BEHAVIORAL AND BRAIN SCIENCES (1993) 16, 187–215
Printed in the United States of America

Continuing Commentary

Commentary on **Camilla Persson Benbow (1988) Sex differences in mathematical reasoning ability in intellectually talented preadolescents: Their nature, effects and possible causes. BBS 11:169–232.**

Abstract of the original article: Several hundred thousand intellectually talented 12- to 13-year-olds have been tested nationwide over the past 16 years with the mathematics and verbal sections of the Scholastic Aptitude Test (SAT). Although no sex differences in verbal ability have been found, there have been consistent sex differences favoring males in mathematical reasoning ability, as measured by the mathematics section of the SAT (SAT-M). These differences are most pronounced at the highest levels of mathematical reasoning, they are stable over time, and they are observed in other countries as well. The sex difference in mathematical reasoning ability can predict subsequent sex differences in achievement in mathematics and science and is therefore of practical importance. To date a primarily environmental explanation for the difference in ability has not received support from the numerous studies conducted over many years by the staff of Study of Mathematically Precocious Youth (SMPY) and others. We have studied some of the classical environmental hypotheses: attitudes toward mathematics, perceived usefulness of mathematics, confidence, expectations/encouragement from parents and others, sex-typing, and differential course-taking. In addition, several physiological correlates of extremely high mathematical reasoning ability have been identified (left-handedness, allergies, myopia, and perhaps bilateral representation of cognitive functions and prenatal hormonal exposure). It is therefore proposed that the sex difference in SAT-M scores among intellectually talented students, which may be related to greater male variability, results from both environmental and biological factors.

A theory explaining sex differences in high mathematical ability has been around for some time

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Although sex differences in high mathematical ability in the SMPY (Study of Mathematically Precocious Youth) data have been known at least since the early 1970s (e.g., Keating & Stanley 1972), the articles that thrust the issue to the forefront of interest were the pair of *Science* papers by Benbow and Stanley (1980; 1983). There, based on roughly 65,000 youth, were the key empirical findings that sometimes seemed to become obscured in the 1988 target article of Benbow: (i) The SAT-M means of boys have always been larger than the means for girls by about 30 and more points in each yearly talent search. (ii) The SAT-M variances for boys have always exceeded the variances for girls. That the corresponding verbal SAT-V scores for the same subjects show no such pattern is critical: There is something special about the math scores, as Benbow's (1988) Table 1 shows. (iii) For a fixed SAT-M score, s, the ratio of the proportion of boys with scores exceeding s, relative to the proportion of girls, has always exceeded one. As s increases, the ratio increases. For example, if $s = 420$, the ratio is 1.5:1, whereas for $s = 720$, the ratio is about 13:1.

These are the main facts that need to be explained. It is clearly a proper role of science to seek an explanation. Yet after Benbow and the commentators reviewed a large body of empirical and theoretical work, no theory was mentioned that could explain any one of the above facts in a rigorous way, let alone all of them.

An adequate theory must account for all the facts, in a coherent way; furthermore, any such theory needs to be testable and falsifiable. There is a rigorous formal theory that satisfies these criteria and was specifically constructed for the SMPY sex differences data. Benbow has known about it at least since 1984, following my exchange of several letters and notes with either her or J. C. Stanley during the interval 1983 to 1985. In 1984 she wrote to me indicating she had read a prepublication draft of the theory that I had sent to her. She offered no criticisms of it. She was sent a reprint of the paper following its publication in 1985 (Thomas 1985). Yet, even though a conceptual interpretation of the varied sex differences in SAT-M is key to the entire target article, there is no acknowledgment of this work in the article, commentary, or response.

The putative theoretical mechanism is an X-linked gene, in two alleles; only the recessive in frequency q is assumed to be facilitative of superior performance. Under a simple genetical model it follows easily that the proportion of facilitated males and females is, respectively, q and q^2. The elementary but important fact that drives the theoretical machinery is that $q > q^2$ for all $0 < q < 1$.

The idea that a genetical X-linked model might provide an explanation for certain sex differences is an old one, and has sometimes been relegated to the scientific scrap heap (e.g., Boles 1980). Vandenberg's (1988) comments suggest that that is where he puts the hypothesis. But this judgment is precipitous and a poorly reasoned one, because there had not been a properly developed model. After all, in its simplest setting the theory says individuals take on but one of two discrete gene values: a performance facilitative one (with probabilities q or q^2, for males and females) or a nonfacilitative one (with probabilities $1 - q$ and $1 - q^2$). This idea is a start, but does not go far enough. Clearly it is no model for observed test scores, which are essentially observations from continuous score distributions. When a properly constructed model is developed that considers this fact, some interesting consequences result (Thomas 1982; 1983; 1985; 1987; 1988). For example, simple correlational tests such as those Zohar and Guttman (1988) proposed are not, as they suggest, easily interpreted as providing evidence for X-linkage (Thomas 1983).

With respect to the SMPY data, one critical consideration is

 0140-525X/93 $5.00+.00

the sampling scheme, which must be modeled from a bivariate perspective. There are two tests, an achievement selection test and the SAT-M test. To be selected to take the SAT requires a score in the top 3% to 5% on the achievement test. Under the proposed model, a two-component bivariate normal mixture density with truncation from below on the achievement variable, the (marginal) distribution of SAT-M scores is neither normal, nor a truncated normal (as Becker & Hedges, 1988, proposed). It is interesting to compare the SAT-M empirical distributions given by Benbow (1988, Fig. 1, p. 219) with a theoretical distribution under the model (Thomas 1985, Fig. 1). They are similar.

Most important, however, the simple model generates predictions that are precisely in order-correspondence with the SMPY facts (i) to (iii) listed above. It is assumed that there is only one parameter that distinguishes between the sexes: q for males and q^2 for females. (Under a gene inactivation model, the situation for females becomes more complicated; cf. Thomas 1988). Thus, the theory predicts that males will always have larger mean scores. As Humphreys (1988) suggests (but not for his reasons), variances are more important: The variance of the males will be larger than the variance of the females when $q <$ .618. Should q be larger, the ordering of the variances is reversed. Note that if the variance for the boys turned out to be smaller than for the girls the model is dead. That is because for the theory to be plausible, q must be small. After all, how many mathematically talented individuals are there? I've suggested (Thomas 1985) that q is much less than .2.

There are other tasks that also quite consistently reveal sex differences in task performance. Witkin's rod-and-frame task is an example. In this task although the male average performance typically exceeds female average performance, the variance for females is typically larger than the variance for males (e.g., Witkin et al. 1985, pp. 137–38). Unlike superior mathematical ability, superior performance on this task is not rare, and thus q may be expected to be large: It has been estimated to be .9 (Thomas 1982). Thus the theory and subsequent applications of it suggest why mathematical ability and certain "spatial" tasks, of which the rod-and-frame is often considered an example, are not consistently related. Quite possibly different genes may be involved.

There are other never-understood empirical facts that are easily predicted from the model. Benbow (1988) notes that similar ordinal data relations emerge in studies done abroad, but the magnitudes of the between-sex differences may be culture-specific. Such a finding is exactly as expected. That gene frequencies vary from population to population is fundamental. And the magnitudes of the sex differences in both mean and variance depend on the value of q. For example, the mean sex difference is largest when q = .5. The relative frequency for color blindness, a sex-linked recessive trait well known to be more prevalent in males than females, is known to vary from culture to culture, showing that gene frequency varies as well (Post 1962).

Another SMPY fact is that boys were selected more frequently than girls, about 57% to 43% in studies prior to 1980. The model also predicts this bias. This does not mean of course that social factors are not operating, only that under the model such a result is expected. Indeed, the only data-specific fact I worried about that seemed to be at odds with the model was that in the early talent searches the best performers were always boys (Benbow & Stanley 1980). Under the model, it is more probable for boys to be top performers, but it should not always be the case. Happily, the all-time highest scorer was a girl who, at age 13, scored 790! (J. C. Stanley, personal communication, 1985).

The model has not been formally tested, although the perfect correspondence between model derivations and empirical data can hardly be regarded as no test of the model. Already it appears to have received more support than any other explanation. The theory requires achievement and SAT-M test score pairs to estimate model parameters and fit the model to data. When I contacted Benbow in 1983, she wrote that the SMPY data were not suitably coded; subsequent contact with gate keepers to other talent search data sets, at least in the United States, indicated there was little interest in testing the model. Typically, letters of inquiry were ignored. Outside of talent search settings, given suitable sampling considerations, one model prediction is that the distribution of mathematics scores should be modeled by a mixture distribution. The data should reject normality and be compatible with a normal mixture, with the appropriate component weights between the sexes quadratically related as q and q^2 are related.

There is no doubt that if tested and fitted to data, the model will be falsified. Given the potential power of thousands of data points, is there any model, particularly one this simple, in *any* area of science that could be expected to survive? Because all models are wrong models, the only relevant question is whether the model is importantly wrong. One must remember that almost 50 years ago McNemar (1942) statistically rejected normality of IQ distributions. Yet normality is still considered a useful model for IQ.

There are several levels at which model construction and testing can be directed, and the proposed model is only one level. It is certainly wrong, however, to conclude that testing models at this level has no implications for models at other levels. For example, under the model there is no reason to suspect there would be sex differences in qualitative thinking or reasoning among samples of high SAT-M scorers. Moreover, the model does not disregard the role of social-environmental variables; it does regard them as negligibly covarying with SAT-M scores, and there appears no reason to reject that assumption given Benbow's (1988) review.

It is interesting to note that the number of traits identified by geneticists as X-linked has been growing linearly with the years, at the rate of about 3 per year from about 1958 to 1982, so that by 1982 there were from 115 to 250 X-linked traits identified, depending on the criterion (McKusick 1983). It would be naive to suppose that behavioral processes were somehow immune to such gene influences. Just why there are virtually no behavioral traits that researchers have agreed are X-linked is itself an interesting question. My answer (Thomas 1987) is that neither model construction nor methodology has been adequate for the task.

The only theory in the literature that can explain all the major findings of the SMPY sex differences data, and does so in a coherent way, requiring only one between sex difference parameter, was never mentioned in 57 pages of text and hundreds of references in *Behavioral and Brain Sciences* 11(2) 1988. The omission is important. It raises serious questions about scholarship and objectivity, and whether outside of "socially acceptable variables" there is any desire to understand high math sex differences.

Editorial Note

The author of the target article has been given the opportunity to see this commentary and has elected not to respond.

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Commentary on John R. Searle (1990) Consciousness, explanatory inversion and cognitive science. BBS 13:585–642.

Abstract of the original article: Cognitive science typically postulates unconscious mental phenomena, computational or otherwise, to explain cognitive capacities. The mental phenomena in question are supposed to be inaccessible in principle to consciousness. I try to show that this is a mistake, because all unconscious intentionality must be accessible in principle to consciousness; we have no notion of intrinsic intentionality except in terms of its accessibility to consciousness. I call this claim the "Connection Principle." The argument for it proceeds in six steps. The essential point is that intrinsic intentionality has aspectual shape: Our mental representations represent the world under specific aspects, and these aspectual features are essential to a mental state's being the state that it is.

Once we recognize the Connection Principle, we see that it is necessary to perform an inversion on the explanatory models of cognitive science, an inversion analogous to the one evolutionary biology imposes on pre-Darwinian animistic modes of explanation. In place of the original intentionalistic explanations we have a combination of hardware and functional explanations. This radically alters the structure of explanation, because instead of a mental representation (such as a rule) causing the pattern of behavior it represents (such as rule-governed behavior), there is a neurophysiological cause of a pattern (such as a pattern of behavior), and the pattern plays a functional role in the life of the organism. What we mistakenly thought were descriptions of underlying mental principles in, for example, theories of vision and language were in fact descriptions of functional aspects of systems, which will have to be explained by underlying neurophysiological mechanisms. In such cases, what looks like mentalistic psychology is sometimes better construed as speculative neurophysiology. The moral is that the big mistake in cognitive science is not the overestimation of the computer metaphor (though that is indeed a mistake) but the neglect of consciousness.

Comments on the Connection Principle

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Searle's (1990a) argument is directed against the cognitive science view of the world that populates the mind with a host of unconscious mental rules (e.g., universal grammar) whose status and operations are to be understood on the model of conscious rule-following operations, except that they are unconscious. The thrust of the argument is that there is a logical connection between our concept of intentionality and our concept of consciousness, and that if this is indeed the case, then it is incoherent to talk about unconscious intentional phenomena.

The argument for the Connection Principle is made in the following six steps (Searle 1990a, pp. 586–88):

Step 1. There is a distinction between intrinsic and as-if intentionality.

Step 2. Intrinsic intentional states, whether conscious or unconscious, always have aspectual shapes.

Step 3. The aspectual feature cannot be exhaustively or completely characterized solely in terms of third-person, behavioral, or even neurophysiological predicates. None of these is sufficient to give an exhaustive account of aspectual shape.

Step 4. But the ontology of unconscious mental states, at the time they are unconscious, consists entirely in the existence of purely neurophysiological phenomena.

Step 5. The notion of an unconscious intentional state is the notion of a state that is a possible conscious thought or experience.

Step 6. The ontology of the unconscious consists in objective features of the brain capable of causing subjective conscious thoughts.

Insofar as the distinction in the first step refers to the distinction between *genuine* intentionality and as-if intentionality, I take it to be unproblematic and obvious.[1] Certain parts of the world can refer to entities beyond themselves (e.g., my mental states) while other parts of the world cannot (e.g., tables and chairs); although for certain purposes, they may be treated as if they can. I suspect that a number of conceptual confusions in cognitive science result from ambiguity about this issue.

The second step introduces the notion of "aspectual shape" and is more problematic. The term does have some clear-cut applications, but the full extent of what is intended by Searle's usage is not clear. Some clear and rather literal examples are provided by visual perception, where the notion of aspect corresponds to the notion of perspective. If I view my car from point A, it appears as having a certain shape or information content. If I viewed the same car from point B, it would appear as having a different shape and information content. For example, from point A, I may be looking at the front of the car and