Philosophy of Science that Ignores Science: Race, IQ and Heritability*

Neven Sesardic†‡
Department of Philosophy, Lingnan University, Hong Kong

Philosophers of science widely believe that the hereditarian theory about racial differences in IQ is based on methodological mistakes and confusions involving the concept of heritability. I argue that this "received view" is wrong: methodological criticisms popular among philosophers are seriously misconceived, and the discussion in philosophy of science about these matters is largely disconnected from the real, empirically complex issues debated in science.

Ignorance more frequently begets confidence than does knowledge.
Charles Darwin, *The Descent of Man*

1. Introduction. Philosophy of science has become increasingly science-impregnated. Today, even graduate students who want to specialize in philosophy of science are aware that without good acquaintance with the relevant parts of science they will simply be unable to write an acceptable dissertation, let alone get their work published in refereed journals later. Therefore, one surely doesn't expect leading philosophers of science to be poorly informed about basic scientific facts in the very domain of their philosophical explorations. But I will try to show that, indeed, there is a topic where ignorance of science is the rule (with very few exceptions), and

*Received February 2000; revised September 2000.

†Send requests for reprints to the author, Department of Philosophy, Lingnan University, Tuen Mun, Hong Kong. Email: sesardic@ln.edu.hk

‡I would like to thank Rik Hine, Arthur Jensen, Hong-Hwa Lee, Gianmatteo Mameli, David Papineau, Alexander Rosenberg, Mark Sainsbury, Dusko Sekulic, Elliott Sober, Kim Sterelny and two anonymous referees for *Philosophy of Science* for helpful feedback on earlier drafts. Needless to say, none of them should be assumed to agree with the main claims of this paper. I am also grateful to Jim Brown, Philip Kitcher, Robert Richardson and Sahotra Sarkar for comments on my criticisms of their views.

Philosophy of Science, 67 (December 2000) pp. 580–602. 0031-8248/2000/6704-0002$2.00

where prominent philosophers of science systematically defend claims that crumble as soon as they are confronted with the pertinent scientific literature. The topic in question is the explanation of racial differences in IQ. In trying to document my thesis, I will obviously have to analyze and evaluate arguments about some substantive issues but I will *not* commit myself to any of the main rival views in the debate. My goal is merely to disclose the *docta ignorantia* reigning in this small segment of contemporary philosophy of science.

2. The "Master Argument."

2.1. Whence Between-Group Heritability? Many philosophers of science think that there is one particular argument that pinpoints the fundamental weakness in the proposed genetic explanation of the black-white IQ difference. The argument says that the proponents of the genetic hypothesis about the interracial gap in IQ arrive at their conclusion by using a blatantly fallacious inference. The suggestion is that Arthur Jensen and the authors of the *Bell Curve* believe (wrongly) that from the claim that IQ is highly heritable among whites and among blacks it immediately follows that the difference in IQ between whites and blacks is also heritable. Once the mistake is diagnosed, we are told, the genetic hypothesis loses all its credibility. Here are some characteristic quotations:

> . . . the existence of significant heritability for IQ within the populations that have been studied does *not* imply that average IQ differences between races are in whole or in any part due to genetic differences. . . . Various writers—the most prominent being Arthur Jensen . . . —have taken the heritability of IQ to show that these differences must have a genetic base. No such conclusions follow. (Papineau 1982, 97)
>
> On the basis of evidence supporting a high heritability value within a subpopulation, Jensen infers that heritability will be (correspondingly?) high in the population as a whole, and that variation between groups has a (correspondingly?) high genetic basis. . . . But there is no intrinsic connection between the magnitude of the heritability within groups and the magnitude of between-group differences. (Richardson 1984, 401, 406)
>
> Arthur Jensen's 1969 article in the Harvard Educational Review started off a current controversy by arguing from heritability within whites to genetic differences between whites and blacks. In 1970 Richard Lewontin gave a graphic example that illustrates why this is a mistake. (Block 1995, 110)
>
> The [hereditarian] argument is based on the assumption that if IQ has high [heritability] in two different populations, then it can be con-

> cluded that the difference in mean IQ between the two populations also has a high group heritability. (Sarkar 1998, 93)
>
> See also Daniels 1976, 143–144, 173–174.

Helen Longino appears to subscribe to the same view because she finds Richardson's analysis "persuasive" and "compelling." (Longino 1990, 8–9) Since the same argument also occupies a central place in Block (1995), and since Philip Kitcher describes Block's article as "the single best diagnosis of the flaws of Richard Herrnstein and Charles Murray's *The Bell Curve*" (Kitcher 1998, 51), it seems fair to include Kitcher among those who endorse it.[1]

The simple argument that has impressed philosophers of science so much[2] was first used by Richard Lewontin ([1970] 1976) in his criticism of Jensen. Ever since, Jensen's views have been routinely dismissed by rehearsing Lewontin's well-known example with two handfuls of seed taken from the same, genetically heterogeneous sample and then planted in two different soils (rich and poor in nutrients): as a result, the phenotypic differences *within* each of the two groups of plants will be 100 percent heritable, but the difference *between* the two groups will be entirely due to differences in two environments (zero heritability). The moral triumphantly drawn from that example is: "You just cannot establish *between*-group heritability on the basis of *within*-group heritability!" Indeed, this is correct. The only question is whether Jensen was really unaware of that elementary truth.

Richardson for one found it amazing that Jensen could have been so "blind." And he offered the following explanation:

> How might we explain this blindness on Jensen's part? It is exactly here that the point that his doctrine is a racist doctrine—as it manifestly is—enters in. The latent racism explains the persistence of the view despite its manifest untenability on scientific grounds. (Richardson 1984, 407)

It is regrettable that philosophers are not more cautious before they resort to the heavy artillery of political accusations. If it *seems* that a respected scholar has been "blind" about something very simple and elementary, minimal fairness requires that you think twice (and read twice) in order to make sure that perhaps it was not you who misunderstood the person in question. I will try to show that in this particular case Richard-

1. Here is another consideration showing that the inclusion of Kitcher is justified: Rose, Lewontin and Kamin (1984, 117–118) use the same argument against Jensen, and Kitcher describes their chapter on intelligence as "brilliant", "lucid", "thorough" and "devastating." (Kitcher 1984, 9)

2. As a referee for *Philosophy of Science* said: "This argument has become practically canonical in our profession."

son jumped the gun, and that a closer look at the texts resolves the problem in a way that does not necessitate a speculation about Jensen's political motives. More generally, I will claim that Lewontin's "master argument" is really a red herring that has directed the philosophical discussion away from the real issues for the last thirty years.

Let me start by describing three possible ways of arguing from WGH (within-group heritability) to BGH (between-group heritability).

(H_1) High WGH entails a non-zero BGH.
(H_2) High WGH, by itself, inductively establishes a non-zero BGH.
(H_3) High WGH, together with some collateral empirical information,inductively establishes a non-zero BGH.

While serious hereditarianism actually involves commitment to H_3, Lewontin and the philosophers of science following in his footsteps have persistently criticized H_1 (or occasionally H_2), with the unfortunate result that they simply never managed to get in contact with the real hereditarian argument (which aims to support H_3).

When Lewontin says, for example, that "[t]he fundamental error of Jensen's argument is to *confuse* heritability of a character within a population with heritability of the difference between two populations" (Lewontin 1976, 89; italics added), or that "[t]here is, in fact, no valid [*sic*] way to reason from [WGH] to [BGH]" (Rose, Lewontin, and Kamin 1984, 118), this only makes sense as an attack on H_1. Again, his assertion that "[t]he genetic basis of the difference between two populations bears no logical or empirical relation to the heritability within populations and *cannot be inferred* from it . . . " (Lewontin 1976, 89; italics added) is best understood as a criticism of H_1, or perhaps H_2. Furthermore, accusing Jensen of the "manifestly incorrect claim" (Lewontin 1975, 399) and "elementary error" (402) sounds more like suggesting an outrageous, inexcusable blunder (like H_1) than like disputing a fairly complex, empirically based bit of reasoning (like H_3) where there might be room for disagreement among reasonable and competent people.

Finally, and most importantly, the very fact that Lewontin thinks that he can disprove a connection between WGH and BGH by merely using his famous example with two handfuls of seed (Lewontin 1976, 89; Rose, Lewontin and Kamin 1984, 118; see also Block 1995, 110) shows that the target here cannot be H_3. For, although his thought experiment does persuasively demonstrate the logical fallaciousness of any attempt to immediately *derive* non-zero BGH from high WGH (à la H_1), it is by itself irrelevant for the evaluation of a much more sophisticated hypothesis[3] (embodied in H_3).

3. In fairness to Lewontin, it should be pointed out that, besides discussing heritability, he does also briefly address the question whether "the lack of effect of correction for

The bad news for the critics inspired by Lewontin's conceptual line of attack on hereditarianism is that Jensen in fact never intended to defend H_1 (or H_2). I will first support this interpretation by quoting from Jensen's publications. Later, I will exhibit the logical structure of his real argument (that happens to be a version of H_3).

The first indication that Lewontin's argument may have missed the mark badly is Jensen's reaction in his exchange with Lewontin:

> The main thrust of Lewontin's argument, as he sees it, actually attacks only a straw man set up by himself: the notion that heritability of a trait within a population does not prove that genetic factors are involved in the mean difference between two different populations on the same trait. I agree. But nowhere in my *Harvard Educational Review* discussion of race differences do I propose this line of reasoning, nor have I done so in any other writings. (Jensen 1976, 103)

Of course, we don't have to take Jensen's word here but if an author protests that his views are distorted this certainly constitutes a good reason to proceed cautiously and explore the matter in more detail. An additional sign that something may have gone awry with Lewontin's understanding of Jensen is the following comment of the geneticist James Crow in the earliest round of comments on Jensen's 1969 paper: "Strictly, *as Jensen mentions*, there is no carryover from within-population studies to between-population conclusions." (Crow 1969, 159; italics added) So, what Lewontin accuses Jensen of not seeing is the same thing that in Crow's opinion Jensen not only saw but *mentioned* as well! The best way to resolve this odd disagreement is to go directly to what Jensen actually wrote:

> So all we are left with are *various lines of evidence, no one of which is definitive alone, but which, viewed all together*, make it a not unreasonable hypothesis that genetic factors are strongly implicated in the average Negro-white intelligence difference. (Jensen 1969a, 80; italics added)

This statement[4] is manifestly incompatible with the belief (attributed to

---

gross socioeconomic class" might be "presumptive evidence" for hereditarianism. (1976, 91) But in this short discussion he nowhere shows that he recognizes how Jensen harnesses global empirical evidence to work *together* with high WGH to build the hereditarian case (vide infra). For Jensen, *both* the relative weakness of environmental influences on IQ *and* high WGH are *essential* parts of the inference to non-zero BGH. Disputing these two premises separately (as Lewontin does) is an *ignoratio elenchi*.

4. Interestingly, Lewontin quotes a section containing that very statement but he refuses to take it at face value and says that this "cant" needs to be "translated into common English." (Lewontin 1976, 89) In the "translation" he then proposes the crucial part of Jensen's statement is lost.

Jensen by many of his critics) that within-group heritability *alone* is sufficient to establish the between-group heritability. Indeed, immediately after mentioning "various lines of evidence" Jensen clarifies his position further by summarizing *six* different empirical arguments that, in his opinion, *together with the high within-group heritability of IQ* lend support to the genetic explanation of the between-group difference.

Addressing the relation between within-group heritability and between-group heritability, Jensen made essentially the same point in many of his other writings. For example:

> I have explained in greater detail elsewhere [1968] that *heritability coefficients by themselves cannot answer the question of genetic differences between groups*, but when used along with additional information concerning the amount of relevant environmental variations within groups and overlap between groups, can enter into the formulation of testable hypotheses that could reduce the heredity-environment uncertainty concerning group differences. (1969b, 220; italics added) . . . other methods than heritability analysis are required to test the hypothesis that racial group differences in a given trait involve genetic factors and to determine their extent. (1973b, 411) . . . Although one cannot formally generalize from within-group heritability to between-groups heritability, the evidence from studies of within-group heritability does, in fact, *impose severe constraints* on some of the most popular environmental theories of the existing racial and social class differences in educational performance. (1973a, 1; italics added. See also: 1968, 95; 1973a, 135–139; 1981b, 502–504; 1982, 126; 1994, 905; 1998, 445–463)

In mainstream philosophy of science, Lewontin's argument against Jensen is repeated ad nauseam, but Jensen's response, if mentioned at all, is dismissed without being properly explained, let alone evaluated or critically considered. To the best of my knowledge, in no other context have philosophers of science demonstrated a systematic bias of that magnitude in presenting the ongoing scientific debate.

The most surprising thing is that the charge against Jensen (that he tried to infer BGH immediately from WGH) is routinely made and then readily transmitted further, without anyone feeling an obligation to produce textual evidence for that attribution. And when, exceptionally, someone does attempt to provide a supporting quotation it turns out that he is actually unable to deliver:

> Does high IQ heritability in the white population, combined with a 15 point black-white mean difference, permit us to conclude anything about the reasons for, or causes of, the IQ gap? Jensen (1972,

> p. 163) clearly believes it does: "It is not an unreasonable hypothesis that genetic factors are strongly implicated in the average Negro-White intelligence differences." (Daniels 1976, 173)

Contrary to what is being suggested here, the quotation from Jensen only confirms that he advocates the non-zero BGH hypothesis; it certainly does *not* show that he inferred it directly from WGH (as Daniels implies).

It is worth stressing that even those who did not read Jensen still had an alternative and very easy way to acquire better understanding of the position they dismissed so hastily. Merely consulting the writings of more sophisticated environmentalists and *opponents* of Jensen would have been quite sufficient to realize the hollowness of Lewontin's "refutation". Surprisingly, this avenue has not been used either.

Among those currently defending the environmentalist explanation of the racial IQ gap a leading authority is James Flynn. At the beginning of his academic career Flynn was attracted to the debate by the desire to prove that Jensen was wrong, and indeed he ended up providing some interesting arguments against hereditarianism. But despite disagreeing with Jensen, Flynn does not think that Jensen's view is the product of conceptual confusion, methodological fallacies or racism. He takes pains to correct the widespread misinterpretations, and insists that Jensen never made the elementary mistake of inferring the genetic explanation of the between-group difference only on the basis of the high heritability of within-group differences:

> [Jensen] does not believe that [heritability] estimates alone can decide the issue of genetic versus environmental hypotheses. However, he argues that the probability of a genetic hypothesis will be much enhanced if, in addition to evidencing high [heritability], we find we can falsify literally every plausible environmental hypothesis one by one. He challenges social scientists who believe in an environmental explanation of the IQ gap between the races to bring their hypotheses forward. Given his competence and the present state of the social sciences, the result is something of a massacre. . . . Far too many of Jensen's critics have not taken up the challenge to refute him in any serious way, rather they have elected for various forms of escape, the most popular of which has been to seize on an argument put forward by the distinguished Harvard geneticist Richard C. Lewontin. (Flynn 1980, 40, 54)

Although Flynn's excellent book *Race, IQ and Jensen* came out in 1980, there is no reference to it in philosophical works addressing the same topic that were published later. This is a curious omission because Flynn's subtle methodological analysis of the whole debate is a kind of contribution that

should be of special interest to philosophers of science. In particular, after Flynn's powerful and very detailed criticism of Lewontin's argument it is rather odd to see scholars defending that same argument with undiminished fervor and without any apparent awareness of the grave objections raised against it.[5]

Now, the best way to see why Lewontin's argument does not work is to be acquainted with the basics of Jensen's real stance. I will here briefly present an interpretation of Jensen's essential position that is both endorsed by Jensen himself and widely accepted by other scholars, hereditarians and environmentalists alike.

The most important thing to recognize at the outset is that Jensen's inference proceeds in *two* steps. The first step consists in arguing for the substantial within-group heritability of IQ. But this is only the first step. Contrary to what Lewontin and his philosopher-followers suggest, Jensen in fact never claimed that this step by itself established (deductively or inductively) that the between-group differences are also heritable. Rather, Jensen thinks that high WGH puts severe *constraints* on admissible environmentalist explanations of the between-group differences in IQ. And then, *in the second step*, he argues on empirical grounds that, given these constraints, none of the proposed environmentalist hypotheses remains plausible.

Here is Jensen's argument in a schematic form.

(A) High WGH of IQ (among both whites and blacks).
(B) Empirical data (mainly about the relation of certain environmental variables and IQ).

(C) Non-zero BGH.

Critics charge that (C) cannot be inferred from (A). Granted. But making this point comes nowhere near addressing the real hereditarian argument, which tries to reach (C) on the basis of *both* (A) and (B).

Another thing to keep in mind here: conceding that (C) cannot be inferred from (A) alone, deductively or probabilistically, should not be taken to mean (as it sometimes is) that (A) is *evidentially irrelevant* for the truth of (C). Jensen's basic claim is that given (A), the empirical information in (B) makes (C) more plausible than (not-C). If (A) were false, however, he thinks that (B) would lose at least some of its force as a reason for ac-

5. Flynn later softened his attitude toward Lewontin (Flynn 1989, 365–366) mainly because he thought that he had himself discovered *empirical* evidence pointing to a possibly workable Lewontin-like scenario. This idea itself has serious problems (see Nichols 1987) but even if it were accepted, Flynn's basic argument against Lewontin's aprioristic attack on hereditarianism would still remain absolutely cogent.

cepting (C). So, in this picture, (A) is an essential argumentative part of the case for (C) although (A) is by itself insufficient to establish (C).

Admittedly, at several places Jensen does say that high WGH increases the probability of non-zero BGH (1973a, 135, 144; 1973b, 408; 1976, 104), but on closer reading it becomes clear that he actually never commits himself to the claim that high WGH, by itself, inductively establishes non-zero BGH. Here is how Jensen explains the reason for postulating the probabilistic relation between WGH and BGH: "In nature, characteristics that vary genetically *among* individuals within a population also generally vary genetically *between* different breeding populations of the same species." (1973b, 408; cf. 1969a, 80; 1973a, 130) He suggests that, as a rule (with almost no exceptions), high WGH of a trait is in nature accompanied by non-zero BGH, and he moreover states that this strong empirical association is regarded as a well established fact by geneticists. These claims were never seriously disputed either by Lewontin or by philosophers of science opposing hereditarianism. However, Jensen warns explicitly that even if the general probabilistic relation between WGH and BGH is conceded, high WGH still does not allow any determinate conclusion about BGH with respect to a particular trait: *additional* empirical evidence is necessary. He concludes the relevant section with a cautionary remark: "As I have pointed out elsewhere, *other methods than heritability analysis* are required to test the hypothesis that racial group differences in a given trait involve genetic factors and to determine their extent." (1973b, 411; italics added)

Now moving forward from the schematic version, let us flesh out Jensen's argument. There are two fundamentally different strategies of defending the environmentalist explanation of the racial IQ gap. Jensen tries to show that neither of these strategies looks plausible when high within-group heritability is *combined* with additional empirical evidence. The first strategy attempts to explain the difference between the two groups by invoking environmental factors that vary within the groups and that therefore enter into the calculation of WGH. Since these factors exhibit a within-group variation, I will call this type of environmentalist explanation "VE theory" (VE = variable environments). The second strategy tries to explain the between-group difference by postulating a factor that has no within-group variance but which is consistently present in one group and consistently absent in the other one. Following Jensen, I will label this kind of environmentalist explanation "X-factor theory". Let me first take up the VE theories.

2.2. VE Theories. Are VE theories automatically refuted by the mere fact of high WGH? It depends. If WGH is 100% the answer is yes. For, if WGH is 100% this means that differences in VE-factors have no effect

on IQ variation within groups, and hence they cannot account for any part of the observed between-group difference in IQ either. On the other hand, if WGH is high but less than 100% then it is still possible that differences in (within-group) variable environments may explain the between-group difference in its entirety. This is in principle possible, but additional empirical evidence may rule out this possibility too.

High WGH tells us that most of the within-group variance in IQ is caused by genetic differences, and that environmental differences in VE factors are comparatively weak causes. Although they are not causally impotent, VE differences have relatively small effects. But then—and this is crucial for understanding the relevance of WGH—if VE differences are indeed so weak as causes the following is true: for the between-group difference to be fully explained by VE factors the two groups must mutually differ with respect to these VE factors *to a very large extent*. The difference in VE required for the complete environmentalist explanation can be so large that, in the light of available data about the group differences in VE, the very hypothesis will sometimes be seen as empirically hopeless. The following table[6] shows the rising constraints that increased within-group heritability puts on pure between-group environmentalism:

TABLE 1. The Rising Constraints

Heritability	0%	10%	20%	30%	40%	50%	60%	70%	80%	90%	100%
Required VE-difference	1	1.05	1.12	1.19	1.29	1.41	1.58	1.82	2.24	3.16	—

The values in the bottom row represent the number of standard deviations (of the distribution of the environmental component of IQ) required for explaining the total IQ gap.[7]

So, if heritability is 40% (or 60% or 80% respectively) the VE difference required to explain the whole IQ gap by environmental causes becomes 1.29 SD (or 1.58 or 2.24). The higher heritability, the heavier burden on purely environmentalist accounts. The gist of Jensen's position is precisely this two-barreled argument. He says, first, that high WGH compels radical environmentalists to postulate very big VE differences between the groups and, secondly, that empirical observation shows that these enormous hypothesized differences are simply not there.

6. After having compiled this table I found out that Jensen gives a similar tabulation in 1998, 455. (There is an obvious misprint in his text, though: the required VE difference for the heritability of 40% should be 1.29 and not 1.77.)

7. These values are in each case obtained by dividing the entire inter-group IQ difference (15 points) by the respective standard deviation (SD) of the environmental component of IQ variance. The latter magnitude is inversely dependent on heritability, and is easily ascertainable once heritability is fixed.

Although this is the barest outline of Jensen's argument I hope it shows how essentially his hereditarian conclusion rests on the empirical evaluation and eventual rejection of different versions of VE-environmentalism. He carefully considered many prima facie promising attempts to account for the black-white IQ difference in terms of VE-factors like SES, educational inequality, malnutrition, teacher expectations, childrearing practices, pre-natal and peri-natal disadvantages[8] etc. (see e.g. Jensen 1981a, 214–226; 1994, 905–906). But in the end he concluded that the explanatory burden imposed on these hypotheses by high heritability was too heavy and that these environmentalist hypotheses, singly and collectively, fell far short of explaining the total between-group IQ variance. From this brief exposition of Jensen's line of reasoning it should be obvious that the suggestion that Jensen derived BGH directly from a single premise (WGH) introduces a ridiculous caricature of hereditarianism that is as easy to refute as it is meaningless to discuss.

2.3. X-Factor Theories. What Lewontin's example with two batches of seed in different environments does show is that even in the case of 100% within-group heritability the between-group heritability can still be zero. Bear in mind, however, that this can happen only if the phenotypic differences between the two groups are caused by an environmental factor that has no within-group variance at all. (Jensen calls this type of postulated environmental influence the "X-factor".) Hence, complete hereditarianism about *within*-group differences is logically compatible with complete environmentalism about *between*-group differences. All right. But Lewontin himself and too many of his ardent supporters thought that the example proved something much stronger, viz. that within-group heritability is *entirely irrelevant* for assessing between-group heritability. This is wrong. As Flynn says: "[T]he real message of Lewontin's example is that we can ignore high [heritability] only if there exists a highly specific and highly unusual set of circumstances. Therefore, it is absurd to say that high [heritability] estimates within black and white respectively are irrelevant. Their relevance consists precisely of this: they force us to look for a plausible candidate for the role of [X-factor]." (Flynn 1980, 58–59)

As both Flynn and Jensen point out, in reality it may be very difficult to find a plausible candidate for the role of X-factor. Namely, this factor ought to be uniformly present in one group and uniformly absent in the other group, and furthermore it should manifest no variation inside either

8. Given Jensen's thorough-going efforts to take seriously every suggested environmental influence and discuss its empirical status in detail, it must come as a surprise that Ned Block could accuse him of basing his judgment on "*selected* facts" and on "excluding information about blacks' less favorable environments". (Block 1974)

group. For this reason, SES and educational inequalities (the usual suspects in the puzzle of racial difference in IQ) are automatically excluded in this kind of scenario because they obviously have a significant variance within both whites and blacks. The same is true of some other popular candidates for X-factor account, and all this shows that the search will by no means be easy. Indeed, high within-group heritability can so severely constrain the X-factor theorizing as to make this type of environmentalism exceptionally vulnerable to disconfirmation. This is exactly what Jensen was trying to demonstrate. And here again he certainly did not argue in favor of the genetic hypothesis by relying solely on the fact of high within-group heritability of IQ but by also extensively analyzing the empirical credentials of prospective X-factor hypotheses, and by finding them sorely wanting.

At first it might seem that there is actually an environmental factor that fits the bill smoothly: discrimination. The reasoning is straightforward. Blacks are *as a group* exposed to pervasive discrimination and racism triggered by their skin color, whereas whites are never disadvantaged by the same kind of social prejudice targeted at their group. Therefore, the argument goes, since this environmental difference operates at the group level (exactly as an X-factor should) it forces itself upon us as a highly probable explanation of the inter-group difference in IQ. A great number of people have found that argument supremely convincing. But on second thought (if there happens to be a second thought, that is) the idea faces serious difficulties. Despite being initially quite plausible, the suggestion that discrimination is an easy answer to the racial IQ gap is flatly rejected by more sophisticated environmentalists as being "simply an escape from hard thinking and hard research":

> But this is simply an escape from hard thinking and hard research. Racism is not some magic force that operates without a chain of causality. Racism harms people because of its effects and when we list those effects, lack of confidence, low self-image, emasculation of the male, the welfare mother home, poverty, it seems absurd to claim that any one of them does not vary significantly within both black and white America. (Flynn 1980, 60)

Put differently, although discrimination at first looks like an X-factor it may turn out to that it can plausibly operate only through mechanisms involving a host of VE-factors. This shows why thoughtful environmentalists don't rush to embrace the explanation by discrimination when invited by Jensen to try that route, and why instead they immediately sense danger and feel like being offered "a poisoned apple, an escape that looks attractive but proves fatal." (Flynn 1999b, 13)

All said, though, one should not conclude that purely environmentalist

scenarios (either of VE or X-factor variety) are ruled out of court. What the foregoing discussion was meant to show is just that high within-group heritability changes the terms of the debate in the sense that it puts additional obstacles in the path of pure environmentalism about group differences. Given the limited objectives of this paper no opinion is expressed about whether environmentalists can overcome these obstacles or not.

2.4. Coda. The environmentalist criticism of hereditarianism that dominates contemporary philosophy of science is so crude and ill-founded that it simply does not connect with the best discussions in the field. The issues are immensely more complicated than what philosophers typically think. Ned Block, the leading philosophical authority on the questions of race, IQ, and heritability insists that the hereditarian position relies on "conceptual confusions" (1995, 99), "flawed logic" (110) and "mistake" (ibid.). I tried to show that, on the contrary, hereditarianism is a perfectly legitimate hypothesis: the alleged flawed logic and conceptual confusions are in this case just in the eyes of the beholders (Lewontin, Block, Richardson, and others). Properly interpreted, hereditarianism is a carefully argued and methodologically sound theory.

Needless to say, this does not mean that it is true or that it should be accepted. The only reasonable way to take sides about this issue is to painstakingly examine the rich empirical material accumulated in the last several decades and to explore different lines of argument based on available data. There is no philosophical road to truth about these things. Knowledgeable environmentalists today are well aware that they cannot win the debate by just relying on the "intuitive plausibility" of their view, and by arguing that any other answer must be the product of muddled thinking or racism (or both).

I hope I have demonstrated in this section that Lewontin and the philosophers who followed him "refuted" Jensen by first distorting his position beyond recognition and that afterwards all went quite effortlessly.

Ned Block does the same thing with *The Bell Curve*. He starts by attributing to Herrnstein and Murray the following principle (a version of the "from within-group to between-group" fallacy):

> if a characteristic is largely genetic and there is an observed difference between two groups, then there is 'highly likely' . . . to be a genetic difference between the two groups that goes in the same direction as the observed difference. (Block 1995, 102)

According to him this principle "underlies all of Herrnstein's and Murray's thinking *even though it is never articulated.*" (Block 1995, 102; italics added) This sounds odd. It is not only that Block's attribution, as he himself admits, cannot be supported by textual evidence from *The Bell*

Curve. Even worse, Herrnstein and Murray in fact explicitly express a statement that goes *against* that principle, and they moreover put that statement in italics[9]: "*That a trait is genetically transmitted in individuals does not mean that group differences in that trait are also genetic in origin.*" (Herrnstein and Murray 1994, 298) On the next page they say: "The heritability of individual differences in IQ does not necessarily mean that ethnic differences are also heritable." It is fairly clear that what Herrnstein and Murray want to say is that within-group information is by itself *insufficient* to establish a between-group conclusion. Additional information is necessary to make that step, and indeed a few pages later they adduce crucial *supplementary* evidence as the missing argumentative link, such as the so-called "Spearman's hypothesis" and the weaknesses of extant environmentalist explanations. (301–311) One can of course dispute that inference too, but (as I have shown in Jensen's case) as soon as one acknowledges the essentially two-step structure of the argument the debate must turn to empirical matters and one can no longer resort to the quick strategy of dismissing hereditarianism as just a crude methodological fallacy.

To sum up: in stark contrast with the empirical orientation and awareness of complexity of the issues that rule among the best advocates of environmentalism and hereditarianism, philosophy of science still largely lives in its own, socially constructed world. Its practitioners are massively beguiled by the belief that hereditarianism can be easily checkmated in a couple of moves discovered by conceptual analysis.[10]

3. Philosophers at Work: *Caveat Lector!* Here are three other characteristic examples showing why information found in philosophy of science sources should be taken with a big grain of salt.

3.1. Measure for (Mis)measure. In philosophy of science, Gould's book *The Mismeasure of Man* is standardly praised as disclosing serious weaknesses and fallacies of hereditarianism. Here are just a couple of representative examples:

9. Curiously, Block quotes this statement in a footnote (and criticizes it for a reason we cannot go into), but he does not seem to notice a tension between that statement and the view he arbitrarily tries to impose on its authors.

10. Mario Bunge suggests that hereditarianism about racial differences in intelligence is charlatanism, not science. He says that Jensen's hypothesis that the lower IQ of blacks is partly due to genetic factors "was unanimously rejected by the scientific community." (Bunge 1996, 106) In actuality, according to the poll of experts in the relevant fields, of all the scientists who felt qualified to express a view on that issue, 53% agreed with Jensen (Snyderman and Rothman 1988, 129).

> No one has done as much as Stephen J. Gould to expose race and intelligence studies for the garbage that they often are. (Brown 1998, 5)

> Stephen Jay Gould has lucidly analyzed how filling the skulls with lead shot, and comparing the weights of the lead, could easily be infected with unconscious biases. (Kitcher 1997, 171)

Brown urges others to follow in Gould's footsteps and fight bad science in the same way. He says that philosophers of science are "uniquely situated" to do this job well: "More than anyone else, they have the skills—logical, mathematical, statistical, methodological, and many more—to ferret out bad science." (Brown 1998, 5)

Perhaps. But are the views attacked by Gould really bad science? Brown, Kitcher and many others have no doubt about this although they provide no supporting evidence for this belief other than Gould's authority. However, the *Mismeasure of Man* is quite controversial as a piece of scholarship. The reviews of Gould's book in *Nature*, *Science* and some other professional journals were highly negative and severely critical (see Davis 1986), in contrast with typically favorable and laudatory comments in the popular press. Therefore, although it still remains perfectly legitimate for philosophers of science to side with Gould and express their admiration publicly, one would expect them at least to notify the reader about the massive presence of these strong dissenting voices as well (assuming of course that they are aware of them).

More to the point, however, Gould's central argument against hereditarians happens to be based on his gross misunderstanding of the position he is criticizing. He says, "a reified Spearman's *g* is still the only promising justification for hereditarian theories of mean differences in IQ among human groups . . . . The chimerical nature of *g* is the rotten core of Jensen's edifice, and of the entire hereditarian school." (Gould 1981, 320) In reality, Jensen's views on the genetic explanation of racial differences in IQ are *totally independent* from the question whether there is only one factor of general intelligence (so-called *g*). Here is what James Flynn, a consistent critic of Jensen, has to say on the matter:

> Gould's book evades all of Jensen's best arguments for a genetic component in the black-white IQ gap, by positing that they are dependent on the concept of *g* as a general intelligence factor. Therefore, Gould believes that if he can discredit *g*, no more need be said. This is manifestly false. Jensen's arguments would bite no matter whether blacks suffered from a score deficit on one or 10 or 100 factors. I attribute no intent or motive to Gould, it is just that you cannot rebut arguments if you do not acknowledge and address them. (Flynn 1999a, 373)

Another argument against hereditarianism in the *Mismeasure of Man* has no problems arising from misinterpretation or logical flaws. However, it seems to have serious troubles with empirical reality (although the news about this is traveling slowly, and has not yet reached mainstream philosophy of science). The argument that helped make Gould's book famous and that left the strongest impression on many readers is certainly his criticism of the skull measurements undertaken by the nineteenth century scientist Samuel George Morton. Gould claimed that the results of Morton's measurements indicating systematic differences in cranial capacity between different races were due to Morton's unconscious bias, and ultimately his racist beliefs: "Morton's summaries are a patchwork of fudging and finagling in the clear interest of controlling a priori convictions." (Gould 1981, 54) Gould then went on to propose a concrete explanation of how the bias worked to distort the measurements, and it is this analysis that Kitcher called "lucid".

Now, elementary logic demands that if you want to argue that someone's mistake is due to some kind of bias or prejudice, you have first to be sure that the person really made a *mistake*. In the case of Morton's measurements there appears to be no room for doubt about his having made the mistake. For, the idea that human races differ in average cranial capacity or brain size sounds to many people like the crudest possible form of racist and pseudoscientific belief. But notice that the belief is nevertheless empirical, and that its truth-value cannot be determined by conceptual analysis or political condemnation. John S. Michael thought that it was worth checking the data, and in 1986 he remeasured the cranial capacities of 201 specimens from the Morton Collection. In a paper published in *Current Anthropology* he presented the results, and showed that the differences reported by Morton were basically *corroborated* by his remeasurements. Although Michael had qualms of a more general kind (e.g., about the legitimacy of "race" as a biological category), with respect to the issue at hand (the craniological data) his conclusion was that he could find no indication of the systematic bias Gould ascribed to Morton, and that in his opinion "Morton's research was conducted with integrity." (Michael 1988, 353)

Gould's explanation of Morton's "error" in terms of racial bias fails for the simple reason that there is no error that needs to be explained (i.e., there is no *explanandum*). In other words, what Morton discovered was a genuine difference. Moreover, this fact is accepted today in standard reference books (see Sternberg 1982, 773; Brody 1992, 301; Mackintosh 1998, 184), and even by scholars who are staunch advocates of the environmentalist account of the racial IQ gap. For instance, Ulric Neisser (one of the leading critics of the genetic hypothesis and the chair of the American Psychological Association Task Force that prepared the report "Intelli-

gence: Knowns and Unknowns") did not hide his strong aversion for the hereditarian views of J. Philippe Rushton when he said: "I do not have the space or the stomach [*sic*] to reply to all the points raised by Rushton." (Neisser 1997, 80) Yet, as a responsible psychologist who knows that this kind of dispute is ultimately resolved by empirical verification, he had no other choice but to concede that with respect to the racial differences in the mean measured sizes of skulls and brains "there is indeed a small overall trend in the direction [Lynn and Rushton] describe." (ibid.) This lesson in respect for hard empirical data is to be commended to all lovers of wisdom.

3.2 Blocking the Whole View. Ned Block is oddly selective in discussing Flynn's views. As we saw, Block endorses Lewontin's "master-argument" without any reservation, but inexplicably, in this context he never mentions Flynn's detailed criticism of that same argument. When Flynn develops his own line against Jensen, however, his ideas suddenly become "interesting", and Block draws on them enthusiastically in his attack on hereditarianism. Oddly enough, he fails to make it known that Flynn himself would strongly disagree with the idea that hereditarianism should be dismissed because of its "conceptual confusions" and "flawed logic." On the contrary, Flynn regards Jensen as a formidable opponent whose work presents an extremely serious challenge to environmentalism. In the very essay which Block cites and uses as a *machine de guerre* against Jensen, here is what Flynn had to say about the debate between Jensen and his environmentalist adversaries:

> The result is something of a massacre, with Jensen showing that the most cherished environmental hypotheses have been sheer speculation without a single piece of coherent research in their favor. For this alone, all seekers of the truth are greatly in his debt. (Flynn 1987, 222)

With this kind of information omitted in Block's presentation, readers unacquainted with the literature will undoubtedly get a highly distorted picture of the controversy. It is as if Flynn's views have been passed through a filter that lets through only the ideas that can be used against Jensen and *The Bell Curve*.

The same bias is discernible in Block's short summary of the whole debate about within-group heritability and the racial gap in IQ. (Block 1995, 115) In that summary Block mentions *only* three pieces of empirical evidence: the so-called Flynn Effect, the data about caste-like minorities, and the relatively small amount of genetic variation between the races. Notoriously, these are all the data standardly used to support environmentalism. Whereas these data are scrupulously discussed in *The Bell*

Curve and then weighed against the *contrary* empirical information, more consistent with hereditarianism, in Block's concise picture of the controversy there is no place at all for the evidence that threatens environmentalism.[11]

There is another manifestation of Block's partiality. Flynn's argument against Jensen (that Block endorses) is based on an observed increase of average IQ test scores in many countries in the last several decades—the phenomenon known as the "Flynn Effect". Flynn's argument about race builds on this and goes as follows. Since this secular gain in IQ must be due to some environmental causes (about which we are now completely in the dark), then there is also a hope that the racial gap in IQ might be accounted for in the same way (in terms of some presently unknown environmental differences). This is an interesting argument but it is hardly to be taken as immediately convincing or methodologically unproblematic, especially not in a contribution that aspires to examine the whole debate in a critical spirit. There are several obvious worries about the argument. For example, psychologist Robert C. Nichols raised a serious objection. He first reconstructed Flynn's argument and presented it in the form of four premises and the conclusion, and then dismissed it as a "faulty syllogism", or as an *obscurum per obscurius* reasoning.[12] But although this kind of criticism ought to be specially congenial to philosophers of science, and is moreover published as an immediate response to Flynn's paper on which Block so strongly relies, Block's paper contains no mention of that highly relevant contribution to the discussion, nor of any other of the conspicuous problems with Flynn's reasoning. Again, the filter blocked that kind of information.

3.3. The Spectre of Racism. The next example is Sahotra Sarkar's chapter "The Obsessions with Heritability" from his book *Genetics and Reductionism*. The book came out in the prestigious series Cambridge Studies

11. In the text Block does briefly discuss some empirical data favoring hereditarianism but he seems to treat them as independent arguments (and not as an essential part of the inference from WGH to BGH). He faces the dilemma mentioned earlier. If he excludes the empirical component of the two-step argument in favor of hereditarianism, he is refuting the mere shadow of hereditarianism and is tilting at windmills. If he includes it, however, the charge of "conceptual confusions" and "flawed logic" simply disintegrates.

12. "By a strange twist of logic Flynn has transformed the genuine mystery concerning test score changes over time into positive evidence that solves the alléged mystery of racial differences." (Nichols 1987, 234) Other scholars are also trying to stimulate "healthy skepticism" about the Flynn Effect (e.g. Rodgers 2000), warning that the phenomenon itself is at present so poorly understood that we should first strive to grasp better its nature and meaning, and only then attempt to explain it (or, for that matter, use it to explain something else).

in Philosophy of Biology, which, despite having started only recently, has already given us some extremely important contributions to the field. The series is advertised as "publishing the very best work in the philosophy of biology". In the case of Sarkar's book, however, the application of these high standards is not always visible.

After uncritically rehashing the old arguments against the use of heritability (based on the possibility of statistical correlation and statistical interaction), Sarkar clinches his attack with a standard speculation about motives: "It is hard, therefore, not to suspect that the continued pursuit of [heritability] is guided, at least to some extent, by non-cognitive, especially, political factors." (Sarkar 1998, 92) He mentions three sets of considerations that point to that conclusion, all of which are utterly inadequate to establish such a sweeping and indiscriminate political accusation. I will comment only on two of them, because they nicely illustrate the scope of ignorance that cripples philosophical discussions about race, genetics and IQ.

(i) Sarkar infers political motivation from the fact that "the traits for which [heritability] continues to be pursued often include those carrying social judgments, even if they are ill-defined." (Sarkar 1998, 93) After giving examples of religiosity and IQ, Sarkar continues:

> Bouchard [. . .] reports relatively high values of [heritability] for "openness," "agreeableness," "conscientiousness," "neuroticism," and "extroversion," *each of which is a trait that carries social judgment*. (Sarkar 1998, 93; italics added)

Sarkar's point is very clear: the continued pursuit of heritability must be politically motivated because the traits whose heritability is studied often include those carrying social judgment, like the five traits mentioned (openness, agreeableness, conscientiousness, neuroticism, and extroversion). Indeed, why were exactly these five traits singled out for heritability studies? One possibility is, as Sarkar suggests, that some people had a sinister political intention to take traits "that carry social judgment" and then perform a heritability analysis in order to develop the hereditarian argument and justify the oppression of certain social groups.

But acquaintance with some very basic psychology points to a much simpler and quite benign explanation for the choice of the quintuple. Namely, these five traits are known in psychology as *the five main personality traits*. If one happens to know this, it becomes quite obvious that these traits were put in the foreground not by Bouchard, but by the wide consensus of personality psychologists. And then, there is absolutely no need to invent a right-wing conspiracy of scientists in behavior genetics. The traits in question became salient in psychology simply because they emerged as very robust results of the systematic empirical research on

human personality. (Needless to say, one can also criticize the way these traits were picked out as "the big five" but that would be a completely different topic, unrelated to discussions about heritability.)

(ii) Sarkar fortifies his imputation of political motives by introducing another consideration as well. He says that it is "hard not to suspect" that research on heritability is guided by political factors because "the work on [heritability] and IQ has been routinely used to argue for genetic inferiority of certain groups, particularly African-Americans." (1998, 92–93) There is no argument here at all. Sarkar just asserts (without offering any evidence) that scholars who accept the genetic explanation of the racial IQ difference entered this research area just because they wanted to give an aura of scientific respectability to their racist prejudices. The crudeness of this way of dismissing a whole research program boggles the mind.

But ironically, Sarkar himself provides the best refutation of his own claim. Speaking about those authors who advocated the genetic account of the IQ differences between ethnic groups Sarkar gives reference to Richard Herrnstein's article "IQ" from *Atlantic Monthly* (1971) and his book *IQ in the Meritocracy* (1973). In reality, however, Herrnstein at that time did *not* subscribe to the genetic hypothesis about white-black IQ variation. In the early 1970s he disagreed with Jensen, and it was *only later* that he changed his mind, and was converted to hereditarianism with respect to racial differences. In the book *IQ in the Meritocracy*, Herrnstein complained about frequent misinterpretations of his views, and distanced himself from Jensen very explicitly:

> My article took what might be called an explicitly agnostic stand on racial (i.e., black white) differences in tested intelligence. . . . I believe that racial and ethnic group differences are hard to pin down as regards inheritance. My interest was not race, but social class differences. (Herrnstein 1973, 12)

As one of the authors of *The Bell Curve*, Herrnstein has become notorious for his views on race, genetics and IQ. This may be a reason why many people who did not take trouble to study the sources tend to believe that he must then have defended the very same ideas in his writings from the early 1970s, which also gave rise to a heated political controversy. But although this mistake is in a sense expected from lay readers, it is disturbing when it is found in a high-profile philosophical publication.

Another philosopher (by the way, a winner of the Lakatos award for *philosophy of science*) who made the same mistake, is Michael Dummett (1981, 295–296). He also accused Herrnstein of racism although at that time Herrnstein had resolutely refused to take a stand on issues involving race. Rather than being a mere oversight, however, Dummett's error looks more like a natural consequence of the quick and perfunctory method he

recommends for reaching conclusions in this context. Namely, he claims that "it should seem . . . *obvious* that contemporary psychologists in the United States and Britain, advancing the thesis of the hereditary inferiority of Negro intelligence, are . . . reflecting prejudices still widespread in these countries", and that hereditarianism about racial differences and IQ, "so obviously conforming to a palpably powerful prejudice, *can be set aside by any rational judge without further examination*" (296, 298, italics added). In other words, it is suggested that the views in question can (and should) be rejected without looking into the relevant empirical evidence at all!

Although one cannot help feeling a peculiar sort of admiration for Dummett's candor here, the fact that he was ready to publicly defend such an idea is yet another indication that the situation in our field has deteriorated to the point that it is necessary to ring the alarm bell. For, it is not only that philosophers of science tend to make judgments about certain scientific issues without seriously studying the literature, or without even properly understanding the theories they attack. Now, in addition, we have a thinker of Dummett's stature openly defending an epistemic norm that legitimizes this awkward way of forming beliefs.

4. Conclusion. Why is this small segment of contemporary philosophy of science in such a sorry state? On reflection, I prefer to leave this question as an exercise for the reader. My aim in this paper is to criticize a deviant philosophical trend, not to explain how it came about or why it spread.

REFERENCES

Block, Ned (1974), Letter to *Psychology Today* (March).
———. (1995), "How Heritability Misleads About Race", *Cognition* 56: 99–128.
———. and Gerald Dworkin (eds.) (1976), *The IQ Controversy*. New York: Pantheon.
Brody, Nathan (1992), *Intelligence*. San Diego: Academic Press.
Brown, James R. (1998), "Intellectual Tithing", *International Studies in the Philosophy of Science* 12: 5.
Bunge, Mario (1996), "In Praise of Intolerance to Charlatanism in Academia", in Paul Gross, Norman Levitt, and Martin W. Lewis (eds.), *The Flight from Science and Reason*. Baltimore and London: Johns Hopkins University Press, 96–115.
Crow, James F. (1969), "Genetic Theories and Influences: Comments on the Values of Diversity", in *Environment, Heredity and Intelligence* (*Harvard Educational Review* Reprint Series No. 2), 153–161.
Daniels, Norman (1976), "IQ, Heritability and Human Nature", in Robert S. Cohen et al. (eds.), *PSA 1974*. Dordrecht: Reidel, 143–180.
Davis, Bernard (1986), *Storm over Biology*. Buffalo, N.Y.: Prometheus Books, 1986.
Dummett, Michael (1981), "Ought Research to Be Unrestricted?", *Grazer Philosophische Studien* 12–13: 281–298.
Flynn, James R. (1980), *Race, IQ and Jensen*. London: Routledge & Kegan Paul.
———. (1987), "Race and IQ: Jensen's Case Refuted", in Sohan Modgil and Celia Modgil (eds.), *Arthur Jensen: Consensus and Controversy*. London: Falmer Press, 221–232.
———. (1989), "Rushton, Evolution, and Race: An Essay on Intelligence and Race", *Psychologist* 2: 363–366.

———. (1999a), "Evidence Against Rushton: The Genetic Loading of WISC-R Subtests and the Causes of Between-Group Differences", *Personality and Individual Differences* 26: 373–379.
———. (1999b), "Searching for Justice: The Discovery of IQ Gains Over Time", *American Psychologist* 54: 5–19.
Gould, Stephen J. (1981), *The Mismeasure of Man*. Harmondsworth: Penguin.
Herrnstein, Richard J. (1971), "IQ", *Atlantic Monthly* (September): 43–64.
———. (1973), *IQ in the Meritocracy*. Boston and Toronto: Atlantic Monthly Press.
———. and Charles Murray (1994), *The Bell Curve: Intelligence and Class Structure in American Life*. New York: Free Press.
Jensen, Arthur R. (1968), "Another Look at Culture-Fair Tests", in *Measurement for Educational Planning: Western Regional Conference on Testing Problems*. Berkeley: Educational Testing Service, Western Office, 50–104.
———. (1969a), "How Much Can We Boost I.Q. and Scholastic Achievement?", *Harvard Educational Review* 39: 1–123.
———. (1969b), "Reducing the Heredity-Environment Uncertainty", in *Environment, Heredity and Intelligence* (*Harvard Educational Review* Reprint Series No. 2), 209–243.
———. (1972), *Genetics and Education*. London: Methuen.
———. (1973a), *Educability and Group Differences*. New York: Harper & Row.
———. (1973b), *Educational Differences*. London: Methuen.
———. (1976), "Race and Genetics of Intelligence: A Reply to Lewontin", reprinted in Block and Dworkin (1976), 93–106. Originally appeared in *Bulletin of the Atomic Scientists* May 1970.
———. (1981a), *Straight Talk about Mental Tests*. New York: Free Press.
———. (1981b), "Obstacles, Problems, and Pitfalls in Differential Psychology", in Sandra Scarr (ed.), *Race, Social Class, and Individual Differences in I.Q.* Hillsdale, N.J.: Erlbaum, 483–514.
———. (1982), "The Debunking of Scientific Fossils and Straw Persons", *Contemporary Education Review* 1: 121–135.
———. (1994), "Race and IQ Scores", in Robert. J. Sternberg (ed.), *Encyclopedia of Human Intelligence*. New York: Macmillan, 899–907.
———. (1998), *The g Factor: The Science of Mental Ability*. Westport, Conn.: Praeger.
Kitcher, Philip (1984), "The Case Against Sociobiology", *The New York Times Book Review* (May 20): 9.
———. (1997), *The Lives to Come*. New York: Touchstone Books.
———. (1998), "A Plea for Science Studies", in Noretta Koertge (ed.), *A House Built on Sand*. New York and Oxford: Oxford University Press, 32–56.
Lewontin, Richard C. (1975), "Genetic Aspects of Intelligence", *Annual Review of Genetics* 9: 387–405.
———. (1976), "Race and Intelligence", reprinted in Block and Dworkin 1976, 79–92. Originally appeared in *Bulletin of the Atomic Scientists* May 1970.
Longino, Helen E. (1990), *Science As Social Knowledge*. Princeton: Princeton University Press.
Mackintosh, Nicholas J. (1998), *IQ and Human Intelligence*. Oxford: Oxford University Press.
Michael, John S. (1988), "A New Look at Morton's Craniological Research", *Current Anthropology* 29: 349–354.
Neisser, Ulric (1997), "Never a Dull Moment", *American Psychologist* 52: 79–81.
Nichols, Robert C. (1987), "Reply to Flynn", in Sohan Modgil and Celia Modgil (eds.), *Arthur Jensen: Consensus and Controversy*. London: Falmer Press, 233–235.
Papineau, David (1982), "Nature and Nurture", *Journal of Medical Ethics* 8: 96–99.
Richardson, Robert C. (1984), "Biology and Ideology: The Interpenetration of Science and Values", *Philosophy of Science* 51: 396–420.
Rodgers, Joseph L. (2000), "A Critique of the Flynn Effect: Massive IQ Gains, Methodological Artifacts, or Both?", *Intelligence* 26: 337–356.
Rose, Steven, Richard C. Lewontin, and Leon J. Kamin (1984), *Not In Our Genes*. Harmondsworth: Penguin.

Sarkar, Sahotra (1998), *Genetics and Reductionism*. Cambridge: Cambridge University Press.
Snyderman, Mark and Stanley Rothman (1988), *The IQ Controversy, the Media and Public Policy*. New Brunswick, NJ: Transaction Publishers.
Sternberg, Robert J. (ed.) (1982), *Handbook of Human Intelligence*. Cambridge: Cambridge University Press.