serious blunders if journals, especially those like *Social Science & Medicine* that deal with different disciplines and subjects, published more commentaries immediately following contentious material. The difficulty is the purely practical one of obtaining a fair representation of different perspectives in reasonable time. In the present case, for example, several leading scholars declined (for contrasting reasons) to comment and it has taken several months to assemble the comments that now appear.

However, there is to my mind, a far more serious threat to standards than the occasional lapse of peer review. This is the unceasing proliferation in the number of journals, increasing at the rate of more than one a day. There is less and less time to review, more and more chance to publish; less and less time to read, more and more material needing to be read. The grave responsibility of those launching new journals should be as carefully measured as should the judgement of librarians forced to select from an ever widening choice out of a steadily contracting budget. It is the kind of real world dilemma that some of our more inward-looking colleagues tend to overlook, but it is one that requires urgent reappraisal.

My last point is that, as my old friend and editorial colleague Charles Leslie knows, I regarded his choice of subject for an invited opening Address at an international conference with a prepared agenda as inappropriate and mischievous. It was inappropriate for a number of reasons: it did not bear upon any conference theme; at the time only a very few of the audience would have seen the Rushton paper; it raised questions of peer review which were not directly the concern of an international audience gathered to consider some of the most important topical problems affecting the social sciences and their relation to medicine and medical care; no-one present could have been expected to harbour any racial prejudice; it was designed for the hustings and to pre-empt conference discussion away from all designated themes. It was a privileged occasion, with publication provisionally guaranteed (without peer review!). But, most important, in so far as it concerned the problem of AIDS, it deflected attention away from such pressing global issues as raised, for example, by the influential views of Mr Abdullah al-Mashad, head of Egypt's Fatwa Committee, as quoted in the *London Times* shortly before the meeting opened. "We must purge society of the Aids patient and those like him, because his existence causes public harm". According to the report, Mr Mashwad then "suggested starvation and denial of medicine as the means of killing the patients. (He) added that pregnant women with Aids must have abortions even if the foetus was more than four months old" [2].

In short, I found it profoundly disappointing that such a strong and deserving case, and one that needed to be presented in the same dispassionate style as its adversary, was handled in such an unfortunate manner.

REFERENCES

1. Flynn J. R. The psychologist. *Bull. Br. Psychol. Soc.* **9**, 363, 1989.
2. *London Times* 4 July, 1989.

REJOINDER

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The comments on my essay speak for themselves in a manner that will not be misunderstood by most readers, and I hope that what I have written is equally clear. For example, I do not myself deny the reality of racial variation, but simply give a narrative account of changes in the ways variations between and within populations are analyzed by human biologists. In fact, I welcome and enjoy racial variations, along with cultural and individual variations, but with a profound sadness that Peter McEwan, despite my efforts, is not persuaded that Rushton's work is a corruption of science. Opinions vary, and his is that my address is written in an inflammatory manner, while Rushton's essay is a genuine scientific effort. He quotes an unnamed scholar as saying that my essay is "totally misdirected", yet he judges Rushton's work to deserve "dispassionate enquiry". Readers will realize that an essential part of my argument is that scientific work requires passion. Particularly in the social sciences we must examine what we are passionate about, and how our passions influence our work.