however, responses to S− are extinguished during discrimination training, S− may function as an aversive stimulus. According to this hypothesis, a shift of the peak or of the area of a generalization gradient, away from S−, would be described as a shift away from an aversive stimulus.

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References and Notes

1. H. M. Hanson, *Science* **125**, 888 (1957); *J. Exptl. Psychol.* **58**, 321 (1959).
2. H. S. Terrace, *J. Exptl. Anal. Behavior* **6**, 1 (1963); *ibid.*, p 223.
3. C. B. Ferster and B. F. Skinner, *Schedules for Reinforcement* (Appleton, New York, 1958).
4. D. B. Blough, *J. Opt. Soc. Am.* **47**, 827, (1957). The values of the neutral density filters were computed as follows. On the basis of Blough's table of photopic sensitivity, a zero increment in density was assigned to the stimulus at which the least sensitivity was obtained (440 mμ) and the largest increment was assigned to the stimulus at which the most sensitivity was obtained (560 mμ). These values were then corrected for the different transmittances of each monochromatic filter. The decrements in intensity at each stimulus were: 490 mμ, −0.3; 510 mμ, −0.5; 520 mμ, −0.6; 530 mμ, −0.6; 540 mμ, −0.8; 550 mμ, −0.8; 560mμ, −0.9; 570 mμ, −1.0; 580 mμ, −1.0; 590 mμ, −0.9; 600 mμ, −0.8; 610 mμ, −0.7; 630 mμ, −0.5; 650 mμ, −0.1; 670 mμ, 0.0.
5. It should be noted that the asymmetrical distribution of area above and below S+ is not due to the fact that a different number of stimuli above and below S+ were used in the generalization test (eight below and six above S+). Asymmetrical distributions of area would also be obtained if the number of responses to the two lowest stimuli (490 and 510 mμ) were omitted from the calculation of area.
6. H. S. Terrace, *Science* **140**, 318 (1963).
7. I thank D. B. Moody and E. Richardson for their valuable assistance. This research was supported by NIH grant MH-05770-02 and NSF grant GB-1629.

3 February 1964

I.Q., Genetics, and Culture

I was rather surprised to read in *Science* (13 Dec. 1963, p. 1477) a report entitled "Genetics and intelligence: A review," by L. Erlenmeyer-Kimling and L. F. Jarvik, purporting to show that "Individual differences in behavioral *potential* reflect genotypic differences; individual differences in behavioral *performance* result from the nonuniform recording of environmental stimuli by intrinsically nonuniform organisms" (italics in original). Whatever the truth of the report's thesis, if any, it cannot be supported by the type of correlation data presented.

In the first place, the nature of environmental influences on intellectual development have by no means been elucidated, and it does not follow that individuals reared together were subjected to similar effective environmental influences or individuals reared apart to dissimilar ones. At the risk of overstating the obvious, I give two examples of the difficulty: rival siblings may be exposed to very different environments though reared in the same home and surroundings, and placing a Negro child in the home of a white foster parent will not make the environment for that child similar to that of his foster brothers and sisters. In the second example, the differences will arise in part from skin color, which is genetically determined, but will be caused by the cultural implications of that color, not by genetic limitations associated with it.

Secondly, there is a long and unsettled controversy over how intellectual *potential* is to be measured. I personally suspect that I.Q. and other tests measure to a considerable degree the extent of cultural (environmental?) conformity between those who construct the tests and those who take them. An intelligent Eskimo would fail I.Q. tests, but I suspect that Erlenmeyer-Kimling and Jarvik would fail to survive an Arctic winter. Since a reliable, independent measure of intellectual potential does not exist, the matter cannot be settled. However, to me the pertinent experiments are those which demonstrate that performance on I.Q. tests is altered by changes in environment.

In the same issue (p. 1436) appears a confusing long article with a similar thesis by J. Hirsch, who sets the physiologists and the behaviorists in a windmill which he labels "believe in the initial uniformity of individuals" and then charges them pell-mell with the lance of genotypic uniqueness. He then attacks "reductionism," the fallacy of which he states to be the assumption of a "one-one relation between different levels of organization," and on the next page discusses the one-to-one relation between genes and behaviour.

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If Daniel meant to say that our data do not establish our hypothesis, then he is, of course, correct; no quantity of data ever established any hypothesis. If he really meant to say that the data do not support our hypothesis, then we can only refer him and other readers to our report and chart.

Daniel also points out that rival siblings may be exposed to very different environments though reared together. Even if this were true for relevant environmental variables, the data still support our hypothesis.

We should like to reiterate the concluding paragraph of our report, in which the important concept of the "norm of reaction" is briefly discussed: "We do not imply that environment is without effect upon intellectual functioning; the intellectual level is *not* [italics in original] unalterably fixed by the genetic constitution." Just as in the example of phenylketonuria cited in the same paragraph, alterations in performance on intelligence tests following changes in environmental stimulation illustrate the concept of the "norm of reaction."

Incidentally, neither an Eskimo nor anyone else, intelligent or unintelligent, could "fail I.Q. tests."

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24 February 1964

Temperature of Metallic Objects in Space

The report by C. Butler and R. Jenkins (*1*) on "Temperature of an iron meteoroid in space" shows an application of thermodynamic theory similar to that used some 6 years ago to predict the solar heating of artificial satellites (*2*). Their report generally agrees with the theory (later confirmed by actual measurements on satellites) thus previously developed for temperatures of a solid body in space and in full sunlight. However, they have neglected the factor, for bodies near the earth, of the shadow of the earth. Consideration of this neglected factor would seem to modify very seriously their categorical conclusions that "the equilibrium temperature of an iron meteoroid just before entering the earth's atmosphere will be close to 90°C," and that any assumptions that meteoroids are "quite cold" just before entering the atmosphere necessarily contradict thermodynamic theory.

If a meteoroid enters the earth's atmosphere at night (and such is the