

Race, the heritability of IQ, and the intellectual scale of nature

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Article:

I would like to address several questions pertaining to this book.

1. What is a race? The book deals primarily with the notion that different average IQ scores for different "races" in the United-States reflect cultural or test bias. However, the terms "race" and "racial" difference are used improperly throughout the book. The reader is exposed to data on the "white," "black," "Mexican-American" and "Asian" racial groups.

As a scientific term, as opposed to a colloquial expression, "race" is precisely defined to be a reproductively isolated, stable, and genetically distinct subpopulation of a species. There is no "white" race anywhere on the globe. One could say there is a Caucasian racial stock, but this grouping includes the people of Iran, Afghanistan, and northern India who have various shades of brown skin. Neither is there a "black" race. Africa contains a number of different ethnic groups, some of which have Mongoloid ancestors. People with very dark skin can trace their ancestries to various parts of either Africa, Melanesia, India, or Australia. The idea of a "Mexican-American" racial group is very difficult to accept. The ancestors of the present-day population of Mexico include the Caucasians of Spain and the native people of Mongoloid extraction. There has been substantial reproductive intercourse between men and women of European, African, and Asian ancestries in the United States, which further blurs any ancient racial boundaries that may have existed.

The author makes no mention of how "black" and "white" were defined in the various studies he cites or how people with parents from different ethnic groups were assigned to one group or the other. It seems to me that if one is seriously interested in the relation between skin color and performance on various tests, which is an enterprise of dubious merit in the first place, then at the very least one should use a light meter to actually measure reflectance of each person's skin.

2. Is IQ normally distributed? The author argues persistently that mental ability is polygenically determined and therefore normally distributed in the population whereas achievement is skewed because it is a product of "abilities, disposition and training." He claims that for the American population in particular "the normal distribution of intelligence is probably the most unrivaled theory in all of psychology" and uses this conclusion to bolster his contention about genetic determination of IQ.

However, the author does not even mention the excellent work of Dorfman (1978), who has demonstrated beyond reasonable doubt that the IQ distribution is not normally distributed: that IQ deviates more from normality than does height: and that a virtually perfect normal distribution of IQ scores reported by Cyril Burt was fabricated. Jensen further expresses his, own bias when he calls an obviously nonnormal set of IQ scores reported by Wechsler "a slightly negatively skewed normal curve" and refers to other instances of skew as "anomalies." If a distribution has statistically significant skew or kurtosis, then it is *not* normal.

The author maintains that IQ for a large sample of American "whites" is "near to normal as can be," whereas for "blacks" the distribution has a lower mean and a slight positive skew. Here Professor Jensen gets into real trouble. Approximately 10 percent of the American population is of African ancestry, whereas about 88 percent is of European ancestry. Pool the "near to normal as can be" IQ distribution for 88 percent of the people having a mean of 100 with the positively skewed distribution of another 10 percent with a lower mean, and you *cannot possibly obtain a normal distribution of IQ for the entire population* in the United States.

Given the diversity of factors which can cause data to deviate from normality, I don't think the shape of a frequency distribution can tell us much about the importance of training for performance on a test of mental ability. Direct measures of experience are necessary, but no such measures are presented in this book.

3. What is the "heritability" of IQ? The assertion by Jensen (1969) that "the best single overall estimate of the heritability of measured intelligence" is .81 has been strongly criticized in articles by Bodmer and Cavalli-Sforza (1970), Hirsch (1970), Lewontin (1970), and many others, as well as in the book by Kamin (1974). The author does not mention or cite any of his critics, and as far as I can determine from his latest book he has not retracted any of his previous views. Instead he states baldly: "Most geneticists who have surveyed the evidence are agreed that some substantial part, probably as much as 80 percent or more of the IQ variance within families (i.e., between siblings) is genetic," and he refers to "the polygenic theory of intelligence, which now generally accepted by geneticists." I would like to know where this opinion poll of geneticists has been published and who comprises this great silent majority to which Jensen alludes.

The author claims published estimates of heritability of IQ range from about .50 to .90, but he does not mention recent studies reporting data for twins reared in the same homes - where "heritability" is in any case liable to be overestimated because of the shared environment - that suggest values substantially lower than .5 (e.g., Scarr-Salapatek 1971; Wilson, 1978).

The author does not even mention the exposure of the fraud committed by Cyril Burt, and instead cites Burt abundantly and relies heavily on Burt's "data" on monozygotic twins reared apart to claim that the environmental influences on IQ are normally distributed." He also writes: "In any particular study, one can always find methodological reasons for some doubt, The convergence of evidence from many studies using different methods, however, leaves little if any doubt concerning the relatively high heritability of IQ." For one thing, the evidence does not "converge" on a particular number. Furthermore, flawed studies will converge on a *biased* answer it, as Kamin (1974) has shown convincingly, the methodological flaws consistently influence the results in favor of higher correlations among biological relatives.

In my opinion the traditional concept of heritability should be discarded altogether because of evidence that hereditary and environmental factors interact throughout the process of development of the brain and behavior and render simple additive models such as $P - G + E$ invalid (Wahlsten 1979; see also Lewontin 1974). What Jensen has written on genetics in his latest book reveals an extremely narrow and outmoded perspective on the biological sciences.

4. Are humans at the pinnacle of an intellectual scale of nature? The author cites research on learning in "animals at different levels of the phyletic scale that is, earthworms, crabs, fishes, turtles," and so on, and refers to "the turtle, which is phylogenetically higher than the fish" in order to muster evidence for the "biological reality" of intelligence or g .

It is wrong to speak in typological language about "the turtle" or "the fish" when in fact only a very few species sampled selectively from among all living species of turtles and fishes have ever been trained on a learning task, let alone a wide range of tasks which would be necessary to test for the existence of g in nonhumans.

Taxonomically, "turtle" is one of four orders in the class *Reptilia*. The contemporary fishes are grouped into three classes - the jawless lampreys and hagfish (*Agnatha*), the cartilaginous sharks and rays (*Chondrichthyes*),

and the bony fishes (*Osteichthyes*). Although fish appeared in the fossil record earlier than reptiles, living fishes cannot be characterized as "lower" than reptiles. Fish continued to evolve after giving rise to the amphibians, the ancestors of the reptiles, and many contemporary fishes show intricate social behavior, including parental care of the young, which is lacking in many reptilian species.

The Aristotelean notion of a linear hierarchy of species or *scale nature* has been repudiated by Hodos and Campbell (1969), especially as it pertains to rankings with respect to the learning ability of various species, but apparently Jensen did not take note of this important paper.

I would also like to point out that two independent reviews of the literature on heredity and learning in animals both concluded that for several nonhuman species which have been, extensively investigated, there is no evidence of a general "intelligence" factor, although hereditary influences on task-specific performance are well documented (Wahlsten 1978; Fuller & Thompson 1978).

From my reading of those features of Jensen's book about which I have specialized knowledge, I conclude that this work is so lacking in balance, rigor, and erudition I cannot recommend it, either to professionals in the life sciences or to the general reading public.