Cross-Cultural Differences

In their paper "Cultural differences in the perception of geometric illusions" [*Science* **139**, 769 (22 Feb. 1963)] Segall *et al.* state that an "inference habit . . . could enhance, or even produce, the Müller-Lyer and Sander Parallelogram illusions." I would like to register some objections.

In their own data, if I read them correctly, children are more susceptible to these illusions than adults. Other investigators have found this to be the case with the Müller-Lyer illusion. If these illusions are due to ". . . habits of perceptual inference which relate to cultural . . . factors . . ." it is difficult to understand why adults, who are presumably more acculturized, have smaller illusions.

Winslow [*Arch. Psychol.* No. 153, 1 (1933)] has reported that chicks are susceptible to the Müller-Lyer illusion. It is inconceivable that chicks succumbed so rapidly to our Western cultural influences. It is just as possible that the Müller-Lyer and Sander illusions are induced by natural electrochemical brain processes propagated by the contours of the figure and that there are physiological differences between the groups. The horizontal-vertical illusion might be induced by some other physiological process or may in fact represent cultural differences. Piaget's centration effect may play a larger role in this illusion, and the groups may differ, for whatever reason, in the time spent looking at the vertical bar.

This is not to say that one or another interpretation is the correct one, but only that the "cultural inference" interpretation runs into difficulties, and that alternative nonenvironmental explanations were unstated.

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Spitz objects on several grounds to the empiricistic interpretation of cross-cultural differences reported in our paper. In so doing, he notes correctly some complexities in our data, thus underlining the magnitude of the research program required before we can claim fully to understand the phenomena in question. In particular, Spitz is correct in pointing out that age trends in studies with the Müller-Lyer figures complicate our interpretive efforts. Existing data show children to be more susceptible than adults, and our line of reasoning demands that somewhere during the life span there must be an increase in susceptibility with age. Research has failed to uncover such an increase, but in no studies reported have children under 3 years of age been used as subjects. We believe that the visual inference habit we assume to be involved in the Müller-Lyer illusion (and others) is well established by the time hand-eye coordination and locomotive skills are achieved (before 3 years of age). It is also assumed that the degree of illusion susceptibility established by that age is a maximum, with the subsequent reduction resulting from an increase in analytic ability, particularly as the individual learns to inhibit form-constancy effects in drawing three-dimensional objects on two-dimensional surfaces. These hypotheses are to be tested in laboratory studies with young children at the University of Iowa Psychology Laboratory, but until we have data to support them, Spitz's point on age trends is well taken.

His other substantive point that chicks are susceptible to the Müller-Lyer illusion, and that this fact throws our interpretive position into serious doubt, is less well taken. In the paper cited by Spitz, Winslow did not demonstrate that chicks are susceptible to the Müller-Lyer illusion. His study involved training five chicks to choose the shorter of each of five pairs of stimuli, and then they were tested on eight Müller-Lyer figures. Winslow reasoned correctly that the chicks having been trained to choose the short member of stimulus pairs would, if susceptible to the illusion, tend to select the usually underestimated portion (the enclosed segment) of the test figures. However, his conclusion that the chicks were susceptible was, in our view, erroneous, because only two of the eight test figures provided appropriate tests of the illusion and on both of these the response tendency clearly was to choose the typically overestimated segment. The chicks tended to choose the usually underestimated segment only when it and its surround was actually shorter than the other segment and its surround. Thus, Winslow's study demonstrated at most that chicks could be taught a discriminative response habit which could transfer to a second set of stimuli.

Even if illusion susceptibility in chicks had been demonstrated, our interpretation of illusion-responses as incidental manifestations of usually valid visual inference habits would not be undermined. One could extend our general argument to other animals who could conceivably learn habits that are ecologically valid for them in a manner analogous to that we hypothesize for humans. Our thesis is simply that various features of the visual environment affect the probability that certain inference habits rather than others will be learned and that these habits will, under certain unusual conditions, lead to nonveridical perceptions. Stimulus configurations like those we have come to call geometric illusions are examples of such unusual conditions.

It is of course still conceivable that physiological processes, perhaps due to hereditary or dietary factors, may explain our data. However, even genetic explanations these days require analysis of the environmental factors which through differential selection contribute to genetic variation over time. We nonetheless accept as fair Spitz's comment that we did not state alternative nonenvironmental explanations. We would welcome attempts to spell out differences in "natural electrochemical brain processes" or other such factors which could account for the complex, bi-directional differences we reported.

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Independent Research Institutes

I am writing to express my concern over your recent editorial "Unnecessary research institutes" [*Science* **139**, 563 (15 Feb. 1963)]. Although I respect your right to "editorialize" as you see fit, I do feel that you used a "shotgun" approach. In an attempt to point out real abuses, you have (unwittingly I am sure) stigmatized a number of respectable institutions. Specifically, your editorial in effect equates small size and financial problems with inability to conduct good research or with motives for establishment based on "pleasure, profit or aggrandizement."

I take issue with this interpretation, which I feel could all too easily be de-