

# EDUCATIONAL THEORY

## Jensenism: The Bankruptcy of "Science" Without Scholarship<sup>1</sup>

By Jerry Hirsch<sup>2</sup>

It perhaps is impossible to exaggerate the importance of the Jensen disgrace, for which we must all now share responsibility. It has permeated both science and the

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1. Some of this material and various parts of this discussion were presented during 1973-74 in addresses to: Case Western Reserve University, Cleveland, Ohio; The Center for Advanced Study in the Behavioral Sciences, Stanford, California; The Psychology Laboratory, Dorothea Dix Hospital, Raleigh, N. C.; The Harvey Mudd Graduate Center, Claremont, California; Hope College, Holland, Michigan; The New York Academy of Sciences; St. Louis University, St. Louis, Missouri<sup>a</sup>; The University of California, Berkeley, California<sup>b</sup>; The University of Illinois at Urbana-Champaign, The Department of Psychology and also the Department of the History and Philosophy of Education; The University of Minnesota, Minneapolis; The University of Pittsburgh; The University of Wisconsin, La Crosse; The University of Wisconsin, Madison<sup>c</sup>; West Virginia University, Morgantown; Wright State University, Dayton, Ohio.

Appreciation is expressed for the many helpful comments and suggestions received on these and other occasions. Also, the following "stonewalling" is to be noted: "When Jensen was shown xeroxed copies of Hirsch's 'evidence,' he refused to read them and declined to make any comment. 'You can quote me as saying what Calvin Coolidge said: No comment, and you can quote me on that.'"<sup>d</sup>

<sup>a</sup> J. Fargen, "Hirsch Contests 'Jensenism'," *University News*, St. Louis University, Vol. 53, No. 19 (March 1, 1974).

<sup>b</sup> A. Thompson, "Psychologist Blasts Jensen," *The Daily Californian*, University of California, Berkeley, Vol. 5, No. 116 (February 26, 1974), p. 3.

<sup>c</sup> S. Kealy, "Hirsch Blasts Jensenism," *Daily Cardinal*, University of Wisconsin, Madison, Vol. LXXXIV, No. 134 (April 8, 1974), p. 1.

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2. At the end of my 1972 Behavior Genetics class at the University of Illinois, a student accused me of "giving us everything to read but Jensen" and he was correct! Accordingly, the next class was required to read the complete HER; each student was assigned a specific portion of its references to read, and those original discussions were to be compared with Jensen's treatment of the same material. This experience proved to have enormous heuristic value. No professor was telling those students what to think or believe. They had seen for themselves and learned from one another.

Appreciation is expressed to the following individuals from my autumn 1973 University of Illinois Behavior-Genetic Analysis class and from my 1973-74 American Association for the Advancement of Science Chautauqua courses at Stanford and Claremont, California, for the bibliographic research they contributed: Mary J. Allen, Meredith Behr, Renee Krasnow, Robert MacCallum, Andy Neher, Karen O'Donnell, Maria Santostefano, Merrill E. Sarty, Louise Shvon, Marc Simon.

I have examined personally and take full responsibility for everything included in this discussion.

universities, and hoodwinked large segments of government and society. Like Vietnam and Watergate, it is a contemporary symptom of serious affliction. It began in February, 1969, when the *Harvard Educational Review* (HER) published the 123-page article by educational psychologist Professor Arthur R. Jensen of the University of California, Berkeley.<sup>3</sup> Jensen marshalls a large amount of evidence there, which he interprets as revealing that (1) intelligence, as measured by I.Q. tests, is mainly (about 80%) genetically determined, (2) on average blacks score (about 15 points) below whites quite regularly, (3) this racial difference in intelligence is attributable mainly to heredity and not to environment, because of the inverse relation between heritability and teachability, which explains (4) "The Failure of Compensatory Education" (opening statement of HER). His interpretation is supported by a brow-beating 159-item bibliography which has overwhelmed both critics and disciples, who join in extolling the "scholarly article. . . . He may find his position politically distasteful but. . . . it is the only way to maintain his scientific integrity. . . . [because] Jensen has done his homework."<sup>4</sup>

The discussion herein focuses on HER although other material is also considered. In the first two sections below, I briefly note some of the wide-ranging and uncritical reactions which Jensen's work has received. The remaining sections are given over to a documentation of the thesis that the Jensenists either knowingly have misled academicians and laymen alike, or failing that, have been simply irresponsible. Both could be true.

#### I. SCOPE OF CONTAMINATION

On a nationwide broadcast William Buckley (1974) assured a vast public, ". . . Arthur Jensen's studies . . . have not been invalidated even though, God knows, they've been subjected to the kind of scrutiny that nothing has since the Ten Commandments"<sup>5</sup>—a "truth" which over the years had repeatedly been widely publicized in nationally syndicated columns.<sup>6, 7, 8</sup>

In an unconvincing disclaimer datelined "Executive Office, The White House," Daniel Moynihan, counselor of "benign" neglect to the Watergate Presidency, wrote to reassure colleagues (soon to find essential federal support discontinued for research, training, education, economic and social programs) that, though Jensen was not "must reading" for the Nixon cabinet, "The subject did once come up in a Cabinet meeting—a perfectly casual enquiry by someone in quite a different field as to whether people in the field accepted this view. . . . No one, least of all the President, had any position. He turned to me. I said that Dr. Jensen was a respectable scientist who had set forth a hypothesis to explain a real enough phenomenon, but. . . .geneticists could not yet say what is the biological basis of intelligence. . . . Lee DuBridge

3. A. R. Jensen, "How Much Can We Boost I.Q. And Scholastic Achievement?" *Harvard Educational Review*, Vol. 39 (1969), pp. 1-123.

4. S. S. Baratz and J. C. Baratz, "Early Childhood Intervention: The Social Science Base of Institutional Racism," *Harvard Educational Review*, Vol. 40, No. 1 (1970). Reprinted in Thomas Weaver (ed.), *To See Ourselves: Anthropology and Modern Social Issues* (Glenview: Scott, Foresman and Company, 1973), p. 308.

5. W. F. Buckley, Jr., "Mr. Buckley Defends His Four Reforms," Firing Line (Telecast), *Southern Educational Communications Association* (February 3, 1974).

6. J. Alsop, "Jensenism Gains Unqualified Help," *Champaign-Urbana News Gazette* (August 26, 1973), Editorial Page.

7. W. F. Buckley, Jr., "On 'Inferiority' of the Negro," *Champaign-Urbana News Gazette* (March 23, 1969), Editorial Page.

8. *Op. cit.*, (February 13, 1973), Editorial Page.

[Nixon's then Science Advisor, ex-President of California Institute of Technology, a physicist] . . . confirmed what for me was basically a guess."<sup>9</sup> Moynihan's remarks were prompted by a quotation from news accounts saying that Jensen was "must reading" in the Nixon administration, which appeared in the exchange (discussed below) between Jensen and Dr. Elizabeth Alfert.

In the *University of Toledo Law Review* (1970, nos. 2 & 3) about 700 pages of an entire double issue were devoted to the problems of disadvantaged students and affirmative action. The symposium featured over 50 pages of rambling "expert" (i.e. Jensen) discussion, introduced with the following remarks by the editors:

*The assumption underlying most compensatory education programs is that the students of minority cultural backgrounds possess innate abilities equal to those of other students even though those abilities do not appear on standardized ability test scores. Most compensatory education programs, therefore, are aimed at minimizing the educational difficulties of minority students by intensive, short-term exposure to the learning environment of the dominant culture. Dr. Arthur R. Jensen **CONTROVERTED** this **PREMISE** in his controversial article "How Much Can We Boost IQ and Scholastic Achievement?" . . . hypothesizing that differences in intelligence are primarily genetically determined and is thus unalterable to any significant extent by environmental manipulations.<sup>10</sup>*

Now, four years later when we appreciate the disaster that has overtaken the legal profession, we see their being duped by Jensenism to be only one small part of their enormous difficulties.<sup>11</sup> Two articles (C. Gerard Fraser, December 7, 1973, and Arnold H. Lubasch, January 8, 1974) and an editorial (January 21, 1974) in *The New York Times* report how Warren E. Burger, Chief Justice of the United States Supreme Court, and Irving R. Kaufman, chief judge of the United States Court of Appeals for the Second Circuit, have recommended that action be taken by the legal profession "to Raise Criteria For Lawyers in Federal Courts" and to effect "Curbs on Inept Trial Lawyers," because there are now too many "incompetent, unexperienced and unprincipled" lawyers in court. "Chief Justice Burger believes that at least one-third of the 375,000 lawyers in the country are not competent to argue a case in court."<sup>12</sup>

Neither justice makes any attempt to attribute their difficulties to racial inferiority, because the overwhelming majority of the incompetents and their professors (now gulled by Jensenism), who failed to teach them, are "lily white."

Of course, lawyers have no monopoly on incompetence. In *Nature*, from an account of a United States National Academy of Sciences report on the life sciences, we learn that "Among the 13,000 biological journals now churning forth from the presses the academy committee finds it possible to identify . . . about 1,000 journals in which more than 90 per cent of the truly significant work in biology appears. *Biological Abstracts* covered some 7,400 journals in 1968 but most of these are

9. D. P. Moynihan, "Comment: Jensen Not 'Must Reading' In the Nixon Cabinet," *Journal of Social Issues*, Vol. 26 (1970), p. 191.

10. *University of Toledo Law Review*, Editors' introduction to "Selection of Minority Students in Higher Education" by A. R. Jensen, Nos. 2 and 3 (1970), p. 445. Bold emphasis is added; italics appear in original.

11. M. T. Bloom, *The Trouble With Lawyers* (New York: Simon and Schuster, 1969).

12. *New York Times*, articles by C. G. Fraser (December 7, 1973) and A. H. Lubasch (January 8, 1974) and an editorial (January 21, 1974).

unlikely to publish anything that will materially advance the progress of science."<sup>13</sup> And, as this tale of Jensenism now illustrates, the situation in education and the behavioral sciences is certainly no better.

Time and again the opposition to Jensenism has resorted to inarticulate and self-defeating hooliganism, so easily perceived as fascist interference with academic freedom and unfettered scientific inquiry.<sup>14</sup> Their negative accomplishment has often been to stimulate newspaper stories and editorials extolling the courage of the Jensenists in their fearless pursuit of "knowledge." Unfortunately, our liberal colleagues seem not to appreciate what was happening, as indicated by the following from my mail: "You realize, of course, that I feel quite strongly that the hooliganism that disrupts Jensen, Herrnstein, et al. is something that one must expect in view of the fact that the indignities done to people are no longer being tolerated. I don't think that violence changes the things that are wrong with society, but I can understand them very well. Therefore I can hardly say that I deplore them; I just think that they are wasting their time."<sup>15</sup> Ironically, we shall now see that all they ever had to do was to read English.

## II. SCHOLARLY INCOMPETENCE

According to Lewontin, "Jensen has surely become the most discussed and least read essayist since Karl Marx."<sup>16</sup> The thrust of my subsequent discussion will be to document in detail the intellectual disgrace that is Jensenism and to show how incomplete is Lewontin's picture: not only has Jensen been "least read" but whatever reading did occur has not been "critical" in a scholarly sense. The "experts" have shown an abysmal ignorance of the literature in their own specialties, despite the "1 or 2 million words of discussion"<sup>17</sup> estimated in *Science* to have been wasted on the heredity-environment pseudo-question brought to such a sharp focus in Jensenism. For example, in the official book review journal of the American Psychological Association we find encomiast Phillip E. Vernon's, "Despite Jensen's superb scholarship . . . minor points . . . might be . . . challenged. I intend to ignore these. . . . In conclusion, I would applaud Jensen for his courage in bringing these problems out into the open. . . ." <sup>18</sup> Two years later Vernon reiterates his approval as he again reviews the same article for the British *New Society* when a ". . . book reprints the article (with minor corrections of details), and this is useful since so many of his critics appear not to have read it." <sup>19</sup> To my knowledge for the first time in its history *Science* turned over the entire book review section of one issue for a panegyrist to reassure its vast readership about, ". . . the nuances and qualifiers that make . . . Jensen's writing credible or at least responsible."<sup>20</sup> (p. 1224) Not satisfied with the foregoing, the point is reiterated in *Contemporary Psychology* by Lee Willerman: ". . . the bitter controversy generated by the publication of Eysenck's book and

13. "'Demise of Scientific Journals' by our Washington Correspondent," *Nature*, Vol. 228 (1970), pp. 1025-1026.

14. "SDS, CAR Silence Banfield in U. of Chicago Disturbance," *Harvard Crimson* (March 23, 1974), *New York Times* (November 23, 1973). Editorial p. 34.

15. E. Tobach, Private Communication (May 14, 1973).

16. R. C. Lewontin, "Race and Intelligence," *Bulletin of the Atomic Scientists* (March, 1970), pp. 2-8.

17. L. Erlenmeyer-Kimling and S. E. Stern, *Science*, Vol. 182 (1973), p. 1044.

18. P. E. Vernon, Review of *Environment, Heredity and Intelligence* by A. R. Jensen, et al., *Contemporary Psychology*, Vol. 15, No. 1 (1970), pp. 161-163.

19. P. E. Vernon, Review of *Genetics and Education* by A. R. Jensen, *New Society* (December 14, 1972), pp. 645-646.

20. S. Scarr-Salapatek, "Unknowns in the IQ Equation." *Science*, Vol. 174 (1971), pp. 1223-1228.

Jensen's (1969) earlier, and more scholarly, discussion. . . . For those interested in reading on this topic, Jensen's monograph is to be preferred . . . excellent reviews . . . already appeared (. . . Scarr-Salapatek, *Science*, 1971), which the content of this review is meant to complement."<sup>21</sup>

Of course, few cases of fervor exceed that of a recent convert from nurture to nature, Herrnstein, whose expiation yields a 249-page book<sup>22</sup> extolling Jensen as its

21. L. Willerman, Review of *The IQ Argument* by H. J. Eysenck, *Contemporary Psychology*, Vol. 17, No. 11 (1972), pp. 585-586.

22. R. J. Herrnstein, *I.Q. in the Meritocracy* (Boston: Atlantic-Little, Brown, 1973). My class has just (Fall 1974) read *I.Q. in the Meritocracy*. There we find that Herrnstein refers or alludes to Jensen (his primary source) no less than 61 times in 249 pages. Herrnstein misrepresents as "a representative sample of 1,000 men" (p. 203) what, in his own reference (Burt 1961, pp. 3, 4, 9, 10), appears as a non-randomly selected set of almost 40,000 cases gathered "at intervals over a period of nearly fifty years." (Burt 1961, p. 4) Moreover, Herrnstein claims that in Barbara Burks' (1928) study ". . . the foster children's I.Q.'s correlated with their natural parents' I.Q.'s more than with their foster parents' . . . *the true father-child or true mother-child correlations were in the .5 range*. In contrast, the foster father-child correlation was essentially zero, while the foster mother-child correlation was about .2." (Herrnstein 1973, p. 183; italics added) Barbara Burks neither had nor presented any data (i.e. no evidence) on the correlations between her foster children's I.Q.'s and those of their natural parents! Herrnstein's account of Burks is simply untrue. On Herrnstein's misuse of Burks we corroborate Arthur Goldberger, whose recent work I am proud to report was inspired by my talk, "Jensenism: Racial vs. Academic Inferiority" at Madison, Wisconsin, April 5, 1974 and whose "assessment of Jensen is close to" mine. (See "Mysteries of the Meritocracy" in N. J. Block and G. Dworkin (eds.) *The IQ Controversy: Critical Readings*, New York: Pantheon 1975).

A most disconcerting and truly dangerous feature of these recent developments emerges in the *credentialist* philosophy expressed repeatedly by Herrnstein. Who speaks and *where* has become more important than what (substance) is said. When Kamin showed the untrustworthiness of Burt's twin-study heritability data, Herrnstein dismissed it as "characteristic of the polemicists in this field to argue that a single unpublished work, like Kamin's, containing no new data, refutes more than a half a century's published research by scores of respected and competent scientists." (*Harpers*, February 1974, p. 103, italics in original—a repetition of his putdown of "this chap at Princeton" on Buckley's Firing Line (Telecast), p. 14 of November 11, 1973 issue (in a series of Note 5).) Kamin's analysis appeared in his invited address, "Heredity, Intelligence, Politics, and Psychology," to the Eastern Psychological Association meetings, May 3-5, 1973. I have learned that the *American Psychologist* refused publication to both Kamin's paper and W.N. Schoenfeld's Presidential Address, "Notes on a Bit of Psychological Nonsense: 'Race Differences in Intelligence'." (Both titles appear in *American Psychologist* 1973, 28, p. 791.) It was at this same meeting that R.J. Herrnstein was suddenly appointed (not elected) to the Eastern Psychological Association Board of Directors. By an incredible coincidence precisely at this time Jensen publishes his first criticism of Burt's work as the lead article in *Behavior Genetics*, (March 1974) a journal, which I am ashamed to say, since its inception has flouted my name on its Editorial Advisory Board, even though I have never once been consulted about anything it has published. Suddenly, Jensen turns on Burt regarding "...the often unknown, ambiguous, or inconsistent sample sizes and the invariant correlations despite varying *N*s from one report to another," etc. The journal alleges that Jensen's initial and final versions were received April 26 and May 22, 1973 respectively, i.e. leaving an incredible 25 days (including four weekends) for mail (?) communication between editors, reviewers and author and for reading, recommending, writing and typing revisions. In the 1972, 1973, and 1974 volumes of that journal, of which I have at hand issues numbers 1, 2/3, 4; 1,4; and 1 respectively, of the 25 other articles handled editorially by an apparently similar procedure, the number of days required for such processing averaged 119 and ranged from a minimum of 38 to a maximum of 233!

Furthermore, Harvard's credentialist-meritocrat now advocates the revelation (with *noblesse oblige*?) of truth from above: "Most people, even most academics, do not have the time, training, or occasion to work through the technical literature on a controversial subject. Instead, they must rely on professionals for a disinterested evaluation." (Herrnstein, *Commentary*, April 1973, p. 62; reiterated in *I.Q. in the Meritocracy*, p. 52.) By Herrnstein's explicit criteria both Jensen and he have the credentials. They are "professionals" (= intellectuals, i.e. Julien Benda's "Clerics" betrayers! See note 121.) So it should come as no surprise to find Jensen reviewing Herrnstein's book with this testimonial: "As a specialist myself in . . . psychology, I can attest that these chapters provide the most up-to-date account of the mainstream theories and research on intelligence that can be found in print today. Both for nonspecialists and students of the behavioral sciences who want an overview of what's what about IQ, this is the book to read." (*Chicago Tribune*, June 24, 1973. Section 7, p. 4.)

authority, heritability as its conceptual keystone (see its Index, pp. 232 and 233) and, devoid of critical acumen as Soviet hagiography, fails to detect the slightest blemish in prophet or dogma—living confirmation of behaviorism's intellectual bankruptcy<sup>23</sup>.<sup>24, 25, 26, 27</sup> and John Dewey's scathingly accurate condemnation of the psychology epitomized at Harvard.<sup>28, 29</sup>

Lest I leave the impression that only behavioral scientists have been gulled by Jensenism, note the following tribute from British geneticist Eaves: "... the HER article . . . is unparalleled in breadth and lucidity as a review of the genetical and environmental determinants and correlates of intelligence. . . [I] commend it as a useful summary of the evidence relating to the genetical determination of individual differences in intelligence and as a source of data and hypotheses relating to differences between races and social classes. . . No one who reads Jensen's papers can fail to recognize a facility with the literature which is shared by few of his critics. . . ."<sup>30</sup> (pp. 250–251)

Even Dobzhansky nods sympathetically: "It is unfortunate that the writings of Jensen (1969), Eysenck (1971), and some others are eagerly exploited by racist propagandists, perhaps without consent of the scientific investigators themselves."<sup>31</sup> (p. 11) And, in Ernst Mayr's updated abridgement of his classic volume on evolution, some solid biological information had to be pruned away to make room for his new

23. J. Hirsch, "Behavior Genetics and Individuality Understood. Behaviorism's counterfactual dogma blinded the behavioral sciences to the significance of meiosis," *Science*, Vol. 142 (1963), pp. 1436-1442.

24. J. Hirsch, "Behavior-Genetic, or 'Experimental,' Analysis: The Challenge of Science Versus the Lure of Technology," *American Psychologist*, Vol. 22, No. 2 (1967), pp. 118-130.

25. J. Hirsch (ed.), *Behavior-Genetic Analysis* (New York: McGraw-Hill, 1967).

26. J. Hirsch, "Behavior-Genetic Analysis and its Biosocial Consequences," *Seminars in Psychiatry*, Vol. 2, No. 1 (1970), pp. 89-105. Reprinted in whole or in part in:

a. *Information Retrieval Center on the Disadvantaged Bulletin* (of Teachers College, Columbia University, 1969), Vol. 4, pp. 3-4, 16-20.

b. Leonard W. Schmalz (ed.), *Experimental Psychology and Social Relevance: An Introductory Reader for the 70's* (New York: Harper & Row, 1971), pp. 299-312.

c. Robert Cancro (ed.), *Intelligence* (New York: Grune and Stratton, 1971), pp. 88-106.

d. S. Chess & A. Thomas (eds.), *Annual Progress in Child Psychiatry and Child Development*, 4th Vol. (New York: Brunner/Mazel, 1971), pp. 63-83.

e. Neil Chalmers, Roberta Crawley, and Steven P. R. Rose (eds.), *The Biological Bases of Behaviour* (London and New York: The Open University Press and Harper & Row, 1971), pp. 243-245.

f. J. McV. Hunt (ed.), *Human Intelligence* (New Brunswick, New Jersey: Transaction Books, 1972), pp. 7-29.

g. Thomas Weaver (ed.), *To See Ourselves: Anthropology and Modern Social Issues* (Glenview, Illinois: Scott, Foresman, 1972), pp. 209-218.

h. Kent S. Miller and Ralph M. Dreger (eds.), *Comparative Studies of Blacks and Whites in the United States* (New York: Seminar Press, 1972), pp. 33-50.

i. William R. Looff (ed.), *Readings in Developmental Psychology* (Hinsdale, Illinois: Dryden Press, 1972), pp. 99-117.

j. Philip G. Zimbardo and Christina Maslach (eds.), *Psychology for Our Times: Readings* (Glenview, Illinois: Scott, Foresman, 1973), pp. 36-47.

k. Clarence J. Karier (ed.), *Shaping the Educational State: 1900-1970* (New York: Free Press, 1975).

27. J. Hirsch, Introduction for *Forced Movements, Tropisms, and Animal Conduct* by J. Loeb (New York: Dover Publications, Inc., 1973).

28. J. Dewey, "Correspondence with Robert V. Daniels, February 15, 1947," *Journal of the History of Ideas*, Vol. 20 (1959), p. 570.

29. Hirsch, "Biosocial Consequences."

30. L. Eaves, Review of *Genetics and Education* by A. R. Jensen, *Heredity*, Vol. 30, No. 2 (1973), pp. 250-251.

31. T. Dobzhansky, *Genetic Diversity and Human Equality* (New York: Basic Books, Inc., 1973), p. 11.

Jensen-based discussion about the "correlation between performance in I.Q. tests and genuine [sic!] intelligence (Jensen 1969)." <sup>32</sup> (p. 406)

Recently Oxford's J. R. Baker relies on both Jensen's "original" and Eysenck's popularization in *the* treatise claimed by the prestigious Oxford University Press in the *New York Review of Books* (February 2, 1974) to be "Written with a thoroughness uncharacteristic in the usual treatment of this subject" and touted with the following rhetoric on the dust jacket: "... it was high time that someone wrote about race as Baker does, i.e. in the spirit of a one-man Royal Commission" by Sir Peter Medawar, nobel laureate, and "A most impressive display of profound scholarship and vast erudition . . . provides the essential basis upon which any objective, rational and scientific discussion of racial differences must proceed" by none other than Arthur R. Jensen! Therefore, it should come as no surprise that "Dr. Baker's conclusion . . . is that in certain racial groups there is a higher proportion of persons capable of developing high intelligence than in other groups." <sup>33</sup>

And finally we learn to be skeptical about the prevailing standards of quality control, scientific judgment and scholarly integrity when we find that Jensen is now deciding on the merits of articles submitted to the *American Journal of Human Genetics*. <sup>34</sup> It makes the Cattell shortcomings understandable (see below).

### III. THE NADIR OF SCURRILITY

On page 31, Jensen <sup>35</sup> presents as his Fig. 4 the graph and caption listed as my Fig. 1 and gives in the text immediately beneath it the following explanation: "... within only six generations of selection the offspring of the 'dull' strain make 100 percent more errors in learning the maze than do the offspring of the 'bright' strain (Thompson, 1954)." And the *only* reference appearing in his bibliography for this figure and the accompanying statement is to a 123- page 1954 publication by W. R. Thompson. Again, on page 456 of the summer issue in Jensen's fallacious riposte to Lee Cronbach's criticism, <sup>36</sup> which appeared in the spring issue, he alleges: "In fact, in the *study* which I cited as an example, and *from which my Figure 4 is taken*, rats were bred for learning ability that generalized across 24 different mazes. . . Fuller and Thompson (1960)." <sup>37</sup> (p. 456; italics added) What Jensen misrepresents to be "24 different mazes" is repeatedly described by his own source (Fuller and Thompson 1960, p. 212) in the singular: "... the Hebb-Williams *maze*. . . It consists of *a* square enclosure . . . *a* starting box . . . *a* goal box. . . Barriers . . . interposed in various ways between the starting box and the goal constitute the problems." <sup>38</sup> (Italics added)

Consultation of Thompson (1954), however, reveals another graph—his Fig. 44 (on page 217) <sup>39</sup> which appears as my Fig. 2. Furthermore, Thompson's article occupies only 23 not the 123 pages Jensen misrepresents it to be. In our exchange at Cambridge, England, I urged the audience to read Thompson for themselves in order

32. E. Mayr, *Populations, Species, and Evolution* (Cambridge: Harvard University Press, 1971), p. 406.

33. J. R. Baker, *Race* (New York: Oxford University Press, 1974).

34. *American Journal of Human Genetics*, Vol. 25 (1973), p. 692.

35. Jensen, HER.

36. L. J. Cronbach, "Heredity, Environment, and Educational Policy," *Harvard Educational Review*, Vol. 39, No. 2 (1969), pp. 338-347.

37. A. R. Jensen, "Reducing the Heredity-Environment Uncertainty: A Reply," *Harvard Educational Review*, Vol. 39, No. 3 (1969), pp. 449-483.

38. J. L. Fuller and W. R. Thompson, *Behavior Genetics* (New York: Wiley, 1960).

39. W. R. Thompson, "Genetics and Inheritance of Integrated Neurological and Psychiatric Patterns," *Proceedings of the Association for Research in Nervous and Mental Diseases*, Vol. XXXIII (Baltimore: Williams and Wilkins, 1954), pp. 209-231.

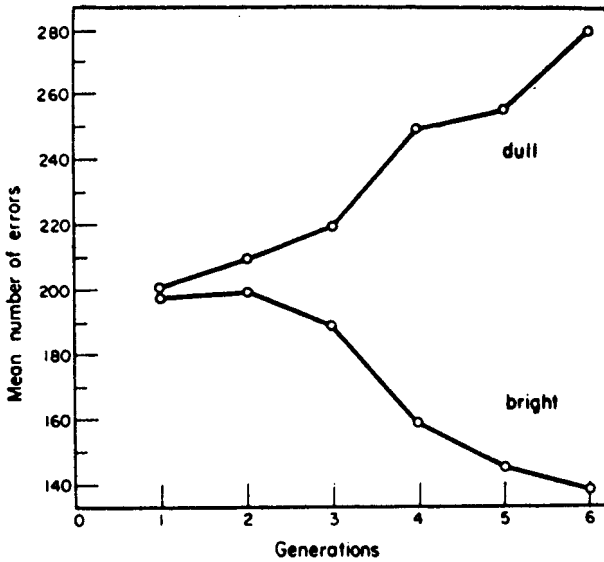
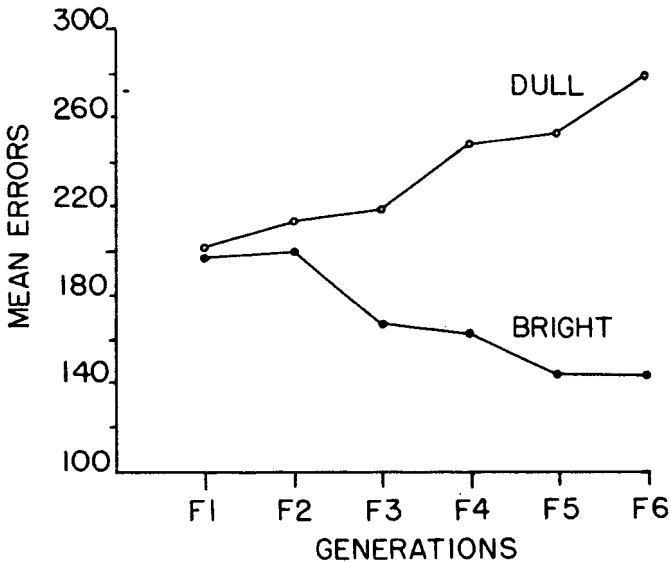


FIGURE 4.

*The mean error scores in maze learning for successive generations of selectively bred "bright" and "dull" strains of McGill rats. (After Thompson, 1954.)*

Fig. 1. Unchanged from A. R. Jensen, "How Much Can We Boost I.Q. And Scholastic Achievement?" *Harvard Educational Review*, Vol. 39 (1969), p. 31.



*Fig. 4. Mean error scores of 'bright' and 'dull' rats selectively bred on the Hebb-Williams maze over six filial generations.*

Fig. 2. Unchanged from W. R. Thompson, "Genetics and Inheritance of Integrated Neurological and Psychiatric Patterns," *Proceedings of the Association for Research in Nervous and Mental Diseases*, Vol. XXXIII (Baltimore: Williams and Wilkins, 1954), p. 217.



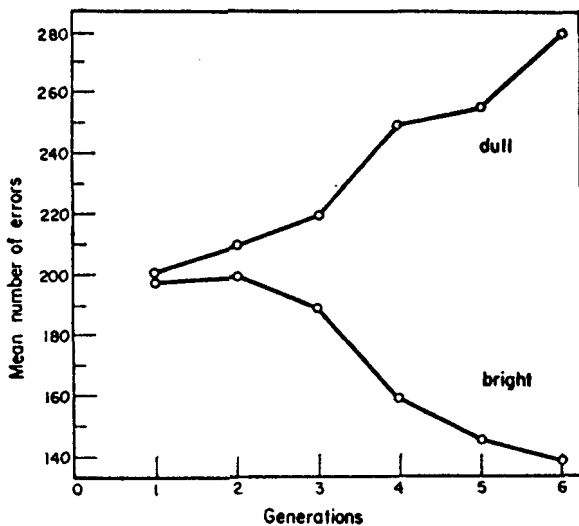


FIGURE 4

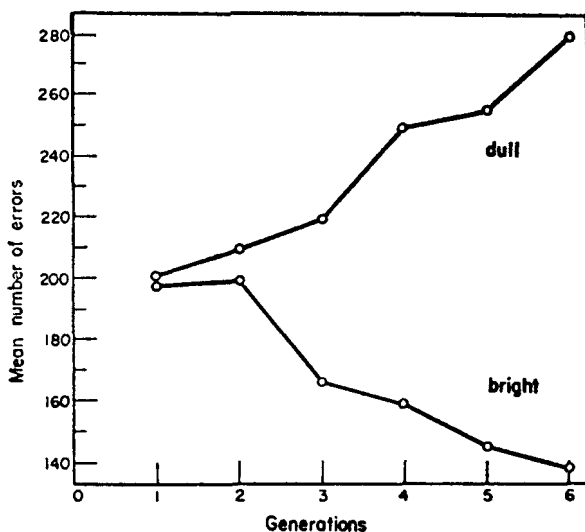


FIGURE 4'. *The mean error scores in maze learning for successive generations of selectively bred 'bright' and 'dull' strains of McGill rats. (After Thompson, 1954.)*

Fig. 3. Unchanged from A. R. Jensen, *Genetics and Education* (New York: Harper & Row, 1972), p. 102.

to appreciate Jensen's misrepresentation. There, Jensen expressed surprise. In the Harper & Row reprinting of his 1969 HER travesty, Jensen (1972) presents on page 102<sup>40</sup> the two figures and caption which appear as my Fig. 3, preceded on page 101 by the following statement in a footnote: "At a meeting of the Brain Research Association on July 17, 1970, in Cambridge, England, Professor Jerry Hirsch accused

40. A. R. Jensen, *Genetics and Education* (New York: Harper & Row, 1972).

me of having *faked* or altered the graph in Figure 4 to make it more strongly favor an hereditarian interpretation. The figure that appears here was directly reproduced (by photography, not re-drafting) from the source in which I found it (Robinson, R., *Genetics of the Norway Rat*. New York: Pergamon, 1965, page 537). In checking this figure against the *original* data, it turns out that one data point is in error. I have had the graph re-drafted with the necessary correction, as shown in figure 4."<sup>41</sup> (Italics added) I never said "faked" or argued "to make it more strongly favor an hereditarian interpretation." (The conference was tape recorded.) I have no way of distinguishing Jensen's fakery from his incompetence. Consultation of Robinson<sup>42</sup> (1965, p. 537), reliance on which he only belatedly confesses in 1972, corroborates it as the previously unacknowledged source of Jensen's figure, which in 1969 was deceptively attributed to Thompson 1954. It also reveals a new Jensen misrepresentation. Jensen's claim in 1972 to have consulted "the original data" is there revealed as misleading because Robinson states explicitly: "The average score for each generation was *presented graphically* in the report but without accompanying figures" (p. 537)—(= no data!). In addition, Thompson himself states: "Mean scores of bright and dull animals of each generation are presented *graphically* in figure 44." (Italics added)

We are informed by two sources (Jensen 1972;<sup>43</sup> *Times Literary Supplement*, Summer 1973<sup>44</sup>) that Jensen's "classic" paper is also reprinted in "*Congressional Record*, May 28, 1969, Vol. 115, No. 88, pp. H-4270-4298."<sup>45</sup> Reassure Jensen fans that they have little to fear: examination of the *Congressional Record* of the United States reveals that their hero has been able to cover his tracks and reduce his risk of perjury or contempt-of-Congress charges—neither the purloined Figure 4 nor the Thompson reference with the false pagination appears there! In place of the latter we find that on page H-4297 in the alphabetically ordered references between D. P. Moynihan and R. D. Tuddenham is a blank space into which has been inserted: "[Page 122 of manuscript was not included in printed copy submitted]", as shown in my Fig. 4, which is a photograph of the *Congressional Record*—he has omitted 30 references and all the figures from the 1969 HER travesty in the version provided to our Congress, the ultimate source of the funds that have financed the Jensenism disgrace.

It was introduced by John R. Rarick, the congressional representative of St. Francisville, Louisiana, "... because of the significant findings . . . [by Jensen] in the field of environment, heredity and behavior." (p. H-4270) The following year under the headline **INTEGRATION MAY HARM BLACKS** (p. H-6319, July 1, 1970),<sup>46</sup> Rarick again introduces Jensen testimony beginning page H-6324. More fully to appreciate Rarick's constituency and philosophy, note: "... Rarick is the most rabidly right-wing member of Congress . . . who some claim was once associated with the Ku Klux Klan, regularly inserts the most vitriolic kind of far-right, sometimes anti-Semitic, propaganda into the *Congressional Record*. Even his fellow Southern conservatives don't take him very seriously." (M. Barone, G. Ujifusa and D. Matthews, *The Almanac of American Politics*, (Boston: Gambit, 1974), p. 398; fortunately, Rarick has now been defeated in the 1974 primaries after four terms in Congress), consult *The Citizen* published by the white Citizens Councils of America, 254

41. *Ibid.*

42. R. Robinson, *Genetics of the Norway Rat* (New York: Pergamon, 1965), pp. 537 and 777.

43. Jensen, *Genetics and Education*.

44. "By the Colour of Their Genes?" *Times Literary Supplement* (London) (August 3, 1973).

45. *Congressional Record*, Vol. 115, No. 88 (May 28, 1969), pp. H-4270-4298.

46. *Ibid.*, Vol. 116, No. 110 (July 1, 1970), pp. H-6319-6326.

Moynihan, D. P. *Employment, income, and the ordeal of the Negro family*. In T. Parsons & K. B. Clark (Eds.), *The Negro American*. Cambridge, Mass.: Houghton-Mifflin, 1966. Pp. 134-159.

[Page 122 of manuscript was not included in printed copy submitted.]

Tuddenham, R. D. *Psychometricizing Piaget's méthode clinique*. Paper read at Amer. Educ. Res. Ass., Chicago, February, 1968.

Tyler, L. E. *The psychology of human differences*. (3rd ed.) New York: Appleton-Century-Crofts, 1965.

U.S. Commission on Civil Rights. *Racial isolation in the public schools*. Vol. 1. Washington, D.C.: U.S. Government Printing Office, 1967.

Fig. 4. Unchanged from *The Congressional Record-House*. Vol. 115, No. 88 (May 28, 1969), p. H4297.

E. Griffith Street, Jackson, Mississippi,<sup>47</sup> and read the accounts of the Citizens Councils and their goal "... to wage unremitting war in defense of segregation," in C. Vann Woodward's *The Strange Career of Jim Crow*,<sup>48</sup> and in Jack Peltason's *58 Lonely Men*.<sup>49</sup> As late as summer 1974 the situation had still not changed. It appears that all states except Louisiana have taken some action "for compliance with Title VI of the Civil Rights Act of 1964. ... Louisiana...refused...to file desegregation plans and has...been sued by the Justice Department for compliance..."<sup>50</sup>

Jensen also reproduces his two versions of Robinson's distorted figure in the June 1972 issue of *Educational Researcher*—house organ of American Educational Research Association, of which he was Vice President—alleging there that "the graph can be found also in Fuller's & Thompson's *Behavior Genetics*, p. 214."<sup>51</sup> His deception is again revealed when we actually do consult Fuller and Thompson, page 214 as shown in my Fig. 5.

In Thompson's original and in Fuller's and Thompson's figure, the maximum difference between graphed coordinates is shorter on the ordinate than on the abscissa, whereas in all the Jensen-Robinson so-called reproductions this relationship has been reversed: the maximum difference between graphed coordinates is shorter on the abscissa than on the ordinate. Also Thompson's original has only *11 steps* on the ordinate scale, each *20 units* apart, whereas in the Jensen-Robinson distortions the ordinate scale has been stretched and "improved" to show *17 steps*, each *10 units apart*. The point at issue is scholarly integrity. Certainly the scientific community has not been awaiting a Jensen to learn that selective breeding is possible.

Further examination of Robinson throws still more light on the nature of Jensen's scholarship—page 777 of Robinson's bibliography lists the Thompson 1954 reference as occupying pages "209-31." Whereas on page 122 of his own bibliography Jensen inflates it to appear as pages "209-331." (Robinson's paginations are of inconsistent style: the immediately preceding reference in Robinson appears as pages "323-7" and the succeeding one as pages "116-22.") Since Jensen has finally

47. *The Citizen*, March and April, 1971.

48. C. V. Woodward, *The Strange Career of Jim Crow* (New York: Oxford University Press, Second Revised Edition, 1966), p. 152.

49. J. W. Peltason, *58 Lonely Men* (Urbana: University of Illinois Press, 1971).

50. "Desegregation Proposals for State Colleges Announced—Fund Cutoff Averted," *The New York Times* (June 22, 1974).

51. A. R. Jensen, "Jensen on Hirsch on 'Jensenism'," *Educational Researcher*, Vol. 1, No. 6 (1972), pp. 15-16.

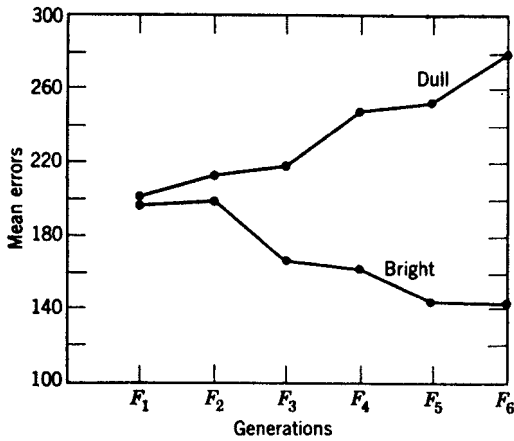


FIGURE 7-3. Mean error scores of "bright" and "dull" rats selectively bred for performance on the Hebb-Williams maze. (Thompson, 1954.)

Fig. 5. Unchanged from J. L. Fuller and W. R. Thompson, *Behavior Genetics* (New York: Wiley, 1960), p. 214.

TABLE 3

*Estimated Prevalence of Children With IQs Below 75, by Socioeconomic Status (SES) and Race Given as Percentages (Heber, 1968)*

SES	White	Negro
High 1	0.5	3.1
2	0.8	14.5
3	2.1	22.8
4	3.1	37.8
Low 5	7.8	42.9

Fig. 6. Unchanged from A. R. Jensen, "How Much Can We Boost I.Q. And Scholastic Achievement?" *Harvard Educational Review*, Vol. 39 (1969), p. 83.

admitted *not* consulting Thompson—his own reference—he had to "invent" the missing digit. He cannot claim to have made a copying error in transcribing it from either Thompson or Robinson.

Jensen's misrepresentations are legion: for the material presented in Table 3 (my Fig. 6) on page 83,<sup>52</sup> he refers readers to Heber 1968. This reference is wrong both on his pages 83, 91, 97 and in his References; first because actually the paper is authored by Heber and Dever,<sup>53</sup> second, and more importantly, the error does not turn out to be simply the inadvertent omission of the second author's name by Jensen or possibly even by Heber from a preprint, because the material in Table 3 does *not* come from the reference to which it is attributed, but rather it has been taken from an entirely different publication, bearing an entirely different title, and written not by Heber

52. Jensen, HER.

53. R. F. Heber and R. B. Dever, in H. C. Haywood (ed.), *Cultural Aspects of Mental Retardation* (New York: Appleton-Century-Crofts, 1970).

alone, but by three authors, Heber, Dever and Conry.<sup>54</sup> Furthermore, inexcusably the material in Table 3 purports to represent comparisons between intelligence scores of different genotypes (races) under the "same" environmental conditions, i.e., Negro-white comparisons with respect to prevalence of children with I.Q.'s below 75 at each of five socioeconomic statuses. This is a disgraceful misrepresentation, because, on tracing the material back to its original sources, as can be done through the reference provided here but not through that to which Jensen attributed it, I find that the data for whites came from the 1937 heterogeneous Terman-Merrill<sup>55</sup> sample drawn from 11 scattered states—California, Colorado, Indiana, Kansas, Kentucky, Minnesota, Nevada, New York, Texas, Vermont and Virginia—whereas the black was what in 1963 Kennedy, Van De Riet and White<sup>56</sup> called "a large, homogeneous sample" drawn exclusively from 5 clustered southeastern states—Alabama, Florida, Georgia, South Carolina and Tennessee. Note also that the original two publications, from which these data have been drawn, are separated by 26 years—no spatio-temporal comparability whatsoever!

Jensen also misrepresents the Heber et al.<sup>57</sup> treatment of the typical falling I.Q. curve with increasing chronological age among disadvantaged children of mothers having I.Q. less than 80 (Jensen's Fig. 10, page 63; their Fig. 7, page 9) by alleging that "They studied the families of 88 low economic class Negro mothers . . ." (page 62) and that they used "A representative sample of 88 mothers . . ." (page 91; italics added in both quotations), when the original authors explicitly state on their page 7: "We have studied the families of 88 low economic class mothers residing in a set of contiguous slum census tracts in the city of Milwaukee. . . . For our survey, we selected 88 consecutive births. . . ." (Italics added) Nothing in the original report identifies the race of the families, and the authors explicitly label their sample as "selected," not representative. Professor Dever informs me (private communication) that "in fact, the data were gathered from both white and black mothers!"

Professor Dever objects strongly to Jensen's misuse of the two papers carrying his name: "Neither paper can be held up as being supportive of a genetic hypothesis for retardation, although we certainly had to face that question. . . . Jensen (page 92) quotes our statement: 'The conclusion that changes in the social environment can cause very large increments in I.Q. for the cultural-familial retardate is not warranted . . .' (on the basis of the studies we reviewed.) If Jensen had to hold us up as authorities in this area, the least he could have done would have been to make it known that we had also reached the conclusion that the genetic viewpoint was equally untenable on the basis of the data which we reviewed (much of which was also reviewed by Jensen),"<sup>58</sup> and Professor Heber stated that his findings were "grossly misinterpreted" by Jensen.<sup>59</sup>

On page 87 Jensen sinks to citing as an "authority" and then distorting the

54. R. F. Heber, R. B. Dever and J. Conry, in H. J. Prehm, L. A. Hammerlynck, and J. E. Crosson (eds.), *Behavioral Research in Mental Retardation* (Eugene, Oregon: Rehabilitation Research and Training Center in Mental Retardation Monograph No. 1, 1968), pp. 1-22.

55. L. M. Terman and M. A. Merrill, *Measuring Intelligence: A Guide to the Administration of the New Revised Stanford-Binet Tests of Intelligence* (Boston: Houghton Mifflin, 1937).

56. W. A. Kennedy, V. Van De Riet, and J. C. White, Jr., "A Normative Sample of Intelligence and Achievement of Negro Elementary School Children in the Southeastern United States," *Society for Research in Child Development*, Monograph 28 (1963), p. 110.

57. Heber, et al., *op. cit.*

58. R. B. Dever. For a ten-page documented exposition of Jensen misrepresentations, contact R. B. Dever, Special Education, Indiana University, Bloomington, Indiana 47401.

59. D. Patrinos, "Retardation: Poverty's Legacy—III: Article Has Research Roots Here," *Milwaukee Sentinel* (Wednesday, June 11, 1969), p. 1.

## HOW REGIONS AND RACES FARE IN DRAFTEES' MENTAL TESTS

Year ended June 30, 1966	Per cent failing Armed Forces Qualification Test		
	WHITE DRAFTEES	NEGRO DRAFTEES	ALL DRAFTEES
<b>SOUTH</b> Ala., Fla., Ga., Miss., N. C., S. C., Tenn.	18%	68%	31%
<b>SOUTH CENTRAL</b> Ark., La., N. M., Okla., Tex.	12%	57%	20%
<b>NORTHEAST</b> Conn., Me., Mass., N. H., N. J., N. Y., R. I., Vt., Del., D. C., Ky., Md., Ohio, Pa., Va., W. Va.	12%	45%	15%
<b>MIDWEST AND WEST</b> Colo., Ill., Ind., Ia., Kans., Mich. Minn., Nebr., N. D., S. D., Wisc., Wyo., Ariz., Calif., Ida., Mont., Nev., Oreg., Utah, Wash.	8%	37%	10%

Note: Data are for Army Areas; Alaska and Hawaii are not included.

Source: Office of the Surgeon General, U. S. Army

Fig. 7. Unchanged from "Mental Tests for 10 Million Americans—What They Show," *U.S. News and World Report* (October 17, 1966), p. 78.

message of a then 3-year old mass-media magazine article<sup>60</sup> by lifting out of context from the first column of its six-column discussion the "fact" that "The largest sampling of Negro and white intelligence test scores resulted from the administration of the Armed Forces Qualification Test (AFQT) to a national sample of over 10 million men between the ages of 18 and 26. As of 1966 the overall failure rate for Negroes was 68 percent as compared with 19 percent for whites" (*U. S. News and World Report*, 1966)—even though immediately adjacent to the *single sentence* on which he based the foregoing discussion is the table appearing as my Fig. 7. Furthermore, about 20 lines above that sentence is an accurate reference to the original study on which the article is based and in which we find the explicit statement "Failure rates clearly and consistently relate to geographical areas. Year after year, men from the West and the Midwest perform better than those from other parts of the country. In the special study of 18-year-olds their failure rate was only half the national average, while men from the South were failing at twice the national rate."<sup>61</sup>

R. B. Cattell departed England for America (where my University of Illinois squandered a career-long research professorship on him) after he was exposed in 1937 by the incomparable J. B. S. Haldane as a propagandist purveying misinformation.<sup>62</sup> Over Cattell's subsequent recantation the editors published their disclaimer stating "the author alone is responsible."<sup>63</sup> His "errors" appeared in a paper bearing the notoriously alarmist title: "Is National Intelligence Declining?"<sup>64</sup>—which often repeated alarm he continues to proclaim with unshakable faith even though no evidence has been forthcoming in almost two human generations.<sup>64a</sup> In Haldane's

60. "Mental Tests for 10 Million Americans—What They Show," *U.S. News and World Report* (October 17, 1966), pp. 78-80.

61. R. DeNeufville and C. Conner, "How Good Are Our Schools?" *American Education*, Vol. 2 (1966), p. 5.

62. J. B. S. Haldane, "Correspondence," *Eugenics Review*, Vol. XXVIII, (1936-37), p. 333.

63. *Ibid.*, p. 334.

64. R. B. Cattell, "Is National Intelligence Declining?" *Eugenics Review*, Vol. XXVIII (1936-37), pp. 181-203.

64a. R. B. Catell, "Differential Fertility and Normal Selection for IQ: Some Required conditions in their investigation," *Social Biology*, 1974, 21, pp. 168-177.

words: "Dr. Cattell writes that his statement as to the inheritance of feeble-mindedness 'was obviously intended as one of a number of general introductory approaches to the subject of intelligence inheritance for the non-technical reader,' and then proceeds to chide those who 'look for precision regarding a concept which is by its nature incapable of being defined precisely.' In fact he draws a sharp distinction between scientific and propagandist statements and evidently classes his own with the latter."<sup>65</sup>

Of course, the leopard did not change his spots in the new environment. Thirty-odd years later it was necessary for Illinois colleague H. W. Norton to warn: "... the statistical analyses reported by Cattell et al. are incorrect and the 'results' they report should be ignored."<sup>66</sup> Once again, like disgraced Vice President Spiro Agnew, Cattell pleaded *nolo contendere*—this time he had thrown away his data!<sup>67</sup>

Now in a new generation we find Cattell's ultra-modern counterpart posing the same eugenic question beginning on page 93 in the section entitled "Is Our National I.Q. Declining?" and concluding on page 95: "Is there a danger that current welfare policies, unaided by eugenic foresight could lead to the genetic enslavement of a substantial segment of our population? The possible consequences of our failure seriously to study these questions may well be viewed by future generations as our society's greatest injustice to Negro Americans"—a philosophic credo important enough to Jensen for him to republish as a separate statement three years later.<sup>68</sup>

The "scientific" justification for this reiterated Hitler-type recommendation (treating an inferior race with "eugenic foresight") includes the following: misinforming readers that: "A number of studies *have shown* that in populations practicing a high degree of assortative mating, . . . (Bajema, 1963, 1966 . . .). Since assortative mating . . . in the long run may have a eugenic effect . . ." (pp. 36-37)<sup>69</sup> and going on to claim "... average generation time . . . is significantly less in the Negro than in the white population. Also *as noted in Bajema* (1966), generation length is inversely related to educational attainment and occupational status; therefore a group with shorter generation length is more likely subject to a possible dysgenic effect." (p. 95) Consultation of Jensen's Bajema references, however, will do nothing to restore our confidence in Jensen's integrity as scientist-scholar, to wit: "The degree of positive assortative mating in relation to intelligence was *not determined* for the population under study" (Bajema 1963, p. 183)<sup>70</sup> and Bajema (1966) was a further study of the same population. There we find the following contradiction: "There is a *positive* relationship between educational attainment and generation length (Table 5)," <sup>71</sup> and "The *negative* relationship between educational attainment and generation length for both sexes in this study. . . ." Bajema's first statement about "positive relationship" incorrectly refers to Table 5 when the supporting data are actually in his Table 6. The contradictory statement about "negative relationship" appears on the very next page in Bajema's *summary*, which could be all that Jensen consulted. Note also that

65. J. B. S. Haldane, "Correspondence," *Eugenics Review*, Vol. XXIX (1937-38), p. 81.

66. H. W. Norton, "Letters to the Editor," *American Journal of Human Genetics*, Vol. 23 (1971), p. 225.

67. R. B. Cattell, and J. D. Hundleby, "Letters to the Editor," *American Journal of Human Genetics*, Vol. 24 (1972), p. 485.

68. A. R. Jensen, "Jensen on Friedrichs' Survey," *Phi Delta Kappan*, Vol. LIII, No. 7 (1972), p. 462.

69. Jensen, HER.

70. C. J. Bajema, "Estimation of the Direction and Intensity of Natural Selection in Relation to Human Intelligence By Means of the Intrinsic Rate of Natural Increase," *Eugenics Quarterly*, Vol. 10 (1963), pp. 175-187.

71. C. J. Bajema, "Relation of Fertility to Educational Attainment in a Kalamazoo Public School Population: A Follow-Up Study," *Eugenics Quarterly*, Vol. 13 (1966), pp. 306-315.

Jensen's statement about a relationship to occupational status is another misrepresentation: Bajema (1966) does not even consider occupational status. (Italics added throughout my use of the Bajema material.)

In other words, Bajema published two mutually contradictory statements on two consecutive pages, one of them "justified" by reference to the wrong data table in his own paper, and Jensen adopts the one that supports the white supremacist philosophy. In the second of his two reviews of the same (= unchanged, even though republished with "corrections") material, is this on what encomiast Vernon based his carefully worded reassurances: "Although he [Jensen] regards it as important to study race differences, he is certainly not a 'racist' and he at no time preached any kind of discrimination against 'inferior' races."<sup>72?</sup>

Jensen's report of one nutritional supplement study provides a shameful example of scholarly incompetence: ". . . there is at least one study which shows that some undetermined proportion of the urban population in the United States might benefit substantially with respect to intellectual development by improved nutrition. *In New York City*, women of low socioeconomic status were given vitamin and mineral supplements during pregnancy. These women gave birth to children who, at four years of age, averaged 8 points higher in IQ than a control group of children whose mothers had been given placebos during pregnancy (Harrell, Woodyard, & Gates, 1955)." <sup>73</sup>(p. 74) The information actually in Jensen's reference is almost unrelated to his discussion: ". . . gathering . . . data *in* . . . two maternity clinics . . . about 800 miles apart. One was the Frontier Nursing Service centered at Wendover, Leslie County, *Kentucky*, and the other was the King's Daughters Maternity Clinic of Norfolk, *Virginia*."<sup>74</sup> (Italics added)

Not only is the geographic location entirely *unrelated* to what he *misrepresents* it to be, but the sample composition is complicated:

The people of this Cumberland mountain district are almost entirely descended from the early migrants to America and are of English, Welsh, Scotch, and Irish ancestry. There has been little infiltration of Southern European, Asiatic, African, Mexican, Indian or Oriental blood. Probably nowhere in the United States is there a more homogeneous group nor one of more rigid, unbending folkways.<sup>75</sup> (p. 15)

Racially, these [Norfolk, Virginia] maternity patients were approximately 80 per cent Negro, the remainder being white patients of varied and mixed nationality. Among their number were Mexicans, Filipinos, Germans, Laskars, Italians, Greeks, Hebrews, and those of French, Irish, and English ancestry.<sup>76</sup> (p. 17)

Also the samples are subdivided according to kind of diet supplement and separate intelligence scores are presented for different groups. Furthermore some children were tested at age 3, some at age 4, and some at both 3 and 4 years. Since the I.Q. results vary with the conditions, no single average score is meaningful.

Unfortunately even my previous example fails to plumb the nadir of incompetence exhibited by both Jensen *and* his discussants, because on his page 58 we are told: ". . . I received a personal communication from Professor Lloyd G. Humphreys who

72. Vernon, "Review of *Genetics and Education*."

73. Jensen, HER.

74. R. F. Harrell, E. Woodyard, and A. I. Gates, *The Effect of Mothers' Diets on the Intelligence of Offspring* (New York: Bureau of Publications, Teachers College, Columbia University, 1955), pp. 13-14.

75. *Ibid.*

76. *Ibid.*



pointed out some arguments that indicate I may have under-estimated the heritability of scholastic achievement . . . [Here I have omitted approximately 150 words.] Rank in high school graduating class, which is an overall index of scholastic performance and is little affected by age yields heritability coefficients below 0.40 in a nationwide sample (Nichols and Bilbro, 1966)."

That 1966 paper by Nichols and Bilbro,<sup>77</sup> as its title in Jensen's bibliography blatantly proclaims, concerns "The Diagnosis of Twin Zygosity" and is completely unrelated to the topic for which it is adduced as reference!

Jensen refers readers to another of his publications in an unsuccessful attempt to justify his misuse of heritability, <sup>78</sup> <sup>79</sup> because the question "is too complex to be considered here." (p. 109) That other discussion reveals a very old misconception shared by Jensen with too many of his sympathizers. There he claims: "The inventors and developers of intelligence tests—men such as . . . Binet . . . —clearly intended that their tests assess . . . the individual's innate brightness or mental capacity."<sup>80</sup> But Binet's ideas were, in fact, quite different from what Jensen misrepresents them to be. As Healy pointed out back in 1915 and as has since repeatedly been recognized, e.g., by Haller (1963, p. 112),<sup>81</sup> Osborn (1966, Vol. 8, p. 816),<sup>82</sup> Tuddenham (1962, p. 487),<sup>83</sup> and so on: "Those who think this scale measures general ability apart from schooling and other advantages should read Binet himself on the subject."<sup>84</sup> And here is what Binet actually says: ". . . l'intelligence de quelqu' un est susceptible de développement; avec de l'exercice et de l'entraînement, et surtout de la méthode, on arrive à augmenter son attention, sa mémoire, son jugement, et à devenir littéralement plus intelligent qu' on ne l'était auparavant . . . ce qui importe pour se conduire de manière intelligente, ce n'est pas tant la force des facultés que la manière dont on s'en sert, c'est-à-dire l'art de l'intelligence, et que cet art doit nécessairement s'affiner avec l'exercice."<sup>85</sup> <sup>86</sup>

On page 74 Jensen reports Rimland's discussion of birth-order effects: "Order of birth contributes a significant proportion of the variance in mental ability. On the average, first-born children are superior in almost every way, mentally and physically. . . . (Rimland [1964, pp. 140–143] has put forth some interesting hypotheses to explain the *superiority of the first-born.*)" Once again, consulting Jensen's reference directly will do nothing to restore anyone's confidence in Jensen's integrity or competence as scientist-scholar, because on page 140, Rimland (1964) states: "The readiness with which psychogenic explanations may be generated to explain the successes and

77. R. C. Nichols and W. C. Bilbro, "The Diagnosis of Twin Zygosity," *Acta Genetica et Statistica Medica*, Vol. 16 (1966), pp. 265-275.

78. Hirsch, "Biosocial Consequences."

79. P. A. P. Moran, "A Note on Heritability and the Correlation Between Relatives," *Annals of Human Genetics*, Vol. 37 (1973), p. 217.

80. A. R. Jensen, "Another Look at Culture-Fair Testing," in T. A. Shellhammer, chairman, *Measurement for Educational Planning* (Princeton: The Seventeenth Annual Western Regional Conference on Testing Problems, Educational Testing Service, 1968), p. 94.

81. M. H. Haller, *Eugenics: Hereditarian Attitudes in American Thought* (New Brunswick: Rutgers University Press, 1963), p. 112.

82. F. H. Osborn, *Encyclopedia Britannica* (Chicago: Wm. Benton, Vol. 8, 1966), p. 816.

83. R. D. Tuddenham, in L. Postman (ed.), *Psychology in the Making* (New York: Knopf, 1962), p. 487.

84. W. Healy, *The Individual Delinquent* (Boston: Little, Brown, 1915), p. 80.

85. A. Binet, *Les Idées Modernes sur les Enfants* (Paris: Flammarion, 1909), p. 143.

86. ". . . the intelligence of anyone is capable of development; with exercise, with training and especially methodically, one can succeed in improving one's attention, memory, judgment, and in becoming literally more intelligent than before . . . what counts in intelligent behavior is not so much the strength of the abilities as the way in which they are used, that is to say the art of intelligence, and that art must necessarily improve with practice." (My translation.)

failures of the first-born . . ." and again on page 141, Rimland states: "The bulk of research on birth order and intelligence . . . reports *no consistent differences in the mean IQ of children in sibling rank* (Schoonover, 1959). Yet the findings are incontrovertible that the first-born are highly overrepresented at *both* tips of the distribution, as we noted in Chapter 7."<sup>87</sup> (Italics added throughout discussion of Rimland material.)

Following his discussion of environmental deprivation and immediately after describing the "extreme case of Isabel . . . who was confined . . . in an attic up to the age of six by a deaf-mute mother, and who had an IQ of about 30 . . ." but whose "IQ became normal by age 8" after being "put into a good environment," Jensen (p. 61) again cites animal work: "These observations are consistent with studies of the effects of extreme sensory deprivation on primates. Monkeys raised from birth under conditions of total social isolation for example, show no indication when compared with normally raised controls, of any impairment of ability for complex discrimination learning, delayed response learning, or learning set formation, although isolated monkeys show severe social impairment in their relationships to normally reared monkeys (Harlow & Griffin, 1965)." Once again we are misled by a Jensen distortion, because in the very paragraph Jensen is abstracting, Harlow and Griffin carefully warn their readers: "It should be noted that these isolated monkeys had some learning experience while under total social deprivation (Rowland, 1964)."<sup>88</sup>

#### IV. A BRITISH ECHO

In the Preface to the 1972 reprinting of HER, once again Jensen exposes his own shoddy standards by recommending "for students and nonprofessionals who lack the technical background in statistics, measurement theory and quantitative genetics,"<sup>89</sup> required for reading Jensen, that they read H. J. Eysenck's (1971) *Race, Intelligence and Education: The I.Q. Argument*—a suggestion endorsed by Cattell (1971): "A book that no open-minded person interested in the issues of our times should fail to read."<sup>90</sup>

I have followed the "authoritative" recommendations of Professors Jensen and Cattell and here is what I have found on reading Eysenck:<sup>91</sup> ". . . the reader will have to rely on the general watchfulness of my colleagues to make sure that I have not tried to *slip anything over on him*" (p. 15; italics added); and: "In preparing this book for publication I was very much aided by Professor A. R. Jensen. . . . Thanks are due to Professor I. I. Gottesman . . . for taking part in an 'At Home' with Jensen and other experts . . ." (p. 16)

In the opening lines of the fine paper on "Race and Intelligence," quoted earlier, Lewontin likens today's Jensenism to the heresy of Jansenism: "In the Spring of 1653 Pope Innocent X condemned a pernicious heresy which espoused the doctrines of 'total depravity, irresistible grace, lack of free will, predestination and limited atonement.' That heresy was Jansenism and its author was Cornelius Jansen, Bishop of Ypres. In the winter of 1968<sup>92</sup> the same doctrine appeared in the 'Harvard Educa-

87. B. Rimland, *Infantile Autism* (New York: Appleton-Century-Crofts, 1964), pp. 140 and 141.

88. H. F. Harlow and G. Griffin, "Induced Mental and Social Deficits in Rhesus Monkeys," in S. F. Osler and R. E. Cooke (eds.), *The Biosocial Basis of Mental Retardation* (Baltimore: The Johns Hopkins Press, 1965), pp. 87-106.

89. Jensen, *Genetics and Education*.

90. R. B. Cattell, Advertisement for *The I.Q. Argument* (American title of Fn. 91) by H. J. Eysenck, *The New York Review of Books* (October 21, 1971), p. 39.

91. H. J. Eysenck, *Race, Intelligence and Education* (London: Temple Smith, 1971).

92. Actually February, 1969, but 1968 is in Lewontin's text.

tional Review.' That doctrine is now called 'jensenism' by the 'New York Times Magazine' and its author is Arthur R. Jensen, professor of educational psychology at the University of California at Berkeley."<sup>93</sup>

In the opening lines of his first chapter, entitled "The Jensenist Heresy," Eysenck tells the same Jensenism-Jansenism story as Lewontin: "In 1653 Pope Innocent X condemned the heresy of Jansenism, proposed by Cornelius Jansen, Bishop of Ypres, as espousing doctrines of 'total depravity, irresistible grace, lack of free will, predestination and limited atonement.' More recently social scientists, educationalists and others have condemned a novel heresy, called Jensenism by the 'New York Times Magazine,' after Arthur Jensen, Professor of Educational Psychology at the University of California, Berkeley, and author of a widely discussed review of methods of boosting intelligence which appeared in the Harvard Educational Review in 1969"<sup>94</sup>—without ever acknowledging Lewontin's paper as the source.

Four pages later Eysenck introduces a long quotation from Jensen by the statement: "This is what he has to say, in an article published in March 1970, in the *Bulletin of the Atomic Scientists*." (p. 21)

Eysenck goes to almost diabolical extremes to omit Lewontin's name and to exclude Lewontin's valuable paper from his "Acknowledgements" and recommended "Further reading." The reader who actually consults Eysenck's reference in the March 1970 *Bulletin of the Atomic Scientists*, however, will be surprised. He will not find there any of the many lines and paragraphs (five pages of them) quoted from Jensen or any words at all written by Jensen, because that issue, the only one cited directly by Eysenck, contains the valuable Lewontin paper Eysenck has tried so hard to conceal. The long Jensen quotation comes from Jensen's response to Lewontin in the May, not the March, issue and carries in its title the words, also expunged by Eysenck: "A Reply to Lewontin."<sup>95</sup> Furthermore, the Jensen material appears deceptively to be one long continuous, i.e., *uninterrupted*, quotation. The deception is revealed when one compares the second paragraph with Jensen's original, as shown in my Fig. 8.

In Eysenck's quotation from Jensen, the second paragraph reads: "These assumptions, theories and practices . . ." and begins without any ellipsis points to indicate that words have been deleted or suppressed: whereas in the original the word "these" is not capitalized because it is the sixth, not the first, word of the opening sentence, which in fact reads: "I agree with Lewontin that these assumptions, theories and practices . . ." And it is this text that Jensen now (p. 59) calls ". . . an admirably lucid and readable discussion . . . suited for students and nonprofessionals who lack the technical background. . . . For being accurate while avoiding the technical, Eysenck's book is in the best tradition. . . ."<sup>96</sup> Certainly, it is an old tradition.

#### V. IS JENSEN HONEST?

Having examined the quality of the scholarship of Jensen and his allies, it becomes of interest to compare what we have seen so far with the criteria Jensen applies to others (enemies?). The remarks quoted earlier from Moynihan were prompted by an exchange between Jensen and Elizabeth Alfert, wherein he charges: "Alfert's article begins with a *falsehood*. It is a *fatuous falsehood* which I conclusively

93. Lewontin, *op. cit.*

94. Eysenck, *Race*.

95. A. R. Jensen, "I. Race and the Genetics of Intelligence: A Reply to Lewontin," *Bulletin of the Atomic Scientists*, Vol. 25, (May 1970), pp. 17-23.

96. Jensen, *Genetics and Education*.

now questioning the assumptions, theories and practices on which they were based.

These assumptions, theories and practices — espoused over the past decade by the majority of educators, social and behavioural scientists — are bankrupt. I do not blame the children who fail to

now questioning the assumptions, theories and practices on which they were based.

I agree with Lewontin that these assumptions, theories and practices— espoused over the past decade by the majority of educators, social and behavioral scientists—are bankrupt. I

Fig. 8. First quotation unchanged from H. J. Eysenck, *Race, Intelligence and Education* (London: Temple Smith, 1971), p. 22. Second quotation unchanged from A. R. Jensen, "Race and the Genetics of Intelligence: A Reply to Lewontin," *Bulletin of the Atomic Scientists* V. 25, No. 5 (1970), p. 18.

refuted many months ago." "Assuming that Alfert has read the references cited in her own article, she must have known that this statement was untrue before she wrote the article for this journal . . . Yet she continues to perpetuate a falsehood."<sup>97</sup> (p. 212) He later continued: "Dr. Alfert is grasping at straws. Her careless research methods are displayed first in the fact that even after I had refuted her claim. . . she still did not take the trouble to seek out the truth on this matter."<sup>98</sup> (p. 219)

It is unmistakably clear that Jensen believes writers will deliberately *misrepresent* in order to hoodwink their readers. Therefore, let us now examine one of the issues in the Jensen-Alfert dispute, because, in the very publication abusing Alfert with "fatuous falsehood," etc., Jensen provides evidence which can now be evaluated in the light of further unambiguous evidence Jensen himself provides on that same issue three years later in the 67-page self-serving Preface to the 1972 reprinting of HER. And let us apply to Jensen the very criteria Jensen so readily applies to others. Exhibit 1: Jensen's Rejoinder to Alfert: ". . . *U. S. News and World Report* interviewed me . . . after learning about the [HER] article . . . they requested a prepublication copy which I . . . provided." (p. 214) Exhibit 2: Jensen in 1972 book: ". . . I was visited . . . by a . . . writer from the *U. S. News and World Report*. . . He knew nothing of the HER article . . . I told my visitor about the HER article."<sup>99</sup> (p. 13) Exhibit 3: Alfert's original Comment: ". . . Jensen released the text of his article to *U. S. News and World Report* . . . before [it] . . . was to be published."<sup>100</sup> (p. 207)

The last is a quotation from a statement by HER editors appearing in the Alfert article, against which Jensen levels the charges of "fatuous falsehood," etc.

Do Jensen's own published words—Exhibit 1—give the lie to Jensen's own published words—Exhibit 2? When he is impugning Alfert, the magazine (I give more context here) "interviewed me on the topic after learning about the article." (p. 214) Then, three years later (again, I give more context) he reveals on

97. A. R. Jensen, "Rejoinder: The Promotion of Dogmatism," *Journal of Social Issues*, Vol. XXV, No. 4 (1969), pp. 212-217.

98. *Ibid.*, pp. 219-222.

99. Jensen, *Genetics and Education*.

100. E. Alfert, "Comment on: The Promotion of Prejudice," *Journal of Social Issues*, Vol. XXV, No. 4 (1969), pp. 206-211. "Response to Jensen's Rejoinder," pp. 217-219.

page 13 that their "staff writer . . . knew nothing of the HER article, *but* was on the Berkeley campus to interview . . . faculty concerning a story he was preparing on 'campus unrest' . . . In the course of the interview, I told my visitor about the HER article and I gave him a Xerox copy of my typescript." (Italics added for emphasis in all of the above, except for magazine title.)

Since the published text occupies 123 printed pages, how many hundreds of typescript pages had to be Xeroxed for the advance publicity and propaganda purposes? It can be asked of Jensen and his fans: which one of the two contradictory accounts, by self-serving Jensen of the same event, is his "fatuous falsehood" and is now to be considered (in appropriate Watergate jargon) "inoperative"?

## VI. BIOLOGICAL MISINFORMATION

The fundamental fallacy, on which rests the superstructure of the racist renaissance spearheaded by Jensen, is exposed in the question asked by the HER title and the false answer given to it on his page 59: How much can we boost traits A & B (= I.Q. and Scholastic Achievement)? His erroneous answer on page 59 takes the form: The fact that heritability  $B < A$  means teachability  $A < B$ . It is based on the fallacious assumption that teachability is the complement of, or varies inversely with, heritability—a fallacy echoed by Jensen disciple Scarr-Salapatek: "Heritability estimates can have merit as indicators of the effects to be expected from various types of [environmental] intervention programs."<sup>101</sup> (p. 1227) When, in fact, there is no relationship whatsoever between teachability and heritability; and furthermore as Moran has now elegantly shown: ". . . for characteristics such as human intelligence in which the genetic and environmental components are correlated, 'heritability' cannot be defined. . . ."<sup>102</sup> So, it is not surprising that she ignores Moran in her latest panegyric: "With the notable exception of Arthur Jensen, not many advocates of high or low heritability are adding to our store of knowledge about human intelligence."<sup>103</sup>

There are several reasons why I have called human heritability estimates "both deceptive and trivial." The conceptual problems have been analyzed in much greater detail elsewhere<sup>104</sup> and will only be summarized here: (1) the norm of reaction, (2) the purpose and use for which heritability estimates are appropriate, and (3) the inconsistency of scaled polyalleles. First, norm (or range) of reaction describes the fact that the same genotype can develop into quite different phenotypes depending on the environment in which it develops (e.g., grow a plant with and without fertilizer, at sea level or above the timberline, etc.) and heritability, an average statistic and *population* measure, provides no information about how a given *individual* might have developed under conditions different from those under which he (she, or it) actually did develop. Jensen denies this in a new footnote: ". . . there has been no evidence that different genotypes for I.Q. are affected differentially by the environment"<sup>105</sup> (p. 141) —a perfectly asinine statement in the light of Jensen's own previous claim about two genotypes (normal and PKU) in one environment (phenylalanine in the diet) or one genotype in two environments (PKU genotype raised with presence or absence of phenylalanine in diet): ". . . a child who has inherited . . . PKU can grow up normally

101. S. Scarr-Salapatek, *op. cit.*

102. P. A. P. Moran, *op. cit.*

103. S. Scarr-Salapatek, "Some Myths About Heritability and IQ," *Nature*, Vol. 251 (1974), pp. 463-464.

104. Hirsch, "Biosocial Consequences."

105. Jensen, *Genetics and Education*, p. 141.

if his diet is controlled to eliminate . . . phenylalanine."<sup>106, 107</sup> (p. 45, p. 120 respectively) But then Jensen is not at home in biology when more is involved than the symbol manipulating game of Mendelian algebra and biometrics, which has a form similar to psychometrics.

He stumbles repeatedly in biology: when he tries to explain the cytological picture in Turner's syndrome, females lacking one of the two X chromosomes, he pontificates: "When their chromosomes are stained and viewed under the microscope, it is seen that the sex-chromatin is missing *from* one of the two chromosomes that determine . . . sex."<sup>108, 109</sup> (p. 32, p. 103 respectively; italics added) Sex-chromatin (the Barr body) and chromosomes are *not* seen together. Chromosomes are seen during the metaphase of mitosis after the nucleus has disintegrated (literally broken down into its chromosome components). Sex-chromatin is seen "at the periphery of the interphase nucleus just inside the nuclear envelope"<sup>110</sup> in cells containing two or more X chromosomes (usually female, one exception being Klinefelter's syndrome—not Klinefelter as given by our "authority,"<sup>111</sup> pp. 204 and 376, both italics added).

Second, heritability measure was developed for a purpose and use unrelated to and inappropriate for education and teaching. "An estimation of  $h^2$  [= heritability] is valuable for planning for selection [= breeding] in the particular population in which it was made."<sup>112</sup> While Jensen's ignorance is unfortunate, his avowed goals are as heinously barbaric as were Hitler's and the anti-abolitionists'. His warning and alarmist language are disgusting enough to warrant repetition: "Is there a danger that current welfare policies, unaided by *eugenic foresight*, could lead to the genetic enslavement of a substantial segment of our population? The possible consequences . . . may well be viewed by future generations as *our* society's greatest *injustice* to Negro Americans." (p. 95; italics added) Note that throughout his writings *we* are whites (not Caucasians) and *they* are Negroes (not blacks). And *we* should treat (= breed!) *them* with eugenic foresight. After all, agricultural eugenics, for which heritability measure was developed, has worked so well *with* plants and animals *for* farm entrepreneurs, why not use it with, or for (which?), man and build the Brave New World by 1984? Galton's dream was to see "man breeding man as his own domesticated animal,"<sup>113</sup> which our slavocracy did profitably enough to fight a civil war to perpetuate. Colman's otherwise very good discussion of "scientific racism" comes to a far too charitable conclusion: "I do not . . . believe that Jensen and Eysenck . . . are racists in the crude sense. . . ."<sup>114</sup> I *cannot* concur.

Again in the new Preface<sup>115</sup> (p. 59) readers are informed of a "major critical effort . . . containing eight articles" written about HER. In that reference we find the following: ". . . the causes of differential intelligence. . . . It seems incredible that anyone still doubts that there are genetic factors involved. The heritability estimates cited by Jensen (1969), even if they are twice too high, should establish this point. . . .

106. Jensen, HER.

107. Jensen, *Genetics and Education*.

108. Jensen, HER.

109. Jensen, *Genetics and Education*.

110. R. Rieger, A. Michaelis, and M. M. Green, *A Glossary of Genetics and Cytogenetics* (New York: Springer-Verlag, 1968), p. 404.

111. Jensen, *Genetics and Education*.

112. S. Wright, *Evolution and the Genetics of Populations* (Chicago: The University of Chicago Press, Vol. 2, 1969), p. 423.

113. N. Pastore, *The Nature-Nurture Controversy* (New York: Columbia University King's Crown Press, 1949), p. 28.

114. A. M. Colman, "'Scientific' Racism and the Evidence on Race and Intelligence," *Race*, Vol. XIV, No. 2 (1972), pp. 137-153.

115. Jensen, *Genetics and Education*.

For simple qualitative biochemical traits, it can be seen that there are some—blood groups, for example—that cannot be altered no matter what is done to the environment.”<sup>116</sup> The absolute fixity of the blood groups has long been a sacrosanct concept to all concerned with heredity. So it is of no small interest to consider the next fact reported by two of the internationally most renowned authorities on the subject: “It dawned slowly to . . . [us] that a certain kind of weak B antigen in seven samples of otherwise group A<sub>1</sub> blood . . . tested over the years was, contrary to all previous experience, an acquired and not an inherited antigen. The proof that the antigen was acquired rather than inherited. . . .”<sup>117</sup>

The relevance of the foregoing to this discussion and to everything Jensenism represents is that this very same blood group story was presented by me to Jensen on a platform shared by us with Robert Cancro, Bruce K. Eckland, Benson E. Ginsburg, Ching Chun Li, and Steven G. Vandenberg before a large audience at the November, 1969, Illinois conference.<sup>118</sup>

If blood types can change should we *assume* intelligence to be less plastic? And Jensen has known but ignored this fact for at least four years.

Another way of appreciating the triviality of Jensenism’s use of heritability to “answer” the nature-nurture pseudo-question is to realize that the answer thus obtained is no more meaningful than any answer given to the equally pseudo-question: “Which is more important in determining area, length or width?”

Third, the quantitative genetic model employed is elegant and impressive but let us never overlook (1) its assumptions, (2) our knowledge of reality, and (3) the fit between (1) and (2). So long as every gene comes in only two forms (= alleles), as in Mendel’s classic study, contradictions are not apparent. Variances and correlations do their job. But consider the following case of a gene having three alleles, A<sub>1</sub>, A<sub>2</sub>, A<sub>3</sub> and note what happens.

Given A<sub>1</sub> = 0, A<sub>2</sub> = 1, A<sub>3</sub> = 2, then A<sub>1</sub>A<sub>1</sub> = 0, A<sub>1</sub>A<sub>2</sub> = 1, A<sub>1</sub>A<sub>3</sub> = 2, A<sub>2</sub>A<sub>2</sub> = 2, A<sub>2</sub>A<sub>3</sub> = 3, A<sub>3</sub>A<sub>3</sub> = 4. For a population with only alleles A<sub>1</sub> and A<sub>2</sub>

$$\text{mean} = \frac{0 + 1 + 2}{3} = 1$$

$$\text{variance} = \frac{(0 - 1)^2 + (1 - 1)^2 + (2 - 1)^2}{3} = \frac{2}{3}$$

For another population with only alleles A<sub>1</sub> and A<sub>3</sub>

$$\text{mean} = \frac{0 + 2 + 4}{3} = 2$$

$$\text{variance} = \frac{(0 - 2)^2 + (2 - 2)^2 + (4 - 2)^2}{3} = \frac{8}{3}$$

We find the absurd result that one population with equal proportions in its set of two alleles will have a much greater variance than another population with equal proportions in its different set of two alleles—a counter-intuitive and unacceptable inconsistency. And human genetic research is revealing an ever increasing number of

116. T. G. Gregg and P. R. Sanday, in C. Loring Brace, G. F. Gamble and J. T. Bond (eds.), *Race and Intelligence*, American Anthropological Studies, Vol. 8 (1973), p. 58.

117. R. R. Race and R. P. Sanger, *Blood Groups in Man* (Oxford: Blackwell, 5th Ed., 1968), p. 31.

118. R. Cancro (ed.), *Intelligence, Genetic and Environmental Influences* (New York: Grune and Stratton, 1971).

loci to be polyallelic. Misleadingly the textbook discussions and illustrations only consider the simplest two allele case. That is why, when Lewontin had to deal with genetic realities involving known polyallelic human loci, he abandoned the classic quantitative genetic model and employed the information measure.

Lewontin's important findings throw into bold relief the extent to which so many have been duped by the propaganda of Jensenism. For *known* human loci, "Less than 15% of all human genetic diversity is accounted for by differences between human groups! Moreover the difference between populations within a race accounts for an additional 8.3%, so that only 6.3% is accounted for by racial classification."<sup>119</sup> (p. 396) With race differences contributing such a small amount, i.e. 6.3%, to human diversity on known genes, there is at this time no basis whatsoever for the specious racist rhetoric alleging race differences make a greater contribution to human diversity on Jensen's sham "intelligence genes" (phrase echoed by Humphreys).<sup>119a</sup>

#### VII. AN INTELLECTUAL WATERGATE

When it was pointed out in my discussion at Cambridge that, instead of imparting new knowledge, Jensen was acting out a classic role: There's been one every generation, go and read Henry Garrett. He said long ago everything Jensen has to say, Jensen protested and claimed that he did have something to add, but never told us what. However, he reassured the audience about Garrett's worthiness. Now Garrett certainly could not be considered to have been an "environmentalist" nor to have been prejudiced against what Jensen represents. Therefore, it is relevant to find the following discussion in Garrett's regular section of *The Citizen*:

#### WHAT IS THE ANSWER

By Henry E. Garrett, Ph.D.  
*Professor Emeritus, Psychology*  
*Columbia University*  
*Past President*  
*American Psychological Association*

*Q: Dr. Garrett, that Dr. Arthur Jensen of the University of California certainly made a splash, didn't he, with his findings? While I am glad to see the publicity given his opinions—that Whites are smarter than Negroes—what's so new about that? It seems to me others, you among them, have been presenting this sort of evidence for a long time.*

*A: What Dr. Jensen did was to find that Negroes have IQs about 15 points lower, on the average, than Whites. This, it is true, has been many times reported. What is interesting about the great amount of publicity given Dr. Jensen's published work is that *publicity* itself. Perhaps times are changing.<sup>120</sup> (Italics added in second paragraph only)*

Publicity (propaganda!) was exactly the point being made by Alfert which unleashed Jensen's scurrilous attack on her. While not all the 159 references in the HER travesty have been considered here, certainly enough material has now been

119. R. C. Lewontin, "The Apportionment of Human Diversity," in T. Dobzhansky, M. K. Hecht and W. C. Steere (eds.), *Evolutionary Biology* (New York: Appleton-Century-Crofts, Vol. 6, 1972), pp. 381-398.

119a. T. Edson, "jensenism, n—the theory that I.Q. is largely determined by the genes," in *The New York Times Magazine*, (August 31, 1969); L.G. Humphreys, *American Psychologist*, Vol. 30, no. 1 (1975), p. 96.

120. H. E. Garrett, "What is the Answer," *The Citizen*, Vol. 14, No. 4 (1970), p. 21.



examined to make clear why we can believe little, if anything, Arthur Jensen speaks or writes.

We have seen that Jensenism turns out to be a moral, not a scientific problem. On the one hand, once again we have been betrayed by the intellectuals (*La trahison des clercs*<sup>121</sup>). That so many unscholarly "experts" would accept such a specious product provides stark proof of the intellectual bankruptcy of our present harried academic system, which overvalues speed and quantity of output at the expense of quality: publish (ever more) or perish! Jensen's list of publications reaches shameful lengths. A glimpse is provided in his *Genetics and Education* book,<sup>122</sup> where he flaunts both a list of 117 articles about HER by other writers and another list of 45 of his own articles for 1967-1972. There, his 5-year count is 1097 journal pages, plus (1) five entries without pagination, (2) the 378 pages of that book (reprinting six of his articles and adding a new 67-page self-serving preface), and (3) another 407-page book with new text,<sup>123</sup> in which we are threatened with still "A third volume soon to be published . . ." <sup>124</sup> (now out in England!). Five years are 1826 days and the page total in my previous sentence exceeds 1826. There are 350 words on his average book page. So, including the duplication already mentioned, Jensen is *responsible* for a *published* torrent of more than 300 printed words every day of 5 years. My extremely conservative estimate does *not* include Jensen's total output or the even greater spate of words about Jensenism in journals and the media.

With such an undisciplined flow of intellectual pollution into our precious channels of scientific communication, no wonder our universities are in financial difficulty. Ironically now, under the headline "Financial Troubles Beset Libraries At Coast Schools," *The New York Times* describes the plight of Jensen's home institution: "The Berkeley Library has temporarily suspended buying books and planning for an addition. Berkeley is reported to be one year behind other major universities in library acquisitions . . ." <sup>125</sup> "Experts"—scientists, scholars, professors—must forever subject to repeated skeptical scrutiny—both empirical and scholarly verification—all aspects of *every* knowledge claim in their province, proposed by no matter whom, published no matter where. Until the requisite caution, skepticism, and humility become integral to our graduate training, which has not been so much too long, Jensenism will remain the rule rather than the exception.

On the other hand, this story becomes part of what the very perceptive sociologist-historian of science Merton credited psychiatrist Sir Lawrence Kubie with suspecting; namely the emergence of a "new psychosocial ailment among scientists which may not be wholly unrelated to the gangster tradition of dead-end kids. Are we witnessing the development of a generation of hardened, cynical, amoral, embittered, disillusioned . . . scientists" <sup>126</sup>—an intellectual Watergate? To paraphrase a *New York Times* editorial on the latter (June 7, 1973)<sup>127</sup>: either Jensenists knew what was being perpetrated and are therefore responsible, or did not and are therefore irresponsible. Like few other things, Jensenism demonstrates today why science without scholarship is bankrupt. *Addendum on page 102.*

121. J. Benda, *La Trahison des Clercs* (Paris: Grasset, 1927).

122. Jensen, *Genetics and Education*.

123. A. R. Jensen, *Educability and Group Differences* (New York: Harper & Row, Publishers, 1973).

124. A. R. Jensen, *Educational Differences* (London: Methuen and Company, Ltd., 1973).

125. "Financial Troubles Beset Libraries at Coast Schools," *The New York Times* (May 6, 1974), p. 19.

126. R. K. Merton, "Priorities in Scientific Discovery," *American Sociological Review*, Vol. 22 (1957), p. 657.

127. *The New York Times*, Editorial (June 7, 1973).