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COMMENTARY

TEXTBOOK TREATMENTS OF THE GENETICS OF INTELLIGENCE

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ABSTRACT

Genetics textbooks have been remarkably unaffected by the discovery of fraud in the work of British psychologist Sir Cyril Burt or by the resulting critical review of other classic studies on the genetics of intelligence. Although Burt's name has nearly vanished from current textbooks, his results continue to be cited in textbook discussions of the heritability of intelligence, as do the results of other studies now recognized as methodologically inadequate. Moreover, genetics textbooks consistently employ confused or misleading definitions of the concept of heritability that, together with the reporting of discredited data, perpetuate a fundamentally inaccurate understanding of the genetics of intelligence. This situation is largely attributable to the practice—generic to textbook writing but in this case taken to an extreme—of authors' liberal borrowing from one another or from a few apparently authoritative works (including earlier textbooks). The extent to which authors rely on these sources for their discussions of the genetics of intelligence is apparently a function both of the controversiality of the subject and of authors' technical insecurity, perhaps reinforced by prior assumptions about the influence of genes on variations in intellectual performance.

INTRODUCTION

ON OCTOBER 24, 1976, Oliver Gillie reported on the front page of the London *Sunday Times* that he had been unable to find any evidence for the existence of Cyril Burt's collaborators, Margaret Howard and J. Conway (Gillie, 1976). Gillie's story served to intensify suspicions of fraud aroused two years earlier when Leon Kamin noted that Burt's correlations for identical twins raised apart and raised together remained the same to the third decimal point, in three different studies, involving varying numbers of twin pairs—results that from a statistical standpoint were literally too good to be true (Kamin, 1974). Gillie's allegations were widely reported in the American press, including general-interest and scientific journals, such as the *New York Times* (1976, p. 4), *Time* (1976, p. 66), *Newsweek* (Panati and MacPherson, 1976, p. 76), *Scientific American* (1978, p. 88) and *Science* (Wade, 1976, pp. 916–919). They were soon followed by other investigations that confirmed the substance of the original charges and also demonstrated the existence of fraud in Burt's reporting of data on parent-offspring regression in his famous paper on intelligence and social class (Burt, 1961; Dorfman, 1978; Hernshaw, 1979).

These revelations were to produce a scholarly reappraisal of standards generally prevailing in studies of the heritability of human mental and behavioral traits, and particularly of intellectual performance. In the book that prompted the search for Burt's coauthors, Kamin had also questioned the reliability of other frequently cited studies, including the remaining three classic investigations of identical twins raised apart by Shields (1962), Juel-Nielsen (1965), and Newman, Freeman, and Holzinger (1937; hereafter cited as NHF)), and the widely reproduced review article by Erlenmeyer-Kimling and Jarvik (1963; hereafter cited as EKJ), which summarized the results of many studies involving individuals of different degrees of relationship. These studies were almost universally thought to have established the fact of a high heritability of IQ (or of intelligence, where IQ and intelligence were equated). Researchers were generally agreed on a heritability estimate falling between 0.75 and 0.80 (Brown and Herrnstein, 1975; Cattell, 1973; Jensen, 1969; Jinks and Fulker, 1970).

Kamin's conclusion—that methodological deficiencies rendered valueless all existing studies on the heritability of intellectual performance—made little impact at the time and remains controversial (Bouchard and McGue, 1981; DeFries and Plomin, 1978). However, even those who refused to accept his verdict in its entirety conceded that the "classic" investigations, including those by Burt (1955, 1958, 1966), and the paper by EKJ (1963), could no longer be considered reliable. And researchers generally agreed that the discrediting of Burt's data rendered problematic the use of other existing studies of separated identical twins. Of the four classic investigations, only Burt's apparently involved twins genuinely raised "apart"; i.e., Burt was unique in claiming no correlation in the occupational categories of the homes in which members of his twin pairs were raised. Hence he appeared to have solved the fundamental problem of studies of the heritability of human mental and behavioral traits—the fact that relatives usually share similar environments—with the consequence that environmentalist and hereditarian hypotheses predict identical results: the closer the genetic relationship, the more similar the relatives. Burt also reported the largest number of identical twin pairs: 53 as compared to the 38 studied by Shields, 19 by NFH, and 12 by Juel-Nielsen.

Current Status of the Intelligence Controversy

Those who accepted the scientific and social value of research on the genetics of intelligence therefore appealed for a new generation of studies, employing more rigorous standards, such as much larger sample sizes, inclusion of more than one type of family relationship, and administration of tests in such a way that those who evaluated the results would not know the relationships between the subjects tested. For example, in their review of behavioral genetics in the *Annual Review of Psychology*, DeFries and Plomin (1978) concluded that studies meeting reasonable methodological requirements are "both possible and necessary if we are to gain insights into the biological bases of individual differences in complex human behavior" (p. 504), while noting that existing studies "do not begin to fulfill" the proposed criteria (pp. 503-504). They also observed that the studies of separated identical twins were particularly problematic, given that Burt was the only investigator to claim uncorrelated environments for his twins (p. 502).

The argument between critics and defenders of the potential scientific and practical value of heritability studies did not abate in intensity as a result of the Burt scandal (if anything, the reverse is true), but it did, to a substantial degree, shift ground. The defenders insist that it is both possible and desirable to design experiments on the heritability of human mental and behavioral traits, including intelligence, that meet "reasonable" methodological criteria (Bouchard and McGue, 1981, especially footnote 8; De Fries and Plomin, 1978; Eckland, 1983; Henderson, 1982, pp. 411-414); that new studies have confirmed the influence of genetic variation on individual differences in intellectual performance, though at a level well below that apparently established by the older studies. Specific estimates differ but cluster at about 0.50 for "broad" heritability, i.e., additive genetic effects plus those due to dominance, gene interaction, and assortative mating, and are less for "narrow" heritability (Caruso, 1983; Horn, Loehlin, and Willerman, 1982; Loehlin, 1980; Plomin and DeFries, 1980; Plomin, DeFries, and McLearn, 1980, p. 377). A few researchers believe that the quality of current data precludes estimation of specific parameters (Bouchard and McGue, 1981). Critics, on the other hand, generally believe that our in-ability to break the association of genotype and environment in studies of the heritability of human mental and behavioral traits renders the concept of heritability of IQ meaningless. Disputes about its degree therefore concern "the magnitude of an imaginary number" (Kempthorne, 1978, p. 19; see also Layzer, 1974; Feldman and Lewontin, 1975; Lewontin, Rose, and Kamin, 1984, pp. 83–129). At the least, methodological difficulties are formidable, as none of the existing studies comes close to meeting minimum criteria; and the enormous efforts required to obtain results worthy of respect could not be justified either on the basis of their intrinsic scientific interest or relevance for social policy, both of which are thought to be near zero (Futuyma, 1979; p. 48; Jacquard, 1983; Lewontin, 1974, 1975). In light of these considerations, critics tend to believe that the persistence of research on the genetics of intelligence can only be explained by political and social bias (Lewontin, 1975, pp. 401–413); their opponents explain objections to this research similarly (Plomin, DeFries, and McClearn, 1980, pp. 373–374).

But if the scientific-cum-political debate continues, the participants do agree that the classic studies have been discredited, or at least outdated, and that the old estimates of heritability obtained from them are no longer supportable. A majority of those general and human genetics textbooks that discuss the genetics of intelligence nevertheless continue to assert the fact of a high heritability of IQ on the basis of these investigations. The textbooks are also characterized by inadequate citation of evidence. Sometimes no sources are cited; ocassionally references are unrelated to, or even are inconsistent with, the text content. Perhaps most surprising and most significant is the degree to which texts also suffer from confusion in their treatment of the *concept* of heritability.

ANALYSIS OF THE DATA

My present study involved an analysis of 28 introductory (general and human) genetics textbooks published in the United States between January 1978 and March 1984. Twenty-two of the texts were published since 1980; the latest available edition of each text was used. To minimize the possibility of bias in interpretation of results, all available texts (28 of 31) were included; however, it does not appear that the findings would have differed in any significant respect had the study been limited to what are apparently the most popular textbooks. (The three I was unable to locate are by individuals represented in the authorship of other, included textbooks). The texts used are: Avers, 1984; Ayala and Kiger, 1984; Brewer and Sing, 1983; Burns, 1983; Carlson, 1984; Edwards, 1978; Farnsworth, 1978; Fristrom and Speith, 1980; Gardner and Snustad, 1984; Goodenough, 1984; Hartl, 1983; Herskowitz, 1979; Jenkins, 1979; Jenkins, 1983; Klug and Cummings, 1983; Levine, 1980; Mange and Mange, 1980; Nagle, 1979; Novitski, 1982; Pai and Marcus-Roberts, 1981; Redei, 1982; Rothwell, 1983; Singer, 1978; Sutton, 1980; Suzuki, Griffiths, and Lewontin, 1981; Tamarin, 1982; Wagner, Judd, Saunders, and Richardson, 1980; Winchester and Mertens, 1983. No texts prior to 1978 were included in order to make reasonable allowance for textbook "lag" following the publicity focused on allegations of fraud in Burt's work. (In any event, no trend in use of sources or explication of concepts is discernible over the period of this study; textbooks published in 1983–1984 do not appear appreciably different from those published in 1978–1979.)

Of the texts, 19 included substantial discussions of the heritability of intelligence. Others included no discussion, or did so only in relation to the question of group differences, or only in passing (a paragraph or less). Of the latter, some simply asserted that intelligence is a polygenic trait. (A number of authors who included more substantial discussions of the genetics of intelligence also asserted that its inheritance is polygenic, some on the ground that IQ scores conform to a normal distribution, a pattern associated with quantitative traits. This, however, is an artifact of text construction. Whatever the underlying distribution of "intelligence," the distribution of IQ test scores will always be normal.) Others simply assert that individual differences in intelligence are the product both of genes and environment.

Claims Regarding the Heritability of Intelligence

According to most texts, the heritability of IQ is high. Some examples: "We have seen that variations in intelligence, as measured by IQ scores, are largely genetic in whites" (Sutton, 1980, p. 497); ". . . it seems likely that at least half, perhaps substantially more than half, of the variability in IQ scores among American whites can be ascribed to variations in their genotypes (heritability in the broad sense)" (Mange and Mange, 1980, p. 542); ". . . 80% of the variance [in IQ scores] is due to genetic influences among members of the population" [according to some investigators], although "the heritability value of precisely 0.8 (80%) . . . cannot be considered reliable" (Rothwell, 1983, pp. 176-177); "Current studies of intelligence show that the genotype has a greater influence on IQ than do environmental factors" (Jenkins, 1983, p. 374); "The correlations in IQ between pairs of individuals who are related to varying degrees support the view that there is a high degree of genetic determination for this trait" (Elseth and Baumgardner, 1984, p. 641). In an interesting reversal of a standard argument from family resemblance, one author maintains that "Perhaps the most important evidence of genetic predisposition to physical and intellectual aptitude is the obvious difference in such aptitudes and achievements in brothers or in sisters. . . . Those who wish for documentation may note the rarity of sibs who reach similar distinction in athletics, music, chess or mathematics, or even such complex behavioural aptitudes as those involved in politics, the theatre, or even sophisticated crime" (Edwards, 1978, pp. 38-39).

Most texts also report specific estimates for the heritability of IQ. (Some report more than one, for example in the substantive discussion, a table, or a problem set). These estimates often exceed the highest values currently under discussion. Authors report heritabilities of 0.80 (e.g., Farnsworth, 1978, Table 5-5; Nagle, 1979, p. 284); a range of 0.60-0.80 (e.g., Mange and Mange, 1980, p. 544; Brewer and Sing, 1983, p. 584; Elseth and Baumgardner, 1984, p. 639); a range of 0.40-0.80 (e.g., Avers, 1984, p. 64; Ayala and Kiger, 1984, p. 655); and 0.30-0.90 (Singer, 1978, p. 121). Other estimates are variously "half to three-quarters" (Jenkins, 1979, p. 126) and "between 0.45 and 0.60" (p. 722), and a more modest "about 0.40" (Tamarin, 1982, p. 293). One recent textbook equates the hereditarian position with an "80-90 percent heritability of IQ" (Carlson, 1984, p. 203).

The source of various statistics is often difficult to trace. In many cases, even when whole tables are reproduced from other works, including other textbooks, there is no attribution of the source or the attribution is simply to "various sources," which comes to the same thing. One text includes a table with "hypothetical" data, noting that "they are similar to data from a number of studies reported in various journals and textbooks" (Winchester and Mertens, 1983, p. 224). Sometimes the citation is misleading, as when the work cited itself reports the results of much earlier research. This is particularly evident in the widespread but largely invisible use made of NFH (1937). From the form in which the data are presented, it appears that a majority of authors making use of NFH (approximately half of those who report any data) have relied on intermediate sources (in particular, Bodmer and Cavalli-Sforza, 1976; Cavalli-Sforza and Bodmer, 1971; Dobzhansky, 1962, pp. 82-88; Lerner and Libby, 1976; Srb, Owen, and Edgar, 1965; Stern, 1973; Strickberger, 1976). Rarely are these or other sources credited; in any case, even when credit is given, readers are unlikely to suspect that the ultimate source of the data reported was a study conducted in 1937. Since the NFH results figure so prominently (in one form or another) in current genetics textbooks, it seems appropriate to remark briefly on its methodology.

The Newman, Freeman, and Holzinger Study

NFH was the only American study involving separated identical twins (19 pairs). Its methodological deficiencies were first detailed by Kamin (1974, pp. 52-56). Among the most serious was the method used to select twins for inclusion in the study. The authors of this Depression-era study were concerned about the possibility of wasting money by mistaking the zygosity of the twins. (The research was conducted in Chicago with twins brought in from across the country). To minimize this possibility, potential subjects were queried by mail regarding their own and others' perceptions of their resemblance; they were required to answer in the affirmative such questions as: "Do you yourselves believe that you are far more alike than any pair of brothers and sisters that you know of?" and the authors explain that they excluded one pair of twins who responded, "A good many people think we are identical twins, but we ourselves do not think we are so very much alike"; and another case when one twin reported that although they looked so much alike that they were sometimes mistaken for each other, they were "as different as can be in disposition, and I am almost as much like my older sister as I am like my twin" (NFH, 1937, pp. 135–136). Indeed, the investigators, apparently unaware of the degree to which their technique could have biased their results, were forthright in asserting that: "In general, it may be said that our collection of identical twins reared apart constitutes a selected group, from which any doubtful cases have been excluded before an invitation was extended to them to appear in Chicago . . ." (p. 136). Results of this study appear repeatedly in genetics textbooks in support of the claim that genetic variability contributes significantly to individual differences in intellectual performance.

The Erlenmeyer-Kimling and Jarvik Study

The most frequently *cited*, and certainly most prominently displayed evidence for the influence of genes on intellectual performance, is EKJ (1963). This brief review article included a figure based on correlation coefficients for mental test scores ostensibly from 52 different kinship studies. The importance accorded this figure in current texts is astonishing given its well-publicized shortcomings (including incorporation of Burt's studies). In 1974, Kamin wrote that: "The EKJ figure has been reproduced in countless psychology and genetics textbooks. . . . The influence of the EKJ paper is difficult to exaggerate" (p. 75), and Kamin warned of its unreliability given a number of methodological problems, including an incorrect report of the number of studies; lack of information regarding the rules used to select coefficients for inclusion in the figure, where different correlations for the same kinship category were reported in the original study; the tabulation of medians for different kinship categories in spite of the fact that different investigators studied different sets of categories, using different procedures, and different tests; and the inclusion of highly problematic data reported by Cyril Burt (pp. 75–87). Even in the absence of these (and other) specific criticisms, it is obvious that a summary of almost entirely unidentified studies conducted in the fifty years prior to publication of the review, i.e., beginning in 1911, and assumably including data fabricated by Burt, should not be reported in current texts as evidence for a high heritability of intelligence. [Four studies, of identical twins raised apart, *are* identified—by NFH, Shields, Juel-Nielsen, and "J. Conway" (a pseudonym of Burt's)]. Bouchard and McGue, who stand poles apart from Kamin in the controversy, write that: ". . . the accumulation of a great many new data along with the discrediting of Burt's important study on monozygotic twins reared apart has outdated that review" (1981, p. 1055).

Nevertheless, of the 19 textbooks devoting more than a paragraph to the issue of the heritability of IQ, 11 cited as evidence the EKJ review. (Another apparently bases a table on EKJ data, but no source is provided.) Indeed, 10 of the 11 prominently display the EKJ figure or a slightly simplified version of it (typically devoting a third to half of a page to the figure), one of them in a version eliminating Burt's data points. In some of these texts, the EKJ figure was the *only* evidence presented on the heritability of IQ—even where authors were apparently aware that Burt's data was worthless. One text included a two-page insert on "Scientific Fraud: The Case Against Sir Cyril Burt," while supporting its claim that "the measured heritability of IQ is relatively high" (Tamarin, 1982, pp. 291–292) by means of the EKJ figure (p. 291). The EKJ paper is almost always accorded great significance, as for example the following analysis of the (unidentified) EKJ figure in one recent text reading, "The study on which the figure is based is a summary of several separate studies, and it points out clearly the strength of the genetic component of IQ. As the genetic relatedness diminishes between pairs of individuals, the IQ correlation also goes down. . . . With such information available, it is hard to deny the importance of the genotype to the IQ trait" (Jenkins, 1983, p. 374). This author also remarks that Burt's data were "manipulated," consequently resulting in their "exclusion from current reviews. . ." (p. 375). Burt's data occasionally appear in other disguises as well, such as references to studies of separated identical twins where the totals (of twin pairs or studies) cited could only have been arrived at by including Burt's data. For example, one author has noted in the substantive discussion that "many very severe critiques have been made of Burt's work" and that "his coresearcher doesn't seem to exist," and cites Kamin, Wade, and Dorfman as references while illustrating the importance of genetic variability on differences in IQ with a diagram based on 122 twin pairs, 53 of which are necessarily Burt's (Pai and Marcus-Roberts, 1981, p. 605 and Diagram 18-4).

The Meaning of "Heritability"

Many texts also appear markedly confused in their treatment of the admittedly difficult, even counter-intuitive, *concept* of heritability. The authors of one textbook note that heritability ". . . is sometimes described in popular articles addressed to the average person as a measure of the degree to which a person's intelligence is determined by the genes inherited from parents. It does not mean this" (Wagner, Judd, Saunders, and Richardson, 1980, p. 544). But it is not only in the popular press that the heritability of IQ is defined as, or more frequently used as if it measured, the contribution of the genotype to an individual's intelligence.

Heritability is the proportion of phenotypic variance in a population that is attributable to genetic variance; a measure of great value in plant and animal breeding. (It is difficult to eliminate non-additive effects in studies of the heritability of human mental and behavioral traits since mating and environment cannot be manipulated by the investigator. Hence the statistics reported in human genetics are usually for "broad" heritability.) Whether broadly or narrowly defined, however, heritability does *not* measure the importance of genes in determining an individual's phenotype. The heritability for any variable trait in a population of clones is 0, not because genes are necessarily irrelevant to the expression of the trait but because there is no genetic variability in a clone. For example, a pair of identical twins is a small human clone (since members of the twin pair are genetically identical); hence there is no heritability of the differences between them. By the same token, when a mixed outbreeding population develops in a controlled, uniform environment, nearly all the variation must be genetic in origin and the heritability of any variable trait is therefore close to 1.0. It follows that the heritability of a trait tells us nothing about its genetic basis. One occasionally reads—though not in genetics texts—that "intelligence is not inherited." Such statements are as misleading as the counterpart "most of one's intelligence is inherited." Humans obviously have intellectual capabilities that frogs, or even other primates lack, and these species differences must have a genetic basis.

Nor is heritability an invariant property of any trait, since estimates of heritability vary with the mix of genotypes and environments, increasing if the environment becomes more uniform, falling if it becomes more variable. Hence, heritability estimates are not generalizable; they apply only to a specific population in a specific range of environments. Moreover, a trait may have a high heritability and be extremely sensitive to environmental change, as in a number of diseases with a broad heritability of 1.0 that can be cured through changes in diet (Lewontin, 1975, p. 389); heritability is not an index of plasticity. It is for these reasons—as well as their belief that it is virtually impossible to design experiments that will eliminate environmental correlation between relatives—that some geneticists have contended that heritability estimates for human populations (particularly broad-sense estimates) are meaningless. Within the field of biometrical genetics there is some argument over whether these estimates are of limited (scientific) value, or of none at all. For example, Hartl (1980) contends that what broad-sense heritability estimates convey "is not much information, to be sure, but it is something, and a little knowledge is probably to be preferred over total ignorance" (p. 310), but then he notes that they are thought to be of *social* significance only because of the mistaken equation of high heritability with insensitivity to environmental change. ("A herd of cattle maintained on a substandard diet could have a very high heritability for growth rate, for instance, but the easiest and fastest way to improve the average growth rate would be to supply adequate feed," p. 311.)

It is perhaps the belief that the heritability of IQ measures the extent to which our intelligence is determined by our genes that explains the importance accorded heritability estimates in many textbooks. In any event, incorrect and sometimes inconsistent uses of heritability abound in texts. In many cases, heritability is correctly defined as a ratio of the genetic to total variance (its character as a population measure may even be stressed) but then *used* as if it measured the contribution of heredity to an individual's phenotype. Moreover, this contribution is often treated (formal disclaimers notwithstanding) as though it were fixed. One leading text defines heritability as the "degree to which a given trait is controlled by inheritance" (Gardner and Snustad, 1984, Glossary, p. 6); another as a measure of "the genetic control of a trait" (Tamarin, 1982, p. 293). A third asserts that "A trait with a heritability of 0 has no genetic basis," and that "Perhaps as much as 75 percent of our intelligence is genetically determined" (Jenkins, 1979, p. 127). Another writes, "Animal and plant breeders find useful a value called *heritability* (H) which expresses the degree to which a trait is influenced by the genotype. . . . A heritability of 1.0 would indicate that the trait in question was produced solely by the action of the genotype. An example is the ABO blood group phenotype. A heritability of 0 means that the phenotype is due entirely to the environment, for example, the accidental loss of an ear or a tail. Intermediate values estimate the relative contribution of heredity, as opposed to environment, in the expression of a trait. Thus a heritability of 0.75 is an estimate that 75 percent of the expression of the trait is due to genotype" (Farnsworth, 1978, p. 93). Another author writes, "The value called *heritability* is used to express the degree to which the phenotypic expression of a trait is influenced by genetic factors. A heritability of 1.0 indicates that the trait in question was produced only by the action of the genotype and was not influenced at all by environment. The ABO major blood group phenotypes are an example of a trait with a heritability of 1.0. A heritability of 0 means that the phenotype is due entirely to environmental influences, such as the accidental loss of a finger or a tail. Values between 0 and 1 represent estimates of the relative contribution of heredity in the expression of a trait. A heritability of 0.65 is an estimate that 65% of the expression of the trait is due to genotype. The remainder of 35% would then be the proportionate influence estimated to be due to environmental influence on phenotypic expression" (Avers, 1984, p. 62). (The heritability of the ABO blood group is 1.0 because the environment is irrelevant to *variance* in the expression of the trait—a point the authors understand, yet fail to communicate in these passages, as a result of their use of misleading "shorthand" expressions.) Some texts are not so much inconsistent as incoherent, e.g., the assertion that "Heritability is a population measure and has no real meaning for an individual, except in an average sense" (Mange and Mange, p. 542). It would be possible, but perhaps not productive, to multiply examples.

CONCLUSION

How can one account for the generally poor discussion of the genetics of intelligence in introductory textbooks today? A large part of the answer is surely found in the carrying to an extreme of a practice generic to the writing of contemporary textbooks, namely, authors' liberal borrowing from one another or from a few older sources thought to be good authorities, such as Cavalli-Sforza and Bodmer (1971), Lerner (1968), Strickberger (1976), and Stern (1973). Few if any of the authors reproducing the EKJ figure, which plays such a prominent role in genetics textbooks, could have read the original 1963 review article in *Science*. If they had, they would presumably have recognized that Burt's research, under his own name and that of "J. Conway," had been incorporated. It also strains credulity to believe that some authors read this article but none of the critiques of it appearing since, in *Science* or elsewhere.

That the most frequently cited source for the claim of a high heritability of IQ is EKJ, and that data from NFH are also widely reported (if less frequently cited) along with the other "classic" studies of separated identical twins, including Burt's, can only be accounted for by the uncritical reliance of textbook authors on the discussions and data presented in other texts. Most of the updating in genetics textbooks appears to have occurred in the references, rather than the text content. Hence, there are textbooks whose substance is based exclusively on such sources as EKJ and the classic twin studies but whose reference sections cite Layzer, Lewontin, Kamin, Kempthorne, and other critics. (It is hard to escape the conclusion that the reference sections of many textbooks are constructed by someone other than the author or by the author working from the *Science Citation Index.*)

What then explains the extensive and undiscriminating use of material from other textbooks that is apparently the root cause of the inadequate treatment of the genetics of intelligence? I offer the following hypothesis, based on analysis of the textbooks and also on informal conversations with science editors and genetics teachers: liberal (and often unattributed) borrowing from other texts is not a practice specific to geneticists. On the contrary, authors of introductory textbooks in virtually all disciplines rely on other texts for summaries of work in fields where they lack expertise. Most textbooks have only one or two authors. They thereby avoid certain stylistic problems associated with multiple-authorship but also insure that the authors are specialists in only a few of the topics they treat. The use of material from other texts represents one solution to this problem; and it is a practice reinforced by the anxiety of publishers who know that instructors are less likely to adopt innovative textbooks (which require them to revise their lectures). However, the inclination to repeat what others have said is particularly pronounced in discussions of the genetics of intelligence. Most contemporary geneticists are probably uncomfortable (in contrast to geneticists of earlier generations) in addressing social, political, and ethical issues—which are invariably controversial and usually distant from their own interests. A reviewer of human genetics textbooks has recently noted that ethical issues are "often avoided in genetics texts" (Weisbrot, 1984, p. 384). The genetics of intelligence is an area where authors may also feel technically insecure—witness the often confused explanations and uses of the concept of heritability. Expected to write on a topic where they lack confidence but which they know is a source of controversy, authors are especially likely to turn for guidance to other textbooks or to a few sources thought to be good authorities. Perhaps what they find appears plausible because it confirms deeply rooted assumptions about the influence of genes on intellectual performance. In any event, whatever the cause(s) of the current situation, the consequence is clear. A majority of genetics students are being taught that intelligence is highly heritable (often linked to an incorrect and ideologically loaded concept of heritability) on the basis of evidence from studies that more properly belong in histories of science, or pseudo-science, than in contemporary textbooks.

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