

In the January, 1972, *Kappan* William Shockley presented his views on "Dysgenics, Geneticity, Raceology: A Challenge to the Intellectual Responsibility of Educators." N. L. Gage, Shockley's colleague at Stanford University, presented contrasting views under the title, "I.Q. Heritability, Race Differences, and Educational Research."

In accordance with a prior understanding, Mr. Shockley then undertook to rebut Mr. Gage and Mr. Gage, after seeing the rebuttal, prepared a final statement. Both of these manuscripts are presented in the pages that follow.

Because both Shockley and Gage made frequent

reference to research and analyses done by Arthur Jensen of the University of California, Berkeley, Mr. Jensen asked for space to explain further work he has done on the topics under examination. Mr. Jensen and Mr. Shockley have few points of difference, but Mr. Gage was asked to comment on Jensen's statement if he wished.

Representative early responses to the initial Shockley-Gage encounter will be found in the Backtalk section of this *Kappan*. In spring issues of the *Kappan*, however, space limits will permit only brief comments on the questions and differences of opinion revealed here. — *The Editors*

A DEBATE CHALLENGE:

GENETICITY IS 80% FOR WHITE IDENTICAL TWINS' I.Q.'S

BY WILLIAM SHOCKLEY

The *Kappan's* January cover announced "A Debate Between William Shockley and N. L. Gage" and an editorial introduction described issues of "the Shockley-Gage encounter." In this second round, the articles are called "Shockley" and "Gage," academic titles and pronouns being used for the authors as persons.

Actually the "encounter" was rather indirect. Gage mentions me as Shockley, the person, only once and makes no specific reference to Shockley, the article. However, the two articles do contain an encounter of ideas that is central to the entire logical structure of Shockley a cornerstone presented in Shockley as its most emphasized single sentence. I quote it here:

"Intelligence, measured by I.Q.,

WILLIAM SHOCKLEY is Alexander M. Poniatoff Professor of Engineering Sciences, Stanford University. The January Kappan carried a special supplement on the relative influence of heredity and environment on human intelligence and performance, featuring his article, "Dysgenics, Geneticity, Raceology: A Challenge to the Intellectual Responsibility of Educators," with a response from N. L. Gage, professor of education and psychology at Stanford. Here Mr. Shockley responds to Mr. Gage, in the final round of the debate promised Kappan readers.

varies more than twice as much from genetic differences as it does from environmental differences for individuals from families like those that raise one of a pair of white identical twins."

The statement is conservative. Figures 1, 2, and 3 of Shockley led to an 80% figure for geneticity of I.Q., leaving less than 20% of I.Q. variance to environment for the defined population.

Gage stated three intentions for its presentation: "... first, to examine ... the data on whites that are cited in the controversy. Then [second] I shall consider the relevance of these data ... to ... Negro-white differences in mean I.Q. Third, I shall call attention to the need for educational research and development that can produce necessary improvement in achievement and attitudes of Negro youth and, ultimately, provide the only definitive test of Jensen's hypothesis."

The examination in Gage of its first stated intention can easily be misinterpreted as pulverizing the cornerstone statement of Shockley, although Gage does not specifically refer to that statement. In this comment on Gage, I shall analyze how Gage responded to the Shockley 80% geneticity assertion; I consider it to be a nonencounter that takes no position on the central statistical issue. The presentation of the second and third intentions of Gage were also

nonencounters. Specifically, Shockley outlined a variety of methods for research on genetic contributions to I.Q. deficits applicable to Negroes that were not restricted to the "only definitive test" proposed in the Gage third intention. These proposed methods were not referred to in Gage, although they were presented in Shockley in connection with Figures 5, 6, and 7 and the discussion of recent research on racial differences in brain anatomy reported in Shockley's reference 33. Thus on these matters pertinent to Gage's second intention, there was no "encounter" of the sort the *Kappan* had called for in asking "one prominent sociologist to respond to Mr. Shockley's paper, [who] promptly refused, arguing ... that [Shockley's] notions on heredity and race are 'wrongheaded' and 'obscene.'" Gage simply ignored the new research proposals of Shockley.

"Dysgenics, Geneticity, Raceology: A Challenge to the Intellectual Responsibility of Educators" was the title of my article. My section heading, "The Moral Obligation To Think," was a call to find the wisest means for diagnosing our national, growing human-quality ills. I believe that this objective demands new, inventive approaches. Professor Gage's discussion of the past history of slavery and discrimination leaves no doubt of the sincerity of his concern

with the tragic disadvantage of black Americans. The difference between Shockley and Gage is typified by the Shockley section heading, "Quantifiable Humanism?," and the Gage heading, "Better Educational R & D Needed." Environmental aspects of human behavioral problems now probably command 100 times more research support than do genetic aspects. Is it not thus imperative to find sound new approaches for human behavior genetics? Was the reluctance of Gage to "encounter" the proposals for new approaches in Shockley an example of the Apple-of-God's-Eye-Obsession described in Shockley?

Gage uses italics for emphasis once in connection with compensatory education and educational R and D and twice more, as discussed below under "Randomness of Environments," in regard to environmental effects on I.Q.

"Not" was used in Gage (p. 312) in rejecting as untrue Jensen's 1969 assertion that "compensatory education has been tried and it apparently has failed." This subject was not emphasized in Shockley. However, a comment is appropriate. Billions of dollars have been spent on compensatory education.¹ Programs have continued, not, as Gage suggested, for "merely a single enthusiastic Presidential administration," but since at least 1956.² A recent survey for the U.S. Office of Education of 1,200 evaluation reports by an independent research firm found, in the group of 326 that they could adequately evaluate, only 10 that "were actually found to be successful when subjected to in-depth analysis."³ The history of compensatory education projects followed by subsequent oblivion has been documented by Roger Freeman in "The Alchemists in our Public Schools." Jensen's "apparently has failed" evaluation is sufficiently warranted, in my opinion, to support my repeatedly stated demand that genetic factors must also be researched. I am not opposed to educational research. Indeed, I have estimated that the methods of Figure 4 of Shockley might affect attitudinal factors for black slum children by improving the control-of-environment student variable of Shockley Figure 7 so as to eliminate about one-third of the achievement deficit.⁴ However, if the remaining two-thirds is genetic, then denigrating the possibilities of significant genetic research and calling for all resources to go into educational research is a prelude to what is described as the Speer syndrome in Shockley.

Randomness of Environments and the 80% Geneticity Controversy

The other two uses of italics in Gage are also intimately associated with the goal of finding optimum educational methods to overcome disadvantages — particularly those of Negroes. The consequence is that a misleading impression is given of the actual facts of geneticity of I.Q., although a careful second or third reading shows that the 80% geneticity estimate of Shockley is not really disputed — indeed it is scarcely "encountered." To clarify the situation and to add to the available information I introduce Figure 1 (space limitations barred it from Shockley). It illustrates the role of randomness of environmental influences on I.Q. for Burt's separately reared identical twins and answers several questions raised in Gage about Burt's study.⁵ I shall here describe the figure by quoting from Shockley the following description of

its content:

"If the differences in environments between pairs of twins are compared with their differences in I.Q. for Burt's compilation, then it turns out — *as makes sense* — that better occupational class of home *does tend* to raise I.Q. — *but this tendency is not a certainty nor are the I.Q. increases very decisive*: Of the 35 cases in which co-twins differed in both I.Q. and occupational class, 23 were concordant — higher class with higher I.Q. — and 11 were discordant — lower I.Q. in the higher class home. The result is significant at the 0.02 level. Each upward step of one social class raises I.Q. on the average about one I.Q. point."

Both Shockley and Gage contain discussions of a famous pair of twins. In Shockley the paragraph immediately following the above quotation starts: "But what about Gladys and Helen [separately reared identical twins] with their 24-point difference [in I.Q.]?" I

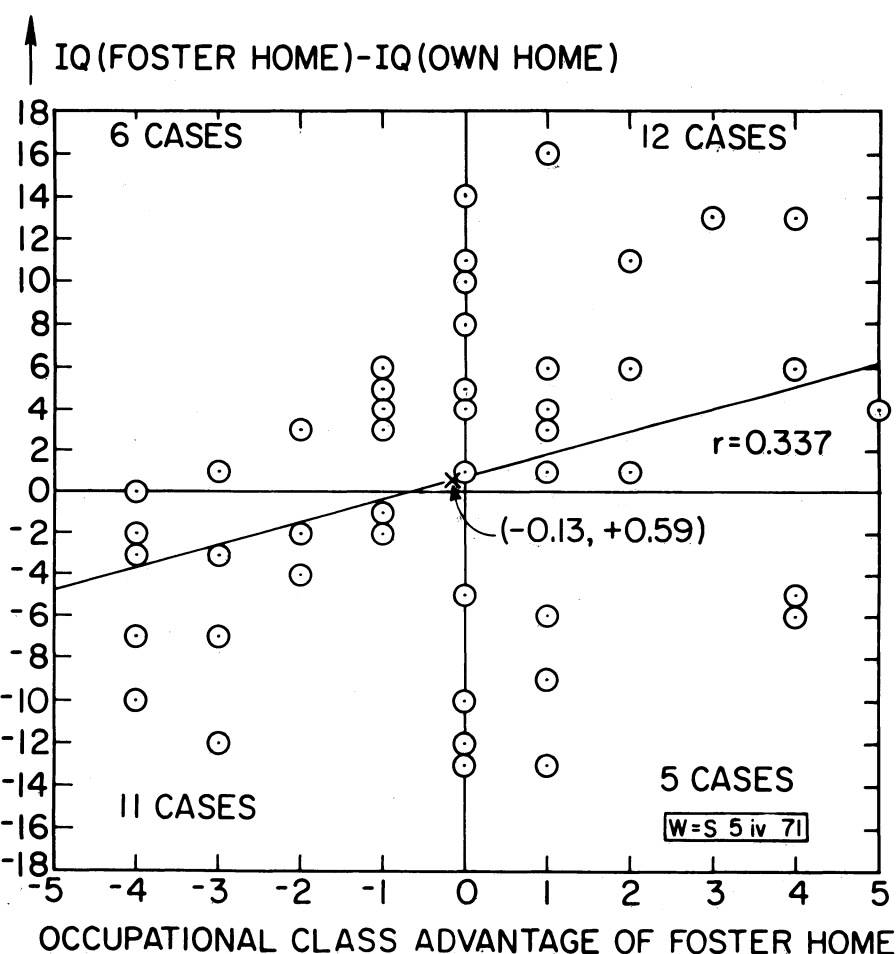


Figure 1 The correlation between I.Q. and occupational class of home (1, higher professional, etc.; 2, lower professional, etc.; 3, clerical, etc.; 4, skilled; 5, semi-skilled; 6, unskilled) accounts for about 26% of the environmental variance or 3.5% of the population I.Q. variance. (S.D. is 7.78 for I.Q. and 2.34 for occupational class differences on the figure.)

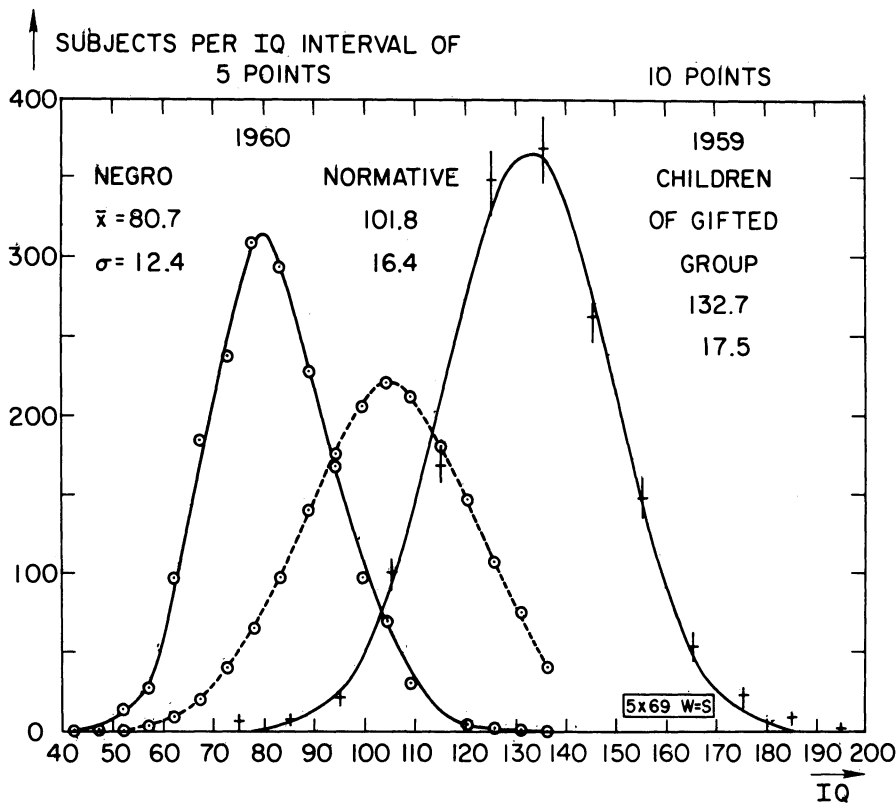


Figure 2 The normative and Negro I.Q. distributions are from Kennedy *et al.*⁸ The curve for the offspring of the Gifted Group is an analytic normal distribution drawn to fit out to two standard deviations. The $N^{1/2}$ statistical uncertainties in the data are represented by the vertical lines. The fit is seen to fall below the data beyond two standard deviations, a result in keeping with Burt's finding (see Shockley¹³ for references and analysis), so that the standard deviation of 16.5 is less than the observed value.

then concluded that if geneticity of I.Q. were 80%, so that environment contributed less than 20% to I.Q. variance, I could, using Census Bureau data and the huge environmental differences between Gladys and Helen, explain almost all of these 24 points. *Furthermore*, my article's analysis reasoned that if environmental influences are randomly distributed for the 122 pairs of twins that Jensen had assembled from four studies (a fact demonstrated by Figure 3 of Shockley), then an exceptionally large case is to be expected. I wrote: "There is only one chance in 100 that the largest value would have been smaller than 17 points." In other words, the Gladys-Helen 24-point I.Q. difference supplies the needed exception required by the 80% geneticity rule.

The significance of this statistical reasoning is missing from the observation that Professor Gage emphasizes with the italicized word "*together*".

"The Gladys-Helen I.Q. difference is rare; the Gladys-Helen I.Q. environmental difference is also rare. To have both rare events occurring *together* is evidence not of randomness but of strong nonchance association." These

sentences must be read in the complete context of Gage to avoid the conclusion that Gage is rejecting the random distribution of environments insisted upon by Shockley.

The following parallel grammatical construction for a situation of known randomness may be helpful: "A large payoff at a roulette table is rare; the occurrence of a double zero is also rare. To have both events occurring *together* when someone wins on double zero is evidence not of randomness but of strong nonchance association." The "*togetherness*" of the two events on the roulette wheel obviously has no relevance whatever to the randomness with which the little ball picks its number; the same is true of the randomness of the normal distribution of environments in which Gladys and Helen chanced to achieve the needed exceptionally large difference in environments and paid off exceptionally with the champion of all reported I.Q. differences.

The focus on finding the optimum environment for raising I.Q. or achievement can again lead to confusion in the longest italicized statement of Gage:

"It is clear that the I.Q. differences

were larger for those twins whose estimated educational disadvantages differed more; *the I.Q. difference and the educational-advantage difference correlated 0.79.*"

This well-documented observation in Gage of 0.79 for effect of environment on I.Q. may provoke doubt about the Shockley upper limit of 20% or 0.20 for environmental contributions to I.Q. variance. How is this conflict resolved? The answer is that the Gage value does not relate to the population variance but only to the fraction of it that corresponds to the difference between twins. (When these considerations are developed, the results are found to be statistically consistent with the other studies compiled by Jensen.) Thus although correlations like the 0.79 are central to problems of educational research, they do not alter the statistics of Figure 1 here or Figure 3 of Shockley; therefore, they do not alter the 80% estimate.

The white twin I.Q. data was examined in Gage in relationship to the racial intelligence controversy. There is another controversy about the 80% geneticity estimate itself. One of my student assistants polled five professors and six students in local psychology departments and not one would accept a value of geneticity as large as 50%, even for a population as narrowly defined as the white identical twins of Figure 1 of Shockley. Here, then, is a challenge for Professor Gage in his response to this comment, a comment heavily influenced by a lack of response to my earlier challenge: "Is my cornerstone more-than-twice statement scientifically valid or not?"

Eugenic Thinking Exercises And Behavior Genetics

The closest that Gage came to a direct encounter was to express concern that Jensen and Shockley might discourage governmental support for educators "... who want a fair chance to try their approach ..." and to make an oblique reference to the Shockley *thinking exercise* in the form of a *voluntary* sterilization bonus plan. This latter reference omitted my qualifying phrase, "regardless of sex, race, or welfare status," and my emphasis on the First Amendment. My estimate of possible "reduced costs of mental retardation care" was indirectly rejected — "drain resources away from educational ... efforts..." Unsureness about relevant behavioral genetic facts was stressed.

Examples of facts that do warrant thinking exercises about eugenics and dysgenics are presented in Figure 2. One of the most dramatic items of evidence for inheritance of high I.Q. is shown. The gifted children selected by Lewis M. Terman in the early twenties had offspring whose average I.Q. was 132.7.⁶ I found that this distribution is accurately fitted by a normal distribution out to two standard deviations from the mean. Furthermore, I found that the children adopted by the gifted group, although significantly above the population average, were lower than natural children (P less than 0.05). What is called familial retardation was wholly lacking – no I.Q.'s below 70 save for 0.5% of mentally defective. This little-recognized fact is perhaps one of the strongest of all pieces of evidence in support of “the two-group approach to the problem of mental retardation.”⁷ From data like these I conclude, contrary to the Gage position, that significant thinking can and should be done – it is a “moral obligation” to do so – about eugenics and dysgenics.

Does familial retardation play a role in the 20% or so of the Negro distribution of Figure 2 that falls below I.Q. 70? If so the total national figure for incidence of retardation might have to be nearly doubled. The climate reflected by Gage will not hasten research on

genetic factors. This climate was eloquently described in a paper introduced by H. F. Harlow at a meeting of the National Academy of Sciences and read by W. A. Kennedy, leader of the group that obtained the Negro data of Figure 2. Kennedy said:

“A second limitation is fear on the part of the scientist of the consequence of working in the area unless he clearly announces beforehand in the prospectus that he is an environmentalist and is consistent with the current national climate, which is that in a country where all men are created equal it is blasphemy to investigate differences. And although no one in the twentieth century is struck dumb for blasphemy, his research funds can be struck, and the effect is the same.”⁹

Professor Kennedy informed me about three years ago that research funds to continue his group's research could no longer be obtained.

I differ most with the Gage position on the possibility of inventive research approaches – as I remarked above in respect to Gage's “only definitive test.” Indeed, anti-evidence or the absence of evidence may sometimes be significant. A striking item of the absence of evidence is the lack of a widely recognized study of the conquest of black disadvantages by environmental improvements. Since 1968 I have emphasized

that significant evidence might result from changes in the characteristic ethnic patterns of intelligence reported by Lesser *et al.*: Chinese low on verbal and high on numerical and the reverse for Negroes.¹⁰

There is a natural explanation for the lack of reports of outstanding beneficial effects of interracial adoptions if man is indeed a mammalian form of life that is governed by the same biologic laws for his behavioral traits as mice studied by Daniel Bovet, Nobel Laureate in medicine and physiology, and his colleagues.¹¹ A relevant sample of the findings is shown in Figure 3. A two-compartment “shuttle box” allows a mouse five seconds to act on a warning light signal to avoid a shock. Strains of laboratory mice that are good learners improve their ability to avoid shocks during the first day's trials not quite so well as a poor learning strain. *But*, after sleeping on it overnight, they start the second day well ahead of where they left off the day before. Poor learners, in contrast, continue day after day to forget overnight and start worse the next day. The poor learners have another problem: The poor learners become progressively more emotional and exhibit a “freezing response” in a significant number of cases.

Does this emotional instability shown by freezing improve if the poor

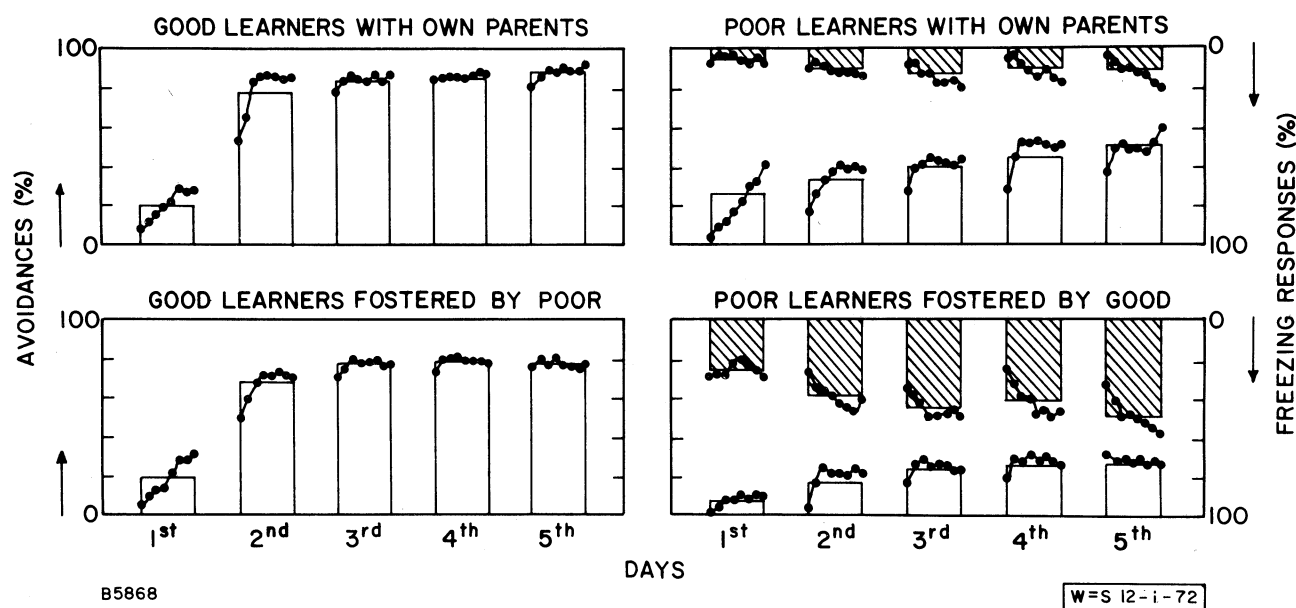


Figure 3 Mice of the “good learner” inbred strain (DBA/2J) “consolidate” the experience of the first day's 400 trials (one point for 50 trials) and start the second day better than they ended the first. “Poor learners” (C3H/He) do not consolidate as well and exhibit freezing responses even worse when fostered by non-freezers. (Redrawn by permission after Bovet *et al.*¹¹)

learners are fostered by the emotionally stable good learners? No! On the contrary, their freezing responses increase so much – about 10% to 50% – as to reduce their success at shock avoidance. Good learners do not become freezers when reared in the home environment of freezing foster parents. Their avoidance scores do drop, however, but only slightly – about 80% instead of 87% on the fourth day.

Would research on the behavior genetics of different subgroups or races of *Homo sapiens* establish similar effects? Do Bovet's mice studies indeed suggest the reason for the lack of reports of statistically significant successes in transracial adoptions of black slum orphans?

My previously stated opinion that the major causes of the American Negro's intellectual and social deficits are primarily hereditary and racially genetic in origin has been reached as a result of considerations like these. The presentation in Gage that such conclusions are based on extrapolating white-twin geneticity data is an error that may permit the intellectual community to avoid the moral obligation to think. I have tried to philosophize on this matter of moral obligation. The results are available in written form as "Three Moral Postulates: Truth – Concern – Death."¹²

The KAPPAN-Voltaire Parallel

"I disapprove of what you say, but I will defend to the death your right to say it." So is Voltaire quoted.

My experience with the *Kappan* has parallels with both of Voltaire's clauses. The *Kappan* has indeed created and defended my right to express what I think and has done so knowing of the criticism it may face as a consequence. I have not always had my right to speak or be printed so zealously defended.

Once in each of the years 1968, 1969, and 1971, after a disruption or the threat of one prevented me from delivering a scheduled speech, Voltaire has been eloquently and publicly echoed – but to no effect. The president of Brown University's chapter of Sigma Xi, the honorary scientific research fraternity, complained bitterly about the cancellation of the twenty-fifth anniversary convocation of Sigma Xi at the Polytechnic Institute of Brooklyn, but my requests to speak either at Brown or P.I.B. were rejected. At Dartmouth College in 1969 my

experience was similar. I am now awaiting a response from Sacramento State College (the case mentioned in the *Kappan* introduction) to my offer to lecture or participate in a TV debate with Professor Mercer or Mr. Mayeske; a spokesman for the Department of Health, Education, and Welfare has credited them with "refuting Jensen and Shockley." The *Kappan* lived up to Voltaire's second clause.

The *Kappan* also lived up to Voltaire's first clause – unwittingly, entertainingly, but, from my viewpoint, disappointingly. Consider this abbreviated quotation from the editorial introduction to the Shockley-Gage encounter:

"The editors . . . have no intention . . . of taking sides on the substantive questions. . . ."

"We believe that bad conditions make bad people. We prefer to regard genetic inheritance . . . as simply . . . environment . . . and ultimately manipulable."

Is this not a claim of impartial non-side-taking promptly followed by rejection of my dysgenic threat? Indeed, if Bovet's mice are like people, would not the *Kappan's* environmentalist emphasis erroneously lead to the conclusion that "bad" emotionally unstable mice that freeze are made so by emotionally "bad" home environments? I propose that a striking inconsistency has arisen because the *Kappan*, in accord with Voltaire's first clause, strongly disapproves of what I say – indeed disapproves so strongly that what I say is rejected without the rejection being realized. If my analysis is sound, and this point gets across, then an inconsistency will have contributed to the principal objective of my strenuous writing efforts for the *Kappan* – to strengthen the moral obligation of my readers to think – even about dysgenics.

1. "Crisis in American Education," an address by Roger A. Freeman, special assistant to the President of the United States, before the annual meeting of the Washington State Research Council, June 19, 1970.

2. From Freeman manuscript printed in Extension of Remarks, *Congressional Record*, April 24, 1969, pp. E 3374-81.

3. Michael J. Wargo, Peggie L. Campeau, and G. Kasten Tallmadge, "Further Examination of Programs for Educating Disadvantaged Children," Final Report of Contract OEC-0-70-5016, Report No. AIR-2026-7/71-FR, July, 1971.

4. "Better Education for Minority Groups?," *Daily Gazette*, Berkeley, April 15, 1970, p. 9.

5. Cyril Burt, "The Genetic Determination of Differences in Intelligence: A Study of Monozygotic Twins Reared Together and Apart," *British Journal of Psychology*, 1966, pp. 137-53.

6. Lewis M. Terman and Melita H. Oden, "The Gifted Group at Mid-Life: Thirty-five Years' Follow-up of the Superior Child," *Genetic Studies of Genius* (Vol. 5). Stanford: Stanford University Press, 1959.

7. Edward Zigler, "Familial Retardation: A Continuing Dilemma," *Science*, January 20, 1967, pp. 292-98.

8. 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 Southeastern United States," Monograph 90, The Society for Research in Child Development, 1963.

9. As quoted from W. A. Kennedy, unpublished manuscript titled "Racial Differences in Intelligence: Still an Open Question?," in W. Shockley letter to John W. Gardner, printed in Extension of Remarks, *Congressional Record*, August 12, 1969, p. E 6849.

10. W. Shockley, "Human-Quality Problems and Research Taboos," *New Concepts and Directions in Education*. Greenwich, Conn.: Educational Records Bureau, 1969.

11. D. Bovet, F. Bovet-Nitti, and A. Oliverio, "Genetic Aspects of Learning and Memory in Mice," *Science*, 1969, pp. 139-49.

12. W. Shockley, "Three Moral Postulates: Truth – Concern – Death," *Letters, Presbyterian Life*, February 1, 1972.

13. W. Shockley, "A 'Try Simplest Cases' Approach to the Heredity-Poverty-Crime Problem," Proceedings of the National Academy of Sciences, June, 1967, pp. 1767-74. □

Jensen – footnotes

(Continued from page 421)

Holzinger, *Twins: A Study of Heredity and Environment*. Chicago: University of Chicago Press, 1937.

3. Cyril Burt, "The Genetic Determination of Differences in Intelligence: A Study of Monozygotic Twins Reared Together and Apart," *British Journal of Psychology*, 1966, pp. 137-53.

4. M. G. Bulmer, *The Biology of Twinning*. Oxford: Clarendon Press, 1970, p. 146.

5. *Ibid.*, p. 39.

6. Burt, *op. cit.*

7. Arthur R. Jensen, "I.Q.'s of Identical Twins Reared Apart," *Behavior Genetics*, 1970, pp. 133-46.

8. Barbara S. Burks, "Mental and Physical Development Patterns of Identical Twins in Relation to Organismic Growth Theory," *Yearbook of the National Society for the Study of Education*, Part II, 1940, pp. 85-96.

9. Burt, *op. cit.*, p. 142.

10. Gage, *op. cit.*, p. 309.

11. Jensen, *op. cit.*

12. Gage, *op. cit.*, p. 309.

13. Cyril Burt, "The Inheritance of Mental Ability," *American Psychologist*, 1958, pp. 1-15, Table 2.

14. Arthur R. Jensen, "Race and the Genetics of Intelligence: A Reply to Lewontin," *Bulletin of the Atomic Scientists*, May, 1970, pp. 17-23.

15. Arthur R. Jensen, "Can We and Should We Study Race Differences?," *Disadvantaged Child*, Vol. 3, J. Hellmuth (ed.). New York: Brunner/Mazel, 1970, pp. 124-57.

16. H. J. Eysenck, *The I.Q. Argument*. Freeport, N.Y.: The Library Press, 1971.

17. Arthur R. Jensen, "Twin Differences and Race Differences in I.Q.: A Reply to Burgess and Jahoda," *Bulletin of the British Psychological Society*, 1971, pp. 195-98. □