

## JENSEN'S THEORY OF INTELLIGENCE:

### A REPLY

ARTHUR R. JENSEN<sup>1</sup>

University of California, Berkeley

The criticism of Jensen's "theory of intelligence" by Humphreys and Dachler lacks cogency because it (a) takes account of only a limited portion of the supporting evidence and (b) supposedly tests the theory by using data from Project TALENT based on mental tests which are far from ideal for this purpose.

The criticism of my "theory of intelligence" by Humphreys and Dachler (1969), although making an interesting and worthwhile methodological point, is based on so limited a view of the theory and of the supporting evidence as to constitute a very weak criticism when viewed in proper perspective. The questionable suitability of the Project TALENT tests used by Humphreys and Dachler for testing hypotheses derived from the theory, as compared with tests and studies specifically designed for this purpose, hardly warrants these authors' overly sweeping conclusion that "...Jensen's published results can be disregarded and that unbiased data lend no support to his theory [p. 426]."

#### *Theoretical Misconceptions*

The theory, which has been explicated in detail elsewhere (Jensen, 1968a, 1969, in press), concerns the organization of mental abilities in individuals. It incidentally predicts, and was originally formulated to explain, relationships between intelligence, rote-learning ability, and socioeconomic status (SES). It postulates two broad types of mental ability: Level I (associative and rote learning ability as measured, for example, by digit-span memory and rote serial learning), and Level II (abstract reasoning as measured by most IQ tests and in purer form by tests such as Raven's Progressive Matrices). The correlation between Level I and Level II abilities, according to the theory, is due largely to the hierarchical functional dependence of Level II proc-

esses upon Level I. The educational and occupational requirements of our society tend to sort out people much more by their Level II ability than by their Level I ability, so that lower and upper SES groups will differ markedly on Level II tests while differing little or not at all on Level I.

Humphreys and Dachler are wrong to argue that a *general* theory of intelligence means that it must *generalize* to the entire population. The theory is about the organization of mental abilities. It makes no statements about population parameters per se. Given such parameters, however, the theory should yield specific predictions. But populations differ in their social class composition and relative frequencies in various SES categories, and a truly general theory of intelligence cannot include these specific population characteristics (though it needs to know them for specific predictions) any more than a general theory of gravitation includes such data as the mass of the earth or air resistance, which nevertheless need to be taken into account in order to make a specific prediction from the law of falling bodies. Humphreys and Dachler have thus failed to distinguish clearly between the theory itself and the ancillary information required to derive any specific test of the theory.

Furthermore, it is not at all clear just what alternative theory or empirical state of affairs prevails according to Humphreys and Dachler. Is it that all kinds of mental abilities are about equally correlated with SES? This is clearly refuted by much solid evidence.

<sup>1</sup> Requests for reprints should be sent to: Arthur R. Jensen, School of Education, University of California, Berkeley, California 94720.

*Questionable Suitability of Project  
TALENT Tests*

If a hypothesis is to receive a fair test, the measuring instruments must have a "resolving power" adequate to the requirements of the theory. Level II is adequately measured by most general intelligence tests, preferably the "culture-fair" variety of nonverbal reasoning tests, such as the Progressive Matrices. Level I is best measured by tasks that involve the least verbal mediation, conceptual coding, mental manipulation, or other transformation of the input in order to respond with the correct output. A variety of standard laboratory rote-learning tasks serve as adequate measures, but simple digit-span memory (especially digits forward) is the best single measure that has been found thus far. The two rote-memory tasks in the Project TALENT battery appear to be tests more of verbal intelligence (verbal reasoning, comprehension, and word knowledge—all Level II abilities) than of rote memory per se, and the correlations in Table 5 by Humphreys and Dachler suggest that this is the case. The Memory for Sentences test is described as "the ability to memorize simple descriptive statements and recall a missing word when the rest of the sentence is provided sometime later." The Memory for Words test is described as "the ability to memorize foreign words corresponding to common English words" (Flanagan & Cooley, 1966, p. 78). Though these tests are far from ideal measures of Level I, the Memory for Words, at least, is undoubtedly closer to Level I than some of the other tests in the battery such as Information, English Total, Mathematics, and Mechanical Reasoning. In accord with my theory, Project TALENT data on a 10% sample of male twelfth graders ( $N = 2,946$ ) show multiple correlations between a number of SES indexes and these Level II-type tests of .53 (Information), .44 (English Total), .46 (Mathematics), .41 (Mechanical Reasoning) as compared with only .24 for Memory for Words (Flanagan & Cooley, 1966, p. E-8).

*Pseudo-Orthogonal Design Not an "Error"*

It is true that the statistical results derived from a pseudo-orthogonal design (equal representation of sample groups that are not of equal frequencies in the population) do not permit inference about the results that would obtain in the total population. But this may not be one's aim, which may be rather to test the significance of differences between certain selected groups formed by a two-way ANOVA design, in which case a pseudo-orthogonal design is not an "error," as it is labeled by Humphreys and Dachler. Imagine a hypothetical situation in which two subpopulations of individuals, A and B, representing 1% and 99% of the total population, respectively, showed correlations between traits  $X$  and  $Y$  of 0.00 for Group A and of 1.00 for Group B. Think of the impracticability of investigating the differences between these two groups if one insisted on their proportional representation in all experimental designs. For predicting correlations in the population as a whole, if that's all one wants, Group A could easily be ignored altogether for the negligible difference it would make. But for discovering the ways in which Group A differs from Group B, it is most economical statistically to make comparisons on equal-size groups. Thus, exploratory investigative strategies involving the use of a pseudo-orthogonal design can be quite warranted. In scientific research, following the Humphreys and Dachler philosophy of methodology on this point would be to maximize the probability of not discovering new and interesting facts of nature.

Besides, the evidence for a main point of my findings does not rest exclusively upon the use of a pseudo-orthogonal design. Neither does it depend upon the use of children in special classes for the educable mentally retarded. Strong evidence from my own laboratory and from elsewhere shows that upper and lower SES groups differ much more on IQ than on rote memory ability. This is an interesting and important finding. It cannot be disregarded on the basis of the Humph-

reys and Dachler critique, for it does not at all depend upon the pseudo-orthogonal design which is the main target of their criticism. The use of a pseudo-orthogonal design and/or of low-IQ children from special classes is relevant to only a few of our earlier studies (Jensen, 1961, 1963; Rapier, 1968). More recent publications present results based on random samples of children from regular classes in schools serving low- and middle-SES neighborhoods (Jensen, 1968a, 1969; Jensen & Rohwer, 1968).

#### *Examples from Jensen's Laboratory*

1. Representative samples of low- and middle-SES preschool children, ages 4-6, showed correlations between mental age and paired-associate learning (with chronological age partialled out) of .10 in the low-SES group,  $N = 100$ , and .51 in the middle-SES group,  $N = 100$ . (The low-SES children were Negro; the middle-SES children were white.) Despite a difference of 18 IQ points between the groups, they did not differ significantly in paired-associate learning, serial learning, and digit span (Jensen, 1968a).

2. Some idea of the discrepancy between digit span (Level I) and Progressive Matrices (Level II) as a function of SES is seen in comparing the 30 *lowest* scoring children in a white, middle-SES school (the lowest 6.1% of children in Grades 4, 5, and 6) with the 30 *highest* scoring children on digit span in a Negro, low-SES school (the upper 7.9% of Grades 4, 5, and 6). The mean digit-span test scores (expressed as percentage of maximum possible score) were 65.3 for the low-SES group and 38.7 for the middle-SES group. The corresponding Progressive Matrices scores (as percentage of maximum possible score) were 64.7 and 72.6, respectively. Also, the regression ( $b$ ) of Progressive Matrices on digit span was different, as predicted, in the low-SES and high-SES groups— $b = .35$  and  $.50$ , respectively (Jensen, 1968a).

3. Low- and middle-SES groups selected at random from regular classes in Grades K, 1, 3, and 6 in low-SES and middle-

SES schools ( $N = 48$  in each of eight groups) showed no significant difference in rote paired-associate learning, although low- and middle-SES groups differed by an average of 15-20 points in IQ (see Jensen & Rohwer, 1968, for a graph of these data).

Are these findings interesting? And possibly important? Should they be disregarded simply because earlier results based on a pseudo-orthogonal design gave rise to the theory which prompted these later studies?

#### *Data from Other Investigators*

1. Large-scale normative data on Stanford-Binet IQs for white ( $N = 2,904$ ) and Southeastern Negro ( $N = 1,800$ ) school-age population samples show a mean difference of 21 IQ points, a difference reflected most strongly in the Vocabulary subtest, which also has the highest correlation with total IQ. Vocabulary is a good Level II measure. The average percentage passing the Stanford-Binet Vocabulary subtest at various ages is 62% for white and 20% for Negro children. On the other hand, the average percentage passing the one subtest that best measures Level I ability—Digit Span—is 50% for white and 46% for Negro children (see Jensen, 1968b, p. 21, for a graphic presentation of these data).

2. The most valuable studies, of course, are those specifically designed for the explicit purpose of testing hypotheses derived from my theoretical formulation. For example:

*Guinagh's study.* Guinagh (1969) tested low-SES Negro ( $N = 105$ ), low-SES white ( $N = 84$ ), and middle-SES white ( $N = 79$ ) third graders on Raven's Colored Progressive Matrices and a digit-span test. Three major theoretical predictions were tested. One of these was not substantiated ("...the scatter-diagrams give no evidence for Jensen's hypothesis that high BLA [basic learning ability as measured in this study by digit span] is necessary for high IQ [measured by Progressive Matrices]."). However, in accord with the

prediction, the low- and middle-SES groups, though differing very significantly on the Progressive Matrices, did not differ significantly on digit span. Correlations (corrected for attenuation) between Progressive Matrices and digit span were .29 for low-SES Negro, .13 for low-SES white, and .43 for middle-SES white. Also, low-IQ and low-SES Negro children with low digit-span scores showed no significant improvement on matrices after a specific instructional program on this type of problem solving, while low-IQ and low-SES Negro children with high digit-span scores showed a significant gain on matrices performance after instruction, with the gains measured against no-instruction matched control groups. This is a most important finding in connection with my formulations regarding two types of mental retardation (Jensen, in press).

*Durning's study.* Durning (1968) analyzed data on 5,539 Navy recruits ("... approximately the total input for a period of six weeks to the Naval Training Center, San Diego"); 95% of them were between 18 and 23 years of age, with an average education of 11.9 years. They were given a battery of standard selection tests including the Armed Forces Qualification Test (AFQT) and a special auditory digit memory test, with a reliability of .89, devised by Jensen. Six hypotheses derived from the theory were tested. Five hypotheses were substantiated at a high level of significance; one was rejected—"Basic learning ability as measured by digit span was not found to bear the 'necessary-but-not sufficient' relationship to general intelligence [AFQT]... the hierarchical relationship between Level I and Level II which [Jensen] observed may be evident only in children [Durning, 1968, p. 61]." Category IV (CAT-IV) recruits (10th-30th percentile on AFQT) predominantly come from low-SES and culturally disadvantaged segments of the population. The correlation (corrected for restriction of range) between AFQT and digit memory for CAT-IV Ss was .21; for non-CAT-IV Ss it was .40; the difference is significant beyond the .01 level. Most important:

"Negro CAT-IVs as a group scored significantly higher on the Memory for Numbers Test than non-Negro CAT-IVs, though the Negroes were lower on most of the standard selection tests [Durning, 1968, p. 21]." Would any other theory have predicted this important finding? It was not a post hoc prediction, either.

### Conclusion

The one aspect of my theory most in question at present is the hierarchical dependence of Level II on Level I. The factorial distinctness of the two types of abilities, their different regressions on SES, and the different regression of Level II on Level I in lower- and middle-SES groups are well substantiated. The theory can be further tested most effectively by means of experiments and measurement techniques specifically devised for testing hypothetical deductions from the theory, as did Glasman (1968) in deriving predictions (which were borne out) about age and SES differences in conceptual clustering in free recall. A series of studies explicitly designed to test the theory are now in analysis and will be submitted to this Journal and others in the coming year. The Humphreys and Dachler article and this reply will have helped to set the stage for the studies that will follow.

### REFERENCES

- DURNING, K. P. Preliminary assessment of the Navy *Memory for Numbers Test*. Unpublished master's thesis, San Diego State College, 1968.
- FLANAGAN, J. C., & COOLEY, W. W. *Project TALENT one-year follow-up studies*. Pittsburgh, Pa.: University of Pittsburgh, 1966.
- GLASMAN, L. D. A social-class comparison of conceptual processes in children's free recall. Unpublished doctoral dissertation, University of California, 1968.
- GUINAGH, B. J. An experimental study of basic learning ability and intelligence in low socioeconomic populations. Unpublished doctoral dissertation, Michigan State University, 1969.
- HUMPHREYS, L. G., & DACHLER, P. Jensen's theory of intelligence. *Journal of Educational Psychology*, 1969, **60**, 419-426.
- JENSEN, A. R. Learning abilities in Mexican-American and Anglo-American children. *California Journal of Educational Research*, 1961, **12**, 147-159.
- JENSEN, A. R. Learning ability in retarded, aver-

- age, and gifted children. *Merrill-Palmer Quarterly Journal of Behavior and Development*, 1963, 9, 123-140.
- JENSEN, A. R. Patterns of mental ability and socioeconomic status. *Proceedings of the National Academy of Sciences*, 1968, 60, 1330-1337. (a)
- JENSEN, A. R. Social class, race, and genetics. *American Educational Research Journal*, 1968, 5, 1-42. (b)
- JENSEN, A. R. Intelligence, learning ability, and socioeconomic status. *Journal of Special Education*, 1969, 3, 23-35.
- JENSEN, A. R. A theory of primary and secondary familial mental retardation. In N. R. Ellis (Ed.), *International Review of Research in Mental Retardation*. Vol. 4. New York: Academic Press, in press.
- JENSEN, A. R., & ROHWER, W. D., JR. Mental retardation, mental age, and learning rate. *Journal of Educational Psychology*, 1968, 59, 402-403.
- RAPIER, J. The learning abilities of normal and retarded children as a function of social class. *Journal of Educational Psychology*, 1968, 59, 102-110.

(Received September 15, 1969)