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# FOCUS

on poverty research

## NATURE-NURTURE NONSENSE

Arthur R. Jensen, the author of that famous *Harvard Educational Review* article<sup>1</sup> in the winter of 1969, may be the most discussed and least read essayist since Karl Marx. Everybody knows, or thinks they know, what he said in that article: Blacks perform less well than whites on standard IQ tests. Compensatory education has not been able to narrow the gap. One reason for the failure is that IQ differences among individuals arise mostly from genetic causes. Black children are on average less intellectually able than whites. And this state of affairs is immutable.

Everyone also knows of the strident personal attacks on Jensen. Fewer people, unfortunately, are acquainted with the serious critiques of Jensen's approach to data and methodology, or with the actual evidence on the heritability of IQ. Arthur S. Goldberger is one of the critics.

### Volume I, Number 1: Spring-Summer 1976

This is the first issue of a newsletter that will have three issues a year: Spring-Summer, Fall, Winter. Its purpose is to acquaint a wide audience with the work of the Institute for Research on Poverty, by means of short essays on selected pieces of research. The articles are written by Felicity Skidmore, Coordinator of Special Projects at the Institute.

The material in any one issue is, of course, just a small sample of what is being done at the Institute. It is our hope that these summaries will whet the appetite of the reader to learn more about the research itself, and more about other research on poverty—an area of vital social concern—by Institute staff.

The views expressed are those of individual members of the Institute; they do not represent the position of the Institute for Research on Poverty, the University of Wisconsin, the Department of Health, Education, and Welfare, or other funding agencies.

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Starting out as an econometrician interested in learning something about hereditary factors in models of socioeconomic achievement, he found himself in the role of a sleuth,<sup>2</sup> as his recent research papers on the subject show.<sup>3</sup>

### Jensen's Approach to Evidence

To investigate the heritability of a trait—the proportion of the total variation in a trait that is due to heredity—one must separate the effect of genes from the effect of environment. In humans this means investigating data concerning differences and similarities among various blood relatives. But relatives tend to have environments with many common features. Because of this, the main data bases that have been used to pursue the study of IQ heritability have contrasted identical twins with fraternal twins, twins separated at birth vs. twins raised together, adopted children vs. own children.

Two collections of data have been relied upon extensively by Jensen. Goldberger's independent assessment of those data and of Jensen's use of them makes intriguing reading.

The first collection consists of IQ data for numerous categories of relatives accumulated by the late Sir Cyril Burt over his long career. Jensen used a full set of these correlations to derive his well-known estimates of IQ heritability published in *Genetics and Education*.<sup>4</sup> The most famous portion of Burt's data relate to identical twins, some of whom were reared apart and some reared together.

There are good grounds for believing that Burt's IQ correlations are bogus (as Leon Kamin documents in detail). Burt provided virtually no documentation of the tests used, of the sampling frame, of the age and sex of the subjects; nor are the summary statistics for his sample (the usual means and variances) published. Moreover, his figures for various kinship correlations contain numerous inconsistencies from one publication to another.

Burt never published the raw data on which his results were based. A table of them was, however, published by

Jensen. There is evidence that the social class assigned to these twins was changed in at least six cases *after* the data had been used by Burt as the basis for numerous published results. There is also evidence that Burt's sample size changed—not only up (which could be explained by discovering more pairs of twins) but also, on occasion, *down*.

But all this is overshadowed by what came next. Burt adjusted the IQ scores of the twins—and said so explicitly in the following fascinating passages culled by Goldberger from Burt's writings:

- It will be unwise to rely exclusively on formal [intelligence] tests of the usual type . . . the only way to be sure that no distorting influences have affected the results is to submit the marks to some competent observer who has enjoyed a first-hand knowledge of the testees.
- The interview, the use of non-verbal tests, and the information available about the child's home circumstances usually made it practicable to allow for the influence of an exceptionally favorable or unfavorable cultural environment.
- By these means we can reduce the disturbing effects of environment to relatively slight proportions.
- Nor were we concerned with a specific *observable* trait, but with differences in a hypothetical innate general factor.
- What I was discussing was not "intelligence" in the popular sense (which usually includes acquired knowledge and skill . . .), but rather the psychologist's attempts to assess the individual's "innate general ability"—a purely "hypothetical factor."

The last is the best: Burt's numbers were even described by their creator as estimated correlations of the genetic component of IQ test scores. His adjustments were directed toward eliminating "unusually" strong environmental effects on his measure of IQ. And then Jensen used the adjusted numbers to measure the relative contributions to IQ of heredity and environment!

The second collection of data comes from a 1928 study of adopted children by Barbara Burks.<sup>5</sup> She, unlike Burt, did provide adequate information of the details of her study. Her data consisted of one sample of 214 families with adopted children placed before 12 months of age and a second (control) sample of 105 families rearing their own children. The samples were matched with respect to age and sex of child, occupation of father, and type of neighborhood. The IQ of parents and children was judged on the basis of the Stanford-Binet test. The home environment of the family was measured in considerable detail.

Jensen uses the Burks study to support his contention that heredity, rather than environment, plays the predominant role in the determination of intelligence. In this instance, it

is not the quality of the original research itself that disturbed Goldberger, but rather Jensen's description and use of the study.

Some quotations from Jensen, along with Goldberger's summary of the relevant part of the Burks study, follow.

1. JENSEN: [*Burks's study was*] representative of a broad cross-section of the U.S. Caucasian population with respect to education, occupation, and socioeconomic level. It is probably safe to say that not more than 5 percent of the U.S. Caucasian population falls outside the range of environmental variation represented in the samples. GOLDBERGER: Burks's adopted children and control group children were confined to English-speaking couples residing in the San Francisco, Los Angeles, and San Diego areas. All of Burks's families were intact; that is, both parents were alive and living together. More than one-third of the adoptive children had private tutoring in music, dancing, drawing. Burks's own guess was that the environments provided by the adoptive families averaged between one-half and one standard deviation higher than the general population. For example, 7 percent of U.S. families were headed by a professional in 1930, compared with 17 percent for the Burks foster homes and 20 percent for the Burks control families.
2. JENSEN: [*Burks's measure of the environment*] included such factors as the amount of time the parents spent helping the children with their school work, the amount of time spent reading to the children, and so on. The multiple correlation (corrected for unreliability) between Burks's various environmental ratings and the adopted children's Stanford-Binet IQ was 0.42. GOLDBERGER: Burks's interviewers did ask about home instruction or attention received by the child; she tabulated the means and standard deviations for the total number of hours spent in this way; and she reported the correlation of this variable with child's IQ. She did NOT use this variable in the multiple correlations.
3. JENSEN: *Even in the case of the adopted children, the single most important environmental factor contributing to variance in children's IQ was the foster mother's intelligence.* GOLDBERGER: This is simply not true. Burks tabulated the simple correlations of some twenty environmental variables with adopted child's IQ. Among the entries are mother's vocabulary, .23; home-quality index, .21; culture index, .25; income, .23; home-ownership, .25; number of books in child's library, .32. For mother's IQ the entry is .19.
4. JENSEN: *Sewall Wright (1931) performed a heritability analysis on these parent-child and IQ-environment correlations and obtained a heritability coefficient of 0.81.* GOLDBERGER: With

one set of assumptions (in which all effects that cannot be attributed to measured environment are attributed to heredity) Wright estimated that 81 percent of the variation in IQ is attributable to variation in heredity. So far, so good. But he clearly states that this is intended as an upper bound. With a different set of assumptions (in which some effects not attributable to measured environment are allocated to genetic-environmental interactions along with unmeasured environmental influences) Wright derived, from the same Burks data, what he describes as his lower bound of heritability, .49. Throughout, environment was measured by a single index.

Jensen's characterization of the Burks study has since acquired a life of its own, as noted by Goldberger. Strikingly similar descriptions of it have been written by both Eysenck and Herrnstein.<sup>8</sup> But Herrnstein adds a new twist.

**HERRNSTEIN:** *The foster children's IQ's correlated with their natural parents' IQ's more than with their foster parents.* **GOLDBERGER:** The Burks study contains no information on the IQs of the natural parents of the foster children. Burks's research group did not meet these parents and did not test them, nor was their intelligence tested by anyone else.

### Jensen's Approach to Statistical Methodology

In the literature on the heritability of intelligence it is usually assumed that genes and environment are independent. Many researchers have questioned the appropriateness of this assumption. Jensen, therefore, has recently extended the classical twin method—a methodology that exploits the fact that identical twins have identical genes, whereas fraternal twins are no more similar genetically than ordinary siblings. His extension allows for the possibility that genes and environment are correlated.<sup>7</sup> Two things about this work should be noted:

In one paper he claims to obtain unique estimates of the variances, and covariance, of genes and environment. He sets up a model that involves (a) the genetic correlations for fraternal twins, (b) the environmental correlations for identical twins, and (c) the environmental correlation for fraternal twins. He then tells us that he solved his system for sixty combinations of values for the three correlations. He further tells us that there was only one admissible solution—which attributed 65 percent of the variance to genetic factors, 28 percent to environmental factors, and 7 percent to the covariance between the two. Goldberger finds a counterexample (which is an admissible solution by all Jensen's stated criteria) which attributes 15 percent of the variance to genes, 84 percent to the environment, and 1 percent to the covariance. He goes on to find that a wide range of estimates is admissible. As Goldberger asks, "Does Jensen's computer have a hereditarian bias? If so, is that an innate, or an acquired, trait?"

In a second piece of work with the twin method, Jensen's calculations purport to show that for a wide range of assumptions, the twin data yield estimates of heritability in the range of .50-.75. Here Goldberger points out an unstated assumption of Jensen's that must be made explicit before we can judge how relevant his conclusions are. One measure, called "the correlation between genotype and environment," in fact denotes (a) the correlation between an individual's genes and his own environment, (b) the correlation between an individual's genes and his identical twin's environment, and (c) the correlation between an individual's genes and his fraternal twin's environment—implying that Jensen assumes they are all equal. Taking (a) and (b) to be equal may be reasonable, but why should anyone assume that the correlation of an individual's genes and his fraternal twin's environment is as high as it would be for an identical twin?

To test how sensitive Jensen's results are to this implied equality, Goldberger tries relaxing it while keeping other assumptions unchanged. If the ratio of (c) to (a) is 1.0, as Jensen chooses to assume, heredity alone accounts for 72 percent of the IQ variation and environment alone for 3 percent (the rest accounted for by measurement error and covariance). If the ratio of (c) to (a) is reduced slightly, to 0.8, the variation attributable to heredity drops to 61 percent and to environment rises slightly, to 9 percent. If the ratio drops to .5—that is, the correlation of an individual's genes with his fraternal twin's environment is assumed to be half as great as for identical twins—the variation attributable to heredity alone drops to 24 percent, and that attributable to environment rises to 42 percent!

More generally, Goldberger remarks on how fruitless it must be to search for meaningful estimates of heritability by the twin method, which uses two pieces of data—the observed correlation of identical twins' IQ and the observed correlation of fraternal twins' IQ—to solve for seven unknowns:

1. Genetic correlation for fraternal twins
2. Environmental correlation for identical twins
3. Environmental correlation for fraternal twins
4. Correlation between an individual's genetic makeup and his own environment
5. Correlation between an individual's genetic makeup and his identical twin's environment
6. Correlation between an individual's genetic makeup and his fraternal twin's environment
7. Heritability

Some good has come out of all this. In 1970 responsible scholars could say, and did, that the weight of the evidence from a variety of correlations among relatives put the heritability of IQ in various human populations between .6 and .8. As a result of investigations by Goldberger, Kamin, Lewontin, and others, stimulated largely (it must be said)

by Jensen, it is becoming increasingly recognized that we have, in fact, *very little idea* what the heritability of IQ, either for whites or for blacks, is.

### "What If" IQ Is Highly Heritable?

Perhaps the strangest thing about the whole debate is its persistence in the face of the fact that heritability is not a concept that can be attributed to a trait as such—only to a trait in a particular population in a particular set of environments.

Jensen's argument has led to widespread confusion between heritability within a population and heritability between populations. The latter concept—heritability of the average difference between populations with respect to a trait—is meaningless, as the following simple example will show.

Height has been proven to be a highly heritable trait in many human populations. The variation in height among Americans, for instance, that can be attributed to environment is almost nil. The variation in height among the Japanese that can be attributed to environment is also almost nil. The current difference in average height between Americans and Japanese is substantial.

But changes in diet (i.e., a single and obvious aspect of environment) have contributed importantly to making each succeeding generation of Americans and each succeeding generation of Japanese taller. The Japanese seem to be growing taller faster. The difference between them may well disappear or even tip the other way. And, within each population, height will have remained just as heritable throughout.

The heritability of IQ for both blacks and whites could thus be known, and we would still have nothing that we could say about whether the observed black-white difference is due to genetic causes, or whether environmental change can influence it.

Let us take another example. The dreadful effects on brain functioning of PKU, an inborn error of metabolism, used to be considered inevitable because their origin was genetic. Since it was discovered that those effects were produced by an impaired ability to absorb certain proteins, however, dietary restriction has been able to prevent them—even though the basic error lies in the genes.

Despite his assertions to the contrary, Jensen has not provided reliable scientific evidence to conclude that differences in performance on IQ tests between blacks and whites are attributable to hereditary factors. Thus, on the 200th anniversary of the publication of Adam Smith's great work, there is no compelling evidence to reject the opinion expressed in the *The Wealth of Nations*:

The very different genius which appears to distinguish men of different professions, when grown up

to maturity, is not upon many occasions so much the cause, as the effect of the division of labour. The difference between the most dissimilar characters, between a philosopher and a common street porter, for example, seems to arise not so much from nature as from habit, custom, and education.<sup>8</sup>

### NOTES

1. Arthur R. Jensen, "How Much Can We Boost I.Q. and Scholastic Achievement?", *Harvard Educational Review* 38 (Winter 1969).
2. Other important sleuths are R.C. Lewontin; see, for example, his "Race and Intelligence," *Bulletin of the Atomic Scientists* 26 (March 1970); and Leon J. Kamin; see his *The Science and Politics of I.Q.* (New York: John Wiley & Sons, 1974).
3. This article does not deal with well-founded arguments that IQ is not the same as intelligence, or that standard IQ tests are culture-bound to an extent that precludes their use as "objective" tests for different populations.
4. Arthur R. Jensen, *Genetics and Education* (New York: Harper and Row, 1972).
5. B.S. Burks, "The Relative Influence of Nature and Nurture upon Mental Development: A Comparative Study of Foster Parent-Foster Child Resemblance and True Parent-True Child Resemblance," *Twenty-Seventh Yearbook of the National Society for the Study of Education, Part I*, (Bloomington, Ill.: Public School Publisher Co., 1928).
6. H.J. Eysenck, *Race, Intelligence, and Education* (London: Temple Smith, 1971); and R.J. Herrnstein, *I.Q. in the Meritocracy* (Boston: Little, Brown, 1973).
7. Arthur R. Jensen, "The Problem of Genotype-Environment Correlation in the Estimation of Heritability from Monozygotic and Dizygotic Twins," paper presented at the First International Congress of Twin Studies, Rome, Italy, October 28-November 2, 1974; and "The Meaning of Heritability in the Behavioral Sciences," *Educational Psychologist* 11 (1975).
8. Adam Smith, *The Wealth of Nations* (New York: Random House, 1937), p. 15.

#### ARTHUR S. GOLDBERGER

"Statistical Inference in the Great I.Q. Debate," paper presented at the Third World Congress of the Econometric Society, Toronto, August 1975.  
"Mysteries of the Meritocracy," in N.J. Block and G. Dworkin (eds), *The IQ Controversy: Critical Readings* (New York: Pantheon, 1976).  
"Jensen on Burks," *Educational Psychologist* 12, no. 1 (1976).  
"On Jensen's Method for Twins," *Educational Psychologist* 12, no. 1 (1976).  
"Jensen's Twin Fantasy" (with R.C. Lewontin), and other unpublished notes.

The papers listed here are also available as Institute for Research on Poverty Discussion Papers and may be ordered using the form on page 14.

## LABOR-MARKET DISCRIMINATION MAJOR CAUSE OF LOWER BLACK EARNINGS

Stanley H. Masters. *Black-White Income Differentials: Empirical Studies and Policy Implications*. Institute for Research on Poverty Monograph Series. New York: Academic Press, 1975.

Why do blacks continue to earn so much less than whites? This disturbing question has provoked much discussion among the general public and stimulated numerous studies by economists and others. Many explanations have been advanced, and most have been accompanied by a certain amount of empirical testing. When these studies are looked at together and compared, however, it becomes clear that the various possible explanations have not been thoroughly tested.

In his recent book, Stanley H. Masters takes an important step forward by thoroughly reviewing the economic literature on the subject. This survey of current knowledge encompasses (1) economic theories and empirical studies of discrimination, (2) previous efforts to attribute income differentials to factors other than racial discrimination, and (3) alternative political analyses and policy recommendations.

From his review Masters isolates four major hypotheses that have been advanced to explain continuing black-white income differentials. He then uses data from the 1960 and 1970 Census and from the Survey of Economic Opportunity to see how well each explanation can be sustained.

The Institute for Research on Poverty was established in 1966, by the Office of Economic Opportunity, as a national university-based center for the study of poverty and policies aimed at its elimination. Since 1974 its primary sponsor and major funding source has been the Department of Health, Education, and Welfare, with which it maintains close contact.

The multidisciplinary research staff at the Institute includes those who hold regular teaching appointments at the University of Wisconsin and divide their time between teaching and research, as well as full-time investigators appointed on a limited-term basis. The director of the Institute is Irwin Garfinkel.

The Institute offers researchers wide opportunity for interchange of ideas, and provides maximum freedom and facilitating service for poverty-related basic research as well as the study of policy effectiveness.

The first hypothesis is that the income of northern blacks lags because so many undereducated and unskilled blacks have migrated from the South. This explanation deserves attention, since 65 percent of all blacks over eighteen living in urban areas (SMSAs) outside the South in 1960 were born in the South. Segregation in housing is the second explanatory factor often cited. According to this view, residential segregation forces blacks to live too far away from many good jobs that would otherwise be open to them, thereby reducing their employment opportunities and thus their incomes. The third hypothesis is that black workers earn less simply because they are less productive, either through lack of skills or lack of effort. The fourth hypothesis: that blacks earn less solely because of racial discrimination in the hiring process—in other words, for no better reason than that employers prefer white skins.

### Neither the Southern Legacy nor Housing Segregation Causes the Earnings Gap

However plausible they may seem, neither of the first two hypotheses is confirmed by the evidence. Not only is there no difference between incomes of families headed by blacks born in the South and those outside, but annual earnings of southern-born black males actually turn out to be slightly higher than of the non-southern-born. The former are also more likely to be in the labor force and less likely to be poor. Recent migrants from the South (black or white) do suffer very temporary adjustment problems, but even this is hardly significant today since migration from the South has slowed down in the last fifteen years.

Segregation in housing is just as unhelpful in explaining differences in black-white earnings. Income differences are no greater in cities where the concentration of blacks varies widely by census tract, nor where blacks are highly concentrated ("ghettoized") rather than scattered in small clusters, nor where blacks are concentrated in the center city as opposed to the suburbs. Masters concludes from his analysis: "If housing segregation has any effect on the relative money income of black males, its effect is too weak to be demonstrated by standard empirical techniques."

### Productivity and Labor-Market Discrimination

If neither the southern legacy nor housing segregation are viable explanations, what of the remaining two hypotheses—differences in productivity and discrimination in hiring? Masters finds that both have substantial explanatory power. Their relative strength depends on the degree of confidence we place in the reliability of schooling or test scores as measures of productivity.

Differences in years of schooling, in the Masters analysis, account for about 10 percent of the racial earnings gap. A large amount of the residual has to be due either to

discrimination or to other facets of productivity not captured in as gross a measure as years of schooling.

Masters, in common with previous analysts, finds no satisfactory way of constructing a convincing productivity measure. But he does argue that use of Armed Forces Qualifying Test (AFQT) data is as defensible as any other available data source. (The AFQT is given to every youth when initially examined for military service. It contains 25 questions each on verbal concepts, arithmetic, spatial relations, mechanical aptitude. It was specifically intended to predict success in general military training and performance.)

Using AFQT-predicted scores, he finds the effect attributable to productivity goes up and becomes the most important part of the gap. The effect of labor-market discrimination is still substantial—about 30 percent of the total earnings difference between races—and is probably an underestimate, given the frequent assertion that such tests are biased against blacks.

If the "true" picture lies somewhere in between his two measures, we can be confident that both labor-market discrimination and differences in scholastic achievement have important effects.

Improvements in one, moreover, can be expected to lead to improvements in the other. Masters suggests,

Reductions in labor-market discrimination, especially for the better jobs, should provide blacks with a greater incentive to obtain a good education. At the same time, improvements in black education might reduce white tastes for discrimination and thus lessen labor-market discrimination.

## The Policy Context

Most agree racial discrimination is bad. There is no such agreement on appropriate policies to reduce it. Not unexpectedly, people's specific views regarding appropriate action tend to depend on their political persuasion—as Masters points out.

Conservatives emphasize efforts to change tastes, and view the strengthening of economic competition as automatically helping to combat discrimination. They strongly oppose legislation in that area.

Liberals, in contrast, place great emphasis on legislation, arguing that the forces of competition are not strong enough to reduce racial discrimination and rejecting the conservative's implicit assumption that legislation will not affect white attitudes toward blacks.

Radicals argue that racial discrimination is inherent in the capitalist system primarily because capitalists can exploit racial tensions to divide and weaken the working class, and secondarily because the psychological insecurity of white workers can be alleviated by emphasizing their superiority

over blacks. This view implies, of course, skepticism concerning the chances of any fundamental improvement for blacks under capitalism.

Which view is the best guide to policy? A rough test of the three competing views can be developed, according to Masters, based on the experience since the Civil Rights Act of 1964:

Given the conservative perspective, this legislation might be expected to have little effect on the economic position of blacks, except in the South. According to the liberal view, the act—and the attitude changes that both led to and resulted from the act—should have led to continued improvements in the relative position of blacks since 1964. Finally, the radical view suggests that the act should have had some initial impact, but that the gains resulting for the average black will not withstand a recession.

Masters attempts to test those positions with a regression analysis of annual data for the period since 1948. The trend toward narrowing income differentials has significantly increased since 1964, controlling for labor-market conditions. From this he concludes that events like the Civil Rights Act have caused continuing improvements in the relative position of blacks.

Recent history, therefore, though by no means conclusive, does support the liberal view. Combining this perspective with his earlier statistical analysis, Masters ends his book by formulating a number of specific policy proposals.

## Policy Implications

- Programs to ease the adjustment problems of black migrants from the South will *not* lead to any major improvement in the income of urban blacks. The poverty problems of the urban black are much more pervasive.
- Contrary to the view of recent researchers, housing desegregation by itself will probably not increase the relative money-incomes of blacks. Housing desegregation will only have a direct effect in reducing money-income differences *if* it makes whites less willing to discriminate in hiring blacks. It may have an indirect effect *if* it leads to less school segregation and *if* school desegregation improves the education of blacks. These are three very big "ifs." (Of course, housing desegregation or educational integration may be valued as ends in themselves.)
- Direct efforts to improve the quality of education available to blacks and to reduce effective discrimination in the labor market stand to have the best payoff in reducing the economic inequality caused by earnings differences between blacks and whites.

(continued on page 12)

## RACE CONFLICT AND COOPERATION IN URBAN POLITICS

*Patterns of Interracial Politics: Conflict and Cooperation in the City.* Peter K. Eisinger. Institute for Research on Poverty Monograph Series. New York: Academic Press, 1976.

It is difficult to remember that just twelve years ago Harrington could classify black ghetto dwellers as "politically invisible," and Banfield and Wilson could describe black elected officials as "politicians first and Negroes second." By the end of the sixties, racial considerations had become probably the most important element structuring urban politics.

Because of this dramatic change, the time is obviously ripe to reassess the place of race in American urban politics and to take a hard new look at interracial political relationships. Peter Eisinger's recent research contributes to such a reevaluation.

From the vast range of issues relevant to this complex subject, Eisinger has chosen as his focus the effect of behavior and belief patterns of both races on the political strategy and relationships chosen by their respective leaders. It is not only a theme that cuts across the whole subject of race in urban politics; it is also of theoretical interest to political scientists concerned with intergroup political relationships in general.

Eisinger's major contribution to the reassessment is his use of survey data on mass opinions and behavior to establish the constraints and opportunities facing the elites—the "would-be leaders"—who confront each other in the political arena. His work is one of the first to argue that the political-participation preferences and belief patterns of the masses are relevant to the strategy of the elites. His study is the most developed attempt to trace which patterns lead to coalition or other forms of cooperation and which ones lead to conflict. And it is a comprehensive effort to contrast the different attitudes toward various forms of political participation held by blacks and whites.

### Protest vs. Politics As Usual

Political protests have been studied through surveys before, of course. The traditional approach has been to ask about particular protests—*antiwar* picketing, *civil rights* demonstrations, *student* protest, *black* protest movement. Political protest as a tool and racial differences in attitude toward it have not been studied as intensively. Eisinger's survey—a sample survey of adults living in the city of Milwaukee—asked both blacks and whites about protest itself; sharp (and statistically very significant) racial differences emerged.

A majority of the blacks (56 percent) held protest to be a device to gain certain ends rather than simple troublemaking or expression of anger. Only 36 percent of whites held this view. Again, 43 percent of blacks thought it should be used more often, as compared with only 7 percent of whites. When asked whether they thought demonstrations were actually *better* than voting, 24 percent of blacks and only 4 percent of whites answered yes.

Not only did more blacks than whites in the Milwaukee sample approve of protest, but more blacks actually took part in protests than whites. For both races, those who had themselves taken part in protest were more likely to approve its use. But even for this group, racial differences emerged. A large majority (71 percent) of black protesters thought there should be more protest. Only one-quarter (26 percent) of white protesters agreed. And this one-quarter was also lower than the proportion of black *nonprotesters* (36 percent) who thought there should be more.

And what did the masses in this study think and do about conventional politics? The whites in the Milwaukee study were generally confident as *individuals* that their voice was important to public decision-makers; blacks were not. Blacks and whites shared the view that the political process was murky. They also shared the view that voting was the only way to wield influence within the political system. But they diverged in their commitment to solving political differences within conventional political rules—a divergence that cannot be explained by controlling for differences in social status. Eisinger suggests,

At the mass level among blacks, many of the standards that help to regulate the conflict process are absent as is basic support for government as an institution for conflict management.

In light of this it is no surprise that blacks were found to vote less, campaign less, and contact officials less.

Why the difference, in Eisinger's view?

A significant and vocal portion of the white community has [throughout U.S. history] rejected the norms of accommodation and peaceful politics on those occasions when racial questions have been at issue. . . . It seems entirely reasonable to conclude that the black disaffection observable in the data . . . is a *product* of the failure of whites to act on the basis of their stated normative pretensions.

### Mass Opinion and Behavior: A Survey

As a by-product of his central objective, Eisinger has some new insights into the problems—and opportunities—of surveying in the ghetto, concluding that previous warnings about the problems (particularly as contrasted with interviewing in white areas) have been overdrawn. Concrete evidence that his optimism was justified is his 80 percent completion rate for blacks (compared, interestingly, with only 70 percent for whites). *(continued on page 13)*

## Progress Against Poverty: 1964-1974

Official poverty statistics include as poor only those whose incomes remain below the poverty line even after government cash benefits—social insurance programs, like Social Security, and public assistance programs, like AFDC—have been counted in their income.

In any given year, the economy generates a set of earnings, property income, and private payments from one individual to another (for example, alimony) that together determine how much money different people have before specific government intervention. For the poor, the overwhelming proportion of this income comes from earnings. To be able to assess how effective our economy is at producing minimum decent standards for all—and then to judge how effective government is at filling the holes left by the market mechanism—we need to identify those who are unable to make it over the poverty line by their own effort.

Robert D. Plotnick and Felicity Skidmore have calculated such a measure—which they term pretransfer poverty—for the first time, and charted its progress using data from the OEO Survey of Economic Opportunity and the Current Population Survey. (See Table 1.)

Between 1965 and 1972, the absolute number of households whose earnings could not carry them over the poverty level rose from 15.6 million to 17.6 million. Because the total number of U.S. households increased over the period, this represented a small percentage decrease—from 25.7 percent to 24.8 percent. In 1968, after three years of strong economic growth and falling

unemployment the incidence of household poverty had dropped to almost 23 percent, but over the whole period 1965-1972 the drop was less than one percentage point.

Two major factors influencing earned income are the rate of economic growth and the rate of unemployment. Plotnick and Skidmore found that families headed by working-age women, the elderly, and unrelated individuals were much less responsive to changes in general economic conditions, and families headed by able-bodied men were much more responsive than the overall average.

- A 1 percent increase in average family pretransfer income was associated with a *decline* in poverty of
  - 1.0 percent for all families
  - 2.1 percent for families headed by a white man under 65
  - 3.0 percent for families headed by a black man under 65
 contrasted with
  - 0.3 percent for families headed by a white or black woman under 65
- A 10 percent increase in the national unemployment rate was associated with a poverty *increase* of
  - 2.7 percent for all families
  - 5.8 percent for families headed by white men under 65
  - 3.4 percent for families headed by black men under 65
 as contrasted with
  - 2.3 percent for families headed by white

Table 1 The Poverty Picture—Excluding Government Benefits (Pretransfer Poverty)

	1965		1968		1972	
	number (millions)	percentage of total	number (millions)	percentage of total	number (millions)	percentage of total
1. Poor persons	40.8	21.3	35.8	18.2	39.4	19.2
2. Poor families	9.4	19.5	8.4	16.6	9.6	17.7
3. Poor unrelated individuals	6.2	50.7	6.5	47.3	8.0	47.9
4. Poor households (2) + (3)	15.6	25.7	14.9	23.2	17.6	24.8



women under 65  
0.1 percent for families headed by black  
women under 65

Overall economic activity, obviously, most affects those who can work. For those who do not work by reason of age, child care responsibilities, disability, or discrimination, a low unemployment rate and healthy economic growth are not enough to ensure minimally decent living standards. For this reason, we must also be concerned with the progress of specific government action to alleviate poverty.

### Government Spending on Social Programs

Government, at all levels, spends a substantial part of the taxes it collects on social programs specifically directed toward improving the economic and social well-being of its citizens as individuals. (All government expenditures, of course, affect the well-being of the population in some way, if only through the tax system that has to finance them. Plotnick and Skidmore limited their analysis to social programs with identifiable recipients.) These include expenditures on cash benefits—such as Social Security and public assistance—as well as programs that provide food, housing, manpower training, health, and education.

Clearly, not all these expenditures are explicitly designed for low-income groups. Many are designed to promote the well-being of the population in general. It is not to be expected, therefore, that the proportion going to the poor should ever approach 100 percent.

Much of it does go to the poor, however—much more, in fact, than they receive from programs directly aimed at fighting poverty. It is interesting to find out *how* much, to see how (or whether) the proportion has changed, and to trace which programs are the most important in money terms. (Table 2 shows the statistical picture of what Plotnick and Skidmore call social welfare expenditures (SWE), for those interested in the detailed figures.)

In 1965, \$75 billion—or 39 percent of all public spending—was spent by all levels of government on social welfare. By 1972 this had climbed in absolute terms to \$185 billion, and as a proportion of all public spending, to 46 percent.

Programs specifically designated as programs for low-income groups accounted for only 12 percent of the 1965 expenditures on social welfare, of which the OEO-initiated programs accounted for less than one-twentieth. By 1972 programs designated as low-income had risen to 18 percent of the total social welfare budget, of which OEO-initiated programs were still a small part—accounting for slightly over one-sixth.

Throughout the period, cash assistance was the largest major category, although as a percentage of total SWE it declined from 49 percent to 43 percent. The next largest throughout was education; its share also declined, but only from 36 percent to 33 percent. The big gainers in proportional terms were goods-and-services (in-kind) benefit programs, mainly because of Medicaid and Medicare and to a lesser degree because of a growth of OEO-related and employment and manpower programs.

Table 2 Total Social Welfare Expenditures (SWE) and Percentage Going to the (Pretransfer) Poor

	1965		1968		1972	
	dollars (billions)	percentage to poor	dollars (billions)	percentage to poor	dollars (billions)	percentage to poor
<b>Total</b>	<b>74.5</b>	<b>42</b>	<b>109.2</b>	<b>40</b>	<b>184.9</b>	<b>43</b>
Cash transfers	36.6	57	45.6	54	80.1	53
Social Security	16.5	62	22.7	58	38.3	58
Public assistance	4.8	89	5.5	81	10.8	87
Nutrition	0.9	37	1.0	39	3.7	70
Food Stamps	0.04	95	0.9	92	1.9	85
Housing	0.3	51	0.4	70	1.8	55
Health	5.7	55	14.1	55	24.6	56
Medicaid			3.3	77	7.5	75
Medicare			4.4	47	7.0	48
Social (and OEO) services	1.4	64	2.6	73	5.3	72
Employment and manpower	0.7	63	2.0	79	3.9	72
Education	27.1	18	40.6	18	62.2	19

How much of all this went to the poor? In absolute terms the amount going to the poor increased from \$31 billion in 1965 to \$79 billion in 1972. And of the amount that went to the poor, a rising fraction has come from programs based on some low-income criterion for eligibility—24 percent in 1965, 35 percent in 1972—also largely attributable to the growth of Medicaid and Medicare, plus public assistance and Food Stamps.

As a fraction of the total SWE, however, the amount going to the poor stayed virtually stable (roughly 42 percent) in both years. This is primarily because education remains such a large component, and (as Table 2 shows) only 18-19 percent of educational expenditures go to the poor.

## How Effective Has Antipoverty Policy Been?

### Official Poverty Statistics

How effective has antipoverty policy been? In answer to this question, Plotnick and Skidmore first assess progress by the government's official definition. Official statistics and government cash-benefit programs (Social Security, Unemployment Insurance, supplemental benefits, and AFDC, for example) were added to private income sources to arrive at the income definition used to assess poverty status.

By this measure (call it *posttransfer* poverty) there were significant though modest decreases in poverty over the period (as can be seen from Table 3). Before cash transfers were counted, the proportion of (pretransfer) poor households declined only very slightly (from 25.7 percent in 1965 to 24.8 percent in 1972). Addition of government cash benefits decreased the 1965 figure by nine percentage points and the 1972 figure by eleven percentage points. Thus, in 1965, 10-1/2 million American households were (posttransfer) poor, constituting 17 percent of the total; by 1972 this had dropped to 10 million, or 14 percent of the total.

Contrasting this with the previous poverty measure, we can see how much progress against poverty government was able to make through its cash benefit (cash transfer) programs.

As this overall progress took place, the composition of those in poverty changed. Compared to 1965, the 1972 posttransfer poor more often lived in female-headed households and in households with young heads. Heads of posttransfer poor households in 1972 were also more likely than their 1965 counterparts to have a high school or college education and less likely to have held any job during the year.

Looking at the effect of the cash system on the poverty status of various groups, Plotnick and Skidmore show how this change came about. In 1965, 33 percent of all pretransfer poor households were taken out of poverty by cash

## Regular Poverty "Audits" Planned

*Progress Against Poverty: Review of the 1964-1974 Decade.* Robert D. Plotnick and Felicity Skidmore. Institute for Research on Poverty, Poverty Policy Analysis Series no. 1. New York: Academic Press, 1975.

In 1964 the War on Poverty was declared. A regular report on the state of that war would seem reasonable. The Institute for Research on Poverty has, therefore, decided to sponsor such a series.

Every two years the Institute plans to publish a comprehensive report on how U.S. poverty is changing—not only with respect to whether it is getting better or worse, but also with respect to which kinds of people are poor. Each report will also discuss antipoverty issues that, at the time of its preparation, are in the forefront of policy or research debate. The first such report has just been published.

*Progress Against Poverty: A Review of the 1964-1974 Decade*, as befits the first in the series, analyzes the whole of the period since the Office of Economic Opportunity was established and the war declared. The book begins with an historical overview of domestic social policy developments, to put the story told by the statistical analysis into a policy framework. The major part of the book is devoted to a detailed statistical description of how government expenditures on the poor have changed, and how the poverty population has changed in size and characteristics in consequence. The last chapter discusses certain issues on the social agenda.

transfers. By 1972, this figure had risen by 44 percent. *But*, the relative generosity of the cash benefit system varied widely across various population groups.

The elderly were heavily favored throughout the period; female-headed families, although starting out relatively well, made minimal progress:

### Percentage Taken Out of Poverty by Government Cash Benefits

Households headed by:	1965	1972
Aged	51	63
Non-aged men with children	11	23
Non-aged persons, no children	19	26
Non-aged women with children	22	23

Table 3 The Poverty Picture—Including Government Benefits (Postransfer Poverty)

	1965		1968		1972	
	number (millions)	percentage of total	number (millions)	percentage of total	number (millions)	percentage of total
1. Poor persons	29.9	15.6	25.1	12.8	24.5	11.9
2. Poor families	6.0	12.4	5.1	10.0	5.1	9.3
3. Poor unrelated individuals	4.5	36.7	5.0	32.8	4.9	29.2
4. Poor households (2) + (3)	10.5	17.3	10.1	15.7	10.0	14.1

### Other Poverty Definitions

To put these figures into a wider perspective, Plotnick and Skidmore show two other measures of poverty progress:

- A *relative measure*, designed to show how the fortunes of the low-income population are moving in comparison to the "typical U.S. standard of living." This gives a more pessimistic measure of progress than the official statistics.
- A *cash plus in-kind measure*, designed to take into account, in addition to cash income, income received in goods and service benefits from government (Food Stamps, public housing, Medicare, and Medicaid). This gives a more optimistic picture of progress than the official definition.

In 1965, government cash programs took 33 percent of households out of poverty as officially measured. The official poverty line in 1965 happened to represent 44 of the U.S. median income, and this 44 percent, therefore, was used by Plotnick and Skidmore to represent a benchmark against which to measure relative progress against poverty. In 1972, cash transfers raised 44 percent of households out of officially measured poverty, but only 34 percent over the relative poverty line (that is, over 44 percent of the U.S. median).

Cash plus in-kind transfers, in contrast, lifted about 50 percent of households over the official poverty line in 1965 and 72 percent in 1972.

### Major Changes in the Poverty Picture Since 1964

1. A larger percentage of persons than ten years ago live in families where earnings and other private income are unable to lift them out of poverty.

2. Government social welfare expenditures — including expenditure on cash, goods, and service benefit programs—have grown enormously in the last decade. Because of it the proportion of the population living in poverty despite government aid has been reduced.
3. Programs providing cash constitute a steadily declining proportion of the benefits going to the poor.
4. Benefit programs providing goods and services to the poor (mainly food and medical care) have grown substantially, both in absolute terms and as a proportion of the total benefit package.
5. This renders the government's own poverty definition, which counts only cash income, an increasingly inappropriate tool to measure progress against poverty.
6. Families headed by an able-bodied man have done proportionally least well out of the growth in government cash benefit programs. The proportion lifted out of poverty by cash benefits, unlike the aged or female-headed households, did not grow at all between 1965 and 1972, and has probably not increased much since.
7. Finally, the distribution of income not including government benefits has worsened. The poor are further away from being able to support themselves at typical American living standards than they were in 1965. The growth of government benefits over the period served to compensate for this, but did not reverse the trend.

(continued from page 6)

- Affirmative-action programs in addition to the 1964 Civil Rights Act are necessary if discrimination in the labor market is to be eliminated. Title VII of the act attempts to eliminate labor-market discrimination by requiring employers to establish color-blind standards. But such standards often fail to be achieved. Schooling requirements for employment may be necessary for productivity reasons, for example. But they may also be used to exclude a disproportionate number of blacks from jobs that do not really require much education. Determining at the individual job level which requirements are genuinely necessary for job performance is difficult and expensive.

Such affirmative-action programs are also necessary if past labor-market discrimination is to be prevented from having its legacy in the future—as when an employer can continue to hire all whites because his present work force is all white and he uses informal word-of-mouth referral for hiring decisions.

The government should require more active affirmative-action programs of government contractors. A real burden of proof should be placed on the employer to show that he has *not* discriminated if he is not employing as many blacks as he agreed to in establishing his affirmative-action goal. Ambitious goals must also be urged.

- Efforts to maintain high levels of aggregate demand should receive top priority in combating labor-market discrimination, since affirmative-action programs are likely to be more successful the more plentiful the supply of good jobs for whites as well as blacks.

- Schools in black neighborhoods should have as many resources available to them as white schools.
- Black community control of schools should be encouraged to make schools in black areas more responsive to the needs and preferences of black students and their families. This should lead to reduced skill differentials between blacks and whites. It is, in any case, indicated on equity grounds, since whites certainly have the option of living in suburbs where they can have a reasonable degree of community control.

• • •

Masters's specific recommendations, as mentioned above, are based on two presumptions: (1) that laws, regulations, and governmental programs have led to and will continue to lead to reduced discrimination in the labor market and reduced inequality in educational opportunities and (2) that resulting improvements in the economic conditions of blacks will lead in turn to a further lessening of discrimination.

He ends his book, however, on a warning note:

The experience since the Civil Rights Act of 1964 does provide some evidence in support of [the liberal's] optimistic position. But too little time has passed since the civil rights revolution of the early sixties for one to be confident that [this] dynamic will lead to continued significant improvement in the relative economic position of blacks. If future events should contradict [this] optimism, and if the values and analysis presented earlier are accepted, then increased attention must be devoted to examining the radical perspective and its policy implications.

(continued from page 7 )

Further evidence that confidence can be placed in the representativeness of his data comes from the fact that on demographic and socioeconomic characteristics the final sample for each group matched the universe from which it was drawn quite satisfactorily (as measured by 1970 Census figures for Milwaukee), with the single exception of high-income whites, who were undersampled.

Can the results be generalized to other cities? Eisinger points out that in racial composition Milwaukee (one of the ten second-largest American cities) closely resembles other northern cities of its size—which makes it comparable to cities like Boston, San Francisco, Indianapolis. Like most cities of comparable size, it is heavily residentially segregated. In such other characteristics as average education level, median family income, unemployment, and occupational structure, it can also be considered representative.

Certain characteristics and traditions, of course, belong particularly to Milwaukee. But, as Eisinger puts it:

What is important is . . . Milwaukee cannot be distinguished *systematically* from other cities. If it does not "stand for" other cities or all cities neither does it stand alone. What we find here, then, is surely suggestive, substantively as well as theoretically, for the politics of other big cities in other parts of the country.

### Black-White Coalition: Prospects for the Future

In Milwaukee, as in numerous other cities that share its social structure, blacks do not play a balance of power role. White elites have not had to rely on black votes to win. Decisions concerning cooperation can therefore be taken on other grounds. Eisinger has formulated a simple framework that enables him to assess, in the light of his data on mass attitudes and behavior, the likelihood of racial coalitions in such circumstances.

The picture he draws is of a continually unstable situation. Would-be leaders face different dictates from their potential followers.

Blacks who seek a political following must take black community support for protest, with its tone of intransigence, seriously. Even if they themselves favor cooperation, their dependence on mass attitudes for continuing political power presents a clear constraint.

White leaders are under mass pressure to espouse conventional politics. From this perspective, black politicians are not ideal partners.

The two races do not differ in amount and style of political activity as much as they do in attitudes. For certain specific goals they are willing to cooperate. Coalitions thus formed, however, are dictated by conditions of the moment. They are unlikely to be stable or enduring.

And Eisinger ends on a slightly ominous note concerning prospects for increased black political participation at the local level:

The routinization of protest implies that the more it is used the less effective it will be. This means that black urban communities that have relied on protest as an important means of wielding influence in the city may [in the longer run] strip themselves of power by their own efforts to gain it through protest. To weaken oneself despite the intensification or persistence of one's own struggle is perhaps both the irony and the true meaning of powerlessness.

### Postscripts for Political Scientists

- Eisinger finds no support for the view that "failure of community" lies at the heart of the urban crisis—as thought by some. Neither race showed any particular concern about it.
- Individual protest participation cannot be explained by low status—"lack of conventional resources on the part of the protesters themselves." The notion that protest is a political tool of the powerless can only be retained if powerlessness is not regarded as an individual attribute, but one that applies to a group.
- Black urban populations can be considered as racial political communities capable of independent and cohesive action. To understand the implications of this, one must "break away from the conventional historical interpretation of the black role in urban politics, and . . . overcome the resistance of those who insist on stressing [the black community's] inability to resolve internal tensions."

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