*Behavior Genetics, Vol. 8, No. 2, 1978*

# SHORT COMMUNICATION

## Sex Linkage and Race Differences in Spatial Ability: A Reply

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*Received 28 Oct. 1977—Final 28 Nov. 1977*

*Stevens and Hyde have questioned only the empirical evidence relevant to Jensen's formulation of a genetic hypothesis to explain race and sex differences in spatial visualization ability, rather than the formulation of the hypothesis* per se, *which at present remains the only non-*ad hoc *hypothesis concerning these phenomena, which are admittedly in need of further empirical investigation. The formulation of potentially falsifiable hypotheses derived from genetic theory is the surest means for advancing knowledge in behavioral genetics.*

**KEY WORDS:** spatial ability; race differences; sex linkage.

Stevens and Hyde (1978) actually make no intrinsic criticism of my formulation of a genetic hypothesis involving an X-linked recessive gene which enhances spatial ability, a hypothesis intended to account for the empirical generalization, frequently found in the psychological literature, that American blacks show a "perceptual deficit" relative to other abilities (Jensen, 1975). The formulation of the hypothesis is technically correct in terms of genetic theory, given certain empirical assumptions. The hypothesis, like all hypotheses, was expressly based on certain assumptions, including (1) that spatial ability is enhanced by a recessive X-linked gene, which, when I wrote in 1975, was consistent with research by Bock and Kolakowski (1973), and (2) that American blacks perform less well on tests of spatial visualization than on other tests, relative to whites, for which there was evidence in several studies cited in my article. Stevens and Hyde

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0001-8244/78/0300-0213$05.00/0 

point out that the existing empirical evidence is inadequate for a rigorous test of my hypothesis. But I already stated that in my article.

Stevens and Hyde, however, do make a contribution by also pointing out two new lines of evidence and analysis which were not in the picture when I wrote my article in 1975. These two points importantly bring into question the basic empirical underpinnings of my hypothesis.

First, some recent researches on the genetics of spatial ability have now cast serious doubt on the X-linkage hypothesis as formulated by Bock and Kolakowski (1973). The present state of affairs is that some of the evidence based on intrafamilial correlations supports the X-linkage hypothesis, while some of it does not. The cause of this scientific anomaly must be understood through further investigation. Science cannot rest content with the latest box-score of conflicting findings. If the X-linkage hypothesis of spatial ability should be definitively rejected, then of course it would not enter into the formulation of any hypothesis concerning racial differences in spatial ability.

Second, a recent critical review (Mandler and Stein, 1977) of the empirical evidence for the spatial ability deficit of blacks suggests that this evidence is weak and in need of more rigorous substantiation. If further adequate investigations reveal no specific spatial deficit in blacks, then, of course, there would be no need for an explanation of such a deficit, and my hypothesis would be rendered moot.

If, on the other hand, the X-linkage hypothesis is borne out by further investigation, and if it is empirically established that there is a spatial ability deficit in blacks, the hypothesis I have put forth is the only presently existing *scientific* hypothesis that would account for the black spatial deficit. It is the only hypothesis that is not merely *ad hoc* and would predict the phenomenon in terms of general genetic principles. It is scientific in the Popperian sense that it is precisely formulated enough to risk falsification, which, according to Popper, is the essential criterion for judging the scientific value of any hypothesis (Popper, 1965, 1968). Another essential criterion of scientific value is that the hypothesis make novel predictions and thereby lead to new knowledge (Lakatos, 1970).

So far, environmentalist hypotheses of the black spatial deficit have been either *ad hoc* (i.e., not derived from any general established principles or model) or so general and vague (e.g., "cultural factors") as not to be falsifiable by any empirical test, and they are therefore nonscientific. (This is not to say that environmental or cultural hypotheses that meet the criteria of scientific usefulness might not be formulated in the future.) Scientific knowledge advances through a continuing process of rejecting specifically stated, empirically testable hypotheses. Therefore, I advocate formulating testable genetic, as well as environmental, hypotheses for all kinds of indi-

vidual and group behavioral differences. I deplore the fatuous notion that no genetic hypothesis should be put forth, if at all, until all possible environmental hypotheses have already been investigated and ruled out. The simultaneous interplay of competing testable hypotheses yielding contradictory predictions makes for the surest, most rapid advancements in scientific knowledge.

Stevens and Hyde raise several other specific theoretical and methodological issues that deserve comment.

First, I can see nothing "inherently illogical" about the *absence* of a sex difference in spatial ability being consistent with X linkage of that ability. If black females, on the average, score higher than males across the board on a number of different ability tests except spatial ability, and if it is hypothesized that some known or unknown factor(s) depresses the performance of males relative to females on all tests, then the lowering of spatial ability in females because of X linkage could make their performance on the X-linked ability equal to (or even higher than) that of males. I have elsewhere proposed an environmental explanation for the frequently observed sex differences in abilities (females scoring higher) across a variety of mental tests and achievements in blacks relative to the same comparisons in whites (Jensen, 1971).

As Stevens and Hyde correctly note, the sex difference in spatial ability is more marked and consistent in postpuberty samples, and future research should indeed take this finding into account. It suggests a trait that is under hormonal control, whatever its genetic nature. This, therefore, is an important lead.

Most important, however, is that future research on spatial ability must depend on factor analysis and factor scores, and not just on raw scores from some one test of spatial ability. The main necessity for this is that virtually all tests of special abilities, such as the Primary Mental Abilities tests, have a larger factor loading on the *g* (general) factor common to all complex cognitive tests than on the specific factors they purport to measure. In any study, a sufficient variety of tests is needed to distinguish, by means of factor analysis, a spatial factor as distinct from the *g* factor and other group factors. Tests of hypotheses should be based, not on raw test scores, but on factor scores of spatial ability that are uncorrelated with *g* or other primary factors that emerge from a large-scale factor analysis. Studies such as those cited by Stevens and Hyde are exceedingly weak for the present purpose and practically irrelevant as potential tests of the X-linkage hypothesis of the black–white difference in spatial ability, if indeed such a difference exists. For example, the Eyferth study used the Block Designs subtest of the Wechsler as a measure of spatial ability; but Block Designs has a loading close to 0.80 on *g*, after correction for attenuation,

which leaves very little variance to be accounted for by the spatial factor. Any obscure influences, environmental or genetic, that might affect *g* in these statistically nonrepresentative samples of interracial children in postwar Germany will act as "noise," swamping the much weaker hypothesized genetic differences in spatial ability. Since the Block Design test measures *g* more than it measures spatial ability, one should predict, from a genetic hypothesis, that interracial boys with black fathers would score lower than boys with both parents white, and this is what was found, at the 0.01 level of significance. (The X-linkage hypothesis would predict no difference between the interracial boys and the white boys if the test measured only spatial ability.) However, girls with a black father and white mother did not differ significantly on Block Designs from girls with two white parents, and the slight difference, in fact, is the reverse of what would be predicted by the X-linkage hypothesis. So these data overall are inconsistent with any hypothesis that has yet been proposed. No environmental or cultural hypothesis known to me would have predicted these puzzling outcomes, either. In fact, there are too many unknowns in these data to warrant any theoretical prediction, and so these data cannot really provide a rigorous test of any hypothesis. The same sort of thing can be said of Backman's study, but since her study was based on Project TALENT data there remains the possibility that it could still be reanalyzed with respect to the specific hypothesis in question, using factor scores on spatial ability, or at least controlling the *g* factor extracted from the other tests in the battery, by means of analysis of covariance or some other multivariate statistical method.

In general, I believe the Comment by Stevens and Hyde performs a service by emphasizing the puzzling inconsistencies and ambiguities in our present empirical knowledge of spatial ability. But I hope it will not reinforce a common misconception about the function of hypotheses in science and the all too frequent tendency immediately to reject any clearly formulated genetic hypothesis of group differences, simply because the existing evidence is too weak or inadequate to provide a rigorous test of it, and to be satisfied with *ad hoc* or vague environmental explanations of whatever evidence presently exists. The virtue of a strong hypothesis, genetic or environmental or whatever, is that it is testable and, in principle, refutable, and therefore invites attack. If the attack is appropriate, based on a marshalling of clearly relevant evidence, our knowledge is thereby advanced, and some better hypothesis can replace the rejected one. The act of testing a clear-cut hypothesis usually involves the acquisition of new and better information, or at least a more penetrating analysis of the existing evidence. It is the pursuit or the avoidance of such a strategy that makes for the differences between

what Urbach (1974) has referred to as "progress and degeneration" in a research program.

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