

Marxism and the IQ Debate: A Response to Professor Sanders

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Sanders is correct in saying in a recent issue of this Journal [10(4)] that the critical analysis of the IQ tradition is one of the chief contributions of Marxism, and more generally of revisionism, to contemporary debate about schooling and social policy. By criticizing my comments on Jensen (Matthews, 1980), he concludes that the verdict on the Marxist contribution must be largely negative: "the contribution is arbitrary, superficial, confused, misleading and mistaken," and there is in fact "no better example of ideologically inspired pseudoscience than the radical Marxist criticism ... " (Sanders, 1985, p. 413).

The breadth of his conclusion is in inverse proportion to the narrowness of his evidence. He criticizes a rich and pluralistic research program on the basis of alleged inadequacies in one chapter of an introductory text on education and epistemology. I am flattered that Sanders' spotlight passed so rapidly over the sustained contributions of Lewontin, Gould, Simon, Kamin, Karier, Block, Dworkin, Lawler, and other contributors to a broadly revisionist and Marxist program. One need not be a Humean to realise that the body ought not to be buried on the basis of illness in such a minor part. Sanders has not provided the final verdict on the Marxist contribution to the IQ debate.

Counsels of hesitancy become more urgent when Sanders uses putative errors in my passing reference to well-known revisionist historical claims to support a global claim that revisionism has to be revised. All this because he says new evidence has been unearthed concerning the role of the intelligence testers in the passage of the infamous 1924 U.S. immigration laws. The spotlighted chapter of my introductory book was hardly the place to amass the detailed history of the frightening marriage between intelligence testing and eugenics in both America and England. That some detail there *may* have been wrong hardly constitutes the overthrow of revisionism.

Sanders criticizes Marxism (as exemplified in my work) 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. This article appraises each of the claims and finds each wanting. It re-argues the essential core of Marxism and revisionism on the subject and raises the interesting critical point of why the genetic hypothesis has come to have such a strong grip upon people's imaginations. There has been considerable and detailed revisionist history written on the political positions of the leading IQ proponents (Thorndike, Goddard, Yerkes, Terman, Burt, and others). Their avowed conservative social aims were clearly advanced by their positions of power in the academy, on government committees, and in state institutions. Their political ideology *did* deform their IQ science. Their tests and theories and those of their legions of followers were used to further racist ends, to select thousands for forced sterilisation, and to condemn millions of children in England and America to dead-end, third-tier education. In broad terms, the revisionist picture is unchallenged. The most recent underpinning of it has been provided by Kevles in his scholarly and well-received *In the Name of Eugenics* (1985).

THE IMMIGRATION DEBATE

Sanders' evidence for the "revision of revision" comes from a single publication: Snyderman and Herrnstein's (1983) re-examination of the role of the major mental-testers in the enactment of the 1924 Immigration Restriction Act. They claim, on the basis of detailed readings of the record, that the first generation giants of American psychology had little influence upon the passage of the acts. If true, this would cause the revision of *one* substantial segment of the revisionist picture. However, the matter is not as clear cut as Sanders believes. One problem in this specific dispute is a frequent confusion on both sides between objective and subjective influences, between environmental and personal influences. The revisionist claim need not be advanced in subjective and personal terms. One need not assert that Goddard and Terman were frequently on the phone to their congressmen or that they actively lobbied senators. Conversely, rebuttals of revisionism based on evidence that no such phone calls or lobbying took place are equally beside the point. At this level, the dispute is biographical and, although important for personal biography, not important for political history.

At an objective, external, or environmental level, the revisionist claim is only strengthened by recent scholarship. Kamin, Gould, and Karier are all careful to state their claims in nonpersonal terms. Gould refers to the

1917–18 testing of 1.75 million army draftees by Yerkes as providing the “objective data that led to” the immigration acts (Gould, 1981, p. 157). Karier says that Terman’s studies of the racial distribution of gifted children (the most gifted from northern and western European stock, and the least gifted from black and Mexican stock) “entered into the dialogue which led to the restrictive immigration of 1924” (Karier, 1972, p. 163). Kamin, in a detailed study of this episode, says that the scientific evidence of the testers became generally known in Congress through the efforts and publications of the Eugenics Research Association and the National Research Council (Kamin, 1974, p. 378). Yerkes was a member of the latter body. Under his leadership, it set up a committee on Scientific Problems of Immigration. The first publication funded by the committee was *A Study of American Intelligence* by Carl Brigham (1923). This was an extensive re-analysis of Yerkes’ army data. Brigham concluded that immigrants from southeastern Europe were largely feeble-minded. He recognized that new immigration laws could afford a slight relief from the contamination of superior American blood but held that the real long-term solution lay with a eugenic program that achieved “the prevention of the continued propagation of defective strains in the present population” (Kamin, 1974, p. 379). Yerkes wrote a laudatory preface for this book, and its author rose to secretaryship of the American Psychological Association. This work, with that of other testers, was frequently cited in submissions to Congressional committees on immigration.

Immigration was an objective problem for the early and enthusiastic champions of an American eugenics program: there was little use in controlling the home gene-pool via sterilization and miscegenation legislation if millions of uncontrolled genes were flooding in from overseas.

Sanders claims (via Snyderman & Herrnstein) that Congress took no notice of this groundswell of scientific opinion and, further, that the testing community believed its research did not favour restrictive immigration. In a very tortured sense both claims may be true. On the first, it is a mistake for revisionism to attribute a superabundance of reasonableness to the 1924 Congress. It is a mistake to suggest that the scientific evidence caused or formed their beliefs. One could predict that they had their answers in advance of their questions. The scientific evidence was nice, a useful post-facto justification, but it is unrealistic to imagine the wheelers, dealers, ward-bosses, business tycoons, and tea-pot dome hangovers who comprised Calvin Coolidge’s Congress actually suspending their judgement while waiting upon the next instalment in the reanalysis of the army Beta test results. When Coolidge signed

the 1924 bill, he proclaimed that "America must be kept American." More than likely, he had this view in advance of the testers' impressive mathematics and scholarly research. As vice-president, he had declared that "biological laws show ... that Nordics deteriorate when mixed with other races" (Kevles, 1985, p. 97). At the time of the immigration debate, both the House and the Senate were controlled by conservative Republicans. The arguments of the eugenicists were widely quoted in debate. Robert Allen declared "the primary reason for the restriction of the alien stream ... is the necessity for purifying and keeping pure the blood of America" (Kevles, 1985, p. 97). In 1920, Harry H. Laughlin, who was superintendent of the Eugenics Record Office, was appointed Expert Eugenical Agent to the House Committee on Immigration and Naturalization. He presented impressive evidence that recent immigrants were biologically inferior and that they jeopardized the blood of the nation.

This specific issue is a matter for detailed historic study. Marxists can only welcome whatever new evidence is brought to light. However, it does not appear that the Snyderman and Herrnstein paper should cause the revision of either the general claim (that leading American psychologists contributed to the climate of opinion leading up to the immigration acts) or the specific claim (that the psychologists personally championed the acts) advanced by revisionism. Sanders says that these propositions "are now in doubt." I do not believe so.

THE WEIGHT OF HISTORY

He does go on to pose a revealing hypothetical question: "Finally, even assuming that early IQ testing was as misbegotten and morally bankrupt as the radical historians claim ... , so what? These historical circumstances have no implications for the validity and integrity of contemporary theory and research on IQ" (p. 405). I have argued strongly that they do; and, contrary to Sanders imputation, my argument is from the history of IQ science, not from theology. He says that one can only hold that the past of IQ science determines the present if one "defends a version of the Doctrine of Original Sin" (p. 405). This claim signals a very unhistorical appreciation of science and a revealingly simple understanding of just what a research program in science is and how it is developed. Above all, for the IQ research program, a failure to acknowledge its historical nature and continuity is a major error.

Unlike Sanders, Eysenck is aware of the influence of the founding fathers of his science.

Because the intelligence tests, originally constructed in the early years of this century, did such a good job when applied to various practical problems, psychologists interested in the subject tended to become technologists eager to exploit and improve these tools, rather than scientists eager to carry out the requisite fundamental research, most of which still remains to be done. (1962, p. 8)

Anastasi has provided an exhaustive and detailed account of how nearly all intelligence tests have been constructed in the shadow of the original 1916 Stanford-Binet test of Lewis Terman. It has been the norm against which the construct validity of its own subsequent revisions and of other tests has been determined. It is the most widely used, influential, and authoritative test. Concerning its own revision, Anastasi remarks that "continuity in the functions measured in the 1916, 1937 and 1960 scales was ensured by retaining in each revision only those items that correlated satisfactorily with mental age on the preceeding form" (1961, p. 204). Surveying the vast technical and the not so vast theoretical literature on IQ, she concludes that "there has been a common identification of Stanford-Binet IQ with the very concept of intelligence" (p. 209).

Given this history of dependence, one can reasonably suppose that faults at the source will affect subsequent development. It is not a theological supposition but rather a *matter-of-fact* one. This dependence is reinforced by a theoretical commitment of nearly all proponents of IQ science, their operationalism. Without an independent theory of intelligence, they cannot address the claims for construct validity of their tests: does this test measure intelligence as distinct from something else? Without theory, they need some touchstone, something conventionally agreed upon as being a measure of intelligence. All other tests will then be validated against this standard. The standard has been the Stanford-Binet.

Binet originally thought intelligence was a function of skull size. For some years he published papers on craniology. This was at the time Galton in England was measuring intelligence by reaction-time measures. Binet slowly abandoned the dominant science of craniology. By 1905, in response to his commission to identify students in need of remedial education, he had developed his battery of 30 tests. These were revised in 1908 and 1911 (the year of his untimely death). He established the tradition of operationalism among testers. He once remarked: "One might almost say, 'It matters very little what the tests are so long as they are numerous'" (cited in Gould, 1981, p. 149). Boring of Harvard in 1923 elevated this empiricism to an article of faith for the IQ tradition: "Intelligence as a measurable capacity must at the start be defined as the capacity to do well in an intelligence test. Intelligence is what the tests

test" (Boring, 1923, p. 35; cited in Block & Dworkin, 1976, p. 425). Most subsequent researchers have paid allegiance to this. Hensen and Herrnstein in the present day are avowed operationalists.¹ Wechsler, the creator of the main alternative to the Stanford-Binet, has said: "Notwithstanding their theoretical views, authors of intelligence scales tend to make use of the same sort of tasks and items. Procedures may vary, but the tests themselves do not differ very much" (1971, p. 52).

But what do the original tests measure? What is the nature of the standard? Binet asked teachers to give him their brightest and dullest students. These provided the reference groups for his test-item selection. Not surprisingly, his scales reflected an extraordinary bias towards scholastic items. Goddard took these scales to America. Terman gave them a local flavour, provided new validation against all-white reference groups of teacher-selected bright and dull students, and produced the 1916 Stanford-Binet test. In this, a child of nine was supposed to: (a) know the date, day of the week, and month of the year, (b) recite the days of the week, (c) calculate the change from 20 cents when 4 cents were taken out, and (d) read a passage and remember six items. When Yerkes utilized this test for his 1918 Army Alpha test (with subsets for number sequences, word analogies, arithmetical problems, synonym-antonym puzzles, and common-sense questions), he included questions such as: "The Knight engine is used in the — Packard Steams Lozier Pierce Arrow" (Kevles, 1968, p. 576).

In the 1960 revision of the test, the vocabulary subset asks children to define: *mosaic*, *piscatorial*, *casuistry*, *brunette*, *ambergris*, and *incrustation*. The picture-identification subset gives pictures of two women — one a neat, tidy, typically Anglo-Saxon college girl, the other an unkempt, black woman — and asks: "Who is prettier?" Clearly all kinds of assumptions, bias, and preferences — in brief ideology — are given expression in these items. Anastasi conservatively counsels that "it should be borne in mind that the Stanford-Binet is primarily a measure of *scholastic aptitude* and heavily loaded with verbal functions" (1961, p. 208). It is not surprising that IQ correlates with school success: the validation procedures ensure that it does. Burt, in his seminal 1909 hereditarian paper, consciously chose and validated his mental tests by correlating performance on them with headmasters' judgements of intelligence.² The interlocking history of dependence in the development of IQ science suggests that Sanders is mistaken in his claim that current theory and research are wholly independent of what preceded them. Research programs just do not work that way: the child is the father of the man.

Sanders acknowledges the oft-pointed-to cultural and scholastic bias of test items but says this is not a criticism of *construct validity* but only of

face validity (p. 409). This is a peculiar distinction to introduce; it has "non-scientific" written all over it. No one should worry about face validity; indeed, in my reading of psychological literature, I do not think many people have. It is an utterly subjective notion: face validity of items and tests will, by definition, depend entirely upon the readers' own conceptions and expectations of intelligence, which themselves may be changeable. The only argument about construct validity is and has been: are the items and tests measuring intelligence? How can performance be explained? This depends upon an objective, agreed upon, external theory of intelligence. One could have such a theory in which knowledge of what car the Knight engine was used in indicates intelligence, but manifestly it is going to be unable to eliminate numerous and simple alternative explanations. Boring, years after Yerkes' testing, said it was "preposterous" to suggest that the Army Alpha test measured native intelligence (Kevles, 1968, p. 576).³

Sanders tries further to rescue the agreed culture-biased items by saying, with Vernon, that these have survived retesting because they have been shown to correlate highly with good overall test performance (p. 409) and conversely that those with overall low scores have been found uniformly to lack knowledge of the car a Knight engine was used in or the definition of *casuistry*. Thus, they are claimed to be unexpected, but good, indicators of intelligence. But this clearly begs the question. It might just indicate that the rest of the test is as culturally biased as the acknowledged items, that the test is measuring neither native intelligence nor ability to learn but rather what has been learnt. This, in turn, is a function of many things: school attended, books in the home, dinner-table conversation, eyesight, parental income, competing nonacademic attractions, and cognitive abilities.

Marxists and radicals are interested in IQ science partly because it has so dramatically come out of the ivory tower. It has always interlocked with social and practical concerns. In England, all children at 11 years of age were subject to a version of the Stanford-Binet test. Their performance streamed them for the remainder of their education into three different types of school systems: grammar, secondary-modern, and trade. This, in turn, determined their adult job circumstance, where they lived, how much they earned, what kind of life was possible, and how they thought of themselves. Millions of children in the western world have been so streamed. Thousands of adults have been sterilized on the basis of test results: 7,500 in Virginia between 1924 and 1972. All kinds of things were thought to be ultimately traceable to intelligence. Terman, in a canonical work in the IQ program, said "moral judgement, like business judgement, social judgement or any other kind of higher thought process, is a

function of intelligence" (1916, p. 11). This justified incarceration and sterilization of those judged morally wayward – drunks, prostitutes, thieves, and so on. The most recent advance of IQism was specifically aimed at dismantling American interventionist programs in education. Jensen's landmark 1969 paper was concerned with explaining the apparent failure of compensatory education in terms of the centrality and fixity of intelligence (Jensen, 1969). A great deal hinges on the outcome of the IQ debate. Most generally, IQ science has buttressed the meritocratic doctrine: Those who succeed do so because of their merit. Burt attributed the English class stratification to the differential distributions of intelligence (1955/1967, p. 281). One of his colleagues Richard Lynn maintains that low innate intelligence is the chief reason why people live in slums (Cox & Dyson, 1975). Jensen quotes favourably E. L. Thorndike's view: "In the actual race of life, which is not to get ahead, but to get ahead of somebody, the chief determining factor is heredity" (Jensen, 1973a, p. 28). The social origins, and function, of meritocracy as a political doctrine have been extensively written upon (see Chase, 1977; Ludmerer, 1972). It is a pity that Sanders did not engage more generally with the issue. His comments and chosen quotations cast the debate in personal terms. This is not the issue. The radical argument is about the objective structure of IQ theory and the external, social function of the testing industry. My own effort was not concerned with proving "guilt by remote association" or with "ridiculing" (p. 406) the early psychologists; it was concerned with showing how political preferences flowed into scientific endeavour and how ideology constrained and distorted theory. The motives and moral standing of the principles is a separate issue. I tried to make this point clear in the chapter, "Ideology," in my own book. The Marxist contribution to the debate has been to indicate that the IQ research program and its associated mammoth testing industry have exemplified a communal flight from understanding which has been supported by significant aspects of the texture of twentieth-century capitalism.

JENSEN ON GENERAL INTELLIGENCE

So much for the first part of Sanders' argument. After rehabilitating the early psychologists and separating the current ones from them, Sanders claims Gould and I seriously misrepresent Jensen's view of the ontological status of *g*; that is, we misrepresent his theoretical interpretation of intelligence. Here Sanders has helpfully drawn attention to writings of Jensen that do amplify his analysis of 1969 and 1979. (These writings, I might add, were published two years *after* my own book; I might be

forgiven for not referring to them.) However, I cannot see how the new writings substantially alter the situation. Sanders admits almost as much.

Gould and I had accused Jensen of unjustifiably reifying g , of treating an intervening variable as a hypothetical construct, and of giving an existence to a mathematical relation. Additionally, I had accused him of equivocation on this matter. Clearly, Jensen believes in Spearman's unitary g factor of intelligence: all his writings testify to this. He even sees g "as an interspecies concept with a broad biological base culminating in the primates" (1979, p. 251). Repeatedly, g is invoked to explain test performance; it is the very essence of intelligence: "we can identify intelligence with g . To the extent that a test orders individuals on g , it can be said to be a test of intelligence" (p. 224). It is ascertained as the explanation for the mean difference in IQ scores between blacks and whites (p. 535). Having so taken Spearman's g into the heart of his theory of intelligence, the question arises as to how this theoretical concept is interpreted.

I admit the correctness of Sanders' criticism that in statistics expressions such as *accounting for* or *explaining* the variance of a set of scores do not necessarily imply a causal analysis. I was wrong in making such a direct attribution to Jensen. Jensen does later, in his 1982 article, say that g is just one of a number of mathematically equivalent factor analyses of the data. This is the point made by Osiander in his preface to Copernicus's *On the Revolution of the Heavenly Spheres*. Later, Bellarmine urged the same point in interpreting Galileo: there are many mathematically equivalent ways of "saving the appearances," as the medievals used to say. This essentially is the crux of Gould's criticism of Spearman, Burt, and Jensen. But, having admitted this, Jensen then goes on to say (as did Galileo) that there are "non-mathematical considerations which must determine preference for one method over the other" (1982, p. 132). He proceeds to list these: positive correlations exist between widely different cognitive tests, there are greater *between*-individual differences than *within*-individual differences in test performance, differences in g accord with common-sense estimation of intellectual differences, and g correlates significantly with reaction-time measure. Sanders concurs that these provide reasons for "imbuing g with additional or surplus meaning beyond its austere statistical definition" (p. 408). This means, in brief, that g is given ontological status. This is precisely what, at the outset, Gould and I said. Sanders seems unwillingly to bite the bullet, and he retreats to fence sitting. Having listed the above reasons, he says they

do entitle Jensen and others to conceive of g as a hypothetical construct – as something more than *just* a shorthand label for a computational algorithm, but still well short of the crude thinghoods implied by both Matthews' and Gould's charges of reification. (p. 408)

Well, after all the huffing and puffing, it does seem that we are back where we started. I cannot follow Sanders down the ontological trail to half-way (almost) existences. If g isn't understood as an intervening variable, a mathematical device, then it is to be understood as having a real reference. This is reification. It is no defense to say that Jensen is having only a little bit of reification.⁴

My argument is not premised upon reification as theoretically sinful *per se*; my own book is in part an argument for ontological and theoretical realism in science and against empiricism and behaviourism in epistemology. Realists are quite happy with theoretical terms which refer to currently unobservable entities, even entities which are in principle unobservable (e.g., force). The issue is whether the reification is warranted or unwarranted. Sanders is correct to say, after Ericson, that reification is not a fallacy for realists. Agreed. But it does not follow that every reification is legitimate. The history of science is littered with the corpses of discarded reifications (e.g., phlogiston). Faculty psychology provided scores of such corpses. Gould and I are saying that the unitary theory of intelligence, with its postulated g , is just one more such case.

THE HERITABILITY OF INTELLIGENCE

Sanders' final criticism of me and defense of Jensen centres upon the heritability of IQ. He makes three claims:

1. I am mistaken in saying that Jensen believes IQ is 80% heritable.
2. Jensen does not equivocate between common-sense and scientific senses of heritability.
3. Jensen does not use a linear model for the analysis of the contribution of different variables to intelligence (pp. 409 ff.).

Most people will be aware that heritability has provoked intense debate. At issue are, on the one hand, specialised points in population genetics and, on the other hand, matters of social policy and educational funding.

Jensen does at many points refer to IQ being 80% heritable. In this, he is at one with a long lineage of hereditarians, including the oft-quoted figure of Cyril Burt. Herrnstein uses it in his classic *IQ in the Meritocracy*, where he says that IQ variance owes nothing to covariance of genetic and environmental factors and everything to genetic variance (80%) and environmental variance (20%) considered separately (1973, p. 183). In a 1970 study of identical twins, Jensen says that "0.824 may be interpreted as an upperbound estimate of the heritability of IQ in the English, Danish, and North American populations sampled in these studies" (1970, p. 133). Again, in his reply to Lewontin, Jensen says "I simply find that 0.81 turns out to be the average heritability value based on all

data which has been reported in the literature, and I have made a most thorough survey" (1976, p. 103).

Sanders does well to point out the numerous passages where Jensen distinguishes heritability as a *population concept* from genetic determination as an *individual concept*. He does indeed know the difference: I have said as much in my own book. "Heritability and genetic determination are two completely different notions. ... Jensen is aware of this, and goes to considerable pains to arrive at his 80% figure" (p. 145). I may have erred in saying that he moves too easily between the technical and the totally different common-sense meanings of *heritability*. Certainly many followers of his do so. Jensen, in other areas, is prone to commonplace talk which only confuses the scientific debate. He does use "dumb" as a synonym for "unintelligent" and "bright" and "smart" as synonyms for "intelligent." And he does reproduce approvingly the amazing claim of E. L. Thorndike that "in the actual race of life, which is not to get ahead, but to get ahead of somebody, the chief determining factor is heredity" and candidly adds that "since then, the preponderance of evidence has proved him right, certainly as concerns those aspects of life in which intelligence plays an important part" (1973a, p. 28).⁵

This is a prime example of the mistaken application to *individual* life-history of what is essentially a population *statistic*. Most readers could be excused for thinking that Jensen had disregarded his own cautions on the subject. It is a prime statement of the meritocratic doctrine, a doctrine Jensen forcefully advocates.

My third supposed error is to have accused Jensen of using a linear model for his analysis of the contribution of genetics and environment to the IQ variance in a population. Sanders commences by saying that this model does not "even remotely capture the general sense of Jensen's heritability analysis" (p. 410). However, one page later, concerning the interactive contribution of genetics and environment to IQ variance, he says that "Jensen's linear statistical model is capable of detecting this possibility." On the very next page, he interprets Jensen as saying that "most of the IQ variance can be explained by the additive contributions of genetic and environmental variance; changes in the environment simply tend to add (or subtract) to the phenotypic value of the IQ for the various genotypes." In other words Sanders himself attributes to Jensen precisely the same model which he accuses me of mistakenly having taken Jensen to use.

The linear model is quite simply a mistaken model for the analysis, although it is one which does allow the kinds of conclusions which Jensen seeks from his data. Heritability is a highly technical matter combining important and reasonably complex aspects of statistics and genetics. It is

not a subject to be treated in commonsense, throw-away lines – and all the more so when crucial matters of public policy are at stake.

Heritability says nothing about how much given parents genetically pass on to their offspring. For a given measure, it is the ratio of variance due to differences between genotypes to the total variance of the measure in the given population; it tells us what fraction of the variance of a particular phenotype in a population can be accounted for by genetic differences in that population.⁶ Thus, the concept of heritability is applicable only for a given characteristic in a particular population in a given set of environments. For a given characteristic (phenotype or trait), there may be different genetic variation in different populations. A given range of environments may have little effect on phenotypic expression, but a different range of environments may have a marked effect on such expression. A heritability figure of itself does not convey a great deal, certainly nothing that complex social policy ought be predicated upon. As I pointed out in my book, 100% heritability is consistent with *no* genetic determination (no phenotypic variation for the selected measure); zero heritability is consistent with total genetic determination (no genotypic variation). Heritability analyses always come with a host of assumptions, all of which have to be made explicit and vindicated *before* any meaning can be given to the raw figure. In the most controlled circumstances and with objective metric phenotype measures, these assumptions can rarely be met. For something as gross and conventional as an IQ score, the assumption would have to be that *no* sense can be made of a heritability figure. Lerner has surveyed heritability estimates for phenotypic traits in poultry – body weight, egg weight, shell thickness and so on. The estimates span ranges up to 50% (Lerner, 1968; see also Layzer, 1976). In a later study, heritability estimates for calf weights of dairy cattle range from 70% when based on separated monozygotic twins to 20% when based on half sisters (Layzer, 1975). Clearly, one should be hesitant in advance about claims for heritability of IQ. Further, one should appreciate that talk of differences in phenotypic measures *between* populations (IQ scores across races) as largely heritable is conceptual nonsense: heritability is a measure *within* a population.

Heritability figures require three conditions to be controlled: genotype-environment correlation, genotype-environment interaction, and gene-gene interaction. The most rigidly controlled laboratory studies of animals and plants give little confidence that these can be controlled. It is recognized by all that, given IQ is a suitable phenotypic measure, studies of monozygotic twins will provide the best estimate of heritability. Here the paired individuals have identical genes and are of the same sex and age. Any differences which exist between them must be attributed to

non-genetic factors or interactive ones. This is why twin-studies have occupied centre stage for so long in the IQ debate. If they are flawed, then most of the evidence for the heritability of IQ is removed. They are so flawed. For the sample of twin studies to yield information about heritability in the population at large, the twins will need to be representative of genetic variation in the population, they will need to be exposed to environments which are characteristic of *cognitively-relevant* environmental variation in the population, and, finally and crucially, there must be no tendency for the environments in which the twin pairs are raised to be systematically correlated. The work of Kamin, Lewontin, Layzer, Goldberger (1976), and others has demonstrably shown that these minimum conditions have not been met in the published studies utilized by Jensen and other hereditarians. In Burt's famous and much referred to 1966 study of 53 pairs of twins (1966), one member of each pair was raised by his or her natural parent. This provides at the outset a genotype-environment interaction which undermines the heritability estimate. Kamin has drawn attention to a considerable number of other major procedural flaws in this particular study which was conducted over a period of 45 years (1974, pp. 40–47). After reviewing all the major sources for the heritability claim, Kamin concluded they all demonstrated profound environmental effects and that the apparent genetic effects were always compounded with the environmental factors.

Most readers of the literature would agree that hard scientific evidence for the genetic hypothesis is not yet available and that what evidence there is is severely flawed. Jensen, in a widely circulated popular piece, frankly admits this much-laboured point: "the genetic hypothesis has not yet been put to any direct tests by the standard techniques of genetic research" (1973b, p. 81). Yet he stridently advocates the hypothesis, supposedly for two reasons:

1. because the default of the environmentalist theory, which has failed in many of its important predictions ...
2. since genetically conditioned physical characteristics differ markedly between racial groups, there is a strong *a priori* likelihood that genetically conditioned behavioural or mental characteristics will also differ. Since intelligence and other mental abilities depend upon the physiological structure of the brain, and since the brain, like other organs, is subject to genetic influence, how can anyone disregard the obvious probability of genetic influence on intelligence. (1973b, p. 81)

This paragraph perhaps reveals what is at the heart of Jensen's error: he imposes a choice between environmentalism and genetic determination and argues that, because the first is inadequate, the second *must* be

accepted even without a strict test. But must one accept the initial choice? Marxists and revisionists have answered *no*. It is extremely rare that a single gene has a phenotypic expression: the most basic physical characteristics result from numerous genetic interactions. Further, the phenotypic expression is the result of the history of interaction between the genetic ensemble and the successive environments through which it passes. To attribute behavioural and mental characteristics to a genetic base is just sociobiological dogma. Biology influences human affairs, but it rarely determines them. The appropriate explanatory model has to be an interactive one, not a dichotomous choice. The brain is the seat of human intelligence, but the striking array of intelligent human achievements and diverse cultures cannot be traced back to differences in brains. Culture, education, nutrition, child rearing, and work all play their specific roles. Marx and Engels argued this sort of case against the early social Darwinists: the latter had neglected the role of productive labour in the evolution of man; men's environment is not just natural, but humanised as well.

IQ-ISM AS IDEOLOGY

The final contribution of Marxists to the IQ debate is to suggest why the genetic hypothesis gained the support of so many luminaries whilst it was based on so little. Beyond revealing the error, criticism must try to explain it. Proper criticism requires an understanding of why erroneous views of the world come to be held. Marx set about this task in his evaluation of the classical political economists and, most famously, in his critique of religion.

For the genetic hypothesis, the broad outlines of a proper criticism are clear enough. With capitalist development, man's control over his most important affairs is progressively reduced, and he is alienated from his own productive powers and from the social means of production. With only labour power to sell, the determining forces of his life are beyond his control. The genetic hypothesis – which in its full-blown state attributes morality, pauperism, wealth, and class-position to genes – locates the source of intelligence within the skin of the individual and beyond his effective control. In this way, it is consistent with the pressing experience of the rest of his life. Not only is it individualist, but it is fatalistic. Compensatory education fails because it is up against the solid wall of nature. The genetic hypothesis theorizes that there is an alienated experience of life and, at the same time, justifies it.

Sanders is free to see the Marxist contribution to the IQ debate as the ultimate example of ideologically inspired pseudoscience. Marxists

will make just such a claim about his and other expositions of the genetic hypothesis. Marxists may be wrong on many things, but on this matter they would appear to have both humanitarianism and evidence on their side.

NOTES

- ¹ For a discussion of this point, see Block and Dworkin, 1976, pp. 424 ff.
- ² This paper is discussed in Gould, 1981, pp. 274 ff.
- ³ Although Boring was an administrator of the army tests, he urged caution in their interpretation (see Snyderman & Herrnstein, 1983).
- ⁴ The *focus classicus* for discussion of intervening variables and hypothetical constructs in psychological theory is MacCorquodale and Meel, 1948. See also Maze, 1954, and Rozeboom, 1956.
- ⁵ As Lewontin points out (1974, p. 84), Thorndike originally made this claim in 1905, years in advance of anything much being known of genetics.
- ⁶ For a collection of measures of a given trait in a given population, the *mean* is the average of these measurements, while the *variance* is the average of the required differences between the individual measurements and the mean. Variance measures the *spread* of individual measurements in a population.

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