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FOCUS

Institute for Research on Poverty
Newsletter

Volume 3, Number 1: Fall 1978

MOVING UP THE JOB LADDER: IS IT HARDER FOR BLACKS?

Did black Americans make any observable economic progress during the decade of the 1960s? Recent studies reveal important labor market gains measured in terms of increased employment, lower unemployment rates, and an improved black-white ratio of median income. Blacks were also observed during the decade to make significant penetrations into white-collar and skilled blue-collar occupations—occupations largely dominated by white males. To what extent did these gains in occupational status occur via the occupational mobility of individual blacks? How do the job mobility rates for blacks compare to those for whites? And what are the roles of education and training programs in occupational mobility?

FOCUS is a Newsletter put out three times a year by the

Institute for Research on Poverty
3412 Social Science Building
University of Wisconsin
Madison, Wisconsin 53706

The purpose of FOCUS 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 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.

In the newest addition to the Poverty Institute Monograph Series, *An Analysis of the Determinants of Occupational Upgrading,* economist Duane Leigh addresses these questions.[1] Using survey data covering the latter half of the 1960s, this study compares the occupational mobility of black and white men in an attempt to uncover racial differences in the upgrading process. There are two basic theoretical approaches to analyzing the underlying factors that cause differences in the labor market opportunities available to different groups of workers: human capital theory and the dual labor market hypothesis.[2] As a framework for his empirical investigation, Leigh tests contrasting implications of these two approaches. Within the literature of labor economics, both theories treat on-the-job training (OJT) as a critical variable in accounting for differences among individuals in job upgrading and earnings. They differ, however, in their assessments of the barriers restricting access to jobs offering upgrading opportunities.

Human Capital Theory

Human capital theorists place primary emphasis on the unequal distribution of endowments of formal training across demographic groups. In particular, job upgrading is typically associated with formal education, which, it is asserted, improves the capacity to learn while on the job. Thus education increases the rate at which an individual can

AN ANALYSIS OF THE DETERMINANTS OF OCCUPATIONAL UPGRADING

by

Duane E. Leigh

Academic Press, $16.00

progress between jobs. The policy implication that follows is an emphasis on education and on the training and retraining of disadvantaged workers, assuming that aggregate demand is sufficient to generate jobs for newly trained workers.

Human capital analysts take note, of course, of the possibility that labor market discrimination may lower the returns to training obtained by blacks relative to whites. Lower returns to blacks, in turn, contribute to the observed racial differential in human capital endowments. Nevertheless, the human capital model (or, more precisely, the neoclassical theoretical framework) does not offer a completely convincing explanation why racial differences in returns to equivalent human capital endowments have not been eliminated, over time, by competitive forces.

The Dual Labor Market Hypothesis

The emphasis on maximizing behavior and competition in neoclassical models has been the focus of criticism by institutional economists since the 1890s. During the 1940s and 1950s, in particular, a number of influential labor economists suggested that the interaction of large corporations and labor unions results in the formation of internal labor markets that are effectively isolated from the competitive pressures of the external market. Building upon this "neo-institutionalist" literature, dual labor market analysts developed the notion of structural barriers between the internal labor markets—the primary sector—and the external labor markets—the secondary sector. The primary sector offers high wages, good working conditions, employment stability, a formal advancement ladder, and equity and due process in the administration of work rules. Jobs in the secondary sector, in contrast, tend to have low wages and fringe benefits, poor working conditions, high labor turnover, little chance of advancement, and often arbitrary and capricious supervision.

In contrast to the human capital stress on unequal formal training, dualists argue that institutional and sociological barriers prevent black workers from gaining access to employment which offers a job hierarchy (i.e., employment in the primary sector of the labor market). As a consequence, blacks tend to receive less OJT than whites for equivalent amounts of formal training. The key problem is therefore one of racial differences in employment opportunities rather than differences in personal characteristics.

Data

The study focuses on occupational mobility as a proxy measure for job upgrading because movement up a job ladder can be regarded as the observable outcome of productive OJT. Since wage increases are expected to be positively associated with job progression, differential access to employment offering training on the job may provide an important insight into observed racial differences in lifetime earnings profiles. From a broader perspective, opportunity for movement up an occupational hierarchy serves to differentiate a "job" from a "career."

Two data sets were analyzed. The first of these is the 1/1000 Public Use Sample of the 1970 Census and the second is the National Longitudinal Surveys (NLS) of labor market experience. The NLS cohorts considered include (1) young men between the ages of 14 and 24 in 1966 and (2) men between the ages of 45 and 59 in that same year. The longitudinal nature of these data is critical; it allows the measurement of occupational change over time for the same individual. Both data bases provide detailed occupational categorizations. Occupational change was measured between 1965 and 1970 for the Census sample and over the 1966-1969 period for the NLS cohorts.

Findings

Leigh's empirical analysis concentrates on three areas: the relationship between educational attainment and occupational advancement; the impact on occupational upgrading of formal vocational training, industry structure, and job tenure; and the impact of interfirm and interindustry mobility on occupational progression. Let us look briefly at the primary results.

First, education is found to have an important effect on the occupational upgrading of both blacks and whites, but the relationship for whites is typically stronger across age categories and samples. Postschool investments in formal vocational training and firm-specific experience, however, have at least as strong an effect for blacks as whites. Second, there appears to be sufficient mobility between employers to make the impact of initial industry and region of residence relatively unimportant in explaining the subsequent occupational upgrading of both blacks and whites. Third, the impact of interfirm and interindustry mobility on occupational advancement tends to be at least as large for blacks as whites, controlling for personal endowments. Among firm and industry stayers, finally, the evidence appears to call for the rejection of the hypothesis that there is a systematic racial differential in occupational upgrading within internal labor markets. It should be emphasized, however, that black advancement is measured from a relatively low initial level. Since a strong inverse relationship exists between occupational change and occupational level, a persistent racial differential in advancement is predicted for black and white men of similar personal characteristics *and the same initial occupational level.* Thus, the findings of this study should not be taken as indicating the disappearance of racial differences in advancement opportunities during the 1960s.

In light of this evidence, Leigh concludes, the dual labor market hypothesis does not adequately describe the labor market status of black men.

Policy Implications

The policy area addressed most directly in this research concerns the impact of education and training on occupational upgrading. The main issue is the premise of dualist writers that the needed human capital is present but unutilized in secondary sector workers. The empirical evidence obtained in this study indicates little support for this premise. On the contrary, the relatively low level of black human capital investments, when coupled with the sizable amount of upgrading derived from education and vocational training for blacks and whites alike, suggests the desirability of continued attempts to improve the relative

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HOW MUCH LESS WILL THE POOR WORK UNDER INCOME MAINTENANCE? QUESTIONS REVISITED

by

Felicity Skidmore

> He knew he had to make some explanation for letting his good land run free. He said, "I guess I'm a lazy man. And my father didn't help me when he left me enough to get along on without working." He closed his eyes but he could feel the relief on the part of [his listeners]. It was not laziness if he was a rich man. Only the poor were lazy. Just as only the poor were ignorant.
>
> John Steinbeck

Why, one may well ask, are people still estimating work disincentive effects? To find out about them, after all, the U.S. government has already spent over $100 million on income maintenance experiments. Why do we need another study?

At least part of the answer given by Stanley Masters and Irwin Garfinkel is the inherent difficulty of narrowing the range of estimates enough to be really useful to policymakers who must estimate costs; no single method can do the job. Numerous studies using different methodologies and different kinds of data have yielded varying results. The authors discuss the strengths and weaknesses of the various approaches (including their own) and give reasons why they place reasonable confidence in their own estimates and in the other studies that yield estimates in the same range.

Another part of the answer, they are convinced, is that the American public and their representatives—those who in the last analysis decide the type and amount of income support there will be—remain much preoccupied for noneconomic reasons with how much less people who receive income support payments (i.e., the poor) will work as a result of those payments, and much concerned with ensuring that the beneficiaries of public programs not get a free ride.

When people worry about free rides they refer mainly to prime-aged, able-bodied men. Society tolerates idleness on the part of the young (who are permitted to spend their time in education and training), the elderly (who deserve leisure as a reward for a lifetime of work), and the disabled (who, for reasons that won't be discussed here, are not expected to work). Society seems somewhat ambivalent, at this time, about mothers—particularly mothers who head families. Raising young children is clearly an activity sanctioned by society. But certain features of AFDC and public attitudes toward it are indicative of changing attitudes toward the role of women and their place in our economy.

Issues of Data and Methodology

The Masters-Garfinkel book, in its careful and detailed discussion of the labor supply issue, provides a primer of what to do and what not to do when estimating labor supply effects of income maintenance. It includes a painstaking literature review of the methodology and results of others.

> **ESTIMATING THE LABOR SUPPLY EFFECTS OF INCOME-MAINTENANCE ALTERNATIVES**
>
> by
>
> Stanley Masters and Irwin Garfinkel
>
> *Academic Press, $17.50*

There are three basic ways to approach the problem and each is vulnerable to different criticisms, as the authors demonstrate. Their view is that each approach is relevant, and reconciliation of their differences provides insurance against gross prediction error.

The first method is to derive estimates using data collected from actual programs—General Assistance (GA) and Unemployment Insurance (UI) are the two that have been studied that aid prime-aged males. This would appear, at first blush, to be the obvious strategy—finding out what people actually do when they are beneficiaries of real programs. The estimates derived from such studies suffer, however, from two major weaknesses which cast doubt on their validity. First, both GA and UI give benefits not only to groups who are expected by society to work but also to groups for whom society has different expectations. The studies done so far have not differentiated among demographic groups in their calculations, leading to overestimates of the disincentive effects that are most relevant to society's concern—the effects on prime-aged, able-bodied men. Second, to get quantitative estimates of how the disincentive effects differ by benefit generosity, program studies are forced to make interstate comparisons. But labor force behavior is a function not only of the transfer benefit opportunities but also of labor market conditions. Since these studies generally cannot separate the different effects their results confound the two.

The second approach to labor supply response estimation is to collect data from income maintenance experimentation: Give one group benefits and measure how their behavior compares with the behavior of a group (the control group) whose characteristics and situation are similar except for benefit eligibility. Several such experiments have been undertaken and their results analyzed. Critics have been loathe to accept the findings without reservation, however, for three major reasons. First, the experiments provided only a temporary income support program. A real program might induce greater work reductions if the beneficiaries' knowledge that they could count on a permanent income guarantee changed their behavior. Or, a real program might induce smaller reductions than under a temporary program if people, knowing the experiment was temporary, decided to use the payments to take more time off than they would choose if they *knew* the payments would always be available.

The second reason involves people who drop out of the experiment before the payment period is over. Such attrition is less likely among those who will lose large payments from the experiment (that is, those whose labor supply is low) than those with relatively high earnings and little or no income support from the program. This differential attrition will, therefore, lead to an overestimate of the disincentive effects.

Third, both because the experiments are temporary and because they affect only some of the potentially eligible population, the results cannot reflect any basic labor market or community changes that might result from a permanent program.

The third approach—the one followed by Masters and Garfinkel—avoids the weaknesses of the other two by using national survey data to estimate, from people's observed responses to the unearned income they happen to receive, what their responses would be to transfers of differing generosity. These data do not, of course, allow direct observation of the effects of income maintenance. But they do contain information on people who are otherwise similar but differ in the wages they command and the amounts of unearned income they receive. "We can thus get indirect measures of the probable effects of income maintenance," the authors argue, "by comparing the work effort of similar people who happen to have different wage rates and different unearned incomes." This approach, although avoiding the weaknesses inherent in the previous two, suffers from its own. For one thing, people with different wage rates and different unearned incomes are likely to differ in other important ways not captured by the survey which may affect labor supply—the attractiveness of their jobs, for instance, and how personally ambitious they are. For another, if someone does not work there will be no wage rate in the survey data and a rate will have to be estimated from other known characteristics (such as age, sex, race, years of schooling). This is especially important for women, since fewer of them work. Last, low-income persons receive very little unearned income except from government transfers, which are most frequently received because the individual cannot work—that is, received as a result of atypical work behavior. Such unearned income must properly be excluded, which leaves survey studies heavily dependent on the few respondents who have large amounts of unearned income not affected by the amount they work. To the extent that these respondents are atypical, the results will be distorted.

The Authors' "Best Estimates"

What is the resolution of all this?

The answer Masters and Garfinkel give is to say: Let's do whatever we can to minimize the causes for bias in our regression estimates. Let's use two independent data sources (the 1967 data from the Survey of Economic Opportunity and the 1972 data from The Michigan Panel Study of Income Dynamics) to see how sensitive the results are to the data used. Let's be extremely careful about what sources of unearned income data we include. Let's control as carefully as the data allow for health status and for personal ambition. Let's calculate separate estimates for the many different demographic groups, since public policy is much more concerned about some groups than about others. Let's then compare our results with the results of others (from studies using the same or similar data, studies using experimental data, and studies using program data) to see to what extent the sources of differences can be satisfactorily explained, and to what extent the various studies can be interpreted as indicating the same orders of magnitude. And throughout let's be aware of the limitations of social science research, even when done under the most meticulous conditions.

All this they do. And they find that their best estimates are generally consistent with those of the other studies they have been able to give high methodological marks to.

Their results indicate that beneficiaries would work somewhat less as a result of a generous noncategorical income maintenance program. A program with a guarantee at the poverty level and a tax rate of 50% could be expected to lead to a reduction in hours worked by beneficiaries of 12-21%. Only a small proportion of this reduction, however, would be attributable to able-bodied, prime-aged men (whose labor supply reductions would be in the neighborhood of just 1-3%). The aggregate expected reductions in labor supply would increase the costs of income maintenance to the taxpayers by a modest amount: 13-21%. But the economic costs to society as a whole would be much lower: A crude estimate suggests that the resulting reduction in Gross National Product would be "only about 3% as large as the potential GNP that Americans have voluntarily foregone by working substantially less than the norm of 75 years ago. More importantly, the efficiency losses to society as a whole [would be] less than 0.3% of total welfare."

The Bottom Line

What does this tell the authors about the appropriate direction income support policy should take?

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SELECTED PAPERS AND BOOKS

Irwin Garfinkel with the assistance of Felicity Skidmore, "Income Support Policy: Where We've Come From and Where We Should Be Going," Institute for Research on Poverty Discussion Paper no. 490-78.

Irwin Garfinkel, "The Effects of Welfare Programs on Experimental Responses," *Journal of Human Resources* 9, no. 4 (Fall 1974): 504-529. Institute for Research on Poverty Reprint no. 130.

Harold Watts and Albert Rees, eds., *The New Jersey Income Maintenance Experiment, volume 2: Labor Supply Responses.* New York: Academic Press, 1977.

Order forms for FOCUS and other Institute publications are at the back.

LONG-TERM TRENDS IN AMERICAN WEALTH INEQUALITY

by

Felicity Skidmore

Simon Kuznets, Nobel Prize Laureate in economics, has postulated that there is—almost inevitably—an early rise and later decline in inequality during long-term modern economic growth. Jeffrey Williamson and Peter Lindert[1] have undertaken a detailed study of wealth inequality trends in America that confirms the Kuznets hypothesis.

Major Conclusions

Seventeenth and eighteenth century America saw relatively egalitarian and stable aggregate wealth concentration. During the first half of the nineteenth century, in contrast, there was a marked rise in wealth concentration. The period from the Civil War to the Great Depression was also one of stability in wealth concentration, but it was characterized by substantially greater inequality than had prevailed in the colonial era. During the second quarter of the twentieth century, wealth inequality again decreased so that inequality of wealth-holding today resembles what it was on the eve of the Declaration of Independence. Those who would argue, therefore, that the degree of wealth inequality is some sort of eternal constant will be uncomfortable with the Williamson-Lindert findings.

Perhaps their most controversial conclusion is that neither changes in age composition nor changes in the size of the immigrant population influenced in any important way these changing trends in American wealth inequality.

Colonial Era

The basic conclusion they reach for this period of American history is that, though the experience of different local areas varied, when the New England or Middle Colonies are examined as a whole, there is no evidence to support the view that aggregate wealth concentration was increasing between the late 1600s and 1774.

The authors come to this conclusion on the basis of data collected from local tax assessment and probate records. They show how the choice of a benchmark date from which to measure trends affects the results, and they discuss which dates should be taken as "normal" periods of economic activity. They demonstrate that wealth inequality was on the rise in fast growing cities which attracted young adults and/or the propertyless, that there was no visible increase in wealth inequality among the slow growing cities, that settled agrarian regions had no stable pattern, and that frontier settlements exhibited some evidence of rising inequality. Their major contribution to the debate on colonial wealth inequality, however, is their proof that generalization from such local histories to an assessment of aggregate wealth concentration can be and has been distorting.

There are no data on wealth inequality for the colonies as a whole until 1774. Colonial historians have traditionally based their conclusions on data from settled urban areas (Boston, Philadelphia, Hartford, New York City) or from older eastern townships or counties (Hingham, Chester). But such evidence takes no account of population shifts, changes in per capita wealth differentials between regions, or changes in wealth inequality in different regions.

Williamson and Lindert develop an aggregate wealth inequality statistic[2] which can be disaggregated into four components: (1) change in concentration of wealth in the settled regions, (2) change in concentration at the frontier, (3) change in the size of the older settlements in relation to the size of the total population, and (4) the ratio of per capita wealth in the older settlements to that in the colonies as a whole. This allows them to use available data on population and average wealth in various regions, plus generally agreed upon conclusions regarding relative wealth inequality in the settled regions versus the frontier, to make their overall assessment that there is no evidence of a trend toward increasing aggregate wealth inequality in the colonial era. They also assess, within the constraints of available data, how this conclusion would be affected by the age distribution patterns of the population and the selectivity of the migration flows and conclude that, if anything, these factors would tend to bias observed wealth inequality upward.

Their explanation for the lack of trend is that the opportunities for wealth accumulation on the frontier were exploited assiduously. This led to extensive and intensive development in the interior and the influx of population and resources from the coast. Wealth per capita grew relative to that in the seacoast settlements which, since the new settlements were comparatively poor to begin with, constituted a levelling influence.

The First Century of Independence

For this period scholars have been able to use probate inventories and manuscript census samples to produce aggregate benchmark statistics on wealth-holdings for 1774, 1860, and 1870. Using these data Williamson and Lindert show that aggregate inequality increased markedly between 1774 and 1860 but remained stable between 1860 and 1870. Their task for this historical period is to assess the reliability and robustness of these estimates and the reasons for the observed trend. They show that inequality was increasing over the period if slaves are excluded. If they are included (both as property and as propertyless members of the population) the trend becomes slightly less steep but still substantial. Possible sources of bias in the probate records and the manuscript census returns have not been ignored; Williamson and Lindert conclude that there is no evidence on systematic bias in the estimates.

The authors then address the "common suspicion" that the trend toward increasing inequality was due to demographic shifts—in either the age distribution or the immigrant mix.

Two techniques are used to test the age effect. First, the 1860 age distribution is applied to the 1774 wealth data; very little difference in trend appears. Second, they examine available data on the relationship of age to wealth-holdings; they conclude that attention to age distribution

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BLACK SUICIDE: AN EPIDEMIC?

by

Roberta Kimmel

There is a popular belief in the United States that very few blacks commit suicide. "We don't do that; we're too in love with life," commented one black observer. But consider these startling statistics:

- Suicide is more common among black males aged 25-29, and among black females 15-19, than it is among whites in the same age brackets.
- The suicide rate for blacks aged 15-34 is now higher than it has been in more than fifty years. Between 1970 and 1975 the rate for males in the 25-29 age bracket increased 42%.
- Among whites, suicide is most likely to occur in middle life or old age, but among blacks the incidence of suicide is highest for the young: 47% of black suicides occur in the 25-34 age bracket.

Clearly this phenomenon, which may be read as symptomatic of deeply embedded social problems, has serious implications for the black population. These suicide statistics not only represent a tragic loss to bereaved family and friends; they also mean an acute loss of human resources whereby the black community is deprived of youthful manpower, the benefits of earned wages, procreative and nurturing sources, and a host of other contributions that these individuals might have made to society.

The patterns of black suicide rates, their implications, and current assessments of the causes are addressed in recent research by sociologist Robert Davis, a Project Associate at the Poverty Institute. Suicide, Davis points out, is still an uncommon occurrence among older blacks, and although it is on the rise among black women (a fact that has generated a good deal of publicity) the core of the problem lies in the rapidly rising incidence of suicide among black males. The ratio of black male to black female suicide is a startling 3.4 to 1 for the total black population and 4 to 1 for the 20-34 age bracket.

Theories of Suicide

Existing explanations of the phenomenon of suicide are largely speculative, for few empirical studies of motive exist. Sociologists are only now beginning to tackle the topic of death in general, and suicide, with its painful implications of failure within family and social networks of support, presents formidable methodological problems.

In the classic treatise *Suicide,* Emile Durkheim proposed that the suicide tendency is the result of collective forces acting upon the individual. Durkheim's Law states that suicide varies inversely with the degree of integration of the group to which the individual belongs. Modern proponents of this theory maintain that a disruption of social relations is the primary causative factor in suicide.

There are many psychoanalytic theories of suicide; relevant here are the psychological and social psychological schools of thought. In its simplest form, the former stresses the aggressive nature of suicide. The urge for self-preservation is so strong, Freud maintained, that the act is directed against another person with whom the individual has identified. Adler, too, emphasized interpersonal factors—the effect of suicide on others—but proposed that suicide results from a lack of social relations that make life purposeless.

Robert Davis, "Black Suicide and the Relational System: Theoretical and Empirical Implications of Communal and Familial Ties," Institute for Research on Poverty Discussion Paper no. 481-78.

Robert Davis, "Black Suicide in the Seventies: Current Trends and Perspectives," Institute for Research on Poverty Discussion Paper no. 483-78.

Some explanations directly address black suicide. Most frequently cited is the *urban stress* (or frustration-aggression) *hypothesis.* It postulates that compounded urban stresses associated with migration, poverty, unemployment, racism, poor housing, and poor education result in violence which often though not always takes the form of suicide.

Proponents of the *status integration theory* believe that as blacks work their way into the middle and upper middle classes they inherit the economic, social, and psychological tensions of their white counterparts. The more upwardly mobile blacks become, the more intense are problems of adjustment and assimilation into the American mainstream. A corroding sense of internal alienation may ultimately result in self-destruction.

The *black family deficit theory* portrays the black family as being unable to meet the fundamental needs of its members for survival, socialization, and transmittal of a viable cultural heritage.

Not least, there are some who argue that the increase in black suicide has been overestimated. It is, they claim, an artifact of statistical reporting: Coroners are investigating ambiguous black deaths much more thoroughly today than in the past.

A Different Model

Davis sets forth a model that speaks more directly than any of the above theories to the concentration of suicide among young blacks. He builds on the *"external restraint theory,"* which holds that suicides vary inversely with such restraining factors as low social status and the insulation that strong communal and familial ties provide. Davis proposes that restraints which have previously tended to produce a solidarity among blacks—for example, the stresses of overt racism and discrimination—have weakened for young blacks:

> Recently . . . there has been an increase in social opportunities (more prestige, better jobs, higher education, etc.) and social status for some blacks. Generally speaking, young black males and females have experienced an uplifting of goals, aspirations, and expectations as a result of the perceived change toward greater opportunities within American society. Concurrently, this loosening of restraints has produced a false sense of freedom and security that has led to individualism and utilitarianism, which have tended

to loosen or weaken the communal and family ties previously serving as a buffer against suicide.

A Statistical Analysis

To examine the possibility of a link between weakening communal and family ties and suicide, Davis has analyzed data for 1970 and 1975 from three sources: annual reports published by the National Center for Health Statistics (1970 and 1975), U.S. Bureau of the Census statistics (1970), and the Current Population Survey (1971 and 1976). Although the state data are for "nonwhites," blacks constitute 90-94% of this category; further, by limiting the sample to the 17 states with the largest black populations, Davis has increased the probability of including black suicides.

Davis looks at two variables which might be viewed as limiting access to stable, positive relations within the black community: inmigration, since it implies the necessity of dealing with new urban stresses, and living alone, since some researchers claim that people in this situation tend to lack close interpersonal ties. Inmigration is measured by the percentage of blacks five years of age and older who moved into a state between 1965 and 1970. Living alone is measured by the percentage of a state's black population living in a one-person household. In addition, the relationship between black suicide and educational level (as a proxy for socioeconomic status) is examined.

Results

For both 1970 and 1975, inmigration is found to be strongly correlated with black suicide. Living alone, however, appears unrelated to the suicide rate. A striking finding concerns the level of education: States having a low percentage of blacks with fewer than nine years of schooling display high black suicide rates. Social theorists have argued that suicides are more common among the upper and middle classes than among the lower class, and this seems to be demonstrated by these results. In sum, Davis concludes, "the current increase in black suicide can be attributed, at least in part, to young, upwardly mobile blacks who are isolated from their families, communities, and social institutions."

Future research by Davis will be aimed at clarifying the relationship between social mobility and social stress, and between social stress and mortality. Detailed mortality tapes from the National Center for Health Statistics will be analyzed in conjunction with migration and socioeconomic data for the 125 largest standard metropolitan statistical areas. Given that suicide is but one reflection of stress, the question whether other forms of stress-related mortality (i.e., homicide, accidents, cirrhosis of the liver due to chronic alcoholism) have increased among blacks at equally dramatic rates for the 20-34 age group bears investigation.

* * *

FORTHCOMING INSTITUTE BOOKS

Fall 1978

Joel F. Handler, **Social Movements and the Legal System: A Theory of Law Reform and Social Change**

Can the legal system be used to change society? The author approaches this question by reviewing thirty-five case studies in four areas of social reform: environmental protection, consumer protection, civil rights, and social welfare. The various conditions under which the courts have effected change are systematically compared.

A strong theoretical framework underlies the analysis. Handler's model incorporates the concept that social movements vary in their ability to mobilize resources to put sustained pressure on the legal system for change. The following concepts are used to explain the success or failure of judicial remedies: 1) the characteristics of groups that are conducive to the mobilization of resources for collective action; 2) the benefit-cost distribution of specific actions that contribute to group mobilization; 3) the structure of the law reformers—their technical and political skills and their funding base, which affects their staying power over the course of a suit; 4) the nature and effectiveness of judicial remedies; and 5) the nature of the bureaucratic situation that facilitates or inhibits the implementation of the social movement goals.

The value of this study lies in its broad perspective of the problems facing social movements. By taking into account the power of the political, economic, and bureaucratic forces outside the courtroom, it goes beyond mere historical description.

Spring 1979

Joel F. Handler, **Protecting the Social Service Client: Legal and Structural Controls on Official Discretion**

A close look at the treatment of clients of public and private social service agencies shows these individuals to be greatly in need of consumer protection. Handler's particular concern is the legal rights of these social service clients, and he questions how effectively these rights are being enforced. Concentrating on the abuses of administrative discretion, he describes the history of due process protection (i.e., by means of appeals and court hearings) and offers a number of legal and structural remedies.

This volume extends two previous studies by the author, *The Deserving Poor* (with Ellen Jane Hollingsworth) and *The Coercive Social Worker,* by drawing on new data, focusing on the sources of the discretionary behavior, and proposing changes in present systems. The book is directed not only at advocates for social service and welfare clients, but also at bureaucrats and policymakers in health, education and employment programs—those agencies characterized by a great degree of administrative discretion and a highly dependent clientele.

These books will be available from the publisher, Academic Press, 111 Fifth Avenue, New York, New York 10003.

Long Term Trends
(continued from page 5)

trends in the antebellum era suggests that the aggregate inequality indices understate the true trend toward wealth inequality.

To assess the effect of the foreign-born they again use their inequality statistic, this time decomposing it into changing inequality within the foreign- and native-born groups, changing inequality between the two groups, and the changing proportion of the two groups in the total population. They estimate that the presence of the foreign-born in the American wealth distribution raised the Gini coefficient in 1860 by only about 2%.[3] They also use available data to show that, although the native-born were wealthier than the foreign-born, the ratio of the wealth of the two groups seemed rather stable over time. The major source of the steeply rising wealth inequality, therefore, must lie in trends toward increasing inequality within both groups.

They then examine the extent to which urbanization accounts for the aggregate trends by disaggregating their inequality statistic into rural and urban components, and conclude that urbanization did raise the degree of inequality during the period, but that its effect on aggregate inequality was relatively modest—increasing it on the order of 3%. "This again implies," they state, "that the vast majority of the antebellum wealth inequality surge in America had its source *within* sectors and regions."

Civil War to Great Depression

Williamson and Lindert again collect, compare, and evaluate what evidence there is on the wealth distribution for this period. Probate records have not yet been exploited; until they are, according to the authors, the evidence over much of the period is scant. What there is suggests that the degree of inequality was about the same at the end of this period as at the beginning. According to statistics from the manuscript censuses there was some levelling across the 1860s (due principally to the massive confiscation from the richest Southerners of slave property as a result of emancipation). According to a Federal Trade Commission special probate estate valuation survey of the 1912-1923 period, some levelling also occurred across the World War I decade. But by 1929 this was reversed largely if not entirely (according to the estate tax data mentioned below). The authors' most likely candidates for the pinnacle of wealth inequality in America are the period around 1860, the period around 1914, and 1929: "That each of these pinnacles was followed by a major upheaval . . . suggests interesting hypotheses regarding the effects of these episodic events on wealth inequality (or perhaps even the impact of inequality on these episodic events)."

Jeffrey G. Williamson and Peter Lindert, "Long-Term Trends in American Wealth Inequality," Institute for Research on Poverty Discussion Paper no. 472-77.

Post-World War I

Williamson and Lindert use two main data sources for the modern period: estimates of top wealth-holder shares as measured by estate tax returns, and the Federal Reserve Board's 1962 Survey of Financial Characteristics of Consumers. They resolved inconsistencies between the two by making the wealth-holding units comparable across data sources and correcting for an error found in the total private net worth estimate used in the Federal Reserve Board survey. The adjusted estimates show that wealth inequality increased between 1922 and 1929, decreased between 1929 and 1953, and has since remained stable.

As they did for previous periods, Williamson and Lindert examine whether this pattern could be just an artifact of changes in the age distribution. They conclude that "age-life cycle effects appear to be a trivial component of aggregate wealth concentration trends in the mid-twentieth century. Regardless of the time span selected, Gini coefficients vary hardly at all in response to these demographic forces. What small impact there is produces increased wealth concentration over time. Thus, it appears that the post-1929 levelling in wealth distribution is understated, and proper adjustment for life cycle effects would make the trend towards greater wealth equality even steeper."

A forthcoming monograph by Williamson and Lindert goes beyond description of these trends to advance a theoretical model of the processes behind them.

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[1]Jeffrey Williamson and Peter Lindert are Professors of Economics at the University of Wisconsin-Madison and the University of California-Davis, respectively.

[2]They do this by dividing the colonies conceptually into an urban (old settlement) area and a rural (new frontier) area and decomposing the colony-wide variance in wealth-holding into the weighted sum of variance within and between each region.

[3]By this measure (the Gini coefficient), if each percentile of the population had the same percentage of total wealth, there would be absolute equality and the Gini coefficient would equal zero. In a state of absolute inequality the Gini would be one.

How Much Less Will the Poor Work . . .
(continued from page 4)

They are refreshing in revealing the prime motivation of their research: "When we began work on this study in 1971 . . . we were advocates of either a universal credit income tax or a negative income tax program to aid the poor. We believed that opponents and potential opponents of such programs exaggerated their potential costs in terms of induced labor supply reductions."

They are not naive enough to believe that any estimates they come up with *per se* indicate anything about welfare reform.

They argue that (as the quotation that starts this article affirms) the work effort of the poor is considered by society to be an important issue in and of itself. The authors say they wanted to satisfy themselves as to the order of magnitude of the disincentive, because it would (and should) influence their policy recommendations.

Their bottom line is that, since the work disincentive effects would appear to be relatively minor, they favor a credit income tax (CIT)—that is, a program which gives the same absolute benefit to everyone and levies from everyone according to their ability to pay (as assessed by the positive tax system).

The reasons they give for favoring a CIT over a negative income tax (NIT)—accompanied by a more progressive tax structure, which would accomplish the same thing—are as follows: (1) They are in favor of increasing the standard of living of the poor, (2) they are convinced that a program which segregates the poor (i.e., they are eligible for a program *because* of their low income) submits them to conspicuous and unfavorable attention, and (3) they are convinced that a program that administers alike to the net receivers and net givers will minimize human divisiveness.

Although they advocate a credit income tax, they would have it implemented cautiously:

> Thus our empirical results, despite their limitations, have reinforced our belief that we should move gradually toward the adoption of a CIT. We hope that they also convince those who have been hesitant . . . for fear that very large reductions in labor supply would result if a reasonably generous program should ever be adopted. Given the limitations on our current knowledge, however, we strongly advocate that new redistributive programs be implemented gradually so that they can be modified easily as we learn more about their effects.

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[1]Stanley Masters is a Project Associate at the Poverty Institute; Irwin Garfinkel is Director of the Poverty Institute and a Professor of Social Work at the University of Wisconsin.

Moving up the Job Ladder
(continued from page 2)

quality of the formal schooling received by black children and to provide manpower training programs that emphasize the acquisition of marketable skills.

The form that OJT programs should take is a highly controversial issue. Previous studies examining the effects of government sponsored but privately operated OJT programs (e.g., Job Opportunities in the Business Sector) have frequently yielded pessimistic conclusions regarding the effectiveness of the programs in training and placing secondary workers in jobs that offer career ladders. However, the alternative that the government create jobs to employ secondary workers runs into the objection that there is little evidence to indicate that public sector jobs provide the training that will qualify workers for primary jobs in the private sector. On balance, the evidence presented in this study speaks in favor of government-sponsored manpower training, especially in the private sector, over permanent public service employment.

Leigh argues, however, that some of the changes in government policy recommended by dualist adherents to restructure jobs held by secondary workers might be desirable. An illustration is reform of particular features of social welfare programs to encourage employment stability (for example, making changes in the Unemployment Insurance program that would raise the cost of labor turnover borne by employers). In addition, a federal wage subsidy applicable to public or private employment is likely to attract increasing attention as a policy measure to supplement the results of the labor market.

Any attempt to deal with institutional arrangements that restrict the intrafirm advancement opportunities of blacks runs into one basic problem: Gains in promotional opportunities or in job security by one group of workers usually mean equivalent losses for another group. Leigh suggests that future research and policy analysis might do well to direct more attention toward examining methods to shift costs away from the incumbent work force and the employer and toward the government, since discrimination in employment is, after all, a social problem and not a private one.

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[1]This monograph was first prepared as a final report to the Manpower Administration (now the Employment and Training Administration) of the U. S. Department of Labor.
[2]For additional discussion of theories of discrimination in the labor market, see "The New Challenges to Orthodox Labor-Market Theory—How New? How Challenging?" FOCUS 1, no. 2:7+.

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