University of Wisconsin-Madison
Institute for Research on Poverty

Focus

Volume 13

Number 3

Fall and Winter 1991

ISSN: 0195–5705

What happens to youth after high school?

by Robert M. Hauser

Professor Hauser is Vilas Research Professor of Sociology and Director, Institute for Research on Poverty

For the past two decades and in the foreseeable future, the key educational transitions among American youth have occurred and will occur during middle to late adolescence. These transitions include, but are not limited to, high school dropout or completion and entry into colleges, universities, or other postsecondary schools. They are a key to the quality and productivity of the future work force because they are the main points at which youth now leave the educational system for work, military service, family formation—and in some cases street or prison life. For the past several years, public attention in the United States has focused mainly on the first of these transitions—high school dropout—but the transition from high school completion to whatever may follow is and will be the most important decision point in the American educational system. High school completion is the single point at which the most Americans leave schooling.[1] It is the point at which the largest share of the cost of schooling shifts from public to private hands—even though there is massive public funding for postsecondary schooling. It is the point that determines access to the kinds of jobs that are and will be most in demand in the American economy of the twenty-first century.

Wage differentials are growing between the college educated and persons with some college or a high school diploma or who are high school dropouts.[2] After increasing from the middle 1960s to the middle 1970s, the real earnings of male high school graduates declined through the middle 1980s. The earnings of high school dropouts relative to high school graduates also declined. After the middle 1970s, the relative earnings of men with college experience took off. Those for college graduates rose most rapidly, from about 20 percent more than the earnings of high school graduates to 40 or 50 percent more than the earnings of high school graduates.[3] There is every reason to believe that these differentials are a valid reflection of the growing demand for a highly educated work force, that they will continue,[4] and that they provide sound and compelling

evidence of the need to monitor and foster the transition from school to the labor market.

What do we know about transitions out of high school in the United States? How do we know it? Do we know enough? And how can we learn more? I will begin with a brief overview of federal data sources and programs. Then I will outline recent trends in adolescent educational transitions, focusing on differences in these transitions among racial and ethnic groups. While I might have chosen some other set of differentials for special attention—for example, gender or income differences—I think that racial and ethnic differences are of particular importance, both because of their obvious relevance to issues of equity and equality of opportunity, and because of their implications for the future American economy. The demographer's stock in trade is the explanation of differences by population composition. If minorities are less successful in educational transitions than majority whites—or even if improvements in the status of minorities occur slowly—the growing share of minorities in the American population will itself reduce the educational quality of the future work force.[5]

Federal data resources

America's youth are the neglected stepchildren of the federal statistical system. The part of the U.S. statistical system run by the Bureau of the Census ignores almost all people in military service in its regular surveys, so we learn nothing about them except yearly estimates of their total number by age, race, and sex. Thus, we do not know each year how many high school graduates enter the military service, yet the absence of these data clouds our estimates of rates, trends, and differentials in entry into the civilian labor market and into postsecondary schools. While reductions in the size of the armed forces reduce these problems, they are compounded by changes over time in the selectivity of entry into and exit from the armed forces. It is a long-standing tradition that the Department of Education, in its longitudinal surveys of student populations, pays too little attention to labor market outcomes, while the Department of Labor, in its surveys of youth, pays too little attention to schooling processes. None of these agencies integrates its statistical activities with those of the Bureau of Justice Statistics, which is itself unable to produce a mutually exclusive and exhaustive account of persons controlled by the criminal justice system. Thus we are bombarded with loud and specious comparisons of the numbers of minority youth in prisons and in colleges.[6] Neither is there integration of education, labor, or justice statistics with those on health and illness.[7] The overall effect of fragmented responsibility and piecemeal coverage is that, once youths leave high school, our statistical system treats them almost as if they had dropped off the face of the earth.

For policy purposes, we need to know what populations are exposed to the risk of each educational transition, the characteristics of persons that affect the transitions, and the timing and outcomes of the transitions. There are three main sources of regular federal data on adolescent education: the institutional data collection programs of the National Center for Education Statistics (NCES); the Current Population Surveys (CPS) of the U.S. Bureau of the Census, especially the October and March surveys; and various supplemental survey programs of the NCES.[8]

The main data collection programs at the NCES obtain information about enrollment counts, institutional resources, and diplomas or degrees awarded. Because educational institutions provide the data for the main statistical series on secondary and postsecondary education (through the School and Staffing Survey and the Integrated Postsecondary Educational Data System), almost no information about students is obtained other than race-ethnicity and sex, and there is simply no way to use these data to assess or analyze educational transitions. The National Assessment of Educational Progress (NAEP) provides increasingly important and detailed measurements of academic performance, but the NAEP is a set of repeated cross-sections—not a longitudinal survey of persons—and its measurements of individual social and economic characteristics have been limited and difficult to analyze.

The CPS of the Bureau of the Census is a large national survey—currently covering about 55,000 households each month—and each October it fields an educational supplement that ascertains the school enrollment status of persons aged 3–34. Unlike the institutional data from the NCES, the CPS covers the nonenrolled population as well as persons who are enrolled in school. The CPS obtains important personal characteristics other than race-ethnicity and sex, most notably age, grade or year in school, year of high school completion, and enrollment status in the previous year. For children who are living in their parents' households or who are living in group quarters while away at school, the CPS data permit us to attach the social and economic characteristics of parents to those of their children. Thus, for some populations we can relate school enrollment and progress to family income, to single-parent households, to the number of children in the household, and to parental education, labor force status, and occupation.

There are major problems in using the CPS data to measure adolescent educational transitions: the samples become excessively small and statistically unreliable when we try to focus on key transitions, especially among minority groups; family income is not measured well; we lose the link between parents and children when children leave their parents' household; the CPS does not cover persons in the military or in institutions, like prisons and jails, that now house a substantial minority of young adults; it tells us little about the schools or colleges in which students are enrolled; and recent content changes in the CPS have reduced the usefulness of the October data. Some of the problems of the CPS are inherent in its design; its primary purpose is to measure labor force participation and employment, and

budget and design decisions are most heavily influenced by the needs of the Bureau of Labor Statistics.

The NCES also has survey programs that provide important data about adolescent educational transitions. These include the National Post-Secondary Aid Survey (NPSAS), which is a longitudinal study of postsecondary school entrants, and a series of occasional longitudinal surveys that begin with students in the eighth, tenth, or twelfth grade. By construction, the NPSAS misses the transition from high school completion to whatever follows; that is, it picks up students who have made the transition to some form of postsecondary schooling and cannot help us understand who does and does not make that transition or what the role of public policy may be in fostering it.

The other longitudinal surveys of the NCES have been initiated once per decade since the early 1970s. The first was the National Longitudinal Survey of the Class of 1972 (NLS–72), which started with twelfth graders. The second was the High School and Beyond survey (HSB), which started with tenth and twelfth graders in 1980, thus covering the high school graduation class of 1982. The third is the National Educational Longitudinal Study of 1988 (NELS–88), which started with eighth graders in 1988. Thus, its members will not graduate from high school until 1992, and under current plans, their success in postsecondary schooling will not be surveyed until 1994. It will be late in the 1990s before we have a detailed post-1982 analysis of transitions beyond secondary school.

The NCES longitudinal studies are well designed and well executed. They begin with very large samples of schoolchildren—25,000 in the base-year sample of the NELS–88—and they obtain extensive measurements of academic performance, school and social environments, and family background. They have been valuable in basic scientific and policy research, and they have occasionally been useful in monitoring trends in educational transitions. Still, there are two main limitations to their use in public policy: They provide only one reading per decade on changes in educational transitions, and—perhaps because of this occasional character—they are not sufficiently comparable in design to permit unambiguous trend comparisons.[9] Think about the fact that we measure the unemployment rate every month, but our only good measurements of the transition from high school to college occur once per decade. My main suggestion for the future development of federal educational statistics is that we establish a parallel set of smaller, annual, school-based longitudinal surveys that will begin in the eighth, tenth, or twelfth grade, be constant in design and content, and fill in the gaps between major decennial surveys.

In response to a congressional mandate, the NCES has also initiated some efforts to improve the measurement of high school dropout. As I understand it, this began with an effort to design the National Household Education Survey (NHES), but the costs of screening households for potential high school dropouts are so great that the NCES can be expected to rely on the CPS to measure dropout in the near future. In its initial phase, the NHES will focus on early childhood education.

Why doesn't the NCES do a better job of measuring adolescent educational transitions? One reason is that its organizational structure is neatly split between elementary and secondary schooling on one side and postsecondary education on the other. Excepting the Longitudinal Studies Branch, which was the home of the NLS–72 and the HSB, there is no organizational unit whose interests cover the transition from secondary school to whatever may follow it. Second, the NCES has recently moved from poor-orphan status among federal statistical agencies[10]—its budget has grown roughly fivefold since the early 1980s. But it is still hampered by limits on its size that force it to subcontract most statistical work, and its agenda is controlled to a substantial degree by the demands of the National Goals for Education.[11] The National Goals proclaim 90 percent high school completion and higher science and math test scores as primary goals, but they focus much less attention on the

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

Institute for Research on Poverty
1180 Observatory Drive
3412 Social Science Building
University of Wisconsin
Madison, Wisconsin 53706
(608) 262-6358

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 provide coverage of poverty-related research, events, and issues, and 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.

transition from secondary to postsecondary schooling, which is mentioned as one among several objectives subsidiary to the goal of "adult literacy and lifelong learning."

Trends and gaps in federal statistics on education

College enrollment

There are major, unresolved inconsistencies between basic series of enrollment counts from the NCES and those from the CPS. Data from the two sources differ by as much as 100,000 for black women in some years (see Figure 1). To some degree, such differences are probably a result both of sampling variability in the CPS and of varying definitions of postsecondary schooling enrollment. One recent effort to reconcile them ended with an admission of failure.[12]

High school dropout

What can we say about high school dropout and high school completion? Dropout and completion are two different things: the former refers to school-leaving during the usual high school ages, whereas high school equivalence can be obtained at later ages. Although most of this analysis focuses on college entry, high school dropout affects the base population of potential college entrants, and some have argued that decreases in college attendance rates among minorities are an "artifact" of increases in their high school completion.[13] Figure 2 shows a CPS-based series of annual high school dropout rates released by the NCES.[14] In each of the tenth-, eleventh-, and twelfth-grade rates, the numerator includes persons whose highest grade completed is nine, ten, or eleven, who were enrolled in school in the previous October, and who were not enrolled at the survey date. At the tenth- and eleventh-grade level, the denominator is the sum of the numerator and of persons with the same highest grade completed who were enrolled in school at the survey date; at the twelfth-grade level, the denominator is supplemented by persons who reported having graduated from high school in the survey year.

The series is conceptually flawed: it ignores "drop-in," the return to school of individuals who were not enrolled in the previous October; it includes twelfth-grade graduates who may not have left the eleventh grade in the preceding year; and it assumes that all persons who remain enrolled have advanced one grade. The first problem could be solved by minor tinkering with the flow of the October CPS questionnaire, and the second can be solved by retabulating existing data. It would take more substantial changes in the schedule to solve the third problem. Even though the data of Figure 2 are highly aggregated—they are three-year moving averages across three, single-year transitions—the jagged lines showing rates for minorities are obvious signs of sampling variability. Given this level of instability at the national level, the prospects are not good that the CPS data will yield timely estimates of differentials in dropout for policy-relevant subpopulations, for example, poor black inner-city residents. Thus, while it is possible to link these data on dropout to the characteristics of parental households within

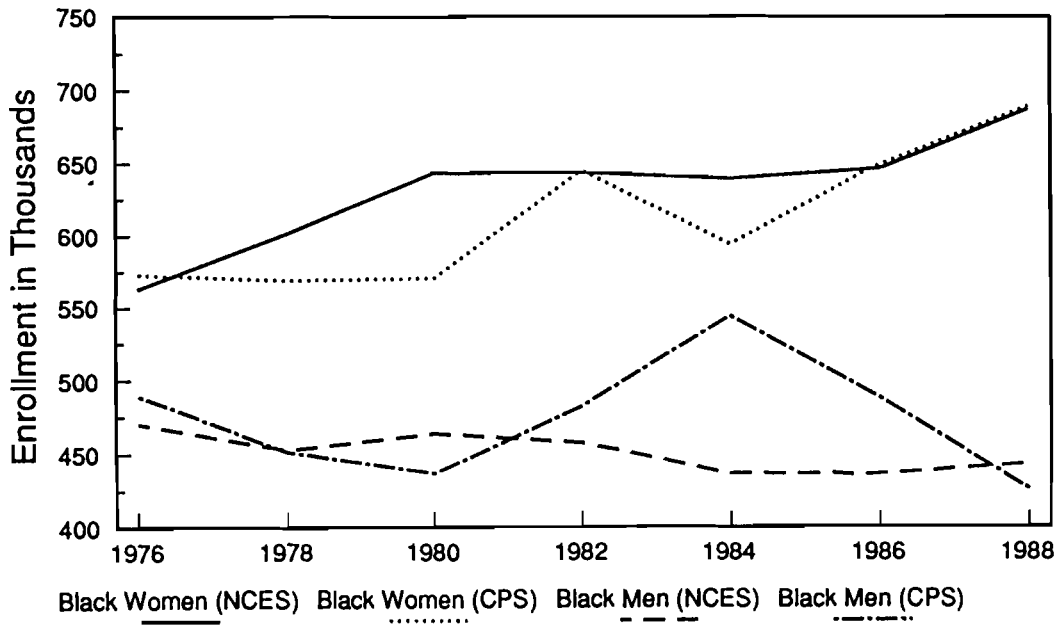

Figure 1. NCES and CPS Estimates of Enrollment in Higher Education: Black Men and Women, 1976–1988

Source: Data are from the Higher Education General Information System and the Integrated Postsecondary Education Data System, National Center for Education Statistics, and the October Current Population Surveys.

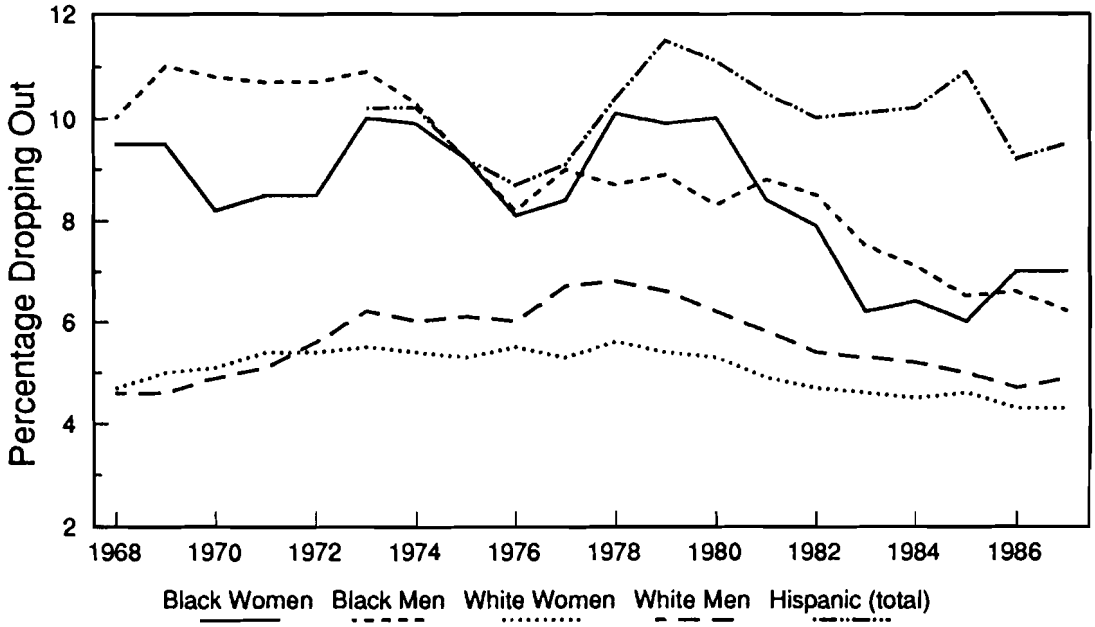

Figure 2. Yearly Dropout Rate from Grades 10–12: Persons Aged 14–24, 1968–1987

Source: Data are three-year moving averages for grades 10 to 12 from October Current Population Surveys, 1967–1987, as reported by Mary J. Frase, "Dropout Rates in the United States: 1988," *Analysis Report,* NCES 89–600, National Center for Education Statistics (Washington, D.C.: GPO, 1989).

the October CPS, they are unlikely to yield timely data on changes in the effects of social background on dropout.

Annual dropout rates are higher for blacks and Hispanics than for majority whites, and they appear to have been declining among blacks and whites since the late 1970s. One might read the current level of public interest in school dropout as a lagged response to the rising rates for whites in the 1970s as well as to the persistently high dropout rates of Hispanics. One serious problem with the rates for Hispanics, aside from sampling variability, is that we cannot distinguish between native- and foreign-born individuals. There is presently no way to determine whether the high dropout rates of Hispanics are the product of years of experience in American schools or of late entrance by the foreign born; surely these are different problems requiring different solutions. For this among other reasons, it is important that regular measurements of nativity be restored at least to the March and October CPS.

Our ability to measure high school completion is far better than our ability to measure high school dropout, for years of schooling are cumulative and irreversible, while school enrollment is not. For example, Figure 3 shows the proportions of whites, blacks, and Hispanics who completed high school by ages 21–24. From 1966 to 1985, high school completion grew from 85 percent to almost 90 percent among whites, and it has grown from less than 70 percent to almost 80 percent among blacks. High school completion remains much lower among Hispanic youth; its peak in the early 1980s was about 60 percent, and it may have declined since then.

If the data of Figure 3 provide better information on high school completion, why need we worry about the defects of the annual dropout rates? First, the data on high school completion are not timely. If we have to wait for people to reach age 24 before we can measure high school dropout, then we are a long way from the source of the problem. The series in Figure 3 uses data from the March CPS through March 1990; there are no more recent data for this age group. It is possible to measure high school completion at younger ages—and the younger the age, the more likely it is that completion means graduation, rather than equivalence—but a narrow, younger age window will also miss more high school completions, since many 18–20 year olds are still enrolled in high school. Second, the older the population, the less feasible it becomes to link the characteristics of children with those of their parents within the CPS; the CPS residence rules link fewer than half of Americans at ages 21–24 to their parents' households.

College attendance

The most common and highly publicized annual measure of college attendance is a rate of enrollment (or, sometimes, participation) among 18–24-year-old high school graduates (or all persons), which can be estimated from the October

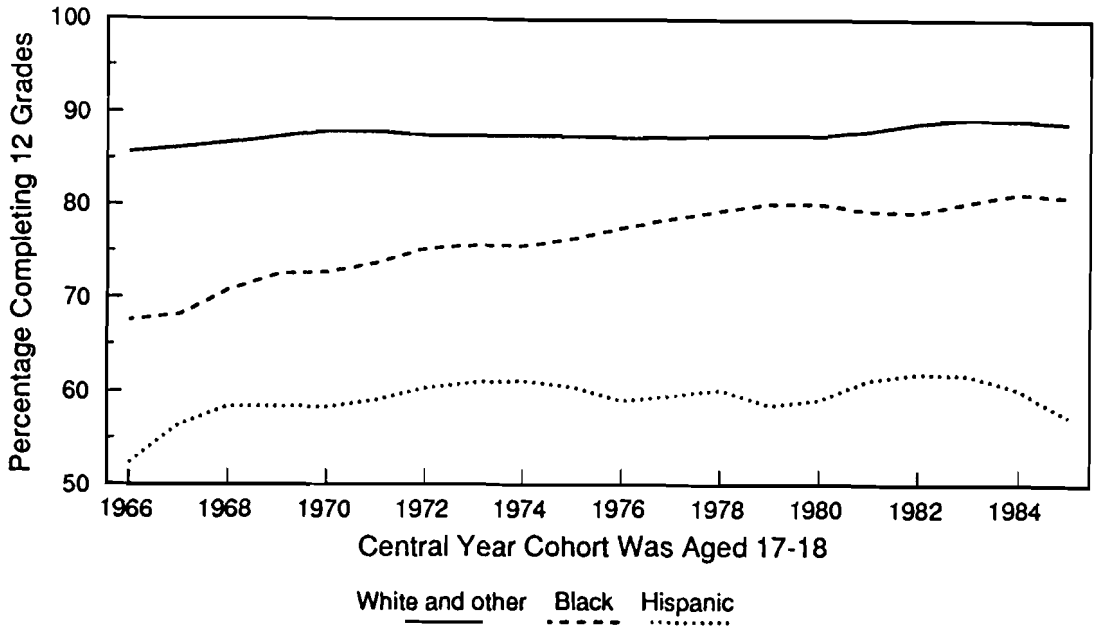

Figure 3. High School Completion by Age 21–24: White, Black, and Hispanic Persons

Source: Data are three-year moving averages for persons aged 21–24 in March Current Population Surveys, 1971–1990.

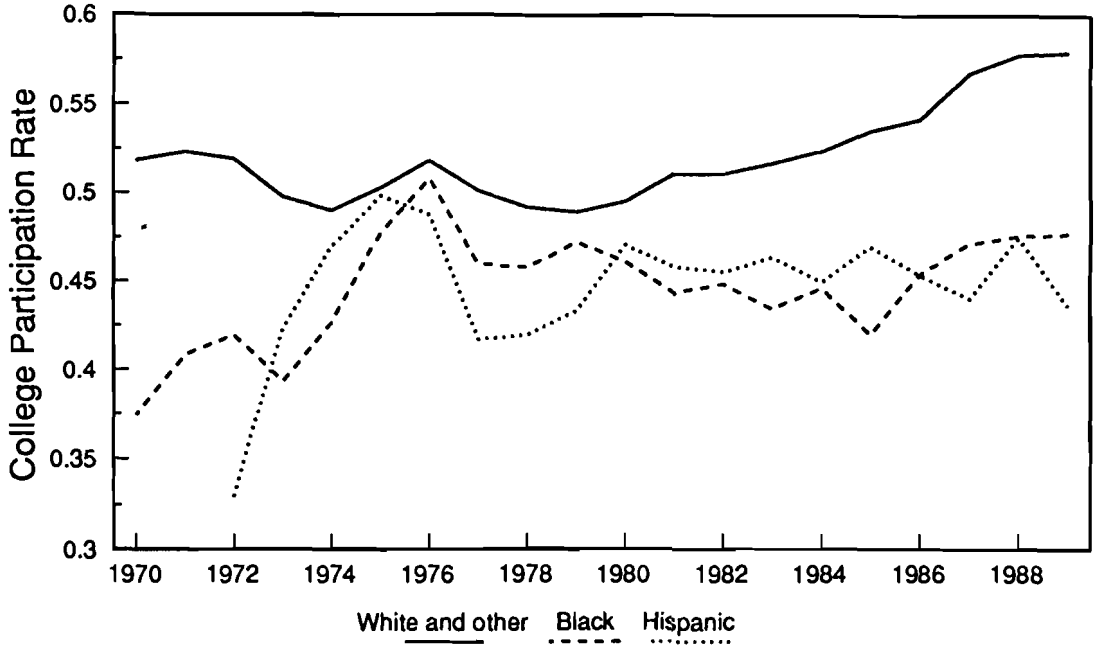

Figure 4. College Participation among High School Graduates Aged 18–24, 1970–1989

Source: Data are from October Current Population Surveys, 1970–1989.

CPS and is regularly published in the Current Population Reports.[15] Its only virtue is statistical reliability: because it covers seven birth cohorts, it does not suffer from the same statistical unreliability as, for example, the annual high school dropout rate. Figure 4 shows the rate of college participation among 18–24-year-old high school graduates, 1970–1989; participants are currently enrolled in a college or university or have completed one or more years of postsecondary schooling. For majority whites, the rate is always at or above 50 percent, and for blacks and Hispanics it hovers around 45 percent. Thus, the variability in participation among persons is close to the maximum, which would be reached if the rate were 50 percent for all social groups. Among majority whites, the participation rate grew rapidly during the 1980s, and it is now about 58 percent. For blacks and Hispanics, the rate appeared to rise and fall rapidly during the mid–1970s, and it has since stagnated. This is one of the bases for public concern that minority college participation is falling further behind that of majority whites.

What is wrong with the college participation rate? It is a conceptual muddle. It does not measure a transition that occurs in any fixed interval of time. It is not timely, for it covers seven birth cohorts, whose experiences after high school may have differed widely. It may not measure school enrollment, for it may include persons who are no longer in school because they completed college or dropped out along the way. It confounds differences in the pace and timing of schooling with differences in enrollment; for example, if postsecondary schooling is prolonged through a mixture of part-time schooling and work, as now more commonly occurs, the participation rate will go up, yet this is scarcely a signal that college has become more accessible. If blacks and Hispanics are more likely than majority whites to go to school part time or to combine school and work, the difference in college chances between majority and minority groups will be understated. The college participation rate also does not include persons who are outside the civilian noninstitutional population, that is, persons in military service or in prisons or other institutions. It shares that defect with all of the college attendance measures that can be obtained from the CPS, but it may be a more important defect in this measure, given the broad age range that it covers, because of the extensive postsecondary educational activities that occur within the armed forces.[16] The college participation rate is, or ought to be, an embarrassment to the federal agencies, public interest groups, and independent researchers who produce and use it.

Worse yet, college participation rates are often calculated by family income, because family income is the only economic characteristic that is attached to the public version of the October Current Population Survey and used extensively in published Census series. The October CPS family income measure is probably the worst income measure obtained in any major federal statistical program, yet it is the main economic measure used in the measurement of access to postsecondary education. It is a CPS control card item, which means that it is asked of anyone entering the sample for the first time in a calendar year.[17] The item is a single question about family income in the twelve months preceding sample entry, not in a calendar year, and the responses are coded in broad groups. By contrast, the March CPS now ascertains about a dozen specific sources of income in the preceding calendar year, and the Survey of Income and Program Participation (SIPP) ascertains more than fifty sources of income.

Whatever the virtues of the family income measure, it is a poor measure indeed when used in conjunction with the college participation rate of 18–24 year olds. The tabulation usually displays participation rates of "dependent" youth, i.e., unmarried persons aged 18–24. The problem is that a large share of the older persons in this age group—and a nontrivial share of the younger ones—are living in their own households, some married and others cohabiting, and "nondependency" in this sense is itself a likely outcome of school completion. The percentage of high school graduates aged 18–24 who were married fell rapidly between 1970 and 1989. Although among majority whites and Hispanics, at least a quarter of those in this age group were married in each year from 1970 to 1989, marriage rates for blacks were much lower.[18]

Incidentally, although numerous social and economic characteristics of the families of school-age children can in principle be attached to their October records, by reconstitution of households—and I have done this for all October surveys from 1968 through 1988[19]—the standard public release of the October surveys is a set of individual records with no household characteristics attached, other than control card items. The October CPS would become vastly more useful for public policy if the files and publications based upon them were redesigned and routinely issued with the characteristics of parents attached to their children's records. This redesign would be most valuable in analyses of school enrollment and progression at younger ages, when almost all children live with at least one parent.

College entry and completion

What measures of the transition from high school to college would be more useful? One such measure can be constructed from the educational attainment questions in the March (or October) CPS. Figure 5 shows the percentage of high school graduates who ever attended a postsecondary school by the time they reached ages 21–24.[20] As with the corresponding series on high school completion (Figure 3), we have to wait some years after the usual age of high school graduation to obtain this measure, but at least we know of what it is a measure. And the time lag is no worse than that implied in the use of college participation rates among 18–24 year olds. The series in Figure 5 is somewhat like that in Figure 4, in that rates of college attendance are higher among majority whites, and the white rates rose rapidly after the mid–1970s, but unlike the series in Figure 4, there is no sign of a mid–1970s bubble in the college

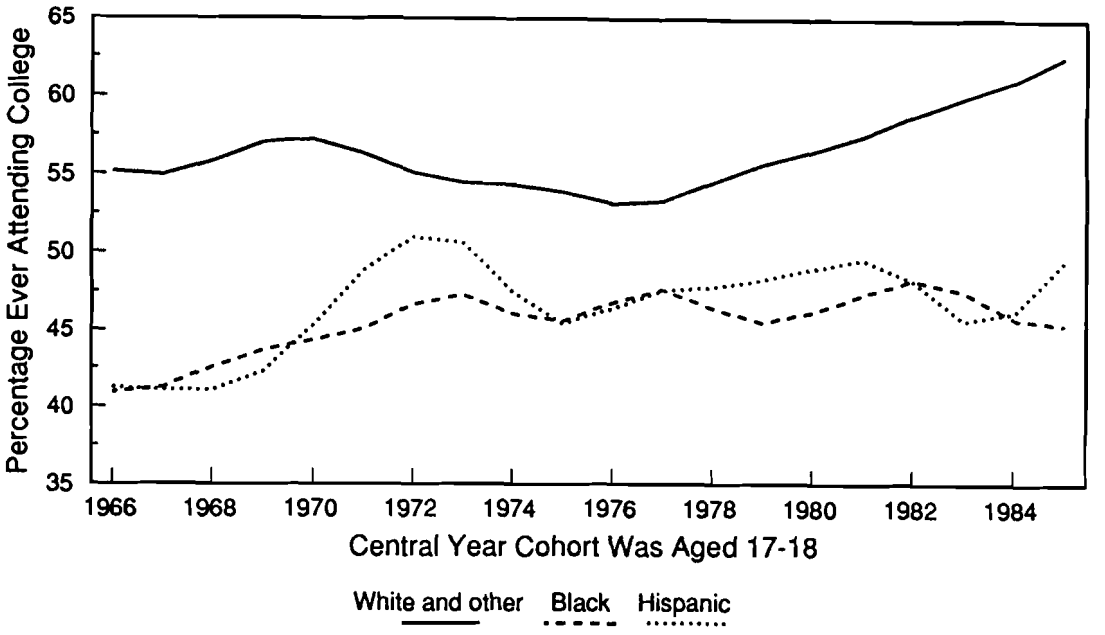

Figure 5. College Entry of High School Graduates by Age 21–24: White, Black, and Hispanic Persons

Source: Data are three-year moving averages for persons aged 21–24 years old in March Current Population Surveys, 1971–1990.

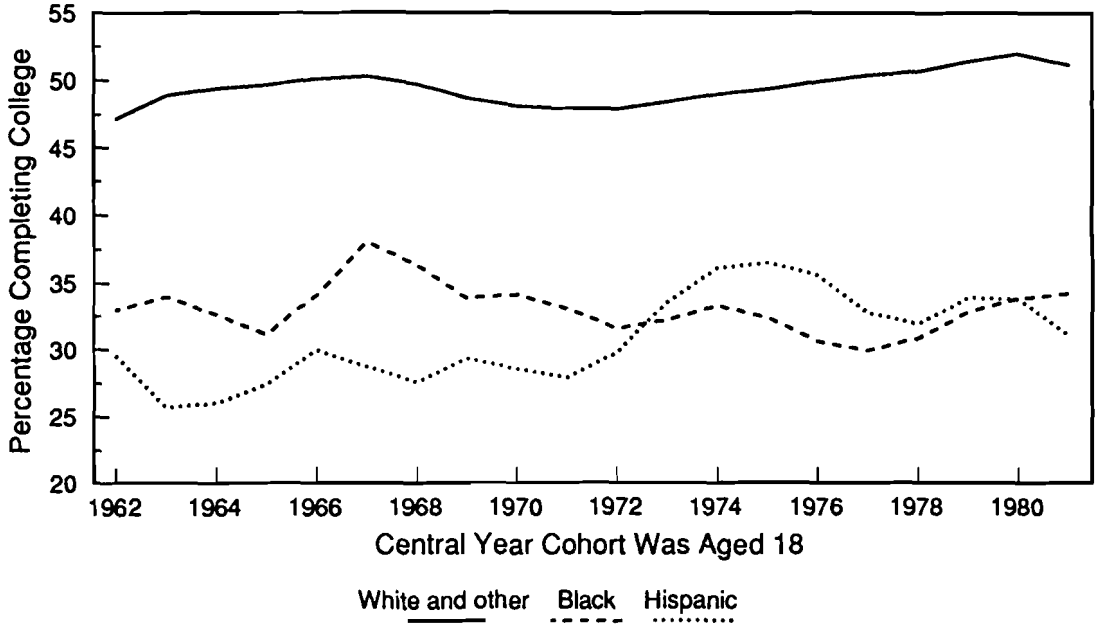

Figure 6. College Completion of College Entrants by Age 25–29: White, Black, and Hispanic Persons

Source: Data are three-year moving averages for persons aged 25–29 in March Current Population Surveys, 1971–1990.

attendance of blacks. There does appear to be a rapid rise and fall in attendance rates among Hispanics, but it occurs earlier than the bubble in the college participation rate. Among minorities, there is little sign of a trend after the mid–1970s in this measure of college experience.

Figure 6 displays the rates of college completion by age 25–29 among college entrants.[21] Again, although the latest available data are used, the series ends with the experience of youth who completed high school around 1980. There is a striking contrast between rates of completion of whites and those of minorities: about half of white college entrants complete sixteen or more years of school, but only one-third of minority college entrants do so. There are no strong trends in the data, but white completion rates may have grown from just under 50 percent to just over 50 percent between the cohorts of the early and late 1970s, while black completion rates may have declined from more than 35 percent to about 30 percent between the cohorts of the late 1960s and those of the late 1970s.

College entry of recent high school graduates

For many years the October CPS has included a question about the year of high school graduation of persons aged 14–24; together with current enrollment data, this permits a highly focused look at the transition from high school to college. We can ask what share of each year's high school graduates were enrolled in college in the following October, and these graduates are almost all young enough to be dependents at the time of the survey. Unlike age-specific rates of college participation, enrollment, or attendance, the college entry rates are both timely and specific. Figure 7 shows this series from 1972 through 1988.[22] Unlike Figure 5, this series shows a peak in black college entry during the middle to late 1970s. At that peak, the college-going chances of blacks were essentially equal to those of majority whites. But the peak was followed by an equally rapid decline that lasted through the early 1980s. For most of the period, Hispanic enrollment chances follow those of majority whites more closely than those of blacks. After a peak in the middle 1970s, there was a sharp decline until 1980, followed by rapid growth that preceded and outstripped the recovery among blacks. There is not necessarily an inconsistency between this series and those of Figures 3 and 5. If blacks are likely to delay college entry, then a decline in the initial transition from high school to college need not lead to a decline in the chance of ever entering college. On the other hand, the costs of delayed or prolonged schooling are real and should not be ignored.

One problem with this series is that it is ordinarily based on the experience of a single cohort of high school graduates as reported in a single October CPS;[23] thus, the number of observations and their statistical reliability are limited. There are typically about 2100 recent high school graduates in an October CPS, of whom about 200 are black and 100 are Hispanic. While it is possible to draw valid conclusions when the data are cumulated over a period of years, the data are not reliable in any one year for minority groups or for

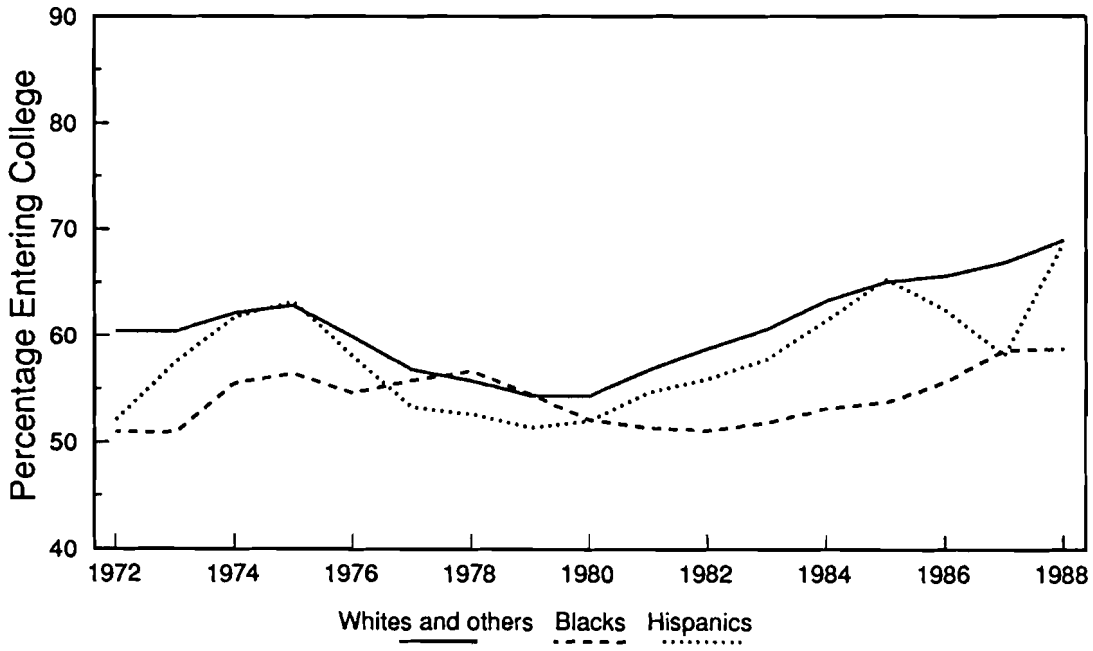

Figure 7. College Entry among Recent High School Graduates: White, Black, and Hispanic Men, 1972–1988

Source: Data are three-year moving averages of model estimates from October Current Population Surveys, 1972–1988.

other similarly small subpopulations. There is a trade-off between timeliness and specificity on one hand and reliability on the other.

To increase the statistical reliability of the series in Figure 7, I used a feature of the October design that has recently been dropped. Until 1988, the CPS identified the calendar year of high school graduation for several years preceding the calendar year of the survey. Using this question, plus other questions on highest grade attended and enrollment in the preceding year, I pooled contemporary reports of the enrollment of each class with the next year's retrospective reports of college enrollment by that class in the October following high school graduation. Although there are changes in population coverage between the first and second year after high school graduation, Figure 7 is based on a statistical model that takes that difference into account.[24]

In 1988 the Census Bureau dropped the detailed responses to the question about year of high school graduation, retaining only the distinction between graduates in the current year and in any previous year. Thus, it is no longer possible to combine observations and increase statistical reliability as I have done in constructing Figure 7. The same question on year of graduation is also the key to measuring delayed college entry and the timing of college completion. With it, we can measure how many individuals have entered college within two years of high school graduation and how many are still enrolled—and the class of enrollment—in each succeeding year. These timing issues are important, and the question on year of high school graduation should be restored to the October CPS.

When I inquired at the Census Bureau about this item, I was told that the answers to it were not necessarily true and that it was costly to retain all of the detail of the actual calendar year. I think that the usefulness of this item manifestly justifies its restoration, with improvements in wording and response categories, if those are necessary. With respect to cost and utility, I note that in the October 1989 education supplement, the truncated year-of-graduation item, with response categories "1989" and "1988 or earlier" is followed shortly by the questions, "Is there a computer in this household?" and "In what year was the computer purchased?" The latter item has the response categories 1989, 1988, 1987, 1986, 1985, 1984, before 1984, and don't know. I rest my case.

The potential utility of the college entry item in the October CPS is illustrated further by the series in Figure 8, which again shows trends in college enrollment of recent high school graduates, but controls for a set of social and economic variables that have been linked to their records. These variables include sex, age, regional and metropolitan location, dependency status, number of children in the household, nonintact family, head's and spouse's educational attainment, household head's labor force and occupational status, farm background, family income, and hous-

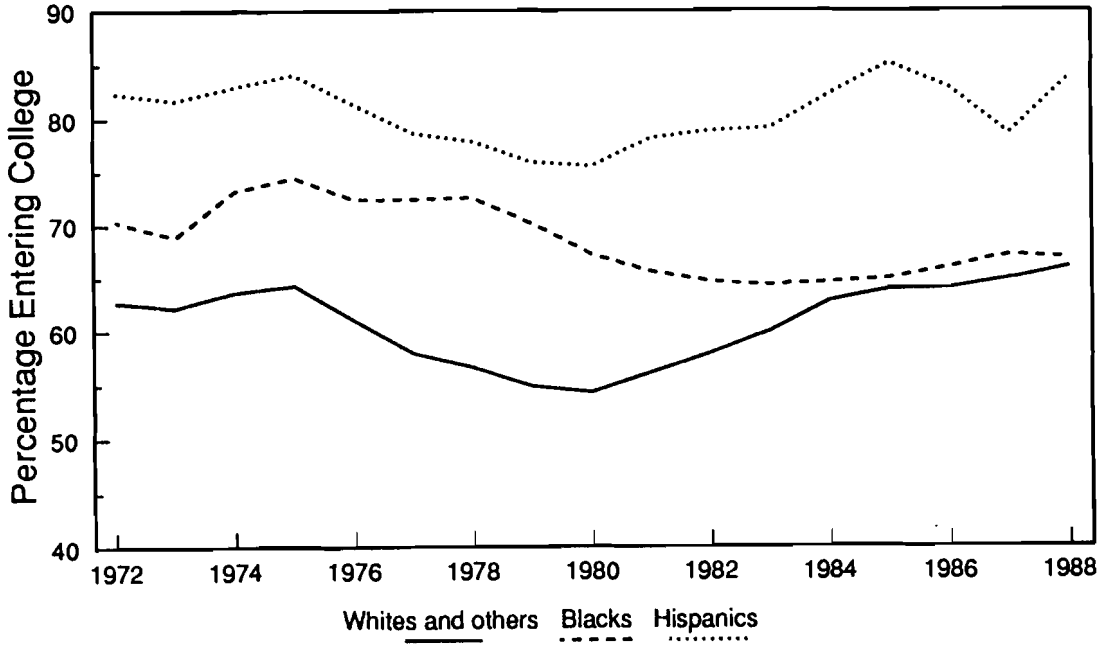

Figure 8. College Entry among Recent High School Graduates with the Average Social Background of Whites: White, Black, and Hispanic Men, 1972–1988

Source: Data are three-year moving averages of model estimates from October Current Population Surveys, 1972–1988.

ing tenure (own vs. rent). Thus, the series in Figure 8 controls differences between blacks, Hispanics, and majority whites in social and family background as well as changes over time in social and family background within each population. The most striking result in the figure is that the statistical controls reverse the differentials among whites, blacks, and Hispanics. Throughout the 1970s and until the middle 1980s, the college entry chances of blacks were better than those of majority whites with comparable social and family background, and the college entry chances of Hispanics have consistently been much higher than those of whites. One reason for the advantage of Hispanic high school graduates may be the selectivity of high school graduation. Only about 60 percent of Hispanics graduate from high school.[25] In the 1970s, though perhaps no longer, we had succeeded in breaking one of the barriers to black economic and social success.[26]

Although the controls change the relative standing of blacks, Hispanics, and majority whites, the basic temporal pattern in each population group remains intact. The basic similarity in trend between white and Hispanic graduates, and the very different trend line for blacks, are both clearer in the adjusted series. The rise and fall of black chances of college entry are not explained away by changes in social or family background, nor are the fall and rise of white college chances. On the other hand, among whites, the growth of college entry chances after 1980 is not as steep or sustained in the adjusted series as in the observed series. And among blacks, the post–1982 increase in college entry chances almost disappears. Obversely, the decline in black college attendance in the late 1970s and early 1980s is not as sharp in the observed series as in the series that has been adjusted for social background. These differences in observed and adjusted trends within the black and white population are explained by steady improvements in the social backgrounds of successive cohorts of high school graduates, especially by increases in parental schooling and occupational status and by decreases in numbers of dependent children in the household.[27]

It should be noted that once social background is controlled, even though blacks' chances of college entry have declined relative to those of whites after the late 1970s, they never fall below those of whites. These findings raise, in a rather pointed way, the question of how much compensation for preexisting population differences ought to be the goal of public policy. If, among persons with the same social background, minority chances of college entry exceed those of the white majority, is there a rationale for expanded efforts to improve the relative chances of minorities? The raising of such a question depends on the accuracy and comprehensiveness of the data available. In my opinion, the federal statistical system ought to be able to produce this kind of analysis of educational chances, though with greater reliability and timeliness and with reference to a longer view of postsecondary schooling and other outcomes.

Improving statistics on the transition from youth to adulthood

I have mentioned several changes in the design of the Current Population Survey that would improve our ability to monitor, explain, and effect changes in educational transitions. But there are inherent limitations to the CPS. Without a major change in design, it would be impossible to increase the reliability of timely measurements from the CPS to an acceptable level for minority groups or other similar subpopulations. It is unlikely that the CPS will ever be expanded to cover the military or other institutional populations, and it is even less likely that the CPS could be augmented with measurements of academic performance. Yet all of these elements—and others—should be a part of our system of educational statistics.

My proposal is a new survey program within the National Center for Education Statistics, the Annual Longitudinal Survey of Youth, that will be designed from the ground up to provide valid, reliable, and timely estimates of educational and other transitions among American youth.[28]

1. The survey need not rival the size of the decennial longitudinal surveys in any one year, but it should be heavily stratified by minority group status, and each stratum should be large enough to yield reliable annual estimates of key statistics, like the proportions dropping out of school, entering college, or joining the military. A rough guess is that a well-designed sample of about 8000 persons per year, equally divided among majority whites, blacks, Hispanics, and Asians and Native Americans (groups that are far too small to show up reliably in the CPS), would be sufficient.
2. The sample should be drawn from schools, not from households. This will simplify both the selection of schoolchildren and coverage of racial-ethnic strata. The design should include the administration of one or more tests of academic achievement or aptitude, and it would be desirable to include at least a baseline interview with a parent as well as with the sample person.
3. My first priority would be to begin the survey at the twelfth-grade level, because most of the variance in educational transitions occurs after the completion of high school, and to maintain and follow the sample for two or three years after high school graduation. Members of the sample should be followed regardless of their movement into and out of the civilian noninstitutional population—to and through the military, jails, prisons, and hospitals. The design could be extended to an earlier starting point, perhaps as early as the eighth grade, with some increase in sample size and a substantial increase in budget. That is, the design could improve the measurement of high school dropout as well as that of postsecondary school transitions. But the choice of an earlier starting point is consequential. It will be essential that the coverage of each cohort be as

complete as possible, and each year's shift to an earlier age increases the possibilities for sample attrition and the costs of avoiding it. Regardless of the year in school at which the survey begins, it should continue to follow youth for the full duration of the panel. Thus, high school dropouts at grade ten or twelve should be followed just as long as high school graduates.

4. With a constant design and limited content, the survey data will be cumulative across years, much like the General Social Survey of the National Opinion Research Center or the October CPS data for recent high school graduates, and the cumulative surveys will provide opportunities for detailed analysis of smaller population groups.

5. The key content areas of the Annual Longitudinal Survey should include social and family background; academic program and courses; academic aptitude and performance; educational, occupational, marital, and military plans and aspirations; work activities and remuneration; and plans and resources for financing postsecondary education or training. In each follow-up survey, preferably beginning in the fall after the nominal date of high school completion, the survey should ascertain educational, work, residency, and family status at the survey date and in the months since the previous interview.

6. The new survey should complement and not supplant other measurement programs. The CPS has many other purposes, even in the measurement of education, than those I have mentioned in this review. Within the NCES the idea of the larger decennial longitudinal studies ought to be preserved, but built around the core content of the annual surveys, as an opportunity to enrich, expand, and experiment with broader content. For example the NELS–88 contains linkages between students and schools, teachers, and administrators that may not need to be a part of the Annual Longitudinal Survey. These or similar linkages might be made an occasional supplement to the core survey, but not one of its annual components.

How realistic is this proposal? Over the course of a decade, it will be about three times as large as the NELS–88 if the sample is initiated in the twelfth grade, and the content could be much less than that of the NELS–88.[29] In my opinion, this is not a large undertaking, and it will be dirt cheap in comparison to its likely benefits for public policy. The Bush administration has undertaken a new initiative to improve economic statistics under which the National Center for Education Statistics will continue to grow. There is room for hope. I think it is time we stopped guessing what our youth are doing with their lives and started measuring it. ■

---

[1]To be sure, college dropout is also large. Slightly more than half of white college entrants complete sixteen years of school by the time they reach ages 25–29, and only about one-third of minority entrants complete sixteen years of school by ages 25–29. However, college dropout occurs over a prolonged period, and it affects only the survivors of the transition from high school to college.

[2]Kevin Murphy and Finis Welch, "Wage Premiums for College Graduates: Recent Growth and Possible Explanations," *Educational Researcher*, May 1989, pp. 17–26.

[3]Robert Hauser, "Measuring Adolescent Educational Transitions among African Americans, Hispanics, and Whites," IRP Discussion Paper no. 951–91, University of Wisconsin-Madison, August 1991, Figures 1 and 2 (pp. 2 and 3).

[4]John H. Bishop and Shani Carter, "The Worsening Shortage of College Graduate Workers," Working Paper, Center for Advanced Human Resources Studies, School of Industrial and Labor Relations, Cornell University, Ithaca, N.Y., 1990.

[5]See Hauser, "Measuring Adolescent Educational Transitions," p. 2, for a discussion of the growing proportion of minorities in the U.S. population.

[6]See Hauser, "The Decline in College Entry among African Americans: Findings in Search of Explanations," in *Prejudice, Politics, and Race in America Today*, ed. Paul Sniderman, Philip Tetlock, and Edward Carmines (Stanford, Calif.: Stanford University Press, forthcoming).

[7] For example, for more than fifteen years, the National Institute on Drug Abuse has sponsored Monitoring the Future, a large annual survey of high school graduates (with selective follow-ups) that does not obtain quite enough information to inform public discourse on trends in access to higher education. See Jerald G. Bachman, Lloyd D. Johnston, and Patrick M. O'Malley, *Monitoring the Future: Questionnaire Responses from the Nation's High School Seniors, 1978* (Ann Arbor: Survey Research Center, Institute for Social Research, University of Michigan, 1980).

[8]For a description of the CPS, see U.S. Bureau of the Census, *The Current Population Survey: Design and Methodology*, Technical Paper 40 (Washington, D.C.: GPO, 1978). For a description of the various programs of the NCES, see *Programs and Plans of the National Center for Education Statistics, 1990 Edition*, ed. Laurence T. Ogle, Indicators and Reports Branch, Data Development Division, Office of Educational Research and Improvement, U.S. Department of Education (Washington, D.C.: GPO, 1990).

[9]For example, transitions beyond high school among students who were sampled as high school sophomores in the HSB in 1980 are not comparable to those transitions among the HSB seniors of 1980 or among seniors in the NLS–72, because persons who were high school seniors in 1982 do not include all persons who were high school sophomores in 1980. This problem could be even worse in analyses of postsecondary transitions in the NELS–88, whose members were drawn into the sample as eighth graders. Fortunately, there are plans to supplement the sample in 1992, the nominal year of high school graduation.

[10]*Creating a Center for Education Statistics: A Time for Action*, ed. Daniel B. Levine, National Research Council (Washington, D.C.: National Academy Press, 1986).

[11]U.S. Department of Education, *National Goals for Education* (Washington, D.C.: GPO, 1990).

[12]Daniel Koretz, *Trends in the Postsecondary Enrollment of Minorities* (Santa Monica, Calif.: Rand Corporation, 1990).

[13]I find this argument implausible, for rates of college entry among whites have been stable or increasing as high school graduation has become almost universal.

[14]Robert Kominski, "Estimating the National High School Dropout Rate," *Demography*, 27 (May 1990), 303–311; Mary J. Frase, "Dropout Rates in the United States: 1988," *Analysis Report*, NCES 89–600, National Center for Education Statistics (Washington, D.C.: GPO, 1989).

[15]U.S. Bureau of the Census, Current Population Reports, Series P–20, no. 443, *School Enrollment—Social and Economic Characteristics of*

Students: October 1988 and 1987 (Washington, D.C.: GPO, 1990). Among the reports that make use of these data are Reginald Wilson and Deborah J. Carter, *Seventh Annual Status Report: Minorities in Higher Education* (Washington, D.C.: The American Council on Education, 1988); Wilson and Carter, *Eighth Annual Status Report: Minorities in Higher Education* (Washington, D.C.: The American Council on Education, 1989); Carter and Wilson, *Ninth Annual Status Report: Minorities in Higher Education* (Washington, D.C.: The American Council on Education, 1990); Thomas G. Mortenson, *The Reallocation of Financial Aid from Poor to Middle Income and Affluent Students: 1978 to 1990*, ACT Student Financial Aid Research Report Series (Iowa City, Iowa: The American College Testing Program, 1990); Mortenson and Zhijun Wu, *High School Graduation and College Participation of Young Adults by Family Income Backgrounds, 1970 to 1989*, ACT Student Financial Aid Research Report Series (Iowa City, Iowa: The American College Testing Service, 1990); and Mortenson, *Equity of Higher Educational Opportunity for Women, Black, Hispanic, and Low Income Students*, ACT Student Financial Aid Research Report Series (Iowa City, Iowa: The American College Testing Program, 1991).

[16]Holly Hexter and Elaine El-Khawas, *Joining Forces: The Military's Impact on College Enrollments* (Washington, D.C.: American Council on Education, 1988).

[17]That is, the October CPS family income item is not part of the Education Supplement.

[18]See Hauser, "Measuring Adolescent Educational Transitions," p. 21.

[19]Hauser, *Uniform October Current Population Survey Person-Household File, 1968–1988*, Codebook for Machine-Readable Data File (Madison, Wis.: Center for Demography and Ecology, University of Wisconsin-Madison).

[20]The CPS educational attainment item asked, "What is the highest grade or year of regular school that . . . attended?" and "Did . . . complete that grade or year?" This series counts anyone who reported attending grade thirteen or a higher grade as having been a college entrant. It is problematic because people may report attendance at noncollegiate postsecondary schools.

[21]The series shows the percentage of persons who ever entered grade thirteen or higher who said they completed grade sixteen or higher. Some persons who completed at least sixteen years of schooling may not have a college degree, and some persons who said they had attended grade thirteen may never have enrolled in a college or university.

[22]Figure 7 shows percentages of male high school graduates entering college as estimated from a statistical model in which there are effects on college entry of year of graduation, year of survey report, sex, race-ethnicity, and interactions between sex and race-ethnicity; under the model the trends vary by race-ethnicity, but not by sex, and Figure 7 would be essentially the same if the reported estimates were for women. The analysis is based upon samples of 6102 blacks, 2801 Hispanics, and 50,348 white and other persons from the October Current Population Surveys, 1972 through 1988.

[23]See, for example, *A Common Destiny: Blacks and American Society*, ed. Gerald David Jaynes and Robin M. Williams, Jr., Committee on the Status of Black Americans, Commission on Behavioral and Social Sciences, National Research Council (Washington, D.C.: National Academy Press, 1989); and Thomas Mortenson, *College Entrance Rates for Recent High School Graduates*, ACT Financial Aid Research Briefs (Iowa City, Iowa: The American College Testing Program, 1990).

[24]Before pooling the contemporaneous and retrospective reports of college entry, I tested for interaction effects between year of graduation and year of survey report within each racial-ethnic group, and there were no statistically significant interactions. Because there is a 50 percent overlap in CPS households from one year to the next, this procedure does not double the effective sample size, but it is a substantial improvement.

[25]In other analyses (Hauser, "Trends in College Entry among Blacks, Whites, and Hispanics, 1972–1988," IRP Discussion Paper no. 958–91, University of Wisconsin-Madison, September 1991 (forthcoming in *The Economics of Higher Education*, ed. Charles Clotfelter and Michael Rothschild [National Bureau of Economic Research]) I show that control of dependency status does not account for changes in racial or ethnic differentials between the observed series (Fig. 7) and the adjusted series (Fig. 8). The trend lines are essentially parallel when dependency, but not other social background variables, is controlled, but the racial-ethnic differentials are even larger. That is, the comparison of Figure 7 with Figure 8 understates the explanatory power of social background variables.

[26]See Hauser, "The Decline in College Entry," and Hauser and Douglas K. Anderson, "Post–High School Plans and Aspirations of Black and White High School Seniors: 1976–86," *Sociology of Education*, 64 (October 1991), 263–277 (IRP Reprint no. 658), for more detailed analyses of the sources of changing black-white differences in college attendance.

[27]See Hauser, "Trends in College Entry."

[28]This is by no means an original idea. It was suggested more than twenty years ago by Otis Dudley Duncan (*Towards Social Reporting: Next Steps* [New York: Russell Sage Foundation, 1969]) and, more recently, by Harold W. Watts and Donald J. Hernandez (eds., *Child and Family Indicators: A Report with Recommendations*, report of the Advisory Group on Child and Family Indicators of the Advisory and Planning Committee on Social Indicators [Washington, D.C.: Social Science Research Council, Center for Coordination of Research on Social Indicators, 1982]).

[29]That is, a survey beginning in the twelfth grade could include about 80,000 students per decade, which is about three times the size of the NELS–88. A survey beginning in the eighth or tenth grade would have to have a larger sample size in order to retain sufficient statistical power after sample attrition and school dropout have taken their toll.

1991 Green Book

Beginning with the 1990 edition, the *Green Book* became the official title of the yearly publication that serves as a resource document on entitlement programs for the Committee on Ways and Means of the U.S. House of Representatives. It has unofficially been called the *Green Book* for many years, for a number of reasons: It has a green cover; the full title is cumbersome—*Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means*—and also inaccurate, since Appendix O, approximately 100 pages of the current volume, is a "Description of other major federal assistance programs not within the jurisdiction of the Committee on Ways and Means"—such programs as Food Stamps and Medicaid.

The first *Green Book,* which came out March 10, 1981, in response to a request by Dan Rostenkowski, then and now chairman of the Ways and Means Committee, was 158 pages long. The 1991 issue, which came out on May 7 of this year, contains 1641 pages. The programs covered include Social Security, Medicare, trade adjustment assistance, Unemployment Compensation, Aid to Families with Dependent Children, child support enforcement, Supplemental Security Income, the Title XX social services block grant program, child welfare, foster care, and adoption assistance (as well as those in Appendix O to round out the picture). As described in the 1991 Letter of Transmittal, the book "integrates a description of each program within the jurisdiction of the Committee with current data regarding the population served by the program, an analysis of interactions with other major programs, and historical background information."[1]

Assembling the yearly *Green Book* has been the task of Wendell Primus, Chief Economist for the committee, since the first volume was printed. He is assisted by approximately 250 people, including analysts from various federal agencies and the committee staff.[2] The data in the book are therefore not only timely, but have the authority of the federal government behind them. Although most of the material in the book may be available elsewhere, the convenience of having it collected in one place greatly enhances its value. As a result, the *Green Book* has come to be much more than a tool for the committee; it has become as well an important resource for scholars, policy analysts, advocacy groups, and the press. And in recent years it has become a political hot potato.

Any single volume provides a current summary of social programs in the nation—how they work, their costs and benefits, and what they are accomplishing. As data accumulate, the annual volumes present a comprehensive numerical history of how the nation has dealt with its most vulnerable members over time and how membership among the vulnerable has changed. According to Primus, however, the *Green Book* is not constrained to update material that has been included in the past. Anything that is deemed of value to the members of the Committee on Ways and Means will find a place in the book.[3]

For the ordinary reader the book contains the answers to many questions, major and minor, that reveal much about the workings of the society of which we are a part. How do social programs in the United States compare with programs in other developed countries? How many teenagers have abortions each year? What happens to children who leave foster care? How much do doctors earn? What are the causes of death of the elderly? How many families receive child support from absent parents?

Poverty data

Since many of the entitlements are designed to counter poverty as well as provide security and equity to U.S. citizens, a section of the book (Appendix I) explores poverty, income distribution, and antipoverty effectiveness. A series of tables demonstrates the effectiveness of cash and noncash transfers (including the federal income tax and payroll taxes) in removing from poverty: all individuals, those in families with children, those with an unmarried household head, married couples with children, and the aged.[4] The tables cover the years 1979 through 1989, thus measuring the results of transfers over the decade. Table 19, Appendix I, which looks at single-parent families, is reproduced here as Table 1.[5] This table demonstrates that in 1979 approximately 30 percent of individuals in single-parent families with incomes below the poverty line were removed from poverty as a result of means-tested transfers, food and housing benefits, and federal tax policy. By 1989 only 17 percent were removed from poverty by those means. The poverty gap for this group (the amount by which the income of those in poverty fell below the poverty line) increased in the same period from $7,392,000 to $11,861,000 (in 1987 dollars).

The *Green Book* attributes the increase in the poverty population that occurred over this period to the following factors: growth in the general population, reduced effective-

Table 1

Antipoverty Effectiveness of Cash and Noncash Transfers (Including Federal Income and Payroll Taxes) for Individuals in All Units with an Unmarried Head and Related Children Less Than 18, 1979–1989

	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Number of poor individuals (thousands):											
Cash income before transfers	11,480	12,458	12,915	13,594	13,751	13,774	13,685	14,069	14,259	14,149	14,074
Plus social insurance (other than Social Security)	11,271	12,207	12,689	13,284	13,501	13,513	13,391	13,790	14,040	13,928	13,820
Plus Social Security	10,367	11,201	11,858	12,579	12,611	12,786	12,581	13,143	13,306	13,167	13,040
Plus means-tested cash transfers (official definition)	9,230	10,362	10,987	11,903	12,063	12,132	11,943	12,487	12,661	12,514	12,388
Plus food and housing benefits	6,901	8,363	9,191	10,312	10,531	10,473	10,418	10,879	10,963	10,947	10,636
Less federal taxes	6,925	8,433	9,423	10,611	10,800	10,695	10,670	11,217	10,977	10,923	10,648
Number of individuals removed from poverty due to (thousands):											
Social insurance (other than Social Security)	209	251	226	310	250	261	294	279	219	221	254
Social insurance (including Social Security)	1,113	1,257	1,057	1,015	1,140	988	1,104	926	953	982	1,034
Means-tested cash, food and housing benefits	3,466	2,838	2,667	2,267	2,080	2,313	2,163	2,264	2,343	2,220	2,404
Federal taxes	–24	–70	–232	–299	–269	–222	–252	–338	–14	24	–12
Percent of individuals removed from poverty due to:											
Social insurance (including Social Security)	9.7	10.1	8.2	7.5	8.3	7.2	8.1	6.6	6.7	6.9	7.3
Means-tested cash, food and housing benefits and federal taxes	30.0	22.2	18.9	14.5	13.2	15.2	14.0	13.7	16.3	15.9	17.0
Poverty gap (millions of 1987 dollars):											
Cash income before transfers	24,431	26,996	28,350	30,452	31,519	30,680	30,722	31,942	32,661	32,142	31,018
Plus social insurance (other than Social Security)	23,742	26,050	27,550	29,463	30,358	29,687	29,822	31,216	31,975	31,627	30,349
Plus Social Security	20,952	23,010	24,657	26,597	27,361	27,046	26,790	28,518	29,172	28,632	27,533
Plus means-tested cash transfers (official definition)	12,014	13,785	15,448	17,309	17,976	17,580	17,562	18,815	19,358	19,481	18,844
Plus food and housing benefits	7,446	8,406	9,960	11,073	11,682	11,341	11,181	12,119	12,304	12,401	12,003
Less federal taxes	7,392	8,384	10,008	11,157	11,756	11,440	11,263	12,221	12,175	12,282	11,861
Percentage reduction in poverty gap due to:											
Social insurance (including Social Security)	14.2	14.8	13.0	12.7	13.2	11.8	12.8	10.7	10.7	10.9	11.2
Means-tested cash, food and housing benefits and federal taxes	55.5	54.2	51.7	50.7	49.5	50.9	50.5	51.0	52.0	50.9	50.5
Poverty rate (in percent):											
Cash income before transfers	50.0	51.6	52.1	54.5	53.8	51.5	50.7	51.4	50.9	49.5	48.1
Plus social insurance (other than Social Security)	49.1	50.6	51.2	53.2	52.8	50.5	49.6	50.3	50.1	48.7	47.2
Plus Social Security	45.1	46.4	47.8	50.4	49.3	47.8	46.6	48.0	47.5	46.0	44.6
Plus means-tested cash transfers (official definition)	40.2	43.0	44.3	47.7	47.2	45.3	44.3	45.6	45.2	43.7	42.3
Plus food and housing benefits	30.0	34.7	37.1	41.3	41.2	39.1	38.6	39.7	39.1	38.3	36.4
Less federal taxes	30.1	35.0	38.0	42.5	42.2	40.0	39.6	40.9	39.2	38.2	36.4

Source: *1991 Green Book*, pp. 1167–1168, Data from the Congressional Budget Office and the committee staff. The explanatory note below is taken from p. 1160.

Note: Poverty under six different income concepts is measured. "Cash income before transfers" is all cash income prior to any benefits from government programs. This would include all earnings, pension income, income from savings, self-employment income, etc. "Plus social insurance" adds to cash income all benefits from social insurance (except Social Security) programs, such as Workers' Compensation and Unemployment Compensation. "Plus Social Security" adds to cash income and social insurance income benefits from the Social Security programs. "Plus means-tested cash transfers" adds to cash income and social insurance income all means-tested transfer income such as Aid to Families with Dependent Children, Supplemental Security Income, and General Assistance. "Plus food and housing" adds to cash, social insurance and means-tested cash income all means-tested in-kind transfers received for food and housing. This would include food stamps, housing programs and school lunch programs. Benefits are valued using the market value method (the private-market cost of the benefits as estimated by the Census Bureau). "Less federal taxes" subtracts from income federal income taxes and the employee portion of federal payroll taxes.

ness of means-tested welfare programs, changes in demographics (for example, the growth of single-parent families), and the reduced effectiveness of social insurance programs. These summary explanations in the *Green Book* do not, however, give a complete picture of factors influencing the number of the poor. Although some demographic factors and government policies increased the number of poor, other factors had the opposite effect.[6] Tax policy changes since 1986—such as the Tax Reform Act of 1986 and the increases in the Earned Income Tax Credit in the Omnibus Reconciliation Act of 1990—have removed over two million people from poverty.

Unemployment compensation

Typical of the program descriptions in the *Green Book* is that of unemployment compensation (UC), summarized briefly here.[7] The UC program was created by the Social Security Act of 1935. It has two principal purposes: to aid workers temporarily unemployed through no fault of their own, and to counter the effects of recessions. Each state operates its own program, and the U.S. Department of Labor oversees the system. The federal portion of the program is paid for by a 6.2 percent tax on employers for the first $7000 paid annually to covered employers. Covered employers are those who paid wages of at least $1500 during any calendar quarter or who employed at least one worker in at least one day of each of twenty weeks in the current or prior year. (There are, of course, exceptions, such as nonprofit organizations and state-local governments.) The federal government credits up to 5.4 percent for states that follow coverage regulations if they do not have delinquent federal loans for UC, making the minimum net federal unemployment tax rate 0.8 percent. The states are supposed to use this credit to finance their programs and half of the federal-state extended benefits program.

The state tax rates vary, based on their own unemployment experience. Although the standard is 5.4 percent of the first $7000 in wages paid, and states can charge a rate of as high as 10 percent, the national average in 1990 was 2.0 percent of taxable wages, which came to 0.7 percent of total wages.

A portion of the federal revenue is used for administration of the system. The rest, with the state portion, goes into trust funds to cover the unemployment benefits and extended benefits.

Approximately 105 million individuals are covered by UC—approximately 97 percent of all wage and salaried workers or about 88 percent of all employed persons. Yet, in 1990, only 37 percent of unemployed persons were receiving benefits. This is far below the peak of 81 percent in April 1975. Of course not all covered workers are eligible for benefits: UC applies only to those who lose their jobs, automatically excluding persons who voluntarily leave their jobs without good cause and those who are fired for misconduct. States have restricted eligibility in a number of ways since 1980, such as by raising the required minimum earnings in a base year needed to receive the minimum benefit. Furthermore, a claimant may be disqualified, for example, if he or she refuses a job offer or does not demonstrate ability and willingness to seek suitable employment.

Minimum weekly unemployment benefits in 1991 range from $5 in Hawaii to $68 in Alaska. Duration of benefits usually varies with the amount of earnings the claimant had in the base year. Minimum length of time for receiving benefits ranges from five weeks in Oregon to 26 weeks in nine states. In fifty states, 26 weeks is also the maximum.

When benefits are exhausted, extended benefits can be obtained through the federal-state extended benefits program for up to 13 additional weeks if the state qualifies for this program. One qualification is that the state's 13-week average unemployment level for insured workers be at least 6 percent. Few states qualify. Approximately 2.3 million individuals exhausted their benefits in 1990, yet only two states—Alaska and Rhode Island—qualified for extended benefits. Current bills in Congress are designed to reach a larger proportion of covered workers who have exhausted their benefits without finding employment. On November 15, 1991, President Bush signed the Federal Supplemental Compensation Act, which temporarily provides extended benefits for 6, 13, or 20 weeks, depending upon the unemployment rate in a state. This bill leaves 18 states without retroactive benefits for those whose UC has expired.

The rich vs. the poor

Perhaps the most controversial tables in the book are those in Appendix J, "The Distribution of Income and Tax Burdens by Household." The tables in this section compare income for a number of groups in the population, before and after state and federal taxes. Table 2 here (which combines data from Tables 21, 22, and 23 in Appendix J) gives the proportion of income received by income groups, before and after federal taxes, over the period 1977–1988, and the share of taxes they paid. Before taxes, in 1977, the poorest fifth of families (based on income) received 4.9 percent of all pretax income, compared to 46.6 percent going to the top fifth. In 1988 the lowest fifth of families before taxes received 3.7 percent of total pretax income, compared to 51.9 percent for the top fifth. In other words the share of income received by the poorest fifth before taxes dropped by 24 percent—(4.9–3.7)/4.9—while the share going to the top fifth increased by 11 percent—(46.6–51.9)/46.6. After taxes, the share received by the poorest fifth of families dropped by 25 percent over this time span, whereas the share received by the top fifth rose by 13 percent. Thus the share of aftertax income going to the top fifth rose slightly faster than the share of pretax income going to the top fifth.

After taxes the top 1 percent of families received 12.8 percent of total aftertax income in 1988, compared to 7.3 percent in 1977, an increase of 75 percent. The share of taxes paid by the upper 1 percent rose only 17 percent, from 13.6 percent to 15.9 percent of total federal taxes paid.

These numbers, produced by the Congressional Budget Office (CBO), have not gone uncontested. The CBO has defended the data against a number of charges, ranging from a failure to explain their methodology to failure to adjust capital gains for inflation. Critics assert, for example, that the CBO grossly overstates the income of the richest fifth of families by including capital gains as ordinary income in the year in which an asset is sold, failing thereby to adjust for the portion of capital gains that is simply inflation.

While acknowledging problems entailed in measuring the distribution of family income and federal taxes, Robert D. Reischauer, Director of the CBO, justified the numbers, which are based on income data reported to the Internal Revenue Service and data from the Current Population Survey of the Bureau of the Census.[8] He described how the CBO calculations deal with such matters as inflation and summed up, "It is my belief that the data and methods that CBO has used to measure the distribution of family income and federal taxes are not biased in a particular direction nor do they distort the trends of the past several decades. This is not to say that CBO's methodology is perfect or that the data that are currently available to CBO are ideal. We are continually taking steps to refine our analyses as better data become available."[9]

If in fact the rich are getting richer and the poor are getting poorer, why are taxes becoming less progressive? According to the *Green Book* there are two explanations of the shift since 1977. The first is the rise in payroll taxes. Pay-

Table 2

Shares of Income for All Families before and after Federal Taxes and Shares of Federal Taxes Paid

All Families (by Income Group)	Income Share, 1977		Income Share, 1988		Share of Taxes Paid	
	Pretax	Aftertax	Pretax	Aftertax	1977	1988
Lowest quintile	4.9%	5.7%	3.7%	4.3%	2.0%	1.5%
Second quintile	10.6	11.6	9.0	9.8	7.2	6.2
Middle quintile	15.7	16.3	14.5	15.1	13.4	12.5
Fourth quintile	22.5	22.7	21.2	21.4	21.6	20.8
Highest quintile	46.6	44.0	51.9	49.8	55.7	58.9
Overall	100.0	100.0	100.0	100.0	100.0	100.0
Breakdown of top quintile						
81 to 90 percent	15.8	15.6	15.2	14.9	16.7	16.4
91 to 95 percent	10.2	9.9	10.2	9.8	11.3	11.6
96 to 99 percent	11.9	11.3	12.9	12.3	14.1	15.1
Top 1 percent	8.7	7.3	13.5	12.8	13.6	15.9

Source: *1991 Green Book*, pp. 1308 and 1309 (combining portions of Tables 21, 22, and 23). Data from Congressional Budget Office tax simulation model.

roll taxes represent a higher share of income for low- and middle-income families than for the rich, because this tax has a cap. It now consists of 6.2 percent of earnings up to $53,400 for Old Age, Survivors, and Disability Insurance and 1.45 percent up to $125,000 for hospital insurance. (Economists generally agree that the employer's share of the payroll tax—almost identical in size to the tax on the worker—is also effectively paid by the worker.) Since only the top fifth of households pay more in income tax than payroll tax, this group has received an overall tax reduction from the 1986 tax reform while the rest of the taxpaying population—excepting those who receive the EITC—faced a tax increase. (In 1978 the OASDI tax on workers was 6.05 percent on earnings up to $17,700.)[10] The other reason is that the income tax has indeed become much less progressive at the very top of the income scale (the top 1 percent) since 1977.[11] No matter how large a person's income, the tax rate remains at approximately 31 percent.

Significance of Ways and Means programs

Programs under the jurisdiction of the Committee on Ways and Means accounted for 33.1 percent of total federal government outlays (less interest) in 1990 and are estimated to take 40.9 percent in 1995.[12] Actual expenditure in fiscal year 1990 for these programs was $415 billion.[13] It will be of continuing interest to assess what is and what is not being accomplished with this investment. ■

---

[1]*1991 Green Book* (Washington, D.C.: U.S. Government Printing Office, 1991), p. iii.

[2]Among those organizations acknowledged in the 1991 volume for their contributions to the project were the Congressional Research Service of the Library of Congress, the Congressional Budget Office, the Office of Research and Statistics of the Social Security Administration, the Office of the Actuary of the Social Security Administration, the Health Care Financing Administration of the Department of Health and Human Services, the Prospective Payment Assessment Commission, the Physician Payment Review Commission, the Department of Labor, the Office of Family Assistance and the Office of Child Support Enforcement of the Department of Health and Human Services, the Census Bureau, the Pension Benefit Guaranty Corporation, the staff of the Railroad Retirement Board, the staff of the Joint Committee on Taxation, the General Accounting Office, the staff of the House Budget Committee, and the Luxembourg Income Study.

[3]Telephone conversation, August 14, 1991.

[4]Tables 17–21, pp. 1163–1172. Unfortunately, in the desperate rush to get the book out, a couple of rows of figures and the title were omitted from Table 20.

[5]An additional advantage of the *Green Book* is that all tables are in the public domain and may therefore be reproduced without the cost and time of obtaining permission.

[6]*1991 Green Book*, p. 1181. An IRP study by Peter Gottschalk and Sheldon Danziger, "Family Structure, Family Size, and Family Income: Accounting for Changes in the Economic Well-Being of Children, 1968–1986" (Discussion Paper no. 934–91, 1990), fleshes out what has happened to poverty among children over the period 1968–1986. They find that the relatively small changes in poverty over the eighteen years observed reflect large, but offsetting, demographic and economic changes. The increase in the number of children in households headed by women increased poverty among children, but at the same time the decline in family size and increased educational attainment of women reduced poverty.

[7]*1991 Green Book*, pp. 465–504.

[8]Letter from Robert D. Reischauer to Representative Dick Armey, Joint Economic Committee, Congress of the United States, June 3, 1991.

[9]Ibid.

[10]*1991 Green Book*, p. 75.

[11]Ibid., p. 1287.

[12]Ibid., p. 1523.

[13]Ibid., p. 1521.

IRP Reprints

Chernick, H., and Reschovsky, A. *The taxation of the poor.* 1990. 24 pp. R 639.

Seltzer, J. *Legal custody arrangements and children's economic welfare.* 1991. 35 pp. R 640.

Idson, T. L., and Robins, P. K. *Determinants of voluntary overtime decisions.* 1991. 13 pp. R 641.

Danziger, S. K., and Farber, N. B. *Keeping inner-city youths in school: Critical experiences of young black women.* 1990. 8 pp. R 642.

Mare, R. D. *Five decades of educational assortative mating.* 1991. 18 pp. R 643.

Haveman, R., Wolfe, B., and Spaulding, J. *Childhood events and circumstances influencing high school completion.* 1991. 25 pp. R 644.

Danziger, S. *Antipoverty policies and child poverty.* 1990. 8 pp. R 645.

Seltzer, J. A. *Relationships between fathers and children who live apart: The father's role after separation.* 1991. 23 pp. R 646.

Lundberg, S., and Plotnick, R. D. *Effects of state welfare, abortion and family planning policies on premarital childbearing among white adolescents.* 1990. 7 pp. R 647.

Sosin, M., Piliavin, I., and Westerfelt, H. *Toward a longitudinal analysis of homelessness.* 1990. 18 pp. R 648.

Mare, R. D., and Winship, C. *Socioeconomic change and the decline of marriage for blacks and whites.* 1991. 28 pp. R 649.

Oellerich, D. T., Garfinkel, I., and Robins, P. K. *Private child support: Current and potential impacts.* 1991. 21 pp. R 650.

Blau, D. M., and Robins, P. K. *Turnover in child care arrangements.* 1991. 6 pp. R 651.

Sandefur, G., Tuma, N., and Kephart, G. *Race, local labour markets and migration in the United States, 1975–1983.* 1991. 39 pp. R 652.

Blau, D. M., and Robins, P. K. *Child care demand and labor supply of young mothers over time.* 1991. 19 pp. R 653.

Moffitt, R. *Program evaluation with nonexperimental data.* 1991. 24 pp. R 654.

The Wisconsin welfare magnet debate: What is an ordinary member of the tribe to do when the witch doctors disagree?[1]

by Thomas Corbett

Thomas Corbett is an IRP affiliate and Assistant Professor, Division of University Outreach, Department of Governmental Affairs, University of Wisconsin-Madison. The author was a contributor to the 1986 study of the welfare magnet phenomenon commissioned by the Wisconsin Expenditure Commission that is discussed in this article. He has remained interested in the subsequent debate about the issue.

Rationality and the doing of public policy

Doing public policy well is a difficult undertaking. This is particularly true when dealing with what are termed wicked problems—when normative, theoretical, and technical contention is high. The welfare magnet issue, defined as the interstate relocation of low-income persons for the purpose of securing higher welfare benefits, is such a problem. Strongly held opinion dominates reasoned debate, even in Wisconsin, a state long associated with progressive and competent government.

A quarter century ago, confidence in government ran high. Faith in the capacity of social science to inform and shape public policy was widespread. Newly developed analytic techniques were expected to displace normative and ideological debate as the ordinary mechanism for conducting public affairs. "Logic, data, and systematic thinking were to compete with, if not dominate, 'politics' in the making of public decisions," Robert Haveman notes of this period.[2] It was anticipated that empirically based policy analysis would enable government to remedy the most refractory social problems, such as poverty.

The reign of rationality as the dominant public policy-making paradigm—even as an academic illusion—was short-lived. By the mid–1970s confidence in rigorous analysis and proactive government had declined visibly.[3] By the 1980s, the role of government and its supportive analytic apparatus in alleviating societal woes was judged to be incompetent at best[4] and perverse at worst.[5]

"Social myths thrive in environments without data," James Heckman asserts.[6] But in the real policy world it is equally plausible that myths thrive *because* of data—the very manner in which they are collected, presented, and interpreted. Policy analysis and political decisions are driven by preferred world views. Such views of how the world really operates are, in turn, expressions of deeply held values. Where issues are complex (e.g., poverty and public dependency), it is easy to engage in perceptual reductionism whereby large amounts of data are summarily reduced to a manageable size and conflicting interpretations are subject to theoretical simplification. For example, it becomes easier to select a portion of the poor to represent, or serve as a proxy for, the entire population, rather than deal with the practical and theoretical consequences of the diversity within the population. A simplified picture makes the policy-making task appear more manageable. Wicked problems seem to yield to simple solutions when the complexity of the issue is minimized.

The so-called welfare magnet issue in Wisconsin is an example of the tenuous link between rational analysis and the doing of public policy. The issue appears straightforward and amenable to empirical examination. Do low-income families relocate to Wisconsin to take advantage of the state's relatively generous benefits in the Aid to Families with Dependent Children (AFDC) program? As suggested in the abridged review of the Wisconsin welfare magnet debate presented below, it remains one of those wicked problems about which conflict and confusion abound respecting theory, evidence, and policy.

The issue and its origins

Because the size of the AFDC guarantee—the amount a family without other income receives in benefits—is deter-

mined by each state, actual welfare payments vary greatly across jurisdictions. Though nominally based on what it costs to live in each state (the need standard), local political and other idiosyncratic factors play an important role in determining benefit size. In Mississippi, for example, a one-parent family of three receives a maximum payment of $397 a month (in AFDC plus food stamps), whereas in Alaska, the maximum payment to the same size family is $1141.[7] It has long been assumed that this variation in the size of welfare benefits causes poor and jobless people to move to those states that provide the most generous benefits; such states are therefore considered welfare magnets. This belief encourages states to lower their benefits, at least below the payments offered in adjacent states, in the hope of exporting rather than importing indigent families.

Fear of attracting the poor is nothing new. The English Poor Laws, upon which the American approach to public assistance was originally based, were designed to restrict the mobility of the poor. In this country, as early as the eighteenth century, harsh measures were employed to deal with poor migrants. These included "warning out" (actively evicting poor transients), residency requirements (requiring an individual to live in an area for a period of time as a condition for receiving aid), and "charge backs" (billing the recipient's prior jurisdiction for assistance provided). Replacing cash payments with poorhouses and workhouses was the nineteenth-century approach to the problem.

As cash programs designed to aid the poor expanded in the middle of the twentieth century and the population became more and more mobile, the magnet question reemerged with increasing frequency. Officials in large northern metropolitan cities worried about magnet effects in the 1950s, in the wake of the massive migration of blacks out of the rural South to industrial centers in the North after World War II.[8] And it resurfaced in the public policy literature in the 1960s. In his 1969 message on welfare reform, President Nixon asserted that "due to widely varying payments among regions, [the welfare system] has helped draw millions into the slums of our cities."[9] Not surprisingly, increasing public concern over this issue coincided with dramatic AFDC caseload increases that can be traced back to the mid–1960s.

Despite the long history of concern over welfare magnet effects, research has been inconclusive. In 1974 Larry H. Long reviewed the early migration literature and asserted that "no study has presented empirical evidence for the hypothesis that welfare payments themselves have attracted large numbers of persons to states and cities with high benefit levels. Most factual analyses have considered the hypothesis and refuted it, but the evidence presented has not been entirely convincing."[10] In contrast, Richard Cebula concluded in a comprehensive 1979 review that the better studies provided definitive support for the welfare magnet thesis.[11] Nathan Glazer, who reviewed the literature on welfare migration for the U.S. Department of Health and Human Services, concluded that "welfare influences [interstate migration] but rather modestly."[12] And Paul Peterson and Mark Rom stated that "when people make major decisions as to whether they should move or remain where they are, they take into account the amount of welfare provision a state provides and the extent to which that level of support is increasing. . . . While the weight of the argument has begun to shift [toward support of the welfare magnet hypothesis], each of the new studies leaves the issue unresolved."[13]

The magnet issue comes to Wisconsin

The magnet issue arose in Wisconsin as the state's AFDC guarantee began to exceed benefit levels available elsewhere, especially in Illinois. In 1970 Wisconsin's AFDC guarantee for a three-person family was identical to that of the median state and less than the guarantee provided in neighboring states such as Illinois and Minnesota (see Table 1). But by the mid–1970s, this guarantee exceeded the median by almost one-half and, more important, exceeded what neighboring states were offering impoverished families. Sufficient concern about the magnet issue existed to warrant obtaining information on prior residential history from all new applicants for public assistance.

The question was fully engaged in the 1980s. The national economy experienced singular difficulties in the early part of the decade. Some argued that the economy was undergoing a process of long-term restructuring. Rustbelt states like Wisconsin were thought to be particularly vulnerable, facing a declining manufacturing base, diminished fiscal resources, and reduced federal revenue sharing. In this context, relatively high public assistance expenditures were

Table 1

AFDC Maximum Monthly Benefit for a Three-Person Family, by Selected States and for Selected Years

	1970	1975	1980	1985	1990
Wisconsin	$184	$342	$444	$533	$517
	(1.00)	(1.46)	(1.54)	(1.60)	(1.42)
Illinois	232	261	288	342	367
	(1.26)	(1.11)	(1.00)	(1.03)	(1.01)
Minnesota	256	330	417	524	532
	(1.39)	(1.40)	(1.45)	(1.58)	(1.46)
Mississippi	56	48	96	96	120
	(0.30)	(0.20)	(0.33)	(0.29)	(0.33)
Median	184	235	288	332	364

Source: U.S. House of Representatives, Committee on Ways and Means, *1990 Green Book* (Washington, D.C.: GPO, 1990), pp. 561–562.
Note: ( ) = Ratio of state's guarantee to median guarantee.

perceived as an insupportable state cost that could dissuade business executives from either remaining in or locating in a given state. Such a competitive environment exacerbated concerns about relative attractiveness and accelerated a self-reinforcing response among states to reduce social expenditures.

By 1985, for example, a family of three on AFDC living in Chicago could increase their cash monthly welfare benefit by almost $200 by relocating to Milwaukee, only ninety miles away (see Table 1). Various local officials pointed to increases in AFDC caseloads, particularly increases in new applicants from Illinois. It seemed obvious to some, and certainly plausible to others, that the increasing gap between the two state welfare programs had resulted in an influx of welfare-motivated in-migrants, especially from inner-city Chicago. This, in turn, was blamed for a worsening of such youth-related problems as school truancy, gang conflict, and drug trafficking.

Empirical work on the issue began in earnest in 1985. At the request of the Wisconsin Department of Health and Social Services (DHSS), Paul Voss of the University of Wisconsin's Applied Population Laboratory conducted a study. Using decennial census data, he estimated that although three AFDC families moved from Illinois to Wisconsin for every one moving in the opposite direction, the disparity could be explained by the size of the population pools in these two areas.[14] According to Voss, "The probability of an AFDC mother living in northeastern Illinois moving to southeastern Wisconsin is no greater than that of an AFDC mother in southeastern Wisconsin moving to northeastern Illinois."[15] This conclusion did not, however, prove convincing to the believers in the magnet phenomenon.

The same year Governor Anthony E. Earl authorized the creation of a Wisconsin Expenditure Commission to examine the state's fiscal picture and to search for ways to make the state more fiscally competitive. This commission established a special committee to examine the welfare magnet issue in detail and resolve the question once and for all. The committee was composed of representatives drawn from several organizations with an interest in the topic: officials from two key state agencies (the Department of Administration and the DHSS); officials from several counties thought most affected by welfare-motivated migration; members of the commission; and members of a research team chosen for the task. Paul Voss headed the university-based research team, which did its work under the auspices of the Applied Population Laboratory. (The Wisconsin Expenditure Commission initially approached the Institute for Research on Poverty, which turned down the opportunity to do the study because of the anticipated political response to any research, no matter how well done, on this inflammatory topic.)

Because the prior work by Voss (and others who used secondary data analyses) revealed nothing about the motivation of those welfare applicants who relocated across state lines at some point before seeking help, the committee felt impelled to move beyond census-type data in search of something more conclusive. They commissioned Voss and his colleagues to carry out a telephone survey with a sample of AFDC applicants in the summer of 1986 to tap the reasons behind their interstate move. These survey data would be combined with data obtained from a brief self-administered questionnaire completed at the time the application process was initiated and with administrative data normally collected by the state. Cognizant that respondents would give "socially acceptable answers," the research team couched their questions in ways designed to obscure the intent of the survey.

The committee's preliminary results—which had to be published before all the data were in—were that between 7 and 20 percent of those who had migrated to the state within the previous five years and who were AFDC applicants in the spring of 1986 were "influenced" to migrate by welfare benefit differentials.[16] They estimated that perhaps 10 percent of all migrants and 30 percent of recent migrants to Wisconsin (those who had moved within three months of the interview) were "motivated" to move because of these differentials. In the pool of all applicants (not just migrants), approximately 3 percent were estimated to be migrants motivated primarily by the higher welfare guarantees in Wisconsin. Adjusting for the fact that not all applicants receive AFDC, it was estimated that those motivated by the welfare differential amounted to merely 50 cases a month.

The survey also revealed that people moved for a number of reasons; the relocation decision was not one-dimensional. Some reasons for relocating—proximity to family and friends, the desire for a better life, and the hope of finding a job—appeared significantly more important than the size of welfare payments. Furthermore, it was found that some areas of the state had reason for concern. The *WEC Report* noted that "migrants for whom welfare played some role in the migration process tend to settle disproportionately in Milwaukee County. Nevertheless, other counties such as Kenosha, Racine, Rock (and perhaps others yet) can be dramatically affected even by small numbers of newcomers."[17]

The welfare magnet committee's answer to the question—Do low-income families move to Wisconsin to avail themselves of relatively more generous welfare benefits?—was far from the crisp resolution of the problem that had been anticipated. The study concluded: "The welfare magnet argument is not without support."[18] In fact, the committee produced so much data that both proponents and opponents of the magnet hypothesis could find evidence supportive of their position. The committee concluded, however, that a statewide policy response was not warranted since freezing benefits would hurt Wisconsin natives as well as in-migrants, and any policy directed only at migrants would raise constitutional questions. In the end, nothing was resolved, and study of the problem was suspended—despite the insistence of the research team that the study was incomplete

and that numerous methodological issues remained to be addressed.[19]

The witch doctors disagree

The magnet debate did not disappear. Partly rationalized by fears of welfare-motivated in-migration, AFDC guarantees were reduced by 5 percent in July 1987. Moreover, calls continued for the enactment of some form of residency requirement, though few pursued this option seriously, given that the courts would almost certainly strike down such a provision. Advocates for some response to the migration problem began to focus on what was called the two-tiered solution. In-migrants would be paid less in benefits than Wisconsin natives; they would receive the amount paid by the state from which they had moved for a period of six months.

As various ideological camps formed in light of the actual benefit cuts and proposals for a two-tiered welfare system, three distinguishable positions on the magnet issue emerged. Some, focusing on selected findings from the 1986 study, argued that AFDC in-migrants relocate for the same kinds of reasons that others do—community-specific attractions and economic opportunity. This might be called the quality-of-life argument. Others essentially dismissed the 1986 study, simply asserting that AFDC in-migrants *must* be coming for the higher benefits—what might be called the welfare-maximization argument. Still others argued that it makes no difference why migrants came; only the fact that they were here counted. We might call this the agnostic argument, since it implies that theory doesn't matter: All that matters is that undesirable families allegedly are moving into the state for a variety of reasons that may never be fully understood, and "something" must be done to alter this migration pattern.

Some of those not immediately involved in the emerging debate found the analysis in the *WEC Report* enlightening. In the summary of the welfare magnet issue literature, mentioned above, Nathan Glazer noted that "this study is unique and rich," and further described the analysis as "careful and persuasive."[20] Not all observers were as impressed. The debate picked up in 1988 when the Wisconsin Policy Research Institute (WPRI) published *Welfare In-Migration in Wisconsin: Two Reports.* The first report in this document, prepared by James Wahner and Jerome Stepaniak, was a study of welfare in-migration patterns and consequences in four southeastern Wisconsin counties—Milwaukee, Racine, Kenosha, and Rock. The second report in the document was a critique of the *WEC Report*, by Richard Cebula and Michael La Velle.[21]

Wahner and Stepaniak, in their *Four-County Report*, looked at the counties that were likely destinations for any welfare-motivated in-migrant because of their urban character and proximity to Chicago. The authors of the report made no attempt to tap the motivations behind the decision to relocate. All families who moved to Wisconsin for the first time and applied for AFDC at some future time were considered to be welfare in-migrants. Defined in this broad manner, the population of welfare in-migrants included nonnatives who had already lived for years in the state before applying for welfare.

Using this definition, Wahner and Stepaniak reported that between September 1985 and August 1988, 74,763 AFDC cases were opened in Wisconsin. Almost three in ten of these (29.3 percent) were cases involving a family head who had never before lived in Wisconsin. Furthermore, "some 46.5 percent or 10,809 of the newly opened cases in Milwaukee between September 1985 and August 1988 were nonresidents with no previous Wisconsin residency. This is a substantial number."[22] In point of fact, these were the same numbers reported by the Wisconsin Expenditure Commission, which had indicated that twice as many approved applicants for AFDC in Milwaukee were new residents (having moved to Wisconsin in the previous five years), compared to the rest of the state (47.7 percent vs. 23.6 percent),[23] and that three out of ten new applicants for welfare were in-migrants in that they had not been born in Wisconsin. Though no really new numbers were contained in Wahner and Stepaniak's report, the magnet question was transformed suddenly from a relatively small problem into a large and ominous one.

But it was and is unclear what these numbers actually mean. Were all these migrants motivated by the higher welfare payments? What would one find if one looked at a sample of applicants for welfare in Illinois? One might find that 30 percent of welfare applicants in Illinois had never lived in that state before. And what sort of interstate migration pattern would be found if one examined new applicants for, say, driver's licenses or bank accounts? If analysts found that 30 percent of applicants for new bank accounts were not Wisconsin natives, would they conclude that Wisconsin's superior banking practices had drawn them to the state? Figures such as "30 percent of applicants are not Wisconsin natives" are little more than so-what numbers—rather meaningless unless they can be analyzed within a sound theoretical framework *and* in terms of appropriate comparative data. (As mentioned earlier, the authors of the *WEC Report* had wanted to pursue such questions but failed to obtain funding from DHSS.)[24]

Wahner and Stepaniak drew the conclusion that "254 AFDC in-migration cases" were added to the caseload each month in the four counties they examined. They also declared that 70 percent of new entrants to the Milwaukee public schools, 58 percent of new beneficiaries of housing assistance, and about 33 percent of arrested juveniles were born outside of Wisconsin. These patterns were interpreted to represent a public policy crisis.

Cebula and La Velle, the authors of the second report, *Re-Examination Report*, claimed to look specifically at wel-

fare-motivated applicants for welfare, defined as anyone who, in the 1986 telephone survey, mentioned welfare at all, even if categorizing it as "not very important." Their conclusion was that in Wisconsin 497 applicants each month were welfare magnet migrants. After adjusting for the fact that not all applicants receive AFDC, they arrived at a monthly estimate of new magnet AFDC cases that was almost five times greater than the one suggested two years earlier by the Wisconsin Expenditure Commission.[25]

Based on their new estimate, they proposed that welfare benefit levels be frozen in Wisconsin until they were in line with the national average, that benefits should be maintained at that average, and that Wisconsin should consider imposing a three-to-six-month residency requirement for eligibility for welfare.

While politicians were debating a policy response to these alarming new numbers, another publication on welfare magnets was published by the Wisconsin Policy Research Institute. This document, titled *The Financial Impact of Out-of-State-Based Welfare In-Migration on Wisconsin Taxpayers,*[26] sought to spell out the fiscal consequences of welfare-motivated in-migration. The definition of welfare migration was widened once again. Now "out-of-state-based welfare in-migrants" included all those who had ever lived outside Wisconsin, no matter how long ago or under what circumstances they chose to move (or return) to Wisconsin. Like Wahner and Stepaniak, the author included, for example, a woman who moved to Wisconsin from Minnesota as a five-year-old and became a first-time applicant for AFDC twenty years later. But this study also included any Wisconsin native who left the state, if, upon returning, she eventually applied for welfare.

The estimated costs of this welfare in-migration phenomenon became truly frightening (see Table 2). According to these estimates 44 percent of the 10,000 AFDC entrants in 1988 were defined as out-of-state-based welfare in-migrants, presumably lured to Wisconsin by the welfare differential. This group, according to Cebula, generated additional costs amounting to $129 million in 1988: $52.9 million for increased benefits; $15.5 million for workers to manage the higher caseload; $54.6 million for educational costs; and $6 million for law enforcement costs. The *Financial Impact* stressed that these costs were additive and probably underestimated the true impact of welfare migration. The reader was also left with the impression that the costs were cumulative; that is, each year another $129 million would be added to the taxpayers' burden from welfare migrants.[27]

The AFDC costs in the paper raise questions rather than provide insights. Space permits me to touch upon only a few of these questions. The $52.9 million additional costs for benefits is based on the assumption that all in-migrants were on the welfare rolls from the first day of the calendar year and received a grant throughout the year. But analysts from the DHSS have pointed out that these migrants would have been absorbed onto the caseload over the course of the year and at least a third of them would have been off assistance for at least one month during the remainder of the year. The DHSS analysts conclude that an average stay on welfare of five months, not twelve months, be used in the computation. In their opinion the estimate in *Financial Impact* overstates the additional benefit expenditures by 140 percent.[28] Furthermore the study uses gross in-migration, ignoring the fact that people leave Wisconsin. The study also assumes that this population is chronically dependent—once on the rolls, always on the rolls. Yet the literature on welfare dynamics indicates that half of all recipients beginning a spell on AFDC leave welfare in a year or two, and only about one in three eventually become long-term dependents.[29]

Table 2

Summary of Increased Costs to Wisconsin Taxpayers in 1988 as Estimated in *Financial Impact,* Report of the Wisconsin Policy Research Institute (in millions of dollars)

	Never Lived in Wisconsin Before[a]	Returning to Wisconsin[b]	Total
AFDC-related costs			
Benefits	$36.5	$16.4	$52.9
Personnel	10.7	4.8	15.5
Subtotal	47.2	21.2	68.4
Education-related costs			
Direct	37.2	16.7	53.9
School lunch	0.5	0.2	0.7
Subtotal	37.7	16.9	54.6
Law enforcement costs			
Subtotal	4.2	1.8	6.0
Grand total	89.1	39.9	129[c]

Source: The *Financial Impact of Out-of-State-Based Welfare In-Migration on Wisconsin Taxpayers* (Milwaukee: Wisconsin Policy Research Institute, 1989).

[a]Defined as not born in Wisconsin but having maintained continuous residence after in-migrating. In-migration may have been in recent or distant past.

[b]Either born or lived in Wisconsin in past and has returned to the state either in the recent or distant past.

[c]State analysts have reestimated this figure. By making adjustments to inflow and exits based on available welfare data, they reduce this figure to about $46 million. They further adjust it by eliminating those in-migrants who did not obtain welfare within 6 months of moving to state and further reduce it to $24 million.

Whether in fact in-migrants are more dependent than others is an open question. The fact that they have had the drive to relocate in search of a better life suggests that they are unlikely to remain on welfare. Yet their drive may extend only to finding the most generous handout. Data on this point are inconclusive, though early results from a new study by Voss and Dana Soloff indicate that welfare use is greater among those who indicated in the 1986 survey that welfare influenced their decision to move.[30]

The educational costs in the table are estimated the same way the AFDC benefits are, on the assumption that the children start school the first day of class and stay in the school system for the entire year. It is further assumed that all welfare migrants have school-aged children. (Even if these numbers were correct, it is obviously in the state's interest to educate poor children, no matter where they lived in the past. Wisconsin, like other states, faces a labor shortage in the next decade and will need an influx of educated young people.)

The rest of the numbers in the table are more perplexing even than the AFDC-benefit calculations. For example, the cost of personnel is based on the assumption that a new welfare worker must be hired for every seven to eight cases added to the rolls and, of course, that the AFDC caseload is increasing. Yet the actual number of cases per worker is 83 (Wisconsin's per-month/per-case total administrative cost is only $26)[31] and no data were provided on actual caseload size changes over the study period. The costs to Wisconsin taxpayers for the school lunch program are typical of the logic used in *Financial Impact*. All AFDC children are eligible for free school lunches financed by the federal government. Whether a child eats that lunch in Chicago or Kenosha, the federal cost was $1.66 in 1988. Because of Wisconsin's efficiency in administering this program, the average cost of producing a school lunch was $1.26, substantially less than the $1.66 subsidy. So there is no increased school lunch cost to Wisconsin taxpayers if a child migrates from, say, the Illinois to the Wisconsin AFDC program. Rather, the federal reimbursement structure would actually help subsidize the cost of school-provided lunches for non-AFDC poor children in Wisconsin.

Perception and reality

Tables 3 and 4 compare estimates of caseload size and costs from the *Financial Impact*—extrapolating from the 1988 table and assuming that the numbers are additive and cumulative—with actual AFDC caseload data. The estimates derived from the logic employed in the *Financial Impact* bear little relationship to reality. Rather than increasing by more than 30 percent over the period from January 1986 to the end of 1988, the AFDC caseload actually dropped by 17 percent, from 100,000 to 83,373. Based on the logic of the *Financial Impact*, the estimated caseload at the end of the decade would be in excess of 140,000, whereas the actual figure was less than 80,000. Not surprisingly, expenditures on AFDC were dropping, abetted in part by the legislation in 1987 reducing the size of the welfare guarantee. Adjusting for this reduction in the predicted scenario would still put AFDC costs at over $64 million per month by the end of 1989, whereas the actual cost was $36,518,922—57 percent of the estimate based on the *Financial Impact*.[32]

Do these numbers mean that the suggested adverse fiscal impact of interstate migration is a fiction? Not necessarily. Other explanations could account for the discrepancy. For example, the aggregate caseload decline could be explained by a massive departure of Wisconsin natives from the welfare rolls, more than balancing the influx of out-of-staters. The administrative data maintained by DHSS, however, reflect no such scenario. The proportion of out-of-staters on the rolls has remained relatively constant, increasing only by 3 percentage points over the 1980s.

Table 3

AFDC Caseload Changes: Hypothetical Scenario and Actual Caseload, 1986–1988

Year and Month	Hypothetical Scenario: Additional AFDC Welfare Migrant Cases per Quarter	Hypothetical Scenario: Estimated Cumulative Caseload Growth	Actual Caseload
Jan. 1986	0	100,000	100,000
by March 1986	1,838	101,838	99,915
June 1986	2,800	104,638	98,660
Sept. 1986	2,802	107,440	97,529
Dec. 1986	2,812	110,252	95,158
March 1987	2,732	112,984	97,198
June 1987	2,763	115,747	95,565
Sept. 1987	2,725	118,472	92,876
Dec. 1987	2,695	121,167	89,312
March 1988	2,616	123,783	90,920
June 1988	2,577	126,360	86,888
Sept. 1988	2,852	129,212	85,870
Dec. 1988	2,190	131,402	83,373
March 1989	2,470	133,872	83,503
June 1989	2,610	136,482	81,244
Sept. 1989	2,904	139,386	79,838
Dec. 1989	2,332	141,714	79,359

Source: Hypothetical scenario is based on *Financial Impact*. Actual caseload from Wisconsin Department of Health and Social Services. Calculations by author.

Note: To derive the hypothetical size of the AFDC caseload, the monthly number of new AFDC cases (e.g., March 1986) is multiplied by 3 to give a quarterly figure and then multiplied by .44 (the percentage of new cases accounted for by out-of-state-based welfare in-migrants). It is assumed that no change occurs in the size of the Wisconsin native population on AFDC.

Table 4

Monthly Expenditures for AFDC: Hypothetical Scenario and Actual Expenditures, 1986–1988

Years	Per-Case Expenditure (1)	Scenario Caseload (2)	Scenario Expenditure (3)	Actual Expenditure (4)	Actual as % of Scenario (3/4 x 100)
1986[a]	$500	100,000	$50,000,000	$50,000,000	100%
1986[b]	498	110,000	54,780,000	47,356,943	86.4
1987[b]	459	120,000	55,080,000	41,953,247	76.2
1988[a]	459	130,000	59,670,000	38,277,811	64.1
1988[b]	460	140,000	64,400,000	36,518,922	56.7

Source: Hypothetical scenario is based on *Financial Impact* numbers. Actual expenditures are from the Wisconsin Department of Health and Social Services. Calculations by author.

[a]January data.
[b]December data.

Table 5

Summary of AFDC Trends over Time: 1985–1989

Year	Approved AFDC Applications	First-Time In-Migrants	First-Time In-Migrants Obtaining AFDC within 3 Months
1985[a]	2,128	620 (29.1%)	252 (11.8%)
1986[b]	2,116	620 (29.3)	249 (11.8)
1987[b]	2,067	606 (29.3)	244 (11.8)
1988[b]	1,938	554 (28.6)	234 (12.1)
1989[b]	1,954	571 (29.2)	236 (12.1)

Sources: *Financial Impact*, *WEC Report*, and DHSS administrative data.

Note: Percentages (in parentheses) are of approved applications.

[a]Based on September and December data.
[b]Based on March, June, September, and December data.

Another possibility is that the in-migrants are taking advantage of programs other than AFDC and food stamps. Perhaps legislation such as Learnfare and new work requirements have made AFDC less appealing, so new migrants are turning elsewhere, such as to the Food Stamp program, for assistance. But this assumption is also not borne out. Expenditures fell in the Food Stamp program just as they fell in AFDC. The only programs that expanded were Medicaid, where cost for health services historically outpaces inflation, and Supplemental Security Income, a program for the elderly, disabled, and blind poor.

During the height of the magnet debate, Wisconsin did not face a welfare crisis precipitated by an onslaught of out-of-staters rushing in to take advantage of generous AFDC benefits. Table 5 indicates that the proportion of new AFDC cases who had never before lived in Wisconsin has remained constant, at about 29 percent. Likewise, the proportion of newcomers who applied for AFDC within three months of moving to the state has been constant over time—about 12 percent. These numbers are unaffected by swings in the AFDC rolls and even remained constant after a cut in the AFDC guarantee.

The policy conundrum: Whom to believe?

Welfare magnet debates tend to be intense and protracted. Irrespective of numbers, the underlying hypothesis remains viable, partly because it is so plausible and partly because it is supported by anecdotal evidence. Lacking precise definitions and data, analysts can build conflicting cases and draw wildly differing conclusions. The Wisconsin debate produced just such ambiguous numbers. By some estimates, three in five applicants lived elsewhere at some point in the past. Roughly one in three moved to Wisconsin for the first time within five years of their welfare application. About one in five are estimated to be recent migrants—to have moved to Wisconsin within three months of applying for assistance. Less than one in twenty are recent migrants who indicated that welfare played a substantive (though not necessarily dominant) role in their relocation decision. And only 1 percent of all AFDC applicants in spring 1986 both obtained welfare and fully admitted that they were drawn to Wisconsin primarily by the welfare differential.[33]

How does one sort through such numbers and pick those that are policy relevant? For policymakers, the analytic context must have been confusing indeed. New studies and conclusions piled one upon another with little progress toward a definitive answer. Was the magnet problem large or small? Did welfare applicants move to Wisconsin primarily for higher benefits, primarily for quality-of-life factors, or for some combination of economic and noneconomic factors? What do the numbers mean?

Equally perplexing is the process by which the small numbers calculated in 1986 quickly got so large and frightening: Consider the continuing shift in conceptual definitions and research methodologies. In 1986, the focus was on estimating the numbers of "welfare-motivated" in-mi-

grants. A substantive test was employed; that is, what proportion of in-migrants who applied for AFDC were predominantly influenced by the welfare differential *and*, therefore, might respond to policies designed to diminish that differential? To answer this question, the intent behind the move had to be tapped. The researchers therefore relied upon a survey methodology. By the end of 1988, all in-migrants who had *never before* lived in Wisconsin were considered by some to be welfare-motivated in-migrants if they applied for welfare. Accessible administrative data could be used to estimate the magnitude of the phenomenon. A year later, the dominance of the agnostic perspective was reflected in the approach employed in the *Financial Impact*. Any welfare applicant who had *ever* lived outside of Wisconsin, no matter how long ago or under what circumstances she chose to move (or return) to Wisconsin, was designated an out-of-state-based welfare in-migrant.

As suggested earlier, the link between policy making and policy analysis is tenuous at best. Those convinced of the magnet problem selected those data and interpretations of the data that supported their preexisting beliefs. Those with the opposite opinion did the same. How one chooses among the available numbers depends upon individual norms and perceptions about the poor. Those fearing a large magnet effect appear to assume that interstate migrants who apply for welfare are the chronically dependent: looking for the best welfare deal and intending to stay on the rolls. An overreliance upon what was intuitively obvious might explain why available caseload figures were not examined to verify whether, in fact, the AFDC caseload was increasing during that period when a large fiscal impact of the in-migration effect was being argued. It was simply assumed that the caseload and the supportive bureaucracy *must* be increasing. In policy analysis, the obvious—when examined carefully and dispassionately—can easily turn out to be not so obvious in the end. This is confusing not only to the ordinary members of the tribe but to the witch doctors themselves.

Those who wish to minimize the magnet effect are no less guilty. Indeed, they are likely to argue that, as conditions in the cores of big cities continue to deteriorate, migrants have much more pressing reasons to relocate than marginally higher benefits. Their very lives are at stake.[34] In focusing exclusively on quality-of-life explanations, such arguments tend to downplay the extent to which welfare-motivated migration does exist. Undoubtedly, both welfare-differential *and* quality-of-life issues explain part of what is going on.

Can rigorous policy analysis contribute anything to such a contentious issue? That might well depend on whether sufficient attention is paid to the following factors:

- *Achieving conceptual clarity.* It is imperative that the policy question be clearly articulated. Which issue is of preeminent policy concern: the in-migration of welfare-motivated persons? of those likely to end up on welfare irrespective of motivation? of the poor in general? or of minority families in particular? These are different questions and invite different processes for answering them as well as different policy responses. The point here is that we must get the question right and define our terms clearly. The debate in Wisconsin became incomprehensible because definitions of the target group kept shifting—from welfare-motivated families to welfare-influenced families to low-income migrants who might need welfare. A policy question cannot be addressed until we state it clearly.

- *Establishing standards of proof.* Would we recognize welfare magnetism if it existed? This is a more difficult issue than would appear on the surface. Namely, what is the threshold level at which a phenomenon becomes a concern, or a problem requiring some kind of response, or a crisis requiring immediate attention? For some, the magnitude of welfare-motivated in-migration measured in the 1986 study required an immediate policy response; for others, it was little more than a concern. Moreover, the consequences of a policy response determine the standard of proof that should be employed. If a policy change will adversely affect a broad class of individuals—all welfare recipients or all recipients who lived elsewhere—evidence that a significant problem exists should be evaluated according to a more rigorous standard.

- *Making an adequate investment.* More rigorous standards of proof require the use of methodologies capable of identifying causal relationships—not merely that X and Y are related *but* that X causes Y. In this instance, it must be demonstrated not only that higher welfare benefits are associated with the in-migration of welfare users but that the size of the benefits causes the migration. Some dispute will always exist about the kind of methodology required to establish causation. What is clear is that the analysis must go beyond the single numbers used in the past. As suggested earlier, finding that 30 percent of applicants are not Wisconsin natives is a "so what" number. Without appropriate comparisons, we cannot determine if that number is high or low. It takes careful investigation and the investment of sufficient resources to move from supposition to proof.

- *Clearly relating evidence to policy.* Even if the welfare magnet hypothesis were proved at a level that warranted a policy response, the appropriate policy response would not be clear. For some, any proof of the magnet hypothesis would buttress calls for further retrenchment of welfare at the state level. Others would use the same evidence to call for an expansion of welfare at the national level through the creation of uniform minimum welfare guarantees. There is no single policy implication to any given research outcome.

What is the real problem?

Debates such as this may well distract the policy community from attending to more fundamental questions.

AFDC plays an increasingly marginal role in helping the poor. Nationwide, AFDC guarantees have declined in value by over 40 percent in the past two decades—though increases in in-kind supports (e.g., food stamps) have offset this drop by about one-half. The decline in the "real" value of AFDC benefits has been evidenced in virtually all states, those with high, medium, and low guarantees. Moreover, AFDC covers a smaller proportion of poor children, less than 60 percent now as opposed to 80 percent in the early 1970s. These trends could well continue as states, ever sensitive to the welfare magnet phenomenon, attempt to maintain their position vis-à-vis one another respecting the generosity of their public assistance programs.[35] While states compete to shove the problem under the rug (i.e., into another state), the proportion of all children who are poor has increased from about 15 percent in the mid–1970s to about 20 percent today.[36]

In short, welfare remains a terrible way to help the needy. It leaves children impoverished and encourages dependence. There must be a better way and the policy community would do well to focus its energies on finding innovative solutions to child poverty and welfare dependency. ■

---

[1]Henry J. Aaron, *Politics and the Professors* (Washington, D.C.: Brookings Institution, 1978), p. 159. The "members of the tribe" refers to anyone who wants answers to complex social and political questions and the "witch doctors" refers to those who try to provide those answers.

[2]Haveman, *Poverty Policy and Poverty Research, 1965–1980: The Great Society and the Social Sciences* (Madison: University of Wisconsin Press, 1987), p. 33.

[3]See Aaron, *Politics and the Professors.*

[4]Friedrich Hayek, *The Fatal Conceit* (Chicago: University of Chicago Press, 1989).

[5]See, for example, Charles Murray, *Losing Ground: American Social Policy, 1950–1980* (New York: Basic Books, 1984).

[6]Heckman, "Social Science Research and Policy," *Journal of Human Resources*, 25, no. 2 (Spring 1990), p. 301.

[7]U.S. House of Representatives, Committee on Ways and Means, *1991 Green Book* (Washington, D.C.: GPO, 1991), p. 597.

[8]For a detailed account of the black migration to Chicago, see Nicholas Lemann, *The Promised Land* (New York: Alfred A. Knopf, 1991).

[9]Richard Nixon, "Proposals for Welfare Reform," U.S. House of Representatives, 91st Congress, 1st session, Document no. 91–146, p. 2.

[10]"Poverty Status and Receipt of Welfare among Migrants and Nonmigrants in Large Cities," *American Sociological Review* 39 (February 1974), 46–47.

[11]"A Survey of the Literature on the Migration-Impact of State and Local Policies," *Public Finance* 34 (1979), 69–84.

[12]Nathan Glazer, *Migration and Welfare,* a report prepared for the U.S. Department of Health and Human Services, Office of the Assistant Secretary for Planning and Evaluation, Income Security Policy, under contract no. HHS–100–86-0021 (David Ellwood and Robert Lerman, principal investigators), Washington, D.C., 1987, p. 23.

[13]"Federalism and Welfare Reform: The Determinants of Interstate Differences in Poverty Rates and Benefit Levels," paper prepared for delivery at the 1987 Annual Meeting of the American Political Science Association, Chicago, September 3–6, 1987, p. 25. See also Paul Peterson and Mark Rom, *Welfare Magnets: A New Case for a National Standard* (Washington, D.C.: Brookings Institution, 1990); and Robert Moffitt, *Incentive Effects of the U.S. Welfare System: A Review*, IRP Special Report no. 48, University of Wisconsin-Madison, 1991. These works had not been published at the height of the Wisconsin debate.

[14]Voss, "Migration of Low-Income Families and Individuals," Applied Population Laboratory, Department of Rural Sociology, University of Wisconsin-Madison, 1985, pp. 13–14.

[15]Voss, "A Demographic Portrait of Wisconsin's People," in *State Policy Choices: The Wisconsin Experience*, ed. Sheldon Danziger and John F. Witte (Madison: University of Wisconsin Press, 1988), p. 92.

[16]Wisconsin Expenditure Commission, *The Migration Impact of Wisconsin's AFDC Benefit Levels: A Report to the Wisconsin Expenditure Commission by the Welfare Magnet Study Committee* (Madison: Wisconsin Department of Administration, 1986). This report is hereafter referred to as the *WEC Report.* It is useful to distinguish between the *welfare-motivated* in-migrant and the *welfare-influenced* in-migrant. The former is likely to have moved to Wisconsin primarily—if not solely—for the higher AFDC benefits. The latter may have taken the higher benefits into consideration but was also influenced by other factors, such as labor market opportunities and quality-of-life considerations. The distinction is not a trivial one. Welfare-*motivated* in-migrants can be expected to respond to policy changes designed to reduce the magnet phenomenon. For the welfare-*influenced* in-migrant, the response would depend on how dramatic the new policy was (e.g., how deeply benefits were cut and for how long). And no effect can be anticipated among low-income migrants (who eventually apply for welfare) who move to Wisconsin without considering the welfare differential.

[17]*WEC Report*, Executive Summary, p. v.

[18]Ibid., p. iv.

[19]The authors of the report identified a number of defects in the survey. They had taken shortcuts in preparing and analyzing the data, and had no comparison data for other jurisdictions or time periods. Aware of the selectivity problems in the sample, they planned to replicate the survey at a later date with a broader sample, such as all low-income female-headed migrant families, rather than merely those applying for welfare. And they felt that to obtain a more complete understanding of the migration phenomenon, it was essential to track the applicants over time, to discover whether in fact they made use of welfare, where they resided, and if and when they obtained jobs.

[20]See Glazer, *Migration and Welfare*, p. 22.

[21]Wisconsin Policy Research Institute, *Welfare In-Migration in Wisconsin: Two Reports.* The first report, titled, *Welfare In-Migration: A Four-County Report*, by James Wahner and Jerome Stepaniak, is hereafter cited as *Four-County Report.* The second report in this document, titled *The Migration Impact of Wisconsin's AFDC Benefit Levels: A Re-Examination*, by Richard Cebula and Michael La Velle, is cited hereafter as the *Re-Examination Report.* Both were published by the Wisconsin Policy Research Institute, Milwaukee, Wis., 1988.

[22]*Four-County Report*, p. 7.

[23]*WEC Report*, Table 24, p. 104.

[24]Paul Voss and I submitted proposals to DHSS in 1987 and 1988, to no avail.

[25]*Re-Examination Report*, pp. 2, 3.

[26]Wisconsin Policy Research Institute, *The Financial Impact of Out-of-State-Based Welfare In-Migration on Wisconsin Taxpayers*, by Richard Cebula (Milwaukee, Wis.: WPRI, 1989). Hereafter cited as *Financial Impact.*

[27]*Financial Impact* states that the in-migration cases "are shown to result in an overall additional aggregate cost (burden) per year to Wisconsin taxpayers of approximately $129 million" (p. 37).

[28]This conclusion is based on conversations with Neil Gleason and Ed Mason, analysts in the Wisconsin DHSS, who had provided the data for the *Financial Impact.*

[29]See Mary Jo Bane and David Ellwood, "Slipping into and Out of Poverty: The Dynamics of Spells," *Journal of Human Resources*, 21, no. 1 (Winter 1986), 1–23.

[30]An unpublished work in progress. Preliminary results suggest that those welfare applicants who said they came to Wisconsin in 1986 to seek better economic opportunities did, in fact, demonstrate a greater attachment to the labor force in subsequent years than those who did admit that the welfare differential motivated their move. Thomas Barton, while writing his Ph.D. dissertation at the University of Wisconsin-Madison, found that about 80 percent of AFDC entrants in Kenosha County, Wis., exited from the welfare rolls of that county at least once over the subsequent 30 months. The exit rate for nonnatives was higher than for those who always were Wisconsin residents. This finding, if confirmed, might explain a curious anomaly. Except for Milwaukee, the AFDC rolls in what were considered "magnet" counties—because of their size and proximity to Illinois—fell by 20 to 25 percent between 1986 and 1989. In Milwaukee, the caseload also fell, but only by 6 percent.

[31]Calculated from data included in the *1990 Green Book.*

[32]Actual caseload figures and expenditures are derived from Wisconsin DHSS management reports.

[33]Data from *WEC Report.*

[34]Officials in Dane County (the site of the state capital) have noted a dramatic influx of low-income minority families in the past several years. For example, public school officials note that the number of elementary school children who experience episodes of homelessness—80 percent of whom are minorities and in-migrants—has been doubling each year since 1987, when 70 children in the county became homeless. There is considerable speculation that the "new" migration flow is from those urban areas already infested with crack cocaine (e.g., Chicago, Milwaukee) to middle-size cities that look safer to economically disadvantaged parents. Some have labeled this the secondary-city migration pattern.

[35]Only one state, California, maintained the real value of its AFDC guarantee over the 1970–1990 period (*1991 Green Book*, p. 601).

[36]Ibid., p. 1051.

Announcements

Fourth Luxembourg Income Study Summer Workshop

The *Luxembourg Income Study* has made comparable several large microdata sets which contain comprehensive measures of income and economic well-being for a set of modern industrialized welfare states. The LIS databank currently covers eighteen countries, including Australia, Canada, France, Germany, Israel, Italy, the Netherlands, Sweden, the United Kingdom, the United States, and several Eastern European nations including Poland and Hungary. Data are available for at least two periods for most of these nations.

The *LIS Summer Workshop* is a two-week pre- and postdoctoral workshop designed to introduce young scholars in the social sciences (economics, sociology, other) to comparative research in income distribution and social policy using the LIS database. The 1990 workshop attracted 25 attendees from 12 countries. The fourth workshop will be held July 19–31, 1992, in Luxembourg. The cost will be 38,000 Belgian Francs (about $1,100), which includes tuition, local travel, and full room and board. International transportation is not included. Students are expected to be subsidized by home countries, national and international research foundations, universities, and other sources, including at least two special scholarships from the Ford Foundation LIS Development Initiatives Fund.

The language of instruction will be English. The course of study will include a mix of lectures and assistance and direction using the LIS database to explore a research issue chosen by the participant. Faculty are expected to include Anthony Atkinson (U.K.), Frank Cowell (U.K.), Peter Gottschalk (U.S.), Richard Hauser (Germany), Kristof Starzec (France), Shelly Phipps (Canada), and the entire LIS staff. Several topics to be analyzed in 1992 include the effects of income security programs in Eastern Europe and cross-national trends in earnings inequality.

Additional information, including application forms, are available from Timothy Smeeding, LIS Project Director (Professor of Economics & Public Administration, 400 Maxwell Hall, Syracuse University, Syracuse, NY 13244, U.S.); Lee Rainwater, LIS Research Director (Sociology, Harvard University, Cambridge, MA 02138, U.S.); or Uwe Warner (LIS at CEPS/INSTEAD, B.P. #65, L–7201 Walferdange, Luxembourg). Applications are due by May 1, 1992.

Postdoctoral Funding Opportunity: IRP Small Grants Program, 1992–1993

The Institute for Research on Poverty and the U.S. Department of Health and Human Services will sponsor the eleventh competition under the Small Grants and Sabbatical Grants Program for research on poverty-related topics during the period July 1992 through June 1993. Two programs are offered: (1) several grants of up to $15,000 each are available to cover up to two months of salary and research costs and do not require residence in Madison or Washington, D.C.; (2) a smaller number of sabbatical grants of up to $35,000 each are available for visitors in residence at either IRP or the Department of Health and Human Services during the 1992–93 academic year. This year the sabbatical grants may include up to nine months of salary for a junior scholar or up to 4.5 months for a senior scholar. Researchers must hold the Ph.D. To obtain guidelines, address the request to Small Grants Program, Institute for Research on Poverty, 1180 Observatory Drive, Madison, WI 53706.

Deadline for receipt of applications: February 14, 1992.

Postdoctoral Fellowships in the Study of Aging

RAND is accepting applications for a postdoctoral fellowship in the study of aging, funded by the National Institute of Aging. This program aims to sharpen the research skills of recent Ph.D.'s in sociology, economics, or related disciplines who have a substantive interest in the study of aging. Research on aging may include topics on the economic well-being of the older population or changes in well-being over the life cycle. Fellows may conduct research on topics such as the distribution of consumption, income, or wealth, the economic consequences of retirement, or the effect of government transfers on the economic status of the older population.

The fellowship, for one year with possible renewal for a second year, carries a stipend of $30,000 to $45,000, depending on qualifications. The deadline for receipt of completed applications is February 1, 1992. Applicants must be U.S. citizens or permanent residents. Interested parties should write Dr. Lynn A. Karoly, RAND, 1700 Main Street, Santa Monica, CA 90407-2138, or telephone her at (310) 393-0411, ext. 7359.

Postdoctoral Fellowship Opportunities at the University of Michigan

The University of Michigan's Research and Training Program on Poverty, the Underclass, and Public Policy offers one- or two-year postdoctoral fellowships to American minority scholars to expand knowledge in this area in all the social sciences. Fellows will conduct their own research and participate in a year-long seminar on Poverty, the Underclass, and Public Policy under the direction of Sheldon Danziger, Professor of Social Work and Public Policy, and Mary Corcoran, Professor of Political Science, Public Policy, and Social Work. Funds are provided by the Ford and Rockefeller Foundations. Applicants must have completed their Ph.D. by August 1, 1992. The application deadline is January 10, 1992. For an application packet, contact the Program on Poverty, the Underclass, and Public Policy, School of Social Work, 1065 Frieze Building, University of Michigan, Ann Arbor, MI 48109-1285.

Job Opportunity: Institute for Research on Poverty

IRP has an opening for a research analyst with a Ph.D. in economics, sociology, demography, or a closely related field. Expertise is required in the use of complex social survey data, such as the decennial census, the Current Population Surveys, and the Survey of Income and Program Participation. Applicants should be interested in the study of poverty, income distribution, welfare services, educational or occupational inequality, or labor markets.

Duties of the job include producing periodic and special reports on trends and differentials in income, poverty, and welfare receipt; preparing analyses and estimates as needed; conducting independent research; and serving as a consultant to IRP affiliates.

The appointment will be an academic staff position at the University of Wisconsin-Madison. The salary is open. Interested applicants should send a letter, a curriculum vitae, and three references to IRP Personnel, Room 3416, 1180 Observatory Drive, Madison, WI 53706.

Application deadline: January 15, 1992.

Order form for FOCUS NEWSLETTER and INSIGHTS (free of charge)

Send to: FOCUS
Institute for Research on Poverty
1180 Observatory Drive
3412 Social Science Building
University of Wisconsin
Madison, WI 53706

Name: _____

Address: _____

_____

City State Zip

(Multiple copies of any issue: $1.00 each)

❑ I wish to receive INSIGHTS

Order form for Institute DISCUSSION PAPERS and REPRINTS

Prepayment required. Make checks payable to the Institute for Research on Poverty in U.S. dollars only.

SUBSCRIPTIONS: July 1991–June 1992
❑ Discussion Papers and Reprints ($50.00)
❑ Discussion Papers only ($40.00)
❑ Reprints only ($25.00)

INDIVIDUAL PUBLICATIONS: (Please fill in number or title and author)

Discussion Papers ($3.50) _____

Reprints ($2.00) _____

Special Reports (prices vary) _____

Send to: Institute for Research on Poverty
1180 Observatory Drive
3412 Social Science Building
University of Wisconsin
Madison, WI 53706

Name: _____

Address: _____

_____

City State Zip

1180 Observatory Drive
3412 Social Science Building
University of Wisconsin–Madison
Madison, Wisconsin 53706

UNIVERSITY OF
WISCONSIN
MADISON