Gould, Goddard, g, and Genes: In Reply to Professor Matthews

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I first want to correct a small, but irritating error in the opening paragraph of Matthews' response. He quotes me as saying, of the Marxist critique of IQ, that: "the contribution is arbitrary, superficial, confused, misleading and mistaken." These words, however, are Matthews', *not* mine. The quotation has been invented. Perhaps my complaint is niggling, but it *is* irritating to have words put in your mouth – quite literally!

Matthews does correctly note that I fault the Marxist critique of IQ "... for failures in four areas: (a) the account of the ideological role of the early testers, (b) the unjustified tainting of present testers with the sins of their forebears, (c) misrepresenting Jensen's view on the ontological status of *g*, and (d) misrepresenting Jensen's view on heritability of IQ." Matthews, however, considers my fault finding unconvincing. So let me try to argue again the points using Matthews' agenda.

The Ideological Role of the Early Testers

I suggested in my article that the radical Marxist critique of IQ actually consists of two somewhat different strands. One strand attempts to sustain the charge that IQ theory and research are little more than thinly disguised racist ideology. The other charges that IQ research is bogus science because it is fatally flawed by various philosophical, psychometric, and statistical mistakes.

The plausibility of the *IQ-as-ideology* charge largely depends upon the veracity of the historical account of the early IQ testing movement in the United States provided by such revisionist historians as Karier (1972), Kamin (1974), and Gould (1981). According to this revisionist history of IQ, the two most celebrated and damning "facts" are the claims that the early testers uniformly held racist views about ethnic group differences in IQ and that they aided and abetted a racist immigration policy which culminated in the passage of the restrictive Immigration Act of 1924. These are the principal factual claims that are offered as testimony to the racist ideological role of early testers.

As it happens, however, the trouble with this revisionist account is that these two factual claims are now disputed. Indeed, they have been bluntly denied. The most detailed and explicit challenge to these claims is that provided by Snyderman and Herrnstein. They contend, contrary to the revisionist historians of IQ, that in

> searching the scientific literature of that time, one can find no consensus for using intelligence tests to restrict immigration. It would, in fact, be easier to substantiate the reverse, that the testing community was at least reluctant, and perhaps firmly opposed, to using tests so irreversibly. (1983, p. 991)

Second, Snyderman and Herrnstein conclude that " ... nothing in the record suggests an important role for tests in the formulation or enactment of immigration policy" (p. 994). Obviously, their reading of the historical record flatly contradicts the revisionists' claims that the psychological testers were racist and that they were willing servants of power.

Matthews' response to these counterclaims is equivocal. As regards Snyderman and Herrnstein's conclusion, he recognizes that "if true, this would cause the revision of *one* substantial segment of the revisionist picture." Further on, Matthews admits with obvious reluctance that "in a very tortured sense both claims [of Snyderman and Herrnstein] may be true." However, despite these admissions, Matthews chooses to reaffirm the disputed factual claims by simply restating them. For example, he quotes Karier's (1972) contention that Terman's studies of gifted children "entered into the dialogue which led to the restrictive immigration of 1924." However, this only repeats the very assertion that is now disputed. As the old adage correctly reminds us: "Saying doesn't make it so."

Next, Matthews recalls for us the variety of political scalawags that made up Calvin Coolidge's Congress, the jingoistic sloganeering of Coolidge himself, and the blatantly racist views of two prominent eugenicists – all of which I am prepared to accept as true. But, without diminishing the indignation that this brief retrospection is meant to arouse, I am at a complete loss to understand how it is relevant to the question at hand. I thought the questions were: Did the early *testers* (not Calvin Coolidge and his cronies) generally view ethnic group differences in IQ as favouring a restrictive immigration policy; and did their views, in fact, assist the passage of the racially discriminatory Immigration Act of 1924? Snyderman and Herrnstein argue persuasively (and contrary to the revisionist account) that the answer is "No!"

Finally, Matthews, in a last ditch effort to save the revisionists' account, recasts their disputed claims into a more general accusation which is at once weaker but more insidious. He alleges "that leading American psychologists contributed to the climate of opinion leading up to the immigration acts." Since there is little or no evidence that the mental testers directly "entered into the dialogue," as Karier first alleged, perhaps they indirectly "contributed to the climate of opinion," as Matthews now vaguely charges. One need not be a Popperian to recognize how nearly impossible it would be to refute such a vague and slippery accusation. Moreover, the stubborn persistence of such accusations in the face of frank counter-evidence teaches that we should worry at least as much about the ideological role of the revisionist historians as about that of the early mental testers whom they seem so bent upon discrediting.

The Sins of Their Forebears

As for my other principal objection to the revisionists' selective history of IQ research, I did put it in a hypothetical form. And, although Matthews considers it "revealing," he appears not to understand it. The point is simply this: Even if one can find evidence of early mental testers' holding prejudiced views about racial differences in IQ, this does not mean that contemporary researchers of IQ also hold the same or similar prejudices. Nevertheless, this seems to be the sort of incriminating inference that the revisionists would have us make. Matthews, for example, says:

> What is so clearly the case with the early work of Goddard and Thorndike becomes more difficult to detect as the decades roll by, until, of course, frauds such as Cyril Burt are unmasked and we are jolted once more back into the recognition that the leopard's spots really have not changed. (1980, p. 152)

To appreciate how damning the allusion to "the early work of Goddard" is meant to be, it is important to realize that the hapless Goddard has been portrayed and defamed by the radical historians as the Dr. Mengele of psychological testing. His infamous reputation derives principally from his supposed claim to have "... scientifically proved that 83 percent of [immigrant] Jews were 'feebleminded,' along with 90 percent of Hungarians, 79 percent of Italians, and 87 percent of Russians (most of them Russian Jews)" (Fallows, 1980).

As it turns out, Goddard apparently never made such a claim. Furthermore, with respect to the lower IQ scores of immigrants, Goddard (1917, p. 270), in fact, maintained "that it is far more probable that their condition is due to environment than it is due to heredity" (cited in Snyderman & Herrnstein, 1983). The final irony in this case of wrongful prosecution or persecution is that:

> Goddard ended up *favoring* the immigration of people who appeared to possess limited present intelligence: Not only would they perform useful work, but

"we may be confident that their children will be of average intelligence and if rightly brought up will be good citizens." Goddard was hardly a great scientist, but he deserves a fair hearing. (Davis, 1983, p. 47)

However, we can set aside this belated rehabilitation of Goddard. To be sure, it serves to caution us about uncritical acceptance of various revisionist claims. Strictly speaking, however, Goddard's guilt or innocence is apart from my point – which is: Even if Goddard's views on ethnic or racial differences in IQ were as repugnant as insinuated, such a fact is utterly without implication for the attitudes and motives of contemporary IQ researchers. Davis has made the same point: "To remind us of these roots in the history of racism is instructive – but to imply a similar prejudice in today's investigators of intelligence is unfair" (1983, p. 46).

As mentioned, Matthews appears not to have understood this point. He evidently believes my claim to have been "that current theory and research is wholly independent of what has preceded it." But clearly such a claim borders on the absurd. Any research *tradition*, by definition, evinces historical continuity. How could it be otherwise? Furthermore, I have no particular quarrel with the examples used by Matthews to illustrate this rather banal thesis of historical continuity. To wit, I agree with Eysenck that the development of a test technology outstripped the development of a theory of intelligence – perhaps to the detriment of the latter. Moreover, I agree with Anastasi that the original Binet Scales have remained the prototype for the measurement of IQ and an important operational definition of intelligence. I agree that historically a close connection exists between IQ and scholastic ability – a scholastic bias if you like. There is nothing either particularly new or, more importantly, self-refuting about these observations. There is nothing in all of this to suggest that IQ researchers ought therefore to close up shop. Whatever is supposed to follow from Matthews' historicity in this instance or why it should stir our moral indignation completely escapes me! However, what does deserve our indignation is, as Matthews himself so neatly puts it, "the unjustified tainting of present testers with the sins of their forebears."

The Ontological Status of g

The second strand in the radical Marxist critique of IQ research is the broad charge that it is bogus science. As part of this more technically oriented critique of IQ theory and research, it has been asserted that this research is premised upon a fundamental ontological error, namely, reification. It is alleged by both Matthews and Gould that IQ researchers from Spearman to Jensen have taken their measurements far too seriously and have deluded themselves (and, more importantly, others) into believing that they are measuring some real, tangible thing. The prime example of this mistake is supposedly the unwarranted "reality" that researchers, especially Jensen, have assigned to Spearman's *g*. Spearman's *g* refers to the common factor that can be extracted from any large collection of intellective tests (especially IQ tests) and is often labelled as "general intelligence." Gould (1981), however, has insisted that *g* is merely an arbitrary result of one kind of factor analysis and, thus, can claim no reality beyond the mathematical algorithm by which it is extracted. In effect, Gould argues that *g*'s meaning is exhausted by its computational or operational definition. Thus, to assume that *g* is something more than a computational result is mistakenly to reify it.

The relevant counter-evidence to this criticism, therefore, is to demonstrate that *g*'s meaning is *not* exhausted by it operational definition. That is, to meet this criticism, it is necessary to show that *g* is correlated with phenomena that are completely independent of its psychometric and factor analytic derivation. And, indeed, this is exactly what the relevant research has variously shown. For example, highly significant relationships have been found between *g* and reaction times to elementary cognitive tasks, evoked electrical potentials of the cerebral cortex, and a number of physical factors (Jensen, 1986). Thus, one cannot but agree with Jensen and others that there *are* good reasons, as I have said, "for imbuing *g* with additional or surplus meaning beyond its austere statistical definition" (Sanders, 1985, p. 408). From this conclusion, Matthews then infers "in brief, that *g* is given ontological status." Before dealing with this further inference, however, the prior conclusion bears repeating. That is, quite contrary to Gould's contention, that there *is* good evidence that *g* is not just the end result of an arbitrary computation and that its meaning exceeds its operational definition.

As regards the implied ontological status or reification of *g*, Matthews' position has become "curiouser and curiouser." Initially, he, alongside Gould, charged that Jensen and others were guilty of reification. But now, since Matthews is no longer sure reification, per se, is a bad thing, he is prepared to reduce the charge to "unwarranted reification." What Matthews means by "unwarranted reification" is never explained. That *g* exemplifies unwarranted reification is again merely asserted: "Gould and I are saying that the unitary theory of intelligence, with its postulated *g*, is just one more such case."

As Matthews correctly observes, "after all the huffing and puffing it does seem that we are back where we started." In the end, the charge of reification turns out to be a red herring. That is, no one reasonably familiar with this research believes *g* to be a unitary, space-occupying, material thing. Therefore, if reifying *g* means mistakenly inferring that *g* is a material entity or thing in this crude sense, then no one commits this fallacy. On the other hand, if *g* can be shown to relate to phenomena that are independent of *g*'s own factorial definition and derivation, then reifying *g*, in the sense of positing it as a hypothetical or theoretical construct, is not a fallacy.

The Heritability of IQ

Historically, the flashpoint for the contemporary IQ controversy was Jensen's estimate of the heritability of IQ at about 0.80. There is no need to review the hullabaloo that it has provoked. I would contend that any reasonably careful reading of Jensen's work confirms that his view typically has been either misunderstood or misrepresented – especially by the radical critics. Matthews provides a case in point with his own assertion "that Jensen believes IQ is 80% heritable." To express Jensen's view in these terms is, it seems to me, to misrepresent and thus to misinform. What is misleading about Matthews' version is that it can easily be misunderstood as referring to "the percentage of an individual's IQ which is inherited" – an imaginary quantity in any event! Jensen's heritability estimate has a very different, subtle, and relatively technical meaning, *viz.*, the estimated proportion of the present population variance in IQ that is "accounted for" by genetic variance. It is doubtlessly a notion that is very difficult to communicate to a popular audience and, thus, all the more deserving of careful exposition. As I have said, Jensen, unlike many of his critics, has been especially scrupulous in this regard. I presume that Matthews now agrees since he admits he "may have erred in saying that he [Jensen] moves too easily between the technical, and totally different commonsense meanings of 'heritability'."

A further technical objection that Matthews raises against Jensen's heritability analysis is the linearity of the statistical model which Jensen uses to parse and estimate the various genetic and environmental contributions to the total IQ variance. He contends that Jensen's "linear model is quite simply a mistaken model for the analysis." As with the earlier reification charge, Matthews apparently believes that there is something profoundly fallacious about the use of a linear statistical model. To add to the confusion, he also apparently believes that I agree that any such linear model is mistaken and that I claim that "Jensen does *not* [italics added] use a linear model for the analysis ... ." As a consequence, it appears to Matthews (and rightly so, given these twin misapprehensions) that when I refer in passing to "Jensen's linear statistical model," I have, as it were, shot myself in the foot. Let me be perfectly clear. First, I do *not* think that the use of a linear model constitutes some kind of *a priori* mistake. Second, I *do* acknowledge that Jensen uses a linear model. I had assumed my position was plain when I asserted that "there is, however, nothing inherently wrong with using a linear statistical model unless, of course, it can be shown to be a particularly bad fit for the data – and that remains to be demonstrated" (Sanders, 1985, p. 410).

In any case, it is evident that it is not the linearity of the model that troubles Matthews because his reasons for rejecting Jensen's statistical model are unrelated to its linearity. Matthews asserts: "There are good reasons why this model should be rejected, and replaced by a heredity/environment interactive model. Genes and environment interact from the very outset and phenotypic expression is a product of this ongoing interchange" (1980, p. 146). Thus, his underlying complaint seems to be that Jensen's statistical model wrongly assumes that the effects of heredity and environment are wholly additive and thereby fails to capture the intrinsically interactive nature of the relationship between heredity and environment. Again, however, Matthews' complaint rests upon a confusion – specifically, a confusion of the two meanings of *interaction.* I can assure Matthews that Jensen's statistical model *does* include an interaction term (V_I) which provides an estimate of the contribution of nonadditive, interactive variance to the total phenotypic variance (see Jensen, 1969, pp. 34–40). Unfortunately, such an assurance is unlikely to satisfy Matthews because he persists in confusing interaction in the statistical sense of a nonadditive source of variance with interaction in the ordinary sense of "intermingle" or "combine." Notice that "combine" can be substituted for "interact" in the Matthews quotation above with no loss of meaning: "Genes and the environment [combine] from the very outset and phenotypic expression is a product of this ongoing interchange." The claim is, of course, true, but, more than a claim, it is a definition of what it means to be a phenotype – such as, say, IQ. As a claim, in fact, it is both trivially true and beside the point. There is nothing in Jensen's statistical modelling that denies the rather jejune point that the genotype and the environment will always be inextricably combined.

Matthews concludes with a more discursive discussion of the technical meaning of heritability and what he takes to be Jensen's genetic hypothesis. I have no great quarrel with his brief exposition of heritability and its interpretive limits – although here too he imports some popular misconceptions into his exposition. For example, he asserts that the evidence for the heritability of IQ depends almost exclusively upon twin studies: "Studies of monozygotic twins will provide the best estimate of heritability .... If they are flawed, then most of the evidence for the heritability of IQ is removed." This is simply false. Twin comparisons, especially identical twins reared apart, merely provide the conceptually simplest illustration of the idea of the relative influence of genes and environment on differences in IQ. The comparison of genetically unrelated children reared together provides another such conceptually simple illustration. As Jensen points out,

> modern methods of [heritability] analysis – made feasible by electronic computers – analyze all of the various kinship correlations simultaneously to get the most accurate overall estimate of h^2 that can be obtained from all the data. This more elaborate methodology also takes into account the effects of dominance, assortative mating, and the correlation between genotypes and environments ... which have different effects on different kinship correlations. (1980, p. 103)

Again, Matthews' criticism is both misinformed and misinforming.

Finally, I will comment briefly on Matthews' rhetorical cadenza which purports to get "at the heart of Jensen's error," namely, what Matthews calls the imposed "choice between environmentalism and genetic determination." I wish only to point out that this false dichotomy obscures a simple but important point about the actual structure of the debate. There are, of course, essentially *three*, not two, general hypotheses about the origins of individual differences in IQ: (a) *genetic determinism*, that differences in IQ are due almost entirely to genetic differences among people; (b) *environmentalism*, that differences in IQ are due almost entirely to environmental differences among people; and (c) *interactionism*, that differences in IQ are due to a significant amount of both. What Matthews insists upon labelling "Jensen's genetic hypothesis" is not the first hypothesis, genetic determinism. Marxist and revisionist rhetoric notwithstanding, Jensen's hypothesis is interactionist. And interactionism acknowledges that, in addition to environmental variation, genetic variation is also significant in determining differences in IQ. Matthews and the Marxists, however, espouse their own self-styled brand of interactionism that denies any important contribution of genetic variation to differences in IQ. They wish to proclaim their interactionism and keep their environmentalism too! Jensen's so-called genetic hypothesis, on the other hand, simply recognizes that genetic variation, as well as environmental variation, contributes significantly to differences in IQ. I would not begin to know how much humanitarianism Jensen's hypothesis has on its side: I will leave the moral one-upmanship to Matthews. I am certain, however, that it has all the evidence.

NOTE

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REFERENCES

Davis, B. D. (1983, Fall). Neo-Lysenkoism, IQ, and the press. *The Public Interest, 73*, 41–59.

Fallows, J. (1980, February). The tests and the "brightest." *Atlantic*, 37–48.

Goddard, H. H. (1917). Mental tests and the immigrant. *Journal of Delinquency, 2*, 243–277.

Gould, S. J. (1981). *The mismeasure of man*. New York: Norton.

Jensen, A. R. (1969). How much can we boost IQ and scholastic achievement? *Harvard Educational Review, 33*, 1–123.

Jensen, A. R. (1981). *Straight talk about mental tests*. New York: The Free Press.

Jensen, A. R. (1986). The *g* beyond factor analysis. In J. C. Conoley, J. A. Glover, & R. R. Ronning (Eds.), *The influence of cognitive psychology on testing and measurement*. Hillsdale, NJ: Erlbaum.

Kamin, L. J. (1974). *The science and politics of IQ*. Potomac, MD: Erlbaum.

Karier, C. (1972). Testing for order and control in the corporate liberal state. *Educational Theory, 22*, 154–180.

Matthews, M. (1980). *The Marxist theory of schooling: A study of epistemology and education*. Sussex, England: Harvester Press.

Sanders, J. T. (1985). Marxist criticisms of IQ: A defence of Jensen. *Canadian Journal of Education, 10*(4), 402–414.

Snyderman, M., & Herrnstein, R. J. (1983, September). Intelligence tests and the Immigration Act of 1924. *American Psychologist, 38*, 986–995.

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