

RACE, GENDER, AND SCIENCE  
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THE “RACIAL”  
ECONOMY OF  
SCIENCE

TOWARD A  
DEMOCRATIC FUTURE

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## IQ: THE RANK ORDERING OF THE WORLD

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### The Roots of IQ Testing

Social power runs in families. The probability that a child will grow into an adult in the highest 10 percent of income earners is ten times greater for children whose parents were in the top 10 percent than for children of the lowest 10 percent.<sup>1</sup> In France, the school failure rate of working-class children is four times that for children of the professional class.<sup>2</sup> How are we to explain hereditary differences in social power in a society that claims to have abolished hereditary privilege in the eighteenth century? One explanation—that hereditary privilege is integral to bourgeois society, which is not structurally conducive to real equality—is too disquieting and threatening; it breeds disorder and discontent; it leads to urban riots like those in Watts and Brixton. The alternative is to suppose that the successful possess an intrinsic merit, a merit that runs in the blood: Hereditary privilege becomes simply the ineluctable consequence of inherited ability. This is the explanation offered by the mental testing movement, whose basic argument can be summarized in a set of six propositions that, taken as a whole, form a seemingly logical explanation of social inequality. These are:

1. There are differences in status, wealth, and power.
2. These differences are consequences of different intrinsic ability, especially different "intelligence."
3. IQ tests are instruments that measure this intrinsic ability.
4. Differences in intelligence are largely the result of genetic differences between individuals.
5. Because they are the result of genetic differences, differences in ability are fixed and unchangeable.
6. Because most of the differences between individuals in ability are genetic, the differences between races and classes are also genetic and unchangeable.

While the argument begins with an undoubted truth that demands explanation, the rest is a mixture of factual errors and conceptual misunderstandings of elementary biology.

The purposes of Alfred Binet, who in 1905 published the first intelligence test, seem to have been entirely benign. The practical problem to which Binet addressed himself was to devise a brief testing procedure that could be used to help identify children who, as matters then stood, could not profit from instruction in the regular public schools of Paris. The problem with such children, Binet reasoned, was that their "intelligence" had failed to develop properly. The intelligence test was to be used as a diagnostic instrument. When the test had located a child with deficient intelligence, the next step was to increase the intelligence of such a child. That could be done, in Binet's view, with appropriate courses in "mental orthopedics." The important point is that Binet did not for a moment suggest that his test was a measure of some "fixed" or "innate" characteristic of the child. To those who asserted that the intelligence of an individual is a fixed quantity that one cannot augment, Binet's response was clear: "We must protest and react against this brutal pessimism."<sup>3</sup>

The basic principle of Binet's test was extraordinarily simple. With the assumption that the children to be tested had all shared a similar cultural background, Binet argued that older children should be able to perform mental tasks that younger children could not. To put matters very simply, we do not expect the average three-year-old to be able to recite the names of the months, but we do expect a normal ten-year-old to be able to do so. Thus, a ten-year-old who cannot recite the months is probably not very intelligent, while a three-year-old who can do so is probably highly intelligent. What Binet did, quite simply, was to put together sets of "intellectual" tasks appropriate for each age of childhood. There were, for example, some tasks that the average eight-year-old could pass, but which were too difficult for the average seven-year-old and very easy for the average nine-year-old. Those tasks defined the "mental age" of eight years. The intelligence of a child depended upon the relation his or her mental and chronological ages bore to each other. The child whose mental age was higher than his or her chronological age was "bright" or accelerated, and the child whose mental age was lower than his or her chronological age was "dull" or retarded. For most children, of course, the mental and chronological ages were the same. To Binet's satisfaction, the mental ages of children in a school class, as measured by his test, tended to correspond with teachers' judgments about which children were more or less "intelligent." That is scarcely surprising, since for the most part Binet's test involved materials and methods of approach similar to those emphasized in the school system. When a child lagged behind its age-mates by as much as two years of mental age, it seemed obvious to Binet that remedial intervention was called for. When two Belgian investigators reported that the children whom they had studied had much higher mental ages than the Paris children studied by Binet, Binet noted that the Belgian children attended a private school and came from the upper social classes. The small class sizes in the private school, plus the

kind of training given in a "cultured" home, could explain, in Binet's view, the higher intelligence of the Belgian children.

The translators and importers of Binet's test, both in the United States and in England, tended to share a common ideology, one dramatically at variance with Binet's. They asserted that the intelligence test measured an innate and unchangeable quantity, fixed by genetic inheritance. When Binet died prematurely in 1911, the Galtonian eugenicists took clear control of the mental testing movement in the English-speaking countries and carried their determinist principles even further. The differences in measured intelligence not just between individuals but between social classes and races were now asserted to be of genetic origin. The test was no longer regarded as a diagnostic instrument, helpful to educators, but could identify the genetically (and incurably) defective, those whose uncontrolled breeding posed a "menace . . . to the social, economic and moral welfare of the state."<sup>4</sup> When Lewis Terman introduced the Stanford-Binet test to the United States in 1916 he wrote that a low level of intelligence

is very common among Spanish-Indian and Mexican families of the Southwest and also among negroes. Their dullness seems to be racial, or at least inherent in the family stocks from which they come. . . . The writer predicts that . . . there will be discovered enormously significant racial differences in general intelligence, differences which cannot be wiped out by any scheme of mental culture.

Children of this group should be segregated in special classes. . . . They cannot master abstractions, but they can often be made efficient workers. . . . There is no possibility at present of convincing society that they should not be allowed to reproduce, although from a eugenic point of view they constitute a grave problem because of their unusually prolific breeding.<sup>5</sup>

Though Terman's Stanford-Binet test was basically a translation of Binet's French items, it contained two significant modifications. First, a set of items said to measure the intelligence of adults was included, as well as items for children of different ages. Second, the ratio between mental and chronological age, the "intelligence quotient," or IQ, was now calculated to replace the simple statement of mental and chronological ages. The clear implication was that the IQ, fixed by the genes, remained constant throughout the individual's life. "The fixed character of mental levels" was cited by another translator of Binet's test, Henry Goddard, in a 1919 lecture at Princeton University, as the reason why some were rich and others poor, some employed and others unemployed. "How can there be such a thing as social equality with this wide range of mental capacity? . . . As for an equal distribution of the wealth of the world, that is equally absurd."<sup>6</sup>

The major translator of Binet's test in England was Cyril Burt, whose links to Galtonian eugenics were even more pronounced than those of his American contemporaries. Burt's father was a physician who treated Galton, and Galton's strong recommendations hastened Burt's appointment as the first school psychologist in the English-speaking world. As early as 1909 Burt had administered some crude tests to two very small groups of schoolchildren in the town of Ox-

ford. The children at one school were the sons of Oxford dons, fellows of the Royal Society, etc., while children at the other school were the sons of ordinary townspeople. Burt claimed that the children from the higher-class school did better on his tests and that this demonstrated that intelligence was inherited. This scientifically stated conclusion, published in the 1909 *British Journal of Psychology*,<sup>7</sup> might have been predicted from Burt's handwritten entry, six years earlier, in his Oxford undergraduate notebook: "The problem of the very poor—chronic poverty: Little prospect of the solution of the problem without the forcible detention of the wreckage of society or other preventing them from propagating their species."

Burt continued his eugenic researches into the inheritance of IQ until he died in 1971, knighted by his monarch and bemedaled by the American Psychological Association. The masses of data that he published helped to establish the "eleven-plus" examination in England, linked to the postwar system of selective education. "Intelligence," Burt wrote in 1947, "will enter into everything the child says, thinks, does or attempts, both while he is at school and later on. . . . If intelligence is innate, the child's degree of intelligence is permanently limited." Further, "Capacity must obviously limit content. It is impossible for a pint jug to hold more than a pint of milk; and it is equally impossible for a child's educational attainments to rise higher than his educable capacity permits."<sup>8</sup> There could be no clearer statement of what had happened to Binet's test in the hands of the Galtonians. The test designed to alert educators that they must intervene with special educational treatment was now said to measure "educable capacity." When a child did poorly in school, or when an adult was unemployed, it was because he or she was genetically inferior and must always remain so. The fault was not in the school or in the society, but in the inferior person.

The IQ test, in practice, has been used both in the United States and England to shunt vast numbers of working-class and minority children onto inferior and dead-end educational tracks.\* The reactionary impact of the test, however, has extended far beyond the classroom. The testing movement was clearly linked, in the United States, to the passage, beginning in 1907, of compulsory sterilization laws aimed at genetically inferior "degenerates." The categories detailed included, in different states, criminals, idiots, imbeciles, epileptics, rapists, lunatics, drunkards, drug fiends, syphilitics, moral and sexual perverts, and "diseased and degenerate persons." The sterilization laws, explicitly declared constitutional by the U.S. Supreme Court in 1927, established as a matter of legal fact the core assertion of biological determinism: that all these degenerate characteristics were transmitted through the genes. When the IQ testing program of the United States Army in World War I indicated that immigrants from Southern and Eastern Europe had low test scores, this was said to demonstrate that "Alpines" and "Mediterraneans" were genetically inferior to "Nordics." The army of IQ data figured prominently in the public and congressional debates over the Immigration Act of 1924. That overtly racist act established as a feature of American

\*"Tracking" in the U.S. educational system is more or less synonymous with "streaming" in Britain.

immigration policy a system of "national origin quotas." The purpose of the quotas was explicitly to debar, as much as possible, the genetically inferior peoples of Southern and Eastern Europe, while encouraging "Nordic" immigration from northern and western Europe. This tale has been told in full elsewhere.<sup>9</sup>

Today many (if not most) psychologists recognize that differences in IQ between various races and/or ethnic groups cannot be interpreted as having a genetic basis. The obvious fact is that human races and populations differ in their cultural environments and experiences, no less than in their gene pools. There is thus no reason to attribute average score differences between groups to genetic factors, particularly since it is so obviously the case that the ability to answer the kinds of questions asked by IQ testers depends heavily on one's past experience. Thus, during World War I, the Army Alpha test asked Polish, Italian, and Jewish immigrants to identify the product manufactured by Smith & Wesson and to give the nicknames of professional baseball teams. For immigrants who could not speak English, the Army Beta test was designed as a "nonverbal" measure of "innate intelligence." That test asked the immigrants to point out what was missing from each of a set of drawings. The set included a drawing of a tennis court, with the net missing. The immigrant who could not answer such a question was thereby shown to be genetically inferior to the tennis-playing psychologists who devised such tests for adults.

### What IQ Tests Measure

How do we know that IQ tests measure "intelligence"? Somehow, when the tests are created, there must exist a prior criterion of intelligence against which the results of the tests can be compared. People who are generally considered "intelligent" must rate high and those who are obviously "stupid" must do badly or the test will be rejected. Binet's original test, and its adaptations into English, were constructed to correspond to teachers' and psychologists' a priori notions of intelligence. Especially in the hands of Terman and Burt, they were tinkered with and standardized so that they became consistent predictors of school performance. Test items that differentiated boys from girls, for example, were removed, since the tests were not meant to make that distinction; differences between social classes, or between ethnic groups or races, however, have not been massaged away, precisely because it is these differences that the tests are *meant* to measure.

IQ tests at present vary considerably in their form and content, but all of them are validated by how well they agree with older standards. It must be remembered that an IQ test is published and distributed by a publishing company as a commercial item, selling hundreds of thousands of copies. The chief selling point of such tests, as announced in their advertising, is their excellent agreement with the results of the Stanford-Binet test. Most combine tests of vocabulary, numerical reasoning, analogical reasoning, and pattern recognition.

Some are filled with specific and overt cultural references: Children are asked to identify characters from English literature ("Who was Wilkins Micawber?"); they are asked to make class judgments ("Which of the five persons below is most like a carpenter, plumber, and bricklayer? 1) postman, 2) lawyer, 3) truck driver, 4) doctor, 5) painter"); they are asked to judge socially acceptable behavior ("What should you do when you notice you will be late to school?"); they are asked to judge social stereotypes ("Which is prettier?" when given the choice between a girl with some Negroid features and a doll-like European face); they are asked to define obscure words (sudorific, homunculus, parterre). Of course, the "right" answers to such questions are good predictors of school performance.

Other tests are "nonverbal" and consist of picture explanations or geometric pattern recognition. All—and most especially the nonverbal tests—depend upon the tested person having learned the ability to spend long periods participating in a contentless, contextless mental exercise under the supervision of authority and under the implied threat of reward or punishment that accompanies all tests of any nature. Again, they necessarily predict school performance, since they mimic the content and circumstances of schoolwork.

IQ tests, then, have not been designed from the principles of some general theory of intelligence and subsequently shown to be independently a predictor of social success. On the contrary, they have been empirically adjusted and standardized to correlate well with school performance, while the notion that they measure "intelligence" is added on with no independent justification to validate them. Indeed, we do not know what that mysterious quality "intelligence" is. At least one psychologist, E. G. Boring, has defined it as "what intelligence tests measure."<sup>10</sup> The empirical fact is that there exist tests that predict reasonably well how children will perform in school. That these tests advertise themselves as "intelligence" measures should not delude us into investing them with more meaning than they have.

### Reifying Behavior

The possibility of behavioral measurements rests upon certain basic underlying assumptions, which should now be clarified. First, it is assumed that it is possible to define, absolutely or operationally, a particular "quality" to be measured. Some such qualities, like height, are relatively unproblematic. To the question, "How tall are you?" the answer in centimeters, feet, or inches is easy to give. To the question "How angry are you?" no such easy answer can be given. Anger has to be defined operationally, as, for instance, how often an individual placed in a given test situation and asked the question by the experimenter responds by hitting him on the nose. This is not a flippant example. "Aggression" in a rat is measured by putting a mouse in a cage with it and observing the behavior and time taken for the rat to kill the mouse. Sometimes this is described under the name "muricidal" behavior in the literature, which presumably makes the exper-

imenters happier that they are measuring something really scientific. Research in this area thus becomes forced into Boring's circularity: Intelligence "is" what intelligence tests measure.

The "quality" is then taken to be an underlying object that is merely reflected in varying aspects of an individual's behavior under widely different circumstances. Thus "aggression" is what individuals express when a man beats his wife, pickets boycott scabs during a strike, teenagers fight after a football game, black Africans struggle for independence from their colonial masters, generals press buttons unleashing thermonuclear war, or America and the Soviet Union compete in the Olympic Games or the space race. The underlying quality is identical with that which underlies muricide in rats.

Second, it is assumed that the quality is a fixed property of an individual. Aggression and intelligence are seen not as processes that emerge from a situation and are part of the relationships of that situation, but rather exist like reservoirs each of defined amount, inside each of us, to be turned on or off. Instead of seeing the anger or aggression expressed in inner city riots as emerging from the interaction between individuals and their social and economic circumstances and as expressive of collective action—therefore a social phenomenon—the biological determinist argument defines inner city violence as merely the sum of individual units of aggressiveness. . . .

Thus verbs are redefined as nouns: processes of interaction are reified and located inside the individual. Further, reified verbs, like aggression, are assumed to be rigid, fixed things that can be reproducibly measured. Like height, they will not vary much from day to day; indeed, if the tests designed to measure them show such variations they are regarded as poor tests. It is assumed not that the "quality" being measured is labile, but that our instruments need greater precision.

### Psychometry and the Obsession with the Norm

Implicit in reification is the third and crucial premise of the mental testing movement. If processes are really things that are the properties of individuals and that can be measured by invariant objective rules, then there must be scales on which they can be located. The scale must be metric in some manner, and it must be possible to compare individuals across the scale. If one person has an aggression score of 100 and the next of 120, the second is therefore 20 percent more aggressive than the first. The fault in the logic should be clear: The fact that it is possible to devise tests on which individuals score arbitrary points does not mean that the quality being measured by the test is really metric. The illusion is provided by the scale. Height is metric, but consider, for instance, color. We could present individuals with a set of colors ranging from red to blue and ask them to rank them as 1 (reddest) to 10 (bluest). But this would not mean that the color rated 2 was actually twice as blue as the color rated 1. The ordinal scale is an arbitrary one, and most psychometric tests are actually ordinals of this sort. If one rat kills

ten mice in five minutes and a second rat kills twelve in the same time, this does not automatically mean that the second is 20 percent more aggressive than the first. If one student scores 80 in an exam and a second 40, this does not mean the first is twice as intelligent as the second.

Surmounting or disguising the scaling problem is integral to the grand illusion of psychometry. Individuals vary in height, and if heights for a hundred or so individuals drawn at random from a population are plotted, they will likely fall into the normal distribution, or bell-shaped curve. If the divisions in one's scale are very fine—say, inches—the bell curve is quite wide. If we had no measures less than feet, and we measured each individual to the nearest foot, the curve would be much narrower at the bottom. The vast majority of individuals in Western society would lie between the five- and six-foot measure. While we know the relationship of inches to feet and could under appropriate circumstances convert from one scale to another, and we know when to use each, as when we are finding a pair of shoes that fit or deciding the best size to make a door opening, we do not know the comparable relationships between different ways of measuring aggression or intelligence. Which scale is chosen depends on whether one wants to make differences of scale appear large or small, and these decisions are those that psychometry arbitrarily makes. The decision that a "good" scale is one in which two-thirds of the population should lie within 15 percent of the mean score of the entire population—the famous normal distribution—is arbitrary, but its power is such that psychometrists chop and change their scales till they meet this criterion.

Yet the power of the "norm," once established, is that it is used to judge individuals who have been located along its linear scale. Deviations from the norm are regarded with alarm. Parents who are told that their child is two standard deviations from the norm on some behavioral score are led to believe that he or she is "abnormal" and should be adjusted in some way to psychometry's Procrustean bed. Psychometry, above all, is a tool of a conformist society that, for all its professed concern with individuals, is in reality mainly concerned to match them against others and to attempt to adjust them to conformity.

Pressure to conform to social norms, and institutions that propagate and reinforce these norms, are, of course, characteristic of all human societies. In advanced capitalist societies and today's state capitalist societies like the Soviet Union or those in Eastern Europe, the norm becomes an ideological weapon in its own right, foreshadowed by Huxley's *Brave New World* and Orwell's *1984* but cloaked in the benign language of those who only wish to help, to advise, but not to control and manipulate. Let us be clear: norms are statistical artifacts; they are not biological realities. Biology is not committed to bell-shaped curves.

### IQ Tests as Predictors of Social Success

The claim that IQ tests are good predictors of eventual social success is, except in a trivial and misleading sense, simply incorrect. It is true that if one measures

social success by income or by what sociologists call socioeconomic status (SES)—a combination of income, years of schooling, and occupation—then people with higher incomes or higher SES did better on IQ tests when they were children than did people with low incomes or low SES. For example, a person whose childhood IQ was in the top 10 percent of all children is fifty times more likely to wind up in the top 10 percent of income than a child whose IQ was in the lowest 10 percent. But that is not really quite the question of interest. What we really should ask is: How much more likely is a high-IQ child to wind up in the top 10 percent of income, *all other things being equal*? In other words, there are multiple and complex causes of events which do not act or exist independently of each other. Even where *A* looks at first sight as if it is the cause of *B*, it sometimes really turns out on deeper examination that *A* and *B* are both effects of some prior cause, *C*. For example, on a worldwide basis, there is a strong positive relationship between how much fat and how much protein the population of a particular country consumes. Rich countries consume a lot of each, poor countries little. But fat consumption is neither the cause nor the result of eating protein. Both are the consequence of how much money people have to spend on food. Thus, although fat consumption per capita is statistically a predictor of protein consumption per capita, it is not a predictor when all other things are equal. Countries that have the same per capita income show no particular relation between average fat and average protein consumption, since the real causal variable, income, is not varying between countries.

This is precisely the situation for IQ performance and eventual social success. They go together because both are the consequences of other causes. To see this, we can ask how good a predictor IQ is of eventual economic success when we hold constant the person's family background and the number of years of schooling. With these constant, a child in the top 10 percent of IQ has only twice, not fifty times, the chance of winding up in the top 10 percent of income as a child of the lowest IQ group. Conversely, and more important, a child whose family is in the top 10 percent of economic success has a 25 times greater chance of also being at the top than the child of the poorest 10 percent of families, even when both children have average IQ.<sup>11</sup> Family background, rather than IQ, is the overwhelming reason why an individual ends up with a higher than average income. Strong performance on IQ tests is simply a reflection of a certain kind of family environment, and once that latter variable is held constant, IQ becomes only a weak predictor of economic success. If there is indeed an intrinsic ability that leads to success, IQ tests do not measure it. If IQ tests do measure intrinsic intelligence as is claimed, then clearly it is better to be born rich than smart.

### The Heritability of IQ

The next step in the determinist argument is to claim that differences between individuals in their IQ arise from differences in their genes. The notion that intel-

ligence is hereditary is, of course, deeply built into the theory of IQ testing itself because of its commitment to the measurement of something that is intrinsic and unchangeable. From the very beginning of the American and British mental testing movement, it was assumed that IQ was biologically heritable.

There are certain erroneous senses of "heritable" that appear in the psychometricians' writings on IQ, mixed up with the geneticists' technical meaning of heritability, and which contribute to false conclusions about the consequences of heritability. The first error is that genes themselves determine intelligence. Neither for IQ nor for any other trait can genes be said to determine the organism. There is no one-to-one correspondence between the genes inherited from one's parents and one's height, weight, metabolic rate, sickness, health, or any other nontrivial organic characteristic. The critical distinction in biology is between the *phenotype* of an organism, which may be taken to mean the total of its morphological, physiological, and behavioral properties, and its *genotype*, the state of its genes. It is the genotype, not the phenotype, that is inherited. The genotype is fixed; the phenotype develops and changes constantly. The organism itself is at every stage the consequence of a developmental process that occurs in some historical sequence of environments. At every instant in development (and development goes on until death) the next step is a consequence of the organism's present biological state, which includes both its genes and the physical and social environment in which it finds itself. This comprises the first principle of developmental genetics: that every organism is the unique product of the interaction between genes and environment at every stage of life. While this is a textbook principle of biology, it has been widely ignored in determinist writings. "In the actual race of life, which is not to get ahead, but to get ahead of somebody," wrote E. L. Thorndike, the leading psychologist of the first half of the century, "the chief determining factor is heredity."<sup>12</sup>

The second error—even if admitting that genes do not determine the actual developmental outcome—is to claim that they determine the effective limit to which it can go. Burt's metaphor of the pint jug that can hold no more than a pint of milk is a precise image of this view of genes as the determinants of capacity. If the genetic capacity is large, the argument runs, then an enriched environment will result in a superior organism, although in a poor environment the same individual will not show much ability. If the genetic capacity is poor, however, then an enriched environment will be wasted. Like the notion of the absolute determination of organisms by genes, this view of genetic "capacity" is simply false. There is nothing in our knowledge of the action of genes that suggests differential total capacity. In theory, of course, there must be *some* maximum height, say, to which an individual could grow; but in fact there is no relationship between that purely theoretical maximum, which is never reached in practice, and the actual variations among individuals. The lack of relationship between actual state and theoretical maximum is a consequence of the fact that growth rates and growth maxima are not related. Sometimes it is the slowest growers that reach the greatest size. The proper description of the difference between genetic types is not in some hypothetical "capacity" but in the specific pheno-

type that will develop for that genotype as a consequence of some specific chain of environmental circumstances.

Nor, of course does the phenotype develop linearly from the genotype from birth to adulthood. The "intelligence" of an infant is not merely a certain small percentage of that of the adult it will become, as if the "pint jug" were being steadily filled. The process of growing up is not a linear progression from incompetence to competence: To survive, a newborn baby must be competent at being a newborn baby, not at being a tiny version of the adult it will later become. Development is not just a quantitative process but one in which there are transformations in quality—between suckling and chewing solid food, for instance, or between sensorimotor and cognitive behavior. But such transitions are not permitted in the rank-ordered view of the universe that determinism offers.

The total variation in phenotype in a population of individuals arises from two interacting sources. First, individuals with the same genes still differ from each other in phenotype because they have experienced different developmental environments. Second, there are different genotypes in the population which differ from each other on the average even in the same array of environments. The phenotype of an individual cannot be broken down into the separate contributions of genotype and of environment, because the two interact to produce the organism; but the total variation of any phenotype in the population can be broken down into the variation between the average of the different genotypes and the variation among individuals with the same genotype. The variation between the average performance of different genotypes is called the *genetic variance* of the trait (that is, the aspect of the phenotype under study—eye color, height, or whatever) in the population, while the variation among individuals of the same genotype is called the *environmental variance* of the trait in the population. It is important to notice that the genetic and environmental variances are not universal properties of a trait but depend upon which population of individuals is being characterized and under which set of environments. Some populations may have a lot of genetic variance for a character, some only a little. Some environments are more variable than others.

The *heritability* of a trait, in the technical sense in which geneticists understand it, is the proportion of all the variation of a trait in a population that is accounted for by the genetic variance. Symbolically,

$$\text{Heritability} = H = \frac{\text{genetic variance}}{\text{genetic variance} + \text{environmental variance}}$$

If the heritability is 100 percent, then all of the variance in the population is genetic. Each genotype would be phenotypically different, but there would be no developmental variation among individuals of the same genotype. If the heritability is zero, all of the variation is among individuals within a genotype, and there is no average variation from genotype to genotype. Characters like height, weight, shape, metabolic activity, and behavioral traits all have heritabilities below 100 percent. Some, like specific language spoken or religious and political

affiliation, have heritabilities of zero. The claim of biological determinists has been that the heritability of IQ is about 80 percent. How do they arrive at this figure?

### Estimating the Heritability of IQ

All genetic studies are studies of the resemblances of relatives. If a trait is heritable, that is, if different genotypes have different average performances, then relatives ought to resemble each other more closely than unrelated persons do, since relatives share genes from common ancestors. Brothers and sisters ought to be more like each other than aunts and nephews, who ought to be more similar than totally unrelated people. The standard measure of similarity between things that vary quantitatively is their *correlation*, which measures the degree to which larger values for one variable go together with larger values of a second variable, and smaller values with smaller values. The correlation coefficient,  $r$ , ranges from + 1.0 for perfect positive correlation, through zero for no relationship, to - 1.0 for perfect negative correlation. So, for example, there is a positive correlation between father's income and child's years of schooling. Richer fathers have better-educated children while poorer fathers have less-educated children, on the average. The correlation is not perfect, since some families produce children who go to graduate school, but it is positive. In contrast, in the United States there is a negative correlation between family income and the number of visits per year to hospital emergency rooms. The lower your income, the more likely you are to use the emergency room as a medical service instead of a private doctor.

One important point about correlation is that it measures how two things vary together but does not measure how similar their average levels are. So the correlation between the heights of mothers and their sons could be perfect in that taller mothers had the taller sons and shorter mothers had the shorter sons, yet all the sons could be taller than all the mothers. Covariation is not the same as identity. The significance of this fact for the heritability of IQ and its meaning is considerable. Suppose a group of fathers had IQs of 96, 97, 98, 99, 100, 101, 102, and 103, while their daughters, separated from their fathers at birth and raised by foster parents, had IQs respectively of 106, 107, 108, 109, 110, 111, 112, and 113. There is a perfect correspondence between the IQs of fathers and daughters, and we might judge the character to be perfectly heritable because, knowing a father's IQ, we could tell without error which of the daughters was his. The correlation is, in fact,  $r = + 1.0$ , yet the daughters are ten points above their fathers in IQ, so the experience of being raised by foster parents had a powerful effect. There is thus no contradiction between the assertion that a trait is perfectly heritable and the assertion that it can be changed radically by environment. As we shall see, this is not a hypothetical example.

Second, a correlation between two variables is not a reliable guide to causation. If  $A$  and  $B$  are correlated, one may be the cause of the other, they may both

be the consequence of a common cause, or they may be entirely accidentally related. The number of cigarettes smoked per day is correlated with the chance of lung cancer because smoking is a cause of lung cancer. The floor area of a person's house and the average age to which he or she lives are positively correlated not because living in a big house is conducive to health but because both characteristics are a consequence of the same cause—high income. For that matter, the distance of the Earth from Halley's comet and the price of fuel are negatively correlated in recent years because one has been decreasing while the other increased, but for totally independent reasons.

In general, heritability is estimated from the correlation of a trait between relatives. Unfortunately, in human populations two important sources of correlation are conflated: Relatives resemble each other not only because they share genes but also because they share environments. This is a problem that can be circumvented in experimental organisms, where genetically related individuals can be raised in controlled environments, but human families are not rat cages. Parents and their offspring may be more similar than unrelated persons because they share genes but also because they share family environment, social class, education, language, etc. To solve this problem, human geneticists and psychologists have taken advantage of special circumstances that are meant to break the tie between genetic and environmental similarity in families.

The first circumstance is adoption. Are particular traits in adopted children correlated with their biological families even when they have been separated from them? Are identical (i.e. monozygotic, or one-egg) twins, separated at birth, similar to each other in some trait? If so, genetic influence is implicated. The second circumstance holds environment constant but changes genetic relationship. Are identical twins more alike than fraternal (i.e. dizygotic, or two-egg) twins? Are two biological brothers or sisters (sibs) in a family more alike than two adopted children in a family? If so, genes are again implicated because, in theory, identical twins and fraternal twins have equal environmental similarity but they are not equally related genetically.

The difficulty with both these kinds of observations is that they only work if the underlying assumptions about environment are true. For the adoption studies to work, it must be true that there is no correlation between the adopting families and the biological families. There must not be selective placement of adoptees. In the case of one-egg and two-egg twins, it must be true that identical twins do not experience a more similar environment than fraternal twins. As we shall see, these problems have been largely ignored in the rush to demonstrate the heritability of IQ.

The theory of estimating heritability is very well worked out. It is well known how large samples should be to get reliable estimates. The designs of the observations to avoid selective adoptions, to get objective measures of test performance without bias on the part of the investigator, to avoid statistical artifacts that may arise from unrepresentative samples of adopting families, are all laid out in textbooks of statistics and quantitative genetics. Indeed, these theories are constantly put into practice by animal breeders who would be unable to have

their research reports published in genetics journals unless they adhered strictly to the standard methodological requirements. The record of psychometric observations on the heritability of IQ is in remarkable contrast. Inadequate sample sizes, biased subjective judgments, selective adoption, failure to separate so-called "separated twins," unrepresentative samples of adoptees, and gratuitous and untested assumptions about similarity of environments are all standard characteristics in the literature of IQ genetics. There has even been, as we shall see, massive and influential fraud. We will review in some detail the state of psychometric genetic observations—not simply because it calls into question the actual heritability of IQ, but because it raises the far more important issue of why the canons of scientific demonstration and credibility should be so radically different in human genetics than in the genetics of pigs. Nothing demonstrates more clearly how scientific methodology and conclusions are shaped to fit ideological ends than the sorry state of the heritability of IQ.

### The Cyril Burt Scandal

The clearest evidence, by far, for the genetic determination of IQ was the massive life's work of the late Sir Cyril Burt. In 1969 Arthur Jensen quite correctly referred to Burt's work as "the most satisfactory attempt" to estimate the heritability of IQ. When Burt died, Jensen referred to him as "a born nobleman," whose "larger, more representative samples than any other investigator in the field has ever assembled" would secure his "place in the history of science."<sup>13</sup> Hans Eysenck wrote that he drew "rather heavily" on Burt's work, citing "the outstanding quality of the design and the statistical treatment in his studies."<sup>14</sup>

The Burt data seemed so impressive for a number of very good reasons. First, one of the simplest ways, at least in theory, of demonstrating the heritable basis of a trait is to study separated identical twins. The separated twin pairs have identical genes, and they are assumed not to have shared any common environment. Thus, if they resemble one another markedly in some respect, the resemblance must be due to the only thing they share in common: their identical genes. The largest IQ study of separated identical twins ever reported, supposedly based on fifty-three twin pairs, was that of Cyril Burt. The IQ correlation of separated twin pairs reported by Burt was strikingly high, more so than that reported in the three other studies of separated twins. The most important aspect of Burt's study, however, was that he alone had been able to measure quantitatively the similarity of the environments in which the separated twin pairs had been reared. The incredible (and convenient) result reported by Burt was that there was no correlation at all between the environments of the separated pairs.

Further, in order to fit a genetic model to IQ data, it is necessary to know what the IQ correlations are for a considerable number of types of relatives—some close and some not so close. Burt was the only investigator in history who claimed to have administered the same IQ test, in the same population, to the full

gamut of biological relatives of all degrees of closeness. In fact, for some types of relatives (grandparent-grandchild, uncle-nephew, second cousin pairs), the IQ correlations reported by Burt are the *only* such correlations *ever* to have been reported. The Burt correlations for all types of relatives corresponded with remarkable precision to the values expected if IQ were almost entirely determined by the genes.

The blunt fact is that Burt's data, which have played so important a role, were reported and published in what is clearly a truly scandalous and suspicious fashion. The implausibility of Burt's claims should have been noted at once by any reasonably alert and conscientious scientific reader. To begin with, Burt never provided even the most elementary description of how, when, or where his "data" had been collected. The normal canons of scientific reporting were ignored entirely by Burt, and by the editors of the journals that published his papers. He never even identified the "IQ test" he supposedly administered to untold thousands of pairs of relatives. Within many of his papers, even the sizes of his supposed samples of relatives were not reported. The correlations were given without any supporting details. The 1943 paper that first reported many of the correlations between relatives made only the following reference to procedural details: "Some of the inquiries have been published in LCC reports or elsewhere; but the majority remain buried in typed memoranda or degree theses."<sup>15</sup> Conscientious scientists usually do not refer interested readers to their primary sources and documentation in such a cavalier way. The reader should not be surprised by the fact that none of the London County Council reports, typed memoranda, or degree theses glancingly referred to by Burt have ever come to light.

The very few occasions when Burt made specific statements about his procedure should have provoked some doubts in his scientific readers. For example, in a 1955 paper Burt described the procedure by which he obtained IQ test results for parent-child, grandparent-grandchild, uncle-nephew, etc. The IQ data for children were supposedly obtained by revising (on the basis of teacher's comments) the results of unspecified IQ tests given in school. But how did Burt obtain "IQs" for adults? He wrote: "For the assessments of the parents we relied chiefly on personal interviews; but in doubtful or borderline cases an open or a camouflaged test was employed."<sup>16</sup> That is, in measuring the "IQs" of adults Burt did not even *claim* to have administered an objective, standardized IQ test. The IQ was said to have been guessed at during an interview! The spectacle of Professor Burt administering "camouflaged" IQ tests while chatting with London grandparents is the stuff of farce, not of science. The correlations reported by Burt on this claimed basis, however, were routinely presented as hard scientific truths in textbooks of psychology, of genetics, and of education. Professor Jensen referred to precisely this work as "the most satisfactory attempt" to estimate the heritability of IQ. When Burt's procedure was publicly criticized, Hans Eysenck was able to write in Burt's defense: "I could only wish that modern workers would follow his example."<sup>17</sup>

The collapse of Burt's claims within the scientific community began when

attention was drawn to some numerical impossibilities in Burt's published papers.<sup>18</sup> For example, Burt in 1955 claimed to have studied twenty-one pairs of separated identical twins and reported that, on some unnamed group test of intelligence, their IQ correlation was .771. By 1958 the number of pairs had been increased to "over 30"; surprisingly, the IQ correlation remained precisely .771. By 1966, when the sample size had been increased to fifty-three pairs, the correlation was still exactly .771! This remarkable tendency for IQ correlations to remain identical to the third decimal place was also true of Burt's studies of nonseparated identical twin pairs: as the sample size increased progressively with time, the correlation failed to change. The same identity to the third decimal place was also true of IQ correlations for other types of relatives published by Burt, as sample sizes increased (or in some cases decreased) over time. These and other characteristics indicated that, at the very least, Burt's data and claimed results could not be taken seriously. As one of us in 1974 concluded after surveying Burt's work: "The numbers left behind by Professor Burt are simply not worthy of our current scientific attention."<sup>19</sup>

The scientific exposure of Burt prompted Professor Jensen to execute a brisk about-face. Two years earlier Jensen had described Burt as a born nobleman, whose large and representative samples had secured his place in the history of science. But in 1974 Jensen wrote, after citing the absurdities that critics had already documented, that Burt's correlations were "useless for hypothesis testing"—that is to say, worthless.<sup>20</sup> But Jensen maintained that Burt's work had merely been careless, not fraudulent; and he also maintained that the elimination of Burt's data did not substantially reduce the weight of the evidence demonstrating a high heritability of IQ. That incredible claim was made despite Jensen's earlier assertion that Burt's was "the most satisfactory attempt" to calculate the heritability of IQ.<sup>21</sup>

The argument over Burt's data might have remained a discreet academic affair and might have tiptoed around the question of Burt's fraudulence were it not for the medical correspondent of the London *Sunday Times*, Oliver Gillie. Gillie tried to locate two of Burt's research associates, the Misses Conway and Howard, who had supposedly published papers in a psychological journal edited by Burt. According to Burt, they were responsible for the IQ testing of the separated identical twins, for the testing of other types of relatives, and for much of Burt's published data analyses. But Gillie could uncover absolutely no documentary record of the existence of these research associates. They had not been seen by, and were wholly unknown to, Burt's closest co-workers. When asked about them by his housekeeper, Burt had replied that they had emigrated to Australia or New Zealand, this at a time *before*, according to Burt's published papers, they were testing twins in England. Burt's secretary indicated that Burt had sometimes written papers signed by either Conway or Howard. These facts led Gillie to suggest, in a front-page article in 1976, that Conway and Howard may never have existed.<sup>22</sup> The article flatly accused Burt of perpetrating a major scientific fraud, a charge subsequently supported by two of Burt's former students, now themselves prominent psychometricians, Alan and Ann Clarke.

The public exposure of Burt's fraudulence seemed to strike a raw hereditarian nerve. Professor Jensen wrote that the attack on Burt was designed "to wholly discredit the large body of research on the genetics of human mental abilities. The desperate scorched-earth style of criticism we have come to know in this debate has finally gone the limit, with charges of 'fraud' and 'fakery' now that Burt is no longer here to . . . take warranted legal action against such unfounded defamation."<sup>23</sup> Professor Eysenck joined in by pointing out that Burt had been "knighted for his services" and that the charges against him contained "a whiff of McCarthyism, of notorious smear campaigns, and of what used to be known as character assassination."<sup>24</sup>

The attempt to defend Burt by assaulting his critics soon collapsed. The eulogy at Burt's memorial service had been delivered by an admirer, Professor Leslie Hearnshaw, and had prompted Burt's sister, in 1971, to commission Hearnshaw to write a biography of her distinguished brother and to make Burt's private papers and diaries freely available to him. When the fraud charges exploded, Hearnshaw wrote to the *Bulletin* of the British Psychological Society, indicating that he would assess all the available evidence and warning that the charges of Burt's critics could not be lightly dismissed. This warning seems to have muted the tone of Burt's more militant hereditarian defenders. Thus, by 1978, Eysenck wrote of Burt: "On at least one occasion he invented, for the purpose of quoting it in one of his articles, a thesis by one of his students never in fact written; at the time I interpreted this as a sign of forgetfulness."<sup>25</sup>

The Hearnshaw biography, published in 1979, has put to rest any lingering doubts about Burt's wholesale faking.<sup>26</sup> The painstaking searches and inquiries made by Hearnshaw failed to unearth any substantial traces of Miss Conway, or Miss Howard, or of any separated twins. There were many instances of dishonesty, of evasion, and of contradiction in Burt's written replies to correspondents who had inquired about his data. The evidence made clear that Burt had collected no data at all during the last thirty years of his life, when, supposedly, most of the separated twins had been studied. With painful reluctance, Hearnshaw found himself forced to conclude that the charges made by Burt's critics were "in their essentials valid." The evidence demonstrated that Burt had "fabricated figures" and had "falsified." There is now no doubt whatever that all of Burt's "data" on the heritability of IQ must be discarded. The loss of these incredibly clear-cut "data" has been devastating to the claim that a substantial IQ heritability was demonstrated.

But what are we to make of the additional fact that Burt's transparently fraudulent data were accepted for so long, and so uncritically, by the "experts" in the field? Perhaps the clearest moral to be drawn from the Burt affair was spelled out by N. J. Mackintosh in his review of the Hearnshaw biography in the *British Journal of Psychology*:

Ignoring the question of fraud, the fact of the matter is that the crucial evidence that his data on IQ are scientifically unacceptable does not depend on any examination of Burt's diaries or correspondence. It is to be found in the data

themselves. The evidence was there . . . in 1961. It was, indeed, clear to anyone with eyes to see in 1958. But it was not seen until 1972, when Kamin first pointed to Burt's totally inadequate reporting of his data and to the impossible consistencies in his correlation coefficients. Until then the data were cited, with respect bordering on reverence, as the most telling proof of the heritability of IQ. It is a sorry comment on the wider scientific community that "numbers . . . simply not worthy of our current scientific attention" . . . should have entered nearly every psychological textbook.<sup>27</sup>

We do not view the uncritical acceptance of Burt's data as an unusual or inexplicable "sorry comment on the wider scientific community." The fraud perpetrated by Burt, and unwittingly propagated by the scientific community, served important social purposes. Professor Hearnshaw's biography essentially saves the face of psychometry by probing the individual psychology of Burt to ask why he should have been moved to such fraudulence. Burt, no longer a nobleman but now victim of a debilitating and psychiatrically distressing disorder, has become the bad apple of psychometry. By 1980, when the British Psychological Society was prepared to draw up its "Balance Sheet on Burt,"<sup>28</sup> there had been a closing of the ranks; the psychometric doyens reiterated their belief that, despite the eviction of Burt, the residual evidence for the heritability of intelligence was strong. The social function of IQ ideology was still dominant.

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RACE, GENDER, AND SCIENCE  
*Anne Fausto-Sterling, General Editor*

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## APPROPRIATING THE IDIOMS OF SCIENCE

### The Rejection of Scientific Racism

*Nancy Leys Stepan and Sander L. Gilman*

This paper considers some writings of minority groups, as they responded to and resisted the claims of scientific racism. In exploring the relationship between language and resistance we focus on two very different groups of individuals stereotyped as different and inferior in the biological, medical, and anthropological sciences, namely African-Americans and Jews. We concentrate specifically on the period of transition to modern science between 1870 and 1920, when the claims of scientifically established inferiority were pressed most insistently by the mainstream scientific community. Our analysis reveals a body of literature by minorities and the marginal *about* the sciences of themselves that has been virtually untouched by historians of science. What did the men and women categorized by the biological and medical sciences as racially distinct and inferior say about the matter? How did they respond to the claims made about them in the name of science?

Limitations of space have made us very selective with materials. Examples have been chosen for their effective illustration of points, and the goal is to open up a problem for discussion in the history of science and racism that has hitherto been almost entirely ignored.

#### The Problem Defined

This paper derives from a consideration of two intertwined issues: the centrality of scientific racism to the Western intellectual tradition, and the absence of sustained criticism of scientific racism from within mainstream science in the period under study. Historians have long been aware of the existence of scientific racism in Western societies, especially its intensification and institutionalization in the second half of the nineteenth century. Scientific racism was significant because it provided a series of lenses through which human variation was constructed, un-

derstood, and experienced from the early nineteenth century until well into the twentieth century, if not until the present day.<sup>1</sup> We assume in this paper that the races that peopled the texts of science in the past were "artifactual," constructed categories of social knowledge. These categories had material weight in the lives of individuals and groups; racial identities were embodied in political practices of discrimination and law, and affected people's access to education, forms of employment, political rights, and subjective experience. Scientific language was one of the most authoritative languages through which meaning was encoded, and as a language it had political and social, as well as intellectual, consequences.<sup>2</sup>

In studying the history of scientific racism, we have been struck by the relative absence of critical challenges to its claims from within mainstream science. This absence is in itself an interesting problem in the sociology of scientific knowledge, since controversy and contention are often taken to be characteristic of science and the route by which empirical certainty is established. When it came to the sciences of race difference, however, disagreements tended to be minor and technical.<sup>3</sup> Since racial science was invariably a science of inequality, produced by European men in an age of widespread racism, to a large (but not predetermined) extent the scientists' own racial identities and identifications prevented them from asking critical questions about their own science—its assumptions, its methods, its content. The concepts within racial science were so congruent with social and political life (with power relations, that is) as to be virtually uncontested from inside the mainstream of science.<sup>4</sup>

One place one encounters a "critical tradition" in relation to scientific racism is in the writings of those stereotyped by the sciences of the day. These writings had a problematic relation to the mainstream, since by the very definitions of racial science the stigmatized were largely outside, or at the margins, of science. Their exclusion was part of the very process of the construction of the sciences of difference and inequality, a result of the scientific expectation that the so-called lower races served mainly as objects of study, but not as scientific truth-seekers themselves. Yet many individuals reacted to scientific racism by actively seeking to enter the relatively closed circle of science, or to use its tools and techniques, to define and defend themselves.

There is far more writing by such individuals than is generally recognized by historians of science. Much of the historical work of uncovering the "struggles and strategies" (in Margaret Rossiter's words) of minority writers, as they confronted a hostile and stereotyping science of race, remains to be done. Our purpose here is to reflect generally on such writing, and to present some examples of the "textual" strategies we have uncovered.<sup>5</sup>

The problem of resistance to imposed meaning and identity in the human sciences is only beginning to be articulated by scholars. The situation contrasts markedly with the issue of social and political resistance, long considered an essential part of historical analysis, for example in the brilliant work on slave revolts and runaway slave communities, or the fight for black or female suffrage. These studies are indispensable to the study of intellectual resistance, since the capacity to formulate challenges to dominant discourses is closely tied to the

political and social resources of the groups making the challenges. Often, the strategies of intellectual resistance—struggles over meaning—mirror or are part of the same process of political struggle. Confronting scientific racism necessitated acquiring a degree of control over the elements of an intellectual idiom, their reassemblage and employment for new ends. How could science be used to transform the racial valuations built into discussions of human variation? Can we discern in the writings of minorities a variety of different tactics, and if so, what were they? As a genre of writing, does it have distinctive features? What effect did the resistance of minorities to negative scientific claims have on the dominant discourse? What does the study of such writings offer the historian of scientific change?

In the history of science, the reasons for theoretical neglect of such questions are rather straightforward. The temptation to think of science as a neutral and universal form of knowledge is still strong, despite many years of criticism of traditional scientific epistemology. By thinking of science as objective, scientists have been in a position to dismiss areas of knowledge from the past that are now viewed as obviously out of date and biased—such as scientific racism—as nothing but "pseudoscience." Studying the resistance of men and women to what has been labeled a pseudoscience is then seen as a narrow endeavor, of interest primarily to the "victims" themselves, but not central to the story of modern science. Furthermore, calling scientific racism a pseudoscience also allows scientists to refuse to confront the issue of the inherently political nature of much of the biological and human sciences, and to ignore the problem of the persistence of racial metaphors of inferiority in the sciences of today.

Critical approaches from outside the history of science have been more useful to our work on oppositional, cognitive strategies, even though scientific writing usually lies outside their purview. Antonio Gramsci's explorations of cultural hegemony and the role of the "organic" intellectual in his *Prison Notebooks*; the deconstructionists' attacks on essentialism and metaphysics, and their emphasis on the radically heterogeneous nature of the text; Michel Foucault's projects on the inherently political character of knowledge, his analysis of power as something dispersed discursively through entire systems of discourses and practices, and the problematic character of resistance from within the regime of power and knowledge; recent analyses of minority writing that examine the degree to which female and minority literary traditions represent explicit or implicit subversions of the dominant discourse: all of these developments are relevant to analyzing scientific racism.<sup>6</sup> Our study can be seen as a contribution to the debate about how people use languages to constitute themselves as self-conscious subjects, or find a self-representation that is not sexualized or racialized.<sup>7</sup> Our work is distinctive in its focus on science as an especially weighty discourse of identity, whose appropriation by oppositional groups was extremely problematic. The period from 1870 to 1920 has been chosen on purpose because, as we shall show, science acquired its modern epistemological, institutional, and cultural forms during this period. Our interest in this paper lies in the interplay between these cultural forms, scientific racism, and scientific resistance. Strategies and mean-

ings are always historically specific and cannot be given transcendent value; our focus throughout is on a particular kind of discourse in a particular moment in history, and the strategies open to resisting individuals and groups.

### The Authority of Modern Science

Responding to scientific racism was peculiarly difficult because of certain characteristics of the emerging discourse of science itself. Our argument is that from the mid-nineteenth century onward, scientific claims could be effectively rebutted only by scientific discourses to which resisting groups stood in an especially disadvantaged and problematic position.

Our period situates scientific racism in a moment of elaboration and institutional embodiment of modern science and its epistemology. It was the time in which, as Morrell and Thackray have remarked in their history of the British Association for the Advancement of Science (BAAS), science became consolidated as "the dominant mode of cognition of industrial society." As a mode of cognition, science was conceptualized as "a sharply edged and value-neutral domain of knowledge"—as an apolitical, nontheological, universal, empirical, and uniquely objective (in part because uniquely methodological) form of knowledge unlike any other.<sup>8</sup> The result was the self-definition of science as *the* nonpolitical, unbiased arena of knowledge.

This conceptualization was not a "natural" outcome of the unproblematic study of nature, but a social outcome of a process whereby science was historically and materially constituted to have certain meanings, functions, and interests. In a complex series of innovations, science's epistemological claims were given definition and institutional representation in the form of new scientific societies and organizations sharply delimited from other institutions. These innovations were tied not only to industrialization, but to the politics of class, and the closing of ranks of bourgeois society in the face of challenges from the working class in the 1830s and 1840s. Race and gender were also crucial to the construction of modern science, in that science was defined as "masculine" and European in its abstraction, detachment, and objectivity.<sup>9</sup>

Morrell and Thackray point out that appeals to an impersonal "nature" are common in times of turmoil; what made the mid-nineteenth century distinctive was the successful institutionalization of a particular view of that "nature."<sup>10</sup> The processes of boundary setting were contested at many points, since they meant the delegitimation of many areas of knowledge and redefinition of those areas as "nonscience" or "pseudoscience." Morell and Thackray, for instance, demonstrate that between 1832 and 1870, as the BAAS created a new ideology of science, practitioners in fields of inquiry ruled "unscientific" were excluded from the association and thereby from representation within "science." Areas fraught with moral and/or political controversy kept a place within the boundaries of science only when purged of those concerns, as scientists adopted the value-

neutral, empirical language now seen as defining science itself. Science as a form of knowledge separated itself from other knowledge systems; in the process, the dichotomies between the pure and the impure (or the applied sciences), the rational and the irrational, the objective and the subjective, the hard and the soft, the male and the female, were given material form. Such polarities, and the institutional boundaries that created and maintained them, were not the inevitable results of a nature merely "discovered" and described; they were the products of active institution creation, demarcation setting, and the successful use of political and cultural resources to achieve these ends.<sup>11</sup>

The formation of the scientific text as a new, standardized cultural genre, replacing the more open, varied, metaphorically porous, literary forms of science, is a further aspect of scientific modernization that bears on the problem of the responses to scientific racism. It was in the late nineteenth century that the modern scientific text as we know it stabilized to become the standard, accepted form of writing in nearly all branches of the natural sciences. We are referring here to the scientific paper—the short, depersonalized, empirical paper that is still the hallmark of science today. In a persuasive account of the scientific paper as a cultural and literary genre that emerged in the specific, historically contingent conditions of the late nineteenth century, Gyorgy Markus argues that the scientific text served normative goals, and through its form—its depersonalized authorship, its demand for a peculiarly competent scientific reader—successfully satisfied the expectations of science for constant innovation and accumulation of knowledge.<sup>12</sup> The neutral style of the scientific paper, the absence of a strong, individualized, authorial "I," the emphasis on the factuality of nature, on a nature revealed by specific methods (experimental, technical)—all these features rendered the scientific text problematic for the nonscientific writer and reader and successfully circumscribed the process of contestation.

The new cultural genre of science was an added impediment to opposition in the realm of scientific racism, since it delegitimized the cultural forms of the earlier period.<sup>13</sup> The scientific text became more sharply distinguished from the literary; in the process the range of literary repertoires of meaning—the opportunity for literary play and hermeneutics—was reduced. One thinks, for example, of the charged and metaphoric language of Charles Darwin's *Origin of Species* compared to the dry, limited vocabulary of the sciences of evolution by the 1920s and 1930s. Darwin could not keep control over the metaphors he introduced (such as natural selection, struggle for survival, survival of the fittest). Nearly every term he used was multivalent and was appropriated in selective and varied ways by very different groups for very different purposes.<sup>14</sup> Though Darwin endeavored in later editions of *The Origin of the Species* to reduce the metaphoric ambiguities of his science, his attempts failed, and until well into the twentieth century Darwinism served as a metadiscourse that opened up, rather than merely closed off, the discussion of nature. By the 1900s in the physical sciences, and by the 1920s in the biological sciences, however, the metaphors of scientific language had become much more tightly controlled. The modern scientific text had

replaced the expansive scientific book, and the possibilities of multivalent meanings being created out of scientific language were thereby curtailed.

### The Marginalization of Moral/Political Argument in Science

The result of the various processes of transformation of science that accompanied industrialization and modernization was that, from 1870 to 1920, science became both more specialized and authoritative as a cultural resource and language of interpretation. It began to replace theological and moral discourse as the appropriate discourse with which to discuss nature. Science also encroached heavily on political discourse, as many political issues were transposed into the realm of neutral "nature," the scientists' province. The outcome was a narrowing of the cultural space within which, and the cultural forms by which, the claims of biological determinism could be effectively challenged.

The effects of this narrowing can be seen in the marginalization and delegitimation of a number of forms of oppositional writing that had been used before about 1860. We will give a few examples. One was to keep in the foreground the moral issues of rights and justice, and to refuse to separate them from scientific ones. These rights, belonging to individuals as members of the human family, rights debated in political and theological terms until the 1850s and 1860s, were increasingly reduced to questions about the racial "natures" of individuals, questions that scientists now claimed had objective, neutral answers. Moral rights were thereby translated into matters of anatomy and physiology.

In the mid-1800s, African-Americans, confronting a "purely factual" science whose message was apparently ever more racialistic in its conclusions concerning themselves, tried to resist the process of reduction and "naturalization" associated with science. In 1854, for instance, the abolitionist Frederick Douglass, in an address titled "The Claims of the Negro Ethnologically Considered," attacked the scientific racists by questioning their logic, their data, and their conclusions concerning the supposed gulf separating the white and black races. He proved to his audience that anatomically and craniologically the similarities between the Negro and the white race far outweighed the differences, that the human species was one, and that the Negro could therefore claim full membership in the human family. But at the end of his address, Douglass made a crucial move from the discourse of anatomy to the discourse of morality and rights: "What, if . . . the case [of anatomical similarity between whites and blacks] is not made out? Does it follow, that the Negro should be held in contempt?" He answered with a resounding "No," because the title to freedom, liberty, and knowledge was not a question of "natural" difference or similarity, but an issue of natural rights and morality. "It is registered in the Courts of Heaven, and is enforced by the eloquence of the God of all the earth." Douglass here asked a question that would virtually disappear from science: What difference does difference make to human

rights? Douglass's answer was that it made no difference, because equality and rights were moral, political, and religious issues.<sup>15</sup> The silence of most scientific texts on this matter after 1860 suggests the power of science to occupy the terrain formerly held by moral discourse, and to disguise the political projects that helped constitute the scientific field.

African-American intellectuals continued to infuse discussions of race with theological, moral, and political concerns. They continued, especially, to evoke the older tradition of Christian monogenism long after it had lost ground as an acceptable style of scientific argumentation in mainstream science. Christian monogenism was one of the few powerful traditions linked to science on which they could draw in self-defense; moreover, theological discourse was one of the most significant intellectual productions of the black community. A notable example of the genre is the work of the Harvard-trained physician and Pan-Africanist Dr. Martin R. Delany. He used the Mosaic story of the Deluge to structure a scientific study of race unity in his *Principia of Ethnology* (1879). His strategy made sense within the black tradition but it rendered his book a cultural and linguistic hybrid unlike white scientific writings on race in the same period, a hybrid reflected in its very title, half-English and half Latin. Religiously oriented ethnography survived as a form because it served the political and psychological needs of the African-American. Isolation from the norms of science meant that those norms were less internalized. The creation of a different narrative form resisted the conventions of science, but as a strategy of resistance, theological arguments had the disadvantage of seeming illegitimate or "unscientific" when measured by the canons of mainstream science.<sup>16</sup>

Another strategy of resistance was the employment of wit, irony, or parody. These literary devices became marginalized and delegitimized in the second half of the nineteenth century precisely because they did not fit the depersonalized, nonauthorial style of modern science. As a style, humor had the advantage of distancing and subverting the claims of science, thereby serving as a strategy of empowerment. An exceptional use of the ironic voice is found in an anonymous article in the black American periodical, the *Anglo-African Magazine*, in 1860. The article, which was almost certainly the work of the black Scottish-trained physician Dr. James McCune Smith, is especially interesting because it represents, as far as we know, the first account of Darwinian ideas by an African-American. Titled "A Word for the 'Smith' Family," the article put Darwin's new science to witty use to defend the unity and success of the black people named Smith. The very commonness of the name Smith was taken as proof of the evolutionary success of black people, who were shown to have thrived, adapted, and multiplied through natural selection and the struggle for survival. The theory of common descent was also used implicitly to poke fun at the pretensions of all the white Smiths who thought they were distinct and superior to black Smiths. All Smiths, the author suggested, were linked together in an evolutionary kinship.<sup>17</sup>

Wit and irony gave a writer control and power over language and content. Parody could perform a similar task of distancing in relation to the claims of

science. But all three literary (or cultural) styles, because they impressed directly the author's own personality on the text, had less and less place in scientific discourse as the century wore on. As strategies of resistance, they too became marginal to the scientific enterprise.<sup>18</sup>

### Using the Scientific Idiom

Despite the continuing resort to political and moral written arguments to challenge the claims of the scientific racists, the professionalization of science made such tactics less effective. By the last third of the century, effective strategies of resistance were often structured by the dominant discourse. Science's conceptual categories, rhetorical styles, and methodologies were adopted.

Of course, science had always been used as a source of ideas and arguments by people challenging the conventional wisdom and dominant ideologies of their day. Feminists in the eighteenth century defended the rights of women in the language of Newton; artisanal socialists in the early nineteenth century called upon Lamarckian evolutionism to serve their antibourgeois, confrontational needs.<sup>19</sup> After the mid-1800s, however, writers were forced to use the sciences of the day more narrowly to challenge scientific claims if they wished to have a hearing within science. As minorities moved into the scientific arena, their own competencies in the scientific idiom grew, as did their ability to meet science on its own terms. It is no surprise, then, to find that evolutionism, hereditarianism, the new Mendelism, and even "eugenics" (which in the United States was racist almost by definition) were embraced by African-Americans, Jews, and other minorities to counter the charges of racial inferiority.<sup>20</sup> The cultural forms of scientific texts were imitated in a necessary process of identification with the norms and standards of science. Blacks produced texts of blackness, with anthropological illustrations of heads, cranial measurements, and scientific tables of racial health and illness.<sup>21</sup> Jewish scientists particularly used scientific representations of self because of their greater access to scientific education and greater commitment to the norms of science.

The use of science to dismantle the claims of science involved the authors in complex processes of transformation of the meaning of terms. Any discussion of the critical responses to scientific racism in the late nineteenth and early twentieth century must acknowledge how the need to meet science on its own terms limited the nature of the response. This was also true of other discourses—for example, literary discourse—but scientific discourse was distinguished by its high barriers of entry.<sup>22</sup> By the end of the nineteenth century, professionalization separated science from both "high" literary culture and popular culture. Science acquired technical procedures and practices, as well as new vocabularies unfamiliar to the nonscientist; these could only be acquired in the professional scientific academy. Only the trained scientist, it was claimed, was able to speak

coherently and legitimately about scientific matters. Disciplinary boundaries were a further disincentive to the kind of critical inquiry that dismantling scientific racism and sexism required.<sup>23</sup>

Exclusion from the academy meant exclusion from the authoritative use of the idioms of science. Whenever racial minorities and women wrote critically about the sciences of themselves, their writing ran the risk of being ignored or dismissed because it came from "outside" professional science, and was therefore by definition "unscientific." Furthermore, resisting the claims of scientific racism by pointing out its subjective, metaphorical, particular, or politically biased nature (namely, that science was articulated around notions of race) was extraordinarily difficult because many minority writers shared the belief of the mainstream scientists in science as a progressive, instrumental, and objective form of knowledge. Given the epistemological status of science, to admit that race, especially one's own, was an issue in science was to make the writer immediately less than fully "objective" and therefore less than fully "scientific."<sup>24</sup> For the African-American or Jew writing as a scientist, or from *within* science, the writer's own status as objective observer of nature was at stake. The problem of both using science as a language of self-assertion and identity, while exposing its essentially *political* character in relation to racial claims, was rarely addressed by resisting groups because rarely recognized.<sup>25</sup>

### Some Tactics and Responses to Scientific Racism

In relation to the terrain and idioms of science, different groups were very differently situated. Access to education and the languages of science varied, and was indeed a part of the social construction of race as a category of human and political difference. Yet even when the barriers were very great, as was the case for African-Americans, critical engagement with scientific racism long predated the Civil War; the way this critical response influenced black periodical writings is relatively unstudied. Jews as a group had far greater access to education in general and were strongly attracted to the scientific professions. In some branches of science, in fact, Jews made up 6 to 10 percent of all scientists, a figure much higher than their total representation in the population.<sup>26</sup>

What is not generally realized is the degree to which Jews themselves engaged with racial science, because of its potentially harmful implications for themselves as a "race." Their cultural production has barely been studied in the context proposed here. As Jews (and other groups stereotyped in the biological and social sciences of the day) were drawn more deeply into the sciences of racial difference, whether in measuring themselves as a race by craniometry and other methods, or by comparing one fraction of the Jewish "race" with another, or by commenting on or contesting the thesis of Jewish pathology and illness, they were tempted simultaneously to embrace and reject the field: to embrace science's methods, concepts, and the promise it held out for discovering knowledge,

and to reject, in a variety of ways, the conclusions of science as they appeared to apply negatively to themselves.

Audre Lorde has stated that "the master's tools will never dismantle the master's house."<sup>27</sup> Yet our studies suggest that when minorities took on the matter of racial science, in the idioms of science, that science was inevitably changed. Even for the most oppressed, science created spaces for self-definition and self-representation. It is these interstices—or more precisely, the strategies that created them—to which we turn now.

Our discussion takes the form of a simple typology. One of the typology's functions is to draw attention to a variety of responses to scientific racism and a range of textual materials that are usually overlooked. By providing a simple classification of these materials, we show that despite the individuality of the responses, despite the very different social experiences of the writers analyzed, despite the fact that "race" was not a unitary category but had multiple meanings, certain similarities in textual strategies can be found in minority responses. These similarities indicate the power of scientific racism to map the terrain within which, and the terms by which, resistance and challenge could be carried out. Recurring tropes, recurring techniques of reenvisioning identity, certain patterns in the tactics of re-representation, characterize the critical tradition we examine.

The kinds of responses illustrated here are not exhaustive or mutually exclusive. We find that a writer employed a number of responses or strategies, either simultaneously or successively over the course of a writing career. Since the dominant ideology of scientific racism was itself heterogeneous, and fueled by contradictory impulses, so were the resisting discourses that echoed, commented upon, and modified them. "Cannibalization," disarticulation and reassemblage, and the employment of multiple modes of attack were the tactics that made sense.<sup>28</sup> Our typology, then, is a highly dynamic one, employed mainly to point out how responses placed the writer in different relations to the dominant discourse, with varying consequences psychologically, intellectually, and scientifically.

The most pernicious effect of racial science was the profound *internalization* of the negative terms and norms of the discourse itself. Insofar as resisting discourses are necessarily tied to, reflective of, and constructed in similar terms to dominant discourses, "internalization" is to a certain extent characteristic of all strategies of intellectual resistance. "Internalization" here, however, means more than this—we refer to the very profound psychological and social introjection of negative images and meanings contained in the stereotypes, in the constructor and understanding of one's self-identity. Such internalization was recognized by the stereotyped people as a common and profoundly problematic outcome of stereotyping discourses. "In the psychology of the human mind, suggestion plays an important part," wrote the African-American George Parker in 1908: "If it be true that as a man thinketh, so he is, then the self-making power becomes proportionately more powerful when applied to a whole race. For years it has been constantly affirmed and reaffirmed that races of African blood have contributec

nothing to humanity's store of knowledge and civilization, and this incessant affirmation has produced a conviction of truth not only in the minds of those who affirm it, but also in the minds of those whom it wrongs."<sup>29</sup>

The psychological processes of identification with and internalization of the dominant discourses of "otherness" are extremely complex and varied. Gilman has discussed the fragmentation of identity that internalization of the norms of "otherness" entails, and the conflicts that arise when we use on ourselves the discourse that labels us as different and unacceptable.<sup>30</sup> Our interest here is in the special weight of scientific discourses of otherness and inequality on individual and group self-consciousness in the late nineteenth and early twentieth centuries, in which the self was understood and represented through a preexisting, racialized science. Absolute application, without qualification, of the dominant discourse to oneself or to one's group is for rather obvious reasons relatively rare. Psychologically the outcome is extreme and potentially devastating self-hatred. The publication of one such self-hating text, by the Jewish student of philosophy Otto Weininger in 1903, was followed a few months later by his suicide. Weininger, a student of the Viennese philosophers Laurenz Müller and Friedrich Jodl, attempted to combine a biology of human sexuality with a philosophy of sexual and racial identity. His self-inflicted death, in the house in which Beethoven had died, reveals the profound conflict he experienced when biological determinism came into direct contact with self-definition.

No matter how bizarre Weininger's attempt to place himself within the discourses of race may appear to be, in his own times he was understood as a scientific investigator whose ideas, though controversial, were nevertheless taken as contributions to scholarship, not as examples of psychosis. His well-known text was called *Sex and Character*, a title not unrelated to the problem of internalization. For even in the case of such self-hating texts, it is evident there must always be a way out of extreme negative self-stereotyping, whether or not the step is taken. As a male author, Weininger tried to rescue himself from complete negativity by projecting onto the female sex all the most negative qualities found in the science of biological determinism. Women were portrayed as biologically predisposed to illness, hysteria, and to an inferior and incomplete form of speaking and thinking. Such strategies of internalization and projection in principle provide a form of psychological rescue for the author. But for Weininger, both homosexual and Jewish, it proved a failure.<sup>31</sup>

The choice of the female as the site of negative projection in Weininger's text was not idiosyncratic but deeply structured by the stereotyping discourses of the day, as is shown in texts by male authors writing in very different social and racial circumstances. For example, the African-American William Hannibal Thomas wrote *The American Negro* in 1901, just two years before Weininger's book, during the height of American scientific racism. Thomas's work provoked an immediate angry reaction within the African-American intellectual community. Kelly Miller, professor of mathematics at Howard University, commented that the book had been "more widely noticed than any other recent work on the race problem," and he drew attention to its similarity to Jewish self-hating texts.<sup>32</sup>

Thomas, a northerner who went to the South to study the Negro and did not like what he found, wrote a text that might have been produced by the most racist white scientist. Yet here too processes of projection, of "self-rescue," and re-representation similar to those in Weininger can be discerned, despite the differences in personality, nationality, and the terms of the dominant, scientific, stereotyping discourse. First, Thomas maintained that the women of the Negro race embodied all of the corrupting sensuality that scientists had attributed to the entire Afro-American race. In this way Thomas implicitly exonerated the male author from the most negative aspects of the stereotyping discourse of science. Second, like Weininger, Thomas attempted to shift the definition of "race" away from a permanent, outwardly visible "sign" such as color, to a subjective, psychological condition within. To Thomas, the term "Negro" referred negatively to a "characteristic form of thought and action" (the equivalent to Weininger's "psychological constitution" of Jewishness), so that any person, of whatever hue, who exhibited such characteristic traits was to be considered a "Negro." The inclusion of all people of a particular color (such as blackness) in the same category was an unjust classification "which acts with great severity against a saving remnant of good men and true women."<sup>33</sup> By projecting blackness onto women and away from color per se, the mulatto author both internalized the terms of racialized and scientized discourse, and distanced himself from complete application of the negative elements of racialist discourse to himself.

A second response to racial science was to accept the terms set by the dominant discourse, but to change the valuations attached to them. The significance of biological race differences was accepted, but the "inferior" element in the hierarchy revalued and renamed. This strategy entailed a transvaluation of the terms of the dominant discourse. For example, blackness became an oppositional structure to whiteness, and negativity was thereby transformed into positivity.

Once again, this kind of strategy is familiar in discourses other than scientific ones; we mean to call attention here to the authority of scientific language in structuring such reversals. We call this strategy "transvaluation" because in such writing the response was always couched in terms similar to the dominant discourse, so that one mythology of identity was in a sense replaced by another. Though as a type, the form of writing moved the resisting author further away from total acceptance or accommodation to the dominant discourse of difference, the terms of the dominant discourse were very far from being transcended. In addition, the reverse stereotype that was created had many of the disadvantages of stereotyping in general—it failed to give space to individuality and variation, and could therefore be circumscribing. Furthermore, the simple process of transvaluation of the terms from negative to positive was not always convincing, since the resisting minority voice was always in a position of lesser legitimacy than that of the dominating voice. For the writer, however, such transvaluations often had considerable weight. Reactive and defensive though transvaluations may have been, the result was often empowerment. Upon the basis of such reversals, political solidarities were created. At times, too, they could result in telling criticisms of the science of race of the times.<sup>34</sup>

Since the strategy of reversal and transvaluation echoed the dominant discourse—indeed it appropriated the binary opposition upon which the dominant terms themselves depended—these reversals had somewhat predictable forms. What is important is to connect the use of such reversals to the politics of discrimination and assimilationism. When patterns of discrimination and negative stereotypes circulated widely in discourses, a dialectical politics of self-segregation, solidarity, and reversals was set in motion. We can see this clearly in the use of reversals and transvaluations in the African-American intellectual and political tradition. The "romantic racialism" espoused by some white abolitionists before the Civil War (a romanticism that included a great deal of condescension, as George Fredrickson has remarked), was embraced by African-American writers themselves in periods of intensified racism. August Meier notes the appearance of such racialism in the 1890s, at a time when anti-immigration sentiment was growing and racial categorization and exclusion were widespread.<sup>35</sup> Marcus Garvey's Back to Africa movement involved this kind of reversal and transvaluation.

These reversals were sometimes grounded in the idioms of science. W. E. B. Du Bois, in an early (1897) and controversial statement, "The Conservation of Races," countered the charge of biologically based racial inferiority of the black population by asserting a distinctive and positive psychic-biological identity for African-Americans.<sup>36</sup> Anthony Appiah, in a recent commentary on Du Bois's text, describes this transvaluation as the "classic dialectic," in that it is the antithesis to the thesis of the "American Negro denial of difference." Instead of rejecting the biological concept of race of the time, Du Bois accepted it and proposed that the different biological races of the world were complementary and necessary to each other. Appiah remarks that, as the dialectic required, Du Bois's reevaluation occurred "in the face of the sciences of inferiority."<sup>37</sup>

The results of such transvaluations of a binary opposition were complex. Reversals sometimes involved not only racial "essences" but the narrative histories of self and the group. For instance, B. F. Lee, in an article on "Selection, Environment and the Negro's Future," argued, as Du Bois did later, that race was not effaced or altered by changes in the environment. Lee posited the "Ethiopian" as a noble type of race which, through its "wonderful primitive instinct of adaptation" would survive to take its turn as the ruling race, when dark skin, as well as all the other traits of the Negro, would become "the highest concept of the beautiful."<sup>38</sup>

Among Jewish scientists, transvaluations were often tied closely to scientific anti-Semitism and the politics of antiassimilationism, especially Zionism. Theodor Herzl set the tone when he accepted the pejorative label "Oriental" for the Jews of Europe, and turned it into the basis for a new political ideology, Zionism. Martin Buber's emphasis on the positive racial identity of the Jew, or Max Nordau's call for a "muscle Jew," must be read in this context and seen as the antithesis of stereotyped Jewish feminization, weakness, mental illness, and degeneracy.<sup>39</sup>

A third response to scientific racism moved the speakers even further from

the dominant discourses. This textual strategy involved "recontextualizations." Writings of this kind were associated with a growing empowerment of minorities in science, an increased familiarity with its idioms and technologies, and, therefore, a new authority in challenging the claims of science on its own terms.

Two rather different aspects of the process of recontextualization can be discerned. First, the tools of science were used either to prove that the supposed factual data upon which the stereotypes of racial inferiority were based were wrong, or to generate new "facts" on which different claims could be made. Second, scientific reasoning was used to question the *explanation* of the facts. "Recontextualization" resulted in new interpretations, new narratives of self and identity, which amounted to a scientific counterdiscourse.

For example, the high mortality and morbidity figures of Afro-Americans were cited repeatedly by white physicians as evidence of "racial" susceptibilities and in exoneration of white inaction in the face of the poverty and misery of the black population. Two issues confronted the Afro-American scholar. Were the facts and figures of mortality and morbidity correct? And, to the extent that accurate information about the Negro was available, what was the meaning of the statistics? A superb example of the process of recontextualization was Dr. Kelly Miller's sophisticated critique of the highly racialistic tract on supposed Negro racial inferiority by the statistician Fredrick Hoffman, *Race Traits and Tendencies of the American Negro*.<sup>40</sup> The German origin of the author made him, Hoffman said, "impartial" in his approach. The factual, impersonal form in which Hoffman couched his argument also made the book's impact powerful. It was, said Miller, "by far the most thorough and comprehensive treatment of the Negro problem, from a statistical standpoint, which has yet appeared."<sup>41</sup> As a scientific statement, it demanded an answer in scientific terms.

Miller's long review of Hoffman's work was the first paper to be published by the newly founded American Negro Academy, established in 1897 in Washington, D.C., as a forum for intellectual discussion, and more specifically to refute or challenge the assertions of white scientists.<sup>42</sup> Miller used the occasion to point out the many errors in Hoffman's data. Hoffman's main thesis was that the Negro race was dying out in America because of racially based morbidity exacerbated by conditions of political "freedom." Miller, using data collected from a wide variety of sources from several cities, showed that the African-American population had actually grown in absolute numbers, that its birthrate was greater than that of whites in many places, and that the data "proving" the death of the black race were so selectively assembled by Hoffman as to be completely fallacious.

Miller also recontextualized the actual statistics of death and disease, establishing a very different understanding of the meaning of the figures. Here the most significant figures concerned high black morbidity and mortality from diseases such as tuberculosis, which Hoffman, in keeping with the science of race of the times, presented as the result of innate racial susceptibilities to illness and degeneration. Miller used comparable data on working-class whites in Europe to make the case that high rates of disease and death were a function not of innate racial susceptibilities but of social conditions (whites in parts of Europe were as

poorly off in housing, employment, education, medical care and health as African-Americans). Miller also neatly turned to advantage some of Hoffman's own arguments and data—for instance, by citing Hoffman's assertion that 50 percent of all Negro children who died under the age of five received no medical attention. Miller's conclusion was that the Afro-American should not be discouraged by scientific inquiry, for "it is a condition and not a theory that confronts him."<sup>43</sup>

W. E. B. Du Bois's *The Health and Physique of the Negro American* (1906), was the first of the Atlanta University publications on black Americans that was supervised by Du Bois himself, after he was invited to Atlanta in 1897. This project was more ambitious than Miller's in that it not only attacked the data on which many of the claims of the white scientists were founded, but collected new data as a means of narrating in a new way the history and condition of the Afro-American people.<sup>44</sup> Du Bois was the most imposing black intellectual of the time, and a pioneer of the new sociology, which he saw as an essential tool in the fight against racism. In addition to combing the U.S. Census records for information, Du Bois arranged to have measured, craniometrically and otherwise, some 1,000 students at Hampton Institute in Atlanta, in strict accordance with the techniques of contemporary racial science.

On the existing data of blacks, the study attacked the conclusions concerning the supposed low brain weights of African-Americans, showing they were based on insufficient numbers (the total number of Afro-American brains that had been measured and weighed in America at the time was 500, from which generalizations about more than 20 million persons of Negro descent in the West had been formed). Furthermore, Du Bois pointed out that no account was given of the age, stature, social class, occupation, nutrition, or cause of death of the individuals whose heads and brains had been measured, all of which separately or together affected the structure of the brain. In cases in which morbidity and mortality rates in the Afro-American race were accepted as correct, Du Bois recontextualized the data, emphasizing social conditions, economic environment, and the political realities of discrimination, as their cause. The evidence showed, for instance, that tuberculosis was "not a racial disease but a social disease," and that high infant mortality in the cities was "not a Negro affair, but an index of social conditions." "If the population were divided as to social and economic conditions the matter of race would be almost entirely eliminated," the report concluded.<sup>45</sup>

Jewish scientists also produced a plethora of statistical data of their own—on Jewish disease rates and head shapes—in order to contribute to, and rewrite, the scientific study of their own identity. Of special interest were the scientific data on the supposed high rates of illness. As trained scientists, many employed by municipal and state mental hospitals, Jewish physicians were in a position to gather their own data on rates of mental illness, and to recontextualize the problem as one not of biology but of the history of Jews in Europe. Raphael Becker, a student of Eugen Bleuler and resident physician of the Zurich University psychiatric clinic, for example, produced a monograph that was widely cited by Jews, demonstrating that the higher incidence of mental illness among Western European Jews compared with non-Jews was the result of a "Jewish inferiority com-

plex," itself a product of "two thousand years of persecution."<sup>46</sup> Biological race, Becker asserted, was not the causal factor. Other Jewish scientists challenged the existing information on supposed Jewish mental illness, much as Miller had challenged the data on black racial susceptibilities to disease, and set out to gather their own, more accurate, statistics.

By such recontextualizations, rejection of the *meaning* of the dominant scientific discourses of difference could be achieved, and efforts at explanation could be steered in innovative directions.<sup>47</sup> The new social sciences, with their growing identity as "sciences," their accessibility, and their early connection to reform projects, were important weapons in the fight against biological racism and sexism, although they could also be used in the elaboration of racial and sexual stereotypes.

Another form of recontextualization was "universalization," whereby a trait or character labeled as peculiar to one race and designated as negative, was relabeled instead as a universal trait of all human beings, so that the racial marker was removed as a sign of inferiority. We see it at work in Delany's universalization of color in his *Principia of Ethnology*, where he argued that the black skin, the supposed "sign" of black inferiority, was the result not of features unique to the black race, but merely of a subtle elaboration of a universal, underlying color process in the skin shared by all human beings. The blackness of the black was therefore only a "concentrated" form of something universal in humans. As Delany put it, "The color of the blackest African is produced by *identically the same* essential coloring matter that gives the 'rose cheeks and ruby lips' to the fairest and most delicate beautiful white lady."<sup>48</sup> Similarly we see a strategy of universalization in the rejection, early in the twentieth century, of the very concept of "tropical" diseases, which scientists in the newly emerging discipline of "tropical medicine" attributed to tropical places and "tropical" (racially dark or racially mixed) peoples.<sup>49</sup> Some Latin American physicians removed the stigma of tropicalization by embracing a "universal" theory of disease causation, namely germ theory. A racial theory of illness was thereby replaced by a nonracial theory of disease etiology.<sup>50</sup>

A last category we want to discuss is one that, theoretically at least, moves the writer to a position of greatest distance from the terms of the dominant discourse. This category involves the creation of an "alternative ideology" that serves to place the minority writer outside the terms of the discourse of scientific racism. Such an alternative ideology functions as (and produces) a genuine oppositional discourse or set of discourses, but this is accomplished not through the appropriation and reassemblage of elements of the existing science, but by positing a radically different world view, with different perceptions of reality, goals, and points of reference. It effectively dissolves the relevance of the stereotyping discourse of science, by conceptualizing the issue of human variation in different, and essentially egalitarian, terms.

It is interesting to ask whether the individuals and groups in the period we have been examining could have created such alternatives to science, and thereby "stepped outside" the discourse and ignored science's claims about hu-

man difference and inequality. Today such alternatives seem attractive and even possible. In our so-called postmodern world, many of the projects of modernism are under attack. Among them is science itself, and its epistemology. Thomas Kuhn's investigations into scientific paradigms, the "strong program" in the social construction of science, the study of the fundamental metaphors that structure science, the feminist examination of gender in the development of scientific epistemology—all of these themes are part of the postmodern reevaluation of science, and all cast doubt on the claims of science to be a value-free, apolitical, universal, or purely empirical form of knowledge. The consensus that once underlay the scientific enterprise has gone, and questions arise about the relativity of scientific knowledge, the fundamentally political character of science, the problems of its rationalistic claims. The fragmentation of the consensus surrounding scientific positivism, a fragmentation that is itself a product of political and social changes since the 1950s, has in turn created political and intellectual possibilities for seeing science in new ways and of imagining a science put to different political ends.

In the period between 1870 and 1920, however, the consensus about science was relatively strong. As we noted earlier, until the middle of the nineteenth century, several world views and discourses about human identity, difference, and destiny competed for cultural and political authority. Theological and political discourses, for example, rivaled science for legitimacy, and potentially offered alternative world views for the representation of human variation along lines different from that of the new scientific racism. By the middle of the century, however, science replaced other discourses in the authoritative representation of the facts of nature. As science acquired status as the preeminently empirical, value-free, and objective type of knowledge, its power to settle political issues concerning "nature" was increased. Theological, ethical, and political approaches to what scientists now presented as problems of mere empirical nature were reduced in their authority. Thus scientists effectively removed from science the most powerful defense against scientific racism and sexism—namely, the assertion that individual and group variation made no difference to issues of justice and rights. As Douglass had said, rights were grounded not in putatively neutral "facts" of different natures, but on other, political and moral, principles.

This is not to say that men and women stereotyped as different and inferior in the sciences of the late nineteenth century, and denied equality and fair treatment on the grounds of their "natures," stopped talking about and fighting for rights—for equal treatment in the law, for political representation, or the right to education. The politically motivated and biased characters of the charges made against them by supposedly "objective" scientists were often intensely felt by the stereotyped. Even as minorities at the turn of the century pinned their hopes on the possibilities inherent in the sciences for uncovering the truth about themselves, many of them saw clearly that racial science violated the standards of neutrality, and that scientists' ideas were fundamentally marked by the relations of power between the dominating and the dominated. The African-American

physician, monogenist, and trenchant critic of scientific racism C. V. Roman, in 1916 (quoting the French journalist, Jean Finot) wrote: "The science of inequality is emphatically a science of white people. It is they who have invented it, and set it agoing, who have maintained, cherished, and propagated it, thanks to their observations and their deductions."<sup>31</sup>

To the mainstream scientists, however, such criticisms were contaminated by their language of engagement and passion, and by their source in the stereotyped or injured parties. By the norms of modern science, passion was taken to be an inappropriate stance in relation to nature and scientific argument. Science called for detachment, neutrality, a depoliticization of the terms of the debate, and the achievement of the suitably scholarly tone—even in matters of such extreme urgency and political consequence as one's own individuality and meaning. This meant, therefore, that when the stereotyped engaged in the scientific study of difference, they too found it difficult to introduce the question asked by Douglass—what difference does difference make?—and to transform the discourses of science into ones of morality and politics.<sup>32</sup>

What we are arguing is that in the era of the successful establishment of science as a epistemologically neutral and instrumentally successful form of knowledge, standing "outside of" or ignoring science was very difficult. With the triumph of "positive" science and positivist ideology, even those with the most radical intellectual and political philosophies and programs—alternative world views—tended to exempt science from their criticisms and to posit science as the one positive form of knowledge that escaped contamination from political and personal factors. A hard-hitting critique of science, as itself a political form of knowledge reflective of power, was, relatively speaking, absent.<sup>33</sup>

Operating within the circle of science, expressing themselves in the terms of the debate set by science, the stereotyped groups' best defense in the heyday of scientific racism was, as we have seen, to use science to point out the errors, the questionable data, and the specious logic of the discourses on human difference and inequality. The dominant discourse of race within science was not without its contradictions and fractures; it did not and could not wholly determine how individuals saw themselves. Even within the confining space and languages of science, Afro-Americans and Jews found opportunities to represent themselves in alternative yet "scientific" terms. In acting as scientists and intellectuals, in insisting on gaining a foothold within the scientific academy, even if on "separate and unequal" terms, they kept open and contested arenas of knowledge that otherwise might have become completely "naturalized" and uncontested.

It is true that the challenges mounted by the minorities we have examined here often made relatively little difference to mainstream science. The white, male academy usually ignored the contributions of minorities to the sciences of themselves. For instance, only one of W. E. B. Du Bois's many articles on the sociology of the African-American was published in the journal of the new profession of sociology, *The American Journal of Sociology*, even during the time when the journal was actively involved in the discussion of race (and even though its editors shared to some degree the antiracist outlook of Du Bois).<sup>34</sup> Similarly,

Jewish scientific writing on the Jewish race and self-identity was more important to Jews themselves than to the establishment circles of science.

But despite the apparently marginal status of the counterwriting produced by Afro-Americans and Jews within scientific racism before the 1930s, their efforts were not without significance. Their work created modes of representation and knowledge essential for the stereotyped themselves. Swedish sociologist Gunnar Myrdal, for instance, in his massive study of race in America in the late 1930s undertaken very much in the spirit of an objective and scientific investigation, found that he was able to call upon a long tradition of scientific work produced by African-American physicians, educators, and social scientists about their own identity, meaning, and status. Myrdal remarked upon the environmentalist emphasis found in these black scientific studies of the Negro, in contrast to the innatist tendencies of the white academy. That emphasis gave black writing, he said, a much more modern tone than white writings of the same period. Myrdal also stressed that black intellectual resistance to the dogma of Negro racial inferiority had a significant impact on the black community itself. By being widely disseminated in the black press, their counterformulation of the issue of race had emerged as a fundamental belief of all black communities, he said, allowing them to refuse to accommodate themselves to white beliefs.<sup>3</sup> The history of resistance to dominant scientific discourses of inequality suggests that, for the stereotyped, challenges to intellectual discourses of difference and inequality were indispensable to their psychological survival and political action.

## NOTES

1. On scientific racism, see Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800-1960* (London, 1982); George Mosse, *Toward the Final Solution: A History of European Racism* (New York, 1981); and Stephen Jay Gould, *The Mismeasure of Man* (New York, 1981).

2. For an interesting discussion of how ideological-linguistic formulations of race have material consequences, see K. J. Anderson, "Cultural Hegemony and the Race-Definition Process in Chinatown, Vancouver: 1880-1980," *Environment and Planning: Science and Society* 6 (1988):127-49.

3. Before 1850 there was an active tradition of scientific antiracism, albeit one with its own racialist condescensions. See Stepan, *The Idea of Race*, chaps. 1-2. After 1850, critics of scientific racism and/or sexism from within science were often themselves in one way or another members of marginal groups (for example, Franz Boas in the United States was Jewish). As for the "technical" disagreements about race, many disputes arose among scientists about which measurement accurately demonstrated the essential differences between human races—facial angle, cephalic index, overall brain weight, brain volume, or some other index. These disputes generated a large literature, but the fundamental principle underlying the science of race difference—that some measurement would indeed reveal categorically the essential racial types—was generally accepted.

4. As an example, one could contrast the caution, care, and argumentative skill with

which Darwin defended his general position on the evolution of species by natural selection, with the broad assumptions and lack of caution with which he discussed male and female differences in the human species in the *Descent of Man*. On this point see Susan Mosedale, "Science Corrupted: Victorian Biologists Consider the 'Woman Question,'" *Journal of the History of Biology* 11 (1978): 1-55; and Evellen Richards, "Darwin and the Descent of Woman," in *The Wider Domain of Evolutionary Thought*, ed. D. Oldroyd and I. Langham (Dordrecht, Holland, 1983), pp. 57-111.

5. Margaret W. Rossiter, *Women Scientists in America: Struggles and Strategies to 1940* (Baltimore, 1982). Our focus in this paper is not on the many groups categorized as "inferior" races but on those few individuals with access to education or scientific writings who challenged the claims about their "race."

6. Gramsci's *Prison Notebooks* has many striking passages: see Michael Walzer, "The Ambiguous Legacy of Antonio Gramsci," *Dissent* 35 (Fall 1988): 444-56. Also valuable is Edward Said's work on Orientalism, especially his comments in "Orientalism Reconsidered," in *Cultural Critique* 1 (Fall 1985): 89-108, and his introduction in Said and Christopher Hitchens, eds., *Blaming the Victims: Spurious Scholarship and the Palestinian Question* (London, 1988), pp. 1-19. See also the two issues on the theme "The Nature and Context of Minority Discourse," in *Cultural Critique* 6 (Spring 1987) and 7 (Fall 1987); Gayatri Chakravorty Spivak, *In Other Worlds: Essays in Cultural Politics* (New York, 1987); and Henry Louis Gates, Jr., *Figures in Black: Words, Signs, and the "Racial Self"* (New York, 1987).

7. See the reflections of Joan Wallach Scott, in *Gender and the Politics of History* (New York, 1988); Teresa de Lauretis, "The Technology of Gender," in *Technologies of Gender* (Bloomington, Ind., 1987), pp. 1-30.

8. Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford, 1981), p. 32.

9. The relation of gender to the emergence of modern scientific epistemology has been explored by a number of historians. See Carolyn Merchant, *The Death of Nature: Women, Ecology and the Scientific Revolution* (San Francisco, 1980); and Londa Schiebinger, *The Mind Has No Sex? Women in the Origins of Modern Science* (Cambridge, 1989).

10. In Britain, science was represented as a "gentlemen's" pursuit (hence the title of Morrell and Thackray's book). Class and gender exclusions from science followed from the social structure of science. The long persistence of the "amateur" in science—one consequence of the gentlemanly and aristocratic nature of British high culture—made British science organizationally distinctive and postponed professionalization. For American science, see Alexandra Oleson and John Voss, *The Organization of Knowledge in Modern America, 1860-1920* (Baltimore, 1979); for aspects of the new epistemology in Vienna, see Erna Lesky, *Die Wiener medizinische Schule im 19. Jahrhundert* (Graz, 1965).

11. Boundary setting in areas of knowledge other than science was not dissimilar—between "high" and "low," elite and popular, the canonical and noncanonical in literature and the visual arts, for example. An especially interesting analysis is Andreas Huyssen's "Mass Culture as Woman: Modernism's Other," in *After the Great Divide: Modernism, Mass Culture, Postmodernism* (Bloomington, Ind., 1986), chap. 3.

12. Gyorgy Markus, "Why is There No Hermeneutics of Natural Sciences? Some Preliminary Theses," *Science in Context* 1 (March 1987): 5-51. In a very different vein, see Mary Louise Pratt, "The Face of the Country," *Critical Inquiry* 12 (Autumn 1985): 119-43, on the emergence of the informational, depersonalized, scientific form of travel literature as a new genre of the late eighteenth century, a form whose lack of vivid and personalized narrative was resisted by the continued use of older forms of travel literature as heroic adventures of personal and geographical discovery.

13. The neutral, universal forms of science disguise the way science is nevertheless grounded on the metaphors and practices of racism and sexism. See for example Luce Irigaray's "Is the Subject of Science Sexed?" *Cultural Critique* 1 (Fall 1985): 73-88.

14. See Gillian Beer, *Darwin's Plots: Evolutionary Narrative in Darwin, George Eliot, and*

*Nineteenth-Century Fiction* (London, 1983); Robert M. Young, *Darwin's Metaphor: Nature's Place in Victorian Culture* (Cambridge, 1985); Ted Benton, "Social Darwinism and Social-Darwinism in Germany: 1860-1900," *Rivista di Filosofia* 73 (1982): 79-121.

15. The speech was a commencement address before the literary societies of Western Reserve College (Rochester, N.Y., 1854). Reprinted in Philip S. Foner, ed., *The Life and Writings of Frederick Douglass*, 5 vols. (New York, 1975), 2:289-309.

16. Martin R. Delany, *Principia of Ethnology: The Origin of Races and Color, with an Archeological Compendium of Ethiopian and Egyptian Civilization from Years of Careful Examination and Enquiry* (Philadelphia, 1879). A great deal could be said about the survival of older cultural scientific forms among groups who needed to resist the growing racialism and the narrowing of the languages within which racialism was expressed in the transition period from about 1830 to 1930. For example, Casely Hayford's *Ethiopia Unbound: Studies in Race Emancipation* (1911), the first African novel, fused fiction with factual "studies" to create a tale of black emancipation and power. Other uses of monogenism by blacks were less religious in tone. A powerful statement was made by C. V. Roman, *American Civilization and the Negro* (Freeport, N.Y., 1971, rpt. of 1916 ed.); the chapter "Racial Differences" engaged directly in rebutting racialism and Roman used the scientific argument for monogenism as the necessary and sufficient foundation for political and moral equality. The survival of traditional environmentalist explanations of difference, dating back to the Enlightenment, is also worth exploring.

17. "A Word for the 'Smith' Family," *Anglo-African Magazine* 2 (March 1860): 77-83. In addition to its irony, the article is interesting for the accurate account of the main points of Darwin's themes in relation to human beings, and for the association McCune Smith makes between Darwinism and monogenism. Within a few years, Darwinism would be successfully integrated into scientific polygenism and put to the service of disunity and inequality. Black writers often resisted Darwinism for this reason, and because it destroyed the religious basis of ethnology and anthropology by stressing humankind's common heritage with animals.

18. For an interesting account of the uses of parody as a strategy of resistance to medical authority and expertise, as well as the limits of this strategy in the face of increasing success and professionalization of science, see Judith R. Walkowitz, "Science and Seance: Transgressions of Gender and Genre in Late Victorian London," *Representations* 22 (Spring, 1988):3-29.

19. Lois N. Magner, "Women and the Scientific Idiom: Textual Episodes from Wollstonecraft, Fuller, Gilman and Firestone," *Signs* 4 (1986):61-80; Adrian Desmond, "Artisan Resistance and Evolution in Britain, 1819-1848," *Osiris*, 2d ser., 3 (1987):77-110.

20. As we show later, the use of idioms of science usually associated with, or carrying, racist meanings, such as eugenics, demanded a huge effort of transformation of meanings. Some examples of the use of the language of eugenics by African-Americans can be found in the black *Journal of the National Medical Association (JNMA)*; see for example, Dr. John A. Kenney, "Eugenics and the School Teacher," *JNMA* 7.4 (1915):253-59; Dr. Barnett M. Rhetta, "A Plea for the Lives of the Unborn," *JNMA* 7.3 (1915):200-205; C. V. Roman, "Some Ramifications of the Sexual Impulse," *JNMA* 12.4 (1920):14-17. In nearly every case, African-American doctors accepted the importance of good heredity in health and emphasized the need for care in reproduction. They called this "eugenics." At the same time, they rejected the antiblack and sterilization themes characteristic of white eugenics. Many women and Jews also embraced the themes and conclusions of eugenics.

21. For an example, see *The Health and Physique of the Negro American*, ed. W. E. Burghardt Du Bois (Atlanta, 1906), especially its plates of "Typical Negro-Americans" prefacing the text.

22. For valuable work on the problem of how dominant discourses in literature structure minority writing and the degree to which the Jewish, female, and black literary traditions represent implicit or explicit forms of resistance to the dominant discourse, see Sandra M. Gilbert and Susan Gubar, *The Madwoman in the Attic: The Woman Writer and*

*the Nineteenth-Century Literary Imagination* (New Haven, Conn., 1979); Mary Poovey, *The Proper Lady and the Woman Writer: Ideology as Style in the Works of Mary Wollstonecraft, Mary Shelley and Jane Austen* (Chicago, 1984); Deirdre David, *Intellectual Women and Victorian Patriarchy* (Ithaca, N.Y., 1987); Henry Louis Gates, Jr., *Figures in Black*; and Sander L. Gilman, *Jewish Self-Hatred: Anti-Semitism and the Hidden Language of the Jews* (Baltimore, 1986).

23. A sign of this separation was the translation of increasingly specialized knowledge of great cultural, symbolic, and material power into more accessible forms for the general public. Journals such as *Nature* and *Popular Science Monthly* were established to carry out this task of translation, initially by scientists themselves. The social circumstances in which successful attacks on scientific racism and sexism could be made also need to be studied in far greater detail. When disciplinary structures are not stabilized, or where new fields come into being that are not fully professionalized, the opportunities for minorities in science are increased.

24. The defensive tone characteristic of critical writing by minorities is explained by the high barriers to entry science had erected. See Evelyn Fox Keller, "The Gender/Science System: or, Is Sex to Gender as Nature Is to Science?" *Hypatia* 2 (Fall 1987): 37-49.

25. This problem is addressed, however, by feminists writing on science today. As Rossiter (*Woman Scientists*) and others have noted, accepting the prevailing ideology of science as unmarked by gender did not protect women and/or minorities in science from discrimination and exclusion. Instead, it only perpetuated and masked the material conditions of that exclusion.

26. According to Willie Pearson, about 950 African-Americans earned doctorates in the natural sciences between 1876 and 1976; the percentage of all natural scientists who are African-American has remained the same for over a century, namely 1 percent. About 1 percent of all PhDs in the social sciences in the United States are African-American. See Willie Pearson, Jr., *Black Scientists, White Society and Colorless Science: A Study of Universalism in American Science* (Millwood, N.Y., 1985), pp. 31-33. For the size of the African-American medical population in the period under study, see Todd Savitt, "Entering a White Profession," *Bull. Hist. Med.* (1987):516. By 1920, African-American physicians represented 2.7 percent of all doctors, compared to only 0.9 percent in 1890. European medical schools opened to Jewish males at the very beginning of the emancipation of the Jews, with the Act of Tolerance of 1782 promulgated by Joseph II of Austria. Jews tended to be clustered in the medical specialties with the lowest status (for example, syphilology, dermatology, and so on); when Ferdinand von Hebra took over the dermatological clinic in Vienna in the 1860s, he was able to recruit only Jewish assistants. See Monika Richarz, *Der Eintritt der Juden in die akademischen Berufe* (Tübingen, 1974), pp. 28-43; and Erna Lesky, *Die Wiener medizinische Schule im 19. Jahrhundert* (Graz, 1978).

27. Quoted in Barbara Johnson, *A World of Difference* (Baltimore, 1987), pp. 1-2.

28. Obviously, the social differences fracturing race (and gender) affected the emphases within the dominant discourse of "natural" difference, as it was applied to particular groups. Thus both black males and Jewish males were described in nineteenth-century anthropological and medical science as "female" in traits and character. How African-Americans and Jews supposedly manifested this femaleness was distinctive. Any study of the reactions to racial science must be alert to the nuances and emphases within the scientific stereotypes, since the responses were fundamentally structured by them, echoing, commenting upon, and modifying the specifics of particular themes. See Desmond ("Artisan Resistance") for the use of the word "cannibalization" to mean the selective use of elements of science whereby "stolen fragments were . . . re-constituted" in such a way as to legitimate an alternative program.

29. George W. Parker, "The Negroid Line in History," *American Methodist Episcopal Review* 25 (October 1908):28.

30. See Gilman, *Jewish Self-Hatred*.

31. Otto Weininger's *Sex and Character* appeared in English translation in 1906. Weininger conceived of both sex and racial character as a continuum, qualities of maleness and femaleness were, therefore, not absolutes, and both aspects could be found in an individual. Nevertheless, the extreme end of the continuum of sex, the female, was postulated as unchangeable and negative. Freud was one scientist who acknowledged the basic validity of Weininger's concept of bisexuality. See Peter Heller, "A Quarrel over Bisexuality," in *The Turn of the Century: German Literature and Art, 1890-1915*, ed. Gerald Chapple and Hans H. Schulte (Bonn, 1981), pp. 87-116.

32. William H. Thomas, *The American Negro: What He Was, What He Is, and What He May Become: A Critical and Practical Discussion* (New York, 1901). Chapter 5, "Characteristic Traits," is of special interest. Kelly Miller, "Review of W. Hannibal Thomas' Book 'The American Negro,'" *Hampton Negro Conference* 5 (July 1901):64-74; the quotations are from pp. 64-65. See also Du Bois's review in *Book Reviews by W. E. B. Du Bois*, comp. and ed. Herbert Aptheker (Millwood, N.Y., 1977), pp. 1-3. Other reviews were by Charles W. Chestnutt, "A Defamer of His Race," *The Critic* (April 1901):350-51; Charles T. Walker, "Reply to William H. Thomas: The Twentieth Century Slander of the Negro Race—Address" (New York, n.d.).

33. Thomas, *American Negro*, p. xxiii.

34. Gates, *Figures in Black*, emphasizes the reactive and negative side of such reversals, and the bizarre stereotypes that can result.

35. George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817-1914* (New York, 1971), pp. 97ff. August Meier, *Negro Thought in America, 1880-1915: Racial Ideologies in the Age of Booker T. Washington* (Ann Arbor, Mich., 1966), pp. 22ff.

36. W. E. B. Du Bois, "The Conservation of Races" (1897), in Andrew G. Paschal, ed., *A W. E. B. Du Bois Reader* (New York, 1971), pp. 19-31. An earlier statement of the theme is Du Bois's Harvard Address of 1890, where he posited the African-American as the opposite race to the Teuton, and suggested that the Teuton needed the more submissive Negro for the full development of civilization. See his *Against Racism: Unpublished Essays, Papers, Addresses, 1887-1961*, ed. Herbert Aptheker (Amherst, Mass., 1985), p. 16.

37. Anthony Appiah, "The Uncompleted Argument: Du Bois and the Illusion of Race," *Critical Inquiry* 12 (Autumn 1985):21-35. A similar example is provided by Kelly Miller's "The Artistic Gifts of the Negro," in his *Race Adjustment: Essays on the Negro* (New York, 1908), pp. 246-57. The political context of Du Bois's embrace of biology was not only scientific racism, but the need to oppose Booker T. Washington's accommodationism. In a period in which there was much discussion of "pure races," of race sentiment as binding nations together, of the need to preserve the integrity of race, it is not surprising to find many minorities embracing for themselves these notions. Du Bois's "biologism" was exceptional in his overall endeavor; he is generally known for his steadfast opposition to biological racism. Other strategies employed by Du Bois in countering scientific racism are discussed later.

38. B. F. Lee, "Selection, Environment and the Negro's Future," *AME Church Review* 20.4 (1904):388-90. The rewriting of African history, from one of nullity or "blank darkness," to use Christopher Miller's arresting title (*Blank Darkness: Africanist Discourse in French* [Chicago, 1985]), to a densely filled history of African civilization, is another aspect of the process of reversals.

39. See Gilman, *Jewish Self-Hatred*, p. 291.

40. Fredrick L. Hoffman's book was published for the American Economic Association. Hoffman, *Race Traits and Tendencies of the American Negro* (New York, 1896). A short variation of this tract appeared earlier as "The Vital Statistics of the Negro," *The Arena* 24 (1892):529-42.

41. Kelly Miller, "A Review of Hoffman's *Race Traits and Tendencies of the American Negro*," *The American Negro Academy, Occasional Papers, No. 1* (Washington, D.C., 1897),

pp. 3-36

42. Alfred A. Moss, Jr., *The American Negro Academy: Voice of the Talented Tenth* (Baton Rouge, 1981), gives a detailed account of this institution.

43. Miller, "Review of Hoffman's *Race Traits*," p. 36.

44. See Du Bois, *Health and Physique*. Du Bois "singlehandedly initiated serious research on blacks in America," says Elliot Rudwick, in "W. E. B. Du Bois as Sociologist," in *Black Sociologists: Historical and Contemporary Perspectives*, ed. James E. Blackwell and Morris Janowitz (Chicago, 1974), p. 46.

45. Du Bois, *Health and Physique*, p. 89. Several white scientists contributed data to the Atlanta University publications. Du Bois revised drastically the themes of African-American difference put forward in the Atlanta University publication no. 2, which had been published in 1897 before Du Bois became director of the conferences and publications on the Negro. In the second publication, the excessive Negro mortality and morbidity were attributed not to the environment but to ignorance of health laws and immorality, a theme that reflected the self-help outlook of the period.

46. Raphael Becker, *Die jüdische Nervosität: Ihre Art, Entstehung und Bekämpfung* (Zurich, 1918).

47. The noted Swedish sociologist Gunnar Myrdal, in studying the problem of American racism and its grounding in the idioms of science, gave pride of place to the new social sciences in dismantling racism, and noted that from the beginning black social scientists took the stand that the American dogma of race inequality was a scientific falsehood. See Gunnar Myrdal, *An American Dilemma: The Negro Problem and Modern Democracy*, 2 vols. (New York, repr. 1972), 1:93. Myrdal's emphasis on the liberating effects of the social sciences reflects the time in which he was writing (the late 1930s and 1940s); he could not foresee how many of the stereotypes of race would be reformulated, following the collapse of the older biological discourse of racism, in the new language of sociology, cultural and social anthropology, and developmental political science.

48. Delany, *Principia of Ethnology*, p. 23, Delany's italics.

49. See Thomas E. Skidmore, *Black into White: Race and Nationality in Brazilian Thought* (New York, 1974), p. 183.

50. See Nancy Stepan, *Beginnings of Brazilian Science: Oswaldo Cruz, Medical Research, and Policy, 1880-1920* (New York, 1976), pp. 57-58.

51. C. V. Roman, *American Civilization and the Negro* (Philadelphia, 1916), p. 149. Jean Finot, a naturalized Frenchman of Polish birth, wrote two of the more remarkable anti-racist and antisexist books of the late nineteenth and early twentieth centuries: *Race Prejudice*, trans. Florence Wade-Evans (Miami, 1969, rpr. of Eng. ed. of 1906), and *Problems of the Sexes*, trans. Mary J. Safford (New York, 1913).

52. This difficulty remains within the field of sex differences. Usually the argument about sex or gender difference is couched in terms of "the actual facts of difference," whether the issue is brain anatomy or function, hormonal or reproductive differences, or differences in psychic or mental operations. What is rarely broached within science is why apparently small differences between the sexes (often so small they remain disputed matters to this day) are nevertheless seen as neutral answers to complex problems of modern social life.

53. For example, Marxists treated science as an unproblematical and highly desirable feature of modern society. The most important exceptions to the relative dearth of strong criticism of the political agendas of science were the socialist scientists, who maintained that science was part of a complex political and social system.

54. Challenges to scientific racism rarely came from within mainstream science, or from those individuals whose own status was most closely tied to science. Resistance came from the marginalized and works critical of biological determinism were rarely accepted by well-known publishers of scientific books; critics rarely held chairs of science in universities; and works of criticism were rarely reviewed seriously except by the stereotyped. On Du Bois, see Blackwell and Janowitz, *Black Sociologists*, pp. 50-52.

55. Myrdal, *American Dilemma*, 1:96.