

The problem is a psychological and cultural one, not a technological one (Hardin, 1968). The psychocultural problems, along with the technological ones, will, I am sure, be solved in the fairly near future for the simple reason that "the establishment" now realizes the dangers. Aldous Huxley (1952) wrote an essay some years ago in which he described how sewage systems were installed for the poor in British cities when the wealthy understood the connection between disease, plumbing, servants, and their own health. (Incidentally, Huxley was concerned with overpopulation and pollution even then.) When "the establishment" realizes that anything is a threat to its members' existence or comfort, something will be done about it. This is why I am not much worried about pollution. The bad air in this country has already begun to make our Congressmen and wealthy people cough!

Finally, for the reasons given above, relative to other problems overpopulation is not extremely important. War is an extremely important problem; an atomic war really could destroy us at almost any time. The problem of human aggression is by far the major problem facing mankind today, and psychology is one of the major fields that can and should study its solution. Here, then, is where psychology should place its major emphasis. Encouragingly, this has already begun.

Of course, peace is not controversial, but the issues directly related to obtaining it are, such as the effect of nationalism, militarism, capitalism, and the new colonialism upon the international tendency toward war. It is these institutions and the attitudes that support them that are controversial, that are of primary importance, and that are well within the province of psychology (as well as other human sciences). These are the issues toward which concerned psychologists should direct their energies. I am afraid these issues, too, may receive the same response from psychologists generally as the response to Bartz's concern. Perhaps this apathy should be considered a major psychological problem for Bartz and other psychologists. We must discover ways to influence psychologists and the rest of mankind to become concerned about these problems.

As a final comment, in spite of Bartz's attack on self-actualization, it should be noted that "self-actualized" people are usually far more concerned than is the average person with these social problems. Perhaps we should devote our time to discovering methods to enable people to be more self-actualizing.

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Concerning Hebb's Criticism of Jensen and the Heredity-Environment Argument

In a recent letter, Hebb (1970) has attempted to demonstrate, with reference to the work of Jensen (1969) and his critics, that the amount of variance attributable to heredity—or to environment—"cannot show how important heredity (or environment) is in determining an aspect of behavior." Hebb employs as an example a situation adapted from a humorous proposal by Mark Twain, namely, that boys be raised in barrels to age 12, and fed through the bung-hole. Hebb asks us to assume 100 boys were raised in this manner, thus guaranteeing practically identical environments for all. Since, Hebb argues, Jensen allows that environment has *some* importance, the boys emerging after 12 years in the barrels should have a mean IQ well below 100. Hebb concludes, "However, the variance attributable to the environment is practically zero, so on the 'analysis of variance' argument, the environment is not a factor in the low level of IQ, which is nonsense."

However, Jensen's argument concerning the proportion of individual difference variation is a relative one, not an absolute one. It is relative to the white population and to that population's range of environments in recent history. This range of environments, it should be clear, is certainly not representative of all conceivable environments, but it does embrace the principal environment that we know, and the environment with which we are mainly concerned. The question of how much variation between individuals in IQ is explainable by the variation between the same individuals in environments is especially important when the environments in question span the only ones that we know. No one—and least of all Jensen—pretends that the environmental proportion of variation in IQ would be the same even if environmental variation were reduced or expanded.

If we were all raised in barrels, we would have no reference point that would enable us to note that these conditions were affecting our IQs. In Hebb's argument, there is a subtle shift in reference points that

enables him to be aware of the effect of being reared in barrels, since he uses the between-barrels variation to argue that environment has no apparent effect, but demonstrates this to be nonsense by invoking the variation in IQ between barrels and the outside world.

The only tenable absolute form of the argument, which Hebb wrongly attributes to Jensen, is actually rather uninteresting. It asks whether environment matters at all, and it would be worth pursuing only if environment had not as yet been known to affect IQ. In this case, scientists would have to attempt to create sources of variation in environments greater than any then known in order to discover whether any at all mattered. In the final analysis, the proposition "Environment does not affect IQ" could only be disproved, but never proven. In the narrow world of barrel-rearing, it would be true that environmental variation was practically nonexistent, and the scientists would have to find some set of artificial conditions to disprove the above proposition. In our own world, we have already disproved it, and so we have turned to the more interesting question of how much of an effect given environments have. This is the question Jensen was addressing.

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On Hebb's Criticism of Jensen

In a recent issue of the *American Psychologist*, Hebb (1970) gives an example in which he tries to show that the amount of variance explained by a particular variable is not to be equated with the importance of that variable in determining an aspect of behavior. Some question remains, however, as to the validity of the criticism, which, if correct, nullifies much if not all of the implications to be derived from Jensen's highly controversial article concerning the heritability of intelligence.

Hebb says that if 100 boys were reared in a barrel they would probably show a lowered IQ, but since they have all experienced exactly the same environment, the amount of variance that can be attributed to the environmental influence on the phenotypic scores of IQ will be zero although, he argues, it is nonsense to say that the environment had no effect on lowering the IQ of the boys. In order to make the above statement, it is not enough to say that the IQ will be

lowered, since one typically needs a control group with which to compare the group reared in a barrel. One must distinguish between the variance within a particular group as opposed to the variance between groups. In order to say that the IQ was lowered by the environment, one would need a control group reared under different uniform environmental conditions. Given the above situation, the variance within each group would be wholly attributed to genetic factors since the environment is constant for the two groups (although different between the two groups). However, in order to make any statement concerning the effect of the environment, one would have to compare the between-group variance to the within-group variance to see if there is any effect.

Does Jensen (1969) in fact use this kind of between-group analysis? In his article he states, for example, that the correlation between monozygotic twins reared apart is .75, thereby showing that 75% of the variance of IQ is accounted for by genetic factors. The reader may wonder why this correlation is not squared, since the coefficient of determination is usually thought of as the amount of explainable variance. However, Jensen has switched to the intraclass correlation for his analysis, and therefore the percentage of variance accounted for is correct. Since he is using the intraclass rather than the interclass correlation, one must therefore question whether there is any between-group comparison that would allow him to say anything about the effect of environment. In the intraclass correlation, the between-groups variance is between the pairs of twins reared apart, while the variance within is the variance within each pair of twins summed over all the pairs. The between-groups variance is therefore not between groups with different but homogeneous environments as in the preceding example, but rather between groups all receiving the same "treatment," as it were. Since this is the case, the amount of variance accounted for is exactly the "within a single treatment variance" that Hebb has correctly identified as being unrelated to the importance that a variable may have in determining an aspect of behavior.

The use of explainable variance without recourse to a between-groups analysis is dangerous and misleading. Let us take a hypothetical example involving environment (E) and intelligence (I). Let us assume that the correlation between these two variables is $r_{EI} = .90$. We can therefore say that 81% of the variance of intelligence is explainable by the variable of environment. Let us "enrich" everyone's environment equally so that we are in effect adding a constant to the environment (let $E' = E + A$). Furthermore, let us assume that intelligence goes up equally for everyone so that $I' = I + b$. If we now correlate E' with I' , the correlation will be .90, since the correlation co-