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On March 23, 1966, the University of Wisconsin received a grant from the Office of Economic Opportunity to establish a research institute to study poverty. In this anniversary issue of *Focus*, we present a history of the Institute and invited articles by four social scientists with long-standing ties to the Institute and its commitment.

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A brief history of the Institute for Research on Poverty

by Elizabeth Evanson

Genesis

Certainly one of the justifications for a large-scale grant to a single institution as opposed to a whole set of small project grants scattered out all over the place, is that you reach a critical mass of research interest when you get a group of people together who have similar interests, but different backgrounds.

Robert Lampman, 1966[1]

The Institute was created in March 1966, when the University of Wisconsin–Madison reached agreement with the U.S. Office of Economic Opportunity to establish a national center for study of "the nature, causes, and cures of poverty." A national center, located in Madison, was a logical response to the issues and the times.

When the federal government undertook new efforts to aid the poor in the 1960s, it also determined that social programs would be studied and evaluated to determine their effectiveness. In 1965 a presidential executive order directed all federal agencies to incorporate measures of cost effectiveness and program evaluation into their decisions. The guiding concept was that the policies and programs then being developed should be shaped by sound logic, firm data, and systematic thinking rather than by good intentions alone.

Charged with implementing the War on Poverty that President Johnson had declared in 1964, the Office of Economic Opportunity (OEO) sought to establish a center where experts would perform basic research, provide counsel, and serve as a ready information source. To distance it from the arena of day-to-day issues and problem-solving, the center should be located outside of Washington. The University of Wisconsin was a likely site in view of its long tradition of applied social policy research,[2] and because several of its faculty members had served on the staff of the president's Council of Economic Advisers when the antipoverty strategy was being formulated. Prominent among them was Robert Lampman, a member of the economics department, who became interim director of the new institute and has remained a guiding spirit.[3]

At first cool to the idea of becoming too closely involved with immediate government activities at the expense of more academic pursuits, the university accepted OEO's offer on condition that the Institute exercise full authority in allocating grant funds to researchers, selecting research topics, and publishing the results. The agreement signed on March 23 describes the essential features that characterize the Institute today, even though the OEO has not existed for many years and the optimistic belief that poverty could be eliminated within one generation has faded.

The agreement specified that the Institute would embrace a number of the social science disciplines; that it would encourage new as well as established scholars to inquire into the origins and remedies of poverty; that it would promote sharing of knowledge among researchers and policy analysts by means of conferences held at periodic intervals (see the list of conferences, page 29); and that it would communicate its findings through a publications program (the list of books that the Institute has sponsored appears on page 33).

Institute staff, then as now, consisted of a director, advised by an internal committee of faculty members and a national advisory committee of members outside the university (see page 24 for the names of those who have served on the National Advisory Committee); researchers, who would hold university appointments and divide their time between teaching and the study of poverty-related topics of their own choosing, subject to approval by the director and the advisory committees; and a support staff of research assistants, editors, administrative and clerical personnel. (The support staff was soon to be joined by a new group of specialists—computer programmers.) Harold Watts, an economics professor at Wisconsin who had trained at Yale University and had spent a year on leave at OEO's Division of Research and Plans, became the first director in June 1966.

Formative years: 1966–1971

Research at the Institute has illuminated the difference to the poverty count of different definitions of poverty, factors behind black and white income differentials, the impact of inflation on the poor, the relationship of migration to poverty, the role of health and education, and many other facets of the poverty problem. . . . The very strength of the Institute in economics has almost defined the mainline of research on the economics of poverty.

National Academy of Sciences, 1971[4]

Once established, the Institute rapidly built up a research staff and began to address the basic questions of poverty research: Who are the poor, and how many are there? How should we measure economic well-being, poverty, and inequality? What are the particular causes of poverty—discrimination, lack of education, poor workings of the market system, cultural factors?

By the end of 1969 the Institute's research staff numbered thirty: ten were economists and nine were sociologists. Other fields represented by research affiliates in the 1960s included political science, social work, law, education, rural sociology, agricultural economics, home economics, psychology, anthropology, and geography.

In addition to individual projects that covered the topics listed in the quotation above,[5] a large portion of Institute energies during the Watts directorship (1966–71) went into a major, pioneering group effort: the design, conduct, and analysis of the New Jersey Income Maintenance Experiment, soon followed by the Rural Income Maintenance Experiment.[6] These experiments studied the differential behavioral responses to varying minimum income guarantees between a randomly selected group of individuals who received benefits and a "control" group of randomly selected persons who did not. The experiments were important to the evolution of the Institute as well as to poverty research in general. Lampman commented in 1981 that it would be hard to imagine what the Institute would have been like without the New Jersey experiment, an outstanding example of interdisciplinary research in close cooperation with government planners.[7]

Journal of Human Resources

One of the valuable results of the formation of the Institute for Research on Poverty was the *Journal of Human Resources,* a quarterly containing rigorous economic analyses with policy implications. The *JHR* began publication in the summer of 1966. It is cosponsored by IRP and the Industrial Relations Research Institute of the University of Wisconsin–Madison.

In part because of its involvement with the complex design of this large-scale experimental undertaking, by 1971 the Institute had become a focal point of long-run research. It had a seasoned staff, a list of publications that included one hundred Discussion Papers, eighty Reprints, and six books, and was building a computing staff familiar with the new cross-sectional data sets that provided information hitherto lacking on the characteristics of low-income households—the Surveys of Economic Opportunity (1966–67) and the annual Current Population Surveys, conducted by the Census Bureau. On the horizon lay the promise of longitudinal data sets, permitting the study of individual behavior over time.

Because the Institute had been given a specific charge, to investigate the nature, causes, and cures of poverty, it evolved in a way that made it more than either a client of government or a program-oriented collection of researchers whose primary objective was, for example, to study antipoverty programs. Its dual purpose, to conduct basic research and to analyze government policy, was inevitably a source of tension, however: should the criterion for selection of a research topic be its advancement of academic knowledge—its contribution to a particular discipline—or its advancement of general knowledge about government social intervention? The two do not always or necessarily coincide.[8] This tension played a continuous part in the history of the Institute as it matured.

Cumulative research, 1971–1981

The existence at the institute of more than 50 social scientists with a large overlap of basic research interests is of itself a powerful force, enabling the kind of close personal contact among researchers that mutually educates and stimulates them.

National Academy of Sciences, 1979[9]

In 1971 Robert Haveman became director of the Institute. An economist who had been for several years on the faculty at Grinnell College, Haveman had also served as a staff member of the Joint Economic Committee of the Congress in 1968–69.

Although the IRP research program continued to build at an impressive rate, the 1970s were years when federal research budgets were tightened and enthusiasm faded for government action on economic and social fronts. The changing political climate in Washington momentarily clouded the Institute's future in 1973, when dismantlement of the Office of Economic Opportunity signaled the end of federal commitment to an institutional embodiment of the War on Poverty. The government nonetheless recognized the continuing need for policy analysis and evaluation research, and OEO's research functions were transferred to the Office of the Assistant Secretary for Planning and Evaluation (ASPE) within the Department of Health, Education, and Welfare, reorganized in 1979 as the Department of Health and Human Services. ASPE supported Institute work throughout the Nixon, Ford, and Carter administrations, and the Institute increasingly supplemented that support with grants from other private and public agencies, notably the state of Wisconsin, the National Science Foundation, and the Department of Labor.

To the regular Institute staff of faculty members with departmental appointments was added, through a postdoctoral program that began in 1973, a cadre of researchers with full-time, two-year Institute appointments. The staff was further enriched by visiting scholars, who began to arrive from other parts of the United States and from other countries as well. It is probably fair to say that the graduate students, postdoctoral fellows, and visiting researchers who have spent time at the Institute over the past twenty years and have since gone on to pursue their studies elsewhere or to work in government currently constitute, together with the present IRP staff, the core of the poverty policy research community.

Irwin Garfinkel served as director from 1975 to 1980. A faculty member at Wisconsin's School of Social Work, he had been encouraged by Harold Watts to study the labor supply effects of the income maintenance experiments, which determined the direction of his research interests.

The studies that were undertaken in these years advanced the social science disciplines while evaluating public programs. Measurement of economic status and social mobility by sociologists and economists constituted a major body of IRP work. Sociological studies examined the relative significance of ability, family influences, and schooling on adult achievement, while economists examined financial aspects of aids to education for poor students.[10] The accumulation of data and development of improved econometric techniques expanded the Institute's original focus beyond absolute income poverty to include relative income measures, assessment of pretransfer poverty, measures of poverty that accounted for in-kind benefits, development of the concept of "earnings capacity," analysis of equivalence scales to account for the different sizes and circumstances of families, and work on income inequality.[11] Historical perspectives were gained from a project measuring inequality in the United States from colonial times, and from a study that analyzed changes in the income distribution in the years following World War II.[12]

Econometric studies of the income maintenance experiments continued, concentrating on the issue of whether providing an income guarantee lowers work effort and on the effects of experimental program administration.[13] In the late 1970s, IRP affiliates became involved in the design and evaluation of the National Supported Work Demonstration.[14] Other studies focused on the models of the inheritance of IQ and on the particular problem of selectivity bias that plagued the social experiments.[15] The development of sizable, nationally weighted data bases and the advancement of computer capabilities permitted, by the end of the 1970s, creation of microdata simulation models designed to evaluate various effects of proposed income transfer and taxation programs.[16]

Another group of studies dealt with welfare law and administration and with the possibilities for integrating income maintenance programs.[17] Sociologists and political scientists analyzed the interconnections of race, segregation, discrimination, and political power.[18] Work on disability policy in the United States led to a cross-national comparison of such policies in industrialized states,[19] and in the same fashion a sociological analysis of class structure and factors affecting income in the United States led to a cross-national examination of social consciousness and class structure.[20]

The Institute's special competence came to be quantitative studies of large bodies of data. The growing collection of such data bases as the Current Population Surveys and the 1976 Survey of Income and Education extended the possibilities for more refined cross-sectional studies and, as data from the Michigan Panel Study of Income Dynamics and the National Longitudinal Surveys, among others, accumulated, longitudinal data gave insight into individual behavior and responses to social programs. Support from the National Science Foundation in 1978 made possible a landmark data-construction project: the production of microdata tapes from the 1940 and 1950 censuses that would permit, upon completion of the project in the early 1980s, comparability studies of social change over a forty-year span. And as these data bases grew, so did the Institute's computing staff, which in 1976 was grouped into a unit headed by a director of computing services.

In 1976 the Institute began publication of a newsletter, *Focus,* whose first issue stated that its purpose was "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." *Focus* and the other IRP publications reflect a theme that has pervaded its history—communication: among the representatives of the various disciplines that produce Institute studies, between the Institute and its sponsoring institutions, and between members of the academic community, the policymaking community, and the public at large.

In 1975 the Institute began collaborative research with the state of Wisconsin. The first project brought IRP staff

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The Institute is a nonprofit, nonpartisan, university-based research center. As such it takes no stand on public policy issues. Any opinions expressed in its publications are those of the authors and not of the Institute.

The purpose of *Focus* is to acquaint a large audience with the work of the Institute by means of short essays on selected pieces of research. A subscription form with rates for our Discussion Papers and Reprints is on the back inside cover. Nonsubscribers may purchase individual papers from the Institute at $3.50 for a Discussion Paper and $2.00 for a Reprint. *Focus* is free of charge, although contributions to the U.W. Foundation–IRP Fund sent to the above address in support of *Focus* are encouraged.

Edited by E. Uhr.

together with state personnel to study the causes of error in the administration of Aid to Families with Dependent Children.[21] It was followed by a joint study, beginning in 1977, to assess the role and scope of emergency assistance programs for the poor.[22] In 1978 Robert Haveman was appointed chairman of the state's Comprehensive Welfare Reform Study, which spawned a number of joint ventures in the ensuing years. Among them was a comprehensive survey of Wisconsin's low-income population, the Basic Needs Study (1978–82), which determined how much income is required for different-sized households in different locales to make ends meet; and the Child Support Reform project, which in 1978 began to explore possibilities for improving the system and led to the major demonstration now under way, described below. In 1979 the state and the Institute initiated a study of tax credits to employers who hire disadvantaged workers (targeted jobs tax credits).

In 1980 Eugene Smolensky, a professor of economics at Wisconsin, long an IRP research affiliate, and an expert in the measurement of inequality and economic well-being, became director. The beginning of his leadership, like that of Haveman ten years before, was marked by a change in political climate in Washington that generated uncertainty about federal support for the kind of studies that the Institute had conducted. In the period that began in 1981 the Institute diversified its sponsorship as well as its research interests.

Continuity and change, 1981–1986

> *Members of the Institute feel that because their organization has a history of pioneering work of scholarly merit and practical value, and because it is housed in a university which provides a rich mix of scholars—in economics, sociology, social work, demography, political science, education, psychology, and law—committed to the study of poverty issues, IRP should continue to seek to understand and solve the many problems related to poverty—problems that, however unfashionable, do not go away.*
>
> Focus, 1982[23]

In 1980 the issue of poverty in America seemed on the verge of eclipse. An IRP document referring to the situation in the late 1970s stated that "income poverty, as officially defined, has decreased dramatically since 1965."[24] And new methods developed at the Institute for valuing in-kind transfers indicated that poverty under this measure had experienced even stronger decline over the past fifteen years. Some observers argued that the variety of cash and in-kind benefits available to those in want had so diminished the incidence of poverty that the dominant issues had become improvement of program efficiency and elimination of dependency among the long-term poor.[25]

The situation soon began to change, however, and rapidly. In the face of inflation, two recessions, and retrenchment in social spending, the proportion of the population in poverty rose sharply after 1979. Growing concern accompanied steady increases in the numbers of the poor. In 1984 debate broke out anew over the question of whether welfare programs were the answer to, or the cause of, the persistence of poverty, or whether other factors should be assigned a prominent role. The topics that Institute researchers had probed for almost twenty years reappeared as priority items on the social policy agenda.

In 1981 the federal government relinquished the practice of dispensing core funding for the operation of a national center for poverty research, but Institute work continued with support from private, other public, and campus sources. In 1983 Congress, in part as a result of concern about increased poverty, partially restored funding by the Department of Health and Human Services for new IRP research projects. That support was renewed by congressional action in 1985. Since 1981 the Institute has increasingly supplemented DHHS funding with backing from other public and private agencies.

The cumulative program of the Institute moved forward in the 1980s as it had in the 1970s, following lines of research laid out earlier. Meanwhile new research questions emerged, and IRP affiliates turned their attention to previously unstudied areas, sketched below. The theme of these years can be characterized as continuity accompanied by change.

Innovation occurred at the staff level as well. Having undergone reductions in personnel after the federal core grant lapsed and the postdoctoral program ended, IRP began increasingly to draw on researchers at other institutions around the country. The Small Grants program awarded funds on a competitive basis for research on poverty-related topics conducted by social scientists not in residence at Madison. Twenty-one such projects were completed or under way in 1986.[26] In cooperation with DHHS, the Institute in 1984 sponsored a conference to assess past and future antipoverty policy.[27] The Institute also invited more non-Wisconsin researchers, many of them former pre- or postdoctoral associates, to collaborate in work on its federally sponsored projects.

The Institute was directed from mid-1983 on by Sheldon Danziger, a professor of social work, who had come to the Institute in 1974 as a postdoctoral associate after graduate study in economics at MIT, where he had become interested in the work of Robert Lampman and Eugene Smolensky on the nature of income poverty and inequality.

The tradition of measuring the level and trend of poverty continued with projects that utilized detailed information from the 1940 and 1950 censuses, making it possible to analyze changes in relative economic status among various demographic groups from 1940 to 1980.[28] The long experience of IRP in measurement studies joined its tradition of social program analysis in a project, sponsored by the Sloan Foundation, that analyzed the forces behind the trend in poverty, focusing on the separate roles of declining economic growth, increasing earnings inequality, and cutbacks in public transfers.[29]

The Institute's tradition of program analysis carried over to a large-scale project that monitored the changes in well-being of female-headed households as a result of 1981 rule changes in the Aid to Families with Dependent Children program.[30]

Other work moved forward in the areas delineated by IRP staff in earlier years: analysis of effects of the labor market structure on low-wage workers, to inform public policy toward disadvantaged workers;[31] examination of the relationship between disability and poverty;[32] the role of demographic change in increasing the risk of poverty among certain demographic groups, in particular the elderly and single mothers with children.[33] The discrimination and segregation studies of the 1970s were complemented in the 1980s by a major analysis of economic discrimination in American society, tracing its effects on racial, ethnic, and gender groups over time.[34]

Whereas in previous years particular aspects of the income transfer system in the United States had been analyzed at the Institute, Robert Lampman in a 1984 monograph (see list of books) offered the first comprehensive description of the costs and benefits that might be judged to result from the growth in all U.S. social welfare spending, public and private, since 1950.

The Institute's experience in managing and analyzing large bodies of data led to a novel undertaking in 1984–85: the establishment at IRP of a national data center to facilitate access to the new Survey of Income and Program Participation. Supported by the National Science Foundation, the center serves as a vehicle for communication between policy analysts and researchers and also permits continuing improvement in the quality of data through feedback to the Census Bureau from users.

The "Wisconsin idea" of academic service to the community continued in joint projects conducted by the Institute and the state and aided by grants from the Ford Foundation. In 1984 the Child Support Assurance Program began to be put to the test, piloting a reform designed to increase equity in the system and to help single mothers achieve self-support.[35] To measure the program's results, a survey designed at the Institute—CHIPPS, the Wisconsin Survey of Children, Incomes, and Program Participation—began in 1985 to gather data on the cost of the reform and public attitudes toward it.

Meanwhile, new topics were added to the Institute's agenda, including research on several minority groups that had not previously been featured in IRP work: Hispanics, immigrants, and Native Americans. A conference in 1982 on Hispanics in the U.S. economy was followed by a two-year project that examined the labor market experience and public assistance utilization of Hispanics, immigrants, and refugees.[36] A successor project is now examining poverty among Puerto Rican, Native American, and recent immigrants in relation to their migration patterns, employment status, and reliance on public transfers. A joint IRP-state project began in 1985 to evaluate the economic well-being of the 30,000 Indians in Wisconsin. And a national conference to be held in November 1986, sponsored by the Institute and the Ford and Rockefeller Foundations, will examine the causes and consequences of poverty among all minority groups in the United States.

Research innovation was also represented by a longitudinal study of the homeless, now in progress; by an extensive examination of the role played by charitable organizations in providing material assistance to the needy;[37] and by a privately sponsored study that estimated the potential savings from introducing computerized automation into the administration of welfare programs, Medicaid in particular.[38]

These are the highlights of the events and the work that have marked the history of the Institute. It seems appropriate to close this narrative sketch with a summary statement of what the Institute stands for, after twenty years of operation: first, commitment by its research staff to use the full resources of the social sciences to enlarge our understanding of the underlying causes and possible cures of poverty; second, selection of the research topics best suited to advance that purpose; third, strengthening the analytical tools and data bases that permit those studies to be conducted. The staff's commitment and resolve remain as strong as they were two decades ago. ■

IRP Directors

Robert J. Lampman, Interim, 1966

Harold W. Watts, 1966–1971

Robert H. Haveman, 1971–1975

Irwin Garfinkel, 1975–1980

Eugene Smolensky, 1980–1983

Sheldon H. Danziger, 1983–

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[1] "The Institute for Research on Poverty," in *Conference on Poverty Research, Communications, and the Public*, ed. Charles E. Higbie (Madison: University of Wisconsin Press, 1966), p. 122.

[2] The tradition dates back at least to the 1890s, when the historian Frederick Jackson Turner and the economist Richard T. Ely established on campus an interdisciplinary program that emphasized economic and social factors. It gained distinction as "the Wisconsin school" of history. A similar label was given two decades later to a branch of labor economics at the university: "the Wisconsin school [was] characterized by an intense zeal for public service and social reform coupled with both a critical attitude toward

traditional economic theory and a disregard for the conventional boundaries between the social sciences" (Paul J. McNulty, *Origins and Development of Labor Economics: A Chapter in the History of Social Thought* [Cambridge, Mass.: MIT Press, 1980], p. 171).

[3] Robert Lampman has provided a more detailed account of the creation of the Institute in "Can and Should Universities Help Government with Policy-Oriented Research?" *Focus* 7:3 (Fall 1984), pp. 8–10.

[4] *Policy and Program Research in a University Setting: A Case Study,* report of the Advisory Committee for Assessment of University-Based Institutes for Research on Poverty (Washington, D.C.: National Academy of Sciences, 1971), p. 17.

[5] The individual projects that were initially funded grew out of the research plans formulated by a faculty interdisciplinary group that met on campus from 1964 to 1966. Titled the Committee on Problems of Poverty and chaired by a member of the economics department, Martin David, the group was charged by the university's vice-president with examining "what the University ought to be doing in the poverty area." David became an IRP affiliate and is now co-director of the Research Network and Data Center housed at the Institute, described later in this article.

[6] The New Jersey experiment was conducted from 1968 to 1972. Its design, operation, and results are described in a three-volume report (see list of books). The results of the Rural Income Maintenance Experiment (1969–72) are summarized in IRP Special Report no. 10, "The Rural Income Maintenance Experiment" (November 1976, 97 pp.).

[7] Lampman, oral history interview, University of Wisconsin Archives, Oral History Project, October–November 1981.

[8] Eugene Smolensky elaborated on this topic in an interview published in *Focus* 6:3 (Summer-Fall 1983), pp. 13–14.

[9] *Evaluating Federal Support for Poverty Research,* report prepared by the Committee on Evaluation of Poverty Research (Washington, D.C.: National Academy of Sciences, 1979), p. 43.

[10] Robert Hauser and David Featherman collaborated on a number of studies (which appeared in the IRP Reprint series) concerning social stratification and social mobility, culminating in their two books, *The Process of Stratification: Trends and Analyses* (New York: Academic Press, 1977) and *Opportunity and Change* (New York: Academic Press, 1978). Robert Mare's work dealt with the sources of educational growth in America; several of his IRP Discussion Papers and Reprints were summarized in *Focus* 3:2 (Winter 1978–79), pp. 5–6. Michael Olneck investigated the relationship of schooling and success, also summarized in *Focus* (5:3 [Summer 1982], pp. 9–14). The economists were Lee Hansen, Burton Weisbrod, and Robert Lampman, whose work appeared in a number of IRP Discussion Papers and Reprints.

[11] In the list of IRP books, see works by Robert Plotnick and Felicity Skidmore, Marilyn Moon and Eugene Smolensky, and Irwin Garfinkel and Robert Haveman, with the assistance of David Betson; also, Timothy Smeeding, "Measuring the Economic Welfare of Low-Income Households and the Antipoverty Effectiveness of Cash and Noncash Transfer Programs," Ph.D. dissertation, Department of Economics, University of Wisconsin-Madison, 1975.

[12] In the list of IRP books, see works by Jeffrey Williamson and Peter Lindert and by Morgan Reynolds and Eugene Smolensky.

[13] In the list of IRP books, see that edited by Glen Cain and Harold Watts, and the three volumes on the New Jersey Income Maintenance Experiment (Vol. I is edited by David Kershaw and Jerilyn Fair, Vols. II and III by Harold Watts and Albert Rees).

[14] This research is summarized in *Focus* 5:3 (Summer 1982), pp. 1–6.

[15] Arthur Goldberger, "Statistical Inference in the Great IQ Debate," IRP Discussion Paper no. 301–75, and "Mysteries of the Meritocracy," IRP Reprint no. 196, 1975; Burt Barnow, Glen Cain, and Arthur Goldberger, "Issues in the Analysis of Selectivity Bias," IRP Discussion Paper no. 600–80 (also Reprint no. 425, 1981).

[16] In the list of IRP books, see the two volumes edited by Robert Haveman and Kevin Hollenbeck.

[17] In the list of IRP books, see those by Joel Handler, Handler and Ellen Jane Hollingsworth, and Handler, Hollingsworth, and Howard Erlanger; and the volume edited by Irene Lurie.

[18] In the list of IRP books, see those by Peter Eisinger; see also Karl Taeuber, Franklin Wilson, David James, and Alma Taeuber, "A Demographic Perspective on School Desegregation in the United States," IRP Discussion Paper no. 617–80, and Franklin Wilson and Karl Taeuber, "Residential and School Segregation: Some Tests of Their Association," IRP Reprint no. 333, 1979.

[19] In the list of IRP books, see those by Richard Burkhauser and Robert Haveman, and by Haveman, Victor Halberstadt, and Burkhauser.

[20] In the list of IRP books, see that by Erik Olin Wright.

[21] See Irving Piliavin, Stanley Masters, and Tom Corbett, "Administration and Organizational Influences on AFDC Case Decision Errors: An Empirical Analysis," IRP Discussion Paper no. 542–79.

[22] This study expanded into a larger one that resulted in an IRP monograph by Joel Handler and Michael Sosin (see list of IRP books).

[23] "IRP Still in Business," *Focus* 6:1 (Fall-Winter 1982), p. 6 [written by Elizabeth Uhr].

[24] IRP, "Grant Application Submitted to the Assistant Secretary for Planning and Evaluation, Department of Health and Human Services, 13 February 1981, for Poverty Research Center."

[25] This is the general tenor of Chapter 1 of Martin Anderson's *Welfare: The Political Economy of Welfare Reform in the United States* (Stanford, Calif.: Hoover Institution, 1978). The chapter begins by stating: "The 'war on poverty' that began in 1964 has been won" (p. 15), and ends: "We should now begin to think about how to revise our welfare strategies to deal with the problem of preventing poverty, to make programs more effective and efficient, to eliminate those programs that are not needed, and to focus more on the social problems that widespread welfare dependency will bring" (p. 39).

[26] IRP Discussion Papers that report the results of Small Grants projects include nos. 736–83, 748–84, 750–84, 758–84, 768–84, 773–85, 780–85, 796–85, 797–85, 799–85, and 803–86.

[27] The resulting conference volume was recently published under the title *Fighting Poverty: What Works and What Doesn't;* see list of IRP books.

[28] IRP Discussion Papers that report the results of this project include nos. 752–84, 759–84, 779–85, 785–85, 786–85, and 790–85.

[29] See Sheldon Danziger, Peter Gottschalk, Eugene Smolensky, and Daniel Weinberg, "The Effects of Unemployment and Policy Changes on America's Poor; and Comment," IRP Reprint no. 545, 1986, and *Income Transfer Policies and the Poor: A Cross-National Perspective,* special issue of *Journal of Social Policy,* Vol. 14, Part 3 (July 1985). See also Reprints nos. 494, 1984; 507, 1985; and 534, 1986.

[30] The results are summarized in *Focus* 8:1 (Spring 1985), pp. 1–8, and in Robert Hutchens, "The Effects of the Omnibus Budget Reconciliation Act of 1981 on AFDC Recipients: A Review of the Studies," IRP Discussion Paper no. 764–84. The studies are reported in IRP Discussion Papers nos. 742A-84, 761–84, and 763–84.

[31] IRP Discussion Papers that report the results of the project include nos. 753–84, 787–85, and the work cited in note 34, below.

[32] This project, conducted by Robert Haveman and Barbara Wolfe, resulted in Discussion Papers nos. 767–84 and 775–85 and Reprints nos. 499 and 500, 1984, and no. 527, 1985.

[33] See the works cited in note 30, above, as well as Discussion Papers nos. 697–82, 760–84, 761–84, 776–85, and 788–85.

[34] This work by Glen Cain is summarized in *Focus* 7:2 (Summer 1984), pp. 1–11, and 7:3 (Fall 1984), pp. 11–17.

[35] The program and IRP publications related to it are described in *Focus* 9:1 (Spring 1986), pp. 1–5.

[36] In the list of IRP books, see the conference volume edited by George Borjas and Marta Tienda. Results of the two-year project are described in *Focus* 8:3 (Fall-Winter 1985), pp. 14–21.

[37] In the list of IRP books, see that by Michael Sosin.

[38] Ralph Andreano, Eugene Smolensky, and Thomas Helminiak, "The Economics of Information Exchange and Automation in Third-Party Insurance: A Study of Medicaid in Wisconsin," IRP Discussion Paper no. 792–85.

Poverty research and the social sciences

by Robert Haveman

Robert Haveman is John Bascom Professor of Economics at the University of Wisconsin–Madison and was director of the Institute from 1971 to 1975. This article is extracted from his forthcoming book, *Poverty Policy and Poverty Research, 1965–1980.* The Russell Sage Foundation provided research support for the book. It is to be published in 1987 by the University of Wisconsin Press.

The War on Poverty and related efforts to create a Great Society are usually associated with community action programs, growing income support for low-income Americans, and a variety of direct interventions in the education and training area. In the 1960s and 1970s these policy efforts succeeded in reducing poverty; seldom, however, are they viewed as having a major impact on the academic community in the United States. Yet they did exert such an effect—a major social science research effort grew up beside, and partly because of, the War on Poverty.

Poverty research has made substantive contributions to social science knowledge and to academic practice and methods. In this article I have singled out advances in four areas: the field of policy analysis and evaluation research, social experimentation, econometric methods to deal with selectivity bias, and microdata simulation modeling.

Policy analysis and evaluation research

The notion that social scientists should be concerned with understanding how social policy interventions affect human behavior and well-being has early roots in all of the social sciences. Not all investigators, however, supported the inclusion of this objective for their disciplines, and many feared that close ties to policy concerns would erode basic progress in the disciplines themselves. Acceptance of the policy analysis role of social science was given impetus by the Progressive movement (which had roots in Wisconsin) at the turn of the century, which held that application of the scientific method to political problems could lead to more effective governmental performance.

Until World War II, what little policy research and evaluation existed was in the domain of sociology, psychology, and public health; economists and education researchers made few contributions. In the early postwar period, policy analysis and evaluation research was largely conducted by social psychologists, who studied, among other topics, the effects of antidiscrimination efforts on attitudes toward blacks and of public housing on health and social adjustment.

The War on Poverty played a major role in stimulating the large burst of policy analysis and evaluation research that occurred in the post-1965 period. Many of the early participants in designing and implementing antipoverty policies were social scientists—primarily economists—convinced that their research methods could assist government in analyzing its activities so as to expand the successful and weed out those that did not work. This faith also resulted in the 1965 presidential order establishing the planning-programming-budgeting system in executive agencies, a development which formalized the role of policy analysis and evaluation research within government.

The rapidly growing federal financial support of antipoverty policy analysts and researchers in government during the 1960s and 1970s was not ignored by academia. Its earliest response was to develop courses in applied policy analysis and evaluation research which emphasized the "science" of policy evaluation—experimental design, survey instruments and data collection, statistical analyses, causal modeling, decision models, benefit-cost analysis. Some of these courses already existed, or were established, within departments of economics, sociology, political science, and psychology. More often, however, such courses grew up in special policy analysis or evaluation research programs or in disciplines closely related to direct service provision, such as social work, public administration, urban and regional planning, public health, and education. In several cases, individual courses, programs of study, or training programs were initiated and supported by private foundations and governmental agencies who wished both to increase the analytical capabilities of existing staff and to provide a pool of new policy analysts and evaluation researchers from which to recruit. The funding offered by the government and foundations was readily accepted by universities and the social scientists involved. And, as related in the historical sketch in this issue of *Focus*, the Institute was born of this union of government and academic interest in the application of applied research and evaluation techniques to antipoverty policies.

These developments posed major challenges both to universities in general and to social scientists in particular. Most major universities have been forced to address the issue of whether or not to establish or expand a program of public policy studies. Many responded affirmatively, and public policy schools were established or expanded at a considerable number of institutions—to name but four, the John F. Kennedy School of Government at Harvard, the Graduate School of Public Policy at Berkeley, the Lyndon B. Johnson School of Public Affairs at the University of Texas, and the Institute for Public Policy Studies at the University of Michigan.

Interest in studying the effectiveness of social policy also stimulated development of private policy-oriented research within established nonprofit organizations, such as the Brookings Institution, the Rand Corporation, and the American Enterprise Institute, as well as new ones, such as the Urban Institute. And it led to the creation of a new private, for-profit, research industry specializing in applying social science research techniques to social policy measures.

Within the academic community in the mid-1980s, although it is difficult to document, one senses that the disciplines of economics and political science—and, to a lesser extent, sociology and psychology—are far more oriented to understanding and appraising what it is that government is and "should be" doing than they were in the mid-1960s. This concern has also spread to many disciplines and quasi-professional fields which are derivative from the traditional fields. It is the rare program, school, or department of urban and regional planning, education, public administration, business administration, health services, social work, and law that does not now have courses or concentrations devoted to the subject of policy analysis and program evaluation.

Social experimentation

The social experiments that began in the late 1960s represented both a major new social science research method and an important emphasis in policy analysis and evaluation research. Nearly all of them concerned aspects of antipoverty policy. Without the explicit declaration of a War on Poverty, this advance in methods would, at best, have been long delayed, and would surely not have evolved as it has.

Not long after the initial antipoverty programs began, policy researchers became discouraged regarding their ability to gauge the effects of the new interventions on recipients, to measure the benefits of programs and compare them with the costs, and to make cross-program comparisons of effectiveness.

At about this same time, a growing number of social scientists became interested in a research technique offering great potential for answering questions regarding the behavioral impacts of social interventions. The technique was the application of the experimental method of the natural and physical sciences to human subjects. The basic model was to identify a set of objectives of a social intervention; to design a program judged effective in attaining these objectives; to administer this program to a randomly chosen set of households; to measure the behavioral patterns of those subject to the intervention (the experimental group) relative to the patterns of those not affected (the control group); to adjust for any other factors not taken into account in the experimental design; and to attribute the remaining difference in behavior patterns to the intervention.

Suggesting controlled experimentation as a technique is easy; designing and implementing an experiment is costly and difficult. Among the questions requiring answers were such diverse ones as what interventions to examine, how to design the experiment—how large should be the sample size, how should its members be selected and assigned to treatment and control groups, how long should the treatment be administered—and, ultimately, how to value the results.

Although many problems were recognized, by the late 1960s a number of leading social science researchers enthusiastically supported experimentation, concluding that the gains exceeded the disadvantages, which included the large financial costs. The result was a series of experiments: New Jersey Income Maintenance, 1968–72; Rural Income Maintenance, 1969–72; Performance Contracting, 1970–71; Gary Income Maintenance, 1971–74; Seattle-Denver Income Maintenance, 1971–78; three experiments with housing allowances, 1973–77; National Health Insurance, 1974–81; the National Supported Work Demonstration, 1975–79; and the Employment Opportunity Pilot Project, 1979–81 (terminated before completion). The undertakings grew more complex through time: whereas the early ones involved relatively simple treatments with relatively straightforward hypotheses to be tested, the later ones involved more complex treatments, often with several interventions designed to be mutually supporting (e.g., income support plus counseling plus training). As a result, the findings of the later experiments were more difficult to interpret and, hence, carry less direct relevance to policy making, at least at the legislative level.

While it would be difficult to claim that all of this research, evaluation, and experimentation had a major impact on legislation and public policy, it did affect social researchers and social scientists in important ways. The following is an effort to characterize them.

• The experimental methodology of the physical sciences was carried into the social sciences to evaluate the activities of antipoverty programs and social policy agencies. In principle at least, social science gained access to the experimental technique, the lack of which had always caused it to appear "less scientific" than the natural and physical sciences.

• The procedures for and requirements of scientific experimentation involving human subjects and social policy treatments became a part of curricula in many standard social science departments and the focus of courses in schools of public policy.

• The scholars and researchers involved in the experiments—and their students—gained a form of knowledge, training, and experience of value to both government agencies and university social science departments. They are now scattered throughout government and universities.

• The experimentation movement, which was an important part of the trend within government to "contract out" research, stimulated the development of numerous profit and nonprofit research firms. These organizations continue to thrive and provide a demand for social science researchers and a supply of services which maintain a focus on rational study and experimentation for social policies.

• Social experimentation encouraged a substantial core of social scientists to retain a commitment to relevant applied research, as an antidote to the highly theoretical-mathematical research-for-its-own-sake emphasis, which has permeated the social sciences—in particular, economics—in the postwar period.

Selectivity bias

The 1970s saw major advances in statistical techniques that enabled social scientists to deal with an endemic problem in the analysis of social behavior—that of selectivity bias. This problem became an issue among econometricians largely because of its pervasive presence in the efforts to evaluate the behavioral effects of the social experiments and federal education and training programs. There was a close tie between poverty research and the development of statistical techniques for correcting selectivity bias.

Bias in estimated relationships is likely to occur when analysis is based on a sample of observations not representative of the larger population for which inferences are desired. This situation will occur when the sample on which estimates are based is composed of subjects who have self-selected themselves into the sample (because, say, they were highly motivated), or who have been selected to be in the sample by some unknown set of criteria, or, in the case of social experimentation, who have been assigned nonrandomly to various treatment categories or have left the experiment through attrition.

The work of econometricians and other applied social researchers in developing techniques to correct for selection bias has had a major impact on empirical economic and sociological research on the determinants of human behavior and on the evaluation of social and antipoverty programs. The techniques developed are at the frontier of econometric analysis, and have contributed to many of the most important evaluation studies and social experiments in the poverty research field. These include the performance contracting in education study; the New Jersey, Gary, and Seattle-Denver income maintenance experiments; federal manpower training programs; the Housing Allowance Demand Experiment; and the estimation of women's labor supply under income maintenance plans.

The sensitivity of researchers to the potential of selection bias in empirical research is now widespread, and the studies which incorporate statistical corrections for the selectivity problem number in the hundreds. These developments have occurred both within quantitative research unrelated to the War on Poverty—for example, in traditional labor economics—and in explicitly poverty-relevant research.

The impression should not be left, however, that these methods yield reliable and easily accessible solutions to the selectivity problems. Most are both difficult and costly to implement. At their core, all of the techniques rely on assumptions about the shape of the distribution of the underlying data. These assumptions, which typically involve normality or symmetry, are both strong and arbitrary; if they do not in fact hold, the estimated results may be at least as biased as making no correction for selectivity.

Although these problems inhibit greater application of the techniques, they nevertheless represent a major methodological advance. As with other such developments, the knowledge frontier for this generation of researchers will be a standard part of the research toolkits of the next generation. These methods exist in large part because of the important role played by antipoverty policy in both highlighting the problem and supporting the research from which this advance in technique emanated.

Microdata simulation models

The War on Poverty and the drive for more rational government policies together provided the stimulus for the development of microdata simulation models which trace the impacts of exogenous factors, including policy implementation, on individuals, taking into account both the characteristics of the individuals and of the policies. It was the demand for more sophisticated, reliable, and detailed estimates of the budgetary and economic effects of proposed social policies that gave rise to microsimulation modeling, aided by the development of computer resources required for the design and construction of this empirical tool.

Microdata simulation involves the creation of computer models designed to simulate the effects of proposed policy changes at very disaggregated levels—individuals, families, firms, industries, and regions. Use of the models enables investigators to examine the full distribution of the effects of particular combinations of policies, instead of working with averages and broad generalizations.

The extensive use made of the models and the estimates which they yielded in policy debates on poverty and social policy reforms both within the executive branch and in the Congress clearly stimulated their development and the interest in and resources devoted to them. The commitment of the academic research community to their development was substantial, in part because of the resources available for this work and because of the interest of policymakers in the results. Individual scholars and research groups at Stanford University, the Institute for Research on Poverty, the University of Michigan, the University of Southern California, Yale University, the Brookings Institution, and the Urban Institute were all actively involved in either developing their own models or in contributing to model development. Acronyms that named the models, some more pronounceable than others, entered the literature, among them DYNASIM (dynamic simulation), MERGE (combining two data files), KGB (Kasten, Greenberg, and Betson were its designers), and CHRD (Comprehensive Human Resources Data System).

Microsimulation has had a significant impact in the social sciences—particularly economics. First, it represents an advance in the frontier of predictive model building. The reliance on microunits and the need to model their behavior added a dimension not reflected in existing macroeconomic models. Data collection to fuel the new work expanded at a great pace. The challenges that the micro models posed for data handling and computer processing, model execution and solution, and the complex sequencing of simultaneous and recursive socioeconomic relationships stretched the capability of analysts.

Second, the ability of the models to incorporate econometrically estimated relationships—for example, income and net wage on labor supply; socioeconomic characteristics on consumption, migration, marriage, childbearing—increased the importance of reliable estimation of these relationships. Estimation of these determinants of behavior had been a long-standing focus of both quantitative economics and sociology. The greater availability in the 1960s and 1970s of the survey data essential to construction of microdata simulation models also increased the capability of social scientists to model and estimate these relationships. And, with the construction of the large models providing a demand and a home for such estimates, the effort given to such research efforts expanded substantially. The increased quantitative estimation of behavioral determinants in both economics and sociology after the mid-1960s, and the development of microdata simulation modeling during the same period, were complementary phenomena.

Finally, the capability of microdata models to provide detailed estimates of policy impacts on highly disaggregated groups met an important need during the late 1960s and early 1970s. As Robert Lampman has described, it was during these times that all policy initiatives, both those in the antipoverty social action area and those with more general objectives, were forced to answer the question, "What does it do for the poor?"

With the success of programs and program proposals contingent in part on the answer given to this question, microdata simulation became viewed as the primary research tool capable of providing the desired information. Daniel P. Moynihan has cogently described the forceful effect of model introduction:

> By early 1969 a simulation model had been developed which permitted various versions of FAP [the Family Assistance Plan] to be "tested" and costs to be estimated. Most of this work was done by The Urban Institute, which made its information available to all who requested it. Thus, in time the Congress was to have before it the same data as the executive branch had worked from. So did persons outside government, persons for the program and persons against it. This was a situation probably without precedent in the development of major social legislation; it disciplined and informed the debate for those in any degree disposed to restraint in the discussion of public issues. Once the president had made the proposal, and congressional hearings were beginning, the Administration could in good conscience make statements about the effects it would have which never previously could have been made with any pretense to accuracy.[1]

Given the nature of the policy issues to which these modeling efforts were primarily addressed—welfare reform, food stamps, employment subsidies, public employment, income transfer policy, tax reform—it can scarcely be doubted that the War on Poverty and the policy emphases to which it gave rise had an important role in stimulating these developments in the social sciences.

Long-term advances

The events that I have recounted in these four areas cumulatively affected the course of social science, altering the topics on which research was undertaken, the methods of that research, and ultimately the state of our knowledge in these fields. The intense concentration on antipoverty measures that set these events in motion has faded, but there can be no doubt that a substantive contribution to both social science knowledge and method has been made, and that it is on these advances that the next generation of research efforts will build. ■

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[1] *The Politics of a Guaranteed Income: The Nixon Administration and the Family Assistance Plan* (New York: Random House, 1973), p. 190.

Poverty research in international perspective

by Alfred J. Kahn

Alfred J. Kahn, a member of the Institute's National Advisory Committee since 1967, is professor of social policy and planning at the Columbia University School of Social Work. His recent work has dealt with international comparisons in the handling of social problems. Among his publications on this topic are two works with Sheila B. Kamerman: *Child Care, Family Benefits, and Working Parents* (New York: Columbia University Press, 1981) and *Income Transfers for Families with Children: An Eight-Country Study* (Philadelphia: Temple University Press, 1983).

There has been no European equivalent to the poverty research carried out over the past twenty years at the Institute for Research on Poverty. Fundamental differences in social policy and in the interpretation of poverty within that policy have inevitably shaped different research agendas. The differences, of course, are of degree and have become less important over time. Events of recent years have created considerable social policy convergence and, not surprisingly, research programs also have become more similar. Interesting cross-Atlantic collaborative work has begun.

What follows is impressionistic and personal. The relevant literature is limited. There has been little formal U.S.-European interchange about the advantages of or strategy for poverty research or its alternatives, although one notes some new interest.

Discovering and measuring poverty

Poverty reached the agendas of several major Western industrial societies in the latter part of the nineteenth century as researchers, journalists, novelists, religious reformers, social workers, and others began to document and report on the causal role of social and economic conditions. They attacked the notion that failure of people and their personal inadequacies were always at the root of economic disadvantage. Gradually "poverty" was distinguished from the moral category, "pauperism," and was counted. Charles Booth reported on London[1] and B. Seebohm Rowntree on York[2] before Robert Hunter[3] and others surveyed U.S. cities. But a tradition was born.

Early in the 1900s Rowntree introduced a subsistence concept of poverty. Incorporated and adopted by W. M. Beveridge,[4] it generated the intellectual and policy tradition which led eventually to the Orshansky poverty line,[5] now the Census Bureau poverty index. In this tradition one determines what it takes to keep the body adequately, but not generously, fed; one takes account in one way or another of the need for clothes, transportation, and many other things; perhaps considers housing separately—and thus sets a minimum standard. Those whose income (sometimes net and sometimes gross) falls below that standard are said to be poor. In its current Census Bureau version, adjusted annually to reflect the changing Consumer Price Index, this "absolute" poverty line is the basis for official statistics as well as controversy. It may be destined for reform, for the reasons suggested by Harold Watts (see his article in this issue) among others.

The institutional response

Social security in its broadest sense (coverage for retirement, survivors, health, invalidity, and disability) was invented in response to the needs of the working population for protection against the risks and uncertainties of the industrial-urban system. Its rationale goes well beyond the poverty question, even though it prevents poverty for many. For those who fell between the cracks or whose social security entitlements yielded too little, various assistance and supplementary benefits schemes were to take over. The Europeans started down this path well before World War I and many nations had well-developed systems before the United States passed its 1935 Social Security Act.

The differences go beyond pace, however. In all societies, the nineteenth-century insight that not all poverty is "pauperism" is lost and relearned periodically. There are also differences in perception of social security protection as relevant to all "average" people. The Europeans in general have been more comfortable with an emphasis on universal social benefits and programs. Thus they have added family allowances, statutory maternity benefits, and (in some cases) extensive housing allowances to the social security package as well. Much of the increase in social protections came after World War II, in the name of solidarity. In a sense Europe moved in the 1950s toward the kind of welfare state enhancement that the United States was only to start in the mid-1960s, and the Europeans began on a higher plateau.

For reasons frequently discussed, the reluctant U.S. initiatives are more often remedial, means tested, and categorical. Such programs are constantly called upon to justify themselves in ways never demanded of universal programs. This need has yielded our more extensive poverty research.

The prosperity of the 1960s also had differing social policy manifestations: In Europe there was considerable benefit and social protection enrichment, including efforts to equalize family burdens and concern in some countries with redistribution and with decreasing income inequality. The United States saw the filling in of some notable gaps in its basic social welfare system. The formal U.S. slogan was "equality of opportunity"; a "poverty war," not a concept of universal protection, became the rationale for a series of actions. The poverty effort, in its formal sense, was organized largely around educational, social service, community, and training programs, many of them remedial. The more significant direct antipoverty measures (transfers) in fact came through expanded participation in Aid to Families with Dependent Children (AFDC) and the development of the Food Stamp program.

At the same time and quite separately, one major social insurance gap was filled in with Medicare, and a health program for the poor was created—Medicaid. Before the mid-1970s, when the poverty war in its formal sense was phased out, we indexed social security and federalized public assistance for the aged, blind, and disabled by creating Supplemental Security Income (SSI).

Poverty research

The U.S. antipoverty effort required a research capacity to provide needed intelligence and assessment in order to continually justify the societal response. The Institute for Research on Poverty was established for this purpose. Although a number of small antipoverty research "think tanks" came and went in West Europe, none had the IRP's sanction, mission, and scale—because none of the other countries had focused its social welfare strategy quite as sharply upon the antipoverty objective. Countries such as West Germany, the Netherlands, and Denmark did not consider poverty as a large problem or good issue around which to shape policy research. Britain, France, and others did far more along these lines.

In general, the relevant European research focused more on income distribution, redistribution, and equality as basic issues—and as subjects which were important to labor market policy and to debates about the size of social benefits. Many investigators studied the comparative adequacy of benefits (child allowance value, pension replacement rates, unemployment insurance replacement). The Organisation for Economic Co-operation and Development (OECD) developed an important and continuing series of comparative studies on the tax/benefit position of the average worker.[6]

Fortunately, from the beginning the IRP staff never defined their challenge as poverty research in the narrowest historical tradition. The Institute developed a basic research program to parallel its work on trends and measurement and its evaluations of social experiments. Its contributions on income distribution, income transfers, and basic research in related social policy areas are familiar to present readers and to Europeans. But its linking of basic policy research to poverty remained extensive and almost unique.

The increasing U.S.-European convergence of the late 1970s and 1980s may be ascribed both to dissemination and to new shared concerns. The American antipoverty research, particularly the IRP work, has influenced some European scholars. The economic downturn and crisis of social expenditure led to a series of remedial projects by the European Economic Community (EEC) and programs (which required evaluation), and to a search for policy with regard to vulnerable groups.[7] Other social and demographic changes have reinforced the process. In various ways Europeans and Americans have become concerned about the poverty risks and related problems of the long-term unemployed, young workers with no access to jobs, the low-income elderly, growing numbers of single-parent families, migrants; Europeans in some countries would add large families to this list.[8] While no European country has undertaken a large-scale poverty research program, more applied poverty research has been carried out, specifically focused, than previously. Moreover the International Labour Office (ILO) and the OECD have commissioned several symposia and modest comparative studies. They have funded and assessed antipoverty projects financed by their social, regional development, and agricultural funds.[9] (These projects, in fact, recall the U.S. poverty war more than the more universal European traditions!)

How have these recent European efforts defined poverty? The answers will be familiar to Americans:

• Most countries (and, thus, most studies) define poverty with reference to a public assistance or supplementary benefits standard. People eligible for aid are "poor." The problems are apparent: variation within and between countries in assistance standards and the manner in which they are set.

• A few studies have followed Beckerman's use of a percentage of per capita disposable income, often choosing an inevitably arbitrary percentage which is close to the public assistance standard.

• Others select a similarly arbitrary percentage of disposable household income.

(The latter two approaches employ an equivalence scale for family size.)

Most countries do not have a poverty line with the policy and administrative applicability assigned the poverty line in the United States. Where public assistance levels are employed they may have empirical roots or historical rationale, lost with adjustments over time. Some countries begin with a minimum wage as an income transfer anchor figure, as France and the Netherlands long did, and this number plays a poverty research role, too. Others also have index numbers, politically set, whose historical rationales are no longer reflected in the actual benefits, but which are the key to a variety of benefit systems—and assessments of benefit adequacy.

Peter Townsend, a British sociologist whose poverty research is extensive and long term, has not been satisfied with absolute poverty lines and has advocated and illustrated a multifaceted effort to conceptualize and measure relative deprivation.[10] In his view one is in poverty if unable to command the resources, access, and rights which are essential to full participation in one's own society. Townsend's arguments are conceptually strong, and his specific work impressive, but there is no evidence that he can solve problems of reliability and validity over time or between investigators, or utilize and operationalize his concepts for between-country comparisons.

New initiatives

The diversity of conceptualizations, preoccupations, and research solutions is illustrated in fascinating detail in a recent report of a 1984 international meeting at which IRP researchers and European scholars assembled to discuss the effects of economic policies on the economic well-being of the poor in their home countries. The introduction comments as follows:[11]

> The goal was to provide an estimate of how income transfer policies in the late 1970s and recent economic changes have affected poverty or income inequality and the work effort of the poor. A concerted effort was made to ensure that the definitions and computational procedures were comparable. What is most remarkable about these papers is that, while all are faithful to the principles guiding the conference, they differ remarkably from one another. They vary in the questions pursued and the methods chosen. In that diversity lies a major lesson—evaluating the redistributive effects of the policies of an administration is a new task for economists and there is as yet no agreed-upon methodology to accomplish it. (pp. 257–258)

An observer is not surprised. The differences in social policy contexts and, therefore, in research over two decades have had their effects.

The convergence of concern about social welfare expenditure and vulnerable groups, and the shared experience with new ideological and programmatic challenges to welfare state traditions, are, nonetheless, yielding some interesting new developments. Obviously, recent progress in data storage and processing are also central determinants.

Readers of IRP publications and other poverty research will recall that there have continued to be advocates for a relative poverty line. What is now the U.S. Census Bureau poverty line for a family of four was 49 percent of the median U.S. income of a family of that size in 1959, 42.3 percent in 1964, 35.2 percent in 1969, and 34.9 percent in 1983. Advocates of a relative line would report the percentage of the total population or of specific demographic groups below a constant percentage of the median over time, usually 40, 42, or 50 percent. From such an approach one gets a very different view as to progress against poverty or a lack thereof over time. The relative line asks whether the poor are partaking of a country's greater wealth and productivity as reflected in average incomes.

The relative line is readily defined and standardized for international comparisons. An American-European team, drawing upon the Luxembourg Income Study (LIS), has adopted 50 percent of the median income as one poverty standard (and the percentage of families in a given category who are in the lowest quintile of the distribution as a "low-income" standard), and has begun to report comparatively on seven countries.

The LIS, a collaborative effort which began in 1982, made its public debut at a 1985 conference at which a series of papers illustrating its possibilities were presented. Currently, most major welfare states have the capacity to use household survey data (in the words of Working Paper No. 1) "to describe the effects of existing policy and simulate the effects of changes in policy."[12] A group of international collaborators has assembled and coordinated a databank of income surveys relating to 1979–81 and resolved a series of complex conceptual, definitional, and procedural issues to launch this seven-country, cross-national effort. Several other countries are being added to the databank and the resource is now available to researchers. Plans are under way to include 1984–86 data. The next several years are secure, and longer-term plans are being developed.

As the LIS effort is refined and more analytic work is published, the comparative picture of income composition and distribution will be enriched and the phenomena of inequality and redistribution will be better understood. In this connection one early product is a new, cross-national, comparative poverty picture based on the relative poverty line.[13] The data refer to the late 1970s and early 1980s. Table 1 illustrates the possibilities.

Whether the relative line is 50 percent or 40 percent of the median income—or some other proportion—one sees some encouragement to regular reporting of relative as well as country-specific yet comparable absolute poverty in the future. One also notes the likelihood that, stimulated by comparative reports, European and U.S. investigators will look more intensively at their own countries as research covering poverty, income distribution, social benefits, expenditures, and redistribution expands its vocabulary and perspectives. This will be welcomed by those who follow such research for policy purposes or see its relationship to the basic development of their social sciences. ■

Table 1

Relative Poverty and Low Income

	Rates of Relative Poverty, by Age Group			
	All	Under 24	65–74	75+
Canada	12.1	22.7	11.2	12.1
Germany	7.2	14.5	12.7	15.2
Israel	14.5	11.7	22.6	27.1
Norway	4.8	16.8	2.7	7.3
Sweden	5.0	17.3	—	0.0
U.K.	8.8	11.6	16.2	22.0
U.S.A.	16.9	28.0	17.8	25.5
Mean	9.7			

	Rate of Relative Poverty, Children			
	All	Single-Parent	Two-Parent	Other
Canada	16.8	51.2	13.2	13.0
Germany	n.a.	n.a.	n.a.	n.a.
Israel	18.6	23.8	13.5	32.3
Norway	5.6	13.7	3.7	10.7
Sweden	5.2	9.8	5.5	2.5
U.K.	10.4	36.4	9.2	6.2
U.S.A.	24.1	60.0	16.6	20.6

Source: Stein Ringen, *Difference and Similarity: Two Studies in Comparative Income Distribution* (Stockholm: Institute for Social Research, 1986). Reproduced with permission.
Note: Percentage of persons/children belonging to families with family-equivalent disposable incomes below one-half of the median for all families. There are several caveats in the original. The missing German rate for poverty for all children is 6.3 percent (see Smeeding, Torrey, and Rein in note 13 of this paper).

1 *Life and Labour of the People of London*, 4 vols. (London: Macmillan, 1892-1903).
2 *A Study of Town Life* (London: Macmillan, 1901).
3 *Poverty* (New York: Macmillan, 1904).
4 In the Beveridge Report: *Social Insurance and Allied Services*, Report of the Inter-Departmental Committee on Social Insurance and Allied Services Consisting of Opinions and Recommendations Only (London: HMSO, 1942).
5 Mollie Orshansky, "Counting the Poor: Another Look at the Poverty Profile," *Social Security Bulletin*, January 1965, pp. 3-26.
6 OECD, *The Tax-Benefit Position of a Typical Worker* (Paris: OECD, 1974, 1978, 1980, and periodically thereafter).
7 See, for example, EEC, *Final Report from the Commission to the Council on the First Programme of Pilot Schemes and Studies to Combat Poverty* (Brussels: EEC, 1981); EEC, *One-Parent Families and Poverty in the EEC* (Copenhagen: EEC, 1982).
8 See, for example, Jonathan Bradshaw and David Piachaud, *Child Support in the European Community* (London: Bedford Square Press, 1980).
9 Wilfred Beckerman, *Poverty and the Impact of Income Maintenance Programs* (Geneva: ILO, 1979); Victor George and Roger Lawson, *Poverty and Inequality in Common Market Countries* (London: Routledge and Kegan Paul, 1980).
10 See Townsend, *Poverty in the United Kingdom* (Berkeley: University of California Press, 1979), and Robert Walker, Robert Lawson, and Townsend, eds., *Responses to Poverty: Lessons from Europe* (London: Heinemann Educational Books, 1984).
11 Sheldon Danziger and Eugene Smolensky, eds., *Income Transfer Policies and the Poor: A Cross-National Perspective*, special issue of *Journal of Social Policy*, Vol. 14, Part 3 (July 1985).
12 Timothy Smeeding, Gunther Schmauss, and Serge Allegreza, "An Introduction to LIS," Center for Population, Poverty and Policy Studies, Working Paper no. 1, Luxembourg, July 1985.
13 Several papers have been circulated: one by Timothy Smeeding, Lee Rainwater, and Martin Rein from the United States, Richard Hauser from Germany, and Gaston Schaber from Luxembourg, "Poverty in Major Industrial Countries," Center for Population, Poverty and Policy Studies, Working Paper no. 2, Luxembourg, July 1985; another by Smeeding, Barbara Torrey, and Rein in the United States, "The Economic Status of the Young and Old in Six Countries," Center for Population, Poverty and Policy Studies, Working Paper no. 8, Luxembourg, July 1986; and a third by Stein Ringen in Sweden, *Difference and Similarity: Two Studies in Comparative Income Distribution* (Stockholm: Institute for Social Research, 1986). These reports compare poverty rates by country and differences between the young and old. Smeeding, Torrey, and Rein have also converted both the U.S. and U.K. absolute poverty lines into national currencies by using the "purchasing power parities" developed by OECD.

Poverty and poverty research, then and now

by Nathan Glazer

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Quite early in my association with the Institute for Research on Poverty—it may well have been at the first meeting of the National Advisory Committee I attended many years ago—I argued there were things about poverty we would not understand without ethnographic research. I had in mind such books as Elliot Liebow's *Tally's Corner,*[1] which had just then been published, and which provided the kind of detailed understanding of individual motivation and perception that could come only from sustained contact and observation. This was admittedly typical of the advice a member of an advisory board, without direct responsibility for spending research funds or making appointments, generally makes. That was not the way the Institute for Research on Poverty was then going, or was to go, and in retrospect I understand two things: One is that there were very good reasons why it could not go that way; and the second is that from the point of view of necessary and key understandings of poverty, ethnographic research is still, difficult as it is, one type of research we must encourage.

The IRP was from the beginning cast in the mold of econometric and public policy research. This was the most productive mold in which it could be cast. Research models were available. One could analyze, using reliable statistical techniques, the influence of one factor on another: of welfare on job-search, or family composition and family decisions; of variants of welfare, experimentally designed or proposed, on these key determinants of poverty. This was the research the IRP was prepared to undertake, and that it did undertake, as in its major series of volumes on income maintenance experiments. This work has played an enormous role in our thinking about poverty, and what we can do about it, and it has played a major role in the policies that are proposed and adopted to deal with poverty. No other line of research could have been as productive: reason enough for this concentration. So there can be no argument with this research. Statistical bases were available, in the census, in the Panel Study of Income Dynamics, and in other data bases developed over the years. A mode of analysis was available. Reliable techniques for distinguishing the effects of different factors on a variety of outcomes were available. This research also fitted in with the mood of the later 1960s in policy research and in politics: it was a mood characterized by confidence that solutions to difficult problems were available, whether the problem was dependency, or dropping out of school, or urban decay. It was a mood too that pretty much pushed aside less tangible elements in affecting human behavior, such as values, or upbringing, or the sense of responsibility, or character. One will not do very well at finding these elements in the census or longitudinal data series. One can struggle with trying to "operationalize" such factors, but one generally comes up with something that is not very satisfying. Recall the controversy over the use of an item on "self-image" in James Coleman's *Equality of Educational Opportunity.*[2] It seems that a good self-image, scored on the basis of one question, correlated with academic achievement. But then what? Did this mean that self-image was improved by academic achievement? Or that a good self-image helped improve academic achievement? Did it mean that students should be encouraged to think well of their achievement by teachers even if they did abysmally —and that then we could expect them to do better? This could be carried to extremes, as in *Pygmalion in the Classroom:*[3] fool the teacher into thinking well of the student. It seemed to make some sense: thinking better of oneself, within limits, will help achievement, and undoubtedly thinking better of oneself will help some people get out of poverty. But can we reliably find out how people think of themselves? Can we determine whether they think well of themselves because they do well, or the reverse? Can we design policies based on this modest connection, which involves so much uncertainty? Hardly. My example is drawn from quantitative research, but research which tried to make use of an insight that came from ethnographic research, or from psychological research, quantitative or otherwise. But one can understand from it why the IRP tended to do little of this kind of research. The economists and their style of work dominated, and few psychologists, political scientists, (nonquantitative) sociologists, or anthropologists were connected with the IRP.

Why it went this way is clear enough: it was a high-return route. And yet the understandings that are necessary to deal with poverty are elusive. Despite the wealth of quantitative findings, something was missing, and what was missing were intuitively comprehensible models of behavior based on detailed and sustained observation and interaction with the subjects of research, and which, when presented to the searcher after understanding of poverty, leads him to say, "Aha! That's just the way it is." The economist and the quantitative sociologist will enter a proper caveat, pointing out we can't be sure that's just the way it is (what would the next observer say?), or that that is just the way it is with the next group of street-corner men or welfare mothers. Despite the caveats, it is this kind of intuitive understanding of the social and psychological mechanisms that sustain poverty that the econometric and public-policy models didn't communicate well.

There were more than methodological difficulties in the way of expanding the role of ethnographic research. The fact is this is the most demanding kind of research—and people don't do it generally more than once. The classics of ethnographic research tend to stand alone, and there are not many of them. The reason I did not add a stream of other studies besides *Tally's Corner* is that there are unfortunately not that many. Finding a good ethnographic researcher is a hard thing: they are, it seems, born not made. Directing them into the time-consuming and demanding task of sustained interaction with people in trouble is not that easy either. And yet the payoff from such research when it works is tremendous. Susan Sheehan's *Welfare Mother* [4] teaches me things about welfare problems, and Ken Auletta's *The Underclass*[5] things about work-training programs, that very well designed evaluations do not. (Of course the evaluations can tell us things no ethnographic research can tell us.)

But what does the ethnographic research tell us? Having made such a pitch for such research, it seems incumbent on me to draw out the additional quantum of understanding that derives from such research. One seems to find two contradictory things in ethnographic research. First, it tells us that we can easily understand other people, whatever their differences in status and fortune, because all human motivations are the same. But then we also discover that people are different, and can be very different. When we see what calculations a woman on welfare makes as to how much effort to put into work or how much to put into trying to get more from the welfare grant, we conclude that is just the calculation we would make in that situation: rationality explains all, economic man (or woman) is to be found even on welfare. When we see how Liebow's street-corner men live, we can conclude, yes, in view of their opportunities or lack of them, that is the way any rational man would behave. But we find other things that mystify us, too: the insistence on working hard even when the rewards are smaller than those available from welfare, or the taking of a self-defeating course (such as teenage pregnancy) even when what seem to be better and more satisfying alternatives are available.

There are elements in human behavior that seem to be based on history, or religion, or a kind of distilled experience, that the economist might sweep together and sum up under the heading "tastes," that nevertheless explain a good deal of behavior and a good deal of poverty—or escape from poverty. These elements, which ethnographic research makes us alert to, are playing a larger role in our thinking about public policy, and in experimental approaches to dealing with poverty, and will inevitably play a larger role in research. We can call them values, or commitments, or the effects of family upbringing, or inherited beliefs, or character, but they form a hard substratum under the social landscape that we try to manipulate in public policy with a calculated combination of reward and punishment. Sometimes that substratum helps get people out of poverty, and sometimes it keeps them there, despite well-designed policies.

I have concentrated in these comments on an individual—or if you will, family or community—level in understanding poverty, even though it is clear that prosperity (as in Eastern Massachusetts) will do a great deal to overcome poverty regardless of people's habits or values or orientations. And yet even an unemployment rate of 4 percent with the easy availability of jobs does not eliminate the role of self-defeating behavior in creating poverty: it has not had a great impact on teenage pregnancy, on dropping out of high school, or on dropping out of the labor force. (Maybe it will do all this in time.) And on the other hand even unemployment rates of 10 percent seem not to overcome the immunity of others to this kind of self-defeating behavior.

We are entering a period—we are in it now—when, I believe, these kinds of differences are going to play a larger and larger role in poverty and poverty research. We will be forced to confront them as a new age of mass immigration brings into the United States new groups that will demonstrate they can make economic progress even in times of adversity, as well as other groups who will apparently be incapable of emerging from poverty even in times of prosperity. Whatever our success in macroeconomics, for which I earnestly hope, we will have to work directly on human motivation operating in ways that we do not fully understand. That is what we are doing now, after all, with teenage pregnancy. Twenty years ago we hardly considered the pregnancy of young unmarried women as a factor in poverty: we concentrated on larger issues: jobs and income maintenance. Yet in the meantime, teenage pregnancy has become one of the major factors in poverty, and neither the availability of jobs or of welfare, some of our best-informed poverty researchers assure us, seems to have had much to do with it. (Charles Murray argues otherwise, but David Ellwood and Lawrence Summers dispute him, and in this standoff I will remain neutral.[6]) But if indeed our policies had little or nothing to do with this single largest change in the character of poverty in twenty years, then something else did. And what could it have been, aside from a change in what was valued and approved behavior? Weak as this explanation appears before the power of economic reasoning, it is all that is available for those who are now trying to deal with this disastrous development. And if the largest single change in the character of American poverty escapes economic analysis and large-scale correlations and regressions in our efforts to understand it, we have a good argument for other kinds of research on poverty. ■

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[1] Boston: Little, Brown, 1967.

[2] Washington, D.C.: GPO, 1966.

[3] By Robert Rosenthal and Lenore Jacobson (New York: Irvington, 1985).

[4] Boston: Houghton Mifflin, 1976.

[5] New York: Random House, 1982.

[6] Charles Murray, *Losing Ground: American Social Policy, 1950–1980* (New York: Basic Books, 1984); David T. Ellwood and Lawrence H. Summers, "Poverty in America: Is Welfare the Answer or the Problem?" in Sheldon H. Danziger and Daniel H. Weinberg, eds., *Fighting Poverty: What Works and What Doesn't* (Cambridge, Mass.: Harvard University Press, 1986), pp. 78–105. For a discussion of the controversy, see *Focus* 8:3 (Fall-Winter 1985).

Have our measures of poverty become poorer?

by Harold W. Watts

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The official measures of poverty in the United States are based on statistical conventions adopted in the mid-1960s and modified only slightly since then. By "official," I mean only that the measures are used and regularly published by the Census Bureau and have received the blessing of the guardians of the statistical standards in the Office of Management and Budget. By "statistical conventions," I mean to convey the notion that our measures are *not* grounded in some self-evident principle or expert consensus but are simply a collection of more or less arbitrary and eminently vulnerable rules. Their most remarkable feature is their widespread and persistent acceptance by the public and by those who make and criticize public policies.

The measures

The most widely reported poverty measure is the percentage of persons in poverty. This is obtained by counting persons in families and unrelated individuals with annual money income (as defined by the Census Bureau) below "poverty thresholds," which vary by family size and composition. (Although this "poverty rate" is the most commonly cited measure, separate rates for family units and for unrelated individuals are also available and were more widely used in the past.) The poverty thresholds are held constant in "real" dollars by adjustments reflecting changes in the annual average Consumer Price Index. The data on income and family status for the annual tabulations come from the March Supplement to the Current Population Survey (CPS), which estimates annual income for the preceding calendar year for individuals in sample households at the time of the March survey.

The association of a binary poverty status (each person is either in or not in poverty) with the CPS sample permits a wide range of tables and subgroup poverty rates to be compiled. At least two reports are published each year in the P-60 (Consumer Income) series of *Current Population Reports.* In recent years a "near poverty" status has been recognized in these tables by counting those whose incomes are no more than 25 percent above the poverty threshold.

The poverty thresholds now in use, the so-called "Orshansky" lines,[1] were the result of multiplying a set of prescriptive food budget totals developed in the Department of Agriculture by a factor of 3.0, which represented conventional wisdom (and some evidence) about the share of total expenditures used for food by those in the lower third of the income scale.

This rather inelegant but plausible structure of thresholds replaced an even cruder measure, which simply used $3000 (in 1964) for all family units and $1500 for individuals. The Orshansky thresholds yielded about the same overall poverty rates in 1964 as the $3000 measure, but made explicit allowance for the size of family units and their age and sex composition. For whatever reason, or lack of it, the Orshansky thresholds were adopted and have proved extremely durable in the face of a great deal of scholarly criticism. No serious rival has appeared with a combination of sound conceptual basis *and* a set of thresholds sufficiently different to make a change worthwhile. To the extent that alternative derivations of threshold levels arrive at more or less the same answers, we can simply add another plausible rationalization for sticking with the familiar Orshansky lines.

There have been many criticisms of the existing measures of poverty by those who have considered them carefully, though few users of the statistics have thought very much about how they are obtained. Sometimes the criticisms are focused on the thresholds and sometimes on the income data that are used with them. Strong arguments have been raised by Fuchs and others that the thresholds should be "relative" and move with median income instead of being fixed in real terms.[2] The implicit equivalence scales embodied in the thresholds for differently composed families have also been challenged on the ground that the scale effects are not the same for food and nonfood consumption items. More fundamental reforms have urged separate standards of adequacy for each major consumption category, e.g., food, housing, clothing, transportation, medical care; with a rule for declaring poor any unit below standard in one or more of the categories.[3] Another line of argument seeks to elicit income or spending norms from the answers to survey questions about what it takes to "get along" in the current social and economic environment.[4]

On the income side, a good deal of current attention is being given to the issue of nonmoney or in-kind transfers as well as to deferred benefits such as pensions, both of which are ignored by the Census money income concept. The pretax nature of the Census measure has also been criticized. A persuasive case can be made, most recently by Ruggles and Ruggles, for using a consumption measure instead of income for comparison with a poverty threshold that is usually rationalized in terms of the consumption that can be afforded at the threshold.[5] The exclusion of assets or liabilities from consideration in the reckoning of disposable resources is another notable gap in the procedure, and one can further challenge the easy acceptance of annual income aggregates instead of flows measured over longer or shorter periods. The validity of the measures obtained from the Current Population Survey can also be questioned, particularly in view of the substantial imputations required by the shortfall in incomes reported, compared to plausible control totals.[6] Even more damaging is the fact that the previous year's income total for a household may be poorly matched to the group of persons in the household in March. The comparison of a poverty threshold based on the March household composition with the previous year's income of those persons may yield a highly fictive picture of their individual or collective experience during that previous year.

Deterioration of the measures

These and many other basic criticisms of the poverty measures all have some merit, the amount in each case depending on how one conceives poverty as an abstract condition and on how far one wants to go in perfecting measures that can estimate a given concept. But another kind of question is being asked in this essay. Have our poverty measures become increasingly out of touch with the one or more concepts that they seemed reasonably suited for ten years or so ago?

In a number of important respects they *have* become worse as indicators of the material conditions under which our least fortunate families and individuals try to work out satisfactory lives. As we have seen, the measures themselves have been based on an array of poverty thresholds that are "fixed in real terms." We must therefore consider how the world may have changed to make this fixed strategy less appropriate. I suggest there are four ways in which the existing measures have deteriorated. First, the thresholds themselves have become more remote from the "mainstream" of living levels generally obtained around the median income. Second, during the past two decades important noncash benefits have been instituted that are not counted in Census money income. Third, taxes of various kinds, partly under the impact of nonindexed inflation, have begun to take an appreciable share of poverty-level incomes, and this is also ignored by Census money income. Fourth, the increasing amount of divorce and the generally changed constellation of household types suggest that annual income within a single household may be, for many persons, the wrong time unit or income unit from which to assess current poverty status.

Distance from median income

When the poverty thresholds were first set out, they were, for the four-person prototype, nearly half of median income of a family that size. By now they are more like a third. They have declined substantially relative to the money income levels commanded by the mainstream. If it is granted that poverty is properly related to some sort of "distance" from the prevailing consumption norms—that relative deprivation is the cause of the many social ills we associate with poverty in a modern society—then it is probably time for a realignment or even adoption of a more explicitly relative set of thresholds. (See Kahn's article in this issue.) I sincerely doubt that the alternative of maintaining a fixed-in-real-terms or so-called "absolute" threshold is a viable alternative for a long period—i.e., a half century or more (unless median income itself stagnates). Assuming some real growth in living standards, a fixed poverty line will sooner or later be ignored as silly or irrelevant, and higher standards will be adopted under some guise.[7] The only choice is between gradual and spasmodic adjustment. However one comes out on that issue, it is about time to make an adjustment that gets us closer to the relative standards adopted in the 1960s.

In-kind income

Noncash benefits have become a more important resource for low-income consumption. During the last twenty years, food stamps, Medicare, and Medicaid have become the most important, but housing subsidies and employment-related benefits (largely employer-provided health insurance and pension contributions) have also grown in coverage and importance. A substantial literature has been produced recently dealing with the issue of how these benefits should be valued.[8] This is an easy question for food stamps but is much more difficult for the medical reimbursement programs and retirement entitlements. There is also no agreement as to how far one should go in including such services as public education, libraries, and other amenities among the measures available to meet "private" needs. For the most part no one has yet faced the issue of how the poverty threshold concept, which was "designed" for comparison with Census money income, should be adjusted for use with a more comprehensive measure of resources. Some statistical opportunists appear content to add new resource components without considering any change in the norm of adequacy. Carried to absurdity, this procedure could completely eliminate poverty on paper by, e.g., attributing the cost of prison maintenance to the families of convicts or imputing some reasonable value for keeping the ambient atmosphere in a breathable condition. In fairness, I would note that the recent literature has stayed well short of such extremes. Assuming that some reasonable ground rules can be established, it seems clear that inclusion of some kinds of noncash benefits is overdue. The neglect of much if not most of the fruit of public policy of the last two decades by our poverty measures simply cannot be defended. Neither the gains from expanding such benefits nor the losses from their recent retrenchment have been reflected. This should be corrected even if all of the puzzles about valuation and true incidence cannot be resolved.

Taxes

Another problem with the use of Census money income is that it is a pretax measure. Whatever the reasons why the Census Bureau has opted against estimating posttax or disposable income, it didn't seem to make much difference so long as there was no income tax levied below the poverty lines. In recent years, however, the zero-tax point has fallen below poverty for many families, particularly when state taxes are considered. This clearly causes an overestimate of the resources available for regular consumption and should be corrected (or else the thresholds should be raised to allow for tax liabilities).

It seems to me that disposable income would be a better concept for many distributional issues, including poverty. Relative norms that might be specified as a percentage of the median income would make more intuitive sense if stated in terms of the after-tax income that most people are familiar with.

Demographic change

Much has been made recently of the apparent "feminization" of poverty. A very large proportion of the total poverty population is in households headed by women, and the share has grown rapidly in the last decade. But there has also been an increase in divorce rates and an increase in the number of "small" households and a concomitant decrease in subfamily units, "undoubling," suggesting a change in dwelling patterns. The poverty measures implicitly assume that living separately means living independently. But with the CPS, we do not measure interhousehold transfers very well. If these are important, then another source of consumption power that is becoming more prevalent than it used to be is being largely overlooked when measuring poverty. Regular money transfers such as alimony or child support are supposed to be measured in Census money income but are probably not very reliably estimated. Irregular and/or in-kind sources are deliberately excluded. This then is another example of how a change in demographic patterns may make a minor flaw in the income measurement conventions into a more important gap that can bias both measures of level and change in poverty status.

The uses of poverty measures

It is relatively easy to make a case that the poverty measures have deteriorated, but the more difficult problem is deciding how they should be changed. One aim might be to restore to poverty statistics something like their original meaning—produce a 1985 model of the Orshansky thresholds for comparison with Census money income or some derivative measure that can be realized in the CPS data. Alternatively one could recognize the inherent inadequacies of that approach along with the opportunities afforded by newly available data to engage in a more fundamental overhaul of our poverty measures. While my own inclination goes toward major overhauls, I do not want to scorn an Orshansky update. That approach has maintained a high degree of acceptance, and for a limited but important range of objectives could continue to be almost as good as more elaborate alternatives. It may be useful here to consider alternative uses of poverty measures and criteria, because for many purposes a relatively crude measure is quite satisfactory.

In this discussion I am primarily concerned with measurement of poverty as a social indicator. By this I mean a quantitative scale that allows meaningful comparison over time, among geographical areas, or across groups of individuals defined by economic, social, or demographic characteristics. In such measures a bias that is fairly constant, even though large, can be tolerated because it is the change or contrast that is of interest. Some of the problems noted for the current measures would have produced fairly constant or minor bias in the absence of change in, e.g., demography or policy.

Measures based on the nose counts are fine for considering whether, or where, or for whom, poverty may be getting better or worse, but as "test scores" for program effectiveness they may have some disadvantages. The reason is that programs may "play to the test" by selecting persons or households close to the poverty threshold to receive their treatment. Many such families will move out of poverty without any treatment, but the program can claim a rescue anyway. The same strategy would urge program evaluators not to select those who are far below the threshold because they are almost certain to be unable to escape poverty by themselves and may be either very expensive or very difficult to rescue by a program. This is an inherent drawback of a nose-count measure relative to, say, a "gap" measure, which can record any improvement, even if it is insufficient to raise income over the poverty threshold. Even gap measures can be faulted for giving a constant value to dollar gains below the threshold and zero for any gains above it. Again, alternatives can be devised that are more suitable as explicit targets for antipoverty programs. Perhaps our social indicators would also be more valuable if they measured income gaps on some other scale that distinguishes degree of severity for poverty. In many cases the indices will move together, but they might distinguish those changes that reflect improvements for the poorest of the poor.

Poverty measures can also be adopted as eligibility criteria for public programs. This is one of the most demanding purposes imaginable. It can almost be said that any criterion that is capable of mass application to data that are routinely collected (as in the CPS) is necessarily too crude and approximate for fair application on a clinical basis. There are many more considerations relevant both to the "true" poverty status of a person and to that person's suitability for a particular program's benefits or other ministrations than can be covered in a practical poverty statistic. An antipoverty program may seek to alleviate obstacles to employment by improving literacy, but it shouldn't rely on the correlation between poverty and illiteracy to screen out clients. Illiteracy itself should be the criterion.

An even more doubtful purpose to which poverty measures may be put is to *define* benefit levels or maximum benefits on an across-the-board basis. Well-designed programs should be sensitive to local variations in prices or other factors and to differential incentive effects. Some situations *should* be treated more or less generously relative to the poverty threshold in order to achieve related but not identical objectives—e.g., discouraging migration to high-cost locations or encouraging greater work effort through fractional benefit-reduction rates. The statistical measures of poverty should be reserved for after-the-fact and universe-level assessments and not for benefit designation.

A strategy for overhauling our poverty measures

In view of the different possible uses of the poverty indicator, it is probably bad strategy to aim at one all-purpose measure. It would be nice, or at least orderly, to have all measures related to one basic abstract notion of what poverty is, however. Alternative measures of the same notion may be in order simply because different amounts and kinds of information are available in different situations. For example it may be possible to secure much more information in a "clinical" or program situation than in preparing tabulations of social indicators from general survey data.

It must be recognized that no measure is going to be perfect or fair in all uses. At the same time any measurement convention must be regarded as subject to change or revision when there is a change in the data base on which a measure is based. Indeed, one of the strongest reasons for reconsidering the poverty measures at this time is the availability of new income, program, and expenditure data from the Survey of Income and Program Participation (SIPP)[9] and the Consumer Expenditure Survey (CEX).[10] The newly established continuous CEX promises to yield a data base that will allow research to be carried out on the possibility of using a consumption measure. Such a measure would avoid some of the problems inherent in the income measure, such as the adjustments for assets, liabilities, and changes in household size. Experience suggests, however that measurement of consumption is relatively expensive, and it does not seem likely that a shift to consumption standards is feasible in the near future.

It would be useful as well as standard statistical practice to maintain the earlier, CPS-based measures for a time to provide experience with their difference. But both could be regarded as measures in pursuit of the same abstract concept.

A proposed strategy is then to enunciate a basic concept that seems suitable as a description of the goal of existing measures (at least when they were established) besides being satisfactory as an abstract guide for establishing new and improved measures. Using that concept, more specific suggestions can be made for exploiting the new surveys and other opportunities for bringing poverty measures as social indicators into closer conformity with the abstraction of poverty.

Consider the following example of an abstract poverty concept on which alternate measures can be based:

> Poverty is a shortage of disposable, fungible resources (measured as a money flow) that prevents regular and continuous access to the minimal necessities of everyday life for all members of an economic household (spending unit).

This concept implies that poverty is to be a matter of degree—the shortage can be larger or smaller. It supposes that "minimal necessities of everyday life" will depend on social norms which surely evolve with the living standards of the entire society. It places primary importance on consumption levels but recognizes diversities in taste and requirements that are accommodated in a mixed market economy by consumer choices constrained by disposable resource flows. It is not implied that current earnings or even money income are the only source of such resources, however. It seems to me that this basic concept is consistent both with the vernacular notion of poverty and with the poverty measures used for the past twenty years. It is also consistent with the idea that the existing measures have become less complete and defensible for reflecting such poverty, as the world has changed the environment in which they operate.

How can the measures of concept be renewed with the richer data now available and allowing for the economic, social, and demographic change of the past two decades? There are several steps in this process. The first is to reach a pragmatic consensus on how noncash items should be treated. Some of these, such as food stamps and modest-sized housing subsidies, are generally as nonconstraining as cash and can be so regarded at their face value. The medical reimbursement programs are more difficult, but my earnest suggestion is that they be ignored whether they are provided by public programs such as Medicare and Medicaid or by employers as a part of compensation. It is consistent both with the past practice and the abstract concept to regard most medical reimbursements as coverage for *extra*-ordinary needs. It places medical "security" in the same category as public education as having important external effects along with private benefits. We have managed to understand poverty fairly well without accounting for educational benefits; we can do the same with medical benefits. The alternative in both cases is to make some very dubious calculation of the "value" of these eminently nonfungible resources to add to the fungible ones. To be consistent the calculation should be made for all persons—not just the poor—and in the end one has a measure that is very remote from anyone's direct experience. There may be a few other noncash programs that supply ordinary necessities that could be included, such as energy rebates, but I would urge restraint unless major distortions seem likely.

The problem of accounting for interhousehold *private* transfers of both cash and "daily needs" such as food and child care is another matter, and one on which very little progress has been made. The SIPP and CEX both provide some information and could provide more, but so far no research has been done toward including such resources in our poverty or disposable income estimates.

The adoption of an after-tax measure of resources would have its greatest effect on the higher-income categories, of course, but unless or until the tax thresholds are raised above the poverty thresholds, it will also affect the measure of poverty. The adoption of a disposable-income basis for all kinds of distributional analysis, including poverty, would be a major step forward. With the new income surveys it is possible to measure income on a monthly basis for persons and the households they occupy. These surveys would provide in each month an estimate of disposable resources that would include the value of those in-kind benefits which meet basic everyday needs, transfers in money or in kind from other households, and a deduction of income taxes paid (or accrued). Ideally, the tax would include both federal and state income taxes and perhaps an imputation in states where sales taxes are used instead of income taxes.

I would also urge that attempts be made to estimate the consumption value of fully paid-for durable goods and owner equity in a primary residence. For the older cohorts particularly, a major part of their daily needs is met by a fully amortized dwelling and an inventory of durable goods. By contrast, age-mates lacking such assets but having the same money income are much worse off in their ability to afford food, fuel, and clothing purchases. A rule for imputing income to nonearning assets more generally might be proposed ("rainy day dissaving") in some cases, but this is a complex issue deserving more study.

With a monthly measure of disposable resources in hand, the next need is for a poverty threshold that can serve as a standard. I would suggest a two-phase approach here. First, the Orshansky structure could be brought up to date by aligning it with half of the median monthly disposable resources for nonaged four-person households. The remaining categories represented in the current structure would be proportionally adjusted. Using this set of monthly thresholds, a "welfare ratio" could be calculated for each household by dividing its disposable resources by the threshold. This ratio could also be attributed to each member of the household as a measure of individual resource adequacy.

Second, using individual monthly indicators of the adequacy of resources, an analyst could construct a variety of measures of poverty. One could be based on 12-month averages, another on the number of months below standard. Periods shorter or longer than a year could be aggregated or summarized. Moreover each of these measures, being associated with an individual, could be tabulated by the individual's characteristics or by household or other environmental characteristics.

More elaborate indicators could also be developed from the basic monthly data. The household resource deficiency or "gap" could be shared out to individuals and then summarized as an indicator of the severity of a person's poverty. Functions of the welfare ratio which reflect a sharply increasing hazard as income is proportionally more deficient could also be devised and could provide a better criterion for evaluating antipoverty policies than the existing nose-count indicators.

For purposes of program eligibility or benefit determination, the specific measures proposed above would be almost as bad as the present ones. The abstract principle *could* be interpreted and applied in some cases using the more detailed and "intimate" evidence of resource deficiency that may be available in a more clinical or programmatic setting, but the earlier stipulation against routine adoption of the indicators still holds.

In the longer run the thresholds themselves may require further reconsideration. Research is being carried on to assess the merit of survey-based expressions of consumption and income norms.[11] These techniques may eventually provide a sound and broadly acceptable basis for setting the level of norms as well as the structure of equivalences needed to cover the full range of household situations. But at the present time results are too preliminary and tentative for early adoption.

Conclusion

There are several ways in which the much criticized but quite durable poverty measures have deteriorated since 1965 as measures of general capacity to consume at the social minimum. Because there is also an opportunity to bring new kinds of data into the measure, now is an especially good time to consider how the measures might be changed. It appears that most of the weaknesses can be corrected and our poverty measures enriched without doing major violence to the intuitive and vernacular notion of what it means to be "poor." ■

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[1] Named for Mollie Orshansky, who first devised them. See Orshansky, "Counting the Poor: Another Look at the Poverty Profile," *Social Security Bulletin*, January 1965, pp. 3–26.

[2] Victor Fuchs, "Redefining Poverty and Redistributing Income," *The Public Interest*, no. 8 (Summer 1967), pp. 88–95. See also the article by Alfred J. Kahn in this issue of *Focus*.

[3] Dwight Frankfather and Lois Thiessen Love, "Poverty and Public Obligation," University of Chicago, 1986, mimeo.
[4] Theo Goedhart, Victor Halberstadt, Arie Kapteyn, and B. M. S. van Praag, "The Poverty Line: Concept and Measurement," *Journal of Human Resources*, 12 (Fall 1977), 503–520.
[5] Nancy Ruggles and Richard Ruggles, discussants of paper by John McNeil and Paul Ryscavage, "Evaluation of Noncash Benefits," in *Bureau of the Census First Annual Research Conference, Proceedings*, pp. 393–398 (Washington, D.C.: GPO, 1985). See also Ruggles and Ruggles, "The Integration of Macro and Micro Data for the Household Sector," *Review of Income and Wealth*, forthcoming.
[6] For a discussion of the problem of underreporting, see *Current Population Reports*, Series P-60, no. 146, "Money Income of Households, Families, and Persons in the United States, 1983," pp. 217–219.
[7] For a contrary view see Robert J. Lampman, *Ends and Means of Reducing Income Poverty* (Chicago: Markham, 1971), pp. 51–56.
[8] See, for example, Timothy Smeeding, "Measuring the Economic Welfare of Low-Income Households and the Antipoverty Effectiveness of Cash and Noncash Transfer Programs," Ph.D. dissertation, University of Wisconsin–Madison, 1975; and U.S. Department of Commerce, Bureau of the Census, *Alternative Methods for Valuing Selected In-Kind Transfer Benefits and Measuring Their Effects on Poverty*, Technical Paper no. 50 (Washington, D.C.: GPO, 1982). A "Conference on Measurement of Noncash Benefits" was conducted by the Census Bureau, December 12–14, 1985, in Williamsburg, Va.
[9] SIPP is a longitudinal survey being carried out by the Census Bureau. It includes 26,000 households, who are being interviewed every four months for two-and-a-half years. The survey provides monthly data on income, transfers, assets, liabilities, and individual characteristics. SIPP Access, the research network and data center for SIPP, is located at the Institute.
[10] The CEX is a joint project of the Bureau of Labor Statistics and the Census Bureau. First fielded in its present form in 1972–73, it was repeated in 1980–81 and is now a continuing survey. It provides data on expenditure patterns, savings behavior, income, assets, and liabilities. It consists of two parts: a diary with daily recordings, and a quarterly survey.
[11] See, for example, Diane Colasanto, Arie Kapteyn, and Jacques van der Gaag, "Two Subjective Definitions of Poverty: Results from the Wisconsin Basic Needs Study," *Journal of Human Resources*, 19 (Winter 1984), 127–138; and Sheldon Danziger, Jacques van der Gaag, Michael Taussig, and Eugene Smolensky, "The Direct Measurement of Welfare Levels: How Much Does It Cost to Make Ends Meet?" IRP Reprint no. 503, 1984.

From Introduction by Sheldon H. Danziger and Daniel H. Weinberg to *Fighting Poverty: What Works and What Doesn't:*[1]

In 1964 no official estimates of the nature or extent of poverty in the United States existed, nor was poverty a focus of government studies or programs. In the aftermath of the Great Depression of the 1930s, poverty commanded little academic attention and few legislative initiatives explicitly designed to aid the poor were proposed. The situation changed dramatically in the 1960s. John Kennedy, influenced by the poverty he observed while campaigning in West Virginia and by contemporary accounts of the plight of the poor, directed his Council of Economic Advisers to study the problem. After Kennedy's assassination, Lyndon Johnson accelerated the work of the Council and, in his first State of the Union speech in January 1964, declared war on poverty. Shortly thereafter he announced a set of companion programs designed to enhance the general welfare and create the Great Society.

In the next decade, as a result of these initiatives, new programs were introduced and old programs were expanded; the emphasis of the federal budget shifted from military spending toward social welfare spending. The prevailing view during that period was optimistic. . . .

That optimism soured as the war in Vietnam replaced the War on Poverty in the headlines and helped destroy faith in the government's ability to solve any problem. Arguments that social problems could not be solved by "throwing money" at them and that the antipoverty attempts had failed were increasingly popular.

By the late 1970s two revisionist views were heard. One suggested that even though the earlier efforts had reduced measured poverty, they had not provided sufficient opportunities for the able-bodied poor to earn their way out of poverty. We had nevertheless learned enough from this experience to reorient antipoverty policy. A second view argued that social spending had grown too large and had become a drag on economic growth. Income poverty had been "virtually eliminated," but work incentives had been eroded for both the poor and the rich, and the incentive to save had been weakened. As a result, these programs should be scaled back or eliminated. By 1982 the latter view had become official policy: "With the coming of the Great Society, government began eating away at the underpinnings of the private enterprise system. The big taxers and big spenders in the Congress had started a binge that would slowly change the nature of our society and, even worse, it threatened the character of our people . . . By the time the full weight of Great Society programs was felt, economic progress for America's poor had come to a tragic halt" (Reagan, 1982). Rather than ask what government could do for the poor, official policy now emphasized what it could not accomplish and how it could be counterproductive.

In 1984, twenty years after the declaration of War on Poverty, the facts were clear—social spending had increased rapidly in real terms and as a percentage of the Gross National Product (GNP), yet poverty as officially measured had declined little. But these facts do not speak for themselves. Simple comparisons of spending trends and poverty trends obscure the diversity of the poverty population and the complexity of evaluating government policies.

[1] Danziger and Weinberg, eds., *Fighting Poverty: What Works and What Doesn't* (Cambridge: Harvard University Press, 1986), pp. 1–2.

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