

Marxist Criticisms of IQ: A Defence of Jensen

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Radical Marxist critics have charged that IQ testing and IQ research, as represented by the work of Arthur Jensen, are either disguised racist ideology or pseudopsychological science. This article argues that the historical evidence marshalled in support of the first charge is both selective and irrelevant, and that the technical arguments advanced to support the second charge that IQ research is pseudoscience reflect both serious misunderstandings and misrepresentations of Jensen's views on the nature and heritability of IQ.

Des critiques marxistes extrémistes ont déclaré que les tests et la recherche sur le Quotient Intellectuel (QI) qui se trouvent dans l'œuvre d'Arthur Jensen sont basés sur une idéologie raciste ou sur des sciences pseudo-psychologiques. Cet article démontre que la preuve historique de la première opinion est à la fois sélective et sans fondement et les arguments techniques présentés pour appuyer la deuxième opinion – que la recherche sur le QI est une pseudo-science – démontrent à la fois une misconception et une incompréhension des idées de Jensen sur la nature et le caractère héréditaire du QI.

This article is based on a recent book by Michael Matthews (1980), *The Marxist Theory of Schooling*. In particular, it is based on a single chapter in the book that addresses "The IQ Controversy." My purpose is to examine and rebut a series of arguments advanced by Matthews in this chapter; taken together they constitute the current Marxist case against IQ theory and research.

Although open to accusations of undue and arbitrary narrowness in short-circuiting the voluminous literature on the IQ controversy, I defy the accusations. There is no lack of literature on the controversy; if anything, there is an overabundance. And considering the buzzing, blooming polemical confusion that characterizes much of this discursive literature, Matthews' circumscribed case against IQ does have certain virtues. It is current (or post-Burtian), sustained, and organized around the major technical issues that make up the core of the controversy. In addition, many of the arguments against IQ that Matthews presents are unique neither to him nor to a Marxist perspective. Rather they are, like old popular songs whose copyright has expired, in the "public domain."

For the most part, then, Matthews' criticisms of IQ broadly represent the radical, critical literature on IQ – both Marxist and non-Marxist alike.

Another circumstance worth mentioning lends particular importance to Matthews' chapter on the IQ controversy. This is the controversy's special status within his larger enterprise – The Marxist Theory of Schooling. For Matthews as well as for other Marxist critics (e.g., Bowles & Gintis, 1972/73; Karier, 1972; Simon, 1978), the debunking of *IQ science* clearly represents the crowning achievement of the Marxist analysis of education. It has become the example, par excellence, of the explanatory power and promise of the Marxist perspective on education. As Matthews instructs, "the examination of this controversy enables us to concretise and illustrate some of our central motifs" (p. 133).

THE TWIN INDICTMENTS

For Matthews, as for most participants in the controversy, *IQ argument* or *IQ research program* refers essentially to the views of Jensen (1969; 1973a; 1973b; 1973c; 1982) on the nature, heritability, and social importance of IQ. But, as I hope to show, Jensen's views are often either wrongly characterized or seriously misunderstood. In general, the radical opponents of IQ science have attempted to substantiate two separate, but related, accusations against this IQ research program. One general accusation is that IQ theory and research are basically ideology dressed up as differential psychology or racist politics in scientific drag! Kamin (1974) is perhaps the best known proponent of this view which he details in *The Science and Politics of IQ*. Similarly, Bowles and Gintis (1972/73) maintain that IQ testing is essentially disguised ideology and, to underscore their point, simply call the entire enterprise *IQ-ism*.

The other general indictment contends that IQ theory and research are essentially pseudoscience. This second charge typically claims that IQ research, especially Jensen's, is not much more than a bogus mixture of the arcane statistical technique of factor analysis and a few misappropriated concepts from population or quantitative genetics. Indeed, Gould (1981), in his widely acclaimed book, *The Mismeasure of Man*, argues that the measurement of IQ is only a surviving descendant of the crackpot "sciences" of craniometry and criminal anthropology.

These contentions, that the theory and measurement of IQ are either ideology or pseudoscience, are obviously not mutually exclusive. Indeed, the two claims support one another. That is, if one can show that IQ testing is nothing more than disguised ideology, then this helps to dispose of it in the dustbin of pseudoscience. Conversely, if one can show that IQ testing is premised upon obviously pseudoscientific concepts and

methods, then the charge that it serves covert ideological purposes surely becomes much more plausible. But while these twin indictments of IQ research do bolster one another, each depends upon different kinds of evidence and argument for its own independent support.

HISTORICITY OR ORIGINAL SIN?

The IQ-as-ideology charge depends upon a revisionist social history of intelligence testing. The importance of historicity, as Matthews explains, derives from "one of the central Marxist–Hegelian claims that the understanding of an object, circumstance or institution involves examining its history. ... Thus when faced with an IQ testing industry, we should initially seek to disclose its history" (p. 152). And an important part of this history is the biographical evidence that the psychologists and psychometricians who were prominent in developing IQ testing held either blatantly or latently racist views.

Matthews' summary of the history of intelligence testing is relatively brief but contains what are now staple items in the Marxist historical record testifying to the ideological, especially racist, roots of IQ or intelligence testing. Thus, Matthews reminds us that Francis Galton, the father of intelligence testing, was also the founding father of the British Eugenics Society in 1908. He retells Kamin's story of how Henry Goddard used intelligence to screen European immigrants coming into the United States and supposedly concluded that "the majority of Jews, Hungarians, Italians and Russians were feeble-minded" (p. 136). And finally, Matthews brings us around to the "Watergate of intelligence theory," the recently discovered fraud of Sir Cyril Burt (Hearnshaw, 1979).

The lesson of historicity in this case is plain. The sordid past of intelligence or IQ testing continues to contaminate its present. The long road from Binet to Burt is, in Matthews' words, "a history of the constant, blatant use of so-called IQ tests to further the process of ruling class control and hegemony over racial groups and the working classes" (p. 152).

However superficially compelling this now familiar historical indictment of IQ testing may be, several objections can be offered. First, some of the historical "facts" themselves are now disputed. Snyderman and Herrnstein (1983) recently re-examined in detail the frequent claim that the passage of the racially biased U.S. Immigration Act of 1924 was aided and abetted by the intelligence testing community. And, contrary to this claim by the radical or revisionist historians of IQ, they conclude that

the testing community did not generally view its findings as favoring restrictive immigration policies like those in the 1924 Act and Congress took virtually no notice of intelligence testing, as far as one can ascertain from the records and publications of the time. (p. 986)

Snyderman and Herrnstein's reading of the historical record even manages to rehabilitate Goddard. On their account, Goddard "... said explicitly that his study 'makes no attempt to determine the percentage of feeble-minded among immigrants in general or even of the special groups – the Jews, Hungarians, Italians, and Russians'" (p. 987). Thus, to some extent, to borrow historian Diane Ravitch's phrase, the revisionists are being revised.

Second, it seems to me that much of what passes for incriminating historicity in Matthews' analysis never gets beyond sinister innuendo. For example, Matthews writes that "... intellectual production does not occur in a vacuum. IQ theory has been produced out of, and for, the school system" (p. 134). This is certainly true; IQ tests were originally developed for use in the school context. This fact about the origins of IQ testing, however, is presented as if it established self-evident guilt by association. Another example: in a section entitled *Handmaidens of Industry*, Matthews points out that

Industrial Psychology was taking wing at the same time as IQ testing; indeed, there was a considerable mixture of flocks. E. L. Thorndike developed testing programmes for large insurance companies; L. L. Thurstone worked for Westinghouse and Carnegie Steel ... (p. 138)

Again, I assume this to be true. But such facts, detached from Matthews' insinuating historicity, are hardly damning in and of themselves.

But finally, even assuming that early IQ testing was as misbegotten and morally bankrupt as the radical historians claim (a proposition now clearly in doubt), so what? These historical circumstances have no implications for the validity and integrity of contemporary theory and research on IQ – unless, of course, Matthews defends a version of the Doctrine of Original Sin. Indeed this seems to be the case when one finds Matthews explaining that "this thumbnail sketch of the milieu out of which the IQ movement emerged has been given because the child is father to the man" (p. 138). Even more revealingly, Matthews concludes

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. (p. 152)

Historicity in Matthews' analysis is reduced to guilt by remote association. But as Jensen (1982) observes:

In distant retrospect, the early history of every science often looks bizarre in some respects. ... Should we ridicule the early astronomers for claiming that the earth is the center of the universe, or the early anatomists for claiming that the heart is the seat of emotion? Why should anyone demand of psychology that it be hatched fully mature and perfect at its very beginnings? (p. 125)

THE MEANING AND MEASUREMENT OF IQ

Following a brief historical indictment of IQ as ideology, Matthews turns to consider what he claims to be the scientific shortcomings of IQ testing. In terms of the earlier distinction, this part of the critique is intended to demonstrate that the IQ research program (apart from its ideological taint) is just plain bad science. One of the fundamental criticisms advanced is that the concept of IQ is based upon (and therefore muddled by) a particularly vulgar form of realism: "The common account of intelligence is a *realist* one; the noun 'intelligence' refers to a real existing ability ..." (p. 139). The assumption that IQ is an actual entity occupying space is, Matthews claims, both wrong and mischievous. Substantially the same charge is made by Gould (1981) in his *The Mismeasure of Man*. In a chapter entitled "Cyril Burt's Real Error," Gould insists (with arch forgiveness) that Burt's real crime was *not* his intellectual fraud, but rather his reification of Spearman's *g* – the statistical proxy for tested intelligence.

Matthews contends that those on the hereditarian side of the IQ debate, and especially Jensen, have fallen into ontological error because they have chosen to conceive of measured intelligence or *g* as a hypothetical construct rather than as an intervening variable – as Matthews would prefer. Matthews introduces the MacCorquodale-Meehl distinction as a kind of ontological *Catch-22*, that is, you posit *g* *either* as a hypothetical construct *or* as an intervening variable. If you opt for the former, you have committed the sin of reification. If you opt for the latter, that is, for *g* as an intervening variable (as Matthews believes you must), then you have, in effect, admitted that *g* or Jensen's "intelligence" is nothing more than a name or a label for the statistical relationships that it conveniently summarizes. Intelligence or *g* "... is merely a shorthand way of saying that in certain circumstances people will regularly be found to act in certain ways; it expresses a relationship rather than referring to an entity" (p. 139). It has no existence apart from its computation – no meaning beyond its empirical reference!

Matthews correctly notes that Jensen does consider and call *g* a

hypothetical construct, but Jensen most certainly does not commit the sort of crude ontological error both Matthews and Gould claim that he makes. Neither Jensen nor anyone else reasonably familiar with factor analysis believes that a craniotomy is likely to reveal a pulsating g or a blood-spattered IQ! Indeed, as Jensen (1982) reminds us, “the most sophisticated discussion of the whole issue of the meaning of factors to be found in the entire literature is Burt’s (1940) chapter entitled ‘The Metaphysical Status of Mental Factors’” (p. 130) in which the fallacy of reifying hypothetical factors is explicitly discussed.

Matthews, however, asserts that Jensen’s own statement of his position on the nature of g contains “... three contradictions within two sentences” (pp. 139–140). Here are the two sentences taken from Jensen’s *Genetics and Education*: “We should not reify g as an entity of course, since it is only a hypothetical construct intended to explain covariation among tests. It is a hypothetical source of variance (individual differences) in test scores” (Jensen, 1973c, p. 77). Matthews then summarizes the contradictions he sees in this statement:

- (i) If g is not reified it is an intervening variable, not a hypothetical construct.
- (ii) If g is not reified it does not explain the observed covariation but merely names it.
- (iii) Nothing can be the causal source of variance unless it has real existence. That is, for g to be a source it has to be reified. (p. 140)

But here Matthews seriously misunderstands Jensen. Jensen uses the words *explain* and *source* in the technical statistical sense. Statistical shoptalk also includes the phrase *accounting for* the variance. However, these phrases (source of, explain, account for) neither denote nor necessarily imply causal claims – and no one familiar with their statistical meaning thinks that they do. Explaining or accounting for the variance in their strict statistical senses mean something like *reducing the data* or *resolving the information*. For example, one might say analogously that the mean and standard deviation explain or account for a set of normally distributed scores. Jensen is clear that g is not understood as a thing or entity and his use of certain statistical expressions does not commit him to such an ontology.

Matthews’ introduction of the distinction between hypothetical constructs and intervening variables does, nevertheless, help establish what seems a persistent source of misunderstanding in the IQ controversy. Both Matthews and Gould contend that Jensen’s working theoretical notion of intelligence, that is, Spearman’s g , is *nothing but* a mathematical summary or resolution of the variation in measured IQ. And Gould’s (1981) extended (and admittedly lucid) explanation of the technique of

factor analysis is intended to make very clear just *how* arbitrary this *general factor* or *g* solution of the covariance is. He takes pains to demonstrate why *g* represents only one of many ways that the correlation information might be resolved, and hence why *g* as a mathematical abstraction cannot claim to be superior to other factor analytic solutions to or resolutions of the mental test data. It is, then, the arbitrary mathematical origin of *g* that both Gould and Matthews regard as proof of its fictional or nominal status as an intervening variable.

But Jensen (1982) too acknowledges that

no knowledgeable factor analyst ... disputes the fact that the various methods or models of factor analysis are all *mathematically* equivalent in their ability to "account for" the matrix of intercorrelations. Other non-mathematical considerations must determine preferences for one method over the other. (p. 132)

(Notice, by the way, Jensen's non-causal use of "account for.") And Jensen (1982) has recently offered a number of good reasons for preferring the factor solutions that yield *g* and for imbuing *g* with additional or surplus meaning beyond its austere statistical definition. First, positive correlations exist among all tests in the cognitive domain regardless of their diverse content and formats. Second, *g* best represents the fact that ability differences *between* individuals are greater than the ability differences *within* individuals. Third, differences in *g* accord with common-sense estimates of intellectual differences. Fourth, recent research has also shown that psychometric *g* significantly correlates with reaction time measures (Vernon, 1983). Such reasons 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 thinghood implied by both Matthews' and Gould's charges of reification. Furthermore, as Ericson (1983) argues:

Reification – the acceptance of unobservable entities as really existing "out there" – is not a fallacy for the realist. To the contrary, realists explain the success of scientific theories on the grounds that these theories have happily invoked theoretical terms that do refer to unobservable entities out there. (Unsuccessful theories fail because the terms do not so refer.) Reification is only a fallacy in a scientific non-realist framework. So construed, it is the treating of unobservables, such as intelligence, as more than a logical construction of empirical data. (p. 323)

In addition to the charge of reification, Matthews' other major criticism of IQ or psychometric intelligence purports to be directed at the *construct validity* of IQ tests. Instead, what he and many radical critics criticize is the *face validity* of IQ tests, that is, whether certain items give the *appearance* of measuring what they are supposed to be measuring – in this case, IQ or

intelligence. Opponents of mental testing have long enjoyed assaulting the face validity of test items. Such assaults, however, consist of little more than choosing a few test items which, on the *face* of it, appear trivial, silly, ambiguous, out-dated, or culturally biased, then posing the rhetorical question, "Can this be a valid test?!" But the question is *not* rhetorical. And its answer, more often than not, is "Yes!" – that is to say, face validity may or may not affect a test's empirical validity. As P. E. Vernon (1979) points out:

Almost all published intelligence tests contain particular items that appear to be defective in various ways. ... The critics usually ignore the fact that in any good test the items have been tried out beforehand and the apparently unsuitable ones actually have been answered correctly more often by those with high total scores (or high M.A.'s) than by those with lower scores. (p. 18)

THE HERITABILITY OF IQ

Perhaps the most contentious issue within the IQ controversy is the claim (now closely associated with Jensen) that intelligence is highly heritable. The claim is often misleading quantified to read: Intelligence is 80% inherited! Matthews hopes to persuade us that this claim is wholly without scientific merit.

There is no doubt that this heritability estimate is at once both the most hotly disputed and the least understood claim in the entire literature on the IQ controversy. Matthews is no doubt correct in attributing the source of the dispute and confusion to the fact that "... the scientific notion of heritability and the commonsense one have *nothing* to do with each other" (p. 145). However, Matthews charges that Jensen vacillates, as suits his purpose, between the notions of scientific *heritability* and common-sense *genetic determination*. According to Matthews, "Jensen moves back and forth between these two unrelated senses: he gets the science wrong and then uses the common-sense notion to support his policy recommendation" (p. 145). Matthews, however, provides no evidence or examples of these devious equivocations.

But does Jensen "get the science wrong"? As Matthews admits, Jensen is aware that "heritability and genetic determination are two completely different notions" (p. 145). Moreover, Jensen, as I think any fair reading of him will confirm, has both carefully explicated and consistently emphasized the technical meaning of heritability. As Matthews must surely know, Jensen (1969) has repeatedly cautioned that "heritability is a population statistic, describing the relative magnitude of the genetic component in the population variance of the characteristic in question" (p. 42); that "it has no sensible meaning with reference to a measurement

or characteristic of an individual" (p. 43); that "... it is not a constant like π or the speed of light" (p. 43); that "estimates of H [heritability] are specific to the population sampled, the point in time, how the measurements were made, and the particular test used to obtain the measurements" (p. 43); that "high heritability by itself does not necessarily imply that the characteristic is immutable" (p. 45); and so on. As these selected quotes from his original *Harvard Educational Review* article clearly show, Jensen (unlike many of his critics) was always scrupulous about spelling out what he means by *heritability* and the important differences from common, but misleading, notions of *inherited*.

In short, Matthews offers no evidence for his allegation that Jensen somehow trades upon a systematic ambiguity between the technical and commonplace meanings of heritability to support his own policy recommendations. In fact, contrary to this gratuitous allegation, much evidence exists, throughout Jensen's published work, of his persistent efforts to forestall and dispel the erroneous common connotations frequently attached to heritability in the technical sense.

However, what Matthews seems to imply in his charge that Jensen "gets the science wrong" are inadequacies in the statistical model upon which Jensen's genetical analysis of IQ variance is based. As briefly as possible, Matthews contends that:

Jensen et al. use a *linear statistical model* for analysis of the contributions of different variables to intelligence. For an individual

$$I = M + H + E$$

where I = intelligence; M = mean intelligence for population; H = variation due to heredity; and E = variation due to environment. 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 the product of this ongoing interaction. (p. 146)

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. Moreover, Matthews' formula (lest it is assumed to be) is *not* Jensen's nor does this formula (overlooking its simplicity) even remotely capture the general sense of Jensen's heritability analysis. In particular, as Jensen has reminded us, heritability "has no sensible meaning with reference to a measurement or characteristic of an individual." Even more emphatically, Jensen (1969) has cautioned that "there is no way of partitioning a given individual's IQ into hereditary and environmental components. ... This is, of course, nonsense" (p. 42).

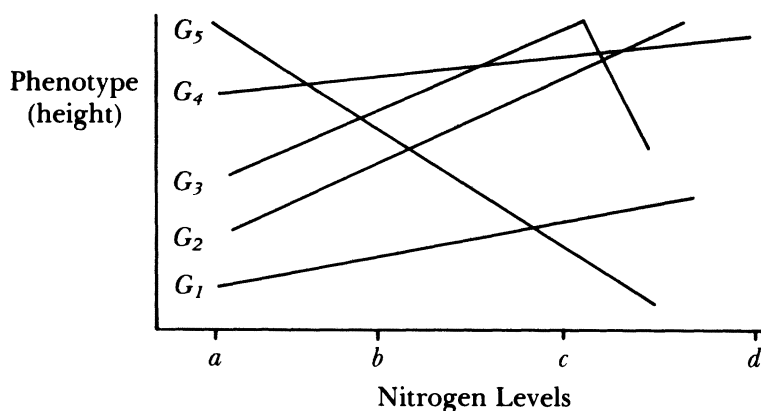


FIGURE 1

NOTE: From "IQ Heritability and Human Nature" by Norman Daniel, 1976, *Boston Studies in the Philosophy of Science*, 32. Reprinted by permission.

Finally, the oft-repeated criticism (here echoed by Matthews) that Jensen fails to consider the *interaction* between heredity and environment *does*, ironically enough, trade upon two very different notions of interaction that "have *nothing* to do with one another." In this instance, it is Matthews, not Jensen, who "moves back and forth between these two unrelated senses" and "gets the science wrong." Matthews' supposedly crucial notion of interaction amounts to the thoroughly uncontentious claim that heredity and environment "inextricably combine" (interact?) at conception. In other words, measured intelligence or IQ represents the phenotype, or the observable result of the inevitable combination or interaction, of heredity and environment. However, the other, very different, technical meaning of interaction explicit in Jensen's analysis is the notion that different genotypes *may* respond quite differently to the same environment – and Jensen's linear statistical model is capable of detecting this possibility in analysing the phenotypic variance. Matthews himself provides a very clear and correct illustration of what is meant by the statistical concept of interaction in Figure 1 that illustrates changes in phenotypic height (corn plants) as a function of genotype and environmental variation (Matthews, 1980, p. 147). What the figure points out is that each of the five strains (genotypes) of corn may achieve very different mean heights (phenotypes) in response to the same nitrogen levels (environment). This example clearly shows that moving to a different nitrogen level environment does *not* have an equal, additive effect upon the phenotypic mean height of each of the five strains of corn. Instead, the

move to a different environment may increase one strain's phenotypic height (e.g., G_1) while decreasing that of another (e.g., G_5). In short, the graph shows that, in this instance, heredity and environment do indeed statistically interact, that is, combine in some non-additive, multiplicative fashion. All this is quite correct and serves to specify clearly what Jensen and others mean by interaction as well.

Two more important critical points follow from this illustration of statistical interaction. First, phenotypic variance in measured IQ, just like phenotypic variance in measured corn height, *can* be analysed to determine whether such non-additive, interaction variance constitutes a significant component of the total observed variance in IQ. Second, the empirical evidence indicates that the phenotypic variance in IQ, unlike the variance in the corn height example, does *not* yield a significant interaction component. That is to say, 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, there are as yet no conspicuous instances of environments which (like nitrogen levels in the corn plant illustration) increase the value of the IQ for some individuals but decrease the value for others. Indeed, such so-called *disordinal* aptitude-by-treatment interactions (ATIs) in the intellectual domain have, in general, eluded discovery (Ragsdale, 1980). Nothing in this current empirical conclusion rules out the possibility of discovering or designing environments that interact with heredity. It rather asserts only that under *present* circumstances, beneficial (or detrimental) changes in the environment are likely to benefit (or deter) everyone. And thus environmental changes are likely to preserve the present variance or observed range of individual differences in IQ. In the IQ debate, variance is the statistical proxy for individual differences or, more normatively, cognitive inequality. And Jensen's claim is that these measured individual differences among us (regardless of their social importance) have proved difficult to eliminate. Thus, even though our collective mean IQ may have risen with steady improvements in our environments, the differences between and among us remain. This is the essential implication of high heritability.

SUMMARY

I have suggested that the radical Marxist case, as prosecuted by Michael Matthews and others, seeks to indict IQ on two counts: that the IQ research program, as represented by the work of Jensen be, first, condemned as racist ideology, and, second, dismissed as pseudoscience. I

argue, however, that the first charge rests upon selective historical half-facts about the early development of IQ testing which are largely irrelevant to evaluating the merits of contemporary IQ theory and research. I also contend that the more technical case against IQ research as legitimate psychological science consists mostly of some arbitrary theoretical preferences, several psychometric and statistical confusions, and quite a few serious misrepresentations of Jensen's published work. Thus, whatever the final judgement of Jensen's views about the nature and heritability of IQ, there is surely no better example of ideologically inspired pseudoscience than the radical Marxist criticism that they have provoked.

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Radikale marxistische Kritiker haben behauptet, daß das IQ-Prüfen und IQ-Forschen, wie in den Werken von Arthur Jensen repräsentiert, entweder verheimlichte rassistische Ideologie oder pseudo-psychologische Wissenschaft seien. Diese Arbeit behauptet, daß historische Nachweise, die zur Unterstützung der ersten Anklage gesammelt wurden, sowie selektiv als auch irrelevant seien, und daß die technischen Argumente, die zure Unterstützung der zweiten Anklage vorgebracht wurden, d.h. daß IQ-Forschung Pseudo-Wissenschaft sei, auf ernsthafte Mißverständnisse sowie falsche Darstellungen von Jensens Ansichten über die Natur und Erbfähigkeit von IQ hinweisen.

Criticos marxistas radicales han acusado a los tests de cociente de inteligencia y las investigaciones a su respecto, representados por el trabajo de Arthur Jensen, de ser ya sea ideología racista oculta o ciencia seudo-sicológica. Este artículo arguye que la prueba histórica que se adelanta en respaldo de la primera acusación es tanto selectiva como inaplicable, y que los argumentos técnicos adelantados para respaldar la segunda acusación de que la investigación en cociente de inteligencia es seudo-ciencia reflejan tanto una seria comprensión equivocada como una falsa representación de las ideas de Jensen acerca de la naturaleza y herencia del cociente de inteligencia.

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