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Interpretation of Heritability

Anastasi's (1971) comments on heritability helped to clarify this concept, but unfortunately her summarizing paragraph tends to obscure and unduly restrict the interpretation of heritability. Let me try to point out where I think Anastasi's concluding comments might lead the reader astray.

To repeat, heritability (in the broad sense, which means that all of the components of genetic variance are included) is the proportion of population variance in a trait that is attributable to genetic factors. In short, it is a ratio of genetic variance to total variance. Like any statistic, it is subject to sampling error, and it cannot be generalized properly to populations or to conditions that were not represented in the sample estimate of heritability. Also, as I have pointed out before, high heritability does not necessarily mean immutability of the characteristic in question (Jensen, 1969, p. 45). Finally, the finding of high heritability of a trait *within* each of two populations that differ in average phenotypic value does not by itself prove that the mean group difference has a genetic component. In other words, heritability of individual differences within groups does not prove heritability of the average difference between groups. On all of these points, it appears that Anastasi and I are in complete agreement.

Now for the points of disagreement: Anastasi wrote,

> Available heritability data do not provide a proper answer to such questions as [*a*] the etiology of an individual's handicaps, [*b*] the origin of ethnic differences in test performance, or [*c*] the anticipated benefits of compensatory education or other programs of environmental intervention [p. 1037].

This quite sweeping statement would seem to diminish incorrectly the meaning of heritability unless we try to be much more precise about each one if its points.

(*a*) Just as the square root of a test's reliability coefficient tells the correlation between obtained scores and true scores, so the square root of a test's heritability tells the correlation between obtained scores (i.e., the phenotypes) and "genetic values" (i.e., genotypes) on the trait being measured. ("Value" refers here to a scaled quantity; it implies no "value judgment.") Without an absolute scale (as is the case for practically all psychological measurements), these values must be expressed merely as deviation scores, that is, as deviations from a population mean. For the "genetic value" to have any valid meaning, it must be expressed (and interpreted) as a deviation from the mean of the population in which the heritability was estimated and also in which the individual in question is a member. Given these conditions, one can determine the standard error of a test score's "genetic value," analogous to the standard error of measurement.[1] It is simply

$$SE_G = SD\sqrt{1 - h^2},$$

where SE_G is the standard error of the genetic value, SD is the standard deviation of the test scores, and h^2 is the heritability (not corrected for attenuation due to test unreliability). For IQ, assuming $SD = 15$ and $h^2 = .75$, the standard error of the genetic value is 7.5 IQ points. This can be interpreted the same as the standard error of measurement. It means that 68% of our estimates of an individual's genetic values will differ less than 7.5 points from this phenotypic IQ, 95% will differ less than 15 (i.e., 2 SE_Gs), and 99.7% will differ less than 22.5 points (3 SE_Gs). In

[1] The analogy is not perfect, however, since true scores and measurement errors are by definition uncorrelated, while genetic (G) and environmental (E) components may be correlated. But this is a soluble problem. The covariance of G and E can be estimated independently and may or may not be included in the estimates of h^2, depending on the interpretation one wishes to give to h^2. Roberts (1967) has suggested that the environment should be defined as affecting the phenotype independently of the genotype. Thus, if individuals' genotypes influence their choice of environments, the environmental variation resulting therefrom would be considered a part of the total genetic variance.

other words, the probability is very small that two individuals whose IQs differ by, say, 20 or more points have the same genotypes for intelligence or that the one with the lower IQ has the higher genetic value. The individual's estimated genetic value, $\hat{G}_i$, expressed as a deviation score, is $\hat{G}_i = h^2 \, (P_i - \bar{P}_p) + \bar{P}_p$, where P_i is the individual's phenotypic measurement (e.g., IQ), and $\bar{P}_p$ is the population mean. Thus, it is clear that, contrary to Anastasi's assertion, one logically *can* give a probabilistic answer to such questions as "the etiology of an individual's handicaps." The statement that an individual's test score is within, say $\pm x$ points of his "true score" is no less probabilistic than saying his score is within $\pm x$ points of his "genetic value."

(*b*) While it is true that heritability *within* groups cannot *prove* heritability *between* group means, high within-group heritability does increase the a priori likelihood that the between-groups heritability is greater than zero. In nature, characteristics that vary genetically *among* individuals within a population also generally vary genetically *between* different breeding populations of the same species. Among the genetically conditioned traits known to vary between major racial groups are body size and proportions; cranial size and cephalic index; pigmentation of the hair, skin, and eyes; hair form and distribution on the body; number of vertebrae; fingerprints; bone density; basic metabolic rate; sweating; fissural patterns on the chewing surfaces of the teeth; numerous blood groups; various chronic diseases; frequency of dizygotic (but not monozygotic) twinning; male/female birth ratio; ability to taste phenylthiocarbomide; length of gestation period; and degree of physical maturity at birth (as indicated by degree of ossification of cartilage). In light of all of these differences, Spuhler and Lindzey (1967) remarked,

> It seems to us surprising that one would accept present findings in regard to the existence of genetic anatomical, physiological, and epidemiological differences between the races . . . and still expect to find *no* meaningful differences in behavior between races [p. 413].

The high within-groups heritability of certain behavioral traits, such as intelligence, adds weight to this statement by Spuhler and Lindzey. Recently, John C. DeFries (in press), professor of genetics at the University of Colorado, worked out the mathematical relationship between heritability within groups and between groups. His formulation has been concurred in by other quantitative and behavioral geneticists.[2, 3] Though it would take too much space to explicate here, what it shows essentially is that unless there is absolutely *no* genetic difference whatever between two populations on the trait in question, there is a definite increasing monotonic relationship between the magnitude of within-groups heritability and between-groups heritability. Therefore, strictly speaking, it is incorrect to claim that there is no relationship whatever between within-groups and between-groups heritability.

Incidentally, although at the time I wrote my article (Jensen, 1969) it was true that there were no even remotely satisfactory estimates of the heritability of IQ in a Negro population, this is no longer the case; there are now two studies (Nichols, 1970; Scarr-Salapatek, 1971).

(*c*) It is also mistaken to argue that heritability has no implications for the probable effects of environmental intervention. Since $1 - h_c^2$ (h_c^2 is h^2 corrected for attenuation) is the proportion of trait variance attributable to environmental factors, the square root of this value times the standard deviation of the "true score" trait measurement gives the standard deviation of the effect of existing environmental variations on the particular trait. For IQ, this is about six points; that is to say, a shift of one standard deviation in the sum total of whatever nongenetic influences contribute to environmental variance (i.e., $1 - h_c^2$) will shift the IQ about six points. (There is good evidence that environmental effects on IQ are distributed normally, at least in Caucasian populations [Jensen, 1970, 1971].) Thus, the magnitude of change in a trait effected by changing the allocation of the existing environmental sources of variance in that trait is related logically to its heritability. This applies, of course, only to existing sources of environmental variance in the population, which is all that can be estimated by $1 - h_c^2$. It can have no relevance to speculations about as yet nonexistent environmental influences or entirely new combinations of already existing environmental factors. With respect to IQ, I believe Bereiter (1970) stated the situation quite correctly:

> What a high heritability ratio implies, therefore, is that changes within the existing range of environmental conditions can have substantial effects on the mean level of IQ in the population but they are unlikely to have much effect on the spread of individual differences in IQ within that population. If one is concerned with relative standing of individuals within the population, the prospects for doing anything about this through existing educational means are thus not good. Even with a massive redistribution of environmental conditions, one would expect to find

[2] Since this was written, the author has learned that this formulation of the relation between within-groups and between-groups heritability was published by Jay L. Lush (1968, p. 312), one of the pioneers in quantitative genetics.

[3] The author is grateful to James F. Crow, John C. DeFries, Everett R. Dempster, and Jay L. Lush for their critical comments on his first draft.

the lower quarter of the IQ distribution to be about as far removed from the upper quarter as before [p. 288].

Bereiter went on to say:

A high heritability ratio for IQ should not discourage people from pursuing environmental improvement in education or any other area. The potential effects on IQ are great, although it still remains to discover the environmental variables capable of producing these effects.

Whether such specific environmental variables having major effects on IQ are or are not discovered in the immediate future, humane persons surely will agree that environmental conditions for the nation's poor should be improved by all possible means.

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Interpretation of Heritability: A Rejoinder

After reading Jensen's (1972) latest discussion of heritability, I would repeat all of the points I made in my earlier comment (Anastasi, 1971). With a lingering hope of further clarifying communication, I shall add just three points. First, it was not the statistical procedures but the empirical data to which they were applied that led me to question Jensen's original conclusions. Second, the probabilistic argument, to which Jensen's present comment addresses itself in large part, is of primary interest when no other information is available regarding the origins of particular differences between individuals or groups. In Hebb's (1970) example of Mark Twain's boys-in-barrels, as in the case of several minority groups, we do have information regarding environmental sources of interpopulation differences. To draw conclusions regarding such group differences from probabilities estimated from intragroup heritability ratios is logically equivalent to diagnosing a child's brain damage in terms of the base rate, with no attempt to obtain a case history or other pertinent data about the individual. Finally, the sort of environmental interventions generally considered in relation to minority group status do not represent simply a reshuffling of environmental variations already existing within such groups (or within *other* groups). Rather what is envisaged are massive changes in the physical or psychological environments of a population as a whole.

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Comparative Psychology: Does It Exist?

Lockard (1971) argued that comparative psychologists have caused the demise of their discipline in part by restricting their study to a few unrepresentative species and by accepting a set of untenable premises about animal behavior. Lockard argued that behavioral biology (whose basis is ethology) has replaced comparative psychology.

The untenable premises listed by Lockard as accepted by comparative psychologists are in part exaggerations and in part false. For example, few comparative psychologists would accept his sixth premise that genetics and evolution are irrelevant to animal behavior. Furthermore, some of the behavioral biologists whom Lockard heralds are guilty of similarly extreme and untenable statements. Lockard's only theoretically sound criticism stems from the Hodos