

DISCUSSION

EGGS, EGGPLANTS AND EGGHEADS: A REJOINDER TO RUSHTON

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"This taboo on race will surely become a major topic of investigation in the 21st century by sociologists of science. There is no parallel to it in history. Not the Inquisition, not Stalin, not Hitler."

Philippe Rushton, *The Scientist*, May 14, 1990, p. 19.

"All social problems in the world are related to ethnicity"

Philippe Rushton, quoted in *The London Free Press*, January 13th, 1990

"When one is up to no good, it is useful to have an excuse. Ideologies often look for their excuses in science. But they pay little heed to science's underlying values, or to the caveats with which scientists surround their arguments. Ideology's only interest in science is the arguments it can furnish in support of its own case. To obtain these, it does not hesitate to distort and pervert scientific argument."

Francois Jacob, *Racism, Science and Pseudo-Science*.

"Race, once a core anthropological concept, is no longer supported by a majority of members of the discipline."

Lieberman, Stevenson and Reynolds, 1989, *Anthropology and Education Quarterly*, p. 67.

Criticizing Rushton's work puts us in something of a quandary: in one sense, any criticism is too much, since it gives Rushton's ideas more scientific credibility than we believe they deserve; in another sense however, even a relatively lengthy critique is not enough to explicate all of his errors, misunderstandings and misinterpretations. Our original article (Weizmann, Wiener, Wiesenthal and Ziegler, 1990) was deliberately brief, omitting many topics that could have been covered. Rushton (1990) has used his reply to reiterate and add to his claims (his tables and figures have been published at least twice before), while ignoring many of our criticisms. In this rejoinder we shall once more attempt to restrict ourselves to major issues.

The r/K model

In the generalized form popularized by Pianka (1970), the r/K model suggested that certain environmental conditions would lead to particular

traits. Under stable and predictable conditions, population growth would reach a near-maximum, and natural selection would favour traits (K-selected traits) that enable an organism to seize, hold and exploit resources in the face of high intra-specific competition. By contrast, under unstable, unpredictable, and less benign conditions, natural selection should lead to traits (r-selected traits) that enhance the ability to reproduce and disperse rapidly. In our earlier paper we stressed that these life-history "traits" were phenotypic, not genotypic, adaptations which do not necessarily covary with one another. We also indicated that the linking of particular traits to r- or K-selection was arbitrary, since, as Caswell (1989, p. 296), has stated recently, "...r/K theory *per se* has almost nothing to say about life-history traits, since it is based on the logistic equation [of population growth], which includes no age- or stage-specific vital rates."

Although Rushton made no previous mention of the problematic status of the r/K model within ecology, he does not contest our account of the severe criticisms of the model made by ecologists. He defends his position by citing a few socio-biologists who have found r/K useful. While as Bruton (1989, p. xiv) notes, many ecologists currently consider the r/K concept to be outdated,

1. The "Eggs" and "Eggheads" of the title refers to what Rushton claims are two contrasting group-evolutionary strategies; the maximization of biological investments in the organs of reproduction vs investments in the organs of cognition. "Eggplants" refers to the chief ingredient in a recipe offered by one of Rushton's sources, the anonymous author of the *Untrodnen Fields of Anthropology*, for remedying male deficiencies in the former.

we have no quarrel with it as a heuristic, which is how ecologists who still find the concept useful use it. Our criticisms, and those we cited, were directed specifically at versions of r/K which link selection narrowly to rigid genotypic traits. Properly understood, the r/K model links environments to evolutionary adaptation; it is not a theory of genes. As Caswell notes: "The existence of phenotypic variation in life-history traits is obvious." "The heritability of life-history variation is more problematic (Caswell, 1989, p. 296)."

We also have no objection to the use of ecological and evolutionary models, including r/K , for the study of ethnic/race/population differences. Indeed such models have for many years been used to analyze the adaptive strategies of ethnic groups (Barth, 1969; Lauwagie, 1979; Hannan & Freeman, 1989). Lauwagie (1979) discussed Gypsies and Travellers in different modern states as ethnic r strategists (complete with very high fertility), but her analysis was neither genetic or racist. These ethnic groups are not a single genetic population, and few would consider fortune-telling, nomadism, tinkering or scrap dealing as genetic traits.

Rushton's use of r/K to explain ethnic differences is not original, although he does not cite any of the prior literature on the topic. Similarly, Rushton's notions of racial differences in precocity, twinning and cranial capacity as covariants of intelligence are not original either. While Rushton does not cite him, it was Jensen (1973, 1980) who first proposed these ideas. Rushton's "contribution" has been to add some additional human traits to this list of covariants, and graft it onto a particular variant of the r/K model in which selection was hypothetically linked to differences between mammalian and insect life-history traits (Pianka, 1970). Rushton thereby converted what is essentially an ecological model into a rather primitive trait-instinct theory.

Rushton argues that human r/K traits are largely genetically based, and that individuals and groups, particularly races (Mongoloids, Caucasoids and Negroids [sic]), differ and to a large extent are defined by their genetically determined positions on an r/K continuum; Mongoloids are the most K , Negroids the least, and Caucasoids are intermediate between the two. (Although not generally remarked upon, Rushton places Caucasoids much nearer the Mongoloid end of the continuum than the Negroid.)

In our original article, we charged that Rushton's application of r/K theory to humans

was seriously flawed, and that even assuming a link between r/K selection and evolutionary traits, many of his predictions were arbitrary and depended on the idiosyncratic assignment of r or K status to different traits. For example, he argued that criminality (hardly a biological trait) is r -selected and altruism (variously identified with "law-abidingness" and "rule-following") K -selected. We pointed out that since K -selection, not r -selection, involves intra-specific competition and aggression, one could as easily argue that criminality is a K - rather than an r -selected trait.

In his reply, Rushton (1990) ignores the point of our criticism and reiterates his original position, arguing that "law abidingness" increases a group's ability to compete with other groups. By defining "keen competition" as a population characteristic, Rushton (1990; see Table 2) evades the point that in the r/K literature K -selected competitiveness characterizes individuals (e.g., Wilson, 1975, p. 101). If one turns back to Pianka's (1970) Table 1 on which Rushton has previously, if not always faithfully (see Weizmann, et al., 1990), relied as the major biological source for r - and K -selected traits, one finds that "keen competition" characterizes relationships between K -selected individuals in the same species as well as between species. Our point is not, as Rushton (1990) seems to think, that criminality is K -selected, but that the model makes no definite predictions at all about crime, and hence the ascription of r/K status to criminality (or most other human social phenomena) is arbitrary.

Rushton (1990) responds to our contention that there is no evidence that K -selection leads to greater altruism by citing assertions from Barash and Wilson to the contrary. Unsupported claims and assertions are not evidence, however. (Rushton also misquotes Wilson: see Wilson, 1975, p. 101). In addition, the concept of altruism as defined by Rushton (1990) and applied specifically to humans is very different from the more general sociobiological concept of altruism discussed by Barash and Wilson; altruism for them is basically nepotism, and has nothing to do with criminality. Although both definitions may be equally Pickwickian, they are hardly synonymous. Apart from these considerations, identifying altruism with "rule following" and its opposite with criminality is simplistic, as anyone familiar with the story of Robin Hood and his Merry Band can testify.

Rushton's assertion that Negroids are less altruistic than other human groups is also

unsupported. In a methodologically well-conducted cross cultural/racial study of altruism (defined in terms of giving actual help and support to others), Whiting and Whiting (1973) observed children from six racial/cultural groups and found the following proportions of children to be above the median on altruism: Nyansongo children of Kenya, 100%; Juxtlahuaca of Mexico, 73%; Targon of the Phillipines, 63%; Taira of Okinawa, 29%; Khalapur of India, 25%; Orchard Town, "a sub-community of Yankees in a New England Town, USA," 8%.

Behind Rushton's (1990) argument that altruism is *K* and criminality is *r* is the implicit (and sometimes not so implicit) assumption that everything human and desirable is *K* and everything animalistic and evil is *r*. His racial hierarchy is a barely disguised hierarchy of humanness.

Race

Any theory like Rushton's that claims biologically based behavioural differences among racial groups requires that people be classifiable into relatively homogeneous groups. If there are genetically determined black minds, white minds and yellow minds, there must be a biologically plausible way of organizing the human species into those types. It is also essential that individuals and groups be reliably assigned to the correct racial category.

Is race a biological category? Are there two, three, or more races?

Rushton believes that determining the identity and number of human races is so easy that he gives E.T. the job, as if a naive observer has insights denied the sophisticated and the trained. Of course, we train as scientists precisely because the world is *not* as it appears. The E.T. conceit illustrates the superficiality of his approach to race. What Rushton would have E.T. do today was actually done more than two hundred years ago by the Swedish naturalist, Carl Linnaeus (1707-1778). He not only named our species *Homo sapiens*, but using skin color also divided it into four varieties: European Man, African Man, Asiatic Man and American Man. J.F. Blumenbach (1752-1840), a German physician and naturalist, to whom we owe the term Caucasian, found five varieties: Caucasian, Mongolian, Ethiopian, Malay and American. His criteria were skull shape, form of nose, skin color and hair type.

Over the last two centuries there have been many subsequent classifications based upon

various phenotypic traits, yielding numbers of races varying from two or three to 35. It is not clear why E.T. and his colleagues would be any less divided on the number of and boundaries between existing races than we terrestrials.

Contemporary taxonomic classification assumes genetic similarity based on common descent. For a race to be of behavioural significance, the members of a race must share genes that lead to common behavioural properties. The only biologically valid approach to definitions of race is in terms of the genes themselves. Such comparisons of genetic material (mtDNA fragments) from different populations proceed by comparing two vectors on *n* criteria. (The description of these methods and their application to racial genealogy relies on the accounts in Jorde, 1985; Cann, Stoneking and Wilson, 1987; Cann, 1988; Stringer and Andrews, 1988; Vigilant, Stoneking and Wilson, 1988). Each population datum is represented as a point in a mathematical hyperspace defined by as many coordinate axes as there are criteria. The mtDNA samples are ordered, based on their mathematical proximity, and the resulting mtDNA "map" is taken as a measure of genetic distance. In order to classify populations into discrete groups, similar mtDNA types are grouped into classes, which may then be grouped further into more inclusive classes. How inclusive the groups have to be to qualify as "races," is not given by the data, but is a decision made by researchers.

While tree diagrams of relationships between populations can be derived from these orderings, there are innumerable ways of constructing the tree, and the resulting tree depends on the particular algorithm employed. In practice, as Jacquard (1985, p. 63) notes, researchers use various methods to analyze their data, explore a single method by changing the parameters which define the groupings, and select the resulting tree or trees that appear most reasonable. In any case, the mtDNA differences among human groups are so small that if the mtDNA were broken at different sites, using different restriction enzymes, one would obtain different groupings and therefore different trees.

The results of these kinds of analyses indicate that the concept of race is biologically inane. Two randomly selected humans can be expected to differ by only about 0.36%. Any large geographic area such as Europe contains 85% to 90% of the entire genetic variation found within the human species. If only 10% of the total

mtDNA species variation represents geographical or racial variation, then two humans drawn from two "races" can be expected to differ only by about 0.04% more than two people of the same race. Racial variation amounts to less than 1/7th of within-group variation.

There is more genetic variation (0.47%) within the sub-Saharan African population than there is within any other human population. Since the variation within Africans exceeds the variation between Africans and any other groups (0.40–0.45%), if one wishes to employ the concept biologically, Africans should not be considered a race, but a group of races. The mtDNA variation within Africa is so high compared to between-area variation, that a West African is likely to be as similar genetically to a Central European as to an East African. The superficial morphological similarities (hair type, nose shape, skin color) so obvious to E.T. and to us, do not indicate much about underlying genetic similarities and differences. It is doubtful whether E.T., if he had scientific training and was without the vulgar prejudices that characterize some humans, would be surprised at this conclusion.

The morphological features that are used by Rushton (and by INTERPOL) to classify individuals into races does not correspond to the genetic reality. Rushton's classification of countries as black, white or yellow, and his claim that this is related to the average number of reported crimes as recorded by INTERPOL says nothing about race as a biological entity. His theory, however, requires that blacks, whites and yellows differ in their criminal behaviour because of differences in their *genetic* predispositions to commit crime.

If we and E.T. (and Rushton?) could see our mtDNA, the way we think about our species might be rather different. We could see the reality of our similarities and differences in new ways that correspond to biological realities, and not to long outdated typological concepts as currently represented by Rushton.

Aggregation

Rushton's discussion of aggregation reveals his continued misunderstanding of the limited value of averaging multiple items, multiple instances and multiple samples. Aggregation provides a more unbiased estimator of true population values only where they are obscured by random error variance. It is of no value in reducing

systematic error. This is clearly revealed in Rushton's use of aggregated cranial size data. Much of this data comes from museum collections, which are not statistical samples in any sense of the term, but were selected and constructed to be typical of the categories they represent. Most of these collections were assembled in the 19th century, when it was generally believed that cranial size/shape were strongly linked to moral and mental traits, and to racial differences in those traits. In fact, these beliefs, which go back to Cuvier and Lamarck, were the initial inspiration for collecting skulls. Aggregation cannot deal with errors due to these attendant biases.

To take a particular example, the Egyptian skulls which constitute the "Ancient Caucasian" sample of Samuel Morton's famous 19th century collection of skulls (Gould, 1978), were collected by George Glidden (Stanton, 1965), the American vice-consul general in Alexandria, an adventurer, a supporter of slavery and the Confederacy. Glidden's motivation in collecting the skulls was to prove that the creators of the ancient Egyptian civilization were white (Stanton, 1965), and that ancient blacks, like those of Glidden's time, existed only in positions of subservience and servitude. Many Northern Europeans and Americans dealt with the embarrassing fact that civilization arose in Africa by denying blacks any significant role in its creation.

How did Glidden know what the race of old skulls found in the Egyptian tombs were? How could he have assigned them to racial categories other than by size and shape, as we know Morton did with other skulls (Gould, 1981)? There is really no justification for assuming that these skulls are what Glidden and Morton purport them to be, and their actual "race" remains unknown. Yet data from these same skulls play a large role in the ongoing argument over racial differences in cranial capacities and their meaning (Gould, 1978; Cain and Vanderwolf, 1990; Rushton, 1988). Rushton (1988) averages (aggregates) the data from Morton's "Ancient Caucasians" with those of "Modern Caucasians" in order to arrive at estimates of Caucasian cranial capacity, thereby making the Caucasoid average larger than the Negroid. The social histories of other museum skull collections are equally peculiar and should not be used, either singly or in the aggregate, to draw any conclusions about racial differences.

Cain and Vanderwolf (1990) have dealt quite adequately with the craniometry-intelligence

issue, and there is no need for us to add anything further (sorely tempted though we may be). We will note however, that Rushton (1990), has now brought in Nuclear Magnetic Resonance Imaging (NMRI) to join mercury, sand, white mustard seed, pearl barley, shot, and rubber bags as a tool for gauging cranial capacity.

Even apart from bias, aggregation does not provide a means of evaluating racial or non-racial interpretation of data sets. When Rushton reports INTERPOL data, he is reporting statistics for a number of countries which differ on a number of clusters of variables which suggest powerful alternative explanations, such as economic development, education, age distribution, including the proportion of adolescent males, climate, nutrition, and the history of colonialism. Converging operations, Rushton's (1990) implications to the contrary, bear no relationship to the value of aggregation.

Rushton's treatment of outliers is inconsistent, but quite revealing of his method and his misuse of aggregation. When the outlier runs contrary to his claims (e.g. Hutterite fecundity), Rushton (1990) treats it as error. When it fits his claims, he proclaims it as an exemplar of the theory. For example, in his reply, he cites Bulmer (1970) (erroneously) as reporting that some [sic] African populations have twinning rates as high as 57 per 1,000. This unusually high rate is reported by Nylander (1981), but *only* for Ibadan, in Western Nigeria: in short, it is an outlier. By 1983, this rate had declined to 23.8 per 1,000 (Weizmann, et. al, 1990). The claim that Negroids necessarily have a higher rate of twinning, a claim central to Rushton's position, is made doubtful by the fact that Swedes have a higher rate of twinning than American Blacks (Nylander, 1978).

We do not have space to deal with all of Rushton's (1990) empirical claims enumerated in Table 1 and characterized as data. We do, however, wish to deal with the claim of Mongoloid intellectual superiority, since that particular claim has attracted much attention. Even the main monger of this myth, and Rushton's source of information on this topic, R. Lynn (1987), has retreated from his earlier (Lynn, 1982) claim that the average Japanese IQ is 111. Our examination of Lynn's (1977) most recent data indicates that the average Spearman's *g* for his Japanese samples is 100.2. Lynn now concludes, rather lamely (1987, p. 817), that for Mongoloids "...a single IQ does not capture at all adequately the

significant features of their intelligence; the intelligence of Mongoloids differs from that of Caucasoids more in the pattern or profile of their abilities than in the overall IQ." (For a fuller discussion of Rushton's IQ claims see Flynn, 1989, 1990).

Racial Origins and the Pleistocene Climate

Rushton disputes our assertion that blacks should be more K-selected than other groups because of their tropical ancestry. In his reply (Rushton, 1990), he states that there is no evidence that the human species evolved in tropical rain forests. We never made such a claim. In fact no one, neither Rushton, nor we, nor anyone else, knows where on the African continent our species evolved. According to our atlas, however, West Africa, home of the largest populations of Africans (and the ancestral homeland of most West Hemisphere blacks) is in the wet tropics, and whatever tree cover it now lacks was produced by fire and axe. If Mongoloids evolved their traits on the tundra, and if Caucasoids evolved theirs in the temperate and boreal forests, then the Negroids evolved theirs in the wet tropics.

Our species did apparently evolve somewhere in Africa and then disperse out of Eden to the other continents and throughout Africa. Rushton seems to believe that present day Africans are some kind of Ur-men who have remained primitive without evolving the K (read human) traits of the racial descendent types found on the other continents. Contemporary African populations, however, are as genetically different from our species' origins as any living in Tierra del Fuego or Lapland.

Rushton's (1990) conjecture that "Siberian cold" led to greater K-ness among Mongoloids is a recapitulation of Lynn's "...evolutionary theory of the principle features of the intelligence of Mongoloids (Lynn, 1987, p. 832)." Lynn's (1987, p. 832) description goes from the emphatic "...if ever there was a niche in which it took brains to survive it was surely Northeast Asia during the ice ages," to the positively hyperbolic: "Even today temperatures of -87 F have been recorded in Siberia, and this would have been just an average day for the Mongoloid peoples during the ice ages."

However, that earlier world was not simply a colder version of today's. V. Geist (1978) describes the periglacial climate as something quite different from Baffin Island or Northeastern

Siberia in January, or from any other existing environment. It was not particularly harsh, but was rather benign and ecologically rich: "...between the Urals and the Altai in the Pleistocene, early man would have had as mixed a bag as camel, moose, reindeer, yak and roe deer (Geist, 1978, p. 201)." "The periglacial ecosystem approached some African ecosystems in diversity, and was apparently far more productive than the climax ecosystems at comparable latitudes and altitudes. (Geist, 1978, p. 188).

Rushton's (and Lynn's) stories about racial origins might have risen above a "*Clan of the Cave Bear*" scenario, were they better informed on current scientific knowledge about Pleistocene climates and eco-systems. We might have been spared the image of ancestral Mongoloids stripping bears of the skins and planning for the future, while their brains grow and their genitals shrink. Rushton's and Lynn's conjectures owe more to Aesop's Fable of the Grasshopper and the Ant than to behavioural ecology and life-history theory.

Evolution, heritability and plasticity

Rushton (1990) accuses us of adopting the double standard of claiming that findings about the influence of environmental factors, such as poverty, generalize from one population to another, while at the same time claiming that heritability does not generalize. The conceptual confusion betrayed by his accusation is informative. Not having money is a fact which will present problems anywhere a money economy exists. Heritability, however, is not a fact: in particular, it is not a gene. Heritability is a descriptive statistic summarizing the degree of genetic variation within a population. It is usually inferred indirectly for human data, and involves a number of assumptions. As Caswell (1989) states, "...it is by now well known that genetic variances and heritabilities are specific to a particular population in a particular environment at a particular time; they are not invariant properties of the species or the trait. Genetic covariances are equally labile, and should not be thought of as fixed properties. They can vary with environmental conditions as well as with such genetic phenomena as inbreeding (Caswell, 1989, p. 301)."

Anyone wondering about the significance of human heritabilities for psychological theory should examine the data in the area where it has been most researched, intelligence. Published

heritability estimates have ranged from 0.2 to 0.8. Which estimate is the one that Rushton would have us believe generalizes? In his review of the area, Henderson (1982) points out that citing a midrange value is no more justifiable than citing an extreme one. As Henderson (1982, p. 411) exclaims, with evident exasperation, "Will the real estimate of heritability of intelligence please stand up?"

Rushton, like many others confuses heritability, a population statistic, with heredity: i.e., genes, which are part of an organismic-developmental process. When Rushton or anyone else finds the gene or gene complexes for hypersexuality, criminality, intelligence, and permissiveness, then we can consider the question of what populations possess this gene or genes and in what proportions.

Rushton's insistence on the generalizability of heritability is actually quite strange, since Rushton's own "theory" would predict that different populations would have different heritabilities for naturally selected traits. Other things being equal, evolutionary selection for a trait tends to reduce genetic variation, and hence trait heritability (Weizmann, et. al, 1990). If selective pressures led, for example, to higher intelligence among Mongoloids, and if intelligence was at all heritable (exactly how heritable does not matter) Mongoloid intelligence would be higher, but its heritability would be lower, than for groups less selected for intelligence.

Finally, Rushton (1990) concedes our point that fertility rates in humans largely reflect social and environmental factors. He thus argues that one should look to indicators of reproductive effort other than population growth to validate his model. His effort to deflect attention away from population growth reveals the absurdity of his extension of r/K selection to humans; after all, K-selection occurs because of resource scarcity due to population pressure (e.g., Wilson, 1975, p. 101). If population can adjust to resource scarcity via phenotypic means, then genetic (i.e., evolutionary) selection will not take place.

Rushton and the French Army Surgeon

Rushton defends the anonymous author of *Untrodden Fields of Anthropology*, whose work he also refers to as "the ethnographic record," and accuses us of denigrating him. Davis and Whitten (1987), however, state in the *Annual Review of Anthropology*, that the author of the

Untrodden Fields of Anthropology, and those of similar works, "...perpetuated a long standing tradition of anthropological pornography, in which cross-cultural data on sexual practice is presented to titillate a Western audience. Titles or publishing houses may even contain 'anthropology,' 'ethnography,' or 'ethnopornography' in their names. (Davis and Whitten, 1987, p. 70)."

We had no intention of denigrating the anonymous pornographer, however; rather our comments were directed at Rushton's standards of scholarship. His use of the "ethnographic record" is not simply to "show the congruence with systematic studies done today, (Rushton, 1990)," but is an important source for "data" on racial differences in females genitalia and comparative penis length for Rushton's three races (Rushton and Bogaert, 1987). It is also the only source of "data" on people "of black admixture." Data from such hybrids is important for claims of genetic determination because hybridizing individuals who differ quantitatively in a polygenic trait should generate offspring with an intermediate level of that trait.

Vidmar (1990) has also called attention to another curious reference on penis size cited by Rushton and Bogaert (1987). This is an article by P. Nobile (1982) which is identified in their bibliography as an article which appeared in *Forum: International Journal of Human Relations*. Professor Vidmar's colleague, Michael Atkinson, could find no library listing of this journal (nor could we), but he finally tracked it down. As Professor Vidmar writes: "it is more commonly known as the *Penthouse Forum*, and can be purchased from the covered display rack at your neighborhood Mac's Milk Store (Vidmar, 1990)."

Additional evidence of Rushton's standards of scholarship is provided by his treatment of the "ethnographic" data. Rushton and Bogaert (1987) state "...in the French West Indies, the size of the penis and the vagina covaried with the amount of black admixture; Arab men, who were often mixed with black had larger penises than Europeans." In fact there is absolutely no evidence that the Arabs described in the *Untrodden Fields* had any black ancestry at all. They are simply described as criminals transported from the French colonies of Algeria and Morocco.

Rushton does not answer our criticisms about his data on racial differences in penis length. Characteristic of his disputative style, he switches the argument from penes to testes. In his reply,

he mentions a paper reporting larger scrotal circumferences in Nigerians than in Europeans. Freeman (1934), however, reported smaller testicular size for American blacks than whites, a finding of which Rushton must be aware since he cites the Freeman reference elsewhere. The author of *Untrodden Fields* agrees with Freeman on comparative testicular size, a "congruence" which Rushton does not mention.

Concluding statement

Although it is easy to criticize Rushton's work, it is difficult to engage him in a serious intellectual exchange. When he is attacked or criticized on a specific datum or theoretical position, he typically ignores the fundamental thrust of the criticism by dragging in yet another datum or source to support his case. Of course, the new material usually turns out to be as flawed as the old. This strategy forms a part of Rushton's version of aggregation; it is as if the whole were greater than the sum of its parts, and as if scientific truth emerges from the sheer accumulation of even questionable data and opinion.

Rushton scavenges whatever materials lay at hand, whether ecology, anthropology, psychology or paleontology. His tendentious borrowing of materials, often themselves tainted by racism, is quite unscholarly. Libraries are full of so-called data which can be used to support almost any point of view about the causes of differences among people. It is the job of the scholar to critically evaluate and weigh this material, and to present his/her conclusions fairly and honestly, acknowledging the limitations of the evidence. Rushton accuses us of nitpicking, of nihilism, of deconstructionism, of setting impossible standards for any theory to meet, and of viewing science as nothing but theory. In fact, all we are demanding is that the normal standards of science and scholarship be applied.

The presentation of Rushton's work in scientific journals and at scientific meetings confers some legitimacy on his work, and forces a response, albeit reluctant, from other scientists. However, the repeated acceptance of his work in reputable publications raises some disturbing questions. These questions go beyond Rushton himself to the performance of those institutions through which science regulates itself.

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