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Chapter 20

The sociology of psychometric and bio-behavioral sciences: A case study of destructive social reductionism and collective fraud in 20th century academia.

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1. DEMONIZATION

The history of science abounds with examples of scientists killed, exiled, or demonized for presenting the right message at the wrong time or to the wrong people. A direct line thus connects the poisoning of Socrates with the public burning of Giordano Bruno, the Catholic Church's condemnation of Galileo's view, and the Spanish Inquisition's devilish torture chambers with The Soviet Union's classical geneticists having to fight for life against Central Party-dictated Lysenkoism. The present chapter updates this tragic history by telling a 20th century sociological story about the demonization of the psychometric and bio-behavioral sciences in general, and of Professor emeritus Arthur R. Jensen from University of California at Berkeley in particular.

1.1 The past

Religious, romantic, political, moral, or idealistic reasons motivated most of the persecutions. The medieval Church demanded, for example, that early cartographers

put the Garden of Eden at the head of their maps to cover "six-sevenths" of the Earth in land, in accordance with the Bible. The data-oriented Gerardus Mercator thought that this representation was not only inaccurate but also dangerously misleading to those who wanted to find their way. What is more – he had the courage to say so in 1544. He was accordingly imprisoned for heresy with the intent to burn him at the stake. Somewhat surprisingly, considering the Zeitgeist of the time, he was subsequently released for "lack of evidence" (Jenkins, 2000).

The leaders of the Amsterdam Jewish community forbade in 1656 any contact with the philosopher Baruch Spinoza with the following words: "Nobody shall have oral or written communication with him. Nobody shall help him. Nobody may come closer to him than four steps. And nobody may read anything published by him."

Voltaire publicly questioned the official wisdom of France, and subsequently faced personal persecution and exile. Not only was he found guilty in defending Descartes, Newton, and Pascal in Lettres Philosophiques, but he also referred to France as frivolous, superstitious and reactionary, and contrasted it to England. He had to hide in Lorraine in 1734 as the Paris police set out to arrest him. Voltaire did not mince his words, and dryly concluded: "It is dangerous to be right in matters on which the established authorities are wrong." If he knew that much, then why did Voltaire touch the matters at all? He provided that answer himself: "If I had not stirred up the subject (e'gaye' la matie're), nobody would have been scandalized; but then nobody would have read me." There are some truths that are better known to everybody, but somebody has to tell them. Voltaire and Art Jensen are equals here.

The ruthless hounding of classical Soviet geneticists, who dared questioning the demonstrably false Lysenkoist view and thus challenging the wisdom of The Central Committee of the Communist Party, extended the deadly line of destructive social idealism well into to the 20th century. It is not known exactly how many fell prey to communistic ideology, but some recent estimates count the numbers to about 100 million people. The Third Reich also persecuted artists and scientists, preferably Jewish, and killed, relatively speaking, roughly as many individuals as fell victims to communist ideology (about 10 million, but then allow for the shorter time frame for committing these horrible crimes against Humanity). Even if not immediately apparent, these two ideologies, the a-biological Communist and the mistaken race-biology based Nazi, had two very different but equally important roles to play in the demonization of psychometrics and the bio-behavioral sciences in the 20th century.

The communist ideology impact was to make the blind leading the seeing, and the Nazi ideology impact was to make everybody blind, deaf, and dumb to anything but Aryan supremacy. Both ideologies had a hostile attitude to counter-intuitive data.

While Eastern Europe has a long history of suppressing free speech and academic freedom, the West still sees itself as a prime example of individual and academic freedom, with the US in the forefront. This chapter purports to document that this is a false and dangerous illusion, in need of revision.

The point will be illustrated in different ways, but the overall purpose is to expose the perpetrators, count the dreadful personal, academic, and public consequences of this surprising and all-embracing example of a 20th century collective fraud, and to suggest a remedy. The chapter provides illustrations of what happened to western psychometricians, bio-behavioral scientists, and behavior geneticists devoted to data that ran counter to preconceived theories or idealistic, moral, or political ideas, but who were not afraid to "e'gaye' la matie're". The examples are mostly taken from what happened to Arthur R. Jensen, who had a formidable sharpness and the audacity to openly challenge the official and sacrosanct notions that social equality presupposes biological identity, and that social and racial malleability is without end. The price he paid was high indeed, but he never shook his hands, and that is his greatness.

1.2 Contemporary demonization

Many members of the London School of Differential Psychology, to which Hans Eysenck and Arthur Jensen also belong, has been demonized in the 20th century. The British psychologist Cyril Burt was, for example, accused of fiddling with his data on the similarity of twins. Because Burt was a leading proponent of the idea that intelligence is largely heritable, this cause célèbre was quickly exploited by social reductionist critics to throw a deadly blow to the entire notion of inheritance of intelligence. Yet, both the previous and the later methodologically better studies of the heritability of intelligence have come up with figures that, on average, compare favorably with Burt's original numbers. What remains of substance of the much hailed defamatory attacks is that an ageing Burt probably was inexcusably careless with the presentation of his own data. The really interesting question has now changed to the question why so many critics still find the Burt case a good reason to reject the entire notion of the major inheritance of intelligence in face of the fact that, once you

remove all Burt's data and use only the updated and technically much better evidence, it does not change one iota of the conclusion that genes count for about 80 percent of the familial transmission of genes for intelligence in late adulthood (but seemingly much less in childhood!)

When the late Hans Eysenck succeeded Burt as a prominent member of the London School, he also got viciously attacked for a life-long promotion of the study of individual differences with a non-exclusive emphasis on the biological side of human nature (see Nyborg, 1997). Ironically, his critics associated his biological interest with underlying Nazi sympathy. It apparently made no impression on critiques that Hans had to fly his native Germany after being beaten up by schoolmates for refusing to join the Hitlerjugend. He even dared to openly challenge his Nazi schoolteacher in class when they were told that Jews were inferior people. Young Hans loved data, so he simply went to the local library to collected evidence that Jewish soldiers were, on average, more highly decorated than other German soldiers fighting in the First World War. Eysenck was not a Jew himself - just an unusually intelligent and brave young man! This bravery found good use in his long-life defense of psychometrics and the biological basis of personality and intelligence. He had to endure physical attacks and personal harassment in countless ways, and to have his lectures blocked at home or abroad.

The late Raymond Cattell may be considered a special kind of member of the London School. He was shamefully denied reception in the last minute in 1997 of a medal for a lifetime achievement award in psychology, endorsed by The American Psychological Association. The initiative to withhold the medal came from Barry Mehler, who also proposed that the late Stanley Porteus should no longer give his name to Porteus Hall at the University of Hawaii (for a characterization of Mehler, see Weyher, pp. xl-xliii, in Lynn, 2001). Mehler seems to have devoted his entire career to attacks on psychometrics and bio-behavioral research, and he has repeatedly attacked the Pioneer Fund for racism (ibid.). This fund supported the research of some members of the London School, as well as scientists outside the circle. Chris Brand, a longtime tenured lecturer at the Edinburgh University, was sacked, and had to endure that his 1996 book: The g factor: General Intelligence and its Implications, was "de-published" by Wiley. The publisher simply took the book off the shelf where it had been on for sale for 6 weeks. Philippe Rushton of University of Western Ontario, Canada, was very close to being sacked and persecuted for "hate speech", and was

actually subjected to a criminal investigation, that ended with nothing. A publisher withdrew and destroyed 45.000 copies of an abridged 2000 edition of his Race, evolution, and behavior, originally published by Transaction Publishers. With characteristic stamina Rushton successfully countered all the wild accusations and kept on with his important work (see chapter 9 in this volume). Thomas Bouchard from Minnesota University in the US, an internationally recognized specialist using twins to study the inheritance of intelligence and personality, has routinely been ferociously attacked over many years. So has sociologists Bob Gordon from the Johns Hopkins University and Linda Gottfredson from the University of Delaware, and many others. Readers interested in the unworthy details of these rueful stories may like to consult Lynn (2001).

1.3 The demonization of Arthur R. Jensen

The above examples was meant to illustrate the fact that anybody critical of the prevailing social reductionism were demonized during the past two third of the 20th century. The attacks came not only from individuals, but also from academic institutions, professional organizations, and the public media.

However, the attacks took on a particularly nasty form in the case of Arthur Jensen - perhaps because he has this tremendous capacity to accumulate solid data and to derive clear implications. The rule of the attackers seems to be that the better the data, the more vicious will be the punishment. The 16th century treatment prescribed for Spinoza looks surprisingly alike to the 20th century treatment given Arthur R. Jensen: Stay away from him! Don't believe him! Disrespect him! Don't read him! Stop him!

Luckily, all this made no impression on Art. He followed Voltaire's advice to "e'gaye' la matie're", and refined the measurement of general intelligence *g*, critically discussed individual and groups difference in *g*, and enquired into the inheritance of *g*. There is no doubt, had Art not "e'gaye'ed" "la matiere" we would probably today have missed the most reliable and broadest applicable general measure psychology has ever devised. That would have been a sad story for the individual, the group, society, and for science.

2. JENSEN – THE SCIENTIST

2.1 Introduction

Arthur Jensen is the perfect case for illustrating which and how particular sociological components go into a well-coordinated attack on academic freedom. First, he is an impeccable scientist – at least in the eyes of all experts in his field. Moreover, he was able to radically change his mind in accordance with new data about restrictions on human development, but he also continued to use the classical methodological tools of psychology. I will on this basis argue that it was not a change of methods, but rather a change of mind, that made him the prime target for countless vitriolic attacks for years to come. The change of mind transformed him, in the eyes of his critics, from being a brilliant educational scientist with a non-offensive mainstream view and a clear devotion to better the conditions for the disadvantaged, into being a bad researcher doing bad science, and that for evil motives.

I am fully aware that all this may sound a bit exaggerated or even somewhat paranoid. Below I will therefore be very specific about each of the above stated claims. I will first substantiate the claim that Arthur Jensen is widely acknowledged by colleagues in his specialist fields – educational psychology and psychometrics – as a primus inter pares, then, that he is a master of methodology, that he did not fiddle with the data, that his findings are mainstream, and that he has no racial inclinations whatsoever.

2.2 Jensen – the impeccable educational psychologist

A recent special issue of Intelligence praises Jensen as a professional in the full meaning of that term and as a person with extraordinary qualities (Detterman et al., 1998). In fact, a reference to a passage by Galton (1869, pp. 24-25) - "Kings among Men" was used to characterize Jensen as "A King among Men". Moreover, no less than four of his books or journal articles have reached the status of citation classics - defined by the Institute for Scientific Information as works with an unusually high frequency of citations in the scientific and professional journals. He is the 47th most cited psychologist in the twentieth century, and 12th among the 19 still alive (Haggbloom et al., 2002). In other words, judged by his academic success and the accolades, Jensen is a prototype of a high-caliber professional.

2.3 Jensen – the infamous

It is therefore puzzling to see that the same Detterman could state with great confidence in the same 1998 issue of Intelligence, that Arthur Jensen will never receive the honors he rightly deserves. To fully understand this we have to go back to one fatal day in 1969, where Jensen's professional and personal life suddenly changed almost overnight. The day before he was a young honorable scientist with a promising career. The next day he was an outcast, rightly deserving verbal and physical abuse. It even became acceptable to many scientists that Jensen and his family deserved to live with realistic life threats. Ongoing projects were compromised, teaching made difficult, his office had to be secured, and his presence at campus required the company of bodyguards for personal safety. While on campus, angry students would regularly shout in choir: "Professor Jensen is in sight – he is teaching genocide". Over the next 30 years he would experience again and again that invited lectures at other universities in several countries were blocked by angry demonstrators. On one occasion he actually had to run for his life under the protection of 50 police officers, only to escape after being locked up in a closet for hours, and then "rescued" from the "scene of crime". As late as in 1999 demonstrators made an attempt to disturb an invited lecture at Edinburgh University, and he had to return to the States after being obstructed in delivering the honorary "Galton Lecture" in London, UK.

Threatened and ridiculed at the personal and professional level, he had to get used to see fearful politically correct professional organizations promote far less qualified colleagues to stardom in the academic and professional hierarchies. Not that I have ever heard Arthur Jensen express even the slightest personal interest in stardom, but even a Jensen cannot get around the fact of academic life that the ultimate measure of one's professional standing is reflected in the recognition by our colleagues. Jensen is indeed "a man that will never receive honors!" So, what is wrong with him?

2.4 Jensen's methods, data, and interpretation

Did Jensen really deserve the extreme punishment from colleagues, organizations, and the public? Perhaps he began to use shaky methods way back in the late 1960s? This obviously is not the case. The methods he used before and also after 1969 are pretty much standard in psychology. In fact, Art is generally acknowledged as one of the methodologically most skilled professionals in his field. In addition to exploiting

classical techniques he has developed new sophisticated tools (such as correlated vector analysis (Jensen, 1998, appendix B), and I am not aware of any serious critique of these..

Did he begin to fiddle with his data around 1969? Wrong again. In the hundreds of attacks on him one rarely find any accusation of questionable data, and in the few actual cases I have yet to see an instance where the discrepancy could not be explained rationally.

It is, of course, quite common in the history of science to see individuals get ousted from the good company for producing far-off-mainstream findings. Did Jensen begin to get "unusual" or "unexpected" results after 1969? Not at all! All his main observations confirm what everybody else finds in the field using similar techniques. In fact, those who have had the good fortune of working closely together with him know painfully well, that he is extremely careful (bordering to the pedantic, if I may say so, Art?) in accepting what counts as good methods, solid empirical data, and sound interpretation. In that respect many of his critics do not match him by half. Even more interesting, there is now considerable consensus in professional circles with respect to most of Jensen's main conclusions – those he arrived at before, as well as those reached after 1969. Most of the basic problems he addressed can now be considered basically solved, and research can safely progress in new directions (Jensen, 1998), which is precisely what Jensen, and many others with him, are busy doing right now.

2.5 Is Jensen a racist?

In the HER article Jensen (1969) began the section on Race Differences by stating: "The important distinction between the individual and the population must always be kept clearly in mind in any discussion of racial differences in mental abilities … Whenever we select a person for some special educational purpose ….we are selecting an individual, and we are selecting him and dealing with him as an individual for reasons of his individuality … The variables of social class, race, and national origin … are irrelevant as a basis for dealing with individuals." Later Jensen writes: "Furthermore, since, as far as we know, the full range of human talents is represented in all the major races of man and in all socioeconomic levels, it is unjust to allow the more fact of an individual's racial or social background to affect the

treatment accorded to him. All persons rightfully must be regarded in the basis of their individual qualities and merits, and all social, educational, and economic institutions must have built into them the mechanisms for insuring and minimizing the treatment of persons according to their individual behavior."

This is hardly the view of a racist generalizing blindly and derogatorily across hundreds or thousands of individuals. We rather see the fingerprints of a responsible and careful educational psychologist with an open eye for existing individual variation, irrespective of race. I will later get back to the puzzling question how on earth Jensen's critics could nevertheless accuse his of being driven by a contemptible, fundamentally racist attitude.

2.6 Is Jensen opposing racial desegregation?

Could all the hate directed against Jensen be partially a function of him opposing racial desegregation in schools? To the contrary, Jensen has always maintained the position of being opposed to segregated schools (e.g Jensen, 1972, p. 51). He is concerned, however, that segregation takes place so that all children benefit from it, as racial balance in schools may not by itself solve existing educational problems. Educational diversity and desegregation need not be incompatible goals, he says, but " … ignoring individual differences in children's educational needs could be most destructive to those who are already the most disadvantaged educationally. The allocation of a school's resources for children with special educational problems cannot be influenced by race; it must be governed by individual needs. Making an association … between the "nature-nurture" question and the issue of racial desegregation of schools is, in my opinion, a most flagrant non sequitur." (ibid., original emphasis). Again, this is hardly the view of a racist segregationist, but rather a balanced expression of concern for the disadvantaged, irrespective of color.

2.7 If not Jensen – then who is to blame?

If Jensen really is not to blame, then who are? Many of Jensen's opponents came from what can somewhat loosely be called the academic left (Gross & Levitt, 1994/1998). My first tentative hypothesis was, accordingly, that the demonization of Jensen was a simple function of a predominantly academic left-wing dissatisfaction with the notion of a largely inherent human inequality in intelligence, threatening their honorable idea

of basic solidarity with the poor. This quickly appeared to be much too narrow an analytic framework, even if the gusty impact of the academic left remained central in the auspices of an extended model (to be presented later).

3. A SIMPLE MODEL FOR DEMONIZATION

3.1 Introduction

It gradually transpired that the full answer to the question of whom to blame for the demonization of Jensen, required nothing less than a full-scale analysis of all the sociological components that interacted to produce the war-like climate surrounding psychometric, differential psychology, behavioral genetics, and the bio-behavioral sciences in general. The analysis required a focus going far beyond the left or right oriented ideologies of some of the combatants. It had to involve also the academic institutions and the public context in which it unfolded.

The analysis was accordingly divided into two parts, even if the overall purpose of the analysis was to expose the destructive play of social reductionism, amounting to a collective fraud, committed by surprisingly many academics and their organizations, by irresponsible universities, and by some professional and even cross-national organizations, all seemingly guided by a debilitating political correctness ideology.

The deeper irony of all this is that the collective fraud seems originally planted by the academic left in order to promote human happiness and solidarity with the disadvantaged, but it ended up by killing both of these honorable intentions, in addition to seriously threatening the academic freedom of individuals, and thereby the entire foundation of modern universities.

The first part of the analysis takes on a very specific form - that of a single case pseudo-experimental study, with a few independent variables and a mapping of the kind and level of demonization. The second part of the analysis, to be presented later, incorporates a number of semi-dependent variables that are useful for the full appreciation of the intricate pattern of collective fraud, spun in a worrisome unison by many parts of modern academia and the public. The variables will be defined as they are used, but a brief overview of all variables in the two-part study may help grasp the larger picture.

Jensen radically changed his mind in late 1960s, and this change appears in the first analysis as an independent variable with two modes: one biologically neutral

and uncontroversial, the second one biologically related and deeply offensive. The second independent variable in the first study pertains to major variations in the prevailing Zeitgeist in the 20th century, a parameter with 4 modes.

The remaining variables, introduced in the second stage of the analysis, include genes for an (in)flexible personality that make it difficult to change one's mind, religious beliefs, moral and ethic agendas, an idealized search for "truth", a widespread fear of elitism, racism, sexism, or inequality, a subjective craving for pedagogical optimism, an urgent need for scientific recognition among peers, the explicit or implicit moral objectives of funding by organizations, public media ideologies, the desire for being political correct and, finally, the implicitly or explicitly stated purposes of political, professional, and academic programs.

It is a fact that there is no simple co-variation among these many inter-dependent variables. Sometimes they act mainly in isolation to restrain individual academic freedom, sometimes synergistically, or they may even interact non-linearly – possibilities that obviously do not facilitate an effect analysis.

3.2 Independent variables in the first part of the analysis

3.2.1 The two Arthur Jensen decision modes. The first decision mode is a pre-1969 mainstream science nurture mode with uncontroversial implications. The second is a post-1969 nature mode with controversial implications.

In the first mode Jensen concentrated on laboratory research and theoretical problems, involving university undergraduate and serial rote learning problems, a topic as far removed as one could get from the focus of his later 1969 HER article on IQ, achievement, race, and genetics. Then, in the mid 1960s Jensen decided to radically change his mind. This section maps the purely empirical reasons for the change, and demonstrates that it had nothing to do with subconscious or preconceived attitudes.

Jensen entertains the personal philosophy that even if a scientist is mostly interested in theory he/she should try and bring this expertise to bear on practical problems whenever needed. Therefore, when a school psychologist asked him to point out a good culture-free or culture-fair test that would work for children diagnosed as educationally mentally retarded (EMR) Jensen – as the helpful person he always is - accepted the challenge. He first did some empirical work to empirically confirm the

school psychologist's suspicion that available tests were quite valid for white middle-class children but did not work well for minority lower-class children. Despite an IQ of 75 or lower, EMR children did not seem nearly as retarded as the white middle-class children with comparable IQs. Thus, when compared to white middle-class children with similarly low IQ, they appeared much brighter socially as well as in playful interactions. Jensen began to wonder whether it was possible to devise a testing procedure to bring this phenomenon under closer scrutiny. The first step was to ponder whether most IQ tests actually assessed prior learning outside the test room, where minority children obviously might be culturally disadvantaged. Next step was to develop various "direct learning tests" that measured the rate of learning something new in the test room. In this phase of clarification, Jensen realized that culturally disadvantaged EMR children from low SES homes performed much better relative to middle-SES EMR children with the same low IQ. He took this to mean that the direct learning tests picked up important behavioral and cognitive differences between low-SES and middle-SES EMR children that the usual IQ tests simply missed.

A first reflection on the results suggested that the culture-fair test had been invented. But Jensen did not stop there. With the characteristic incisiveness that permeates all Jensen's research, his ensuing research involved large-scale studies of school children. Moreover, Jensen also "… inevitably became deeply immersed in the rapidly growing educational literature of the 1960s on the psychology of the culturally disadvantaged – at that time a new term for the children of the poor, specially racial minorities such as Negroes, Mexican-American, Puerto Ricans, and American Indians, as well as poor whites." (p. 7). Unfortunately, many of the reports at the time were still in the form of unpublished research reports, and they were accumulating rapidly, so Jensen, with characteristic meticulousness, "… felt a need to scan all these reports, winnow them to find the most substantial and methodologically sound studies, classify them, and digest and organize the results into a reasonably coherent body of knowledge which could be summarized in a book …" (p. 7).

The attempt to develop tests fair to culturally deprived low SES minority children must strike everybody as laudable. So must the colossal work Jensen put into the systematization and updating of the relevant educational literature. I have taken quite a number of colleagues by surprise when informing them that Arthur Jensen

truly worked along such lines. They knew for sure from several critical sources that he was the prime enemy of the deprived of this world – in particular of blacks.

However, a genuine surprise was also in store for Jensen. In his own words: "What stuck me as most peculiar as I worked my way through the vast bulk of literature on the disadvantaged was the almost complete lack of any mention of the possible role of genetic factors in individual difference in intelligence and scholastic performance. In the few instances where genetics was mentioned, it was usually to dismiss the issue as outmoded, irrelevant, or unimportant, or to denigrate the genetic study of human differences and proclaim the all-importance of the social and cultural environment as the only source of individual and group difference in the mental abilities relevant to scholastic performance. So strongly expressed was this bias in some cases, and so inadequately buttressed by any evidence, that I began to surmise that the topic of genetics was ignored more because of the particular author's social philosophy than because the importance of genetic factors in human differences had been scientifically disproved. … At that time I was largely but not utterly ignorant of the research on the genetics of mental abilities" (pp. 7-8). It became obvious to Jensen that, in order to fully understand what caused individual difference in intelligence and scholastic performance, he also had to review the total world literature on the genetics of human abilities. That was the frugal time for a radical change of mind, informed basically by data and in a spirit of a genuine surprise.

He wrote a number of articles on what he had learned. The articles elicited an invitation to talk in 1967 at the annual convention of the American Educational Research Association (AERA; Jensen, 1968). At the meeting he pointed out that present education had failed by not taking into account innate or acquired difference in abilities. He further pointed out that the ideal of equality of educational opportunity can actually do harm, quite like a physician treating all patients with the same medicine. He finally noted that optimal instructional procedures may not be discovered if we do not take into account the wide range and diversity of abilities, with the effect that we may unwittingly alienate many children.

In the process of reviewing literature, Jensen became impressed with the Coleman report on Equality of Educational Opportunity, published in 1966. This study was based on more than half a million children, and presented massive evidence that " … discrepancies in educational achievement by different social class and racial

groups are correlated to only a slight degree with inequalities in those variables over which schools traditionally have control. The data made it abundantly clear that biological and social environmental factors associated with social class, race, and family background accounted for most of the variance in intellectual ability and scholastic performance." (p. 10). At the next annual meeting of the AERA in 1968 Jensen presented his Level I- Level II theory on a triple interaction among social class, intelligence, and rote learning ability.

The two AERA addresses led to an invitation to write the now (in)famous article "How much can we boost IQ and scholastic achievement" for the Harvard Educational Review (HER; Jensen, 1969). The invitation was quite explicit, and the reader is strongly urged to carefully inspect the outlay in toto, presented at page 11 in Jensen (1972). Thus, contrary to what many still believe today, and in face of the fact that the HER editorial board later denied it, Jensen was explicitly invited to comment, among many other things, on his "… position on social class and racial differences in intelligence" (my emphasis). The article was published on 28 February 1969. This is the day the basis for Jensenism was established: "The theory that an individual's IQ is largely due to heredity, including racial heritage. [1965-1970]; after Arthur R. Jensen (born 1923), U.S. educational psychologist, who proposed such a theory; see –ism] – **Jen'sen-ist, Jen'sen-ite'**, n., adj." (e.g. The Random House and Webster's Unabridged Dictionaries).

The HER article proved that Jensen had felt forced by solid empirical evidence to switch from mode 1 of publicly laudable neutral research on serial learning effects and on the helping of the culturally disadvantaged, to mode 2 stressing 1) the existence of individual and race difference in intelligence, 2) the failure of compensatory education, and 3) that a purely environmental hypothesis may perhaps not any longer suffice and 4) that help for the disadvantaged better acknowledges the differences.

Then all hell broke loose. To fully understand the violent dynamics of this we have to connect Jensen's change of mind to the prevailing Zeitgeist mode, in which the HER article surfaced.

3.2.2. Zeitgeist modes. The model therefore operates with four Zeitgeist modes: 1) A pre-1940 form where biological explanations were generally accepted, 2) a 1940-1980 blank slate form where Lysenkoism, behaviorism, and hostile anti-nature attitudes dominated, fuelled partly by communist ideology, partly by Nazi misuse of

eugenics to promote nasty genocide programs 3) a brief 1980-1990 relational-interpretative form of anti-science interlude and, finally 4) a post-1990 period where new evidence from progressive neuro-biological sciences (molecular sciences, neurochemistry, neurophysiology, neuroimaging) and behavioral genetics slowly begin to make biological explanations partly acceptable to at least some researchers outside orthodox academic left circles.

This simple model predicts that had Jensen presented his newly established conclusions in the HER article during the pre-1940 Zeitgeist mode 1 he would have received largely laudatory critique. This was the time when eugenics movement flourished in many countries, and it was quite common to talk seriously about a genetic basis for individual and group differences in intelligence. Biological explanations were generally accepted and recommendations by the political right as well as by the left were used in support of "progressive" social policies in many countries.

Segerstråle (2000) mentions several factors that may explain the turning away from mode 1 biological or nature explanations toward the mode 2 "official environmentalism" or nurture explanations so domineering in the twenties and thirties in the US. Among them are the growing social influence of immigrants and northern urban blacks, the Great Depression, a growing skepticism against social Darwinist arguments, and the dwindling support for the eugenics movements after reports of escalating Nazi sterilization practices. Add to this the 1968 American Anthropological Association unanimous resolution to denounce racism (Degler, 1991), and it becomes understandable that "… there was a dramatic decrease in articles on race and sex differences (ibid, pp. 203-205). Segerstråle also cites Barkan (1992) and Degler (1991) for observing that anthropologists Franz Boas, Ruth Benedict, and Margaret Mead were successfully promoting the notion of the importance of culture over biology in explaining behavior, and Provine (1973) for noting "… the strong political drive for an environmentalist attitude in academia this time." The UNESCO 1952 statement made it virtually illegitimate to use race as an explanatory factor; it emphasized that there were no differences among the races, and this was largely accepted on face value by large sections within academia and also outside it. Barkan (1992, pp. 342-343) found that "… biological explanations [were] replaced by cultural

analysis. Rigid views of hierarchies among human groups largely yielded to relativism and indeterminism."

From 1990 onwards, a number of philosophical and text-reading movements were formed, where deconstructionism, post-modernism, and debunking of science prevailed. Many of these movements included hostile reactions towards biological thinking but also science in general.

The tides slowly change again about 1990 or so, thanks to the exponentially increasing knowledge from functional genomics and the molecular sciences, combined with truly breathtaking developments in behavior genetics and brain imaging sciences. Surprisingly, even if it has once again become somewhat acceptable to mention the biological side of human nature - at least in some circles - in mode 3 and 4, this is, unfortunately, also is the time when political correctness prevails not only in academia at-large but also in the public press. Post-modernist theory and "standpoint" epistemologies make some progress in debunking science, and Jensen's lectures are still occasionally obstructed during this period.

Anyway, Jensen had no choice but to present his mode 2 thinking in the HER article in the middle of the 1940-80 mode 2 Zeitgeist, simply because that was the time when he first discovered that he in the first part of his professional career had seriously underestimated the biological side of his work. Jensen has, as mentioned, an unusual high regard for data, he is honest, and he is willing to let science be guided by data, even if they speak against his previous view.

Finally, he had the personal flexibility needed to turn around 180 degrees and re-interpret the observations in the cold light of new and better evidence. This is more than can be said for many of his opponents. As will be demonstrated later, them openly distrusted unequivocally good data, and even admitted in public that they preferred to interpret them in reassuring light of already preformed notions (i.e. they subjected texts to moral reading, see below). The difference in the mental flexibility of Jensen and his critics will play a role in the second part of the analysis.

As soon as Jensen's decision mode 2 collided with Zeitgeist mode 2, the following predictable but unworthy series of events played out.

4. THE ATTACKS

4.1 The immediate reactions

Jensen's HER article was immediately given unprecedented publicity, and many of the reactions could be likened more to an emotional hullabaloo than to presenting counter-factual evidence.

I will give a fairly detailed description of the reactions, because I know from personal experience that many people simply find it impossible to believe the many unworthy postures of "honorable" scientists. They either flatly reject that the unfair attacks on Jensen ever took place, or they may say that he most certainly deserved a "qualified response", or they may even call it a balanced discussion. To counter such an evasion from facts, I will in this section draw heavily on Jensen's own account of some the details of the retaliations from the time of the publication in 1969 and up to 1971 (Jensen, 1972).

After reading the section, I will ask the reader to judge the scientific honesty of those same scientists who questioned Jensen's honesty, as he went from decision mode 1 to 2 in Zeitgeist mode 2. To be fair, the later section on "Defenses" also outlines the basically positive reactions, but the present section serves the main purpose of illustrating the nature and causes for the inexcusably bad academic climate. It was so bad that Jensen exclaimed in despair, "Most of the main points of my [HER] article were never mentioned, being completely displaced by the racial issue, which was often a grotesque parody of what I had actually written."

4.1.1 Reactions in or by academic journals. It is instructive to first monitor the timid reactions of the editorial office of the Harvard Educational Review in the aftermath of Jensen's 1969 HER article (1972). The Board undoubtedly was under great pressure from many individuals and organizations as part and parcel of a collective fraud (see later), but the ensuing events "… are unprecedented in the history of scholarly publication in America …" because "… the Boards academic wisdom and adherence to traditional principles of scholarly publication were pathetically wanting." (p. 23).

Among other things, the Board sent out a false statement, denying that they actually had invited Jensen to comment on race differences. The board then denied Jensen a copy of the statement, but sent it out to everybody else asking for it. The Board then halted the Winter issue with Jensen's article and declined orders from University bookstores. The reason they gave for this was that "The Jensen article …

presents a view of intelligence that we feel must be read in the context of expert discussion from other psychologists and geneticists." Apparently, what they really meant was that academics cannot be trusted to think for themselves, and so they needed the proper guidance by the critics of Jensen to reach a "correct view". The Board then decided not to sell reprints to Jensen of his own article, even if everybody else could order them. Next, it was hinted that Jensen's rejoinder to the critics could not be published in the ensuing Spring issue, but rather would appear in the much later Summer issue. They then reversed this decision, but only after massive intervention. Still, the Board refused to sell reprints of the original article to Jensen, even if other authors could still obtain copies of their articles the usual way! However, after the Board was reminded by eight " … faculty members of the Department of Educational Psychology at a large Eastern university …" that the "interim distribution of the article appears to be at best anti-intellectual and at worst a form of censorship." (p. 26-27), Jensen was finally "allowed" to buy copies of his own article!

It may be hard to believe, but the sad story of the cowardice of the HER editorial board does not end here. The Spring issue was planned to have four or five discussants of Jensen's original article, but was upped to seven. Being fair, as always, Jensen found that for the most part they were "…. reasonably thoughtful, scholarly attempts to deal with the issues by my paper.", and characterized by a "… generally moderate tone and lack of any essential disagreement with the main points of my article …" (p. 27). However, the Board had refused to publish previously invited papers from two high-caliber solicited contributors - Ellis Page from University of Connecticut and Michael Scriven from University of California at Berkeley - despite being delivered on time. The two papers apparently did not only fail to sufficiently "put down" Jensen's stance. They even dared take a critical stance on his critics. Being under such pressure the Board apparently could not allow itself to take any chances.

Worse was still to come! The Summer 1969 issue of HER contained some twenty articles and letters "… most of them only masquerading as serious critiques of my article. Likening me to Hitler (p. 592) … was apparently not beneath the Editorial Board's standards …" as was not the fact that some of these articles "… contained factual, methodological, and theoretical errors and unsubstantiated accusations against my article." The Board further accepted to publish Deutsch's strong claim that

"perhaps so large a number of errors [in Jensen's article] would not be remarkable were it not for the fact that Jensen's previous work has contained so few, and more malignant, all the errors referred to are in the same direction: Maximizing differences between blacks and whites and maximizing the possibility that such differences are attributable to hereditary factors (p. 254)". It is telling to note that Deutsch was not able to back up his charges despite repeated requests to do so. When finally forced by demands of the Committee of Scientific and Professional Ethics and Conduct of the American Psychological Association, Deutsch came up with a "… by any standard … pathetic document." (p. 28-29 in Jensen, 1972).

The Board of HER demonstrated further anomalies. It now refused Jensen the right to rejoinder to the critique! The Nobel Laureate in physics, William Shockley, fared no better. He was able to demonstrate that there were fatal errors in one of the critical HER articles: "Social Allocation Models of Intelligence: A Methodological Inquiry" by Light and Smith from Harvard University. The model suggested that even if the heritability of intelligence was as high as .80, the mean white-black IQ difference could be accounted for entirely in terms of environmental differences. What Shockley demonstrated was that the model generated a number of completely absurd results, highly discrepant with common observations. Despite the fact that the Shockley paper expressed no opinion at all in the matter of race differences, but simply pointed out essential logical infirmities and wide discrepancies from well-known facts in the Light and Smith article in HER, the Board nevertheless refused to publish Shockley's critique. This is another example of HER's inexcusable censure, pure and simple. Shockley's paper was eventually published in another journal in 1971.

4.1.2 Reactions by Academic Institutions

4.1.2.1 The American Psychological Association. The powerful American Psychological Association sponsored a division called The Society for the Psychological Study of Social Issues (SPSSI). This division issued on May 2, 1969, a statement, meant to discredit major points in Jensen's 1969 HER article. Parts of the statement were aggressively distributed to newspapers across the nation and to several professional journals, to be published in toto (e.g. American Psychologist, November, 1969, pp. 1039-1041). The statement contained remarkably sweeping counter-

conclusions (but no data) about observations for which there already was substantial confirmation, or the arguments were twisted. For example, it said "… we believe that statements specifying the hereditary components of intelligence are unwarranted by the present state of scientific knowledge … such statements may be seriously misinterpreted". Not one word about the massive confluent evidence from twin and adoption studies. It stated that "The evidence points overwhelmingly to the fact that when one compares Negroes and Whites of comparable cultural and educational background, difference in intelligence test scores diminish markedly." No mentioning of the fact that when one controls for education much of the IQ variance is taken away. The statement said that "... a more accurate understanding of the contribution of heredity to intelligence will be possible only when social conditions for all races are equal and when this situation has existed for several generations." It was not stated that this, obviously, would make all future studies on race difference virtually impossible, nor did it acknowledge that such a restrictive condition was not really called for, either.

With respect to compensatory education it said: "One of our most serious objections to Jensen's article is to his vigorous assertion that compensatory education has apparently failed." "We maintain that a variety of programs … have been effective and … carefully planned intervention … can have a substantially positive influence on the performance of disadvantaged children". One should have thought that APA sponsored honest scientists would have felt obliged at this point to back up their strong counterclaim with clear evidence, or that APA would have asked for it.

The statement further pointed out "… a number of Jensen's key assumptions and conclusions are seriously questioned by many … It is thus an oversimplification to try and explain complex behavior in terms of "heredity versus environment" (original emphasis). Having examined Jensen's data "… we find that observed racial differences in intelligence can be attributed to environmental factors." Present-day intelligence tests are "Largely developed and standardized on white middle-class children …" and "… tend to be biased against black children …".

It may be hard to believe, but SPSSI people then reaffirmed their "… long-held position of support for open inquiry on all aspects of human behavior." They emphasized in particular that "… in the study of human behavior a "variety of social factors may have large and far-reaching effects… " so " … the scientist must examine

the competing explanations … and … exercise the greatest care in his interpretation." I feel confident that at least some APA ears have turned red, at least in retrospect.

Jensen's response came promptly, and was published in the same November issue of American Psychologist. Had Jensen actually set heredity versus environment or denied the possible impact of a variety of social factors in his HER article (or elsewhere, for that sake)? Not at all! What he said was: "The preponderance of the evidence is, in my opinion, less consistent with a strictly environmental hypothesis than with a genetic hypothesis, which of course, does not exclude the influence of environment or its interaction with genetic factors." (p. 82, my emphasis). Moreover, Jensen explicitly warned readers against the error of pitting heredity versus environment in a section sub-headed "Heredity versus Environment" (pp. 44-46).

It may very well be that the SPSSI people capitalized on the chance that even responsible scientists would not themselves take the trouble to read Jensen's original HER article, but the question still remains: Why on earth should the SPSSI people lie openly and want to blatantly misrepresent Jensen's position? How could honest APA scientists hold a "… position of support for open inquiry …" when they at the same time call for the impossible scenario that the social and cultural condition for whites and blacks must be kept equal for generations, before anybody can even publish in the field? My guess, and that of others is, that such a claim is a camouflaged attempt to censure, and to cover a closed mind that would forever preclude proper analysis.

The SPSSI people claimed that IQ tests are inevitably biased against black children. This is patently wrong, but this claim had at least one good effect. It made Jensen undertake the formidable task of reviewing the entire world literature on test bias. This resulted in a book (Jensen, 1980), which confirmed that well standardized tests contain no ethnic bias when properly used, and when applied in other cultures with proper caution.

With respect to the SPSSI claim that compensatory education programs are effective and notably lift the performance of disadvantaged children – where was their documentation for this? Where is the evidence today, a third of a century and millions of dollars later? True, when one compares blacks and whites, holding education and culture constant, the usual 15 IQ point difference in IQ shrinks but, as mentioned previously, this takes a sizeable part of the variation out of the equation as IQ differences accounts for a significant proportion of the educational variation. Perhaps

the SPSSI people ought to take seriously their own call to "… exercise the greatest care in … interpretation."

4.1.2.2 Reactions by The American Anthropological Association. On March 5, 1970, the American Anthropological Association (AAA) presented a list of 16 resolutions to all its members, in which they obfuscated Jensen's position, implied positions he never held, and called Jensen a "chauvinist, biased racist".

Resolution 15 thus concluded that Jensen's article "… is not consistent with the facts of psychology, biology or anthropology." It said that "… Jensen's article is wholly inadequate …", and that "All races possess the abilities needed to participate fully in the democratic way of life and modern technological civilization."

Resolution 16 then requested that all members return to their homes from the meeting and "… use all available outlets in the national and local media to inform the general public concerning the correct facts about the nature of human variability."

Like with the previous APA statements, we again see a seriously flawed statement from a "responsible" professional organization, reflecting a chilling lack of obligation to present "facts" to substantiate of their strong counterclaims. Instead of presenting all relevant data they ran a data-free cheap-shut vendetta against Jensen.

As usual, Jensen got it right when he commented: "In science the only thing that really counts is a preponderance of the facts and converging lines of evidence." (1972, p. 42). This honest view apparently does not resonate well with broad professional psychology and anthropology circles, and makes one wonder what science really meant to these corrupt moralizing and politicizing organizations.

4.1.3 Claims from other sides for breach of honesty and ethics. Jensen had further reason to wonder. He repeatedly wondered why his critics could get away easily with vicious ad hominem attacks– an approach so readily embraced by wide circles - while suspending most critical and scientific standards?

He wondered why there apparently were no cost associated with writing in a nationally syndicated newspaper: "Some of the more outraged souls, black and white, would like to settle the whole thing by proving that they have IQ enough to tie a noose that will fit Jensen's neck."

He wondered why six distinguished Berkeley social science professors could get away with writing in the Berkeley student newspaper that Jensen "… was extremely naïve about the nature of cultural differences in test performances" whereas nobody apparently bothered to ask the distinguished professors what precisely they had done to enlighten us? Could they muster more than pure and simple despect? We actually do not know till this day!

Jensen wondered why he could not hear the voices of the remaining hundreds of social science professors in this discussion? Obviously, even first year students with a rudimentary understanding of fair play and knowledge of the basic rules of science ought to have felt obliged to get the record straight? Few did. As I will argue later, we here begin to see the vague contours of a far-reaching collective fraud with the purpose of framing Jensen. They could neither frame him on his data nor on his methods, but they could exploit the frontal collision between Jensen's politically incorrect mode 2 nature decision, and their own beloved Zeitgeist mode 2 nurture conviction.

One discussant in HER Spring 1969 issue claimed that Jensen was "girding" himself for a "holy war against environmentalists". Did any of the other critics go back to Jensen's 1969 HER article to check for themselves whether his position was war-like or not, and faithfully went back to correct this untrue statement? Not one, as far as I can see! Did any of the critics double-check Jensen's major conclusion, and reported back that Jensen actually provided clear and frank support to the notion that environmental factors were also important? Not one. Possible faint attempts to correction drowned in the mud.

There are two competing interpretations of all this. Either, most of the environmentalists did not themselves read Jensen's source text but relied on misrepresenting second-hand sources, as ammunition for their crusade. Or, they actually read the original but subjected it to "moral reading" (Segerstråle, 2000), whereby the "… critics [of sociobiology] employed a particular style of textual exegesis … aimed at revealing the true meaning [of sociobiology]" so that "… the critics' interpretation of the true meaning [of sociobiology] came to overrule their targets' protests. The critics profited from the prevailing post-war taboo on biological explanation of behavior." (p. 2).

Considering the sometimes no more than superficial similarity between the sociobiology and the IQ "wars", it seems a reasonable assumption that Jensen's critics also applied the "moral reading" approach when studying IQ and race texts. They could, of course, also have headed for something else, partly obscure to themselves, in a self-perceived "non-war" against Jensen, but I will concur with Segerstråle, that moral reading is the more likely interpretation.

A group called "Psychologists for Social Action" urged at the Annual Spring 1969 convention of the Eastern Psychological Association, that Jensen should be expelled or at least censured by the APA. There is no registration of what other members at the meeting had to say to this tactics, which reminds me of other sinister epochs in history. Spinoza, Voltaire, and the witches of all times would surely have recognized the patterned silence surrounding controversial matters. Apparently, no scholars openly disagreed with the mob at the meeting. Perhaps some honest members bend their head in shame, but most kept their mouth shut for personal comfort.

In this way Jensen's critics could at no apparent costs question the existing and well-documented individual and group differences in intelligence. Considering their earlier call for open inquiry and honest assessment, it is almost empirically bizarre that the critics called upon the Rosenthal & Jacobson 1968 study - Pygmalion in the Classroom - which concluded that increased self-esteem improves performance. Perhaps they hoped, by some sort of analogy, that black IQ could be raised by improving black self-esteem? What critics did not say, perhaps did not know or, more likely, did not care about is, that all later major reviews of the Rosenthal effect have come out negative. There is, in fact, no support at all for Rosenthal's strong claim, and all replications of the original study have failed to confirm the idea that teacher expectancy raises IQ or promotes scholastic achievement. All this seems to boil down to a rather obvious strategy: Rather than openness and honesty, the professional Eastern Psychological Association and, by association, the authoritative APA, were trying to frame Jensen according to the prescription: Don't care about science, as long at the attacks visibly harm Jensen!

4.1.4 Campus activities. Various handbills were passed out on campus asking students to join demonstrations in Jensen's class. Placard-bearing students gathered at the University's Board of Regents with the message: "Fire Jensen", or held up such

placards under his office windows while shouting “Fight racism! Fire Jensen”, or pamphlets with his picture and the text: “HITLER IS ALIVE AND WELL AND SPREADING RACIST PROPAGANDA AT BERKELEY”. Come and help fight in the struggle against racism at Jensen’s class!”. To attain maximal effect, time and place of the lectures was kindly provided. At the same time a sound-truck circled campus with full volume on its loudspeaker for the simple message: “Fight racism! Fire Jensen!” Slogans scrawled on his office door or in the elevators: “Jensen Must Perish” or “Kill Jensen”, kept appearing despite being removed as fast as they were scribbled.

“Students for a Democratic Society” (SDS) made up their own screwed definitions of true democracy and academic freedom. They thus succeeded in preventing a lecture at the University of California’s Salk Institute at La Jolla campus, Jensen reports, by continuously clapping hands in relay, so as not to tire out. After about an hour of this, the lecture was called off. The lecture next day had to be delivered to privately announced invitees. This strategy angered the SDS students so much that the campus police at Berkeley got wind that the SDS Berkeley chapter had held a rally to plan reprisals with threats so virulent that it was deemed advisable that Jensen should be accompanied on the campus, to and from classes, and in the parking lot, by two plain-clothes bodyguards, for two weeks. I wonder precisely which kind of democracy they had in mind. Most appalling, it appears that neither their professors, nor anybody from campus administration, saw able to comment on the deep irony here. Most everybody dug for cover, but not Jensen.

4.1.5 Threats to the home. Three years after the publication of the HER article threatening phone calls were still made at home late at nights, despite an unlisted phone number. At one time the threats were deemed so realistic by the police that the Jensen family had to abandon their house and move elsewhere for a while.

4.1.6 The silencing of colleagues. Jensen was far from alone in being harassed and in having his rights to free speech hurt. Luckily, some of these colleagues neither accepted to be silenced. Professors Richard Herrnstein, William Shockley, Philippe Rushton, and others also had their lectures cancelled by demonstrators. In 1971 Herrnstein wrote an article in The Atlantic Monthly suggesting that a society based on equality of opportunity would turn out to be a society where social stratification is based on IQ classes. The idea was originally set forth by Young (1958) and further

elaborated in 1994 by Herrnstein and Murray in The Bell Curve, and convincingly confirmed by others, including Gottfredson (chapter 15 in the present volume). Herrnstein's lectures were interrupted, and posters were carried around on campus with the text: Wanted for racism.

4.2 The later reactions

4.2.1 Introduction. Did all the fuss end there back in the early seventies? Jensen certainly hoped so. A little more than three years after the original publication of the HER article he wrote in the preface to his Genetics and Education (1972): "The storm of ideologically, often politically, motivated protests, misinterpretations, and vilification prompted by this article has by now fortunately subsided, with most encouraging signs of being displaced in professional journals and conferences (and now to a large extent even in the popular press) by rational and sober consideration of the educational and societal implications of the important issues raised in this article. The heat and smoke have largely abated, which is all to the good; yet the concerned interest of the kind I had originally hoped my article would stimulate has continued to grow."

Jensen surely was up for a great surprise here. His positive evaluation of the situation in academia kind of reflected the wishful thinking of an honest and hard working scientist, who wanted to go back to work again. Little did he anticipate the heat still in store for him for another 30 years in the 20th century. The unmeasured amount of outright hatred, personal persecution, defamation, and vilification even spilled over into the 21st century! In 1999 demonstrators tried to block a lecture by Jensen at the Edinburgh University. Jensen was asked to give an invited Lecture at the Galton Institute in London, but demonstrators successfully took over the arrangement. The police was called in, but they apparently was not asked to make any difference to troublemakers and scientists: they simply cleared the building for everybody! Jensen had to return to the US without being able to address the audience.

I fear that the damage done by the dismissive organizers of the meeting not only allowed for an obvious breach of free speech, but it also provided a clear message to the demonstrator about how to succeed in future actions with no personal risk at all!

4.2.2 Salvador Luria. The molecular biologist at MIT and self-declared socialist, Salvador Luria (Luria, 1974a; b), was interviewed by Segerstråle about his view on Jensen, IQ, and race research in the early seventies. A Nobel Laureate, Luria saw himself perfectly justified in straightforwardly dismissing IQ research as scientifically and socially useless, and in accusing Jensen and IQ research of just politicizing.

Luria said: "Jensen started an article in the Harvard Educational Review by saying that compensatory education had been tested and it had failed. That was not so, and I fought. … because that was a political, a straight political issue, white vs. black … Jensen's was a definitive political action … IQ data are a reasonable predictor for … certain people['s] … function in a certain type of school … beyond that, that IQ has any relation to anything … from the point of view of success in other ways, I would say it cannot be denied, but there is zero evidence here. I read a little bit more: there is zero evidence to me …… there is no evidence for intelligence … having expert teachers interview children we would get much more information than in IQ tests … those tests … are not based on any scientific background. You see, it has something in common with Creation science. You say something, and then you insist it may be so because somebody said it in a book. … claims about a high heritability of IQ [are] 'nonsense' … the question of how to get the most out of each person according to his or her ability was not a biological problem. These were all 'socio-political traps' beyond the scope of science. " (interviewed in February 1982, and reported in Segerstråle (2000, pp. 245 ff., italics added by Segerstråle).

I have previously dubbed such an approach The Lord Nelson strategy (Nyborg, 1972). You put the sextant in front of your blind eye, and report that you see nothing. This was precisely what Lord Nelson did, and he commanded the British fleet to continue bombarding Copenhagen, even after the Danish King had presented the white flag for surrender within sight. The total destruction of the city bastions and abduct of the Danish fleet were the goals, and fair play or correct observation had nothing to do with it.

In other words, either Luria is a simple ignorant opening his mouth too much in matters he obviously knew not enough about, or he deliberately looked away from solid data and perhaps thought he could get away with a gross misrepresentation of Jensen's position and data. It is certainly to be hoped that young 21st century scientists will inevitably get very uneasy whenever they see how cavalierly scientists of the

highest ranks thought they could gallop to sweeping conclusions, riding on fast horses but with surprisingly little empirical baggage. Luria's statement: "I read a little bit more: there is zero evidence to me …" comes true in a way that perhaps does not serve his image as a responsible scientist well!

4.2.3. The Sociobiology Study Group of Science for the People (1976a; p. 182) also rode on fast horses. They simply declared to the world, that: "The claims that there is a high heritability of IQ … have now been thoroughly debunked."

4.2.4. The American Anthropological Association. A major critical attack on Jensen and IQ research was further launched in the form of eight articles, collected under the title Race and intelligence by The American Anthropological Association in the early seventies (Brace, Gamble & Bond (1971). The titles alone tell a story, if neither about strict scientific objectivity nor about neutrality: "The Promotion of Prejudice", "Cultural Myopia", "Illogical IQ Theory", "Flaw in Jensen's use of heritability data", "Pseudo-issues", "Racialist Comeback", "Inadequate Evidence and illogical Conclusions", "How Racist use 'Science' to degrade black people" or "Jensen's dangerous half truth".

In addition, the authors accused Jensen of one-sidedness, and The American Anthropological Association endorsed the accusation! It may be no coincidence that Franz Boas was one of the founding fathers of this organization, and that Margaret Mead and other luminaries of his school were loyal members (see later).

4.2.5. Richard Lewontin and Stephen Jay Gould. Ullica Segerstråle (2000) provides an interesting analysis of the last quarter of the 20th century research on sociobiology. I will in several instances in this chapter draw on Segerstråle's excellent analyses, partly because she points to parallel events in the equally heated sociobiology and IQ debates, and partly because she enjoyed a unique insider position in the critics' camp. However, I part company with her interpretation of Jensen's role in the controversy (see later).

Segerstråle notes that Richard Lewontin, professor of biology at Harvard University, a member of the Sociobiology Study Group, was considered by many the chief opponent of sociobiology and " … the upholder of good and moral science

against bad and dangerous pseudo-science." (p. 18). Here bad science means science that can be socially abused, whereas good science produces pure knowledge. Another vocal member of the group was professor at the Museum of Comparative Zoology, Stephen J. Gould. The study group later connected to the Boston chapter of Science for the People, a national forum for left-wing academic activism (Walsh, 1976), under the name The Sociobiology Study Group for Science for the People.

Segerstråle was granted observer status at some of their meetings and reports on critical discussions of "biological determinism" and on psychometric studies showing a sex difference in math ability in an atmosphere " … of righteous moral indignation at dangerous 'biological determinist' theories and their creators." (p. 21). The group was very active and successful in promoting their view, and was even granted a two-day symposium at the meeting for the prestigious American Association for the Advancement of Science (AAAS) in Washington, DC, in February 1978, to carry through well-attended critical discussions of sociobiology.

Segerstråle's account of the personal attacks on sociobiologist Edward O. Wilson at the meeting looks like a déjà vu of what had already happened to Arthur Jensen: "Just as Wilson is about to begin, about ten people rush up on the speaker podium shouting various epithets and chanting: 'Racist Wilson you can't hide, we charge you with genocide!' While some take over the microphone and denounce sociobiology, a couple of them rush up behind Wilson (who is sitting in his place) and pour a jug of ice-water over his head, shouting 'Wilson, you are all wet! (p. 23).

Again we see the previously mentioned disturbing aspect of the obvious attempts to censure free speech: Nobody from the AAAS intervened. No officials showed the demonstrators and mockers of academic freedom to the door, or called the police to have them doing it. This particular type of irresponsibility on the part of officials is an unhappy feature that we will see repeating itself in many later situations where Arthur Jensen and others came under attack. It may be no coincidence that Stephen Jay Gould was later called to preside over this organization (see later).

4.2.6. Edward Wilson's reservations. Segerstråle raises an interesting question: Why was sociobiologist Wilson not more cautious about suggesting links between genes and human behavior, when he saw how badly Jensen and Herrnstein were treated earlier? But he was, she observed. On page 554 in his major opus

Sociobiology Wilson actively played down the social significance of IQ - despite clear evidence to the contrary! In fact, Wilson went out of his way to downscale the importance of IQ in the last chapter of Sociobiology and instead emphasized other bases for social success. Frankly, I find it hard to believe that a man of Wilson's stature and insight did not know the facts, such as those presented in chapter 15 in this volume, or much earlier in the 20th century. Wilson even tried another common strategy to avoid being framed like Jensen - he succumbed to the idea that race is not a meaningful biological concept.

However, these concessions did not help Wilson a bit, because the academic leftists nevertheless applied their "moral reading" strategy and became able to reveal the "hidden message" in his writings as reflecting a justification of existing social and racial inequalities. His was a no-win position, even if he downplayed the race and intelligence cards, and neither were Herrnstein's and Jensen's, whom certainly did not downplay any of them.

4.2.7. Lewontin. It is an interesting twist that Lewontin accused American academics for falling back to old attitudes and using "… untrue statements, facts which are not facts, logic which is not logic, and prove that there are important genetic differences between races" (1975b; c) while, at the same time, the Civil Rights Act in 1964 prohibited discrimination in hiring and thus promoted equal opportunity ideas and affirmative action, and countered notions of inequality, racism, sexism, biologism, conservatism, and elitism.

4.2.8. The New York Times. Segerstråle takes it as a good illustration of how firmly the academic intelligentsia was holding on to "… the 'total' environmentalist position… " when The New York Times in 1973 published a Resolution against Racism, signed by over 1,000 academics from different institutions all over the US. Not only did it declare: " …all humans have been endowed with the same intelligence." It also condemned the research by Jensen and other as both unscientific and socially pernicious. It went as far as to threaten, that 'racist' researchers deserve no protection under the name of academic freedom" and it urged liberal academics to resist "racist" research and teaching."

This culpable resolution indicates that more than 1,000 scientists in the US thought that scientific results are to be construed or annulled by simply signing a pamphlet. The resolution reminds me of the prescriptions the Jewish community in Amsterdam gave for the perpetrator Spinoza in 1656, of the Nazis prescriptions for how to treat Jews, artists, and homosexuals in the 1930s and 1940s, and of the pamphlet signed by hundreds of German scientists to testify on the bad quality of Albert Einstein's "Jewish" science. Not without good humor, Einstein later remarked that just one good argument would have sufficed.

Alas, there is little reason for humor in the fact that so many American 20th century scientists had learned so little from the horror stories of fascist or communist suppression of scientists or artists with "entartede" or "false consciousness" views. The prominent member of Science for the People, Joe Alper (1982) bundled Edward O. Wilson and Arthur R. Jensen under one hat, and declared to the world that they together were "the scientific racists of the past" rather than "the Ku Klux Klan or the Birchers." Do we see guilt by association and blood from the past spilled over honest scientists on a low-cost basis? Did any of the thousand+ scientists have any quarrel with that?

4.2.9. Who is lying: Plato and Jensen - or Gould himself? Gould (1981) devoted a whole book to expose Plato's and Jensen's lies, and called it The Mismeasure of Man. Gould said: "This book is about the scientific version of Plato's tale. The general argument may be called biological determinism" and is about "…the claim that worth can be assigned to individuals and groups by measuring intelligence as a single quantity." (p. ii, original emphasis).

Gould was even more specific, when he in 1996 let the 1981 version of The Mismeasure of Man reprint. He now "…treats one particular form of quantified claim about the ranking of human groups: the argument that intelligence can be meaningfully abstracted as a single number capable of ranking all people on a linear scale of intrinsic and unalterable mental worth … this limited subject embodies the deepest (and most common) philosophical error, with the most fundamental and far-ranging social impact, for the entire troubling subject of nature and nurture, or the genetic contribution to human social organization." (p. 20, original emphasis).

The result of ranking people according to intelligence in a single series of worthiness is, according to Gould, "… invariably to find that oppressed and disadvantaged groups – races, classes, or sexes - are innately inferior and deserve their status. In short, this book is about the Mismeasure of Man." (p. 21).

But who is lying here? The simplest and most direct way to find out is to transcend the borders of academia, and check for oneself whether people out there in the real world can in fact be ranked usefully by Jensen's general intelligence *g* measure, in a way that makes sense in terms of test reliability and predictive validity. Gottfredson and many others have already taken the trouble to collect the relevant evidence, and the reader is urged to inspect the results in chapter 15 in this volume.

Gould, of course, knows of these data, but he does not accept their usefulness. Why not? Because Gould sees Howard Gardner's concept of multiple intelligence as "… the major challenge to Jensen in the last generation, to Herrnstein and Murray [1994] today, and to the entire tradition of rankable, unitary intelligence marking the mismeasure of man." (p. 22). Gardner's exceedingly broad definition of intelligence allows for an easy and attractive escape from one-dimensional intelligence ranking. Thus, most people are good at something; it may not be intelligence as traditionally defined, but if we just call it intelligence we can justifiably say that most people are intelligent. If we just incorporate talents for dancing or football, for understanding other people, or oneself, or nature, we can establish a multidimensional realm of intelligence that supplants the single series of unworthiness measure, and proves that oppressed and disadvantaged groups – races, classes, or sexes - are not innately inferior and deserve their status. Apparently, we don't even have to establish scales for measuring these intelligences (Gardner has not), we don't have to check whether four of these intelligences inter-correlate significantly and reflect *g* (they do), and we don't have to take into account whether the remaining intelligences inter-correlate significantly (they don't), or whether they have predictive validity (they don't; see Jensen, 1998, or consult chapter 19 in this volume).

4.2.10. Gouldian self-promotion. Having demonstrated in The Mismeasure of Man that Plato and Jensen are lying, Gould goes on to assure the reader that he feels quite competent in doing what he must do: "I feel I have a decent and proper grasp of the logic and empirics of arguments about biological determinism. … I am fully up to

snuff (I would even be arrogant and say "better than most") … in fallacies of supporting data. … my special skill lies in a combination … rarely combined in one person's interest … special expertise in handling large matrices of data … I therefore felt particularly competent to analyze the data, and spot the fallacies, in arguments about measured differences among human groups. … I therefore found my special niche [and] … combine the scientist's skill with the historian's concern." and focus upon "… deep and instructive fallacies (not silly and superficial errors) in the origin and defense of the theory of unitary, linearly ranked, innate, and minimally alterable intelligence." (p. 24-26).

Gould is, in his own words, not at all bothered by such a narrow-minded complaint as: "Gould is a paleontologist, not a psychologist; he can't know the subject and his book must be bullshit." That is simply nonsense, Gould says: "The subject that I did chose … represents a central area of my professional expertise – in fact, I would go further and say … that I have understood this area better than most professional psychologists who have written on the history of mental testing, because they do not have expertise in this vital subject, and I do." (p. 40). Given this formidable insight, what then has Gould to say about the measurement of intelligence he so detests?

4.2.11. Gould on factor analysis. Gould assures us that he feels at home in judging factor analysis, the purpose of which is to derive common axes in a positively correlated data matrix. He was therefore terrified to learn that this technique "… might have arisen in a social context to a particular theory of mental functioning with definite political meaning ... that Spearman had invented the technique of factor analysis specifically to study the underlying basis of positive correlation among test."

What was so terrifying about that? Well: "… principal components of factor analyses are mathematical abstractions, not empirical realities – and … every matrix subject to factor analysis can be represented just as well by other components with different meanings, depending on the style of factor analysis applied in a particular case. Since the chosen style is largely a matter of researcher's preference, one cannot claim that principal components have empirical reality (unless the argument can be backed up with hard data of another sort …" … "Spearman had invented factor analysis to push a certain interpretation of mental tests – one that had plagued our century with its biodeterminist implications." … "Factor analysis had been invented

for a social use contrary to my beliefs and values." I felt personally offended … and this book … ultimately arose from this insight and feeling of violation. I felt compelled to write The mismeasure of Man." "Furthermore … the harmful hereditarian version of IQ had not developed in Europe … but in my own country of America, honored for egalitarian traditions." (pp. 43-44). The mathematics of IQ testing, "… the key error of factor analysis lies in reification, or the conversion of abstractions into putative real entities." (1996, p. 48).

Perhaps Gould's fear would have been even larger had he fully understood the nature and power of factor analysis, a topic treated with exceptional expertise by world authorities like John Carroll (1993; or chapter 1 in this volume) or by Jensen (1998: The *g* Factor book).

4.2.12. Gould on biological determinism. Why is biological determinism so dangerous, asks Gould? ": … because the errors of biological determinism are so deep and insidious, and … appeal to the worst manifestations of our common nature … reductionism … reification … dichotomization … hierarchy … When we rejoin our tendencies to commit these general errors with the sociopolitical reality of a xenophobia, that so often (and so sadly) regulates our attitude to "others" judged inferior, we grasp the potency of biological determinism as a social weapon – for "others" will be thereby demeaned, and their lower socioeconomic status validated as a scientific consequence of their innate ineptitude rather than society's unfair choices." (p. 27).

If we do not counter it we will see: "… resurgences of biological determinism correlate with episodes of political retrenchment … or … fear among ruling elites, when disadvantaged groups sow serious social unrest." "What argument against social change could be more chillingly effective than the claim that established orders, with some groups on top and other at the bottom, exist as an accurate reflection of the innate and unchangeable intellectual capacities of people so ranked?" "Resurgences of biological determinism correlate with periods of political retrenchment and destruction of social generosity." We must therefore raise awareness, that "… calls for solidarity among demeaned groups should not be dismissed as mere political rhetoric, but rather applauded as proper reactions to common reasons for mistreatment."(p. 28).

The reader is here invited to speculate on which direction Gould's fear would take if biological determinism were not an error of interpretation but a fact of life. Would Gould blame Nature for the destruction of social generosity, and to what effect? Moreover, if we knew more about the causes or mechanisms of biological determinism, would we not be better able to intervene and much more effective in easing the conditions for the disadvantaged? Gould's hostile and square position leaves no room for alternatives to blaming Jensen and others for things they are not responsible for and actually tries to counter.

4.2.13. Gould on individual and group differences. Arthur Jensen is responsible, in Gould's opinion, for one such recurrence "… with a notoriously fallacious article on the supposed innateness of group differences in IQ … " which coincided with " … the onset of a conservative reaction that always engenders renewed attention for the false and old, but now again useful, arguments of biological determinism." (p. 30). Gould does not even consider that Jensen actually published his HER article precisely at the time when he realized that he had seriously under-estimated the biological impact on development, and had to switch to decision mode 2. Gould just could not resist the temptation to politicize the change and claim it coincided with a conservative swing. Ironically, Zeitgeist mode 2 points to the golden heydays where academic leftists like Gould had their greatest hit rate in fighting what they saw as biological determinist attitudes. Apparently, to Gould the matter is just a question of interpretation – yours or mine? And whatever you say or write, it has to reflects your moral or political stand!

However, Gould did not consider updating his 1981 The mismeasure of Man book until the The Bell Curve by Herrnstein and Murray, surfaced in 1994. The Bell Curve signified, in Gould's opinion "…a swing of the political pendulum to a sad position that requires a rationale of affirming social inequalities as dictates of biology" where "… the theory of unitary, rankable, innate, unalterable intelligence acts like a fungal spore, a dinoflaggellate cyst, or a tardrigrade tun – always present in abundance, but in an inactive, dormant, or resting stage, waiting to sprout, engorge, or awake when fluctuating eternal conditions terminate slumber." Should anybody be particularly surprised that the "… publication of The Bell Curve coincided with … a new age of social meanness unprecedented in my lifetime …" and that this new "…meanspiritedness [is consonant] with an argument that social spending can't work

because, contra Darwin, the misery of the poor does result from the laws of nature and from the innate ineptitude of the disadvantaged?" (p. 32).

Again Gould manages, in a florid and hostile manner, to tie an empirically loaded work, drawing upon solid data collected by hundreds of scientists over several decennia, to subjective motives reflecting the most evil and asocial tendencies of his time.

4.2.14. The critics as rational firefighters. This tactics makes it understandable why Gould and other critics so often emphasize the meanspiritedness, the notorious fallacy, the falseness, and the social meanness of Jensen and others. We just have to combine the moral reading style of the critics with their left oriented position and pessimistic view on the lack of solidarity with the poor, and we see immediately why the critics simply must define themselves as defender of human freedom, equality, and dignity, and why they felt they had to assume a very active outgoing role here. Lewontin, Rose and Kamin (1984) provide at good example of this in the following passage, characterizing their almost "Einsatz kommando"-like urge:

> "Critics of biological determinism are like members of a fire brigade, constantly being called out in the middle of the night to put out the latest conflagration, always responding to immediate emergencies, but never with the leisure to draw up plans for a truly fireproof building. Now it is IQ and race, now criminal genes, now the biological inferiority of women, now the genetic fixity of human. All these deterministic fires need to be doused with the cold water of reason before the entire neighborhood is in flames." (p. 266).

Gould stresses again and again the urgent need for policing academia, because, in the brutal but necessary fight against biological determinism we must:

> "… never flag in our resolve to expose the fallacies of science misused for alien social purpose …" for a simple reason: "We pass through this world but once. Few tragedies can more extensive than the stunting of life, few injustices deeper than the denial of an opportunity to strive or even to hope, by a limit imposed from without, but falsely identified as lying within." (Gould, 1996, p. 50).

It pays off to ponder again whether it is Nature, and not Jensen, who stunts life and denies opportunities? Just think for a moment, if the new insight from the molecular and brain sciences is combined with behavioral genetics' brand new way of defining the impact of environmental factors (within versus between family, and shared versus non-shared), would hold the best promise for optimizing the conditions for the deprived? Gould never entertains such a possibility, because he sees evil plots everywhere, and surely knows whom to blame!

4.2.15. Postmodernism. According to Segerstråle (2000) the "old" academic left eventually partly transformed itself, so that: "The new 'cultural left' in academia, … instead focused their energy on postmodernist theory and 'standpoint' epistemologies, where sociobiologists were " … now being dismissed as old-fashioned defenders of the truth" (p. 308).

Seen in this perspective, it is perhaps little surprise to note that some postmodernists express a rather hostile attitude to IQ testing. In the recent symposium - Psychological Assessment from a Social Constructivist Point of View - at the XXVII Meeting of the International Psychology Congress in 2000 in Stockholm, Sweden, Yvonna Lincoln and Kenneth and Mary Gergen questioned the basic validity and legitimacy of psychometric test.

Some of their critique was directed at the idea that test administrators actually believe in an objective reality. Testers further assume that they can measure and predict the characteristics of the objects. Testers believe that their methods of measurement are independent of what they measure, and that the choice of measures will not influence the studied subject. Test administrators believe that observer status is objective, but this is suspect because, irrespective of the unit of measurement and method, objectivity is compromised by the theoretical orientation and purpose of the study.

A further problem with IQ testing is, according to Kenneth Gergen, that psychometrics disregards our relational and situated connectedness. Mary Gergen went on to question the value of psychometric studies of individuals, because what really is measured is the construction of the meeting between the tester and the tested, and the chosen method sets the agenda for what actually can be seen. It is, in fact, the semantic content that defines the understanding of the individual under scrutiny.

Yvonna Lincolm finally questioned the entire legitimacy of psychology as a discipline, because it is based on the test ideology taken from psychometrics - "we" can and "the others" cannot. To solve this crisis we have to open up for a constructive dialogue about ideologies, according to Lincoln.

This is a good example of a straw-man approach. The postmodernists first set up a completely unrealistic description of the blatant idiocy of IQ testers, and then shoot them down in one cheep shut. Frankly, I have never met any serious psychometrics subscribing to such outdated positivist positions, neither have I found any example of it in the modern psychometric literature.

Let us turn the postmodern critique on its head for a moment: How long time and how cheaply can the of psychometrics get away with notoriously sidestepping the massive evidence for the high psychometric test reliability and predictive validity, amassed over close to a century. Post-modernist critics repeatedly violate the "Total evidence rule" by reporting a fraction of the empirical evidence as if it was all. Such meetings nevertheless attract a large and often enthusiastic crowd. When they return to their home institutions they eagerly share their important new insight with students and colleagues. As this happens again and again, I am forced to conclude that something amounting to a collective blindness to certain data has infected much of modern academia.

5. THE DEFENDERS

5.1 Introduction

Even if the resistance to Jensen's work was overwhelming, there were also some notable examples of scientists who dared to defend Jensen, even if this brought them right into the frying pan as well.

5.1.1 Edson and Stevens.

One of them was Lee Edson (1969) from New York Times Magazine. Jensen found that he stood out as producing a " … thorough, thoughtful, and well-balance story …" on the incident. Edson's article stimulated more letters-to-the-editor than any other article New York Times Magazine had ever received.

In one such letter Harvard psychology professor S. S. Stevens expressed the opinion that: "The environmentalists have had the microphone in recent years and

they have talked up an American brand of Lysenkoism, which holds that brain power can be taught. That notion draws much of its powerful appeal from the hope we all feel that somehow we can shake the world and make it better, right now. Practically everybody is trying to improve somebody."

Stevens further wrote: "That concept of the IQ has, I believe, proved itself the most important quantitative concept contributed thus far by psychology", and that " we gain nothing by turning our backs on the process of biological inheritance which sets the design for our size and appearance, and for much of our behavior."

5.1.2 Bereiter. Another defender was Bereiter (1970), who inspected all the early fuss and came to the interesting conclusion that apparently "the educator need not concern himself with genetics because, in the first place, he is constrained to working with environmental variables and must therefore do the best he can with them …. and because, in the second place, education deals with individual children of unknown genetic potential, so that normative data on genetic differences have no application." (p. 298).

However, even if valid points for the teacher in the classroom, they are potentially relevant at the level of educational policy dealing with populations rather than with individuals. Here, individual differences in intelligence should encourage us, according to Bereiter, to look for alternative teaching methods that do not rely so heavily upon IQ abilities, and also influence our expectations of what can be accomplished.

5.1.3 Zigler. Also Zigler (1968) revealed little patience with the environment reductionists: "… our nation has more to fear from unbridled environmentalists than … from those who take the biological integrity of the organism seriously. It is the environmentalists who have been writing review after review in which genetics are ignored and the concept of capacity is treated as a dirty word. It is the environmentalists who have placed on the defensive any thinker who … has had the temerity to suggest that certain behaviors may be in part the product of read-out mechanisms residing within the programmed organism. It is the unbridled environmentalist who emphasizes the plasticity of the intellect, who tells us one can change both the general rate of development and the configuration of intellectual

process, which can be referred to as the intellect, if we could only subject human beings to the proper technologies. In the educational realm, this has spelled itself out in the use of panaceas, gadgets, and gimmicks of the most questionable sort. It is the environmentalists who suggest to parents how easy it is to raise the child's IQ … It is the environmentalist who have argued for pressure-cooker schools, at what psychological costs, we do not yet know."

5.1.4 Shockley. A much less forgiving critic of social reductionism and equality-makings, than Jensen, was that of the late physics professor and Nobel Laureate William Shockley, mentioned earlier. He urged without success the U.S. National Academy of Sciences to sponsor research on the genetics of intelligence. Shockley diagnosed the major problem here as a thought-blockage caused by a theologico-scientific delusion, called the "apple of God's eye obsession" – God meaning for some the proper socio-biological order of the universe. True believers hold that God has designed nature's laws so that good intentions suffice to ensure humanity's wellbeing; the belief satisfies a human need for self-esteem. Any evidence counter to man's claim to be the "apple of God's eye" … provokes retaliation … or else the … obsession had to be painfully revised." An important antithesis to a feature of the contemporary form of the "apple of God's eye obsession" is, according to Shockley: "… the theory that intelligence is largely determined by the genes and that races may differ in the distribution of mental capacity". (Shockley, 1971a, p. 307).

5.1.5 Davis. Davis (1978) also went to the rescue. He found that the critics were confusing the normative with the empirical while falling prey to "The moralistic fallacy", because they suffered from a "fear of facts." Perhaps this fear emanated on the basis of a fear of potential social misuse of data! Davis (1976) certainly thought so when commenting on research in the effects of having an XYY karyotype and on behavior genetics in general: "… I suggest … It is the conviction that an attention to genetic factors in behavior will have reactionary social consequences … " and that "… attention to genetic factors in behavior 'only serves to propagate the damaging mythology of the genetic origins of "antisocial behavior", and so it interferes with the job of eliminating the social and economic factors involved in such behavior."

However, we should never, in the words of Davis, try to 'legislate the facts of nature'. Davis (1986) also commented on the raging IQ debate, and on Gould's frontal

attack on IQ research in general and on Jensen in particular. He stressed that Gould's critique of research on race and sex differences in cognitive abilities rested mainly upon outdated craniology and other mistakes of the past, whereas Gould largely omitted the much more sophisticated contemporary approaches, thus misleading the public about current research. Instead of truthfully reporting on reliable methods and high predictive validities, Gould questioned whether general intelligence, *g*, really existed at all. Logically, as he concluded that it does not, he accordingly also had to dismiss its heritability. This would be the coup de grâce to the idea of IQ being inherited.

5.1.6 Page and 50 American scientists. Ellis Page united with 50 other scientists, including Jensen, Eysenck, Herrnstein, and four Nobel price winners, to send out a resolution (Page, 1972). The resolution was a reaction to the fact that reporting on the importance of heredity for human behavior had "… brought psychologists and other scientists under extreme personal and professional abuse at Harvard, Berkeley, Stanford, Connecticut, Illinois, and elsewhere."

After referring to antiscientific moves in the past, the statement reported on today's "…similar suppression, censure, punishment, and defamation …" where "…positions are often misquoted and misrepresented; emotional appeals replace scientific reasoning; arguments are directed against the man rather than against the evidence. Among the attackers are nonscientists, political militants on campus, academics committed to environmentalism, knowable scientists that are silent out of fear.

The result is that "… it is virtually heresy to express a hereditarian view, or to recommend further study of the biological bases of behavior. A kind of orthodox environmentalism dominates the liberal academy, and strongly inhibits teachers, researchers, and scholars from turning to biological explanations or efforts."

This statement of support elicited much criticism. Vetta (1973) thus noted in an amendment to the resolution in American Psychologist that the signers could not have seen much of Jensen's work because, had they investigated it, they could not have "… failed to notice the deficiencies, the contradictions, and the outright misrepresentations." Vetta may have done a good old moral reading of Jensen's text and spotted the errors but, like Deutsch, could not tell the world about them in any precise manner.

5.1.7 Segerstråle. I have drawn extensively on Segerstråle's 2000 book, because she was in a rather unique situation to comment on the sociobiology and IQ debates. Originally educated in organic chemistry and biochemistry at the University of Helsinki, Segerstråle moved from hard science to the sociology of science, doing her doctoral research at Harvard University. This unique background allowed her to for example consider the nature-nurture debate from a biological as well as from the sociological-philosophical perspective. Moreover, Segerstråle actively participated in some of the meetings on the academic left, allowing her to peek into the hinterland of the critics and thus provide us with a better understanding of the context for their moves. Finally, Segerstråle personally interviewed many of the prominent combatants on both sides of the fence.

Segerstråle notes that there is little doubt that Lewontin's sociopolitical position was based on his devotion to Marxism in practice, which served as "… a 'coupled' moral-cum-scientific agenda …" that made him think that "good science" is unproblematic, and "bad science" is in need of explanation. His two specific tasks were accordingly to "… demonstrate the 'scientific error' of scientists with 'incorrect' political beliefs, and .. to unmask these beliefs in their scientific text and show how the latter 'errors' led to the former one" (ibid. p. 41).

In an early critique of Jensen, Lewontin (1970) strived to "…display Professor Jensen's argument, to show how the structure of his argument is designed to make his point and to reveal what appear to be deeply embedded assumptions derived from a particular world view, leading him to erroneous conclusions."

Like Gould and other leftists, Lewontin often practiced an aggressive and hostile ad hominem character assassination approach, and did not even shy away from talking about the common "carelessness, shabbiness and intellectual dishonesty …" in the study of intelligence (1975a). He claimed that such students " … sometimes tell deliberate lies because they believe that small lies can serve big truths (1981).

In the public TV broadcast Lewontin (1975b) further said: "We know now that brain size has nothing to do with intelligence …", and that earlier and contemporary scientists were "… lying about genetic differences while posing as experts."

Were that the case, we have several "liars" writing chapters to the present volume, including the editor (see chapters 6, 9, and 10, respectively). Is it really a lie

that brain size correlates about .3 - .40 with IQ? Is it a small or a big lie that the inheritance for IQ rises from a lowly .20 in early childhood to a hovering .75 in late adulthood? If no lie, then we see examples of the remarkable disrespect Lewontin and other academic leftists show for, what experts consider solid data. We see an almost unrestrained urge to communicate false messages to the public, in the service of self-assumed moral considerations and self-proclaimed openness in scientific matters. However, an old word says: Never throw stones if you live in a glass-house yourself. If data were stones, the critics were soon homeless.

Halfway through the book, Segerstråle (2000) mentions a striking feature of the whole debate: "The burden of proof was on sociobiologists and IQ researchers to prove their innocence, not on the accusers to prove the formers' guilt … 'politically correct' academics felt that they could require sociobiologists and others to be careful in their actions and choice of words, while they did not see the need to censor their own language when they accused the former of political intent. Sociobiologists were held to high standards, while the critics of sociobiology felt they could get by with easy dismissals of sociobiological theorizing … Anti-sociobiologists were allowed to see all sorts of links between sociobiology and unsavory politics, but the sociobiologists were not allowed to respond that sociobiology's alleged political intent was a 'lie' (or, 'simple lie')." (p. 192).

Segerstråle's analysis of the logic behind the critics' reasoning suggests that it was not traditionally scientific but rather of a moral-legalistic kind, applied to science, and here we are back once again to the moral reading strategy. When critics apply moral reading to texts, they: " … imagine the worst possible political consequences of a scientific claim. In this way, maximum moral guilt might be attributed to the perpetrator of this claim." (2000, p. 206). Plato was thus a big liar, not because he assumed human diversity exists and is largely innate, but rather because people can be defined on a scale according to their worth – some are inherently gold, others silver, and then there are those of bronze (Chorover, 1979, p. 25).

Segerstråle (2000) also asks how we can explain the critics' astounding disregard for the original context of their citations, and concludes that "In fact, one might describe the critics' data selection process as a rather blatant case of what Charles Babbage in his The Decline of Science in England (written in 1830!) famously called 'cooking', that is, selecting only those pieces which (in his words)

'will do for serving up'." Perhaps the critics saw only the pertinent parts of the text to be criticized and disregarded the rest as noise? A moral reading could also be used as pedagogical material "… showing the 'innocent reader' just how sociobiological explanations were cleverly constructed to support a particular political point" (p. 212).

This view harmonizes well with my impression of Gould. He leans more on tremendous rhetoric skills, the Lord Nelson strategy, and broad public accept of no limits to human development, than on adherence to honest empirical evidence, logic, and obedience to the "Total Evidence Rule", which says that nothing but the whole truth will suffice.

5.1.8 Gross and Levitt. Gross and Levitt (1994) provided a scorching analysis of the academic situation in their book – Higher Superstition: The Academic Left and its Quarrels with Science. They launched a heavy attack on the Academic Left (AL), and it surely is no coincidence that they begin the book with a citation from Bertrand Russell's (1968) autobiography: "I find that much unclear thought exists as an excuse for cruelty, and that much cruelty is prompted by superstitious beliefs."

A major point is that muddleheadedness has throughout history been a much more potent force than malevolence or nobility " as it "… blunts our wisdom, misdirects our compassion, clouds whatever insights into the human condition we manage to acquire." Gross and Levitt have few illusions about the likely impact of their writings: "Even if it be the most futile of all things to crusade against the muddleheadedness of the AL people, this quixotry is at least to be preferred to just passively registering the damage done to science by the AL" (p. 1). This critique rang a bell: Many of Jensen's most vocal critics confessed to a leftist political inclination.

Gross and Levitt wanted, first of all to avoid muddleheadedness in their own quarter, so they set out to "…first define what unites the AL individuals." They found that ALs do not "… have a well-defined theoretical position with respect to science … but a noteworthy uniformity of tone, and that tone is unambiguously hostile … [toward] some of the uses to which science is put … toward the system of education … toward the actual content of scientific knowledge and toward the assumption … [that] scientific knowledge is reasonably reliable and rests on a sound methodology …" to an extent that "… irrationality is courted and proclaimed with pride." (p. 2-3; authors' emphasis). The group of ALs, furthermore, typically comprises humanists and social scientists, rarely working physical scientists. ALs can often be identified under

the umbrella of postmodernism in fields like literary criticism, social history, cultural studies, cultural constructivism, postmodern philosophy, feminist theory, deep ecology, deconstruction, and so forth.

"The assumption that makes specific knowledge of science dispensable is "… above all, the moral authority with which the academic left emphatically credits itself … sufficient to guarantee the validity of the critique." (p. 6).

Higher Superstition then goes on to analyze the impact of AL on a multitude of areas that, while highly interesting by themselves, would bring us too far away from the present context. Moreover, my selective quotes from their informative and broad-spectered analysis do little justice to Gross and Levitt's painstaking attempt to define what they mean by the academic left. However, they suffice to bring better into focus the fact that it was people from the AL camp that provided the most explosive ammunition for the ferocious attacks on Jensen. This is not to deny that Jensen has also been attacked by irrational right wing fundamentalists, some with a clear theologico-creationist leaning, but the ALs were definitely not only more vocal but also more vicious.

Let me repeat the important source for the concern Gross and Levitt expressed for the sanity of modern academia – " … an open hostility toward the actual content of scientific knowledge and toward the assumption … that scientific knowledge is reasonably reliable and rests on a sound methodology. (Gross & Levitt, 1994, p. 2). This is one of the major concerns that forces Gross and Levitt to "... attack [the] … academic or cultural left … constructivists and relativist sociologists of science … for challenging science's ability to produce knowledge which was in any sense 'truer' than other types of knowledge. There is a sense of solidarity within the academic left, a solidarity of a political rather than an intellectual nature … a preoccupation with science as power … [a] distrust of experts … [an] obsession with textual analysis …"

I entirely concur with Gross and Levitt in this analysis, and will in the last part of this chapter corroborate on the grave consequences they see of the serious politically and morally inspired attacks on science.

5.1.9 Carroll. Carroll (1997) found several good reasons to respond to the unfair and surprisingly uninformed critique of intelligence research. He first noted that the publication of Herrnstein and Murray's The Bell Curve: Intelligence and

Class Structure in American Life from 1994 had spawned a veritable cottage industry in which almost numberless reviews, critiques, editorials, were written – rarely by the informed specialist – to express mainly negative views of their data, analyses, and conclusions. Thus, works by Fraser (1995) and Jacoby and Glauberman (1995) doubt the emphasis on individual difference in intelligence as a factor in social success, and question the concept of intelligence, the instruments, and the methodology of psychometrics. With never-failing energy Gould (1994) repeated the claim that Herrnstein and Murrey were mistaken in "…assuming that intelligence 'is depictable as a single number, capable of ranking people in linear order, genetically based, and effectively immutable." (p. 139).

One unfortunate result of all this commotion has been, according to Carroll, that many 'public intellectuals' see psychometric research and intelligence as discredited pseudoscience alien to the ideals of a democracy (Giroux & Searls, 1996). As Carroll finds that psychometrics is a sound and fair-minded scientific discipline, he undertook the task to re-examine the six propositions that Herrnstein and Murray stated as being beyond significant technical dispute in psychometric research, to see whether they in fact live up to the current consensus among most experts. This re-examination is all the more important, because Carroll is considered by most experts in psychometrics one of the most central scientists for empirically supporting the modern hierarchical model of intelligence. It really is a shameful sign of the contemporary thinking that most critics prefer Gould's self-confident but not well-informed treatment of factor analysis to Carroll's eminent and empirically cautious 1993 book, or to Jensen's 1998 book on The g factor.

All the six propositions had been widely criticized as being false and pseudoscientific, but Carroll found them on the re-examination:

> "…to be reasonably well supported. Most experts agree that there is a general factor *g* on which human beings differ. It is measured to some degree by most tests of cognitive aptitude and achievement, but more accurately by tests designed to measure it. It corresponds to most people's concept of intelligence. It is quite stable over the lifespan, and properly constructed and administered IQ tests are not demonstrably biased against different social groups. It is substantially influenced by genetic factors, but also by environmental factors."

Carroll also found that some psychometric findings about *g* have been poorly presented to the public or widely misunderstood, so he urges the public to recognize that:

> (I) psychometrics (literally, mental measurement) is a rigorous scientific discipline that has resolved many questions concerning cognitive abilities; (2) general ability scores should be taken not as direct measures of hereditary intelligence, but rather as measures of rate of progress over the life span in achieving full mental development; (3) there are many other cognitive abilities besides *g*; (4) important sources of variation in *g* or IQ are environmental; (5) the IQ is possibly more an indicator of how fast the individual can learn than it is of the individual's capability of learning; and (6) much more research is needed to resolve questions about the role of individual differences in cognitive abilities in a democratic society. These conclusions can be reached whatever one's views may be about the validity of Herrnstein and Murray's claims about the significance of variation in intelligence for social problems."

Carroll accomplished two things with his analysis. First, he showed - once again - that the psychometric analyses of intelligence are well founded in the empirical world, something the critics either flatly deny or try hard to circumvent. Second, the conclusion is entirely independent of Herrnstein's and Murray's treatment in The Bell Curve, bur neither does it contradict their book.

The critics are now, once again, pushed to the wall by empirical and methodological arguments, and their accusations for underlying "bad motives" or "unconscious race aversions" lost power. Carroll is careful, nevertheless, to points out that we still have much to learn, that there still are lacuna in our knowledge, and so forth. But the overall conclusion is clear: psychometrics is not the pseudo-science the public is made to believe by the critics.

5.2 Truth and asymmetry

When dealing with the controversy between, on the one side, sociobiologists, psychometrics, and behavioral geneticists and, on the other side, the critics, Segerstråle (2000) in many ways defended Jensen against the unfair attacks. However, we now arrive at a point where disagree with Segerstråle's insightful analyses. The main

reason for the divergence is, that Segerstråle sees both parties as defenders of the truth: " … it is just that they have different conceptions of where the truth lies." (2000, p. 1). In contrast, the IQ controversy has nothing to do with symmetrical defenses of some truth.

In fact, there are several ways to demonstrate that the IQ controversy was deeply asymmetrical. One of the parties is fairly well characterized by a series of brutal and merciless ad hominem attacks by a group of aggressive and ruthless ideologues, moved more by self-assumed moral authority than truth or, as Gross and Levitt (1994/1998) prefer to express it, by a shameless moral one-upmanship, going far beyond truth and data. The other party is better characterized as a group of hard-working scientists moved more by empirical arguments than by anything else; their endeavor involves correlations and experimentally controlled data and not at all some self-assumed moral authority.

I agree completely with Segerstråle when she invites the reader to inspect "… the relentlessness with which the critics kept attacking their targets, who were accused not only of "incorrect" political and moral stances, but also of "bad science". However, the character of the plot changes radically, when we inspect the sincere and honest presentations, and the tempered and fact-oriented rejoinders of Arthur Jensen. There is nothing in Jensen's work or in his personality that compares to the hostile and vicious attacks launched routinely by the academic leftist firefighters. It takes little effort to see that it is complete nonsense to talk about Jensen's hostility, because there is none. Neither is there, to the best of my knowledge, any serious critique of the empirical side of Jensen's works, which cannot be explained rationally.

Segerstråle further sees the controversy as a clash of different traditions coming from two different academic camps; they live in two different worlds of factual knowledge and taken-for-granted assumptions. She then uses social psychological theory to predict that any incoming information will be aligned with existing convictions, well-known cognitive defense mechanisms will protect members of each camp from being challenged on their existing knowledge, and members within each camp will reinforce each other's beliefs.

This diagnosis has obvious shortcomings in terms of asymmetry. The critics disregarded factual knowledge on basis of their standpoint, whereas Jensen took nothing for granted. The critics singled out Jensen and the behavioral geneticists for ridicule and punishment, but not vice versa. The critics kept repeating the vicious

attacks as good "firefighters" do, whereas Jensen and the behavioral geneticists spent much time in developing new methods and steadily amassing a mountain of increasingly more precise data - that substantiated their own position and increasingly lamed the critic's claim.

Perhaps Segerstråle may have missed the vital asymmetry in the scientific and personal approaches of the two parties because of a common inclination of many philosophers to emphasize reasoning and logic over data. According to her, everybody had a battle to win, and everybody deserved a prize for this. However, in terms of precious data, only one party deserves a prize in the controversy – a prize for amassing a surplus of confluent evidence. The critics basically continued to flatly deny, misrepresent, or ridicule that very same evidence and endlessly repeated their moral condemnation of the collectors.

I may agree with Segerstråle when she in chapter 15 - Capitalizing on Controversy - states: "… it was in each side's interest to define the 'issue' under debate in a way that benefited their own side, so that they themselves would be seen as being correct and the opponents wrong." (p. 299). However, I do not see the evidence to back up the claim that "… both parties … may have been interested in keeping the controversy going because of the chances for short-term and long-term profit …". Even if Segerstråle reports mainly on the sociobiology debate that occupies most of her fine book, Jensen and the behavioral geneticists are by association hit as well by the accusation.

In fact, nothing could be more wrong. Jensen is a self-declared strongly non-political person to the extent of being embarrassed over this himself. He responds to critique with data, analyses, and interpretation, not for harvesting profit – politically or morally. His real intent is to hasten back and check the real world for its reality. This claim is easy to check: Just inspect his many works or the responses he offers the many critics. Similarly, the behavior geneticists I know are preoccupied with amassing family, twin, and adoption data, or with the analysis of quantitative trait loci, or study the molecular basis of intelligence, rather than fueling any kind of controversy. It is, in fact, quite difficult to see whatever interests Jensen or behavior geneticists could possibly have in keeping any kind of controversy going. They already had paid so dearly for hostile publicity fueled by the critics, in terms of loosing funding or attracting negative attention from colleagues and professional organizations. Therefore, to "… depict the participants as involved in competition for

peer recognition, pursuing recognition-capital in both the scientific and moral realms", frankly makes little sense in Jensen's case. He would happily skip the moral aspect for personal and professional survival - and for gaining new data apt to guide the treatment of deprived children.

In other words, Segerstråle's analysis goes wrong precisely where she makes too close an analogy between the ongoing sociobiology, IQ, and behavioral genetics debates. It may be true that in the sociobiology debate, "… those who stood to gain the most were scientists who could promote their own scientific theories as both scientifically and morally/politically superior by probing another scientist both scientifically and morally wrong … by ascribing scientifically and morally untenable views to suitable opponents …" (p. 303). However, Jensen and the behavior geneticists obviously had been much better off, if their critics had left them alone to do their research and present their results without having to fight the time-consuming demonization, politization, and accusations of morally wrongdoing. They generally believe that good data ought to speak for themselves. Where Segerstråle correctly emphasizes the socio-political, philosophical, and opportunistic sides of the critique she pays, in my opinion, too little attention to the hard science aspects of Jensen's and the behavior geneticists' work.

On the final page of her Defenders of the Truth, Segerstråle condenses her major point: "I am arguing that moral/political concerns, far from being an obstacle to be eliminated, were in fact a driving force both in generating and criticizing scientific claims in this field, and that the field was better off because of this. We see, then, the importance of moral and metaphysical commitments in science. They motivate scientific work, they sustain it in the face of adversity, and they drive scientists to closely scrutinize the claims of opponents. It seems to me that moral/political criticism is an important and healthy phenomenon in science, particularly in fields which depend largely on plausibility arguments." (p. 408, original emphasis).

This may be the way many philosophers of science or sociology see it. We are theoretical, moral, and political beings, and this is what drives us as scientists. It is good for us to be challenged on moral/political grounds, because only then we will do our utmost to optimize the task in hand. Segerstråle misses here, as said before, the importance of experimental design and solid data, that Jensen and the behavior geneticists see as the essence of their endeavors. She also misses the importance of pure multidirectional and genuine curiosity that might drive a scientist in any

direction, in accordance with the serendipity principle that guides Jensen: He originally sat out in one direction, but the findings persuaded him in the 1960s to radically change his mind. Despite being harassed, threatened, loosing funding, ridiculed, and wasting oceans of time on trying to respond to ridiculous accusations and wild misrepresentations of his position, he continued to pour out solid data, to satisfy his curiosity, and to test entirely new hypotheses.

Far from being the case that "The characters in my story are all defenders of the truth – it is just that they have different conceptions of where the truth lies", it seems to me that nobody in the IQ wars in fact defended truth in any proper sense of that term. True statements about the world is heavily linked to positivism (or mathematics) – but the last real positivist probably died shortly after the turn of the 19th century. What seemed to have taken place is that the academic left distorted the evidence and substituted truth with moral one-upmanship in the IQ controvercy, whereas others, in particular Jensen, carefully collected and defended data along the lines of confluence and increasing precision, and talked a lot about probabilities, but never called upon truth.

The fundamentally different nature of the two enterprises, and of the combatants, is worth keeping in mind. It was Jensen who refined 20th century psychology's most reliable, stable, and broadest applicable measure - general intelligence *g* - extended it, and brought it safely into the 21st century, despite the twists, shouts, and obvious malevolence of the academic left. A man of lesser ability, personal courage, and scientific integrity would long ago have succumbed to the virulent antiscientific assaults, and psychometrics would have had much less to offer science today. Rather than one truth against another, it was a battle of data against misconceived moral and ideology.

A further asymmetry was safely identified by Segerstråle: "Instead of checking for themselves … it seems that many academics rather took the critics' interpretation at face value … why read the original when the critics' conclusion was eminently plausible? (2000, pp. 14-15). So many of the critics did not care to read the original works and check essential facts before they jumped to unsound conclusions.

Another thing is whether they all exercise "coupled reasoning", i.e. held a belief that a scientific position different from one's own must be politically motivated? Davis' 1983 critique of Gould may seem like just another example of the

coupled reasoning that the critics were originally accused of using. Gould was blinded by Marxist ideology and such a bad scientist will inevitably make error upon error when discussing IQ research, according to Davis. Segerstråle concluded that from each side's perspective, the other side's position clearly looked ideologically biased. The critics "… wanted to unveil and debunk IQ research as 'bad' science … " with " its potential social misuse… ". However, for Davis "… the promise of good science was connected to its potential social usefulness", and so he had to debunk the politically inspired attacks on good science (p. 233).

Perhaps Segerstråle's claim that many participants in the sociobiology debate applied extensive coupled reasoning is correct, but Jensen certainly did not, even if he was the most viciously attacked. My professional and personal acquaintance with Jensen tells me that he is resolutely apolitical, and I have seen him react with visible impatience whenever someone asks for his most likely political stance in IQ matters. I feel pretty sure his reaction will be: Look at the data; what does it tell you? Anybody who cares to read his many and detailed responses to critique will immediately spot this strategy. To give an example, my own position on the likelihood of a sex difference in *g* differs from Jensen's (see chapter 10 in this volume), but I have never heard Art link this scientific disagreement to political motives on my part; He rather challenges me on my methods and data, and this is precisely as it should be.

In other words, Segerstråle may have a point that many apply moral reading of opponent texts in the search for "hidden or unconscious" moral or political truth, but I must insist that moral reading is wasted on Jensen's texts. On the other side, Jensen obviously hopes that his research can be put to good use for individuals in school and elsewhere, but this is no license to include Jensen in the camp of researchers who apply coupled reasoning, not even if Gould and others say so three times. There is no scientific use in linking people devoted to coupled reasoning together with scientists aiming to demonstrate empirically that IQ research can be used to smooth the progress of individual learning. Davis actually referred to Jensen's warning that great harm would take place to individuals in the educational system if we do not maximize the opportunity for development in each individual, entirely regardless of race or income. If this is coupled reason, it is at least of a completely different nature than the one characterizing Gould and other critics, who detests evaluation and ranking of individuals according to IQ scales, and sets out to destroy those who do.

5.3 Lewontin, IQ, and natural science

There is an interesting twist to Lewontin's (1975a) critique of research on intelligence, a foible that demonstrates one of his particular kinds of selective blindness to existing data. His basic position is that "… the only truly scientifically interesting questions about cognitive traits can be asked at the molecular level." Psychometricians were motivated, yes, and what motivates them "… 'must' be their underlying sociopolitical bias that was driving these researchers to bad science." (Segerstråle, 2000, p. 201). In other words, bad psychometric ideology or motives lead to bad methods and bad science. It is indeed remarkable that Lewontin either did not realize, or perhaps did not want to acknowledge, that Spearman as far back as at the beginning of the 19th century defended a molecular analysis - yes, explicitly urged his colleagues to identify the secrets behind his general intelligence factor *g* – undoubtedly pure physics and chemistry of the brain, he ventured. With that feat, physiology would have achieved one of its greatest triumphs, he said. It was just that Spearman did not command proper methods for doing molecular analyses, and it is not fair to criticize a scientist for not having access to non-existing methods he would have loved to use.

Lewontin also appears to have missed the fact that Jensen had over many years steadily accumulated data to suggest that *g* is related to a multitude of brain physiological parameters, and that he explicitly used this evidence to argue that *g* is not just the "wisp of archane mathematical machinations", that he was accused of blindly believing in. Jensen even pursued the question whether *g* - physiology connections go through ontogenetic, phylogenetic, or perhaps environmental mechanisms. The late Hans Eysenck, also viciously attacked by the leftists for unsound abstractions, repeatedly stressed the essentially biological nature of personality and *g*. Hans actually discussed at some length which (brain) chemicals would be relevant for such a proposition. Lewontin and other critics seem to miss that many neuroscientists successfully use brain imaging techniques to illustrate how important neurochemical parameters correlate with cognitive problem solving. The present editor (Nyborg, 1994) wrote an entire book on the molecular basis of human nature and intelligence.

How could Lewontin fail to acknowledge all these attempts to reveal the "molecular" basis of *g*, and instead postulated all kinds of malevolent political motives or bad science? If this type of highly selective reporting is not bad science, then what is? But then again, it becomes fully understandable how Lewontin could reach the conclusion that our present ignorance is enormous and " … the need for the socially powerful to exonerate their institutions of responsibility for the problems they have created is extremely strong …" and that "… any investigations into the genetic control of human behaviors is bound to produce a pseudo-science that will inevitably be misused." (Lewontin, 1975a).

Segerstråle (2000, p. 202) concluded that, apparently: "…it was morally wrong for a scientist to produce anything else than absolutely certain knowledge. " (original emphasis). She further noted that this represents "… in a nutshell the general moral-cum-scientific spirit characteristic of the Sociobiology Study Group …" (p. 203). May I add: This is not just bad science. It is a distortion and antithesis to science.

6. GENES, CULTURE AND HUMAN DEVELOPMENT

6.1 Introduction

Why were so many people desperately afraid to acknowledge even the slightest conservative effects of genes on human development and behavior? Given a choice, why would most people rather subscribe to an extreme version of the environmental paradigm, such as the one nourished by the founder of modern anthropology, Franz Boas, and his followers in the first third of the 20th century, than admit to even a moderate form of genetic determinism? Jensen certainly wondered.

Sociobiologist Edward O. Wilson also took up this dilemma in his latest opus, Consilience: The Unity of Knowledge from 1998. Point of departure for the discussion was the "Standard Social Science Model (SSSM), as defined by Cosmides and Tooby (1992). The SSSM sees culture as a system of symbols and meanings that mould individual minds and social institutions. This idea sounded reasonable enough to Wilson, but the SSSM also sees culture as the product of environment and historical antecedents, not reducible to elements of biology and psychology, and here Wilson strongly dissents, because the model implies that the human mind cannot create culture but is the products of that culture. Obviously, the SSSM cannot be

defended just on basis of fear that genetic determinism is morally wrong as it easily lends support to sexism, racism, war, and class division as inevitable phenomena.

6.2 Plausibility, reality, and explanations

Likewise, why are so many social scientists readily prepared to see another scientist as a moral pariah if he dared question the unfounded notion that developmental differences are 100 percent determined by environmental factors? Sheer ignorance is not the case for all "…otherwise objective and dispassionate intellectuals [who] display such vehement moral indignation and even zealous combativeness toward any explanation of human behavior differences, especially social class and racial differences, that propounds genetic factors as playing a part." (Jensen, 1972, p. 55).

Obviously, Jensen had good reasons to mull over this question. His preliminary answer reflects an incredible fair and decent man's reasoning, considering the hateful context for the discussion. He even goes as far as to believe that those who have most strongly opposed him have "… done so out of noble but mistaken sentiments …" and that "… their motives are not entirely discreditable." As he says: "We all feel some uneasiness and discomfort at the notion of differences among persons in traits that we especially value, such as mental abilities, which have obviously important educational, occupational, and social correlates … our first tendency is to minimize them or explain them away. This is even more true when we are confronted with group differences; it seems to us so intrinsically unjust that some socially defined groups, thought no fault of their own, should be disadvantaged with respect to traits which all persons value that we are easily inclined to deny such differences or at least attribute them to relatively superficial and external causes and appearances, such as prejudice, biased tests and observations, discriminatory schooling, racism, and other similar explanations which tend to place blame and guild on other persons and forces in society. And there is considerable plausibility to such thinking …" (ibid. p. 55-56).

Where then does all this "plausibility" come from? Two places, according to Jensen: A human proclivity to place blame for disadvantage or misfortune, and simple Skinnerian shaping. To see the blame placing mechanism we just have to look back to ancient times, where "… natural disasters such as volcanos, earthquakes, and floods were blamed on the ill-will of personified gods." Whereas the physical sciences now

provide “natural” explanations for this “… the social sciences still have not moved beyond personified blame, leveled at “society”, “the establishment”, “Capitalism”, or whatever – personified entities at which we can vent our anger much as one can feel angry at an individual who intentionally commits a personal offense.” (p. 56).

Jensen then goes on to offer illustrative examples of Skinnerian shaping from his own rich experiences in teaching psychology and education. Any statement that minimizes, explains away, glosses over, or places blame on personified institutions for mental and educational differences between individuals or groups “… is met by an unmistakable rush of warm approval from the audience.” (p. 56). This approval “… shapes more than anything else the speaker’s utterances further toward eliciting more waves of warm approval from the audience … lessens the audience’s anxiety … almost palpable, with bits of laughter and the rustle of relaxing tensions among the listeners,” with the effect of reinforcing the speaker in that direction, often unconsciously and even against his will.”

From my own experiences from lecturing on the development of individual and groups differences in intelligence and behavior genetics, these reactions certainly make you think twice next time you present the data. Try a little exercise for yourself. Give a brief lecture on sex differences in intelligence, and begin with the massive documentation for a male superiority in 3-D spatial abilities. You immediately see a surprisingly coordinated and self-reinforcing tension. The audience suddenly moves, some straighten up, turn toward the neighbor and whisper a brief comment. The other nod approvingly, send an overbearing smile, or even laugh demonstratively loud. Some begin to eagerly scribble hasty notes on paper, preparing for an angry rebuttal, that will surface a few minutes later. At that time it simply is not possible to cure the open hostility of the audience, even if you now document, that females are superior to males in some verbal abilities. Minds are set for blaming somebody – the messenger of the bad message. Now present the same data for new students, but this time first report on the female verbal superiority. You will see but approving smiles, and then you can get away unhurt reporting on the male superiority in spatial ability.

These students are not dumb. What is failing here is that many (most?) modern psychology students are not trained properly in independent and critical scientific thinking. They rather think in plausibility terms, and are well accustomed to argue in politically correct ways. They prefer moral to empirical reasoning and reading, and

many are impressed by post-modern relativism, to such an extent that they automatically launch an antiscientific critical program as if that was the last word on the matter. To many of them science and data are texts waiting to be contextualized - not carefully controlled attempts towards increased precision. Most are not aware that they are betraying scientific stringency, and feel good by attacking any messengers of "bad" information. They got their coupled-reasoning lessons from Gould, Lewontin, and modern French philosophers, and they want to feel good, socially safe, and justified.

Jensen (1972, p. 57) asked several colleagues what intellectual reasons they could see for denying a genetic basis for behavioral differences. The most common reason was that "… such knowledge, if it is established and generally accepted by the scientific and intellectual community, might be used by some persons for evil purposes, to promote racial prejudice, discrimination, and segregations and to justify or rationalize the political suppression, and economic exploitation of racial minorities and the Nations' working class in general."

Jensen is not moved by such arguments: "… these consequences do not logically follow from the recognition of genetic behavioral differences. Nearly all scientifically important knowledge can be used for good or ill. Intellectuals should be concerned with men's purposes and the uses to which knowledge will be put; they should newer think in terms of suppressing knowledge or the quest for it".

Another expressed worry is the fear that differences in gene frequencies for some traits will automatically compromise the moral ideal of equality expressed in "all men are created equal", and would hinder equality before the law, education, civil rights etc. But this is not so, Jensen says: "Realization of the moral ideal of equality proclaimed in the Declaration of Independence, of course, does not depend upon either phenotypic or genotypic equality of individuals' psychological qualities."

Still another misconception that pops up repeatedly in the attacks on Jensen is that genetic differences between populations are "… somehow, sui generis, intrinsic, unchangeable, protoplasmic differences." (author's emphasis p. 57). This is a completely wrong and ignorant notion, promulgated in racist tracts, Jensen says: "There is nothing at all "intrinsic" or "immutable" about human gene pools", as specific gene frequencies reflects mainly "… varying degrees of geographic and

social isolation of breeding groups and natural selection", through differing environmental pressures.

6.3 Summing up the critique

When carefully analyzed the critique boils down to a number of sociopolitical and moral attitudes that for the most parts can be condensed to the following statements, that defenders have to take into account:

1. All individuals and human groups are the same with respect to intelligence, personality, and behavior.
2. Academics must speak with one voice thereabout.
3. It is the duty of scholarly and other organizations to enforce politically correct ways of talking about the origin of individual and group differences.
4. However, should any difference be found, it must be ascribed to environmental factors.
5. The prime task of the social scientists is then to change these environmental factors in such a way that the difference disappears.
6. Should any difference resist environmental intervention, it should be ascribed to the need for further research, lack of funding, or too little time to correct.
7. Never should the differences be explained by genetic factors or gene-environment correlation or interaction.
8. Should the differences nevertheless suggest a genetic component, environmental factors must immediately be invoked to annul them.
9. Should the differences nevertheless remain, stricter than normal scientific criteria must be established before any genetic influence is accepted by, say, requesting identical environment for all individual or groups.
10. Any behavioral scientists claiming even a moderately genetic effect must immediately be sanctioned against.
11. Non-environmentalist outcomes should be misrepresented, strawmen invented and torn apart, or possible but implausible alternative solutions should be put into effect.

12. It should be emphasized that there are certain subjects that should not be investigated at this time in history.
13. Should any scholar be unable to understand or accept the much stricter criteria for differential psychological research, he should be punished.
14. Should any journalists be unable to understand the rules for politically correct presentation, higher editorial levels must intervene and correct.

7.0 DESTRUCTIVE SOCIAL REDUCTIONISM AND COLLECTIVE FRAUD

7.1 Introduction

The critique of Jensen is a perfect example of how 20th century academic freedom has come under siege in the West, as it was previously in the East. It suggests that the hostility of the academic left towards individualism and biological explanations plays a major role in ruining the research climate in modern academia, despite superficial declarations of adherence to open-minded research and obligatory cocktail-party proclamations of freedom for all.

The more we look into the literature on this depressing scenario, the more destructive the social reductionist point of view appears, and the more serious becomes the threat to academic freedom, even to a former left-oriented person like myself.

Segerstråle raised a pertinent question, also pondered by Gross and Levitt: How on earth could the environmentalist/culturalist position become so forceful in academia, and why was it automatically linked to progressive politics. Segerstråle traces the answer to "… the post-Second World War situation and particularly … the famous UNESCO agreement in 1952, which effectively put a ban on biological research in human behavior. It was precisely this taboo that sociobiologist Wilson, and before him, IQ researcher Arthur Jensen and the behavioral geneticists, were breaking." (2000, p. 30).

There is more to the story than that, however. As we saw, the demonization of Jensen could be dealt with analytically at a surface level in terms of the previous simple model, according to which Jensen switched from neutral decision mode 1 to biological mode 2 in Zeitgeist mode 2, a change towards biological thinking at a time where all such manifestations were banned, punished on a personal basis, and where confirmatory data were seen as politically motivated. Clearly, the broad sweep, the

generality, and the noticeable hostility towards Jensen across many layers of academic and public life cannot be fully appreciated within such a narrow analytic frame.

We need to eyeball the full social-academic-organizational-political-public horizon in order to understand in details why so many scientists, professional and international organizations, and the press at large, could so easily unite in such a surprisingly effective self-reinforcing synchrony, and act almost like a well-disciplined team to muster the brutal and direct force against apostates. We have to combine all the destructive elements of social reductionism – such as the role of religion, the egalitarian fiction, the self-perceived moral superiority of the critics, the open suppression of empirical alternatives, the corruption of professional organizations, the urge toward political correctness, the threats to biological projects and funding – in order to fully understand the explosive sequence of events and how they finally amount to nothing less than a large scale collective academic fraud, and even "inverse" fraud. The following section introduced some semi-dependent variables needed for the second part of the analysis.

7.2 Semi-dependent variables

7.2.1 Equality. Garrett (1961) described a journalistic credo called "egalitarian orthodoxy" involving flat denial or a softening of the likelihood that genes may partly explain race, sex or individual differences in intelligence, personality or interests etc.

Linda Gottfredson is even more explicit here. In an article – Egalitarian fiction and collective fraud (1994) she said: "Social science today condones and perpetuates a great falsehood … or 'egalitarian fiction' … that racial-ethnic groups never differ in average …*g* … general … ability …" While individual scientists' intellectual dishonesty is well-known, little attention has been given to the ways in which collectives of scientists "… have perpetuated frauds on the scientific community and the public at large."

She further noted that no scientist in the collective can probably be accused of fraud in the usual sense, but "… their seemingly minor distortions, untruths, evasions, and biases collectively produce and maintain a witting falsehood." (ibid. p. 53)

Which social processes could be responsible for this? asks Gottfredson. After having established the general agreement among experts about the existence of a real average difference, she points to the results of an important study by Snyderman and Rothman (1988) - The IQ controversy: The media and public policy - providing

strong evidence that the general public receives a highly distorted view of opinion among ‘IQ experts’ (ibid. p. 54). The public press has left the opinion that many experts agree that intelligence cannot be defined well, that IQ tests cannot be used outside the school, and that they are biased against minorities, even if most experts are of the opposite meaning. This is interesting because the study also showed that most experts privately agree with Arthur Jensen, who is constantly exposed in the media for holding just such views. Despite the change in expert view toward Jensen, obviously guided by the overwhelming weight of the evidence, the public impression has not moved correspondingly. Gottfredson takes Snyderman’s and Rothman’s findings to suggest that many “ …experts misrepresent their belief or are keeping silent in the face of a public falsehood. It is no wonder that the public remains misinformed on this issue.” (p. 55).

Linda Gottfredson was close to being sacked from Delaware University in the US for accepting a research grant from the previously mentioned Pioneer Fund for investigating IQ-occupation relationships. Her characterization of the rather bleak situation in 20th century academia is illustrative:

> "Perhaps the most aggressively perpetrated collective fraud in the social sciences today is that which sustains the egalitarian fiction. This is the frequent but false assertion that intelligence is clustered equally across all human populations, that is, that there are, on average, no racial-ethnic disparities in developed mental competence." (Gotfredson, 2000).

Gottfredson’s notion of collective fraud will be used in the present analysis, but the scope will not be restricted to race differences in intelligence, but will include the entire social reductionistic conspiracy against any researcher, who dares investigate individual or group difference in physique, intelligence, personality, achievement, or behavior in general, and the evolutionary, genetic, physiological, or brain bases of these differences. I will term this the “Grand academic leftist collective fraud” hypothesis.

7.2.2 The role of religion and philosophy. Jensen notes that definitions always arise in a particular context of understanding, and that context differ from one period to another (termed Zeitgeists in this chapter), and from one scientist to another. The

early context for intelligence was Platonic philosophy and Christian theology. Jensen (1998, p. 1) observed: "This vastly delayed the study of … intelligence …. as manifesting individual differences … [intelligence] was identified with the soul and seen as a perfect, immaterial, universal attribute of humans, and both definitions were counterproductive. It took a Darwin (1859/1872) to counter blatant environmentalism (e.g. Locke, 1690) and to realize that the evolution of intelligence is basically a biological phenomenon common to man and other animals, a Spencer (1820-1903) to defend Darwin, counter dualism, and hammer out that, intelligence is a physiological mean for individually adjusting internal to external conditions, a Galton (1822-1911; 1869) to establish differential psychology which sets the study of individual and group differences on a solid scientific track, and a Spearman (1904) to define and measure intelligence objectively."

7.2.3 The egalitarian fiction. Gottfredson (1994) saw no need to mingle her words when she wrote that egalitarians often assert that the egalitarian promise is absolute truth beyond scientific scrutiny whereas the opposite view may be discredited through misrepresentation, by contradicting arguments never made while ignoring what was actually said, by attributing political preferences to an author that he never has had, or by simply alleging fraud or gross incompetence with no substantiation. "The study of race and intelligence is something they tell us, that no decent person – let alone a serious scientist – would ever do and that every decent person and serious researcher would oppose. Thus, in a kind of Orwellian inversion, marked by what Gordon (1993) calls 'high talk and low blows,' the suppression of science presents itself as science itself. Intellectual dishonesty becomes the handmaiden of social conscience, and ideology is declared knowledge while knowledge is dismissed as mere ideology." This is all the more tragic because enforcement of the egalitarian fiction " … tries to defy a reality and produces what it was meant to avoid, that is, producing pejorative racial stereotypes, fostering racial tensions, stripping members of lower-scoring groups of their dignity and incentives to achieve, and creating permanent social inequalities between the races."

7.2.4. The role of funding organization. Most scientists need funding in order to do research, and most funding agents make an attempt to define what they find worth funding. As demand is usually much larger than supply, the individual scientist

has to conform to – or at least better pay close attention to – which projects the funding agents think is worth supporting. This is all well known, but what is perhaps less acknowledged is, that the basis motivation of most major philanthropic funds in the US, and probably also in Europe, changed markedly in recent times.

Heather Mac Donald (2000) took the trouble to describe the change in a series of essays, now collected and published as a book – The burden of bad ideas. According to Mac Donald, quite radical changes took place in, what in the present context corresponds to the middle of Zeitgeist mode 2, i.e. around 1960-70, where large funding agents got increasingly inspired by left oriented ideas emanating from within the American academy, from political think tanks, and from organizations for the arts and sciences.

Before the change, such foundations as Carnegie, Ford, Mellon, Mott, and Rockefeller gave most of their money to establish concert halls, hospitals, libraries, museums, or universities, with the goal of extending the opportunities of the less fortunate. After the change, the foundations began to support projects that, instead of seeking mobility and success for the less privileged, rather promoted "advocacy" and "empowerment" by way of "community action" and "collaboratives" to overthrow the "racist, sexist, and classist edifice" upon which America had been founded. This change of mind, from traditional values, to prevailing left oriented political-economic-cultural themes had, according to Mac Donald, a profound effect on research at The Ivy League Universities, the National Institutes of Health, the Centers for Disease Control, but also the New York State Regents, the New York Times, and the Smithsonian Institute were affected by the change of mind.

Obviously, the changes also affected the nature of educational policy, and research on "critical-thinking skills", "community-building", "brainstorming", "student-centered learning" substituted to some extent the older "content-based" curricula and ability guided teaching.

Mac Donald is, according to Peter Savodnik (2000, p. 38), almost alone in describing this major change in funding in the US in the 1960s, and he ends by concluding: "The hugely wasteful social-engineering experiments have … wrought … widespread havoc on the people least able to defend themselves against the well-funded programs of America's radical establishment."

Project Head Start comes to mind here. It was the conspicuous lack of documentation for a clear benefit for the culturally deprived children involved in this

multi-million dollars program that alerted Arthur Jensen, and the negative results of his (and other's) analysis got him into troubles. While we still are waiting for a documentation of the lasting positive effects of such programs, we can speculate on how easily the academic left was able to redirect major funding their particular way.

Whatever the answer is to that question, the massive redirection of research funding no doubt socialized many researchers away from what they originally planned, and towards projects that conform to the new goals. This most certainly would drain the funding for psychometrics and behavioral genetics.

7.2.5 Individual suppression of academic freedom. Gottfredson (1994) wondered why the experts keep their mouth shut about the obvious, and provided the answer herself: Because IQ experts have learned to "live within a lie", quite like the people living under communist rule in Eastern Europe, as so aptly noted by Vaclav Havel. Here ordinary citizen were complicit in their own tyranny because they silently had to play the game of the rulers and thus unwillingly became supporters of the tyranny they detested.

Coleman, who is perhaps best known for his monumental report on Equality of educational opportunity (Coleman et al., 1966) knew precisely how it is to live within a lie. He, thus, later (1990-1991) reflected with regret on why he deliberately neglected certain unpopular aspects of his otherwise eminent social science analysis. The excuse he gave was, that academics establish norms for themselves for which kind of questions to raise and which to avoid. One of the most influential norms is: Never ever raise questions about possible biological roots to intellectual race- and sex-related differences. Any academics "know" by heart that such questions rapidly and inevitably raise incredible tensions forcing faculty to harsh repercussions, so they have to be avoided at all costs, even if truth is one of them. Unfortunately, not only truth suffers here. Coleman admitted that our possibilities of ever coming to grip with important aspects of the causal basis for the social phenomena studied may be permanently stultified.

7.2.6 Collective suppression of academic freedom. While the reasons for individuals to keep their mouth shut in dangerous matters like IQ are fairly obvious, it is more complicated to answer the question why groups of experts keep their mouth shut about the obvious? Could it be that there is now a collectively structured silence,

where groups of social scientists deliberately subordinate scientific norms to political preferences and create a kind of pseudo-reality?

Wolf (1972) noted that many contemporary social scientists keep "… presenting inconclusive data as if it were decisive; lacking candor about 'touchy' subjects …; blurring or shaping definitions (segregation, discrimination, racism) to suit 'propagandistic' purposes; making exaggerated claims about the success of favored policies (compensatory education and school integration) while minimizing or ignoring contrary evidence." They are under great professional and institutional pressure, because peer recognition is the currency of academic and scientific life and decisive for promotion, status, and funding. Even the smallest digression from politically correct ambitions could irreparably damage an otherwise successful professional career. Even just expressing respect for the "right" people counts on the positive side, whereas "… honoring, defending, or even failing to condemn the 'wrong' sort of individual or idea ..." might stain one's reputation (p. 56).

According to Gottfredson (1994), such a system breeds intellectual corruption. This is precisely what appears to be happening today in the social sciences on matters of race and intelligence. While certainly being a personal annoyance, all these threatening activities had the unfortunate effect of silencing colleagues who otherwise might have joined in the defense of Jensen's cause. Jensen received a large number of supportive letters, but many of the writers explicitly stated that they preferred to remain anonymous, so as not to be subjected to a similar treatment. Jensen (1972) gave an example of a colleague who got his paper returned with proper payment and a letter from the editor explaining "… we have finally decided against entering the controversy altogether". When Jensen urged the author to try and publish his paper elsewhere, he said: "… because of the abuse which you have received, I have no intention of submitting my paper for publication elsewhere." (p. 47)

Gordon (1993) argues that many social scientists demonstrate their party loyalty to the egalitarian fiction by enforcing it in myriads of small ways in their academic routine by off-handedly dismissing race differences in intelligence as racist claims, blaming the victim, or discouraging students and colleagues from doing "sensitive" research. Overt censorship is common to those "not knowing where to step".

Gottfredson (1994) finds that "… the lie is gradually distorting and degrading all institutions and processes where intelligence is at least somewhat important …

public schools, higher education, the professions, and high-level executive work." (p. 58). She concluded that " … society is being shaped to meet the dictates of a collective fraud. The fiction is aiding and abetting bigots to a far greater degree than any truth ever could, because its specific side-effects – racial preferences, official mendacity, free-wielding accusations of racism, and falling standards – are creating deep cynicism and broad resentment against minorities, blacks in particular, among the citizenry."

7.2.7 Collective bias in academia. All this had the chilling effect of silencing large parts of academia, and began more and more to look like a sweeping collective fraud, extending downwards to university administrators and funding agents, and upwards to huge professional organizations, and to public policy where individual politicians could harvest easy votes, and where the political left and right parties, creationists, and other with heavily vested interests in evading the role of biology and individual differences in intelligence for human behavior, and keep a kind of socially based pseudo-solidarity with the disadvantages.

There are many further ways to censor than the overt forms, according to Gordon (1993). One is to establish speech-codes on campus, another to subject National Institute of Health research application to an extra layer of review for politically "sensitive" grant proposals, still another to ban a particular funding sources. This latter became policy of the University of Delaware because, as the University said, funding of research on race "…conflicts with the university's mission to promote racial and cultural diversity". (Gottfredson, 1994, p. 56).

7.2.8 Collective bias in professional organizations. Not just individuals can be harmed by opposing the current dogma of the social sciences that all differences in intelligence - individual, sex, or race differences - are caused by some form of discrimination or omission. So can scientific organizations, and they are noticeably sensitive to this potential danger. Gottfredson (1994) explains: "It raises the public and scientific respect for the organization whenever it honors an individual that lives well up to the dogma, and degrades it in the eyes of others should a non-dogmatic person be awarded. It provides respect to issue statements conforming to the dogma even, or perhaps in particular, if it pours scorn on non-conformers, like Jensen."

7.2.9 Bias in national and cross-national organizations. Even such high-profiled organizations like UNESCO and UN take part in the collective fraud. I previous referred to factually incorrect statements by such organizations. Recently, United Nations Secretary General, Kofi Annan declared that intelligence: "… is one commodity equally distributed among the world's people" (Hoyos & Littlejohn, 2000). It takes only a brief inspection of the massive cumulative long-term documentation for marked national differences in IQ by Lynn and Vanhanen (2002) to see, that suggest that such counter-factual statements neither serves the credibility of the organization as such nor of its top representatives. It may very well be that the purpose of the statement was meant politically or strategically, but cross-national policy based on lies - great or small – might easily bounce back in non-productive ways.

7.2.10 Devastating political correctness (PC). Webster's New World Dictionary of American English (1994) describes political correctness as "… orthodox liberal opinion on matters of sexuality, race … usually used disparagingly to connote dogmatism, excessive sensitivity to minority causes …" Weyher (1998) refers in a discussion of PC to a cover story in Newsweek (24 December 1990) where it is said that: "P.C. is Marxist in origin, in the broad sense of attempting to redistribute power from the privileged class (white males) to the oppressed masses. It represents the values of social equality and social justice over that of free speech."

"For the first time in our history, Americans have to be fearful of what they say, of what they write, and of what they think. They have to be afraid of using the wrong word, a word denounced as offensive or insensitive, or racist, sexist, or homophobic." These words are from a lecture by Bill Lind at a conference at George Washington University in 1998. We have seen PC in other countries, now we have it here, and primarily on campuses, but it is spreading throughout society. Historically, PC is Marxism translated from economic into cultural terms, and the parallels to classical Marxism are very obvious, according to Lind. It is the child of a totalitarian ideology and it is deadly serious: "… the student or faculty member who dares to cross any of the lines set up by the gender feminist or the homosexual-rights activists, or the local black or Hispanic group, or any of the other sainted "victims" groups that PC revolves around, quickly find themselves in judicial trouble. Within the small legal system of the college, they face formal charges … and punishment."

The formally installed "speech codes" at some campuses reflect PC, and the strong statements from minority students organizations against Jensen, as well as the violent reactions towards anybody transgressing the not so fine line, all tell a story of repression of academic freedom, that surely will inform researchers of any stripes of what is best to do here and now, and it may explain in part why individuals as well as large professional groups bow to PC. Whatever the PC term precisely refers to, transgression of it can issue a deadly blow to one's scientific reputation.

7.3 Summing it all up

Gross and Levitt (1998) took the trouble to sum it all up. They stressed that the critics rode on a too high moral horse. Perhaps they were too good to be true. They were willing to sacrifice Jensen at the price of their own scientific integrity and honesty. They practiced selective reading, omitted major points, denied well-established research, and was carried to fame on morally well-sounding statements that sat well with the public. Educators, eager to find some consolation for the slow progress in rising the learning curves for the disadvantaged, welcomed the promises of easy progress and participated all too willingly in the attacks on messengers of bad news. Colleagues noticed the unmerciful treatment of Jensen and bend their heads in silence. Young scientists soon realized that their future could no safely be built on pursuing a career in psychometrics or behavioral genetics. Granting committees, such as the Pioneer Fund soon realized that funding people like Jensen rapidly raised critical questions about their own sinister motives – didn't they have hidden racist leanings, didn't they have neo-nazi connections, etc. The smears would take no end, even if they showed the critics to the door by exposing their errors (Lynn, 2001).

7.4 The "inverse" fraud of Gould and Lewontin

Fraud is defined in the present context as the critic's deliberate distortion of solid evidence on individual and group differences in physique, intelligence, personality, and behavior, and as the misrepresentation of scientists that collect such data. However, the critics also use the term fraud but in an inverse form. To the critics, fraud could be spotted through moral reading and massaging of texts to reveal the truly evil motives behind apparently innocent data.

Gould was a tireless master of inverse fraud. He thus warned us "… how theory and unconscious presupposition always influence our analysis and organization

of presumably objective data." (1996, p. 49). Previously, in his original (1981) version of The mismeasure of Man, he had said: "If the cultural influences upon science can be detected in the humdrum minutiae of a supposedly objective, almost automatic quantification, then the status of biological determinism as a social prejudice reflected by scientists in their own particular medium seems secure." (p. 58). Moreover: "In reanalyzing … classical data sets, I have continually located a priori prejudice, leading scientists to invalid conclusions from adequate data, or distorting the gathering of data itself. In a few cases … we can specify conscious fraud as the cause of inserted social prejudice. But fraud is not historically interesting except as gossip because the perpetrators know what they are doing and the unconscious biases that record subtle and inescapable constraints of culture are not illustrated. In most cases discussed in this book, we can be fairly certain that biases – though often expressed as egregiously as in the cases of conscious fraud – were unknowingly influential and that scientists believed they were pursuing unsullied truth." (Gould, 1996, p. 59, original emphasis).

Many other examples of inverse fraud can be found in the 1986 book by Schiff and Lewontin – Education and class: The irrelevance of IQ genetic studies. In the foreword, Halsey accurately reflects the particular direction and aggressive intent of the book by stating: "… the authors steadfastly and indeed belligerently declare their ideological bias to environmentalism …" (in Schiff & Lewontin, 1986, p. v.), and on the next page he characterizes Sir Cyril Burt "… as a dominating figure who slid from obsession through pseudo-science into outright fraud." (p. vi).

The Schiff and Lewontin book refers to Franz Boas (1912), who in 1909-10 measured the heads of 13,000 immigrants born in Europe and of their children born in America. Boas found striking effects on the cranial form as a function of the length of exposure to an American upbringing. Boas, who often targeted "scientific racism" or false thinking about races, took this result as proof that racial head characteristics depend on environmental rather than genetic factors, and concluded that those who think otherwise are racists. In particular the disciples of Boas, such as anthropologists Ruth Benedict, Margaret Mead and Ashley Montagu were instrumental in promoting this kind of social reductionist view of human nature.

However, Sparks and Jantz (2002) have 90 years later re-examined Boas' published data and found, that the effects of the new environment on head form were "insignificant". They found "negligible" differences between parents' and children'

head form, in comparison to the differentiation among ethnic groups. It is food for thought that Gould, Lewontin, and many other critics have use this study to bolster a social-reductionistic view on race. They are the people who call for the uttermost caution in interpretation of data, while at the same time accuse Jensen of dishonesty.

Schiff informs us on page xi: " … that questions concerning genetic effects are essentially irrelevant to … access to education. Later (in Schiff & Lewontin, 1986, p. xiii) he declares that "… theories of innate differences arise from political issues …", and in their introduction to the book Schiff and Lewontin state that "… we try to show that, as far as education is concerned, most genetic studies are not only unsound but are also irrelevant (p. xiii).

Discussing phrenology Schiff and Lewontin (1986) states, "As it turns out, there is no correlation at all between the size of an adult's brain and his or her ability to perform intellectually. (p. 7). They therefore see their book as a direct attempt " … to oppose the errors of biological theory of social class, and to present competing evidence that class is a social phenomenon, created by the structure of social relations, and not dictated by our genes (p. 14, original emphasis), and they further claim that "… the nature-nurture debate is actually a smokescreen for a debate over the interaction between individual differences and social structure" (p. 17).

Many IQ experts try to cover this by using double-talk, and "The most sophisticated type of double-talk concerning the word "intelligence" is that of Jensen (1980), whose technical analysis boils down to the definition attributed to Binet ("intelligence is what my test measures").

Schiff and Lewontin (1986) conclude the first part of their book by stating that "… procedures used to validate "intelligence" tests are as socially determined as the tests themselves. The high degree of sophistication of some of their procedures only serves to mask an unwillingness to face the social, psychological, and ethical questions posed by the construction and use of IQ tests" (p. 32-33), that "… discussions about IQ usually fail to distinguish clearly between questions of fact and questions of values. In addition, they are often obscured by technical confusion". There is a "… refusal to consider social class as a basic component of present reality. Finally, the circular nature of attempts to validate IQ scores stems from this same inability to question current social values."

The authors then react strongly against the idea that social inequality may be attributed "… to differences in innate ability between the children of the different

social classes, as revealed by differences in the distributions of IQ scores." because " …white middle-class people decide who is intelligent and who is not", and as long as " … teachers, filled with goodwill and with ethnocentric naïvity, view human intelligence through their own school training, the academic failure of working-class children will be built into the school and social system (p. 125).

In counting the many errors about genetics and their social consequences Schiff and Lewontin (1986) draw attention to a "striking feature": " … the degree to which a supposedly "Scientific" field is permeated with basic conceptual and experimental errors … much of the discussion of the biology of intelligence would simply evaporate if fundamental biological and statistical notions were applied to the genetics of human behaviour with the same degree of rigour and logic that is standard in, say, the study of milk yield in cattle or body weight in mice." (p. 169).

Discussing the why of intelligence testing, Schiff and Lewontin (1986) state that "The purpose of the IQ test is to identify the potential winners presumably so that society will not waste its precious resources on those whose abilities are insufficient" and behind lies "…. the claim that this social organization is an inevitable manifestation of human biology, that the war of all against all is a natural law." (pp. 184-185).

In a section called Error 12: If it is new and complicated it must be true, Schiff and Lewontin (1986) say: "Partly through self-delusion, and partly through a deliberate attempt to mystify the innocent, some of those who have written about the genetics of IQ have tried to make the story more believable by making it more complicated." by "… introducing a complex mathematical model involving many variables and parameters and finding the set of parameters that best fits the data." and so "… for that reason alone seem deeper and more "scientific" (e.g. Eysenck 1979, p. 3)" and "It is absurd to think that the numbers that come from such models have any meaning." (p. 185-187).

This is an excellent example of an inverse fraud win-win strategy running along the line: If head I win, if tail you loose. If Jensen used the same old simple outworn methods, the field has stagnated; if Jensen developed new and more complicated methods a false sense of depth is pretended. Never mind if the new methods provides more reliable results with broader applicability in other areas. Jensen has to be framed in a catch 22-situation.

But the story of inverse fraud does not end here. The social implications of the many conceptual errors that have been propagated in the field of IQ studies come together, according to Schiff and Lewontin (1986), to press home a single major theme where the bottom line is: "Differences between social class and races are heritable and unchangeable ….". Therefore "… social policy that attempts to change either the structure or the assignment of groups to it is misdirected, as waste of time, and even harmful because it raises hopes that are bound to be dashed. It is essentially an argument for the inevitability and justice of the status quo. It is fairly obvious who the argument serves." (p. 187).

Bouchard and McGue are also treated unkindly by Schiff and Lewontin (1986). They reported in 1981 on resemblance correlations for 43 parent-offspring and 69 siblings. The comment from Schiff and Lewontin (ibid.) was: "Since these studies provide essentially no genetic information, one can wonder why society has paid scientists to repeat essentially the same observation for so long." Apparently, when scientists strive to reproduce potentially controversial observations they are at fault, and this principle can be used as a weapon against the enemy. Again, either way, you lose. Presumably, the many later confirmative studies raise even more serious questions about the sinister motives of those who did them and those who financed them.

Schiff and Lewontin (ibid) motivate the writing of their book with the goal of providing the reader with a key to the literature on nature-nurture and IQ, so that by following their prescriptions the reader will be able to focus on the general principles rather than on any particular study, and "…concentrate on the questions rather than on the answers." (p. 192).

Key reading seems here to be just another word for moral reading or coupled reasoning: Disregards the data and concentrate instead on why the researcher took the trouble to investigate the biological basis of race or intelligence. This kind of reading is, in fact, essential for understanding the true nature of social reductionist critique and its destructive nature. However, what is at stake here is more than a particular moral standing or reading of texts in the nature-nurture and IQ debates; it is rather an example of an immoral and destructive instruction in how to dismiss data, however solid, in order to promote what Gottfredson defines as collective fraud.

It is therefore not surprising to see that Schiff and Lewontin (1986) concluded: "In our opinion, the most striking fact of the whole IQ story is the contrast between

the use of IQ to account for social heredity and the deliberate or unaware avoidance of a direct analysis of that heredity, and that " … a significant fraction of the scientific establishment has handled this issue in what appeared to be an inappropriate way." (p. 223-225). The psychometric approach to human intelligence misses "… the capacity to ask question, to oneself and to others". "The biological deterministic approach … misses another specific feature of homo sapiens. It is homo sapiens who decides … how his society is organized …".

7.5 Inverse illusions

Schiff and Lewontin have, quite like Gottfredson and others, a rather pessimistic view of the calamities in academia, but the signs differ radically.

To Schiff and Lewontin (1986), most workers in academia seem to suffer from two contradictory illusions: "The illusion of complete academic freedom …a denial or lack of awareness of social and economic pressures influencing scientific workers …" and the opposite illusion of " …complete helplessness … Most scientists fail to recognize that the type of question they ask and the type they choose to ignore derive both from social pressure and from a personal choice." (p. 226 –227).

To Gottfredson and other, Schiff, Lewontin, and Gould tried their uttermost to limit the academic freedom; moreover Jensen et al. were painfully aware of the many pitfalls associated with the long haul of collecting solid data that could stand the test of critical control in a climate so hostile to their research.

While Jensen found himself mostly engaged in hard empirical work, Schiff and Lewontin (1986) felt free to speculate - without a self-perceived obligation to collect the relevant data - what the problem really was. They saw fit to conclude: "… the amount of knowledge about child behavior accumulated among schoolteachers is greater and of a different sort than that accumulated by academic psychologists. Even more instructive … is the fact of trying to change [educational processes] … scientists may not possess the most important part of the existing knowledge about human behavior, specifically about human intelligence …those who believe that they have a monopoly on something may not be the best judges of the legitimacy of that monopoly."

These hypotheses definitely deserve interest to the extent Schiff and Lewontin want to make comparisons among the predictive validity of teacher knowledge and

the predictive validity of *g*. They did not do any of the hard work needed. However, the data are already out there. Why didn't they call upon it?

Lewontin and Schiff instead offer the following truly breathtaking scenario: "… the direct observation of human mental processes is potentially available to four billion observers. The scientific authority granted to a few concerning the functioning of the human mind may then be largely usurped." They seem to suggest: Skip science, and thy will see the light! This is an inverse illusion.

7.6 Gould in hell?

Gould's self-esteem seems not slighted towards the meek end. He never doubted that he was on the right path when he said: "May I end up next to Judas Iscariot, Brutus, and Cassius in the devil's mouth at the center of hell if I ever fail to present my most honest assessment and best judgment of evidence for empirical truth."

Speaking metaphorically, of course, I am afraid his wish will come true (provided that anybody any longer believes in such spooky things!) Neither did Gould present an honest assessment of those he countered nor did he pass the best judgment of their empirical findings. No doubt his social ambitions and care for the disadvantaged were deeply rooted in an honest responsibility, but he was a person who fought for a beautiful ideal of equality by attacking innocent scientists that as faithfully as possible presented data as they saw them, painfully aware of all the possibilities for making errors that are built into such an enterprise. Gould, and other academic leftists, never abstained from vicious ad hominem attack at the cost of their scientific integrity. This stands in sharp contrast to most of those they attacked and demeaned, with Jensen as the prominent counter-example.

Gould neither understood nor accepted the massive critique of his position, and he turned aggressively against anybody who questioned him. His description of his own reaction to colleagues taking him to task is telling. "The nadir certainly arrived (with a bit of humor in the absurdity) in the Fall 1983 issue of the archconservative journal, The Public Interest, when my dyspeptic colleague Bernard D. Davis published a ridiculous personal attack on me and the book under the title "Neo-Lysenkoism, IQ, and the Press." Gould also attacked The Bell Curve by Herrnstein and Murray (1994) in strong words by critiquing the illogic of the general argument, and the inadequacies of the book's empirical claims. Gould then became

"…particularly pleased because Mr. Murray became so apoplectic about this article …" (Gould, 1996, p. 48).

This is neither the language of science, nor is his exhilarations particularly productive, even if Gould may have scored points in certain quarters with this style "… because many people felt that I had provided a comprehensive and fair (if sharp) commentary …".

7.7 The burden of the academic left

Gross and Levitt (1994/1998) went as far as to worry that the existence of the academic left "… has to be read as the manifestation of a certain intellectual debility afflicting the contemporary university: one that will ultimately threaten it." At the same time, Gross and Levitt are eager to assure us that obviously not all left oriented in academia or elsewhere are to be blamed, even if "… that's where most (but certainly not all) of the silliness is coming from …"

The damage done to universities by the leftists can hardly be underestimated, say Gross and Levitt: "Prestige-laden departments in the humanities and the social sciences are thickly populated – in some by now well-known cases we might say, without opprobrium, "dominated" – by radical thinkers." Not only academic institutions, but also "Scholarly associations are often dominated by these same stars…" Here they refer to an analysis by Fromm (1993). It is no longer unusual to see that administrators at universities either themselves are " … prominent left-wing figures …" or "… more bland … ". Either way, they have to take into account the fact that the local campus left is an important and stable segment of the academic community, whose views must be taken into account…" Therefore: "Often, when administrations take official positions on social issues – particularly those involving race, ethnicity, and gender questions – the tone, and the jargon as well, is indistinguishable from that of the militant left." Gross and Levitt, 1994/1998, p. 34).

One might here add that this applies in particular, whenever individual, sex, or race differences in intelligence, are in question, or behavioral genetics results are presented. The remarkable passivity of many university officials whenever Jensen was obstructed or attacked springs to mind here.

Gross and Levitt noted that contemporary academic presses "… pour out dozens upon dozens of volumes, grounded in left-wing theory …" and that there are

"… learned journals … whose purpose is avowedly political and unapologetically leftist. Universities by the score are delighted to host conferences and symposia …" that resound with left-wing rhetorics. (p. 34).

7.8 Where lefties go in, righties go out!

Gross and Levitt wonder how this regrettable deterioration and corruption had taken place in academia. Taking into account the isolation and neutering of significant left-wing sentiments in the world of "real" politics, Gross and Levitt speculate that, perhaps " … recruitment into academic careers, especially outside the exact sciences, has been altered in a way that lures people with left-wing sympathies and hopes for radical social change into scholarly careers, while simultaneously bright young students of conservative bent are less and less enchanted at the prospect of joining the professoriate. … a diffuse phenomenon, largely inadvertent and unplanned. … [but where] … the process has had the crucial goodwill of a kind of academic "silent majority", the great body of professors who, while they may distance themselves from doctrinaire ideological formulations and exotic new social theories, somehow continue to believe vaguely that the left, broad construed, remains (after all these decades) "the party of humanity," the locus of right thinking; and that it deserves to be nurtured and encourages even if it goes overboard from time to time in the vehemence of its views." (Gross and Levitt, 1994/1998, p. 35).

8. THE FUTURE

8.1 What can be done to counter the collective fraud?

Even if truly worried over the widespread corruption of academia by the left, Gross and Levitt (1994/1998) do not call for a "depoliticization" of the classroom. Honest and undogmatic intellectuals, left-oriented or not, is what Gross and Levitt call for (p. 35).

Another countermove is to continue to amass data. This is precisely what Jensen and many others have been doing, and these data has bolstered the claims Jensen made in his 1969 "How much can we boost IQ and scholastic achievement" HER article. Thus, despite many claims to the contrary, the 15 points black-white IQ difference has not diminished over time, even if it fluctuated. Rushton and Jensen (2002) continue to illustrate that an increasing amount of evidence is consistent with

the notion that the race difference has genetic as well as environmental components (see also chapter 9 in the present volume, and chapter 18 for reservations). The support for a heritability estimate of IQ of about .80, as originally suggested by Burt, pours in from family studies (that do not allow for separation of genetic and environmental effect), and from twin and adoption studies (that do allow for the separation). We also know now that IQ heritability is low in early childhood (.20), and that it increases steadily over the life-span to reach the above mentioned .80 in late adulthood. It has been demonstrated again and again that properly administered IQ tests are not culturally biased, that IQ measures have better reliability and predictive validity than any other measures provided by 20th century psychology. It is also generally acknowledged that the insane discussion of whether IQ or *g* is a reified thing in the head is a dead issue long ago. What really counts is its operational definition and practical validity.

One of the most ironic aspects of the nature-nurture debate is that behavior genetics has succeeded in developing new and more precise measures of the effects of environmental factors on individual development, than the social reductionist could ever dream of. Where environmentalists still claim that early rearing or deprivation exert a massive impact on development, but are never providing the much-needed tabulation of effects, behavior genetics separates the shared and non-shared factors, and studies them in within- and between-families designs. The results have been stunning. The longer an adopted child remains in a new family the more it differentiates away from it developmentally, psychologically, behaviorally, and physically, and the more it grows towards increasing similarity with the biological parent it does not know. The heritability coefficients grow with age for all these traits, and the intelligence of the child becomes increasingly similar to that of the biological parents, whereas it looses all similarity with the intelligence of the foster parents after age 5 or 6. These observations contradict traditional social learning theory, and they keep pouring in.

Gottfredson's (1994) cure against the collective fraud is to break down the egalitarian fiction, and avoid all its harm. This does not require heroism, but rather "… for scientists to act like scientists – to demand, clearly and consistently, respect for truth and for free inquiry in their own settings, and to resist the temptation to win easy approval by endorsing a comfortable lie." (p. 59). It may sound easy, but it is not. It has been said that a theory dies only when its inventor dies or, phrased more

elegantly by Max Planck: "...a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

This suggests that the readiness with which we accept, construct, or defend certain types of theory may have a genetic basis. In fact, traditionalism, a core dimension of attitudes involving conservative versus liberal views on a wide range of issues, has been found to show a heritability of about .30 in adoption and several twin studies (for overview, see Plomin et al. (2001, pp. 246-247). However, there is higher assortative mating for traditionalism than for any other psychological trait (about .50). When this is taken into account, the heritability for traditionalism rises to .50 and the shared environmental influence drops to about 15 percent (Eaves, Eysenck, & Martin, 1989).

The surprisingly robust and unshakeable nature of a given individual's scientific persuasion or philosophical orientation may thus be understood in terms of an underlying genetic propensity to hold a liberal or conservative attitude. To the extent this makes sense, stubborn traditionalism is a factor that has to be encompassed in any serious understanding of the sociology of science. It may be manifested in the dogmatic search for particular "truths" or a moral or ethical agenda at the academic left, or it may explain the dogmatic inflexibility of conservative scientists to change their mind. In any case, Jensen cannot have gene(s) for traditionalism. As I have demonstrated, he has on several occasions radically changed his mind when the data told him to do so (e.g. his level I – level II theory, and the role of genetics in development). Other most likely have them, perhaps in a dominant allele form, and this would work against any easy remedy for the devastating consequences for the operations of the academic left, the steadfastness of the Zeitgeist, and the unbelievable solidity of personal certainty about where to find the truth. The genetic predisposition may also partly explain why the science wars became so vitriolic, and why it is so very very difficult to change the course of science through revolutionary shifts. The other parts of the explanation may be identified in the dynamic social interaction of the many semi-dependent factors outlined in this chapter and made responsible for the synergy of the collective fraud.

An important factor in countering this inertia is to change the education of young scientists. Instead of learning them to win arguments through persuasion, misrepresentation, ridiculing, censoring, or sacking, we ought to instruct them to

critically search for solid data, and let the data speak with the weight that confluent evidence gives it. In short, let them in the words of Gottfredson do what they are expected to do: To act like responsible scientists. This obviously will not remove all the stones on their way, but it will at least not let them be trapped so easily in the snares of social reductionisms and collective fraud.

A third factor is to ensure that the administrative layers of academia are instructed in countering prevailing PC, and that funding agents let go their tendency to support only facile PC areas.

Finally, let only those who patiently and competently search for durable data get a price. Jensen is such a person. He is a King! He deserves the throne.

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