Poverty and poverty research, then and now

by Nathan Glazer

Nathan Glazer is professor of education and sociology at Harvard University and an editor of *The Public Interest.* He has been a member of the National Advisory Committee of the Institute since 1968. Among his major monographs are *Beyond the Melting Pot,* with Daniel P. Moynihan (Cambridge, Mass.: MIT Press, 1963) and *Affirmative Discrimination: Ethnic Inequality and Public Policy* (New York: Basic Books, 1975).

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. ■

---

[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).