

(AIDS), both in and among countries. On a per capita basis, for example, Black Caribbeans are now known to have as big an AIDS problem as do Africans and Black Americans (Rushton, 1990a).

Finally, a word about Zuckerman's (1990) objection to certain viewpoints being presented on television and to his wanton splicing of political with scholarly criticism (at least 10 references to "racism" in the last two pages, along with a juxtaposition of genetics research with Nazi experimentation). I wonder if someone would be favorably reviewed if they questioned "environmental determinists" for making TV appearances and associated them with positions leading to Stalin's gulags. Exploiting the victims of World War II for current political purpose is quite inappropriate. Unfortunately, I have learned about selective intolerance in the academy first hand since the presentation of my views to the American Association for the Advancement of Science (Rushton, 1989; see Gross, 1990).

A truth must be faced: Across time, country, and circumstance, African-descended peoples show similarities that, on average, differentiate them from Caucasoids who, in turn, show characteristics differentiating them, on average, from Orientals. It may be worth recalling the words of the deeply pious Blaise Pascal when faced with the Copernican hypothesis: "If the earth moves, a decree from Rome cannot stop it." Readers may fervently wish that genetically based race differences in behavior did not exist, but the data show otherwise.

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Unanswered Questions about Racism and Scientific Purpose

Howard H. Kendler

University of California, Santa Barbara

Zuckerman (December 1990) argued that the concept of *race* is too vague to be used in scientific research and that "Generalizations about innate intelligence . . . of large and genetically diverse segments of the species are open to criticism on the grounds that they serve no scientific purpose" (p. 1301). Is it unreasonable to note that affirmative action programs do make racial and ethnic distinctions and consequently scientific studies of group differences are justified because such programs assume implicitly or explicitly that intelligence and other aptitudes are equally distributed among all breeding populations? If a particular ethnic or racial group is underrepresented in certain prestigious positions, such as university professors, it therefore follows from an affirmative action commitment that certain "racist" policies or attitudes are responsible for the social inequality.

Can it also not be argued that Zuckerman's (1990) accusation that those who investigate racial differences, defined in terms of certain genetically determined attributes (e.g., morphological, serological), serve the cause of *racism* is both unfair and misleading? As psychologists, should we not try to distinguish between different meanings of the pejorative term *racism* instead of passively accepting one particular dictionary definition? Should we not demarcate racist attitudes that advocate differential treatment of members of different racial groups, such as the school segregation of Blacks, from those "racist" interests that encourage research in racial or ethnic differences either in disease (e.g., sickle-cell anemia, Tay-Sachs), or academic and athletic aptitude?

Are not genetically rooted differences in behavior possible considering the evolutionary pressures that have been exerted on different breeding populations? Can attempts to solve or ameliorate the social conflicts among different racial and ethnic groups profit from empirical evidence, or is such strife better dealt with by the political power of competing ideologies? Are *racial differences* and *racial superiority* equivalent terms, or is one factual and the other a value judgment? Is it absurd to suggest that one can decouple facts from values and therefore, if genetic differences were found in academic or athletic aptitudes, that such findings would have no direct implications for social policy (Hunter & Schmidt, 1976; Kendler, 1981)? Would not a political democracy be free to decide in such cases which social policy of job allotments be adopted—one based on individual merit, on racial or ethnic membership, or some combination of both? Can a political democracy cope more effectively with clear-cut policy questions than with hidden ideological agendas?

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Truth and Consequences: Responses to Rushton and Kendler

Marvin Zuckerman
University of Delaware

As in previous responses to critiques of his work, Rushton (1991, this issue) restates his "truth," ignoring or sidestepping the major criticisms and adding new "data" to clinch his case. Many of the critiques of Rushton's theory and "data" by psychologists, anthropologists, and ecologists have been published since my article (Zuckerman, December 1990) was written. Readers should consult these as well as Rushton's responses to them. I will limit my response here to the points made in Rushton's comment.

"Populations that produce the fewest gametes average the largest brains" (Rushton, 1991, p. 983). This correlational statement is based on an ordering of three populations (races) on three variables: dizygotic twinning, fertility (birth rates), and

brain size. Rushton interprets differences in dizygotic twinning as an index of a genetically determined gamete production rate, and says that "no known environmental variable is capable of producing the inverse relationship between gamete production and brain size" (p. 983). But twinning is affected by dietary influences on the production of follicle stimulating hormones. Twinning rates have declined by 40%–50% in Europe and Nigeria in the decades since World War II, probably reflecting improvements in diet (Weizmann, Wiener, Wiesenthal, & Ziegler, 1990). Eysenck (1991) has recently suggested that racial differences in IQ could be eliminated in large measure, or even completely by dietary aid. Changes in nutrition could also account for IQ and brain size and height increases over the last half century in economically developed nations (Lynn, 1990).

Multiple births are only a very minor part of population birth rates, and it is differences in the latter that constitute the heart of Rushton's application of *r*-*K* theory to humans. Birth rates (fertility) reflect human beliefs, values, and the use of contraception as well as gamete production, and contraception is obviously a more crucial factor than gamete production. All of these, except gamete production, are known to be related to social class, which has been ignored as a variable in most of the racial comparisons. The race difference in fertility is confined to Blacks of lower education. Among Blacks with any college education and among wives of professional men, the birth rate tends to be lower for Blacks than for Whites (Weizmann et al., 1990). In regard to head and brain size, Rushton ignores the anomalies pointed out by his critics. Gross brain weight is more relevant than skull size to any hypothesis involving brain function, although it is still neurologically naive. Black Americans were shown to have heavier brains than American, French, and English Whites, and these in turn had heavier brains than Kenyan Blacks (Tobias, 1970). As with personality, the variability within the three "races" makes general comparisons among them meaningless, and aggregation only serves to hide the variability.

Rushton (1991) implies that Stringer's (1990) review of the evidence for human evolution somehow supports his own theory of an evolutionary progress in intellectual capacity and social and sexual restraint going from the Black to the Mongoloid races. All that paleontology shows is that all extant human races are variants of a species that evolved in East Africa about 200,000 years ago and then

spread to Asia and Europe, with racial physical features probably evolving later in response to climatic conditions. Contrary to Rushton's assertion, the tropics are regarded as more stable environments favoring *K* selection (Weizmann et al., 1990) and cold weather is normally an agent of *r* selection (Anderson, 1991). Anderson described the fallacies of Rushton's ideas from the perspectives of an ecologist. In terms of ecology alone, one could make a better case for African populations being *K*-selected and Asian populations being *r*-selected.

It is still not clear why Rushton chose to analyze extraversion (E) and neuroticism (N) and ignore psychoticism (P) in his analysis of Barrett and Eysenck's (1984) cross-cultural study. Rushton's theory suggests differences in socialization (P), not in sociability (E), between the races. Lack of parental and marital investment, lack of sexual restraint, and criminality are characteristics of the antisocial personality. P and sensation seeking are related to psychopathy, and E is *not* (Harpur, Hare, & Hakstian, 1989). Blacks score *lower*, not higher, than Whites on sensation seeking, and the ordering of the races on P in Barrett and Eysenck's study is in the reverse order to that predicted by Rushton's theory. Rushton totally ignores the basic data from the study showing that overall similarity of personality does not show grouping by the racial characteristics of the countries involved.

Rushton (1991) prefers to use national crime statistics, a dubious index of basic personality differences given the relation of crime rate to socioeconomic status. Rushton faults me for using national data, such as the incidence of antisocial personalities in the American population, but counters the lack of international evidence for his theory in the P dimension with a study of teachers' ratings of "social adjustment" in 4- to 6-year-old children in French Canadian preschools.

The illustration of Nazi research used in my article (Zuckerman, 1990) was taken from a paper by Anne Harrington (in press) and concerned the question of whether all scientific research is beyond challenge on ethical grounds. Admittedly, this was an extreme example, because the research resulted in physical harm to the victims rather than mere derogation. But behavioral scientists also had a role in legitimization of Nazi racial ideology (Muller-Hill, 1988).

Copernicus's hypothesis was rejected on theological, not scientific, grounds. Whatever Rushton believes, his critics have sound scientific reasons for questioning his proposition. His implicit compar-

ison of himself with Copernicus and his unshakable belief in the absolute truth of his conclusions suggest an attitude that is immune to scientific criticism.

Kendler's (1991, this issue) criticism deals with the second part of my article and is limited to the area of intelligence and abilities. Kendler says that affirmative action programs are based on the assumption of equal distribution of abilities among all "breeding populations." Modern population geneticists do not regard Blacks, Whites, and Asians as "breeding populations," and researchers on race do not use morphological or serological criteria for race because there are no such infallible criteria.

Even if one accepts the behavior-genetic evidence that 50% to 70% of the variance in measured intelligence in Whites is based on genetics, this still leaves a substantial influence of environment. It is a common misconception that something with a strong genetic influence is not changeable by environmental manipulation. Affirmative action is not based on the assumption of equal abilities, but on an assumption of some malleability of abilities and of the motivational elements that are also important in academic and vocational achievement.

Kendler (1991) cites Tay-Sachs disease and sickle-cell anemia as examples of worthwhile purposes of racial definition (are Jews a race or an ethnic group?). Genetically caused diseases are not the same as complex traits; the latter are usually polygenetic and based on complex interactions and correlations between heredity and environment that are just beginning to be studied.

Kendler (1991) is correct in saying that policymakers will use congenial conclusions from behavioral scientists. Are the findings on the sources of racial differences firm enough to provide a basis for social policy? How should they be applied? Saying that a trait is 50% genetic and 50% environmental does not tell you which 50% is most influential for a given population, at a given time, in a given environment, or how modifiable the trait is, or how one might go about changing it. In the absence of sound data on interaction effects it would be best to continue to operate on the basis of policies consistent with the basic values and goals of the society.

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Comment on Biaggio and Bittner

Rosamond Gianutsos
Sunnyside, NY

Biaggio and Bittner's (December 1990) article should have been called "Traditional Clinical Psychology and Optometry." My concern is not what the article said, but what it did not say. The authors cited examples from *clinical* psychology; however, there are other areas of psychology that will find optometry a very important allied specialty.

Some years ago, Gianutsos, Ramsey, and Perlin (1988) reported that more than half of the individuals admitted for head injury rehabilitation had significant visual sensory impairments that warranted optometric intervention. We have described ways in which psychologists and others in brain injury rehabilitation can collaborate effectively with optometrists (Gianutsos & Ramsey, 1988; Gianutsos, Perlin, Mazerolle, & Trem, 1989).

Many visual system problems caused by brain injury are not experienced for

what they are; hence they go undiagnosed, untreated, and unappreciated by caregivers, including neuropsychologists. Instead, there is an overreliance on the psychological explanatory constructs, such as neglect, inattention, and dyslexia, without adequate evaluation and treatment of the visual system. Of particular concern, and rarely appraised, are nearpoint acuity, the integrity of the visual fields, and binocular function (Gianutsos & Matheson, 1986). Optometric intervention is often straightforward, and in some cases dramatic results have occurred. In virtually all cases optometric input has clarified the outlook.

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A Working Example of Biaggio and Bittner's Proposed Collaboration Between Psychology and Optometry

Eileen Schanel-Klitsch, Pamela F. Blaskey, and Maria L. Parisi
Pennsylvania College of Optometry

David P. Roeltgen
Hahnemann University

Biaggio and Bittner (December 1990) described an experiment in collaboration between optometry and clinical psychology at Pacific University. They noted the advantage of the university setting for the interdisciplinary collaboration of psychology, optometry, and all the health sciences. It should be noted that this type of collaboration can also exist and flourish outside of the university setting.

At the Pennsylvania College of Optometry, psychologists and optometrists, along with other health-related professionals, have worked closely together in clinical, research, and academic pursuits

for years. For example, at the William Feinbloom Vision Rehabilitation Center, a holistic approach to care for the visually impaired has been used since 1977. Visually impaired clients are seen not only by an optometrist (or an ophthalmologist if necessary) for their visual needs, but also by a social worker and rehabilitation teacher, and by a psychologist when necessary. This interdisciplinary team approach to patient care is the norm, and has been enormously effective in providing quality care (Bailey, Helsel-DeWert, Thiele, & Ware, 1983; Brill, 1976; Magrab & Schmidt, 1980). In The Learning Center at the Pennsylvania College of Optometry, diagnostic and remedial services for children and adults with various learning and behavioral problems are provided. The Learning Center provides care by utilizing the services of psychologists, optometrists, reading specialists, speech and language therapists, psychiatrists, a neurologist, and an audiologist.

Our experience at the Pennsylvania College of Optometry has supported several of the theories that Biaggio and Bittner (1990) proposed. All clients who come to The Learning Center have a thorough visual examination by the optometric staff along with a psychoeducational examination. Approximately 40% of the children who are referred to The Learning Center are found to have visual problems along with their reading, attention, or behavioral difficulties. Discussion of the findings of the examinations at the interdisciplinary meetings allows for the sharing of knowledge and expertise to form a coordinated approach to treatment. Psychologists help the vision professionals develop treatment plans that use the principles of learning theory, and the vision professionals ameliorate visual difficulties that had been undiscovered and may be contributing to learning or behavioral difficulties. The interdisciplinary team approach provides each client with all of the specialty care he or she needs in one setting, allowing for the sharing of information in a timely and efficient manner.

A recent case seen at The Learning Center clearly shows how the interdisciplinary approach to client care works. An 11-year-old girl (JW) had recently experienced a severe loss of visual functioning. Her primary care optometrist could not obtain reliable information from her, nor could the pediatric optometrists at The Eye Institute. Therefore, she was simultaneously referred to The Learning Center for a psychoeducational evaluation and to electrodiagnosis for an electrophysiological workup of her vision. A psychologist at The Learning Center, experienced in test-