

# Educability and Group Differences

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**Professor Thoday reviews a recent book  
by Arthur R. Jensen.**

THE recently published book by Jensen<sup>1</sup> is a very difficult one to review, not so much because the subject is controversial, but because so much of opinion in the area is strongly held, so that critical evaluation of the relation of data to hypothesis is limited. Thus, to review the book properly one would need not only to consider other books that have resulted from the publication of Jensen's article<sup>2</sup> in *Harvard Educational Review* but also to consider each important statement in each book in relation to the original literature on which the statement is based. Even this would be inadequate, for it is my experience that the original sources seldom give enough information about sample structure or detail about results and statistical techniques to permit satisfying judgment and that correspondence with the authors of the paper is needed.

No reviewer can do all this especially in such an inter-disciplinary field. If one does any of it, however, one is brought face to face with a most unsatisfactory situation: a scientific area in which, because of the impact of strongly felt ideopolitical points of view, there is wholly inadequate objective criticism of the relation between data and conclusions either by authors or their readers.

Jensen, in his original *Harvard Educational Review* article<sup>2</sup>, raised old questions because he was, I am sure rightly, disturbed that much literature, and a good deal of policy, is based on the assumption that variance in performance of IQ tests, and thus in correlated aspects of educability, arises largely or wholly from environmental reasons, despite the strong evidence for a large within-group genetic component. He also raised the question of between-group differences, in particular the average difference between American white and negro social groups, maintaining that it was plausible to postulate a genetic component of this difference also. It should be stressed that Jensen only put this forward as a hypothesis, not as proved, but it was for putting forward this hypothesis that he has been most attacked. It should, however, be remembered that to postulate genetic variance in 'educability' even in a non-racial context is unacceptable to many.

(The groups mentioned in the previous paragraph are primarily social groups, though they correlate partially with biological ancestry. Leach, reviewing this book in the *Listener*, made this a major criticism of Jensen, holding that he must not make a biological comparison between two groups unless the groups are defined by purely biological criteria. This is illogical. The whole question is whether two socially defined groups differ biologically in a certain variable or whether the difference is all a consequence of social factors.)

Jensen's book is about group differences, but is, as was his original article, based on the evidence concerning within-group differences. Here there are two points which may be regarded as well established.

(1) Relative performance in IQ tests correlates with relative performance in aspects of education that are highly relevant to success in and the needs of modern societies. This statement remains true even if we recognise that relative IQ is not to be equated with relative 'intelligence', and that the

related aspects of educability are not to be equated with the whole of educability.

(2) Within-group IQ variance has a large genetic component. There is room for argument whether Jensen's estimate of 80% may not be on the high side, but there can be no doubt that heritability of IQ ranks high compared with heritabilities reported in the generality of studies of continuous variables, whether behavioural or physical, in man or other organisms.

The high within-group heritability of this variable does not necessarily imply anything about between-group differences, but such differences need explanation. There are, however, fundamental methodological problems, of which Jensen is well aware, which arise from the necessary fact that different social groups differ in their environmental circumstances and are also partial genetic isolates. Any environmental differences are therefore necessarily correlated with possible gene frequency differences and vice versa. Peculiarly critical experimental design and cautious interpretation are therefore required in this field.

The two main kinds of between-group difference of mean IQ are those concerning socioeconomic groups (or social 'classes') and those concerning 'racial' groups, particularly the United States white-'negro' comparison (I place negro in inverted commas since the United States negro population has much white ancestry).

Considering within-race socioeconomic groups (the correlation between IQ and 'class' is about 0.5), since (among United States and British whites) within-group heritability is high, social mobility is high and social mobility is correlated with IQ, it would be very surprising if gene frequency differences were not the explanation of part of the differences of mean IQ. This, however, is a long way from proof, still less estimation of the relative magnitude of the genetically and environmentally caused components of the group differences.

In his book Jensen gives little attention to 'social class' differences. His one argument about the evidence (pages 155-6) is, however, weak, for though he shows certain correlations are incompatible with a zero correlation between IQ genotype and social class, he makes no allowance for error in the estimates, and the correlations would be compatible if, for example, IQ heritability were 70% instead of the 80% he takes.

## Three Questions

Most of his book concerns the 'racial' comparison. The essential fact here is that United States 'negroes' score on the average one standard deviation below the United States white mean in IQ tests.

Three questions can be asked about the causation of this difference. Is the difference (in whole or part) a result of gene frequency differences? Is the difference a result of environmental (including cultural) differences in factors that also vary within the groups? Is the difference a result of some environmental-cultural factor exclusive to the United States 'negro' population?

I shall consider the second question first. It is, of course, clear that there are within-group environmental variables that affect IQ. Otherwise within-group heritability would be 100%, and the correlation between identical twins would be 1. That some of this non-heritable variance is associated with socioeconomic group is strongly indicated by Skodak and Skel's study which showed that adopted children from

mothers of low IQ had higher mean IQs than expected on a purely genetic model. Jensen deals with this in a note to chapter 11 (page 241).

The negro population is, of course, low in mean socio-economic status (SES), so that it is reasonable to postulate this as an environmental factor accounting for some of the group difference. Attempts to equate for SES have, however, failed to account for much if any of the difference between white and negro social groups. Likewise, though extreme malnutrition can lower IQ, attempts to investigate effects of nutrition in the United States show negative results (malnutrition at levels found, say, in Africa is, of course, irrelevant to an assessment of the United States negro-white difference). Likewise, most of the other often postulated environmental factors, motivation, reaction to race of tester, language deprivation, inequalities of schooling, culture loading of tests, seem unable to account for the difference. Indeed, when tests which are regarded as differing on culture loading are used, quite the opposite result than that predicted on the culture loading hypothesis is obtained. The white-negro difference is least on the most culture loaded tests. Furthermore, other 'racial' minority groups have higher average IQ than United States negroes, some higher than United States whites. Having considered the evidence, Jensen regards the evidence relating to my second question as quite inadequate to account for the negro-white difference in the United States.

This brings me to the third question, which is, as Jensen puts it, to postulate an unknown factor X. This has to have certain negative features if it is to fulfil its postulated function, for it must be a factor that does not affect minority groups other than negroes, for these do not show the low IQ mean that the 'negro' social group does, nor can the factor be one that affects 'deprived' whites. It has to be exclusive to 'negroes'.

It needs, however, to be pointed out that there are cultural factors exclusive to this negro social group, such as awareness of negro or slave ancestry and awareness of real or supposed social attitudes to 'negroes', and it is not reasonable to discount these simply because they present hypotheses difficult to test, and because some other hypothesis fits the data. Jensen seems to do this.

For example, in arguing for the plausibility of the genetic hypothesis Jensen puts considerable stress on the argument from regression. This argument goes as follows. The 'white' social group has a mean IQ of about 100, variance about 15<sup>2</sup> and heritability 80%: the 'negro' social group has a mean one standard deviation lower, about 85. Then if we take a sample of whites of IQ about 120, an unbiased sample of their sibs must regress to the mean IQ of 100, that is, genetic theory requires that the sib mean should be about  $100 + 20 \times 0.5 \times 0.8$  or 108. (20 is the deviation from the mean, 0.5 the genetic correlation between sibs, 0.8 the heritability.) This it does, which is not surprising since it is part of the evidence that heritability is 80%. By contrast the sibs of a sample of negroes of IQ 120 should regress to a mean IQ of 85, that is, assuming that heritability within the negro social group is also 80% then it should be  $85 + 35 \times 0.5 \times 0.8$ , or 99. By the same argument the sibs of whites of IQ 80 should have a mean of 92, but those of negroes of IQ 80, only 83. Jensen claims that these expectations are found in fact; in other words, the data fit genetic predictions. (He does not quote the data, but after correspondence with him I am satisfied that the results are available and do not seem to involve biases such as might have permitted alternative explanations.) Jensen therefore regards the evidence as supporting the genetic hypothesis.

For some time I fell into the same trap. But it is a trap, for populations must regress to their own mean whatever the cause, genetic or environmental, of the mean differences between the populations. This evidence is therefore as compatible with explanation in terms of the environmental factor

X as in terms of the genetic hypothesis. It adds nothing whatsoever to the strength of the genetic hypothesis. All these results reveal is that heritability within the negro group is of the same order as that within the white group.

I do not think this a culpable error on Jensen's part. So much of the argument concerning environmental factors is open to criticism, and he has been subject to such virulent attack (see the documentation in the long preface to his *Genetics and Education*<sup>3</sup>), that it must be wellnigh impossible for him to criticise the evidence relating to genetic hypotheses as closely and objectively as he has that concerning environmental factors.

## Summarised Data

But another example is less pardonable. Jensen, Eysenck and also Shockley among others have each proposed that evidence might be obtainable from inter-group hybrids and from studies of correlations, among 'negroes', between IQ and amount of white ancestry as estimated from marker genes. Jensen is well aware of the difficulties such studies must present, notably with respect to non-randomness of those involved in inter-group matings (see his chapter 9) and he deals in a different chapter with the one piece of evidence of this kind that he seems to think good, giving it much less stress than Eysenck did in his book *Race, Intelligence and Education*<sup>4</sup>. It is, however, a piece of evidence that illustrates well the difficulties and extreme dangers of reliance on summarised data.

This is the evidence of De Lemos<sup>5</sup> on Australian aborigines, and it concerns Piagetian tests whose results correlate strongly with those of culture-reduced IQ tests. De Lemos gave Piagetian conservation tests to aboriginal children who were divided into two groups, according to Mission records of their ancestry, as full and part aborigines. Most of the part aborigines had, it seems, one white greatgrandparent. Jensen reproduces the data direct from page 316 of De Lemos's paper<sup>5</sup> (see Table 1), and in this form it looks convincing.

**Table 1** Table 17.1 of Jensen's Book<sup>1</sup>. (Comparison of the Number of Part-Aboriginal and Full-Aboriginal Children showing Conservation\*)

Test	Full Aboriginal N=38	Part Aboriginal N=34	$\chi^2$	P
Quantity	4	18	15.21	<0.001
Weight	16	25	7.23	<0.01
Volume	2	8	3.59	0.05 < P < 0.10
Length	12	20	5.37	<0.05
Area	3	10	4.23	<0.05
Number	3	9	3.22	0.05 < P < 0.10
Total ‡	40	90	36.14	<0.001

\* From De Lemos<sup>5</sup>.

‡ The chi square test for Total (given by De Lemos) is statistically inappropriate here, since pooling more than one observation from the same subject violates the requirement of independence of observations upon which the chi square test depends.

But Jensen had already presented these data before<sup>6</sup> in a different form taken from De Lemos's PhD thesis<sup>7</sup> as shown in Table 2.

Not only are the significance levels now much less impressive, though total sample size is for some reason rather larger, but one immediately sees that Table 1 confounds an age difference with the ancestry difference, whereas Table 2 shows age to be, as of course it must, most important in relation to these tests. One is left asking whether age differences within the 8 to 11 and within the 12 to 15 year groups might account for the significant results left in Table 2. Examination of De Lemos's original paper shows that age

differences are not adequately controlled, and that the data cannot be regarded as demonstrating that the ancestry difference has significant effects. It turns out that this conclusion has already been published by Vetta<sup>8</sup>, who also refers to a replication study by Dasen, which had negative results. These papers came out too late for Jensen to refer to them, for his bibliography contains nothing later than 1971. But he criticises others in other contexts for failing to equate for age, and he really should have seen the implications of the difference between these two Tables reproduced from his own publications.

I have considered this example in detail because it illustrates the difficulties one is faced with in judging conclusions in this area. Jensen has, often cogently, criticised evidence purporting to support relation between IQ and various environmental factors. Some of his own arguments are open to comparable criticism. The treatment of De Lemos's data and the consideration of regressions suggest that Jensen suffers from the same kind of conscious or unconscious bias as many of his opponents, that is to say he is prepared to accept evidence that seems to support his hypothesis with less critical examination than he would give to evidence purporting to be against him.

## Lessons

One is left therefore, despite all the literature, with these conclusions. First, there is no evidence which reveals whether the negro-white IQ difference has any genetic component or any environmental component. Both hypotheses (and any intermediate hypothesis) are equally consistent with the facts. (There is one exception, United States negro women score higher than United States negro men, a difference for which no genetic model seems to fit the facts.

**Table 2** Table 1 from Ref. 6. (Numbers of Full-Blood and Part-Blood Australian Aboriginal Children Passing Piagetian Conservation Tests and the Significance Level (*P*) of the Difference\*.)

Total <i>N</i> =	Age 8 to 11 years			Age 12 to 15 years		
	Full 25	Part 17	<i>P</i>	Full 17	Part 21	<i>P</i>
Tests						
Quantity	2	6	<0.1	2	15	<0.01
Weight	9	11	<0.1	7	17	<0.01
Volume	0	5	<0.05	2	4	N.S.
Length	10	10	N.S.	3	13	<0.05
Number	0	4	<0.05	3	8	N.S.
Area	1	4	N.S.	2	8	N.S.

\* Source: De Lemos<sup>7</sup>.

Jensen himself has elsewhere<sup>9</sup> put the difference down to environmental factors of the nature of 'X', but does not mention the matter in this book. It should account for at least one tenth of the United States negro-white difference.)

Second, no statement about causation of IQ variation should be taken at its face value, whoever the authority. Every statement requires most careful consideration of the detailed data on which it is based.

Third, the more we would like to believe some statement about the causation of IQ variations, the more closely should we examine the data and logic behind it.

If we learn these lessons, Jensen may have done us a service. He is perfectly correct in maintaining that the hypothesis that group differences in IQ (or anything else) have a genetic component is plausible and that there is no evidence against it. This means that no evidence which purports to demonstrate the relevance of some environmental component can be regarded as adequate unless it be demonstrated from the same evidence that the groups being compared could not differ genetically. It follows also that we have no reason to expect that different groups will have the same distribution of attributes and that demonstrations

that different groups have different frequencies of success, for example in some educational selection process, cannot be regarded as good evidence that that selection process does not provide equality of opportunity for the individuals concerned. These are important lessons that all—including editors—should bear in mind and try to act on.

But I believe there is a risk that Jensen may have also done us a disservice, for the controversy about group differences is proving a stick to beat him with, to the detriment of rational discussion of individual variation and the importance of the genetic component of individual variation in attributes that correlate with IQ.

There is now a danger that the controversy about group differences may lead the evidence about individual differences to be swept under the carpet. But our relatively recent knowledge concerning the ubiquity of genetic variation is of the utmost scientific, political, and philosophical importance. Variation in IQ, and variation in the related aspects of educability, are but one example. Let us remember that, to assume that IQ variation is of no moment, or to assume that it does not have a substantial genetic component, not only flies in the face of the facts, but puts us in a position where it can be held that the state through the educational system may make what it will of anyone. In truth, individuals, however malleable, are different and should not be treated as if they were the same. Controversy about the causation of group differences must not lead us to ignore this, and difficulties that arise because groups differ in the frequency with which they meet certain educational criteria must not lead to our assuming that everyone is able to meet the same educational criteria. It is not true that everyone can reach the same academic standards if provided with adequate opportunity, and the heritability of IQ is a partial measure of that untruth. Equality of educational achievement must prove an unrealisable ideal. Those who raise the hope that it is realisable must bear responsibility for the resulting widespread individual disappointment and all its consequences.

Our societies need to be organised to allow for individual differences, including such differences in aspects of educability. Indeed, if our societies were so organised we might perhaps slowly come to realise that it does not necessarily matter if group differences prove to have a genetic component, for the genetic variation within groups and the overlap between groups are more important and no demonstration that a group difference has a genetic component could justify 'racism'. Perhaps also if we spend less effort considering group differences and more on individual differences and their genetic component we may become more capable of reasonable discussion of the implications of this individuality, the ways in which society should accommodate it, and the extent to which any particular dimension of individuality, such as IQ and its associates, may or may not be overstressed in our present system of status and economic rewards.

I thank Dr J. B. Gibson for drawing my attention to the difference between Tables 1 and 2.

<sup>1</sup> Jensen, A. R., *Educability and Group Differences*, pp. xiii+407 (Methuen, London, June 1973), £3.90.

<sup>2</sup> Jensen, A. R., *Harvard Educational Review*, 39, 1 (1969).

<sup>3</sup> Jensen, A. R., *Genetics and Education* (Methuen, London, 1972).

<sup>4</sup> Eysenck, H. J., *Race, Intelligence and Education* (Temple-Smith, London, 1971).

<sup>5</sup> De Lemos, M. M., *Int. J. Psychol.*, 4, 255 (1969).

<sup>6</sup> Jensen, A. R., *Harvard Education Review Reprint Series*, No. 2, 211 (1969).

<sup>7</sup> De Lemos, M. M., thesis Australian National Univ. (1966).

<sup>8</sup> Vetta, A., *Int. J. Psychol.*, 7, 247 (1972).

<sup>9</sup> Jensen, A. R., in *Contributions to Intelligence* (edit. by Cancro, R.) (Grune and Stratton, New York, 1971).