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## PREFACE

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IT IS IMPORTANT TO NOTE at the outset that this book is incompletely described by its title. Inclusion in the title of all the appropriate qualifications and elaborations would not only have horrified our publisher but would have presented the book designer with an almost impossible task in fitting the title onto the cover. As the reader will discover, the "race differences" we discuss are largely differences between various racial-ethnic groups in the United States, and "intelligence" mostly refers to performance on conventional intelligence tests. Furthermore, our discussion of this topic is chiefly focused on the question of the relative influences of the genes and the environment on such differences, and on some of the social implications of different answers to this question. We have, however, included in appendices some material that extends the discussion beyond the limited focus of the text.

One might well wonder why any behavioral scientist of good sense would willingly, or even reluctantly, become involved in the tangled morass of data, methods, ideologies, and emotions that currently surrounds the question of the relative importance of genetic and environmental variations in accounting for racial-ethnic IQ differences. In this case, it was not one behavioral scientist but three, all of whom generally consider themselves rational. Partly in defense of our reasonableness, but primarily to keep the historical record complete, we think it appropriate to recount some of the events that led us to embark on this project, as well as to outline the manner in which we attempted to carry it out.

In an important sense, this book may be considered a lineal descendant of Robert Sessions Woodworth's 1941 monograph *Heredity and Environment: A Critical Survey of Recently Published Material on Twins and Foster Children*. Not only were both books prepared under the auspices of the Social Science Research Council, but both deal with heredity and environment in relation to intellectual performance. However, this book had its own particular history. At the time this project was conceived, the National Academy of Sciences was finding the topic of race differences a rather slippery one and had rejected a recommendation from one of its own committees that some sort of review of the subject be prepared. A member of that committee, Ernest R. Hilgard, discussed with O. Meredith Wilson the possibility of the Center for Advanced Study in the Behavioral Sciences serving as the host for a small group of invited scientists who would do the task that the Academy had declined. The plan was eventually proposed to one of us (Lindzey) who was active in the affairs of the Social Science Research Council. The question was subsequently brought before the Committee on Problems and Policy of SSRC, where the general importance of a factual and objective survey of existing evidence bearing on the issue was agreed on, and a decision made to refer the matter to the Committee on Biological Bases of Behavior, as the existing SSRC committee that was best qualified to make further recommendations. Lindzey was currently a member of this committee and both he and Spuhler had served on an earlier version of the committee, known as the Committee on Genetics and Behavior.

The Committee on Biological Bases of Behavior agreed that the project was worthwhile and should be pursued if appropriate scholars could be found to prepare the report. Lindzey had planned to take a leave of absence the following year and he agreed to participate in

the project if suitable collaborators could be located. He and Spuhler had previously collaborated on an essay on racial differences in behavior, and Spuhler's background in physical anthropology and human biology provided essential additional competence. Loehlin and Lindzey had been colleagues at the University of Texas for a number of years, and although both were psychologists interested in behavior genetics, their backgrounds were distinctively different, with Loehlin possessing quantitative skills very much needed for the proposed study. It was finally agreed that the project would be undertaken under the auspices of SSRC's Committee on Biological Bases of Social Behavior with the primary participation of Spuhler, Loehlin, and Lindzey, assuming that adequate funding could be found to provide the three authors with some released time from academic responsibilities. Given this plan, the Center for Advanced Study in the Behavioral Sciences generously extended an invitation to the three participants to spend the 1971-72 academic year at the Center.

At this point, Henry W. Riecken (who was then President of the Social Science Research Council) and Lindzey set out upon a search for project funds that, rather to their surprise (given the manifest current interest in the problem and the frequent professions of the need for an objective analysis of the relevant data), proved to be quite elusive. There seemed to be little or no interest in the proposal on the part of federal agencies (not altogether surprising) or private foundations (somewhat more surprising). Some even suggested they would only be interested if we were willing to specify in advance just what the conclusions of the study would be! The one exception to this rather dreary set of interactions was the U.S. Office of Child Development and, specifically, Professor Edward F. Zigler of Yale University, who was then Director of the Office. He was consistently encouraging, and his agency eventually provided major support for the project. Additional financial aid was provided by the University of Texas, and by the National Science Foundation, through its support of the Project on Science, Technology and Society that was administered by the Center for Advanced Study in the Behavioral Sciences.

During the year at the Center our first step was to arrange for an Advisory Board that included representatives of relevant scientific specialties, representatives of the minority groups covered by the study, and persons who were familiar with public policy decisions and the role that scientific data might play in such decisions. This Board consisted of Anne Anastasi, Judge David L. Bazelon, William

Bevan, Marvin Bressler, James E. Cheek, Kenneth B. Clark, Carleton S. Coon, James F. Crow, Theodosius Dobzhansky, Richard A. Goldsby, Roger W. Heyns, Ernest R. Hilgard, C. C. Li, Alfonso Ortiz, Manuel Ramirez III, Curt Stern, Charles W. Thomas, John W. Tukey, Sherwood L. Washburn, and Dael Wolfe.

The members of the Advisory Board were informed of our general plan for the monograph, its outline, and the steps we planned to take in preparing it, and were invited to respond to this information in any manner they chose: later, they were sent the draft manuscript for their comments and criticism. We are deeply grateful to all the members of the Board for their support and cooperation and we would like to express particular gratitude to Professors Anastasi, Bevan, Crow, Dobzhansky, Hilgard, Ramirez, Stern, Tukey, and Wolfe whose detailed and specific comments on the manuscript were extremely helpful to us in arriving at a final version.

In addition to arranging for the Advisory Board, we were especially fortunate, early in our planning and study, in being able to meet personally and consult with a number of social scientists who in addition to their professional and academic credentials possessed the unique perspective gained from being members of U.S. minorities. These consultants included Edward J. Casavantes, then with the United States Commission on Civil Rights and now at California State University, Sacramento; William Hayes and William D. Pierce, West Side Mental Health Center, San Francisco; Frank L. Morris, the Russell Sage Foundation; Walter L. Wallace, Princeton University; and Robert L. Williams, Washington University. In recognizing here their helpful contributions to our own education, we do not, of course, mean to imply an endorsement on their part of either our project or its final product, this book. With each of these individuals we discussed our general plans, the proposed chapter outline of the book, and various sources of data that might be important to consider. We also reviewed a list of what we considered to be major issues in the area and even discussed whether the project should be pursued further at all—our consultants, like potential funding sources, were by no means of one mind on this question. Each of the consultants was also later sent a draft of the manuscript and invited to make suggestions for revision.

Not only were we aided by a Board of Advisors and a series of consultants but we also attempted to capitalize on the unusual and diverse talents of our fellow Fellows at the Center. There were some

general seminars early in the year in which we discussed key issues with our interdisciplinary colleagues, and later a number of smaller meetings and informal discussions with Fellows who were particularly interested in one aspect or another of the project. This provided a valuable opportunity for interacting with highly trained scholars whose disciplines ranged from the humanities to the biophysical sciences, although the majority were from the behavioral sciences. Among the Fellows we are particularly indebted to for helpful discussions concerning the project were Lee Cronbach, David Dunelski, Nathan Glazer, Robert Hodge, George Lakoff, Joshua Lederberg, Robert LeVine, Robert Nozick, Henry W. Riecken, and David Wiley. We are particularly grateful to O. Meredith Wilson and his staff at the Center for having provided advice as well as a remarkably attractive and stimulating setting for our work.

Finally, we sent the draft manuscript, in two installments, to some fifty additional colleagues in the biological and behavioral sciences, inviting their comments and criticisms. Quite a few responded with extended critiques and suggestions. Others commented more briefly on matters of special interest to them. In a few cases we were fortunate in being able to discuss the manuscript directly with these persons. Among those to whom we are particularly grateful for helpful suggestions are Joan and Stephen S. Baratz, Ned Block, Philip K. Bock, Jack Bresler, Jan Bruell, Luigi Cavalli-Sforza, Raymond B. Cattell, Bernard Davis, Ralph Mason Dreger, Otis D. Duncan, Hans J. Eysenck, Stanley M. Garn, Perry Gluckman, Irving I. Gottesman, Henry Harpending, Richard J. Herrnstein, Joseph Horn, Christopher Jencks, David Jenness, Arthur R. Jensen, Ashley Montagu, R. Travis Osborne, T. Edward Reed, Sherman Ross, H. Eldon Sutton, William Shockley, Steven G. Vandenberg, and Lee Willerman.

In identifying these various distinguished scientists and scholars, we do not, of course, mean to imply or suggest their endorsement of this book. We sent them a draft—in which all of them found at least some room for improvement. Their suggestions were of help to us in putting the draft into its final form. We do want to acknowledge that assistance, plus the encouragement that many of them provided us, but we do not wish to have them saddled with any of our views that they do not share.

In short, we wish to be quite explicit concerning the responsibility for what is contained in this volume. The statements and conclusions in the book have benefited from the wise advice and support of many

individuals and agencies, but we, the authors, are solely responsible for what is said.

We are grateful to Noel Dunivant, Jr., for his conscientious help in checking references.

The royalties from this book have been assigned to the University of Texas, the University of New Mexico, and the Center for Advanced Study in the Behavioral Sciences, to provide financial assistance for minority-group students and scholars.

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John C. Loehlin  
Gardner Lindzey  
J. N. Spuhler

Race  
Differences  
in  
Intelligence

# The Problem and Its Context

IT MAY BE THAT WHILE SPECIALISTS in the physical and natural sciences have been embroiled in debates over arms control, atomic energy, atmospheric and water pollution, supersonic transportation, and organ transplants, social scientists have had less than their share of the problems associated with the intersection of science, technology, and public policy. If so, this disparity is fast being removed by the recent eruption of scientific and public concern about individual and group differences in intelligence, their determinants, and the implications of all of this for social and political decisions. Questions concerning the meaningfulness and predictive utility of any estimates of general intelligence, the stability of such estimates, and the relative contributions of genetic and environmental factors to intelligence have remained among the most difficult and emotionally charged issues within the social sciences for more than five decades. When these questions are reexamined in the context of racial and social class

differences in a society ridden with unresolved tensions in these areas, it is not surprising that the result should be a massive polemic in which personal conviction and emotional commitment often have been more prominent than evidence or careful reasoning.

The goal of this book is to provide a sober, balanced, and scholarly examination of the evidence that bears upon the role of genetic and environmental factors in the determination of group differences in ability in the United States. Although the focus of our effort will be upon the existing evidence that bears significantly upon this question, we will also be concerned with the policy implications, if any, that such evidence may have, and also with the extent to which further research in this area should be given priority, and what varieties of such research may be most promising in terms of providing significant and clear-cut evidence.

## THE GENERAL QUESTION

There is no issue in the history of the social sciences that has proved to be quite so persistently intrusive as the question of assessing *the relative importance of biological and environmental determinants of behavior*. Indeed, an interest in the contribution of what is given as opposed to what is learned far predates the emergence of the biological and social sciences. The modern versions of the question began to emerge following the formulations of Galton and Darwin in the nineteenth century and became more sophisticated early in the twentieth, after the development of the discipline of genetics from Mendel's discoveries and the appearance of a variety of techniques for measuring aspects of behavior quantitatively.

The initial presentations of the major ideas of Darwin, Galton, and Mendel occurred in the span of a few years even though it was to be many decades before all of these ideas were brought together and integrated. Darwin's *Origin of Species*, in which he developed his theory of evolution and particularly his view of the importance of natural selection, was published in 1859. His emphasis upon the continuous and orderly development of new forms of life from other forms and the decisive role played by fitness, or reproductive advantage, was from the beginning linked to behavioral as well as physical attributes and thus directly relevant to social scientists. Galton's concern with the importance of inheritance in determining high levels of achievement was clearly influenced by the ideas of his cousin

Darwin. Galton's studies, published under the title *Hereditary Genius* in 1869, can be seen as an application of Darwin's concepts to an area of human behavior of unusual interest. Galton was able to show that among persons judged to be outstanding in their achievements the number that had biological relatives who were also judged to be outstanding vastly exceeded the number expected on the basis of chance. Galton also introduced the study of twins as a method of providing evidence concerning the relative contributions of heredity and environment. Altogether independent of the work of Darwin and Galton was the plant research of Mendel, first published in 1866, that provided the basis for the laws of segregation and independent assortment of inherited traits. But it was not until the turn of the century that the importance of Mendel's work was recognized and his two laws were suitably elaborated to serve as the foundation for the discipline of genetics. Shortly thereafter the term "gene" was applied to the basic unit that produced the regularities in inheritance that Mendel had noted. At almost the same time the label "genetics" was applied to this field of inquiry and the distinction was made between the *genotype*, the underlying biological makeup of an organism, and the *phenotype*, the external and observable aspect of the organism. What began as the study by a few persons of simple characters or attributes rapidly changed into an entire discipline that was concerned with simple and complex characters and their analysis at many different levels, ranging from molecular processes to the quantitative analysis of polygenic characters in large populations.

It was also around the turn of the century that psychologists began to show serious interest in individual differences and their measurement. The term "mental test," which was first suggested by J. McKeen Cattell (1890), provided a label for a set of diverse activities all of which were intended to identify and measure dimensions of variation among individuals that would be useful in predicting significant aspects of behavior. Initially most of these measures assessed simple sensory or sensory-motor components of behavior, but from almost the beginning of these developments there were psychologists who believed that complex and not simple processes would prove to be the key to predicting "intelligent behavior." It was this perspective that eventually led Binet and Simon, in an attempt to predict the probability of educational success, to develop the Binet-Simon Scale (1908) that in the revision by Lewis Terman (1916) became the Stanford-Binet, the most famous of all tests of intelligence. This

instrument, with its associated concept of the intelligence quotient or "IQ," pointed the way to the development of group testing of intelligence and other special abilities or aptitudes during World War I, and all of this signalled the onset of the Mental Testing Movement. The movement was marked by the vigorous development and application of a wide variety of tests in an extraordinarily diverse number of settings. Although psychological tests have not been without their detractors, even at the outset of the movement, there is little doubt that such tests represent one of the most significant technological accomplishments of the social sciences.

While mental tests were emerging and the discipline of genetics was developing, the social sciences were struggling also with the question of how to deal with the relation between genetic variation and behavior. Although social scientists had early contact with ideas of biological determination, particularly in the writings of Galton and Spencer, the social sciences as they developed in the United States demonstrated a very strong preference for formulations and findings that emphasized environmental control of behavior. A clear illustration of this is provided by the reactions to the writings of William McDougall and those of John B. Watson.

McDougall's instinct-based psychology emphasized the biological inheritance of mental characteristics:

It will help to make clear the influence of innate qualities, if, by effort of imagination, we suppose every English child to have been exchanged at birth for an infant of some other nation (say the French) during some fifty years. At the end of that period the English nation would be composed of individuals of purely French origin or blood. . . . What would be the effect? . . . gradually, we must suppose, certain changes would appear; in the course of perhaps a century there would be an appreciable assimilation of English institutions to those of France at the present day, for example, the Roman Catholic religion would gain in strength at the cost of the Protestant. (1920, p. 165)

Watson, on the other hand, strongly influenced by Pavlov's findings and theories concerning the conditioned response, took a radically environmentalist position:

Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select—a doctor, lawyer, artist, merchant-chief and, yes,

even into beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations and race of his ancestors. (1926, p. 10)

McDougall's views, after some modest contemporary popularity in the United States, ebbed rapidly in influence. By contrast, Watson's Behaviorism captured the main body of American psychology with a grip that has only begun to be loosened by the cognitive psychologies of the last decade or two and by the emergence of behavior genetics and psychobiology as interdisciplinary specialties.

Of course, most social scientists most of the time have taken positions somewhere between the hereditarian and environmentalist extremes, accepting the reality that both the genotype and the environment contribute significantly to behavioral variation, but differing in the relative weights to be assigned to the two in accounting for particular aspects of behavior. It was only in the heat of polemic, or in the hands of enterprising journalists, that the joint importance of these two factors was ever denied. Even McDougall saw the changes in England as taking place "gradually," and Watson, in the sentence immediately following the passage quoted above, hedged: "I am going beyond my facts and I admit it . . ." Nonetheless, the consensus position of American social science has always been much closer to Watson than to McDougall.

## A MORE FOCUSED QUESTION

Given the rapid development of tests of mental ability in the early part of this century and the obvious significance of mental ability for the individual and society, it is not surprising that discussions of the impact of hereditary and environmental factors upon behavior came to center on intelligence. Thus, during the 1920's and 1930's one of the most active areas of investigation in the social sciences was *nature versus nurture in relation to the IQ*. Questions concerning the relative stability of a person's IQ over time, the extent to which it may be modified through environmental manipulation, and its usefulness in predicting significant performance provided the nucleus for a series of investigations that united certain investigators (and universities) and sharply divided them from others. Many of these studies were based on twins and adopted children and this research was surrounded by acrimonious disputes that led to an influential monograph by the psychologist Robert S. Woodworth (1941), which provided a

scholarly and dispassionate discussion of the existing evidence and a clear statement of many of the underlying issues.

In recent decades, the nature-nurture issue with respect to intelligence has mostly lain quiescent, until its dramatic resurgence in 1969 in the form of the so-called "Jensen controversy." All along, a scattering of investigators in the emerging interdiscipline of behavior genetics have been addressing themselves to research on the genetic basis of human abilities. Examples of work in this country may be found in two volumes edited by Vandenberg (1965, 1968), and there has also been a vigorous tradition abroad, as exemplified by the work of Burt (1966) in Great Britain, Husén (1959) in Sweden, and others. Meanwhile, genetically oriented workers in the field of mental retardation made steady progress and even some fairly spectacular advances, as in the successful treatment of phenylketonuria (PKU), and the discovery of the chromosomal basis of Down's syndrome (formerly called "mongolism"). In general, research on abilities during this period tended to focus on individual variation rather than social class or racial differences; the authors were wary of drawing strong implications for education or other matters of pressing social concern; and for the most part little public excitement ensued. There were a few minor flurries following the publication by Audrey Shuey (1958, 1966) of a book reviewing studies of black and white differences on IQ tests in which hereditarian conclusions were reached, but the evidence was indirect, and in any case reviews with very different conclusions were available (Klineberg, 1944; Pettigrew, 1964; Dreger and Miller, 1960, 1968). So Shuey's volume on the whole did not generate a great deal of excitement. Shortly, however, the picture was to change profoundly.

## THE CONTROVERSIAL CONTEMPORARY QUESTION

It seems unlikely that there has ever been a controversy that has involved a more complex tangle of ethical, public-policy, emotional, measurement, design, and inference issues than the attempt to determine the relative contribution of genetic and environmental variation to group differences in intellectual performance. Since publication of the controversial monograph by Arthur R. Jensen (1969) the debate has been carried on in speeches, interviews, and technical journals as well as in the mass media. Far from identifying areas of clear agreement and resolvable differences, these discussions have for the most

part led to further polarization of positions and could probably be assessed fairly as not having offered much enlightenment to the genuinely interested but uncommitted observer. Those concerned with related public-policy issues have found little to guide or instruct them in making wise decisions. For every expert witness it seems possible to find another purporting to be equally expert who provides contrary advice.

Jensen's lengthy article was published by the *Harvard Educational Review* in 1969. The publication was titled "How much can we boost IQ and scholastic achievement?" and defended three major theses: (1) IQ tests measure a general-ability dimension of great social relevance; (2) individual differences on this dimension have a high degree of genetic determination (about 80 percent); and (3) educational programs have proved generally ineffective in changing the relative status of individuals and groups on this dimension. Jensen also took up Burt's (1961) suggestion that because social mobility is linked to ability, social-class differences in IQ probably have an appreciable genetic component.

All of this would undoubtedly have produced some concern and response from environmentalists, particularly those actually involved in programs of compensatory education. This potential response, however, was enormously magnified by a very small portion of the article dealing with racial differences in intelligence and concluding with the statement:

So all we are left with are various lines of evidence, no one of which is definitive alone, but which, viewed all together, make it a not unreasonable hypothesis that genetic factors are strongly implicated in the average Negro-white intelligence difference. The preponderance of the evidence is, in my opinion, less consistent with a strictly environmental hypothesis than with a genetic hypothesis, which, of course, does not exclude the influence of environment or its interaction with genetic factors. (p. 82)

Although this statement is considerably more cautious and carefully qualified than many generalizations encountered in the social science literature, the sociopolitical climate of the times in the United States was such that the remark led to a storm of protest. The *Harvard Educational Review* alone published at least a dozen rejoinders, most of them highly critical, as well as a reply by Jensen. Critical commentaries have since appeared in dozens of other publications ranging from the technical journals to mass media; among the early sharp

criticisms were articles by Hirsch (1970) and Lewontin (1970). Many of Jensen's critics questioned or denied the validity of IQ tests; some of them disputed the evidence for the high heritability of intelligence; a number pointed out the logical hazards of arguing from individual to group differences on any trait; and almost all found flaws in many of the studies cited by Jensen. In a recent book-length statement (1973a) Jensen has defended and extended his position on racial differences.

As the controversy widened, Jensen became the target of a variety of political and personal abuse, from both outside and within the academic community. A sobering account of this period is presented by Jensen in the preface to his collected writings on the topics of genetics and education (Jensen, 1972a). An article by Herrnstein (1971) dealing with genetic variation, social class, and intelligence also aroused a storm of controversy even though it said virtually nothing on the topic of racial-ethnic differences in intelligence. An expanded treatment of this topic (Herrnstein, 1973) also includes a full discussion of the personal consequences of the controversy for the author.

Soon the debate about race and IQ was taken up in Great Britain, stimulated by the publication of a book by H. J. Eysenck (1971), a distinguished British psychologist not noted for reluctance to engage in controversy. Eysenck's position in the book generally agreed with Jensen's, but Eysenck took on additional targets as well (such as the Irish). The book provoked a flurry of commentary in the British communications media, and led to a physical assault upon Eysenck as he was attempting to deliver a talk at the London School of Economics.

Predating the Jensen controversy, but to a considerable extent brought into public prominence by it, was a running argument in the United States between the Nobel laureate physicist William Shockley and his fellow members of the National Academy of Sciences. Shockley urged the Academy to take vigorous and positive action to investigate racial differences in IQ, and in particular, the possible dysgenic effect of the reproductive patterns of the black population of the United States. The Academy appointed several committees to consider Shockley's proposals and their work led to two reports (National Academy of Science, 1968, 1972) that, although acknowledging the legitimacy of research in this area, declined to consider it of special scientific urgency. A number of Shockley's attempts to present his views in public have also been associated with

disruptions or threats of disruption leading to the cancellation or termination of the speeches.

Another major debate running through this period and interweaving with the others was that emanating from the survey, published under the title *Equality of Educational Opportunity*, that had been commissioned by the Department of Health, Education, and Welfare and conducted by a team of distinguished social scientists, headed by the sociologist James Coleman. This massive study, published in 1966 and popularly referred to as the "Coleman Report," indicated that at the time of the survey systematic differences in funding and facilities between U.S. schools serving predominantly black and predominantly white populations had virtually disappeared—except for the fact of segregation itself; that average black and white differences in academic performance remained large and persistent; and that measurable characteristics of the schools appeared to have remarkably little effect on the academic performance of the students attending them. The observed differences in academic achievement between schools seemed largely to reflect differences in the attributes of the students entering them, rather than differences in the pupils' experience in the schools.

This too aroused much controversy. The Coleman Report findings were given a key role by Jensen in his argument for the importance of genetic influences in scholastic achievement. A considerable amount of debate and discussion, and reanalyses of the Coleman data—most of which tended to support the original findings—are presented in a recent volume edited by Mosteller and Moynihan (1972), and the political and social implications of the findings of the Coleman study and other related investigations are discussed at length by Christopher Jencks in his recent book, *Inequality* (1972), which seems certain to keep the argument alive.

## TECHNOLOGY ASSESSMENT AND PSYCHOLOGICAL TESTING

In recent years influential persons in both political and scientific circles have come to view assessing the broad implications for our society of technological changes as one of the most significant of all contemporary problems (National Academy of Sciences, 1969; Brooks and Bowers, 1970). Although discussions of technology assessment generally have centered on the natural sciences and engineering,

there has been occasional recognition of the potential importance of the behavioral sciences in any systematic attempt to understand fully the broad effects of new technological and scientific developments. In a few instances, such as the controversy in regard to the supersonic transport, studies have been conducted that have been aimed directly at an understanding of the psychological or behavioral consequences of introducing a technological change.

Less clearly understood has been the fact that psychology is already embarked upon a large-scale assessment of an important part of its own technology—the intelligence test. Current concern with racial-ethnic differences in measured intelligence and the relative contributions of genetic and environmental components to the observed differences is to a considerable degree rooted in an interest in the social consequences of classification and prediction on the basis of psychological tests. The attempt to understand these consequences and to control them in a socially desirable manner represents a clear example of technology assessment, and such a view of the process may serve to lessen somewhat the extremes of emotion often associated with this issue. It may be argued that even those who stand in direct opposition to each other in their interpretation of the basis for group differences in intelligence can share an interest in seeing that psychological tests are used in a more useful and sensible manner than is now often the case.

A series of legal suits brought against school boards and employers charging that psychological tests have served to discriminate against minorities or culturally deprived groups has placed the whole issue quite clearly in an assessment framework. Moreover, a Supreme Court decision (*Griggs et al. v. Duke Power Company*; see also *United States v. Georgia Power Company*) has placed definite limits on the circumstances under which psychological tests may be used appropriately and legally, and comparable questions are being raised in a variety of educational, legislative, and judicial settings throughout our nation. Indeed, the future of large-scale intelligence testing within our society remains at this juncture quite unclear.

### **SOME CENTRAL AND COMPLICATING CONSIDERATIONS**

The paragraphs that follow include discussion of some persistent misconceptions that have beclouded attempts to understand the roles

of heredity and environment in influencing group differences in behavior, as well as some matters of fact and definition central to such an analysis.

*"Genetic" does not mean "unchangeable."* There is a general confusion between the immutability, or lack of environmental responsiveness, of a trait and its degree of genetic determination. There has been a tendency among many social scientists as well as the general public to believe that when a trait is strongly influenced by genetic factors there necessarily is little possibility for environmental influence. In fact, the ability to manipulate a trait by environmental means may be greatly facilitated by understanding its genetic basis. A classic example is the low intelligence resulting from the gene-based metabolic disease phenylketonuria (PKU). If infants having the genetic constitution for the disease are fed a special diet low in the amino acid phenylalanine, the accumulation in the brain of toxic metabolic products is prevented, and their intelligence develops to a fairly normal level. Throughout this volume we will seek to remind the reader that a high level of heritability for a given trait or character is not to be automatically equated with a low level of modifiability.

*Equality is not identity.* Many believe that provision for social equity or "equal treatment" is inconsistent with accepting the existence of biological and psychological individuality. This view appears to derive from a failure to distinguish between "equal" and "identical." It is indeed the case that if two persons who differ in relevant biological or psychological properties are accorded *identical* treatment, identical outcomes would not be expected as the result. But this does not mean that there couldn't be different treatments that are *equal* in terms of some appropriate social metric (say, cost) that would when applied to these two persons yield outcomes that are equal in terms of some other appropriate social metric (prestige, say, or income). If not—if equal inputs cannot be found that yield equal outcomes—the question of whether social equity should be defined in terms of inputs or outcomes becomes moot.

*General intelligence or special abilities?* There is no broad consensus about the utility of the concept of general intelligence and, more specifically, the merit of employing one indicator of intelligence, rather than several. This topic will be explored at some length in Chapter 3 of this book. In general, we will take the position that both

the view of intelligence as a unitary dimension and as a composite of special abilities have value in appropriate contexts. We would only add here our belief that any very narrow approach to how "intelligence" (or any other trait) is to be indexed or measured, although not without certain practical advantages, can have unfortunate long-term scientific and social consequences.

*Different meanings of "race."* The concept of race is used quite differently in the biological and social sciences generally, as well as in varying senses by individual scientists. In the biological sciences, races are customarily considered as biological subspecies, defined by gene frequencies, while in demography and related fields race customarily refers to ethnic identification. An individual's race, for purposes of the U.S. Census, may be quite different from a biological classification of him. Obviously, no discipline has an inherent right to the word, but use of it in different senses can lead to misunderstanding and confusion, especially if one discipline perceives another as clumsily or maliciously misusing "its" term. In this book, to avoid ambiguity, we will usually employ "race" or "racial differences" in the sense of the biologist, as elaborated in some detail in the next chapter. For the demographer's sense of "race," we will prefer the cultural anthropologist's term "ethnic group." In neutral or ambiguous cases we will often use the hybrid term "racial-ethnic group." We do not expect to be entirely consistent in our use of the term "race"; sometimes precedence will be taken by idiomatic usage. Our title is a case in point: logically it should be "racial-ethnic differences in intelligence" but this just seemed too cumbersome.

*Disagreements about "heritability."* There are several different formal definitions of heritability, and a good deal of ambiguity concerning the extent to which heritability estimates can be generalized from one set of conditions to others. In addition, there is disagreement about whether information concerning the heritability of a given character or trait *within* one or more groups provides any information about the likelihood that genetic variation plays a role in determining differences *between* two or more groups. These topics will be discussed in Chapter 4.

*Gene-environment interactions and correlations.* There are many different types of gene-environment interactions and correlations that